What critics said about the First Edition of this book

'Maxwell is advocating nothing less than a revolution (based on reason, not on religious or Marxist doctrine) in our intellectual goals and methods of inquiry… There are altogether too many symptoms of malaise in our science-based society for Nicholas Maxwell's diagnosis to be ignored.'

Professor Christopher Longuet-Higgins, *Nature*

'a strong effort is needed if one is to stand back and clearly state the objections to the whole enormous tangle of misconceptions which surround the notion of science to-day. Maxwell has made that effort in this powerful, profound and important book.'

Dr. Mary Midgley, *University Quarterly*

'The essential idea is really so simple, so transparently right… It is a profound book, refreshingly unpretentious, and deserves to be read, refined and implemented.'

Dr. Stewart Richards, *Annals of Science*

'Maxwell's book is a major contribution to current work on the intellectual status and social functions of science ... [It] comes as an enormous breath of fresh air, for here is a philosopher of science with enough backbone to offer root and branch criticism of scientific practices and to call for their reform.'

Dr. David Collingridge, *Social Studies of Science*

'Maxwell has, I believe, written a very important book which will resonate in the years to come. For those who are not inextricably and cynically locked into the power and career structure of academia with its government-industrial-military connections, this is a book to read, think about, and act on.'

Dr. Brian Easlea, *Journal of Applied Philosophy*

Cover illustration:
detail from *The Effects of Good Government in the Town*, 1337–9, by Ambrogio Lorenzetti, fresco in the Town Hall, Siena

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Pentire Press

FROM KNOWLEDGE TO WISDOM MAXWELL

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FROM KNOWLEDGE TO WISDOM

A REVOLUTION FOR SCIENCE AND THE HUMANITIES

NICHOLAS MAXWELL

SECOND EDITION

More comments on the first edition of *From Knowledge to Wisdom*

'This book is a provocative and sustained argument for a 'revolution', a call for a 'sweeping, holistic change in the overall aims and methods of institutionalized inquiry and education, from knowledge to wisdom' ... Maxwell offers solid and convincing arguments for the exciting and important thesis that rational research and debate among professionals concerning values and their realization is both possible and ought to be undertaken.' **Professor Jeff Foss, *Canadian Philosophical Review***

'Wisdom, as Maxwell's own experience shows, has been outlawed from the western academic and intellectual system ... In such a climate, Maxwell's effort to get a hearing on behalf of wisdom is indeed praiseworthy.'
Dr. Ziauddin Sardar, *Inquiry*

'Maxwell's argument ... is a powerful one. His critique of the underlying empiricism of the philosophy of knowledge is coherent and well argued, as is his defence of the philosophy of wisdom. Most interesting, perhaps, from a philosophical viewpoint, is his analysis of the social and human sciences and the humanities, which have always posed problems to more orthodox philosophers, wishing to reconcile them with the natural sciences. In Maxwell's schema they pose no such problems, featuring primarily ... as methodologies, aiding our pursuit of our diverse social and personal endeavours. This is an exciting and important work, which should be read by all students of the philosophy of science. It also provides a framework for historical analysis and should be of interest to all but the most blinkered of historians of science and philosophy.'
Dr. John Hendry, *British Journal for the History of Science*

'… a major source of priorities, funds and graduates' jobs in 'pure science' is military … this aspect of science is deemed irrelevant by the overwhelming majority of those who research, teach, sociologize, philosophise or moralize about science. What are we to make of such a phenomenon? It is in part a *political* situation, in its causes and effects; but it is also *philosophical*, and this is Nick Maxwell's point of focus. Such a gigantic co-operative endeavour of concealment, amounting to a huge deception, could be accomplished naturally by all educated, humane participants, a 'conspiracy needing no conspirators', *only* because their 'philosophy of knowledge' envelops them in the assurance that their directors, paymasters and employers have nothing to do with the *real* thing – the research. This, to me, is the heart of Maxwell's message.'
Dr. Jerry Ravetz, *British Journal for the Philosophy of Science*

'This book is written in simple straightforward language … The style is passionate, committed, serious; it communicates Maxwell's conviction that we are in deep trouble, that there is a remedy available, and that it is ingrained bad intellectual habits that prevent us from improving our lot … Maxwell is raising an important and fundamental question and things are not going so well for us that we should afford the luxury of listening only to well-tempered answers.' **Professor John Kekes, *Inquiry***

'Because Maxwell so obviously understands and loves science as practiced, say, by an Einstein, his criticisms of current science seem to arise out of a sadness at missed opportunities rather than hostility … I found Maxwell's exposition and critique of the current state of establishment science to be clear and convincing … Maxwell is right to remind us that in an age of Star Wars and impending ecological disaster, talk of the positive potential of means-oriented science can easily become an escapist fantasy.'
Professor Noretta Koertge, *Isis*

'In an admirable book called *From Knowledge to Wisdom*, Nicholas Maxwell has argued that the radical, wasteful misdirection of our whole academic effort is actually a central cause of the sorrows and dangers of our age . . .Thinking out how to live is a more basic and urgent use of the human intellect than the discovery of any fact whatsoever, and the considerations it reveals ought to guide us in our search for knowledge. . . In arguing this point . . . Maxwell proposes that we should replace the notion of aiming at knowledge by that of aiming at wisdom. I think this is basically the right proposal. . . Maxwell is surely right in saying that [the distorted pursuit of knowledge], because it wastes our intellectual powers, has played a serious part in distorting our lives.' **Mary Midgley, *Wisdom, Information and Wonder***

'[T]here is...much of interest and, yes, much of value in this book...Maxwell is one of those rare professional philosophers who sees a problem in the divorce between thought and life which has characterized much of modern philosophy (and on both sides of the English channel, not merely in the so-called 'analytic' tradition'); he wishes to see thought applied to life and used to improve it. As a result, many of the issues he raises are of the first importance … He has . . produced a work which should give all philosophers and philosophically-minded scientists cause for reflection on their various endeavors; in particular, it should give philosophers who are content to be specialists a few sleepless nights.'
Professor Steven Yates, *Metaphilosophy*

'Maxwell [argues for] an "intellectual revolution" that will affect the fundamental methods of inquiry of science, technology, scholarship and education, looking not for knowledge for knowledge's sake, but for wisdom, which he says is more rational and of greater human value and holds the potential to alleviate human problems and institute social change. A humanist and philosopher, Maxwell presents his ideas with eloquence and conviction. This book will appeal to persons in many different disciplines – from science to social studies.' ***American Library Association***

'This book is the work of an unashamed idealist; but it is none the worse for that. The author is a philosopher of science who holds the plain man's view that philosophy should be a guide to life, not just a cure for intellectual headaches. He believes, and argues with passion and conviction, that the abysmal failure of science to free society from poverty, hunger and fear is due to a fatal flaw in the accepted aim of scientific endeavour – the acquisition and extension of knowledge. It is impossible to do Maxwell's argument justice in a few sentences, but, essentially, it is this. At the present time the pursuit of science – indeed the whole of academic inquiry – is largely dominated by 'the philosophy of knowledge'. At the heart of this philosophy is the assumption that knowledge is to be pursued for its own sake. But the pursuit of objective truth must not be distorted by human wishes and desires, so scientific research becomes divorced from human needs, and a well-intentioned impartiality gives way to a deplorable indifference to the human condition. The only escape is to reformulate the goals of science within a 'philosophy of wisdom', which puts human life first and gives 'absolute priority to the intellectual tasks of articulating our problems of living, proposing and criticizing possible solutions, possible and actual human actions'. The philosophy of wisdom commends itself, furthermore, not only to the heart but to the head: it gives science and scholarship a proper place in the human social order. . . Nicholas Maxwell has breached the conventions of philosophical writing by using, with intent, such loaded words as 'wisdom', 'suffering' and 'love'. 'That which is of value in existence, associated with human life, is inconceivably, unimaginably, richly diverse in character.' What an un-academic proposition to flow from the pen of a lecturer in the philosophy of science; but what a condemnation of the academic outlook, that this should be so. Mr. Maxwell is advocating nothing less than a revolution (based on reason, not on religious or Marxist doctrine) in our intellectual goals and methods of inquiry ... There are altogether too many symptoms of malaise in our science-based society for Nicholas Maxwell's diagnosis to be ignored.'
Professor Christopher Longuet-Higgins, *Nature*

'Maxwell's thesis is that the evident failure of science to free society from poverty, hunger and the threat of extinction results from a 'fatal flaw in the accepted aim of scientific endeavour'. . . It is precisely because of 'the accepted aim' that acquisition of knowledge, which presumably originated as an essential strategy for survival, has given rise to the relentless pursuit of new and better ways of achieving the exact opposite. . . For Maxwell, the solution is obvious – a radically new approach to the whole business of intellectual inquiry. . . It is hard to argue with these aims . . . If we could only change the way people *feel*, Maxwell's solution would be easier, if not easy.' **Professor Norman F. Dixon, *Our Own Worst Enemy***

'a sustained piece of philosophical reasoning which makes a real contribution to the reinstatement of philosophy as a central concern. We need to follow Maxwell's lead in constructing a philosophy of wisdom.'
P. Eichman, *Perspectives on Science and Christian Faith*

Comments on Maxwell's The Comprehensibility of the Universe: A New Conception of Science

'Nicholas Maxwell's ambitious aim is to reform not only our philosophical understanding of science but the methodology of scientists themselves ... Maxwell's aim-oriented empiricism [is] intelligible and persuasive ... the main ideas are important and appealing ... an important contribution to the philosophy of physics.'
J. J. C. Smart, *British Journal for the Philosophy of Science*

'Maxwell has clearly spent a lifetime thinking about these matters and passionately seeks a philosophical conception of science that will aid in the development of an intelligible physical worldview. He has much of interest to say about the development of physical thought since Newton. His comprehensive coverage and sophisticated treatment of basic problems within the philosophy of science make the book well worth studying for philosophers of science as well as for scientists interested in philosophical and methodological matters pertaining to science.'
Professor Cory F. Juhl, *International Philosophical Quarterly*

For more comments on books by Nicholas Maxwell, see the final pages of this book.

From Knowledge to Wisdom

A Revolution for Science and the Humanities

Nicholas Maxwell

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In memory of my mother and father

and to Christine van Meeteren

Note about the Second Edition

For this second edition, the book has been updated throughout. It has a new introduction, and three new final chapters – chapters 12, 13 and 14. Chapter 6 has much new material about academia around the year 2003.

About the Author

Nicholas Maxwell has devoted much of his working life to arguing that we need to bring about a revolution in academia so that it comes to seek and promote wisdom and does not just acquire knowledge. Apart from the present book, he has published four others on this theme: *What's Wrong With Science?* (Bran's Head Books, 1976), *The Comprehensibility of the Universe* (Oxford University Press, 1998), *The Human World in the Physical Universe* (Rowman and Littlefield, 2001) and *Is Science Neurotic?* (Imperial College Press, 2004). He has also contributed to a number of other books, and has published numerous papers in science and philosophy journals on problems that range from consciousness to quantum theory. For nearly thirty years he taught philosophy of science at University College London, where he is now Emeritus Reader in Philosophy of Science and Honorary Senior Research Fellow. He has given lectures at Universities and Conferences all over Britain, Europe and north America, and has taken part in the BBC Programme 'Start the Week' on Radio 4. In 2003 he founded Friends of Wisdom, an international group of people sympathetic to the idea that academic inquiry should help humanity acquire more wisdom by rational means (see www.knowledgetowisdom.org). More information about his life and work can be found on his website: see www.nick-maxwell.demon.co.uk

Preface to the First Edition 1984

This book argues for the need to put into practice a profound and comprehensive intellectual revolution, affecting to a greater or lesser extent all branches of scientific and technological research, scholarship and education. This intellectual revolution differs, however, from the now familiar kind of scientific revolution described by Kuhn. It does not primarily involve a radical change in what we take to be knowledge about some aspect of the world, a change of paradigm. Rather it involves a radical change in the fundamental, overall intellectual aims and methods of inquiry. At present inquiry is devoted to the enhancement of knowledge. This needs to be transformed into a kind of rational inquiry having as its basic aim to enhance personal and social wisdom. This new kind of inquiry gives intellectual priority to the personal and social problems we encounter in our lives as we strive to realize what is desirable and of value – problems of knowledge and technology being intellectually subordinate and secondary. For this new kind of inquiry, it is what we do and what we are that ultimately matters: our knowledge is but an aspect of our life and being.

I shall argue that a necessary, though not a sufficient, condition for us to develop cooperatively a better, more humane world is that we have in existence a tradition of rational inquiry of this new kind, giving priority to life and its problems, devoted to the enhancement of wisdom. At present we have no such tradition. As a result we are all more or less severely handicapped in our capacity to resolve in desirable and good ways problems we encounter in our personal and social lives. Many of our present-day social and global problems are in part due to our long-standing failure to develop such a tradition of genuinely rational, socially active thought, devoted to the growth of wisdom. This basic Socratic idea has been betrayed, and as a result, to put it at its most extreme, we now stand on the brink of self-destruction. In the circumstances, there can scarcely be any more urgent task for all those associated in any way with the academic enterprise – scientists, technologists, scholars, teachers, administrators, students, parents, providers of funds – than to help put into practice the new kind of inquiry, rationally devoted to the growth of wisdom.

Introduction to Second Edition 2007

Academia as it exists today is the product of two past great intellectual revolutions.

The first is the scientific revolution of the 16th and 17th centuries, associated with Galileo, Kepler, Descartes, Hooke, Boyle, Newton and many others, which in effect created modern science. A method was discovered for the progressive acquisition of knowledge, the famous empirical method of science.

The second revolution is that of the Enlightenment, especially the French Enlightenment, in the 18th century. Voltaire, Diderot, Condorcet and the other *philosophes* had the profoundly important idea that it might be possible to learn from scientific progress how to achieve social progress towards an enlightened world. They did not just have the idea: they did everything they could to put the idea into practice in their lives. They fought dictatorial power, superstition, and injustice with weapons no more lethal than those of argument and wit. They gave their support to the virtues of tolerance, openness to doubt, readiness to learn from criticism and from experience. Courageously and energetically they laboured to promote reason and enlightenment in personal and social life.

Unfortunately, in developing the Enlightenment idea intellectually, the *philosophes* blundered. They botched the job. They thought the proper way to implement the Enlightenment Programme of learning from scientific progress how to achieve social progress towards an enlightened world is to develop the social sciences alongside the natural sciences. If it is important to acquire knowledge of natural phenomena to better the lot of mankind, as Francis Bacon had insisted, then (so, in effect, the *philosophes* thought) it must be even more important to acquire knowledge of social phenomena. First, knowledge must be acquired; then it can be applied to help solve social problems. They thus set about creating and developing the social sciences: economics, psychology, anthropology, history, sociology, political science.

This traditional version of the Enlightenment Programme, despite being damagingly defective, was immensely influential. It was developed throughout the 19th century, by men such as Saint-Simon, Comte, Marx, Mill and many others, and was built into the intellectual-institutional structure of academic inquiry in the first part of the 20th century with the creation of departments of the social sciences in universities all over the world.

Thus academic inquiry today, devoted primarily to the pursuit of knowledge and technological know-how, is the outcome of two revolutions: the scientific revolution, and the later profoundly important but very seriously defective Enlightenment revolution. It is this situation which calls for the urgent need to bring about a *third* revolution to put right the structural defects we have inherited from the Enlightenment.

The urgent need for this *third* revolution is the subject of this book.

But what, it may be asked, is wrong with the traditional Enlightenment Programme? Almost everything. In order to implement properly the basic Enlightenment idea of learning from scientific progress how to achieve social progress towards a civilized world, it is essential to get the following three things right.

1. The progress-achieving methods of science need to be correctly identified.
2. These methods need to be correctly generalized so that they become fruitfully applicable to any worthwhile, problematic human endeavour, whatever the aims may be, and not just applicable to the one endeavour of acquiring knowledge.
3. The correctly generalized progress-achieving methods then need to be exploited correctly in the great human endeavour of trying to make social progress towards an enlightened, wise world.

Unfortunately, the *philosophes* of the Enlightenment got all three points wrong. And as a result these blunders, undetected and uncorrected, are built into the intellectual-institutional structure of academia as it exists today.

First, the *philosophes* failed to capture correctly the progress-achieving methods of natural science. From D'Alembert in the 18th century to Popper in the 20th, the widely held view, amongst both scientists and philosophers, has been (and continues to be) that science proceeds by assessing theories impartially in the light of evidence, *no permanent assumption being accepted by science about the universe independently of evidence.* But this standard empiricist view is untenable. If taken literally, it would instantly bring science to a standstill. For, given any accepted scientific theory, T, Newtonian theory say, or quantum theory, endlessly many rivals can be concocted which agree with T about observed phenomena but disagree arbitrarily about some unobserved phenomena. Science would be drowned in an ocean of such empirically successful rival theories.

In practice, these rivals are excluded because they are disastrously disunified. Two considerations govern acceptance of theories in science:

empirical success and unity. But in persistently accepting unified theories, to the extent of rejecting disunified rivals that are just as, or even more, empirically successful, science makes a big persistent assumption about the universe. The universe is such that all disunified theories are false. It has some kind of unified dynamic structure. It is physically comprehensible in the sense that explanations for phenomena exist to be discovered.

But this untestable (and thus metaphysical) assumption that the universe is comprehensible is profoundly problematic. Science is obliged to assume, but does not know, that the universe is comprehensible. Much less does it know that the universe is comprehensible in this or that way. A glance at the history of physics reveals that ideas have changed dramatically over time. In the 17th century there was the idea that the universe consists of corpuscles, minute billiard balls, which interact only by contact. This gave way to the idea that the universe consists of point-particles surrounded by rigid, spherically symmetrical fields of force, which in turn gave way to the idea that there is one unified self-interacting field, varying smoothly throughout space and time. Nowadays we have the idea that everything is made up of minute quantum strings embedded in ten or eleven dimensions of space-time. Some kind of assumption along these lines must be made but, given the historical record, and given that any such assumption concerns the ultimate nature of the universe, that of which we are most ignorant, it is only reasonable to conclude that it is almost bound to be false.

The way to overcome this fundamental dilemma inherent in the scientific enterprise is to construe science as making a hierarchy of metaphysical assumptions concerning the comprehensibility and knowability of the universe, these assumptions asserting less and less as one goes up the hierarchy, and thus becoming more and more likely to be true. In this way a framework of relatively insubstantial, unproblematic, fixed assumptions and associated methods is created within which much more substantial and problematic assumptions and associated methods can be changed, and indeed improved, as scientific knowledge improves. Put another way, a framework of relatively unspecific, unproblematic, fixed *aims* and methods is created within which much more specific and problematic aims and methods evolve as scientific knowledge evolves. (A basic aim of science is to discover in what precise way the universe is comprehensible, this aim evolving as assumptions about comprehensibility evolve.) There is positive feedback between improving knowledge, and improving aims-and-methods, improving knowledge-about-how-to-improve-knowledge. This is the nub of scientific rationality, the methodological key to the unprecedented success of science. Science adapts its nature to what it discovers about the nature of the universe.

So much for the first blunder of the Enlightenment.

Second, having failed to identify the methods of science correctly, the *philosophes* naturally failed to generalize these methods properly. They failed to appreciate that the idea of representing the problematic aims (and associated methods) of science in the form of a hierarchy can be generalized and applied fruitfully to other worthwhile enterprises besides science. Many other enterprises have problematic aims; these would benefit from employing a hierarchical methodology, generalized from that of science, thus making it possible to improve aims and methods as the enterprise proceeds. There is the hope that, in this way, some of the astonishing success of science might be exported into other worthwhile human endeavours, with aims quite different from those of science.

Third, and most disastrously of all, the *philosophes* failed completely to try to apply such generalized progress-achieving methods to the immense, and profoundly problematic enterprise of making social progress towards an enlightened, wise world. The aim of such an enterprise is notoriously problematic. For all sorts of reasons, what constitutes a good world, an enlightened, wise or civilized world, attainable and genuinely desirable, must be inherently and permanently problematic. Here, above all, it is essential to employ the generalized version of the hierarchical, progress-achieving methods of science, designed specifically to facilitate progress when basic aims are problematic.

Properly implemented, in short, the Enlightenment idea of learning from scientific progress how to achieve social progress towards an enlightened world would involve developing social inquiry as social *methodology,* or social *philosophy*, not primarily as social *science*. A basic task would be to get into personal and social life, and into other institutions besides that of science – into government, industry, agriculture, commerce, the media, law, education, international relations – hierarchical, progress-achieving methods (designed to improve problematic aims) arrived at by generalizing the methods of science. A basic task for academic inquiry as a whole would be to help humanity learn how to resolve its conflicts and problems of living in more just, cooperatively rational ways than at present. This task would be intellectually more fundamental than the scientific task of acquiring knowledge. Social inquiry would be intellectually more fundamental than physics. Academia would be a kind of people's civil service, doing openly for the public what actual civil services are supposed to do in secret for governments. Academia would have just sufficient power (but no more) to retain its independence from government, industry, the press, public opinion, and other centres of power and influence in the social world. It would seek

to learn from, educate, and argue with the great social world beyond, but would not dictate. Academic thought would be pursued as a specialized, subordinate part of what is really important and fundamental: the thinking that goes on, individually, socially and institutionally, in the social world, guiding individual, social and institutional actions and life. The fundamental intellectual and humanitarian aim of inquiry would be to help humanity acquire wisdom – wisdom being the capacity to realize (apprehend and create) what is of value in life, for oneself and others, wisdom thus including knowledge and technological know-how but much else besides.

In short, if the Enlightenment revolution had been carried through properly, the three steps indicated above being correctly implemented, the outcome would have been a kind of academic inquiry very different from what we have at present.

This difference, over time, would be bound to have a major impact. What we have at present, academic inquiry devoted primarily to acquiring knowledge and technological know-how dissociated from any intellectually more fundamental concern to help us resolve our conflicts and problems of living in more cooperatively rational ways – dissociated, that is, from the pursuit of wisdom – is a recipe for disaster. Scientific knowledge and technological know-how enormously increase our power to *act*. In endless ways, this vast increase in our power to act has been used for the public good – in health, agriculture, transport, communications, and countless other ways. But equally, this enhanced power to act can be used, and has been used, to cause human harm, whether unintentionally, as in environmental damage (at least initially), or intentionally, as in war. It is hardly too much to say that all our current global problems have come about because of the successful scientific pursuit of knowledge and technological know-how dissociated from wisdom. The appalling destructiveness of modern warfare and terrorism, vast inequalities in wealth and standards of living between first and third worlds, rapid population growth, environmental damage – destruction of tropical rain forests, rapid extinction of species, global warming, pollution of sea, earth and air, depletion of finite natural resources – all exist today because of the massively enhanced power to act (of some), made possible by modern science and technology. Nevertheless, science as such is not the problem, but rather science dissociated from the pursuit of wisdom, the result of our failure to put right the structural defects in academic inquiry, inherited from the blunders of the Enlightenment.

Hence my conclusion: we urgently need to bring about a *third* intellectual revolution, one which corrects the blunders of the Enlightenment revolution, so that the basic aim of academia becomes to promote wisdom, and not just

acquire knowledge. Every branch and aspect of academic inquiry needs to change if we are to have the kind of inquiry, both more rational and of greater human value, that we really need.

The task of this book is to make clear what this third revolution, from knowledge to wisdom, amounts to, what its implications are for science, for social inquiry, for the humanities, for education, for the relationship between academia and the social world; to make clear what the reasons are for the revolution, and how urgently the revolution is needed, how big an impact it would have on our capacity to resolve our current immense, intractable global problems, how important it is that humanity should acquire a kind of inquiry rationally designed to help us learn how to create a better world.

When the first edition of this book was published, in the Orwellian year of 1984, I did not expect it to bring about the called-for revolution overnight. At that time the cold war was still in existence, Margaret Thatcher and Ronald Reagan were in power, reactionary policies dominated, the future looked grim, and it must have seemed quixotic in the extreme for someone to urge that we need to bring about a revolution in academic inquiry so that the basic task became to promote wisdom. I did, however, hope that the message of the book would gradually disseminate throughout the academic world, and would gradually come to exert a certain influence on academic policy. I hoped that, at least, the argument of the book would become generally known to historians, philosophers and sociologists of science, to educationalists, and to others professionally concerned with the aims and methods – the philosophy – of inquiry. In particular, I hoped that philosophers would become aware of the argument of the book, in view of its ramified implications for philosophy, indeed for the very nature of the discipline. The blunders inherited from the Enlightenment, that are built into the intellectual-institutional structure of current academic inquiry, are above all *philosophical* blunders, blunders about what the overall aims and methods of inquiry ought to be. It becomes the prime duty and responsibility of *philosophers* to shout out, loud and clear, that we need to bring about an intellectual and institutional revolution in the aims and methods, the whole structure and character, of academic inquiry, so that it takes up its proper task of helping humanity learn how to create a wiser world. This, after all, is even a somewhat traditional task for philosophy: 'philosophy' means 'love of wisdom'.

In all this I was to be bitterly disappointed. When the book first appeared it got some good reviews, and some lousy reviews. It went into paperback twice, and then quietly went out of print in 1992, and seemed to die. And yet

the basic message of the book was just as relevant and urgent as it had ever been. The revolution I argue for had not, and still has not, taken place.

A number of factors were, I believe, responsible for the failure of the book's message to receive greater attention. It is possible that I, as author, did not blow my trumpet hard enough in public places – the newspapers, the radio, and so on. (I became absorbed by the problems of quantum theory; and how exhausting and humiliating it is, in any case, for an unknown person to try to speak in public places. I did however do all that I could to put the basic message across, in an apparently endless sequence of lectures and articles.) Again, there are powerful mechanisms built into academia, discussed in the book, which are designed to preserve the status quo, and marginalize and neutralize a message such as the one of this book, calling for a change in the aims and methods of science, and of academic inquiry. Another factor has to do with the state of philosophy at the time. Philosophers, especially in the USA, were split into two camps: so-called Continental philosophy, and analytic philosophy. The Continentals, suspicious of science and reason, were unlikely to be enthusiastic about my book. The analytic philosophers, still absorbed in a kind of conceptual analysis, could only have been baffled by what they would see as 'the absurd pretensions' of the book. Philosophy, properly conceived, fits into neither conventional mould. It has the task to tackle rationally our most general, fundamental, urgent problems – problems that cut cross all conventional boundaries of academic discipline and speciality. This book does just that; it tackles the fundamental, urgent, and much-neglected problem: What kind of inquiry can best help us create a good world? To an analytic philosopher – obsessed with technical puzzles about concepts and meaning, and seeking to preserve a modest territory secure from the mighty onslaught of science – a book tackling such a broad and fundamental problem, and daring to challenge aspects of the scientific enterprise, must have seemed nonsensically over-ambitious. And the fact that I was not very polite about academic philosophy and philosophers in the first edition cannot have helped!

The neglect of historians, sociologists and philosophers of science has a somewhat different explanation. This has to do with the impact of the so-called 'strong programme' in the Sociology of Science. The strong programme holds that science is inherently social in character. Scientific knowledge is just one belief system amongst others, without privileged access to the nature of reality. There is no such thing as scientific progress, only change of scientific 'belief'. The scientific picture of the world is, in short, a myth, a social construct; it does not deserve to be taken more seriously than any other, rival system of beliefs. I vividly remember attending the annual

conference of the British Society for the Philosophy of Science in Edinburgh many years ago. The strong programme, rather understandably, received a great deal of discussion: it was created by Barry Barnes and David Bloor, both at Edinburgh University. We philosophers of science concluded that it was too silly to be taken seriously. In fact it subsequently became enormously influential. It exercised a major influence over History of Science. Many historians of science came to believe that scientific knowledge is no more than a social construct; they abandoned the serious central problem of the discipline – the problem of understanding how scientific progress has come about – and instead sought to show that scientific change (not progress!) has been determined by social factors. Some philosophers of science sought to point out the fallacies of this movement, but failed to stem the tide. It is not surprising that the first edition of this book, tossed into this battle, was somewhat neglected. Historians and sociologists of science, seeing how seriously the book takes such notions as scientific progress, scientific rationality and scientific knowledge, could not but regard the book as belonging to the enemy. Philosophers of science, on the other hand, seeing that the book is critical of aspects of science, and concerns itself with the human and social implications and aspects of science, also took it for granted that the book came from the enemy camp. Both sides of the dispute, locked in their anti-science/pro-science debate, missed the point.

In order to come to grips with the human and social aspects of science it is essential to consider the *aims* of science, not just the intellectual aims, but social and humanitarian aims as well. And it is essential to consider, not just natural science, but social inquiry and the humanities as well – indeed the whole academic enterprise. Judged from the standpoint purely of its *intellectual* aims, natural science must be judged to have made extraordinary progress in improving knowledge and understanding of the world. But judged from the standpoint of social and humanitarian aims, it is much less certain that science, and academic inquiry more generally, have achieved such extraordinary success. As I have remarked, many of our most serious global problems have come about as a result of population growth, technological development, modern industry and agriculture, all made possible by modern science. The failure of academic inquiry to help humanity learn how to deal wisely with its new, immense powers, acquired from modern science and technology, has everything to do with intellectual blunders, inherited from the Enlightenment, and now built into the institutional/intellectual structure of academic inquiry. The profoundly important task, especially for all those who care about the rationality, the intellectual integrity of inquiry, and its social value, is to free academic inquiry of these Enlightenment blunders.

This involves, first, freeing natural science from an influential, widely upheld, but untenable and irrational *philosophy of science* which, unfortunately, most philosophers of science take for granted, in one or other form, as the *sine qua non* of scientific rationality. It also involves transforming social inquiry so that it becomes social methodology or social philosophy rather than social *science*. Unfortunately, sociologists and historians of science, influenced by the strong programme, presuppose, and base their work on, just the kind of conception of social science that needs to be rejected. As I explain in chapter 5, one incidental outcome of the revolution I argue for in this book would be that the current deep division between Sociology and Philosophy of Science would entirely disappear: these two disciplines, still at loggerheads with one another, would become one and the same discipline. The current dispute between the Sociology and Philosophy of Science is a symptom of the deep malaise from which the whole of academic inquiry suffers, in seeking knowledge rather than trying to seek and promote wisdom by cooperatively rational means.

My hope, of course, is that those who attack, and those who defend, scientific rationality will both come to realize that what is being fought over is not rationality, but a characteristic kind of *irrationality* masquerading as rationality, and both parties will drop their current rather sterile dispute and join with me in seeking to develop a *more* rational, more objective kind of science, and academic inquiry generally, the outcome being a kind of academic inquiry that is of greater human value.

One consequence of the neglect of the first edition of this book by those concerned professionally with studying science has been that the central ideas and arguments of the book have not, during the past twenty years, filtered into the literature.

Some ideas I came up with during the course of developing the central argument of the book have, it is true, subsequently been developed independently by others. Thus I argued that emotion has a fundamental and rational role to play in inquiry devoted to the pursuit of wisdom; this anticipates, to some extent, subsequent work, by some neuroscientists, psychologists and philosophers, on the fundamental role that emotion plays in cognition, and on the vital role emotion can have in guiding us towards that which is of value in life. Again, the account I gave, in chapter ten, of what I call 'the generalized Darwinian research programme' anticipates, to some extent, a great deal of subsequent work on implications of Darwin's theory for the social sciences. Yet again, I argued that values play a fundamental role in science, so much so that recognition of this fact by the scientific community could only help enhance the objectivity, rationality, and

human value of science: some of these points are much more widely acknowledged today than they were in 1984. In chapter 10 I argued that what is of value is an inherent, intrinsic, aspect of human existence, of our human world; this anticipated subsequent discussion of 'value-realism' in the philosophical literature. During the course of developing the central argument of the book, and discussing objections to it, I developed an account of the evolution of consciousness, and of the fundamental role played by imagination in human consciousness; these ideas have been developed subsequently by others, especially since consciousness became a respectable topic to study, some time in the 1990's. The version of quantum theory, sketched in chapter 10 anticipates, to some extent, contributions to quantum theory made subsequently by others. I cite these examples of anticipations in part in an attempt to indicate what I judge to be the extraordinary potential intellectual *fruitfulness* of the conception of inquiry I argue for in this book, inquiry devoted to the pursuit of wisdom.

But despite subsequent intellectual developments such as these, anticipated to some extent by the first edition of this book, the central ideas and arguments of the book have not appeared elsewhere (except in my own subsequent publications) during the intervening twenty years. The passage of time has not in any way rendered these ideas and arguments out of date. They remain as relevant today as they ever were. In preparing this second edition I have not had to change, in any significant way, the *intellectual content* of the book. In the main I have confined myself to changing references to topical events, such as the cold war, the nuclear balance of terror, the Soviet Union and the policies of Margaret Thatcher. I have also brought references to books and articles on relevant topics up to date. And I have added occasional sentences here and there throughout the book designed to clarify ideas and arguments.

There is, nevertheless, a considerable amount of new material in this second edition. Chapter 6 has six new sections comparing aspects of academia around 2003 with academia twenty years earlier, in 1983. Chapters 12, 13 and 14 are entirely new.

In chapter 12 I say something about deeds and developments, in and out of academia, which are relevant to the thesis of the book, or which may be regarded as attempts to do what I argue needs so urgently to be done: put what I call *the philosophy of wisdom* into practice in schools and universities, in personal and social life.

In chapter 13 I respond to my critics. I reply in some detail to criticisms directed at the first edition of the book.

Chapter 14 discusses contributions I have made to the conception of natural science I defend in this book (which I call *aim-oriented empiricism)* after publication of the first edition in 1984. I also show how this conception of science can resolve philosophical problems about science, discussed in chapter 9, which the current, widely accepted, orthodox conception (which I call *standard empiricism*), cannot solve. There are, in particular, three fundamental problems in the philosophy of science which aim-oriented empiricism solves – or so I argue. First, there is the problem of verisimilitude: Granted that physics moves from one false theory to another, what does it mean to say that physics makes progress? Second, there is the problem of simplicity or unity: What is the simplicity, unity or explanatory capacity of a theory, given that any theory can be formulated in many ways, some simple, some horrendously complex? And third, there is the problem of induction: What methods are employed by science in deciding what theories to accept, and what grounds are there for holding that theories, so accepted, constitute knowledge? These three problems are solved within the framework of aim-oriented empiricism, and the solutions are such that it is clear the problems cannot be solved granted standard empiricism. This constitutes powerful grounds indeed for rejecting standard empiricism and accepting aim-oriented empiricism in its stead.

The object of this book is to make a contribution towards changing the overall aims and methods, the intellectual and institutional structure, of academic inquiry. But the book also has a more direct and personal message: it seeks to indicate a new way of thinking about ourselves in the world, a new way of seeing, a new vision. This gives absolute priority to the miracle of our existence in this strange universe, the supreme value of conscious life, and sentient life more generally, our fundamental problem being the problem of realizing what is of value in life as we live. The scientific quest for knowledge and understanding, the technological quest for solutions to practical problems, are but aspects of the central and fundamental quest: to see, to experience, to enjoy what is of supreme value in existence, whatever this may be. Impersonal, academic inquiry, properly organized and constituted, is there to aid what really matters, the searching, the explorations, that we individuals engage in as we live, in seeking to apprehend, to experience, to participate in, what is of value, potentially and actually, in existence. *The philosophy of wisdom* is not just a conception of inquiry; it is also a way of life.

Chapter One
Human Suffering and the Need for a Comprehensive Intellectual Revolution

Our planet earth carries all too heavy a burden of killing, torture, enslavement, poverty, suffering, peril and death. It is estimated that over nine million people die each year from hunger and poverty (World Health Report 2000). And yet it seems we have the capacity to produce enough food for everyone to get enough to eat, given a more just distribution of land and food, more just global trading arrangements, and less wasteful priorities of food production in the developed world. Life expectancy in the developed world is seventy-seven years or more; in the poorer regions of the underdeveloped world it is a mere forty-five years or lower. In the developed world, on average, fewer than six children out of one thousand die during the first year of life; in the poorer regions of the underdeveloped world one hundred and fifty out of a thousand die during their first year. Ten and a half million children under the age of five die each year from preventable causes. Somewhere between forty and fifty-five million people died as a result of the last world war; and a larger number of people have died in wars since then.[1] Dictatorships are commonplace amongst the nations, the criminally insane even seizing and holding power, dictatorial power being maintained by means of terror, arbitrary imprisonment, torture and execution – and such dictatorships have even been supported by democracies. There are the threats posed by rapid population growth, by future scarcities of water, food and oil, and above all, by global warming. And there is the havoc and peril caused by the spread of armaments all over the world, conventional, chemical, biological and nuclear, together with terrorism, and the wars that it provokes (as in Afghanistan and Iraq).[2]

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[1] These statistics of unnecessary suffering and death have been gleaned from World Health Report 2000 (WHO), United Nations Statistical Yearbook (2002), the United Nations Human Development Report (2002), Elliot (1972), Clodfelter (2002), African Development Report (2002) and Veneman (2006).

[2] The best overall account, to my knowledge, of our human, global problems, and of our present incapacity to respond, sanely and rationally to them, is given by Higgins (1978). Higgins' 'seventh enemy' is the inertia of our institutions, which renders them, and us, incapable of responding to the crisis. The central thesis of the present book is that the intellectual/institutional inertia of the academic enterprise is, in a major way, responsible

Danger, suffering and death are inevitable aspects of life, imposed on us as a result of our living in, and being a part of, the natural world. The danger, suffering and death just indicated, experienced by so many, are not however caused solely by natural phenomena: they are our own creation, our own responsibility, caused by our own actions, or by our failure to act.

The problem to be tackled in this book can be put like this. What kind of rational inquiry gives us the best hope of helping us progressively to resolve our most urgent problems of living – such as those indicated above – thus helping us to develop a more humane, a more just, a happier, a saner and more cooperative world? What kind of science, technology, scholarship and education is best designed to help us promote human welfare, realize that which is genuinely of value in life? What ought to be the basic intellectual aims and methods of such an inquiry, and how ought these to be related to our personal and social aims and methods in life?

Insofar as academic inquiry does try to help promote human welfare, it does so, overwhelmingly, at present, by seeking to improve *knowledge* of various aspects of the world. It does this in the hope that new knowledge, thus obtained, will be used to help resolve social problems in a humane and just fashion. The view that rational inquiry ought to help enhance the quality of human life by, in the first instance, improving knowledge is, one might say, the official basic creed of the whole scientific/academic enterprise. The view can be traced back at least to Francis Bacon in the seventeenth century, and perhaps back to the ancient Greeks. It has been almost unthinkingly taken for granted by almost everyone associated with the development of science, scholarship, universities and education in the western world, and elsewhere. And as a result the view is now firmly built into the whole intellectual-institutional structure of the scientific/academic enterprise.

The central claim of this book can now be put like this. Granted that inquiry has as its basic aim to help enhance the quality of human life it is actually profoundly and damagingly *irrational, unrigorous,* for inquiry to give intellectual priority to the task of improving knowledge. Rather, intellectual priority needs to be given to the dual tasks of articulating our problems of living, and proposing and criticizing possible solutions, namely possible

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for the general inertia of institutions, social and international relations and arrangements. Other works also to be consulted in order to get some sort of picture of human world-wide problems are Dubos and Ward (1972); Ward (1979); Meadows et al. (1974); Scientific American, (1980); P. Harrison (1979); Foley (1981); Maddox (1972); SIPRI (1979); Alien (1980); Eckholm (1982); The Committee . . . (1981); Goodwin (1982); Schell (1982). For more recent literature, see especially Stiglitz (2002) and Mason (2006).

human actions. Problems of knowledge and understanding need to be tackled as rationally subordinate to intellectually more fundamental problems of living. In order to develop better solutions to the appalling human problems indicated above, it is not primarily new knowledge that we need; rather what we primarily need is to *act* in new, appropriate ways. The fundamental intellectual task of a kind of inquiry that is devoted, in a genuinely rational and rigorous way, to helping us improve the quality of human life, must be to create and make available a rich store of vividly imagined and severely criticized possible actions, so that our capacity to act intelligently and humanely in reality is thereby enhanced. In order to improve our capacity to resolve the appalling problems confronting humanity today, we need, as a matter of urgency, to develop a new more rigorous kind of inquiry, in many ways radically different from what we have at present, having, as its basic aim, to improve not knowledge only, but rather *wisdom*.

There is thus, I claim, a major intellectual disaster at the heart of western science, technology, scholarship and education – at the heart of western thought; and this long-standing intellectual disaster has much to do with the human disasters of our age, our incapacity to tackle more humanely and successfully our present world-wide problems. In order to develop a saner, happier, more just and humane world it is certainly not a *sufficient condition* that we have an influential tradition of rational inquiry devoted to helping us achieve such ends. It is, however, I shall argue, a *necessary condition*. In the absence of such a tradition of thought, rationally devoted to helping us solve our problems of living, we are not likely to resolve these problems very successfully in the real world. It is this which makes it a matter of such profound intellectual, moral and social urgency, for all those in any way concerned with the academic enterprise, to develop a kind of inquiry more rationally devoted to helping us resolve our problems of living than that which we have at present.

In this book, then, I argue for the need to put into practice a profound and *comprehensive* intellectual revolution affecting to a greater or lesser extent all branches of science, technology, scholarship and education. The intellectual revolution that I advocate differs however from the now familiar kind of scientific revolution so brilliantly described by Kuhn (1962). For I do not here advocate a change in what we take to be knowledge about some aspect of the world, a change of theory or 'paradigm'. Rather, what I advocate is a radical change – a radical evolution – in the overall, fundamental aims and methods of inquiry. At present we have a kind of academic inquiry that has, as its basic intellectual aim, to improve *knowledge*. This needs to be transformed, I shall argue, into a kind of rational inquiry

that has, as its basic intellectual aim, to improve wisdom. This new kind of inquiry is, I shall argue, potentially both more rational (more intellectually rigorous) and of greater human value than what we have at present, inquiry restricted, as far as its intellectual aims are concerned, to the improvement of knowledge.

I shall develop the argument by articulating, comparing and contrasting two rival views about what the basic intellectual aims and methods of inquiry ought to be. I shall call these two views *'the philosophy of knowledge'* and *'the philosophy of wisdom'*. In arguing for the need to put into practice the philosophy of wisdom as opposed to what we have at present, inquiry pursued in accordance with the philosophy of knowledge, I shall be arguing for the need to transform our whole conception of the social sciences and humanities: I shall be arguing for the need to develop a new relationship between the social sciences and humanities on the one hand, and the natural sciences on the other hand: above all, I shall be arguing for the need to establish a new relationship between inquiry as a whole and human life, our personal and social worlds. The revolution that I seek to advocate has widespread intellectual repercussions for science and scholarship; it also has repercussions for the whole institutional structure of the academic enterprise, its place and role in human life. The revolution – or evolution – of basic aims and methods for inquiry that I wish to advocate thus combines intellectual and institutional or social changes.

So far, what this book sets out to accomplish has been characterized so briefly, that misunderstandings are more or less inevitable.

Thus, to begin with, it may be thought strange that I should refer to social and political problems that confront us globally, and in the third world, and yet say nothing about problems of the industrially-advanced first world. Is there not poverty, injustice, only partially-realized democracy or even totalitarianism, much unnecessary human suffering and waste and death here too, as well as in the third world? Should not a kind of rational inquiry that is devoted to helping us realize what is of value in life help us to develop more just, cooperative, fruitful ways of life in Europe, in Russia and the USA as well as in Africa, Asia and South America? My answer to these questions is: yes, of course.[3] Above, I merely sought to indicate what seem to me to be the most urgent, the most desperate problems confronting people in the world

[3] On the basis of statistics concerning such things as the incidence of suicide, madness, alcoholism and war, Fromm comes to the tentative conclusion that the sanity of industrially advanced societies must be called into question. (See Fromm 1963.) For a survey of poverty in Britain, see Townsend (1979).

today. To this I would add, however, that in seeking to change things for the better in the first world, we ought always to take into account the far more severe problems and plight of people in the third world. A good motive for attempting to bring about social and political changes in the industrially-advanced world is indeed just to help the poor of the third world by putting a stop to first-world violence and exploitation in the third world. The record of the last fifty years or so is not too good. During the cold war years, the first world east-west conflict was fought out primarily in the third world – in Korea, Vietnam, Cambodia, Africa, the Middle East, and South and Central America. It was the defenceless poor of the third world who suffered the worst consequences of first-world conflict – either through war, or through USA- or USSR-backing for corrupt, puppet, totalitarian regimes, in Latin America, in Africa, in Asia. More recently, the IMF and World Bank have inflicted economically damaging policies on poor countries as the price for aid, and have intervened disastrously in countries of the former Soviet Block as they struggled to make the transition to the free market (Stiglitz, 2002). And, at the time of writing, the USA has developed a new policy of engaging in pre-emptive war against poor countries perceived to pose a threat, as exemplified by the Afghanistan war of 2002, and the Iraq war of 2003. Those of us who live in the first world need to strive to put our own house in order in part because of the havoc we help to cause at present elsewhere.

In the second place, puzzlement may arise in connection with my apparently exclusive concern with large-scale social, political, global problems. I declare that we need a new kind of rational inquiry that helps us to realize what is of value – and yet I seem to ignore where it is that all that is really of value in life is to be found. For is it not the case that, for each one of us, what is of value has to do with the particularities of our own personal lives, our experiences, feelings, desires, achievements: what we do and share with those we know and love? Is not salvation always personal and particular, and never to be found in large-scale schemes for the resolution of problems that confront millions of people together? Once again, with this I agree. As the argument unfolds I shall develop and repeatedly emphasize the point that inquiry, if pursued in a genuinely rational fashion (in accordance with the philosophy of wisdom) must be recognized to be fundamentally personal and interpersonal in character, an aspect of life, our own seeking after the realization of what is of value to us personally. But I shall also emphasize that inquiry must have a social, institutional and traditional character – even an impersonal aspect – if it is to perform its proper personal function of helping us to get in touch with what is of value in the world, in each other, and in ourselves. In particular, a vital task for inquiry – for education – is to help us

to take up our proper share of adult responsibility for our common world. Our most passionate desires, joys and concerns no doubt quite properly have to do with particular people and things of our own personal life: but we ought also to have some care and concern for all those millions and millions of strangers known to us only by hearsay, who also live with us on earth. Only when it is a commonplace for individuals everywhere to have some measure of informed concern for their fellow citizens of the earth, will there be an end to the nightmare dangers and disasters that now beset so many millions of people as a result of actions and attitudes of other people. Perhaps the most important task of the kind of inquiry and education to be characterized and advocated in this book (inquiry pursued in accordance with the philosophy of wisdom) is just to help each one of us individually to inform and enrich our abstract knowledge of millions of strangers with something of what we feel for those few people we are acquainted with, and those even fewer we love – so that we become capable of recognizing the humanity of millions of distant strangers too. Inquiry as at present constituted (pursued in accordance with the philosophy of knowledge) not only fails to help us connect up personal and public realms in this way. Even worse, it actually intensifies the gulf between personal and public worlds, in that it demands, as we shall see, that a decisive gulf be maintained between personal feelings and values on the one hand, and public, objective facts and knowledge on the other hand.

In the third place puzzlement may arise over my apparently exclusive concern with the human, practical or social use and value of science and scholarship. For does not inquiry have an intrinsic intellectual value, quite apart from any practical applications it may lead to? Is not inquiry something worth pursuing for its own sake, a vital part of our culture and civilization in its own right, like poetry and music? Once again, with all this I agree. A major part of the argument of this book is just that the philosophy of knowledge fails to do justice to the intrinsic intellectual value of inquiry, so that pursuing inquiry in accordance with its edicts does much to obscure and sabotage the potential intellectual value of science and scholarship. In order fully to develop and make available the intellectual riches inherent in diverse aspects of science and scholarship it is essential to put the philosophy of wisdom into practice. I shall argue that inquiry pursued for its own sake is, at its best, an aspect of love, our shared endeavour to see, to apprehend that which deserves love, in the world and in ourselves. This is true even of a subject as apparently remote from love as theoretical physics. At its intellectual best, theoretical physics is an expression of our shared love for that aspect of the world which has to do with its underlying structure or

architecture. Physics is of intrinsic intellectual value to the extent that it does enable us to see, to apprehend, to love, something of this architectural grandeur inherent in nature. The greatest exponent of physics pursued in this kind of way is perhaps Einstein. It is to this conception and experience of physics that Einstein alluded when he wrote in a letter to a friend: 'You have given me great joy with the little book about Faraday. This man loved mysterious Nature as a lover loves his distant beloved. In his day there did not yet exist the dull specialization that stares with self-conceit through hornrimmed glasses and destroys poetry' (Dukas and Hoffmann, 1979, p. 42). The philosophy of knowledge destroys the poetry of physics not just because it permits dull specialization and self-conceit to flourish. More fundamentally, it does so because it demands that a gulf be maintained between, on the one hand, the intellectual domain of science and knowledge, having to do with objective fact and truth and, on the other hand, the personal domain of 'subjective' experiences, feelings and values, having to do with such things as joy, fear and love. The result is that it becomes nonsensical to speak of physics as a shared act of love for our world. In order to become a lover of the universe, with Kepler. Faraday, Einstein and others, we need to bring together shared concern for objective, impersonal truth and reality, and our own personal instinctive feelings and imaginings. As we shall see, it is just this which the intellectual standards of the philosophy of wisdom encourage and demand. Upholders of the philosophy of knowledge may, or may not, value personal love of 'mysterious Nature': in either case, for them any such personal attitude cannot have anything to do with the intellectual integrity and success of physics. From the standpoint of the philosophy of wisdom, as we shall see, this division between the personal and the intellectual ought not to be attempted. The intellectual integrity and value of physics itself is intimately associated with its success in expressing and promoting an attitude of love for Nature. Thus Einstein's legitimate intellectual objections to the ultimate acceptability of orthodox quantum theory had everything to do with the high intellectual/personal aspirations that he had for physics – orthodox quantum theory being capable only of claiming 'the interest of shopkeepers and engineers', in that it merely correctly predicts the results of experiments and does not help reveal to us 'the Old One', the architectural grandeur of the universe – orthodox quantum theory thus being, for Einstein, intellectually 'a wretched bungle' (Przibram, 1967, p. 39).

From the standpoint of the philosophy of wisdom, if even something as ostensibly cold and impersonal as physics ought to be pursued as an aspect of love, then most certainly the biological sciences, the social sciences and humanities, and the technological sciences such as engineering and medicine,

ought so to be pursued. It is just this which the philosophy of knowledge denies and obscures – and thus, on being put into practice, sabotages. As a result, both the intrinsic intellectual value of inquiry, and the value of our lives, our capacity to love aspects of our world, are undermined.

Quite generally, I wish to argue, our task, in engaging in rational inquiry, is to see, participate in, and help to grow what is significant and of value in existence in the cosmos, questioningly, enjoyably if possible, and above all lovingly.[4] What I mean by this will I hope become clearer as the argument unfolds.

I have now a few remarks to make about the way I expound the argument in the rest of this book.

In chapter 2 I expound the philosophy of knowledge. In chapter 3 I state the basic objection to the doctrine: inquiry pursued in accordance with the philosophy of knowledge fails to satisfy the most elementary requirements for rationality, and as a result must have damaging consequences for almost every aspect of life. In chapter 4 I give a first exposition of the philosophy of wisdom in terms of rational problem-solving. In chapter 5 I expound a somewhat improved version of the philosophy of wisdom formulated in terms of a somewhat improved notion of 'aim-oriented' rationality.

At this point it may be wondered whether it really is the case that it is the blatantly and damagingly irrational philosophy of knowledge, as opposed to the more rigorous and valuable philosophy of wisdom, that at present predominates over the academic enterprise. In chapter 6 I give grounds for holding that the philosophy of knowledge does indeed at present prevail in scientific and academic practice. We see, first, what I said in the first edition about various aspects of academia around the year 1983; I then report on the state of affairs around 2003 for the present edition. Then, in chapter 7, I assess the basic argument of the book.

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[4] In my view it is important to take seriously Popper's point that when it comes to social and political planning, priority should be given to the piecemeal removal of specific cases of avoidable human suffering and injustice, as opposed to the attempt to create an ideal society by means of holistic or Utopian planning. (See Popper, 1961, section 21; and 1969, especially vol. 1, chapter 9.) It is for this reason that I begin with, and lay the greatest stress on, the endeavour to alleviate avoidable human suffering, danger, injustice and death, as opposed to the Utopian endeavour to create 'a more loving world'. However, as we proceed, I shall offer some substantial criticisms of Popper's anti-Utopianism, in chapter 8.

In chapter 8 I let the opposition speak. I expound a number of arguments criticizing the philosophy of wisdom and defending the philosophy of knowledge, and I try to rebut these arguments.

In chapter 9 I demolish the philosophy of knowledge in that department of inquiry where it would seem to be the most defensible – physics pursued for its own sake. I argue that the pursuit of knowledge in the physical sciences cannot be dissociated from the pursuit of understanding, from the problematic presupposition that the universe is, in some way, comprehensible. In chapter 10 1 tackle the fundamental problem: how can there be life of value granted that the world really is comprehensible more or less as modern science tells us it is?

In chapter 11 1 say something about how I myself came to hold the views advocated in this book, and something about the ideas, work and efforts of a number of people and groups during the 1960s and '70s, mainly in Britain, both inside and outside universities, all of which can be interpreted to be a part of a general movement away from the philosophy of knowledge towards the philosophy of wisdom. The revolution that I advocate is already under way!

For this second edition of the book (as I indicated in the introduction) I have added the following chapters. Chapter 12 sets out to discover whether changes that have taken place in academia since 1984 can be regarded as adding up to a movement away from the philosophy of knowledge and towards the philosophy of wisdom. A rather mixed story emerges – although we shall see that some changes have taken place which can be regarded as steps towards the philosophy of wisdom. In chapter 13 I reply to criticisms of the first edition of the book. Finally, in chapter 14 I give an account of improvements I have made, since 1984, to the philosophy of physics expounded and defended in this book. This view, which I call 'aim-oriented empiricism', holds that physics makes the substantial, highly problematic and largely implicit assumption that the universe is (more or less) physically comprehensible. Rigour demands that this implicit assumption be made explicit so that it can be criticized and, we may hope, improved. In chapter 14 I show how this view – a central component of the overall argument of this book – is able to solve outstanding fundamental problems in the philosophy of science which current orthodox views cannot solve.

Chapter Two
The Philosophy of Knowledge

The philosophy of knowledge can be summarized as follows. The proper aim for rational inquiry is to acquire knowledge about the world, objective knowledge of truth. Ultimately, no doubt, knowledge is sought as a means to the end of achieving that which is humanly desirable and of value. At the most fundamental level of all, in other words, the aim of rational inquiry may well be to help promote social progress, human welfare and enlightenment. In order to achieve these fundamental human, social aims, however, it is essential that rational inquiry devotes itself, in the first instance, to achieving the purely *intellectual* aim of acquiring objective knowledge of truth. Only by dissociating itself decisively from the goals, values and beliefs of common social life, so that claims to objective knowledge can be subjected to scrupulously *rational* assessment, can inquiry accumulate genuine knowledge, thus ultimately being of benefit to humanity. Rational inquiry must, as it were, ignore human need in order to help fulfill such need. Truth, not that which is humanly desirable, must be the central intellectual concern of rational inquiry.

Aspects of this basic idea can be traced back to the ancient Greeks, to the Presocratic philosophers, to Plato, Aristotle, Euclid, Archimedes. It is however with the rise of modern science in the sixteenth and seventeenth centuries that the philosophy of knowledge really comes into its own – with the work of Copernicus, Kepler, Galileo, Descartes, Huygens, Hooke, Boyle, Leibniz, and above all with the work of Newton, set out in his *Principia* of 1687. More than anything else, it was the quite unprecedented predictive and explanatory success of Newtonian theory, drawing together and improving on what had gone before, which appeared to demonstrate so conclusively that new, genuine, valuable knowledge about the world can indeed be achieved. A new, assured method for acquiring knowledge had, it seemed, been discovered. The philosophy of knowledge is first and foremost a philosophy of science (here called *standard empiricism*) which, when generalized, becomes a philosophy of all of inquiry. The philosophy of knowledge owes its prestige and influence to being closely associated with the great intellectual success of natural science – in the first instance, Newtonian science.[1]

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[1] 'For a good account of the deification of Newton in the eighteenth century – associated both with actual scientific research and with popular attitudes towards science – see Gay (1973, vol. 2, ch. 3 and ch. 4, section 2).

Francis Bacon, somewhat earlier than Newton, was perhaps the first person to give a clear, powerful and influential expression of the basic ideas of the philosophy of knowledge in something like its modern form. In his writings Bacon stressed the following cardinal points. As a result of acquiring genuine knowledge of Nature, we can enormously enhance our power to act, to do good, to transform the human condition immeasurably for the better. In order to achieve such radical human, social progress, progress in knowledge, in science, is essential. This is to be achieved by means of organized inquiry which bases its results firmly on the ground of observation and experiment, the speculations, prejudices and myths of philosophers and of ordinary social life, of 'common-sense', being firmly ignored (or at the very least not being accepted as true on trust).

There can be no doubt that these ideas, expressed so clearly by Bacon, came to exert a powerful influence over the rise and subsequent development of modern science. Many scientists and thinkers, over the centuries, have been inspired by the Baconian idea that knowledge is of great human, social value. The idea that organized inquiry is needed in order that knowledge may be progressively acquired inspired the founding of the Royal Society, the first official scientific society, having, as it did, royal patronage, and being to some extent a model for subsequent scientific societies. Finally the idea that knowledge is to be acquired by ignoring speculations of philosophers, and instead arriving at results based on observation and experiment, has dominated all subsequent science. The details of Bacon's own methodology for science may be incorrect, and may be ignored by good science, as Popper, for one has stressed. Even Popper, however, a vehement anti-Baconian if ever there was one, nevertheless advocates, as the central tenet of his philosophy of science, a thesis that is central to Bacon's empiricism: *a priori* knowledge about the world being impossible, all scientific claims to knowledge must be assessed solely with respect to experimental success or failure (the simplicity or unity of theories playing a role as well).[2] This Popperian version of Bacon's empiricism exercises a

[2] A basic purpose of Popper's philosophy of science is to defend the central tenet of what I call *standard empiricism* – in turn the central component of the philosophy of knowledge – namely *'the principle of empiricism* which asserts that in science, only observation and experiment may decide upon the *acceptance or rejection* of scientific statements, including laws and theories' (Popper, 1963, p. 54). Actually, as we shall see below, standard empiricism is a somewhat broader doctrine than this. It allows that considerations of simplicity, unity or explanatoriness may legitimately influence what theories are accepted and rejected in addition to empirical considerations: the crucial point is that no

profound influence over modern science, and is accepted as valid by most modern scientists.[3] Newton has been generally interpreted as having defended a version of this standard empiricist doctrine in his *Principia*, and his massive authority has given credence to the doctrine.[4]

Another major historical source of the philosophy of knowledge is Descartes' enormously influential dualistic theory of mind and matter. Cartesian dualism divides up reality into two sharply distinct worlds: on the one hand the objective world of fact, matter, physical reality; on the other hand the subjective world of mind, consciousness, personal experience, value. Once this view is accepted (as it was at one time by most scientists in one form or another), it becomes natural to suppose that rational inquiry, science, will reflect in its overall character the sharp split between fact and value, objective reality and subjective feelings and desires, which is asserted to exist in reality by Cartesian dualism. The intellectual standards of the philosophy of knowledge do indeed reflect the Cartesian dualistic view of the world, in this way.

In one respect Descartes was somewhat at odds with the philosophy of knowledge in that he held that reason as well as experience is a source of knowledge. This *aprioristic* methodology was influential for a time, on the

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substantial thesis about the world must be permanently accepted as a part of scientific knowledge independently of empirical considerations.

3 That ultimately only observation and experiment can decide the fate of laws and theories in science is a point constantly affirmed by scientists in textbooks, popular lectures and elsewhere, often, as in the case of such figures as Medawar, Bondi, Eccles and others, Popper's insistence on the key role that empirical refutation has for science being enthusiastically endorsed. Most scientists acknowledge, however, that consideration of simplicity or unity play an important and legitimate role in deciding what theories are to be accepted in addition to empirical considerations.

4 In the *Principia* (1962, first published 1687), vol. 2, pp. 398-400, Newton formulates his famous four 'rules of reasoning in philosophy', encapsulating his conception of what we would now call 'scientific method'. His fourth rule states: 'In experimental philosophy we are to look upon propositions inferred by general induction from phenomena as accurate or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur by which they may either be made more accurate, or liable to exceptions'. This certainly gives the impression that Newton is defending an inductivist version of standard empiricism; it has generally been interpreted in this way. Newton's first three rules of reasoning, however, specify non-empirical, *a priori* requirements that acceptable theories must meet, and are formulated in such a way that it is clear that these rules make substantial assumptions about the nature of the universe. This violates standard empiricism, and is much closer to the 'aim-oriented empiricist' view I expound and defend in chs. 5 and 9 below.

Continent at least if not in England. But with the eventual downfall of Cartesian physics and the triumph of Newtonian physics, Descartes' *a prioristic* methodology seemed discredited. *A priorism* lingered on for a time, most notably perhaps, in a modified form, in the thought of Kant. The view had little influence, however, over the development of science. Not only did the position seem to be intellectually indefensible – despite all of Kant's obscure ingenuity. In addition the best candidates for *a priori* knowledge – Euclidean geometry and the principles of Newtonian mechanics – were successively dethroned from this exalted position by developments in mathematics and physics: by the development of consistent non-Euclidean geometries in the nineteenth century, and by the development and empirical success of Einstein's special and general theories of relativity in the twentieth century.

By the eighteenth century, Bacon's basic ideas had come to seem, to the thinkers of the Enlightenment, almost commonplace. In essence, Enlightenment thinkers made one vital addition to Bacon's version of the philosophy of knowledge: they stressed the importance of acquiring knowledge of man, of society, of history, in addition to acquiring knowledge of Nature, for achieving social progress, human enlightenment. Thus Vico, Montesquieu, Helvetius, d'Holbach, Voltaire, Diderot, Gibbon and Hume were all concerned, in various ways, to do for man, culture, society, or history, what Newton had done for Nature: to put 'moral philosophy', the study of man, on as sound a footing as Newton had put natural philosophy, the study of Nature: see, for example, Gay (1973).

In the universities in Europe during medieval times, Christianity undoubtedly constituted the dominant philosophy. The basic aims and methods, assumptions and values, of almost all intellectual work were set by Christian doctrine. Throughout the eighteenth and nineteenth centuries, the Bacon-Newton-Enlightenment version of the philosophy of knowledge came more and more to predominate, until by the mid-twentieth century it had come to reign supreme throughout almost all scientific, academic thought and work. The great industrial, technological and medical progress, achieved in the so-called western world at least during the nineteenth and twentieth centuries, intimately associated with scientific progress, seemed to confirm entirely Bacon's vision. Many writers of course continued to stress the importance of other factors for human progress besides progress in knowledge: factors such as faith, morality, imagination, tradition, justice, political liberty, democracy, legal reform, economic progress, industrial development. Some expressed suspicion of the idea that real human progress could be achieved through progress in science and technology. Few however

doubted that knowledge is at least necessary, if by no means sufficient, for human progress. No one seems to have challenged the basic tenet of the philosophy of knowledge, namely that rational inquiry should be devoted in the first instance to the achievement of knowledge.

Today the Bacon-Newton-Enlightenment philosophy of knowledge, suitably qualified, is built into our socio-cultural order. It exercises a profound influence over most scientific, academic work and thought, and the way this is related to the rest of society and culture. Versions of the doctrine, under various labels, have been assailed by Romanticism, by postmodernism, by sociologists of science, by some historians of science, and by anti-rationalists and those espousing anti-science views. Despite these attacks, the doctrine continues to dominate academic inquiry, as we shall see in chapter 6. In one absolutely crucial respect, all these standard objections to the philosophy of knowledge have entirely misunderstood what is wrong with the doctrine, and what needs to be done to transform it into something of far greater intellectual and human value. What is wrong is not, as Romantic critics tend to maintain, that the philosophy of knowledge gives too great an emphasis to reason; quite the contrary, as we shall see, it is the lack of reason that is the problem.

I shall now formulate in a little more detail, in the following nineteen points, that version of the philosophy of knowledge, inherited from Bacon, Newton and the Enlightenment, which has come to be embedded, I claim, in the whole intellectual/institutional structure of modern scientific, academic work and thought. (What follows, let me emphasize, in order to avoid possible misunderstandings, is my best attempt at a sympathetic exposition of the doctrine that I shall subsequently criticize and reject as irrational, and as intellectually and humanly damaging.)

1. Ideally, the basic social or humanitarian aim of inquiry[5] is to produce that which is of human, social value, inquiry thus contributing to human welfare, to human progress, to the quality of human life. In this respect inquiry does

[5] Inquiry is presumed here to be *rational, organized inquiry* – inquiry having something of a public, social or institutional character. We cannot, however, identify inquiry with *science,* since this leaves out of account rational branches of inquiry devoted to the acquisition of knowledge – such as historical research perhaps – which cannot be held to be scientific. Nor can we identify inquiry with *academic* inquiry – since this leaves out of account scientific and technological research, conducted in research institutions or in connection with industry or defence, pursued in accordance with the edicts of the philosophy of knowledge, and yet 'non-academic'. Roughly speaking, the philosophy of knowledge assumes that genuinely rational inquiry is the union of scientific and technological research on the one hand, academic research on the other hand.

not differ from other socially valuable human enterprises, such as theatre, medicine, literature, art, law, industry, education, democratic government. All these enterprises may be held to have the common aim of producing that which is of human value, thus contributing to the quality of human life.

2. The specific intellectual aim of inquiry is to produce objective knowledge of truth – and also to provide technological know-how, explanations and understanding. In other words, inquiry contributes to the common human aim of producing that which is of human value by, in the first instance, realizing the distinctively academic or intellectual aim of producing reliable, objective, factual knowledge, insofar as this can be achieved.[6]

3. Mathematics, statistics and logic are concerned to improve knowledge of formal, *a priori* or analytic truth. The physical sciences are concerned to improve knowledge about diverse aspects of the physical universe. The biological sciences are concerned to improve knowledge about life. The social sciences and humanities are concerned to improve knowledge about diverse social and cultural aspects of human life. The technological sciences are concerned to improve knowledge needed in order to realize diverse, valuable, practical social goals.

4. In improving our knowledge and understanding of truth, inquiry contributes to the quality of human life in two rather different ways.

First, the contribution is direct. The search for truth is of intrinsic human value, of value when engaged in for its own sake. In pursuing pure research, at either first or second hand, and in observing the scrupulous intellectual standards required in order to pursue such research successfully, we can be spiritually enriched in much the same way in which we can be enriched by taking part in artistic endeavour. Pure science and scholarship, like music, literature and art, contribute directly to our culture, our civilization.

Second, the contribution is indirect. As a result of improving our knowledge and understanding of truth, we may discover how to apply our new knowledge to help realize important human, social objectives, help solve human, social problems. Pure science, in other words, leads to applied

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[6] In principle a much more modest version of the philosophy of knowledge can be upheld, according to which inquiry eschews altogether the aim of benefiting humanity, the basic aim of inquiry being merely to acquire knowledge irrespective of whether this is of value to people or not. It is, however, difficult to justify the modern academic enterprise in terms of this excessively modest version of the philosophy of knowledge. Why should vast sums of public money be spent on organized inquiry if this in no way aims to be of benefit to people? In any case, this modest version of the philosophy of knowledge will be refuted in chapter 9.

science, to technology, which, we may hope, is used in humanly beneficial ways, to help promote human welfare.

Thus, inquiry is of intrinsic or cultural value, when pursued for its own sake, and of pragmatic or technological value, when pursued as a means to the realization of non-academic, human, social ends. To say this is not to say that it is always clear of any particular piece of research whether it is of value culturally or technologically, or both together. Nor should it be assumed that pure research always comes before technological research, technology 'applying' the results of previous pure research. Scientific research that is predominantly technological in character can produce successful results before theoretical explanation and understanding of these results have been achieved. Technological research may even, on occasions, throw up results which lead directly to important theoretical developments, to progress within pure science (a famous example being Carnot's work on the efficiency of steam engines which led to the development of thermodynamics). It is still possible, nevertheless, to make a sharp distinction between the two ways in which inquiry can be of value: of value in itself, or of value as a means to the realization of non-academic, social objectives of value, such as health, comfort, communication, transport, etc.

5. The fundamental methodological prescription of the philosophy of knowledge can be formulated like this. It is absolutely essential that the intellectual domain of inquiry be sharply separated from, and preserved from being influenced by, all kinds of psychological, sociological, economic, political, moral and ideological factors and pressures which tend to influence thought in life, in society. Feelings, desires, human social interests and aspirations, political objectives, values, economic forces, public opinion, religious views, ideological views, moral considerations, must not be allowed, in any way, to influence scientific or academic thought within the intellectual domain. Only questions of fact, truth, logic, evidence, experimental and observational reliability and success must be considered. Only those factors must be considered, and allowed to be influential, which are relevant to the determination of truth and the acquisition of knowledge. All additional extra-academic human, social considerations and factors must be ruthlessly held at bay and ignored.[7]

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[7] Popper puts the point like this: 'It is clearly impossible to eliminate . . . extra-scientific interests and to prevent them from influencing the course of scientific research. And it is just as impossible to eliminate them from research in the natural sciences – for example from research in physics – as from research in the social sciences. What is possible and what is important and what lends science its special character is not the elimination of

The reason for all this is simple. The fundamental intellectual aim of inquiry is to improve our knowledge of objective, factual truth. We can only hope to achieve this aim if we allow only issues of fact and truth to influence our choice of results and theories. The moment we allow our human desires and values, our political objectives and ideologies to influence the way in which we accept and reject theories and results within scientific, academic inquiry, knowledge of objective fact must inevitably be subverted or corrupted. The objectivity, intellectual integrity, and rationality of inquiry must be undermined. Objective knowledge of truth will degenerate into prejudice and ideology. Scientific, academic inquiry must lose its entitlement to the claim that it achieves and produces authentic, objective knowledge of truth.

It is not just the intellectual integrity of inquiry that is at issue here: the human value of inquiry is at issue as well. For the human, social value of inquiry resides precisely in its capacity to produce genuine objective knowledge of truth. Almost paradoxically, in short, in pursuing inquiry we must, within the intellectual domain, ruthlessly ignore all questions concerning human values and aspirations precisely so that inquiry may ultimately be of genuine human value and may help us to realize our human aspirations.

The Lysenko episode in Soviet biology provides us with a classic illustration of just how disastrous can be the outcome, in both intellectual and human terms, if these simple points are violated. As a result of the imposition of Lamarckian ideas on Soviet biology, for ideological reasons, through external political pressure, not only was Soviet biology severely retarded from an intellectual standpoint; in addition, all this had disastrous consequences for Soviet agriculture, which in turn had harmful human, social repercussions.[8]

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extra-scientific interests but rather the differentiation between the interests which do not belong to the search for truth and the purely scientific interest in truth ... In other words, there exist purely scientific values and disvalues, and extra-scientific values and disvalues. And although it is impossible to separate scientific work from extra-scientific applications and evaluations, it is one of the tasks of scientific criticism and scientific discussion to fight against the confusion of value-spheres and, in particular, to separate extra-scientific evaluations from *questions of truth'* (Popper, 1976, pp. 96-7). The classic statement of the need to exclude values from the social sciences is perhaps the one to be found in Weber (1949).

[8] Anyone who has any doubts about the appalling and disastrous consequences of Stalin's support for Lysenko should read Medvedev (1969, 1971).

6. An important qualification must now be made to what has just been said. It must of course be conceded that in pursuing scientific, academic research, scientists and academics are in fact motivated by the desire to achieve all kinds of 'extra-academic' goals. All kinds of 'extra-academic' factors and considerations influence the research aims that scientists and academics in practice pursue. Academics may pursue research in order to discover what will be of benefit to humanity. Academics may well be motivated by passionate intellectual curiosity, by the desire to achieve lasting recognition, to win the esteem of colleagues – or by the desire to advance an academic career or earn a living. All kinds of moral, social, cultural, philosophical, ideological, economic and political factors and considerations may influence the aims which scientists pursue in their research, the problems that academic research seeks to solve. This influence operates roughly as follows. The research problems that are in fact tackled, the research aims that are pursued, by the scientific academic community, are the outcome of (a) decisions of individual scientists and academics as to which problems they seek to investigate, which research aims they pursue; (b) decisions of various research institutions as to what type of research should be undertaken; (c) decisions of funding bodies as to which scientists, institutions and research projects should receive financial support; (d) more general policy decisions as to what kinds of research should receive financial support. All these types of decisions may be influenced by all kinds of extra-academic personal and social factors and considerations – in addition, of course, to being influenced by scientific, intellectual considerations.

The all important point, however, is that when it comes to the assessment of results, the assessment of potential contributions to knowledge, the assessment of scientific, academic progress, all these extra-academic factors and considerations, aims and desires, must be ruthlessly ignored, adequacy to the facts, the evidence, the truth, alone being taken into account. In the context of research or discovery, all kinds of extra-scientific, extra-academic personal, social, evaluative factors may legitimately influence scientists and academics in their choice of research aims and problems. In the context of justification, verification, corroboration, or assessment of results, however, one aim only must be taken into account, namely the aim to discover truth, to accept that which constitutes authentic, objective knowledge of fact.[9]

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[9] A number of historians of science have argued, explicitly or implicitly, that in order to understand the origins and development of modern science it is essential to see science in its personal, social and cultural context, influenced by diverse personal, social, political, economic and religious factors. See, for example, Koestler (1964); Manuel (1968); Merton

All this might be summed up as follows. The intellectual domain of inquiry must be shielded from the potentially corrupting influence of a largely irrational society if inquiry is to retain its rationality, objectivity, intellectual integrity, entitlement to the claim that it produces genuine knowledge. It must, of course, be recognized that inquiry forms a part of the social order, all kinds of social factors influencing the aims of research. This influence is entirely harmless, however, as long as results are assessed entirely with respect to truth and their capacity to contribute to objective knowledge, social influences here being excluded.

This basic requirement, that a sharp demarcation be maintained between the intellectual domain of inquiry and the rest of society, has a number of further consequences, some of which are now indicated.

(1970); Teich and Young (1972); Webster (1975); Mandrou (1978). In addition, sociologists of knowledge and of science have argued for the need to take sociological factors into account in understanding science – either internal to the scientific community or also external. See, for example, Hagstrom (1965); Ben-David (1971); Mulkay (1979). Others have been concerned to point out the important role that various kinds of human interests play in science, and have done so, either tediously and tendentiously, as in the case of Habermas (1972), or brilliantly, with a wealth of factual detail, and with real moral concern, as in the case of Greenberg (1971). Others again, following Francis Bacon, have argued in various ways for the need for science to give greater priority to helping to promote human welfare, justice and liberation: for example, Bernal (1967); Ravetz (1971); Easlea (1973). None of this diverse work, and none of these diverse arguments, in any way goes against the basic claim that science ought to be pursued in accordance with the edicts of the philosophy of knowledge and, indeed, is, and has been, so pursued. The one essential requirement that must be fulfilled, if science is to be pursued in accordance with the philosophy of knowledge, is that results of research are assessed solely with respect to truth and falsity. This clearly leaves endless room for personal, social and cultural factors to influence choice of research problems, aims and priorities – and thus to influence what science comes to develop knowledge about. How much science is done, public attitudes towards science, the use that is made of new knowledge, the very adoption of the philosophy of knowledge itself by the scientific community: all these vital aspects of science in any society constitute social and cultural aspects of science inevitably linked, in one way or another, to other aspects of the given society. In brief, many critics of scientific and academic orthodoxy have at most criticized only extremely crude versions of standard empiricism and the philosophy of knowledge, and have left uncriticized and unexamined more sophisticated versions of these doctrines that exercise such a profound, and damaging, influence over so much scientific and scholarly work. (More recently, central tenets of the philosophy of knowledge have been challenged by 'social constructivist' critics but, as we shall see in a moment, these criticisms profoundly miss the point.)

7. The intellectual *aims* of scientific, academic inquiry must be sharply distinguished from the personal, social aims implicit in much scientific, academic research. The intellectual aim of all scientific, academic inquiry is, quite simply, truth, the attainment of objective knowledge of value-neutral truth, together with the development of theories which successfully predict and explain factual truth. The personal, social aims of scientific, academic inquiry, on the other hand, may be manifold, as has been indicated above.
8. The intellectual *problems* of scientific, academic inquiry – scientific, academic problems – must be sharply distinguished from human, social problems. Intellectual problems arise when we do not know how to achieve the basic aim of inquiry, namely knowledge of truth. Intellectual problems emerge when we discover that our knowledge is defective or incomplete in some way – when theories and experimental results conflict, or when experimental results receive no satisfactory theoretical explanation. Human, social problems, on the other hand, arise when we do not know how to achieve human, social goals – enough to eat, good living conditions, health, friendships and love, justice, peace, a productive and creative way of life, happiness.

Intellectual problems have an impersonal, objective character, in that they can be conceived of as existing relatively independently of the particular thoughts, experiences, aims and actions of individual people. Human, social problems, on the other hand, are essentially problems experienced and confronted by people in their lives. Such problems cannot be detached, as it were, from the actual thoughts, experiences, aims and actions of people in society.
9. Just as intellectual aims and problems must be sharply distinguished from social aims and problems, so too intellectual *progress* must be sharply distinguished from social progress. Intellectual progress has to do exclusively with the extent to which science, or inquiry more generally, acquires knowledge and understanding of truth, in an entirely impersonal, objective sense. Thus the intellectual progress of science is not to be assessed in terms of the extent to which *people* enhance their personal knowledge, understanding and appreciation of the world around them, or the extent to which this enriches their lives. (All this is a question of psychological and sociological change, provoked perhaps by the advancement of science, but not itself to be identified with the intellectual advancement of science.) The intellectual progress of science is quite distinct from any increasing human value of science, from the capacity of science to promote social progress. Intellectual progress is to be assessed solely in terms of the extent to which intellectual aims are being realized, intellectual problems are being solved.

Thus it is entirely possible for science itself, on the intellectual level, to be making great strides forward even though the human value of science is decreasing, and the tendency of science to help promote human progress is decreasing. We may well hope and believe that scientific progress helps lead to human progress. Human progress is not, however, a part of the definition of scientific progress.

10. Rationality, intellectual standards, scientific, intellectual criteria of acceptability – as these arise within the context of inquiry – are concerned exclusively with the assessment and evaluation of claims to knowledge, the assessment of results with respect to truth, adequacy to fact. Rationality, intellectual standards are in no way concerned with the assessment and evaluation of the personal, social, moral dimensions of the aims of research. Scientists and academics may well hope that good human, social, moral aims motivate scientific, academic research actually being pursued, and that good moral, social considerations influence where relevant the choice of research aims in appropriate ways. The evaluation and assessment of the human, social, moral dimension of the aims of scientific, academic research lies, however, beyond the scope of scientific, intellectual standards, beyond the scope of reason.

Quite generally, in fact, the evaluation and assessment of personal feelings, desires, aims and moral views lies beyond the scope of scientific, academic rationality. In particular, wisdom – being intimately associated with the personal and the evaluative – lies beyond the scope of rationality. It is no part of the intellectual aim of intellectual inquiry to enhance our wisdom.

None of this should be taken as implying that personal feelings, desires, moral views – and wisdom – are of no importance for life, and even for science. Quite the contrary. It is just that these things lie beyond the range of scientific, academic rationality, outside the scope of intellectual standards, which are concerned exclusively with the assessment of truth and claims to knowledge.

11. At the centre of the philosophy of knowledge, forming the paradigmatic core of the doctrine, there is a more specific philosophy of *science*, here called *standard empiricism*. All that the philosophy of knowledge asserts about inquiry as a whole, standard empiricism also asserts about science; and in addition it makes the following crucial assertion: when it comes to the assessment of results in science, the assessment of scientific propositions, laws and theories, these results must be accepted and rejected solely with respect to empirical success and failure, to the justice that they do to observational and experimental evidence, the simplicity, unity or explanatoriness of theories also being taken into account, but no thesis about

the universe being accepted permanently as a part of scientific knowledge independently of empirical considerations. For a time, perhaps, in science, choice of theories may be biased in the direction of some untestable metaphysical conjecture about the world, some paradigm or 'hard core', in the kind of way described by Kuhn (1962) and Lakatos (1970). In the end, however, empirical success or failure alone must decide the fate of scientific theories. In the context of discovery, of course, scientists may quite legitimately be influenced in their thinking, their choice of problems and conjectures, by all sorts of extra-empirical, non-rational considerations – philosophical or metaphysical ideas, even personal, religious, political or economic interests and considerations (and it is in this way that social and cultural factors influence the development of science). The crucial point is that when it comes to the context of justification or assessment, all untestable or metaphysical theses about the world must be ignored, theories being accepted and rejected solely on the basis of the justice they do to the data of scientific observation and experimentation – the simplicity or unity of theories also, perhaps, being taken into account.

A rationale for adopting this basic tenet of standard empiricism can readily be given. We do not and cannot possess *a priori* knowledge about the world, secure knowledge arrived at independently of all experience. Only by comparing our theories about the world with the world itself via our experience of it in an ultimately wholly unbiased, impartial fashion, can we hope to improve our scientific knowledge about the world. Thus a discipline which permanently biases the selection of theories in the direction of some untestable metaphysical conjecture, upheld in an *a prioristic* fashion, cannot procure authentic knowledge, and cannot be scientific. Such a discipline can only produce dogma, ideology, or religious faith.

12. Standard empiricism and the philosophy of knowledge both require, quite essentially, that a sharp distinction can be drawn between (a) the context of discovery, and (b) the context of justification (the context of the appraisal of theories or results from the standpoint of truth). If such a sharp distinction can be drawn, then it becomes intelligible at least to assert that the scientific character of science, and the rational character of rational inquiry more generally, are bound up only with the way potential contributions to knowledge are assessed with respect to truth, in the context of 'justification'. It becomes intelligible to hold that all sorts of extra-rational, and even irrational, psychological and social factors may entirely legitimately influence scientific and rational thought in the context of discovery, the context of inventing new ideas, choosing research aims and problems, without the rationality, the intellectual rigour, of science or inquiry thereby being

undermined. All this becomes highly suspect, more or less unintelligibly, the moment the possibility of drawing a sharp distinction between the contexts of discovery and justification is cast into doubt. Classic statements of the importance of distinguishing sharply between these two contexts, reason applying only to 'justification' and not to discovery, are the following by Reichenbach and by Popper. '. . . the way, for instance, in which a mathematician publishes a new demonstration, or a physicist his logical reasoning in the foundation of a new theory, would almost correspond to our concept of rational reconstruction; and the well-known difference between the thinker's way of finding this theorem and his way of presenting it before a public may illustrate the difference in question. I shall introduce the terms *context of discovery* and *context of justification* to mark this distinction. Then we have to say that epistemology is only occupied in constructing the context of justification' (Reichenbach, 1961). 'The initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it. The question how it happens that a new idea occurs to a man – whether it is a musical theme, a dramatic conflict, or a scientific theory – may be of great interest to empirical psychology: but it is irrelevant to the logical analysis of scientific knowledge. This latter is concerned not with *questions of fact* (Kant's *quid facti*?), but only with questions of *justification or validity* (Kant's *quid juris*?). Its questions are of the following kind. Can a statement be justified? And if so, how? Is it testable? Is it logically dependent on certain other statements? Or does it perhaps contradict them? In order that a statement may be logically examined in this way, it must already have been presented to us. Someone must have formulated it, and submitted it to logical examination. Accordingly I shall distinguish sharply between the process of conceiving a new idea, and the methods and results of examining it logically. As to the task of the logic of knowledge – in contradistinction to the psychology of knowledge – I shall proceed on the assumption that it consists solely in investigating the methods employed in those systematic tests to which every new idea must be subjected if it is to be seriously entertained' (Popper, 1959, p. 31).

13. There is general agreement amongst proponents of the philosophy of knowledge that the empirical sciences can be ordered into a rough kind of hierarchy. At the bottom, at the most fundamental level of all, we have theoretical physics, and closely associated with it, cosmology. Ascending, we have the theoretically less fundamental parts of physics such as solid state physics and physical chemistry; a little higher, we have the whole of inorganic chemistry, and alongside chemistry astronomy, astrophysics and the earth sciences (all specialized applications of physics and chemistry). Ascending

still higher, we have the biological sciences with organic chemistry, molecular biology, biophysics and biochemistry at the base, sciences such as zoology, botany, anatomy, neurology, genetics half way up, and ecology and the study of animal behaviour at the top. Higher still, we have the social sciences, anthropology, sociology, psychology, linguistics, economics, political science and history.

According to one view – reductionism – we should seek to reduce all these sciences, at least in principle, to theoretical physics. According to a rival view – anti-reductionism – this is either an unrealizable goal, an undesirable goal, or both.[10] The important point is that both views agree that the empirical sciences can indeed be hierarchically organized along the lines indicated, with what is intellectually and explanatorily fundamental at the bottom, each science becoming progressively less and less intellectually fundamental as we ascend to the top. What this means is that a science at one level presupposes and, where relevant, uses the results of sciences at lower, intellectually more fundamental levels, whereas the reverse is not the case. Theoretical physics does not presuppose or use theories from sociology, whereas sociology constantly uses, even if only in an obvious and crude way, theories and results of physics (such as the existence and persistence of gravitation). Or to take less extreme examples, chemistry presupposes physics (especially the theory of atomic and molecular structure and quantum theory) whereas fundamental theoretical physics presupposes and borrows nothing from chemistry (apart occasionally from a piece of chemical technology for instruments, which is another matter altogether).

14. Rather more controversially, a somewhat analogous hierarchical ordering can be discerned within the logical and mathematical disciplines. At the base there is logic. A little higher up, there is set theory. Almost the whole of the rest of mathematics can be interpreted as amounting to more or less specialized applications of set theory.

15. Intellectually respectable inquiry is, according to the philosophy of knowledge, almost entirely to be identified with professional, expert, scientific, academic inquiry. It must, of course, be conceded that intellectual inquiry of a kind does go on outside universities and research institutions, in society, as an integral part of our lives. We are all, all the time, improving our knowledge and understanding of various aspects of the world that concern us, as we live. Such personal intellectual inquiry hardly deserves, however, to be esteemed very highly from an intellectual standpoint, just because by and

[10] For a clear, radical statement of the reductionist, physicalist position, see Smart (1963). For anti-reductionist views see Koestler and Smithies (1969); Popper and Eccles (1977).

large it does not satisfy the kind of intellectual criteria that have been spelled out above. Our personal thinking is hopelessly intermingled with our personal lives, our actions, desires, feelings, prejudices, values. Such personal thinking is to be construed as a legitimate object of study for academic psychological and sociological research, rather than itself being an important part of intellectually respectable scientific, academic inquiry.

16. Professional, expert scientific, academic inquiry is thus, according to the philosophy of knowledge, in a position to deliver authoritative judgements concerning questions of fact and truth – where knowledge has indeed been established. People in society who are not experts, not scientifically, academically qualified, not themselves engaged in scientific, academic research, cannot be expected to provide cogent, authoritative criticisms of scientific, academic results, arising out of their own personal views. Academics cannot be expected to treat those who are not academically qualified as *colleagues* propounding ideas, theories, arguments, criticisms that need to be taken seriously, on the intellectual level. The ideas of people in society can of course be *studied* empirically, by psychology and sociology: such ideas do not themselves however, constitute serious contributions to scientific, academic knowledge.

Scientists and academics are only entitled to their special scientific, academic authoritativeness insofar as they restrict themselves to delivering purely factual judgements, judgements concerning truth, that lie within their own particular speciality, their field of academic competence. The moment academics deliver themselves of value judgements, moral or political judgements, they cease to speak in a scientifically, academically authoritative fashion, and speak simply as human beings, as citizens.

17. Insofar as we seek to conduct our own personal thinking in an intellectually respectable fashion, we must endeavour, according to the philosophy of knowledge, to make our thinking conform, on the personal level to the general principles of intellectually acceptable scientific, academic thought, that have been spelled out above. We must seek to set up a sharp distinction, within ourselves, between our thinking and reasoning, on the one hand, and our emotions and desires, on the other hand. Our beliefs, our knowledge, that which we accept as true, must be subjected to the same kind of impartial, objective, intellectual appraisal found within scientific, academic inquiry at its best, all considerations of personal feelings, desires, aims and values being ruthlessly ignored. The mind must be sharply separated off from the heart, promptings of the heart not being allowed to influence what is accepted by the mind. A main purpose of education is to encourage students

to acquire the capacity to appraise ideas in this kind of impersonal, objective, rational fashion.

18. Literature, and art more generally, according to the philosophy of knowledge, make no kind of direct contribution to the intellectual domain of intellectual inquiry. Great literature and art may perhaps have some kind of inspirational value for some brands of intellectual inquiry: they do not, however, have any kind of direct rational contribution to make to intellectual inquiry just because literature and art do not contribute to *knowledge*. In literature, ideas, feelings, values and imaginary human actions are almost invariably interspersed with one another in a complex fashion, as in life. In addition, our emotional responses to literature have a great deal to do with our assessment of its cultural value. Literature does not seek to improve our knowledge of truth, and does not seek to comply with basic intellectual requirements of a search for truth. Literature and art may, however, of course, themselves be legitimate objects for intellectual inquiry, about which we may seek to develop factual knowledge.

19. Ideas, in order to be capable of objective rational appraisal, must be entirely factual in character, capable of being true or false, and thus potential contributions to knowledge. Thus religious views, ideologies, social and political policies, personal philosophies, which intermingle judgements concerning facts and values in an essential way, are incapable of objective, rational assessment and have no place within the intellectual domain of scientific, academic inquiry (though of course factual theories about religious views, ideologies, etc., in fact held in society, do have such a place). All such ideas may be said to be, in an important sense, irrational.[11] In particular a 'philosophy' of some enterprise, a view about what ought to be, ideally, the basic aims and methods of the enterprise, has no place within the intellectual domain of scientific, academic inquiry. For such a 'philosophy', being a view about ideal aims and methods, must inevitably intermingle factual and value judgements.

It deserves to be noted that both the philosophy of knowledge and the philosophy of wisdom are philosophies of inquiry in this common-sense conception of 'philosophy', in that both are views about what ought to be, ideally, the basic aims and methods of inquiry. Thus neither of these 'philosophies' can have any very respectable place within the intellectual domain of inquiry pursued in accordance with the philosophy of knowledge. In this way the philosophy of knowledge preserves itself from criticism. Once the philosophy of knowledge is adopted and put into practice, accepted

[11] This point is well made, in connection with ideology, in Harris (1968, ch. 1).

intellectual standards effectively debar critical, rational discussion of philosophies of inquiry. Claims to knowledge can be critically discussed: but the adoption of the aim to acquire knowledge as the basic aim for inquiry becomes more or less immune from critical reconsideration.

In an analogous, somewhat more limited way, standard empiricism, once adopted and put into practice by science, preserves itself from effective criticism within science. For standard empiricism implies that only testable factual hypotheses deserve consideration within science. Standard empiricism is not itself a testable factual hypothesis: hence it ought not itself to be critically discussed within science. Discussion of rival philosophies of science must be sharply separated off from science itself, if science is to retain its intellectual integrity as science. And indeed such discussion is at present by and large confined to the ineffective intellectual ghetto of 'the philosophy of science', where it has little hope of influencing aims and methods actually adopted in scientific research.[12]

It should perhaps be noted, finally, that proponents of the philosophy of knowledge can quite consistently acknowledge that 'reason' can be applied to actions and decisions quite generally, and does not have to be restricted in its application to its basic (philosophy-of-knowledge) task of assessing claims to knowledge. There is in fact an extensive literature on such topics as rational decision theory, practical reasoning, rational action. (See Morganstern and von Neumann, 1944; Jeffrey, 1965; Borger and Cioffi, 1970; Wilson, 1974; Raz, 1975, 1978; Harrison, R., 1979.) Two requirements must however be satisfied if this is to conform to the philosophy of knowledge. First, 'rational'

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[12] One contemporary scientist, no doubt expressing feelings shared by many of his fellow scientists, puts the matter like this: ' . . . 'the Philosophy of Science' nowadays . . . [is] arid and repulsive. To read the latest symposium volume on this topic is to be reminded of the Talmud, or of the theological disputes of Byzantium. It is not now a field where the amateur philosopher may gently wander and pick a few nosegays. It is fiercely professional and technical and almost meaningless to the ordinary working scientist . . . This is doubly unfortunate: the divorce of Science from Philosophy impoverishes both disciplines' (Ziman, 1968, p. 31). More recently, Steven Weinberg has declared 'The insights of the philosophers I studied seemed murky and inconsequential compared with the dazzling successes of physics and mathematics. From time to time … I have tried to read the current work on the philosophy of science. Some of it I found to be written in a jargon so impenetrable that I can only think that it aimed at impressing those who confound obscurity with profundity…. only rarely did it seem to me to have anything to do with the work of science as I knew it. … I know of *no one* who has participated actively in the advance of physics in the postwar period whose research has been significantly helped by the work of philosophers' (Weinberg, 1993, pp. 133-4).

decision-making and 'rational' action must conform to the edicts of the philosophy of knowledge to the extent that (a) it is based on 'rationally' obtained knowledge and (b) rules of reason – such as the demand for consistency – are themselves of the type stipulated by the philosophy of knowledge (having to do with the assessment of claims to knowledge). Second, research into such topics as decision theory, practical reasoning and rational action must itself conform to the edicts of the philosophy of knowledge: it must seek merely to improve knowledge about these topics. On the whole, the existing literature on these topics does indeed satisfy these two requirements.

What has been spelled out in this chapter is summarized in the following two diagrams. Figure 1 depicts the intellectual, rationalistic aspects of inquiry, as conceived by the philosophy of knowledge, and figure 2 depicts how the intellectual domain of inquiry is conceived to be both dissociated from, and yet influenced by, and influential upon, the social world.

This completes my exposition of the philosophy of knowledge.

I do not claim that everyone associated with the academic enterprise accepts the philosophy of knowledge. Nor do I claim that all scientific/academic work proceeds precisely in accordance with the precepts of the philosophy of knowledge. The best of actual academic work embodies, perhaps, a mixture of the philosophies of knowledge and wisdom. I do not claim, either, that there is no change within academia. On the contrary, in chapters 6, 11 and 12 I shall discuss a number of developments during the last 40 years or so that can be regarded as steps away from the philosophy of knowledge and towards the philosophy of wisdom. What I do claim is that during the last 400 years or so, with the gradual decline in influence of Christian thought in the universities, something like the above conception of intellectual inquiry has progressively become the predominant creed, so that today it still exercises a profound and far-reaching influence over science, scholarship, technological research and education. This conception of what ought to be the basic aims and methods of inquiry has shaped the whole way in which scientific, academic inquiry has developed in the so-called western world, so much so that it is now built into the whole intellectual/institutional structure of the academic enterprise, and the way this is related to life, to the rest of the social world. The philosophy of knowledge still constitutes the orthodox conception of rational inquiry, and there is no rival conception of rational inquiry in the public domain. And it is not just science, scholarship and education that are influenced by the philosophy of knowledge: through these, the philosophy of knowledge exercises its influence, to a greater or

lesser extent, over many aspects of our personal and social lives. Our very psyches, the way personal thought, feeling, desire and action tend to be interrelated, are affected by the prevalence of the philosophy of knowledge. The widespread tendency for there to be a split between emotion and desire on the one hand, and intellect and rational thought on the other, can be attributed to the pervasive influence of the philosophy of knowledge. The central, most urgent point I have to make is however this: quite generally, our overall capacity to realize what is of value in life is adversely affected by the fact that it is the philosophy of knowledge, and not the philosophy of wisdom, that predominates in our world as the official ideal conception of rational inquiry.

In order to appreciate just how massive and extensive is the influence of the philosophy of knowledge over present-day academic work and thought, and over the rest of life, it is essential to consider what academic work and thought would be like if based on the edicts of some radically different philosophy of inquiry. At present, just because the philosophy of knowledge is so widely presumed to be the only sane possibility, its ubiquitous influence becomes invisible. Only in Chapter 6, with the rival philosophy of wisdom before us, will we be in a position to consider seriously the question of which philosophy predominates in practice.

Versions and aspects of the philosophy of knowledge, under various labels, have long been subjected to attack, from many quarters, as I have already indicated. The Romantic movement, from the outset, opposed 'Single vision & Newton's sleep' (Blake), and what it saw as the oppressive domination of scientific rationality, so highly valued by the Enlightenment. Instead of upholding science, knowledge and reason as engines for the liberation of humanity, Romanticism valued art, imagination, inspiration, individual genius, emotional and motivational honesty rather than careful attention to objective fact. Almost all subsequent opposition to the philosophy of knowledge stems from, or echoes, the Romantic rebellion. There is the movement Isaiah Berlin has identified as the 'Counter-Enlightenment' (Berlin, 1979, ch. 1). There is existentialism, with its denunciation of the hegemony of reason, its passionate affirmation of the value and centrality of irrationality in human life, from Doestoevsky, Kierkegaard and Nietzsche to Heidegger and Sartre (see, for example, Barrett, 1962). There is the onslaught against the Enlightenment undertaken by the Frankfurt school, by postmodernists and others, from Horkheimer and Adorno to Lyotard, Foucault, Habermas, Derrida, MacIntyre and Rorty (all

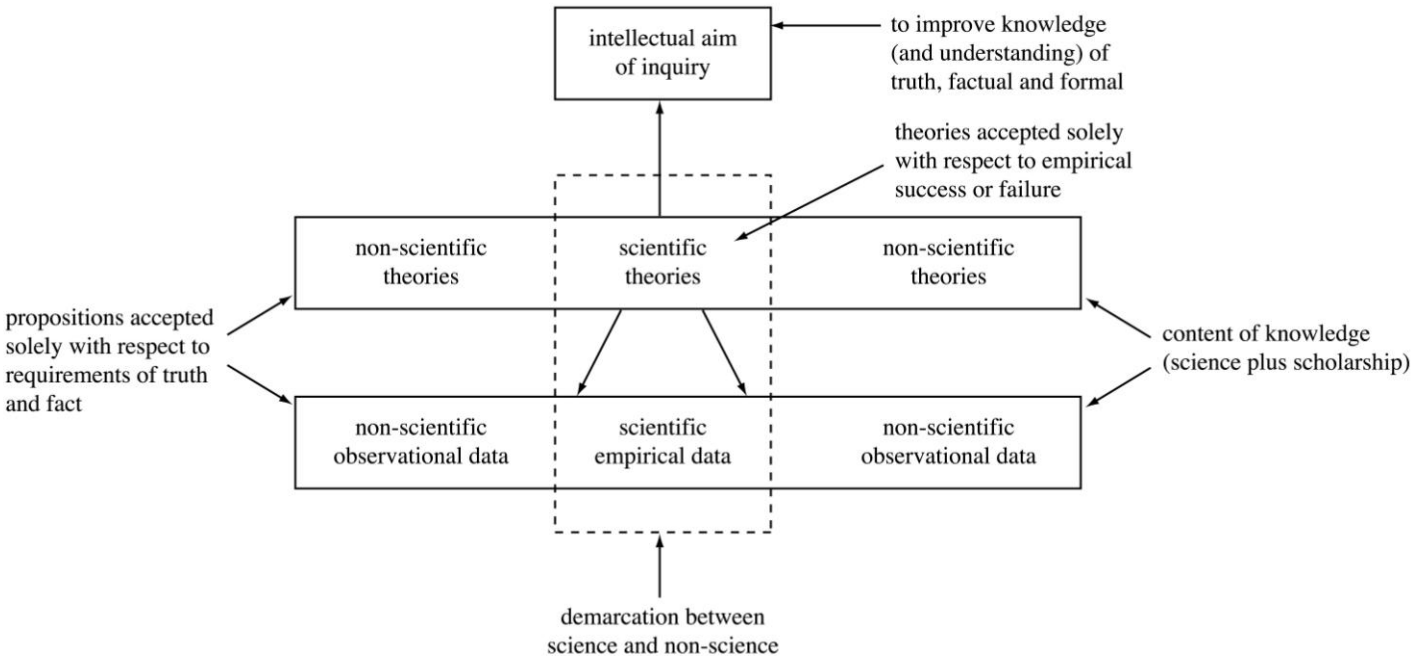

Figure 1 Intellectual domain of inquiry according to the philosophy of knowledge

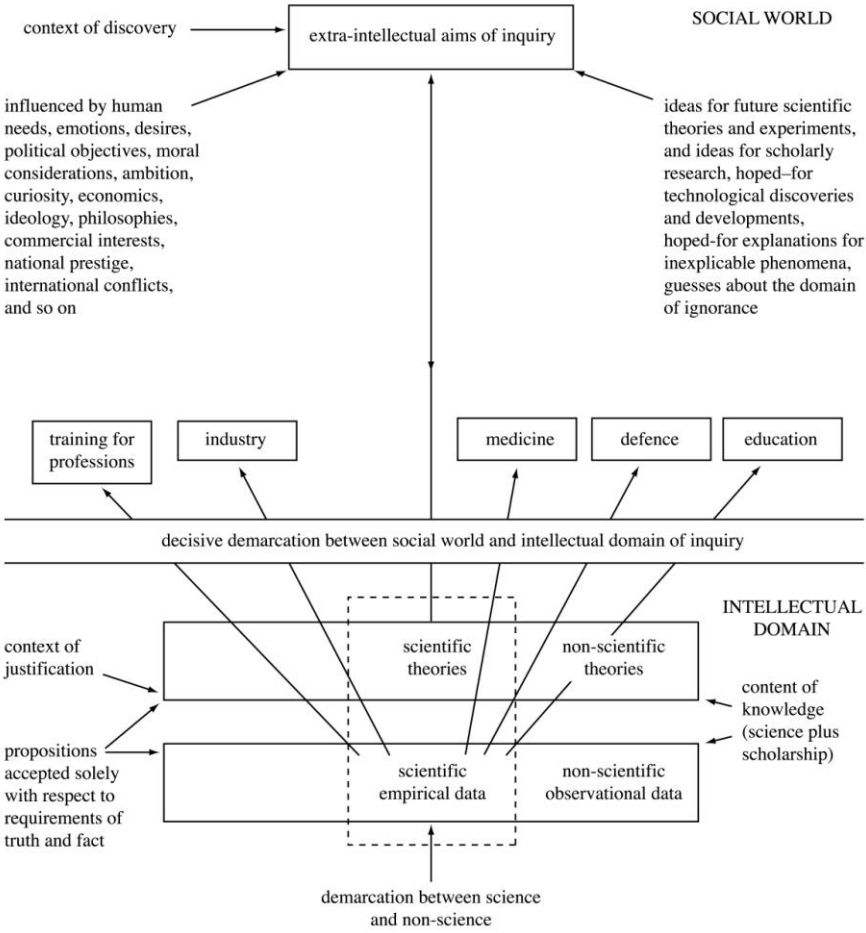

Figure 2 Relationship between the intellectual domain of inquiry and the social world according to the philosophy of knowledge

recently sympathetically expounded and assessed by Gascardi, 1999). The soul-destroying consequences of valuing science and reason too highly is a persistent theme in literature: it is to be found in the works of writers such as D.H. Lawrence, Doris Lessing, or Max Frisch (see his *Homo Faber*). There is persistent opposition to modern science and technology, and to scientific rationality, often associated with the Romantic wing of the green movement, and given expression in such popular books as Marcuse's *One Dimensional Man*, Roszak's *Where the Wasteland Ends*, Porritt's *Seeing Green* and Appleyard's *Understanding the Present*. There is the feminist critique of science and

conceptions of science: see, for example, Fox Keller (1984) and Harding (1986). And there are the corrosive implications of the so-called 'strong programme' in the sociology of knowledge, and of the work of social constructivist historians of science, which depict scientific knowledge as a belief system alongside many other such conflicting systems, having no more right to claim to constitute knowledge of the truth than these rivals, the scientific view of the world being no more than an elaborate myth, a social construct (see Barnes and Bloor, 1981; Bloor, 1991; Barnes, Bloor and Henry, 1996; Shapin and Schaffer, 1985; Shapin, 1994; Pickering, 1984; Latour, 1987). This latter literature has provoked a counter-attack by scientists, historians and philosophers of science seeking to defend science and aspects of what is being called here 'the philosophy of knowledge': see Gross and Levitt (1994), Gross, Levitt and Lewis (1996), and Koertge (1998). This debate between critics and defenders of science came abruptly to public attention with the publication of Alan Sokal's brilliant spoof article 'Transgressing the boundaries' in a special issue of the cultural studies journal *Social Text* in 1996 entitled *Science Wars*: see Sokal and Bricmont (1998).

It may be asked: Given this torrent of criticism of aspects of the philosophy of knowledge, what do the arguments of this book have to add, especially in view of the fact that a great deal of the above criticism was published after the publication of the first edition of this book, in 1984?

I cannot over-emphasize the importance of my response to this question for a proper understanding of this book. My arguments here are dramatically and profoundly different from the above. They are indeed *diametrically opposed* to the above criticisms, insofar as the above views oppose scientific rationality, seek to diminish or restrict its influence, or hold that it is unattainable. My central point is that we suffer, not from too much scientific rationality, but from not enough. What is generally taken to constitute scientific rationality, as set out in the above nineteen points of the philosophy of knowledge, is actually nothing of the kind. It is a characteristic, influential and damaging kind of *irrationality* masquerading as rationality. The philosophy of knowledge suffers from severe intellectual defects, and it is these defects that are responsible for damaging cultural and humanitarian defects of science, and of academic inquiry more generally, to be discussed below. Science is damaged by being trapped within a widely upheld but severely defective philosophy of science, namely standard empiricism; free science from this defective philosophy, provide it with a more intellectually rigorous philosophy, and it will flourish in both intellectual and humanitarian terms. Likewise, academic inquiry, quite generally, is damaged by being trapped within the intellectually defective philosophy of knowledge; free it to be

pursued within the framework of the more rigorous philosophy of wisdom, and it will flourish, in both intellectual and human terms. In ensuing chapters I shall argue that reason, the authentic article, arrived at by generalizing the progress-achieving methods of science, can only have profoundly liberating and enriching consequences for all aspects of life, and thus deserves to enter into every aspect of life. It is not reason that is damaging, but defective pretenders to reason, false claimants, and above all those that I have already indicated: standard empiricism and the philosophy of knowledge.

As we shall see in ensuing chapters (especially chapters 3-5 and 9), standard empiricism fails to exhibit science as a rational enterprise because it fails to solve the problem of induction, fails to justify the crucial role that simplicity, unity and explanatoriness play in theory choice in science, and even fails to explain what simplicity, unity or explanatoriness *are*. These failures stem from the basic failure to identify correctly a proper basic aim of science: to discover in what way the universe is comprehensible, it being presupposed that the universe is comprehensible in some way or other. The failure, in other words, involves failing to acknowledge that substantial metaphysical assumptions concerning the knowability and comprehensibility of the universe are built into the aims of science. There is also a failure to acknowledge that values are inherent in the aims of science, and that science is, inevitably and quite properly, an integral part of a political programme to improve the human condition. Even more seriously, academic inquiry quite generally, pursued in accordance with the philosophy of knowledge, when judged from the standpoint of helping humanity realize what is of value in life by intellectual means, is so grossly irrational that it violates three of the most elementary rules of reason conceivable, as we shall see in chapter 3. And these intellectual failings have, as we shall see, far-reaching damaging humanitarian repercussions.

I have, I hope, said enough to make clear that I am engaged in an activity that is diametrically opposed to rather well-known criticisms of scientific rationality, indicated above. This difference is absolutely crucial when it comes to the all-important task of improving the rigour and the human value of science, and of academic inquiry more generally. It is only when the rationality defects of the *status quo* have been clearly and precisely identified that we are in a position to see what needs to be done to remove these defects. The conception of science, and of academic inquiry, that I defend – aim-oriented empiricism and the philosophy of wisdom – emerge as a result of changing standard empiricism and the philosophy of knowledge just sufficiently to overcome the rationality defects inherent in these views.

Thus, far from seeking to oppose or undermine scientific rationality, my aim is exactly the opposite: I seek to enhance and promote scientific rationality, to such an extent, indeed, that it becomes fruitfully applicable to all aspects of life.[13] I emphasize this point because some comments on the first edition of this book entirely misunderstood its message, and interpreted it as yet another effort to undermine the rationality and objectivity of science: see, for example, O'Hear (1989, pp. 224-30). In the heat and dust of the battle between those who attack and defend orthodox conceptions of scientific rationality, what has been ignored is just the central thesis of this book: both parties in the battle are wrong. Those who attack or seek to undermine scientific rationality could hardly be engaged in a more damaging intellectual activity. On the other hand, those who defend orthodox conceptions of scientific rationality are actually defending a species of *irrationality*. Progress is integral to science. But we need not just progress in knowledge, but progress in scientific rationality as well.

The last point is especially important. Scientists are, of course, devoted to making progress in knowledge and understanding. But when it comes to scientific rationality, to scientific method, to our conception of science, a fierce, almost dogmatic resistance to the idea that here some kind of progress or change might be needed sets in. Scientists cling to the standard empiricist idea that evidence alone should determine what is accepted and rejected in science, in part because they fear that if other factors are allowed to exercise an influence over the content of science the floodgates will be opened, metaphysical and philosophical doctrines, values and political creeds will come to dominate, and the objectivity and authenticity of scientific knowledge, upon which depend its value to humanity, will be sabotaged and corrupted. Attacks against scientific objectivity and rationality by sociologists and historians of science, and others, only serve to intensify this fear, this fierce resistance to the very idea that the orthodox conception of science might genuinely stand in need of improvement.

This is perhaps the most serious obstacle that the project I am trying to launch with this book has to overcome. Scientists, by and large, refuse even to consider arguments designed to show that current conceptions of scientific rigour or rationality are seriously defective, and need improvement. Implications for the nature of science that come from developments within science *are* accepted by scientists: an example is Einstein's discovery of

[13] 'Enhanced rationality' will turn out, however, to be closer to a synthesis of traditional Rationalism and Romantism, and an improvement over both, rather than the triumph of the former over the latter, as we shall see in chapter 5.

special and general relativity, which led to a new, or at least to a much more explicit, role for symmetry principles within theoretical physics. What scientists are not really prepared to consider are arguments which demonstrate the need to improve aspects of science that come from without science, especially if they come from that much despised discipline, the philosophy of science.[14] (But this situation will change if ever, one day, the scientific community should come to accept the argument of this book. For aim-oriented empiricism, as we shall see, demands an interplay between improving knowledge on the one hand, and improving aims-and-methods on the other, so that science adapts its nature to what it discovers about the universe – a generalization of the innovation that Einstein brought to the methods of physics. Science and the philosophy of science are, on this view, in constant fruitful interplay with one another, in a way that is quite different from what we have at present.)

The fierce reluctance of the scientific community to consider the need to improve standard empiricism has spilled over into the philosophy of science. An outsider might well suppose that philosophers of science spend their working hours cooking up new philosophies of science, new views about what the aims and methods of science ought to be. Nothing could be further from the truth. Since the heyday of logical positivism in the 1930s, philosophers of science, like scientists, have taken for granted without question one or other version of standard empiricism and the philosophy of knowledge: the task for philosophy of science is to solve the problems that immediately arise once these presuppositions are made.

There are, to begin with, problems already mentioned, concerning induction and simplicity. How can scientific knowledge about the world be acquired if theories are selected by means of empirical considerations *alone* (as standard empiricism demands)? There will always be infinitely many different theories which agree equally with all available evidence, but which disagree about unobserved phenomena (at times, places or physical conditions not yet put to the test): what rationale can there conceivably be for selecting just *one* of these theories (by means of empirical considerations alone) as alone embodying knowledge, conjectural or otherwise? It is generally agreed that scientists quite properly choose simple rather than complex theories, other things being equal. But what is simplicity here? Is not the simplicity or complexity of a theory something aesthetic and

[14] I write this in the hope of being refuted. And in truth the situation is not entirely hopeless. Some scientists have taken note of the ideas of Popper and Kuhn. A very few scientists have even taken note of my ideas.

subjective, at best entirely dependent on the language that is used to formulate the theory? If judgements concerning the simplicity or complexity of theories are subjective or language-dependent in this way, how can such judgements have a legitimate role to play in science in influencing which theories are to be accepted and which rejected? Again, if scientists do persistently prefer simple to complex theories, does not this mean that they are in effect prejudging the universe itself to be in some sense simple, permanently biasing choice of theory in the direction of the metaphysical doctrine that nature is simple, thus violating standard empiricism and turning science into a sort of religious dogma? How can permanent preference for simple theories be reconciled with the claim that theories are selected *impartially* with respect to empirical success or failure?

There is the problem of verisimilitude. Entirely irrespective of whether we can *know* that science makes progress, what can it *mean* to assert that science makes progress towards increased knowledge of truth, when all scientific theories (so far) strictly are *false,* and no sense can be made of the idea that one false theory is closer to the truth than any other?

There is the problem of the miracle of scientific progress (see for example, Wigner, 1970, pp. 222-37). In dreaming up new theories scientists have, it seems, potentially infinitely many embryonic ideas to choose from, to try to develop into fully fledged theories to be put to the test of observation and experiment. The chances of hitting upon an idea that subsequently turns out to be empirically more successful than its predecessors would seem to be infinitely remote. And yet it is just this infinitely improbable act that has been performed again and again by creative scientists – by people like Kepler, Galileo, Newton, Lavoisier, Dalton, Faraday, Maxwell, Darwin, Planck, Einstein, Bohr, Heisenberg, Schrödinger, Dirac, Watson and Crick, Gell-Mann and Ne'eman, Salam and Weinberg. Often, indeed, the key ideas for successful new theories in physics are invented by mathematicians, apparently while uninterested in, and even in ignorance of, the relevant problems of physics. Thus Apollonius developed the theory of conic sections some 1,800 years before Galileo and Kepler discovered that stones and planets move in conic sections. Gauss and Riemann developed Riemannian geometry many decades before Einstein discovered that gravitation is a manifestation of the Riemannian structure of space-time. Hilbert developed his theory of Hilbert space without any idea that just this is what is needed in order to formulate quantum theory, a discovery made subsequently by von Neumann. It almost seems as if there is a mysterious concordance between the nature of the physical universe on the one hand, and the nature of the human mind, pure human thought, on the other hand. All this is difficult, if

not impossible, to understand given standard empiricism. Indeed the central tenet of standard empiricism – that all our knowledge about the world is acquired through impartial testing of theories, there being no rational method of discovery in science – seems all but refuted.

Finally there are problems about scientific practice. Successful science just does not seem to be pursued straightforwardly in accordance with the edicts of standard empiricism. All too often, scientists fiercely defend theories far less successful empirically than their rivals, even on occasions theories that are ostensibly decisively refuted, and even sometimes theories that are inconsistent, and which thus cannot conceivably be true: and subsequent developments show that this highly anti-standard empiricist behaviour was indeed scientifically fruitful and correct. All this can be found in the work of Bohr in developing early quantum theory, and in the work of Einstein, in his cavalier dismissal of Kaufmann's apparently decisive refutation of special relativity in 1906, and in his espousal of the inconsistent photon theory of light from 1905 onwards. (See, for example, Jammer, 1966; Holton, 1973; Pais, 1980; 1982.)

The innocent might well conclude from this list of problems that no doubt can remain: standard empiricism and the philosophy of knowledge are untenable, and an altogether different philosophy of rational inquiry must be developed. (This is of course the view of this book.) Academic philosophy of science is based on exactly the opposite position. The vast body of work done in the field during the last few decades almost unthinkingly takes for granted that acceptable solutions to the above problems must presuppose the overall framework of standard empiricism, and the philosophy of knowledge. Nothing could illustrate more strikingly the extraordinarily dogmatic, irrational manner in which the philosophy of knowledge is upheld. Most academic philosophy of science, indeed, has served to obscure the fact that standard empiricism, our whole conception of science and of rational inquiry, is in deep intellectual trouble. Thus attention has been focused onto ever more elaborate and technical contributions to ever smaller fragments of the problem of induction – taken to be the problem of justifying the rationality of science in standard empiricist terms. This is true, for example, of most of the 1,130 publications on induction referred to by Kyburg (1970), which appeared mainly in the years 1950-70. Again, attention has been focused onto technical problems of simplicity, the unthinking presupposition being that proposed solutions to the problems can only be acceptable if compatible with standard empiricism. See, for example, Popper (1959, ch. VII, 1963, p. 241); Rudner (1961); Bunge (1961); Ackermann (1961); Barker (1961); Goodman (1972 ch. VII); Davies (1973, chs. 4 and 5); Sober (1975); Hesse

(1974 ch. 10). For a good survey of more recent literature see Salmon (1989). In these ways, attention has been deflected away from the real intellectual and human problems that confront science, and organized inquiry more generally. Above all, attention has been deflected away from the basic problem of this book, indicated in Chapter 1 – the basic problem of the philosophy of inquiry: *what ought to be the overall intellectual aims and methods of inquiry if it is to give us the best possible rational help with realizing what is of value to us in life?* If it is essential to reason to articulate basic problems and propose and criticize possible solutions – and basic to irrationalism to block the doing of this – then most contemporary philosophy of science must be judged to betray reason and embody irrationalism.

In defending scientific orthodoxy, philosophers of science have in effect put into practice what Snow once brilliantly called 'the technique of the intricate defensive' (Snow, 1964, p. 67). Discussion of the problems confronting standard empiricism – the central component of the philosophy of knowledge – becomes so elaborate, technical and abstruse, that the simple and decisive objections to the position are lost sight of by everybody, and the position is preserved by default.

Isaiah Berlin once argued, eloquently and persuasively, that the task of philosophy is to call into question basic presuppositions that dominate both thought and life, usually in unnoticed ways (Berlin, 1980, ch. 1; see also Burtt, 1965). It is just this Berlin conception of philosophy that this book attempts to put into practice. Much traditional philosophy of science must be judged to have done the exact opposite of what Berlin advocates, in that it has not developed valid criticisms of standard empiricism and the philosophy of knowledge, and looked for better conceptions of rational inquiry but, on the contrary, has obscured the need to make such criticisms and innovations, and has made the task of seeing what is wrong and what needs to be done all the more difficult. Even Feyerabend, the licensed court jester of orthodoxy, in effect also makes these elementary mistakes, in that his challenge to orthodoxy takes the predictable form of romantic irrationalism or, as he calls it, methodological anarchism. If standard empiricism must be rejected, Feyerabend in effect presumes along with his opponents, then reason itself must be rejected: see Feyerabend (1975, 1987).

Insofar as it has been recognized that problems such as the above do call into question what the basic aim and methods of science ought to be, the response has been, over the years, to develop a number of different versions of standard empiricism – thus further obscuring that it is standard empiricism as such that is the source of the trouble. (In what follows, the terms for the diverse positions are in part my own.) There are first of all pre-

standard versions of empiricism: (1) *infallible heuristic empiricism,* which asserts that from empirical data alone infallible theoretical knowledge can be arrived at by means of inductive methods; (2) *fallible heuristic empiricism,* which asserts that sound knowledge can be arrived at by induction from empirical data – even though this knowledge may be fallible, and may need subsequently to be revised (Bacon, Newton); (3) *aprioristic empiricism,* which asserts that basic metaphysical principles proved by reason together with empirical investigation suffice to enable us to procure almost infallible scientific knowledge (Descartes, Huygens, Leibniz). Versions of standard empiricism proper begin with acceptance of the thesis that the scientific character of science lies in the way results are assessed, and not especially in the way results are first discovered. Diverse versions of standard empiricism are: (4) *infallible inductivism,* which asserts that once laws and theories have been formulated, they can be securely established as true by being derived inductively from empirical data; (5) *fallible or probabilistic inductivism,* which asserts that inductive verification of laws and theories remains fallible or probabilistic, open to revision: see Herschel (1831); Mill (1843); Jevons (1924); Reichenbach (1938); Hempel (1965); (6) *hypothetico-deductivism,* which asserts that hypothetical laws and theories are to be assessed by means of the empirical verification and falsification of propositions deduced from them, there being no such thing as inductive rules of reasoning from data to theories (Peirce, 1931-58; Schiller, 1917, 1921); (7) *falsificationism,* which asserts, in qualification of hypothetico-deductivism, that there is nothing approaching even tentative verification in science, all scientific knowledge being irredeemably conjectural in character, progress being made only through the empirical falsification of theories (Popper, 1959); (8) *standard theoretical pluralism,* which asserts, in addition to falsificationism, that existing theories can only be severely tested if many rival testable theories are persistently developed, since every genuine test is invariably a crucial experiment attempting to decide between rival hypotheses (Feyerabend, 1965); (9) *paradigmism,* which asserts that initially empirically successful or progressive theories – paradigms or hard cores – are accepted and developed within a research tradition, until a rival paradigm or hard core supporting a rival research tradition becomes more empirically progressive, in which case the new paradigm is adopted (Kuhn, 1962; Lakatos, 1970); (10) *standard implicit craftism,* which asserts that the empirical assessment of scientific results is a craft which cannot be adequately encapsulated in any neat set of explicit rules or methods (Polanyi, 1958; Ravetz, 1971); (11) *standard instrumentalism,* which asserts that knowledge in science is confined to empirical laws and data, it being impermissible to interpret high-level theories as embodying

knowledge of an unobservable real physical world – theories being no more than devices for systematizing empirical knowledge, or being implicit definitions of key scientific terms (Duhem, 1962; Poincaré, 1952; van Fraassen, 1980); (12) *standard theoretical realism,* which asserts that it is legitimate to interpret unrefuted scientific theories as tentative conjectures at least about the nature of unobservable physical reality (Popper, 1963; Smart, 1963).[15]

It must, of course, be acknowledged that this list does not even begin to do justice to the variety and misguided sophistication of standard empiricist thought published in recent years. It must also be acknowledged that a few thinkers have rejected standard empiricism (though not necessarily the looser, broader philosophy of knowledge). Notably Russell (1948), in attempting to solve the problem of induction, argued that science must be interpreted as presupposing that nature is uniform and lawful – a metaphysical doctrine to be presupposed to be true independently of all empirical considerations. Even more notably, Einstein (1973), in his later years, repeatedly affirmed his conviction that the universe is comprehensible, and his conviction that science could not sensibly proceed without presupposing the universe to be comprehensible. These views are closer to the philosophy of natural science to be outlined in chapters 5 and 9 as a part of the philosophy of wisdom.

One standard contentious issue within the philosophy of knowledge is the question of how broadly or narrowly the scope of science is to be conceived. There are many natural scientists who hold that the term 'science' should be restricted to the physical and biological sciences, humanistic disciplines such as psychology, sociology, history or anthropology being unworthy of being deemed to be a part of science in that they have failed to develop sufficiently powerful predictive and explanatory theories.[16] There are many scholars in

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[15] Since the publication of the first edition of this book in 1984, Snow's 'technique of the intricate defensive' has been brilliantly developed further by the exploitation of specialization: philosophy of science has been split into distinct philosophical studies of distinct sciences, the serious problems that confront standard empiricism becoming even more obscured, or at least unfashionable, as a result.

[16] Ulam expresses an attitude common amongst natural scientists and mathematicians when he writes: 'In social science, a layman like myself feels that there is no theory or deeper knowledge at the present time. Perhaps this is due to my ignorance but I often have the feeling that by just observing the scene or reading, say, the *New York Times,* one can have as much foresight or knowledge in economics as the great experts. I don't think that for the present they have the slightest idea what causes the major economic or socio-political phenomena except for the trivialities everyone should know' (Ulam, 1976, p. 301). Ziman

the humanities who enthusiastically agree with this general view as to where the demarcation line between science and non-science is to be drawn – even if perhaps for somewhat different motives. Historians of ideas, art critics, literary critics, historians in general and others of this persuasion deplore the attempt to turn humanistic studies into empirical science: for them, detailed, sympathetic, illuminating and insightful knowledge and understanding of aspects of human life and human creations cannot, in the nature of things, be achieved by the simplistic, factual, empirical approach of science. The attempt leads only to dull and useless collections of facts or, even worse, to empty theoretical verbiage, that is neither science nor scholarship.[17]

Ranging against this fairly orthodox position, with advocates in both the natural sciences and the humanities, there is the view that scientific method can be, and ought to be, fruitfully employed in such human disciplines as economics, psychology, anthropology, sociology, politics and history. This general viewpoint in turn splits into two opposing camps. On the one hand there are those who, like Eysenk (1965), Skinner (1973), Broadbent (1973), believe that scientific method is the same wherever it is to be employed, the methods of the social sciences thus being the same as those of the physical and biological sciences – impartial appraisal of claims to knowledge by means of empirical data. On the other hand there are those who, like Giddens (1976), hold that the social sciences must adopt methods that are in important ways different from those of the natural sciences. In studying aspects of the human world – as one main argument for this position holds – we study ourselves, that which we in part create, something not encountered within the natural sciences: this important difference between the natural and social sciences requires that the two sorts of science adopt methods that are in important respects different.[18] In addition there are somewhat more philosophical demarcation debates about what precisely is to be demarcated

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comes to much the same conclusion, even though he puts it more cautiously and politely, see Ziman (1968, pp. 26-9, 1978, ch. 7).

17 A good example of someone who holds this sort of view in Isaiah Berlin. See, for example, Berlin (1980, 1979). The attitude pervades much of Berlin's work, and is especially marked in his defence and celebration of the significance of Vico: see Berlin (1976).

18 R. D. Laing puts the matter like this: 'It seems extraordinary that whereas the physical and biological sciences of it-processes have generally won the day against tendencies to personalize the world of things or to read human intentions into the animal world, an authentic science of persons has hardly got started by reason of the inveterate tendency to depersonalize or reify persons' (1965, p. 23).

from what, and for what reason.[19] The point I wish to stress here is that these debates about how science is to be demarcated from non-science are all debates *within* the overall framework of the philosophy of knowledge, unthinkingly taking this framework for granted. Even those hostile to the spread of scientific method into humanistic disciplines nevertheless take for granted basic tenets of the philosophy of knowledge (and are thus already profoundly, if unconsciously, influenced by a generalization of one conception of scientific method).

Furthermore, a range of different versions of the general doctrine of the philosophy of knowledge have also been developed. Some versions, as propounded for example by Brentano, Frege or Popper, conceive of the intellectual domain of inquiry primarily in an impersonal or non-social way, as an autonomous realm of intellectual entities, such as propositions. Other versions, as propounded, for example, by Locke, Hume or Kant, conceive of inquiry in a rather more individualistic way, in terms of ideas entertained, or judgements made, by individual minds. Then again, other versions, as propounded, for example, by Polanyi, Barnes, Bloor or Ziman, conceive of the intellectual domain primarily in institutional or social terms, as a component or aspect of social life. Some versions of the philosophy of knowledge, as upheld perhaps by Einstein, Popper or Polanyi, stress the fundamental importance of knowledge pursued for its own sake, of so-called 'pure' science and scholarship. Other versions, as advocated by Bernal, for example, stress the importance of the practical, pragmatic or technological aspect of science and knowledge. The philosophy of knowledge is not, as I have noted, committed to the fallacious view, implicit in the terms 'pure' and 'applied' science, that theoretical explanation and understanding of phenomena invariably come before technological knowledge and development. The philosophy of knowledge can accommodate the point that technological knowledge is often developed in the absence of, and before, corresponding theoretical understanding is achieved – technological

[19] We may be concerned to demarcate: (a) empirical from non-empirical theories (b) scientific from non-scientific inquiry (c) science at its ideal best from merely competent shading into incompetent science (d) science from pseudo-science (e) knowledge from non-knowledge (f) rational from non-rational or irrational inquiry. We may be concerned not just with problems about how to draw lines of demarcation between these different domains, but in addition with problems of providing a rationale for drawing demarcation lines where proposed. Popper's famous solution to his problem of demarcation suffers from a tendency to collapse together (a) to (d), the traditional problem (e) being treated as if it were no more than an aspect of problem (a) or (b), the whole problem of providing a rationale for the proposed demarcation line being neglected.

developments and problems even on occasions directly stimulating subsequent theoretical developments, as in the case of Carnot's contributions to thermodynamics. Some upholders of the philosophy of knowledge may advocate that the search for knowledge be related to definite political ideals and objectives, for example those of socialism, liberalism, or of the free market system (this being permissible from an intellectual standpoint according to the philosophy of knowledge, so it may be argued, as long as only research aims and priorities are at issue). Others may be more concerned to stress that the search for knowledge be kept free from political convictions and programmes. Versions of the philosophy of knowledge may well concede, or even emphasize, that broad social, cultural, political and economic factors play a major role in influencing how science and scholarship develop (in permitting or encouraging the traditions and institutions of 'rigorous' inquiry into matters of fact to develop, in providing financial support for such research, and in influencing choice of research aims and priorities). All such views, however diverse they may be in other respects, nevertheless deserve to be considered versions of the philosophy of knowledge insofar as they hold that the basic intellectual aim of rational inquiry is to acquire knowledge, the intellectual worth of potential contributions to inquiry being assessed solely with respect to the contribution that they make to knowledge of truth, intellectual progress thus being distinct from the capacity of inquiry to promote social progress.

So far in this section I have discussed some intellectual or philosophical problems that confront the doctrines of standard empiricism and the philosophy of knowledge, and some of the orthodox responses that have been made to these problems. But there are also, much more seriously, humanitarian problems – as they may be called – that result from attempting to put these (intellectually defective) doctrines into actual academic practice. The academic enterprise is plagued by a range of cultural, educational, social, political and moral problems that all, in one way or another, have to do with the failure of modern science, technology, scholarship and education to be of value to people in life. There is the problem of the inherent triviality of much science and scholarship, the esoteric, jargon-ridden, specialized character of the research obscuring that it is devoid of any real intellectual or practical value, apart from a tendency to promote careers and flatter vanity. There is the scandal of the priorities of world scientific research, around one quarter of the world's budget for scientific and technological research being devoted to military research, some 95 per cent being spent in, and devoted to the interests of, the developed world. There is, as a result, the tendency of much science and technology to serve the interests of the wealthy and powerful,

often at the expense of the interests of the poor and powerless, in this way helping to increase inequality and injustice in the world. There are the horrors that scientific knowledge has made possible – nuclear bombs, intercontinental missiles, the means for chemical and biological warfare. There is the highly suspect role that scientific experts have played in actively promoting the nuclear arms race. There are all our modern problems of global warming, depletion of natural resources, pollution, rapid extinction of plants and animals, and the destruction of their natural habitats, caused by population growth, modern agriculture and industrial development made possible by science and technology.

Valuable contributions to an improved appreciation of major humanitarian problems associated with modern organized inquiry such as these have been made by Jungk (1960), Barzun (1964), Ellul (1964), Greenberg (1971), Roszak (1970), Ravetz (1971), Higgins (1978), Calder (1981), Zuckerman (1982) and many others. This body of work fails however to repudiate the philosophy of knowledge as a damagingly irrational conception of inquiry which urgently needs to be replaced by a more rational kind of inquiry pursued in accordance with the philosophy of wisdom. Either the discussion is confined to political and moral issues, the framework of the philosophy of knowledge being taken for granted (Greenberg); or some orthodox version of the philosophy of knowledge is propounded (Ravetz); or it is argued that the pursuit of knowledge needs to be committed to socialist as opposed to capitalist goals (Rose and Rose, 1976); or reason is identified with what amounts to the philosophy of knowledge, and it is argued that reason needs to be more severely restricted (Roszak) or even repudiated altogether (Feyerabend) – as if too much reason is the problem, rather than too much of a characteristic kind of irrationality masquerading as reason.

The over-riding impression from all the literature so far discussed, however, is that the two kinds of problems – intellectual problems confronting the philosophy of knowledge and humanitarian problems confronting the actual organized pursuit of knowledge in the world – have little to do with one another. This in itself accords with the philosophy of knowledge edict that intellectual and humanitarian problems must be dissociated from one another.

It is a central tenet of this book that the two kinds of problems are intimately interconnected. As I shall argue in chapter 9, many of the above intellectual problems cannot conceivably be solved within the framework of the philosophy of knowledge. Standard empiricism and the philosophy of knowledge, as ideals of rational inquiry, stand decisively refuted. As the above intellectual problems indicate, it is actually profoundly irrational to try

to acquire scientific knowledge about the world by selecting theories solely with respect to empirical success or failure; and more generally, it is profoundly irrational to attempt to help people realize what is of value in life by pursuing knowledge, solving problems of knowledge, in a way which is dissociated from a more fundamental intellectual concern with problems of living. If inquiry is *rationally* to help us realize what is of value to us in our lives, it is essential that it gives intellectual priority to our personal and social problems of living, problems of knowledge and technology being tackled in a way which is intimately associated intellectually with discussion of our problems of living, as the philosophy of wisdom requires. Once inquiry irrationally dissociates problems of knowledge from problems of living, as demanded by the philosophy of knowledge, almost inevitably the pursuit of knowledge will come to suffer from the kind of humanitarian defects indicated above.

The argument of this book, indeed, goes further than this. For my basic argument is that a major root cause of so many of the calamities of the twentieth century that humanity has inflicted on itself – the wars, the concentration camps, the totalitarian regimes, the poverty and starvation amidst plenty, the millions upon millions of lives unnecessarily devastated and destroyed – is our long-standing failure to have developed in the world a genuinely rational kind of inquiry devoted to helping us realize cooperatively what is of most value in life. Our self-inflicted calamities in the end result from our general failure to tackle our common problems in a cooperatively rational way: and this in turn is the consequence of our long-standing failure to develop socially influential traditions of inquiry and education devoted to the promotion of cooperative, rational problem-solving in life. In this way the intellectual disasters of the philosophy of knowledge are a distant echo of the human disasters suffered by so many people. In the circumstances, there can scarcely be any more important task for all those in any way concerned with science, technology, scholarship and education than to help develop a more rational kind of inquiry devoted to the promotion of social wisdom.

Proponents of standard empiricism and the philosophy of knowledge may acknowledge the importance of moral and social problems associated with science: they will not however – and here we see the cunning of the philosophy of knowledge – recognize these problems as in any way calling into doubt the integrity of science itself – or calling into doubt the whole way in which we at present conceive of science, or of rational inquiry more generally. For, of course, these are moral, political and social problems, and as such must be, as the philosophy of knowledge stipulates, entirely dissociated from scientific or intellectual problems of knowledge. As *human*

being a scientist may well be concerned about such issues; as *scientist* his task is to concern himself exclusively with problems of fact, truth and knowledge. As one author has put it, presupposing the philosophy of knowledge and defending a version of standard empiricism: '. . .it is commonplace to speak of progress, meaning an improvement in the material or the 'spiritual' conditions of life. Although that sense of progress is unquestionably important, I shall say virtually nothing about it in this essay. My exclusive preoccupation will be with what I call *'cognitive progress,'* which is nothing more nor less than *progress with respect to the intellectual aspirations of science.* Cognitive progress neither entails, nor is it entailed by, material, social, or spiritual progress' (Laudan, 1977, p.7). The success or failure of science, and of our conceptions of science are, in other words, to be judged solely with respect to the capacity of science to realize intellectual aims, disregarding entirely questions as to whether in pursuing these intellectual aims science helps to increase human happiness, or helps to increase human suffering, unnecessary death and injustice. It is this cavalier dismissal of problems of material and spiritual progress as having nothing to do with problems of intellectual progress – inevitable once the philosophy of knowledge is consistently presupposed – that would have so horrified people like Voltaire and Diderot, for whom science was in essence an engine for the promotion of human enlightenment. Carelessly, unthinkingly, the age of reason, the age of enlightenment, has been betrayed!

We shall see, however, in chapters 11 and 12, that during the last two or three decades a number of developments have taken place in different disciplines, often unrelated to one another, which taken together can be interpreted as constituting a general movement towards the philosophy of wisdom.

If this last point is correct, then it must also be said that this nascent intellectual revolution, from knowledge to wisdom, has so far proved to be somewhat fragmentary, confused and ineffective. The precise and comprehensive character of the change in intellectual aims and methods that is needed, and the precise reasons why this change is needed, have not so far been very clearly articulated, recognized or understood by those urging change in diverse disciplines towards a more humanitarian or socially committed kind of inquiry. In order to criticize the philosophy of knowledge it is not, for example, sufficient to argue that all knowledge is value-laden. A proponent of the philosophy of knowledge will interpret this to be either a platitude (in that what is being asserted is merely that values influence us in what we decide to acquire knowledge about) or a gross fallacy (in that what is being asserted is that factual knowledge always contains an evaluative

component so that the truth or falsity of factual propositions depend on value judgements). Nor, in order to criticize the philosophy of knowledge, is it sufficient to argue merely that science and scholarship ought to be more socially concerned, committed or responsible: proponents of the philosophy of knowledge can readily agree. Indeed it is not sufficient at all merely to criticize the philosophy of knowledge, however cogent and decisive such criticism may be. What is needed rather, is what this book seeks to supply, a clearly formulated alternative to the philosophy of knowledge that is demonstrably more rigorous intellectually and more useful and valuable socially (at least potentially) than what we have at present. Even though such an alternative has been available since the publication of the first edition of this book in 1984, its existence has not been widely known. And as a result the noble efforts of many individuals in diverse contexts to help develop a more enlightened kind of science, technology, scholarship and education, more intelligently, sensitively and effectively devoted to serving the real interests and aspirations of people in life, remain frustrated, ineffective, misunderstood.

In the circumstances it is not surprising that the philosophy of knowledge (more or less as formulated above) continues to be in practice overwhelmingly the dominant intellectual creed of the academic enterprise, exercising its influence over almost every aspect of science, technology, scholarship and education. It influences such things as: what is to count as a contribution to inquiry; criteria of acceptance of potential contributions for publication in academic journals and books; the kind of criticism that is to be permitted to filter into the intellectual domain of inquiry; the content of academic courses, lectures and seminars; conceptions of scientific and intellectual progress; intellectual values and priorities; the content and style of academic contributions and discussion; the accessibility or non-accessibility of academic discussion to non-academics; the awarding of academic qualifications and prizes; academic careers and promotions; the manner in which intellectual research receives, or fails to receive, financial support; criteria for choice of research aims to be actively pursued; the power structure of academic institutions and disciplines; the way in which diverse academic disciplines are differentiated from one another, and interrelated with one another; the role of experts in economic, industrial, cultural and political life; the whole way in which the academic enterprise is related to the rest of the human world. There is hardly any aspect of scientific and technological research, scholarship and education that is not affected by the almost universal adoption in practice of some version of the philosophy of knowledge. There is no discipline or speciality that remains unaffected.

In criticizing the philosophy of knowledge, and in advocating that it be replaced by the philosophy of wisdom, my primary concern is with the doctrine as embodied in academic practice and in the whole institutional structure of the academic enterprise: I am not concerned only to criticize doctrines expounded by philosophers, historians and sociologists of inquiry. My primary concern is with what academics do, rather than with what they claim to be doing. Or rather: my primary concern is with the preservation and growth of what is of value in human life, concern for the actual aims and methods of academic inquiry being secondary and subordinate to that.

Chapter Three
The Basic Objection to the Philosophy of Knowledge

Some objections to the philosophy of knowledge have already been indicated. I now state what is, in my view, the central, fundamental objection. It is, I suggest, both simple and decisive. Inquiry pursued in accordance with the philosophy of knowledge violates the most elementary requirements for rationality conceivable – and as a result inevitably tends, in characteristic ways, to betray the interests of humanity.

An elementary requirement for rationality is that, in seeking to solve problems we (1) articulate, and seek to improve the articulation of, the basic problems we hope to solve, and (2) propose and critically assess possible solutions.[1] To this one might add that when we break up our primary, basic problems into a number of subordinate, secondary problems, we (3) tackle these subordinate problems in close association with our primary problems, so that subordinate and primary problems continue to be relevant to each other as we proceed.

It is just these absolutely elementary, general requirements for rationality which are utterly violated if inquiry is pursued in accordance with the philosophy of knowledge.

For what are the basic problems that inquiry, pursued in accordance with the philosophy of knowledge, is designed to solve? The basic (humanitarian) aim of inquiry, let it be remembered, is to help promote human welfare, help people realize what is of value to them in life – knowledge being pursued as a means to this end. But in order to realize what is of value to us in life, the primary problems that we need to solve are problems of *action* – personal and social problems of action as encountered in life. From the standpoint of achieving what is of value in life, problems of knowledge and technology are invariably subordinate and secondary to problems of action. Solutions to problems of knowledge and technology contribute to the realization of value in life by extending our capacity to act.

Thus, if inquiry is to pursue its basic (humanitarian) aim of helping us to realize what is of value in life in a way which accords with the above elementary requirements for rationality, then inquiry must give absolute intellectual priority to the tasks of (1) articulating our problems of action and

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[1] '. . . the method of all *rational discussion* . . . is that of stating one's problem clearly and of examining its various proposed solutions *critically*' (Popper, 1959, p. 16).

(2) proposing and critically assessing possible solutions – possible personal and social actions. Furthermore, inquiry must (3) tackle subordinate, secondary problems of knowledge and technology in close association with problems of action, so that problems of knowledge and technology continue to be relevant to those problems of action we need to solve in order to realize what is of value to us in life.

It is just these elementary requirements for rationality that inquiry pursued in accordance with the philosophy of knowledge violates. Far from intellectual priority being given to the tasks of articulating problems of living, proposing and criticizing possible solutions – problems of knowledge and technology being tackled as rationally related subordinate, secondary problems – it is all the other way round: problems of knowledge and technology are tackled in a way that is intellectually dissociated from problems of living, the latter, indeed, being excluded from the intellectual domain of inquiry altogether.

In short, inquiry pursued in accordance with the philosophy of knowledge makes the disastrous intellectual mistake – from the standpoint of contributing to the realization of what is of value in life – of giving sustained attention to subordinate, peripheral problems (of knowledge and technology), while discussion of the primary, problems (of personal and social action) are excluded from the intellectual domain of inquiry altogether.[2]

Inevitably, profoundly undesirable consequences result for all aspects of human life (including inquiry itself) if organized inquiry is irrationally restricted to solving problems of knowledge intellectually dissociated from problems of living – as demanded by the philosophy of knowledge in a misguided attempt to preserve reason. I now indicate six such inevitable undesirable consequences (to be further elaborated during the course of the rest of this book). I also indicate, where relevant, how these consequences manifest themselves *in reality*, as a result of organized inquiry *in reality* conforming to the irrational edicts of the philosophy of knowledge. I have already given some grounds for holding that much of the academic enterprise does in reality conform to the philosophy of knowledge: further, more substantial grounds will be given in chapter 6.

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[2] This objection does not apply to that version of the philosophy of knowledge, referred to in footnote 6 to chapter 2, according to which the basic aim of inquiry is merely to acquire knowledge irrespective of whether this is of human value of not. This modest version of the philosophy of knowledge will however be refuted in chapter 9.

1 *There are profoundly undesirable consequences for the general quality of human life*. If people everywhere are to have their best chances of realizing what is of value to them in life, then it is essential that people everywhere can tackle in cooperatively rational ways their common problems of living. The greater the general failure to do this the more unnecessary human failure, suffering and death there will be in the world. But in order for cooperative rationality to develop as an integral part of living, it is essential that institutions of learning – schools and universities – devote themselves to promoting cooperative rationality in life. It is essential that scientific and technological research, scholarship and education give absolute intellectual priority to the tasks of articulating problems of living, and proposing and criticizing possible solutions. In refraining from doing this – as a result of complying with the philosophy of knowledge – organized inquiry fails to do what it most needs to do if it is to help people everywhere realize what is of value to them in life. In scrupulously restricting themselves in their professional capacity to the pursuit of knowledge, scientists, scholars and teachers ignore their central, most vital professional task: to help promote the cooperative rational search for what is of most value in personal and social life. Simultaneously, reason and humanity are betrayed.

If all questions about what we want, what is of value in life, and what we need to do in order to realize what is of value, were entirely unproblematic and uncontroversial, then it might be reasonable to exclude consideration of such questions from rational inquiry. If such questions somehow lay irredeemably beyond the reach of reason, then it would be necessary to exclude them from rational inquiry. In either of these cases, rational inquiry could only be of benefit to humanity by providing knowledge and technology, as Francis Bacon in effect supposed. But both these suppositions are false. Questions about what problems of living we should try to solve, what actions we should perform, in order to achieve what is of value are profoundly problematic and controversial. They can be, and urgently need to be, tackled rationally, at the very least in accordance with the basic strategies of rational problem solving (1), (2) and (3) indicated above. It is here, indeed, that our greatest failures lie, and our greatest need for rational learning exists. Almost all our major social problems exist not because of lack of knowledge and technology, but rather because of a general failure to develop in the world traditions of cooperative rational problem-solving and learning devoted to enabling people to realize lives of value and justice. Consider the following major social problems confronting humanity today, already referred to at the beginning of chapter 1: problems of extreme poverty, of disease, malnutrition and starvation experienced by millions of people in the poorest

parts of the world; the spread of HIV and aids; problems posed by the existence of dictatorships maintained by force, all political opposition being suppressed, elementary political and civil rights being annihilated, non-violent critics of the regimes suffering arbitrary arrest, imprisonment, and even torture and execution; problems posed by vast inequalities of wealth and power between people, both within nations, and between nations; problems posed by the spread of armaments, conventional, chemical, biological and nuclear; problems of war, within nations and between nations; problems posed by terrorism and the response to terrorism; and, perhaps most serious of all, problems posed by impending global warming. In order for these problems to be progressively resolved in just and humane ways it is necessary for millions upon millions of people to act in new, appropriate ways, in rational response to the problems individually and cooperatively. Even when it is necessary to develop new knowledge and technology in order progressively to resolve such problems – knowledge relevant for the assessment of proposals for action, for example, or technology relevant for the curing of disease, for the production of food or for birth control – nevertheless such knowledge and technology only assists the just and humane resolution of such social problems insofar as it helps to make it possible for people to perform appropriate cooperative actions (knowledge and technology on their own resolving no such problems of living). The overwhelming need is for millions upon millions of people to discover how to act in more cooperative rational ways than at present, in response to their common and differing problems: and it is this desperately important need that organized inquiry ignores when it restricts itself with scrupulous irrationality to the pursuit of knowledge. It is, of course, true that the existence in the world of a tradition of imaginative, open, public, critical, humane and cooperative discussion of basic human problems of living and how they are to be solved is not in itself *sufficient* to ensure that such problems will be tackled in practice in rational, cooperative, humane ways. Rational discussion does not ensure rational action. The existence of such a tradition is however, I maintain, a *necessary* condition for rational social action. In our vast, complex, diverse, interdependent, rapidly changing human world there is no chance that more rational, cooperative, humane ways of tackling our major social problems will develop in the absence of sustained *discussion* of how such problems are to be tackled, diffused throughout the social world. Only by cooperatively imagining and criticizing many possible actions (the heart of reason) can people discover those rare, complex, coordinated actions which permit everyone to benefit. It is just this which makes it a matter of such urgency that organized inquiry should take

up its proper, fundamental intellectual-social task of helping to promote and sustain such discussion, so that it becomes capable of guiding social action.

At present irrationality in life is everywhere apparent. It is apparent in the lamentable failure of humanity to resolve the appalling social problems just indicated. But quite apart from the persistent horrors of the last hundred years or so, our general failure to develop cooperative rationality in the world is apparent even in the most democratic and liberal societies in existence today, in that in such societies institutions everywhere are organized on hierarchical rather than cooperative lines, with the few people at the top making decisions that the many are to carry out without question. In Britain government is conducted with absurd secrecy, the population being ruled almost like children. (The recent freedom of information act has not helped much.) Irrationality is manifest in the crudity of the ideals and creeds that govern people's lives – religious, moral, political, economic – the most influential doctrines often being inherited almost unchanged from the nineteenth or eighteenth century – even further back in the case of religion. Irrationality is even more strikingly apparent in the attitudes that so many people adopt to the creeds that govern their lives. Instead of adopting the rationalist attitude that all such creeds amount to no more than inevitably imperfect proposed solutions to life's problems, to be fiercely criticized and improved on wherever possible, just the opposite attitude prevails. Upholders of such doctrines – whether religious or political – all too often regard all doubt and criticism as inherently bad and hostile. In science it is a commonplace that progress is achieved because of a persistent endeavour to criticize and improve existing theories. Everywhere in personal and public life one finds the exact opposite of this: doctrines dominating personal and social life are fiercely protected from criticism and improvement. Or criticism takes the form of licensed tomfoolery, reactionary in import in that it gives the appearance of effective criticism while allowing everything to proceed as usual, unaffected. The result is that we are burdened in our personal and social lives with political and religious doctrines – proposals for living – which have been protected from criticism and improvement since their first advocacy, and which, as a result, are grotesquely irrelevant to our present circumstances and problems. Ideas dominating our lives are treated in ways which violate utterly even the most elementary of requirements for rationality, indicated above. The result is that only pitifully slow, intermittent progress is made in developing ideas more adequate and conducive to the realization of value in life. And the result of this in turn is that our *lives* make only pitifully slow, intermittent progress towards the realization of what is of value.

The academic enterprise bears a heavy burden of responsibility for the persistence of this damaging irrationality that pervades the world in failing, over the last century or so, to develop a kind of organized inquiry wholly devoted to the promotion of rationality in life.

Without doubt this is by far the most serious and general undesirable consequence of pursuing inquiry in accordance with the irrational precepts of the philosophy of knowledge. The remaining five undesirable consequences in effect amount to special cases of the above general undesirable consequence.

2 *There are undesirable consequences for the quality of human life as a result of scientific and technological progress*. In a world where humane cooperative rationality prevails, scientific and technological progress is bound to be beneficial (setting aside unlucky accidents). But in a world where such rationality is largely absent, scientific and technological progress is as likely to lead to human suffering and death as to human good. In a world where there are immense injustices, persistent, violent conflicts between people, and where national and international politics are often conducted at the moral level of gang warfare, the products of scientific and technological progress, however nobly sought, and however potentially beneficial to humanity, will be used to imprison, enslave and kill. Even a mere lack of cooperative rationality in human affairs can have the outcome that new technology, potentially beneficial, is used in ways that cause much unintended and unforeseen human suffering and injustice. Thus research in pure physics, nobly motivated, led to the possibility of the atomic bomb. The Manhattan project, motivated originally by the understandable desire to ensure that Hitler should not alone possess the atomic bomb, led to such bombs being dropped on Hiroshima and Nagasaki, and to the subsequent superpower nuclear arms race, which exacerbated the cold war, and might well have led to nuclear Armageddon. The so-called 'green revolution', carried through in order to increase food production so that the hungry may eat, has, in many places, not had this effect owing to economic and social conditions, such as grossly unequal distribution of ownership of land. The development of automation and artificial intelligence, potentially enormously beneficial, nevertheless in practice also threatens to create human suffering in that it helps to create unemployment. Lead used to be added to petrol to solve the 'knocking' problem of motor car engines, even though it was known that ingested lead causes brain damage, especially in young children. Industrialization of the wealthy nations, made possible by science and technology to a considerable extent, makes it possible for the wealthy nations of the world to use up an

inordinate share of the world's natural resources, at the expense of the poor in the third world. In this way science and technology, via industrialization, make possible the development of vast inequalities of wealth and power, a state of extreme international injustice.

The scientific community cannot of course be held to be solely responsible for the suffering and death caused by the products of scientific research in the hands of others. It can however be held to be responsible for the fact that scientific and technological inquiry have been developed in such a way that they are dissociated from a more fundamental intellectual concern to promote cooperative rationality in life. This is the great intellectual and moral sin of the scientific community.

3 *There are undesirable consequences for scientific and technological research itself – for the priorities of research.* If there is a general lack of cooperative rationality in the world, not only will this lead to the products of scientific research being used in harmful ways: it will also lead to science itself becoming corrupted, in that the aims, the priorities, of scientific research will come to be corrupted. Instead of the aims and priorities of research being intelligently chosen so as to help relieve human suffering, help promote human welfare, on the contrary, in all likelihood, the aims and priorities of research will come to reflect merely the special interests of the scientific/academic community itself, and the interests of those who have sufficient wealth and power to fund and guide research. This is almost bound to occur once the search for new knowledge and technology is irrationally dissociated from a more fundamental endeavour to promote cooperative rational problem-solving in life.

Writing in the seventeenth century, Robert Boyle, one of the founding fathers of modern science, had this to say about what he called the 'Invisible College' – a sort of forerunner of the Royal Society, and thus of organized scientific research. 'The 'Invisible College' [consists of] persons that endeavour to put narrow-mindedness out of countenance by the practice of so extensive a charity that it reaches unto everything called man, and nothing less than an universal good-will can content it. And indeed they are so apprehensive of the want of good employment that they take the whole body of mankind for their care.' (Quoted in Werskey, 1978, p. 13.) A modern science and technology that put into practice the spirit of Boyle's Invisible College – thus genuinely devoting itself to the welfare of humanity – would today clearly give priority to the problems and needs of the poorest people on earth. Problems of third-world sanitation, agriculture, malnutrition, disease, housing, transport, education, appropriate technology, would be the

central focus of much of the world's scientific and technological research. The social sciences would be centrally concerned with the manifold social, cultural, economic, psychological, political and international problems associated with the plight of the world's poor, especially having to do with the way in which the rest of the world interacts with the third world.

Does modern science succeed in devoting itself to the interests of humanity in this way? The answer must surely be that it does not. As we have noted, something like 95 per cent of the world's expenditure on scientific and technological research supports research conducted in the developed world, being devoted primarily to basic (or 'pure') research, military research, and research related to the economic, industrial and social needs of the developed world. It has been estimated that roughly a quarter of the world's investment in research and development is spent on military research, over half a million scientists working on the development of new weapons.[3] Some 15 per cent of the world's research budget is spent on pure science, much of this being siphoned off into high energy physics, of little conceivable potential relevance or interest to the world's poor. Only a very small fraction of the world's scientific and technological research is devoted to the problems of poor people living in the third world.[4] It is clear that modern science and technology fail quite lamentably to 'take the whole body of mankind for their care'.

Let me emphasize again that this state of affairs is almost bound to arise once the scientific/academic enterprise suffers from the characteristic kind of irrationality advocated by the philosophy of knowledge. It is of course in any case entirely to be expected that the wealthy and powerful will try to subvert scientific and technological research so that it serves their own interests. However, if the scientific/academic community put into practice the view that its basic intellectual and professional task is to promote cooperative rationality in life, then there would at least be general agreement that it is the professional duty of every scientist and scholar to try to discover and draw attention to the often subversive influence of wealth and power

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[3] Not much seems to have changed with the end of the cold war. In 2003/4 the UK spent approximately £2.7 billion on military research and development (R&D), about 30% of the total UK annual R&D budget, 40% of government R&D personnel being employed by the Ministry of Defence. The USA spends more than half its R&D budget on military technology – in 2004 the USA Department of Defence budget for R&D was $62.8 billion. See Langley (2005).

[4] For a more detailed discussion of these points see Norman (1981, especially chs. 3 and 4). See also Smith (2003) and Langley (2005).

and, where possible, to check and oppose it. From this perspective, scientific and technological problem-solving massively unrelated to problems of living of those whose needs are the greatest, is both irrational and immoral. But once the philosophy of knowledge is accepted and put into scientific/academic practice, exactly the opposite situation prevails. From this perspective, the fact that scientific and technological problem-solving is massively unrelated to, or even in direct opposition to, the most urgent and desperate needs of humanity, does not in any way call into question the rationality or the morality of science itself. For, according to the philosophy of knowledge, the primary – perhaps the only – intellectual and professional obligation of the researcher is to acquire authentic, objective knowledge, unrelated to any programme or ideology for bettering the general condition of humanity. It is indeed a primary duty of the scientist – so proponents of the philosophy of knowledge may argue – to *dissociate* the search for knowledge from any political or ideological programme or viewpoint. This must be done precisely so that science may genuinely serve the interests of humanity – by producing genuine, objective knowledge. Thus as long as modern science and technology produce authentic knowledge and reliable technology, there can be no intellectual or moral failing internal to science itself – even if science and technology happen to benefit the wealthy rather than the poor. To try to commit research to a more humanitarian programme would actually be to *subvert* the objectivity, the intellectual integrity, the scientific character, of science. It would actually go against the real interests of humanity!

A scientist must not fake his results, and any scientist caught doing this will immediately be ostracized by the scientific community. It seems that scientists may, however, with impunity produce all sorts of fake arguments when it comes to gaining funds for research projects, just because, according to the philosophy of knowledge, the whole issue of research aims and priorities lies outside the domain of the rational, the scientifically, objectively discussable and assessable. Leading scientists may employ such intellectually disreputable arguments as that support for research in high energy physics is essential for economic development, essential for the preservation of science and civilization, and far from being ostracized, such scientists, if successful, will be showered with scientific rewards and honours.[5] That which is intellectually and morally disreputable from the

[5] Numerous examples of leading scientists employing such intellectually disreputable arguments as these in order to obtain funds for research are described, with scathing comments, by Greenberg (1971). Greenberg is concerned primarily with the politics and

standpoint of a kind of inquiry devoted to promoting cooperative rationality in life becomes wholly honorable from the standpoint of the philosophy of knowledge, it being rather systematic criticism of research priorities that becomes intellectually disreputable. In this way, acceptance of the philosophy of knowledge not only blinds the scientific community to the moral and intellectual scandal inherent in the priorities of current scientific research; it has the further effect of transforming legitimate criticism of the *status quo* into a dangerous threat to the objectivity, intellectual integrity, and scientific character, of science. Legitimate criticism is ostracized wholesale as irrational and ideological.[6]

A further point is this. Even if there is the desire, and the power, to develop scientific and technological research aims and priorities in directions genuinely of maximum value to humanity, nevertheless it is still highly problematic to make good choices of research aims and priorities. For in order to make such good choices we must bring together good metaphysical, scientific, social and value decisions concerning such things as the domain of our ignorance, what is potentially scientifically discoverable, what is most urgently needed by people, today and in decades to come. We must discover that highly problematic region of overlap between the scientifically discoverable and the humanly desirable. A kind of inquiry that gives

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the immorality of pure research in the USA in the years 1945-70. He gives a number of examples of scientists putting forward arguments such as that science 'has now become the basis for the advance of our economy' thereby echoing the litany that proponents of basic research have regularly uttered since the establishment of the science-government partnership at the end of World War II (p. 30). Greenberg goes on to point out how deplorable is the case for supposing that basic research has much to do with economic growth. He also remarks 'the cathedral metaphor occurs repeatedly in the public pronouncements of the statesmen of science, as, for example, in the words of Philip Handler, chairman of the biochemistry department at Duke University, chairman of the National Science Board, and a member of the President's Science Advisory Committee: The edifice which is being created by science . . . is fully comparable to the cathedrals of the Middle Ages or the art of the Renaissance . . . ' and Greenberg comments 'that the building of pyramids and cathedrals exacted a monstrous toll from the masses that were supposedly elevated by these edifices is never discussed' (p. 35).

6 'One of the leading statesmen of pure research privately protested the publication in *Science* of a news article that described the Hindsight report' [a Defence Department report arguing that weapons development gained little from pure research]. 'Description of so heretical a thesis, he felt, was tantamount to advocacy of it' (Greenberg, 1971, p. 31).

intellectual priority to promoting cooperative rationality in life has the capacity to help us make good decisions about these matters, in that it demands that inquiry incorporates explicit, imaginative, and critical discussion of actual and possible aims and priorities for scientific and technological research, rationally related to explicit, sustained, imaginative, and critical discussion of problems confronting people in their lives. The methodology, the intellectual standards, of inquiry of this type are thus designed to help us to develop and choose aims and priorities for scientific and technological research best designed to be of greatest value to people in their lives, to be of value to humanity, with justice.

In contrast to this, the philosophy of knowledge banishes discussion of research aims and priorities – and discussion of life aims and priorities of people – from the intellectual domain of inquiry. In particular, the philosophy of *science* of standard empiricism – the hard core of the philosophy of knowledge – banishes discussion of untestable ideas (metaphysical or evaluative) from the intellectual domain of science. Thus the methodology, the intellectual standards, of the philosophy of knowledge, far from aiding, actually seriously obstruct, the task of discovering and choosing good aims and priorities for scientific research. Instead of there being sustained imaginative and critical discussion of actual and possible research aims as an integral part of scientific discussion – in scientific papers, monographs, textbooks, lectures, seminars and conferences – the debate rather is confined primarily to those with power to decide, such as grant-giving bodies, heads of research institutions, those concerned to determine science policy.[7]

4 *There are undesirable consequences for the cultural (or 'pure') dimension of scholarship and science.* All branches of inquiry in the end owe their intellectual value to their capacity to enable people to realize what is of personal value in life. This obviously holds for the practical or technological dimension of inquiry: it also holds, I claim, for the cultural dimension of inquiry. Whether the subject is history, anthropology, cosmology, philosophy or pure mathematics, the discipline is of intellectual value, from a cultural standpoint, insofar as it can be used by people to extend their own personal capacity to see, experience, know, understand or appreciate significant aspects of the world, or

[7] Chapter 6 gives an indication of the extent to which discussion of actual and possible scientific research aims, and how these do and might relate to problems and aims of living, is in practice excluded from the intellectual domain of science, as defined by 'science abstracts'.

significant possibilities. Personal (and interpersonal) inquiry is what ultimately matters: impersonal inquiry is but a means to that end. As Einstein once remarked: 'Knowledge exists in two forms – lifeless, stored in books, and alive in the consciousness of men. The second form of existence is after all the essential one; the first, indispensable as it may be, occupies only an inferior position' (Einstein, 1973, p. 80). All inquiry – practical *and* cultural – is in the end to be evaluated from the standpoint of its capacity to enrich human life.

However, if inquiry is to be of value in this way, it is absolutely essential that priority is given, within inquiry, to the *activity* of people (1) articulating their own personal problems of knowledge and understanding, and (2) proposing and criticizing possible solutions. All other more impersonal intellectual problems need to be tackled as elaborations of personal problems, as secondary and subordinate to primary, personal problems of knowledge and understanding. If this is done, then the cultural dimension of inquiry can flourish, in that impersonal intellectual problems of scholarship and science can become sensitively and intelligently responsive to personal problems of knowledge and understanding encountered by people in life. It becomes possible to pursue the humanities and social inquiry in such a way that they are devoted to helping people articulate each other's problems of living and each other's possible and attempted solutions, so that people separated by space, language or culture may enter imaginatively into each other's lives, thus improving mutual understanding – of value in itself, and of value in that it is essential for cooperative rational action. (In the case of history, of course, communication can proceed in one direction only.) In this way, what is potentially best in the humanities and social inquiry can flourish. Equally, it becomes possible to pursue natural philosophy as the cooperative outcome of the passionately personal endeavour to improve personal knowledge and understanding of the natural world. Children, on being introduced to cosmology, can begin by articulating problems of understanding confronting their own childish cosmological ideas (see Maxwell, 2005c, 2007a, section 7). Proposed solutions to such problems offered by others can also be considered, so that from the outset the ideas of Democritus, Kepler, Galileo, Newton, Darwin, Einstein and others can be used to develop and solve one's own personal problems of understanding. Just as in history or anthropology we endeavour imaginatively to see the world as seen by others, so too in science we can endeavour to see the world as seen by Boscovich, Faraday, Pasteur, Planck or Weinberg, thus improving our own understanding of nature. As a result of using public science in this way. we can come to appreciate both the cooperative, and the passionately personal character of

the best of science. We can share in the noble quest to understand revealed in the lives of people like Kepler, Faraday, Darwin or Einstein. The best of what there is in science, from a cultural standpoint, can flourish.

All this is sabotaged when scholarly and scientific research is sharply dissociated from personal problem-solving in life as demanded by the philosophy of knowledge. For, once scholarship and science become dissociated intellectually from the endeavours of people outside universities to improve their personal knowledge and understanding, the vital personal and interpersonal dimension of inquiry tends to disappear from view. Scholarship and science tend to become esoteric, formal, scholastic and decadent, remote from the interests and concerns of non-academic life, pursued for the sake of academic career and status rather than for the sake of shared personal understanding. Social inquiry fails to promote person-to-person understanding between people in the world; natural science fails to promote cooperative, personal understanding of the natural world.

These undesirable consequences of irrationally dissociating scholarly and scientific problem-solving from personal problem-solving in life are, I suggest, everywhere apparent in the modern academic world.

5 *There are particularly undesirable consequences for inquiry, when the object of study is ourselves, aspects of our human world – as a result of social inquiry being pursued as social 'science'.* Instead of helping us to see, to discover, what is of most value in people, in institutions, in artefacts created by people, rather the social 'sciences' (pursued in accordance with the philosophy of knowledge), eschewing 'value' in order to be 'objective', 'factual' and 'scientific', must inevitably invite us to see people, society and culture, in a value-denuded way, thus obstructing our capacity to see value in life. Instead of giving priority to the task of articulating problems of living, proposing and criticizing possible solutions, the social 'sciences' must rather confine themselves to acquiring knowledge, profoundly influential assumptions about human problems and their possible resolution being placed beyond critical discussion. Not only does this deflect attention from the central task of articulating our problems of living, proposing and criticizing possible solutions. In addition it must tend to lead social 'scientists' to advocate, in a wholly surreptitious fashion, ways of conceiving of our problems and how they are to be solved when they ostensibly only advocate neutral items of social knowledge. For inevitably, built into supposedly purely factual descriptions and theoretical explanations of social phenomena – in economics, sociology, social psychology, educational psychology and so on – there must be some implicit presuppositions about what our problems are, what it is possible and

desirable to do in order to solve them, in these fields. Such presuppositions about what really matters remain, however, hidden and repudiated. Thus in seeking to make the social sciences rigorously factual and scientific, social scientists actually help to sabotage rationality in life. In ostensibly propounding factual knowledge they surreptitiously advocate approaches to life and its problems, at the same time in all honesty denying any such thing is being done, the intimated views as to what our problems are and how they are to be solved thus being placed beyond criticism, even beyond recognition. Yet again, instead of seeking to help improve the understanding that people have of each other in society, the social 'sciences' (pursued in accordance with the philosophy of knowledge) must rather give priority to the improvement of knowledge and understanding of people and social phenomena within the social 'sciences' themselves, of a specialized, professional kind, dissociated from and often unintelligible to people in society.

The idea that the chief aim of social inquiry ought to be to develop specialized knowledge of social phenomena may indeed be held to be one of the most seriously damaging implications of the philosophy of knowledge. For it is above all widespread acceptance of this implication which effectively puts a stop to academic inquiry being pursued fundamentally as the promotion of humane, cooperative problem-solving in life.

The situation is, however, in some respects even worse than this might suggest. Not only does general acceptance of the philosophy of knowledge prevent social 'scientists' from taking as their fundamental task the promotion of rational cooperative human problem-solving in society: even worse, it leads social 'scientists' to produce work which, if anything, serves actually to encourage social manipulation – thus further obstructing rational, cooperative, social action.

As far as the natural and biological sciences are concerned, it is quite clear that improved theoretical knowledge of natural phenomena does on occasions lead to valuable new technology. This depends crucially on improving our knowledge of the *lawfulness* of natural phenomena. Our knowledge predicts that if such and such conditions obtain, such and such will reliably be the outcome. Knowledge of this type enables us successfully and reliably to manipulate natural phenomena to our advantage.

The philosophy of knowledge in effect takes for granted that a similar procedure ought to be attempted as far as the social sciences are concerned. The human, moral, implications of this, however, are horrifying. For in essence what is being advocated is this. First, the social sciences need to develop improved theoretical knowledge of laws governing human behaviour and social systems. This knowledge then enables us to predict that if such

and such human, social circumstances are realized, such and such will reliably be the outcome. As a result, we are in a position to develop useful social technology. But this amounts quite simply to developing techniques of human, social manipulation. Built into the very enterprise of the social sciences, conceived of in this way, is the ideal of developing more effective techniques for manipulating people.[8]

The saving grace of this procedure is perhaps its ineffectiveness. People are not just natural phenomena. Human actions are not law-governed in the way in which natural phenomena are (even though all physical processes occurring in connection with human actions may be law governed). People are capable of discovering that they are being manipulated, and capable of disrupting manipulative predictions, whereas natural phenomena do not have such capacities. As a result, fortunately, manipulative social technology is not very effective, except in extreme circumstances, as in the case of torture or brain-washing.

Despite its inevitable ineffectiveness, the approach to social engineering that I have just described nevertheless has, I suggest, seriously damaging human repercussions. For in a thoroughly insidious fashion, it insinuates the

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[8] The most explicit and thoroughgoing exponent of the view that social inquiry needs to be pursued as the science and technology of human manipulation is of course Skinner (1973). In a sense we all ought to be grateful to Skinner, for he simply makes sharply explicit assumptions that are implicit in a great deal of social science. Skinner, one might say, provides an unintended *reductio ad absurdum* of the whole programme of social inquiry pursued as social *science* and social *technology* on analogy with natural science and technology. It is most important in this connection to recognize that Skinner's general programme of developing a technology of human manipulation based on a predictive theoretical knowledge of human behaviour is quite distinct from the specific way in which Skinner proposes to implement this programme, namely in terms of behaviourism. The general programme can survive intact even if behaviourism collapses. In opposing Skinner, it is the general programme that needs to be criticized, and replaced with something better; it does not suffice to refute behaviourism (even though, in my view, behaviourism deserves to be rejected as an absurdity, based as it is on an operationalist misunderstanding of the nature of physical science, which holds unobservable entities to be 'unscientific').
In exceptional cases, of course, manipulation of people may well be morally legitimate, as when the manipulated person cannot take full responsibility for himself due to being insane, mentally retarded, very young, or obsessional in some way, the person knowingly and voluntarily submitting to manipulative treatment, aversion therapy let us say, in order to gain greater self-control.

idea that our fellow human beings are to be dealt with by means of manipulative techniques, this, furthermore, being the proper scientific, rational way to deal with other people. The idea that we might wish to join with our fellow human beings in worthwhile, valuable cooperative endeavours almost disappears.

There is a further consideration. Specifically psychological or social laws governing human, social action are only likely to be applicable as long as fixed, human, social aims are pursued in fixed, stereotyped fashions, by means of fixed methods. The moment active, human, social, intellectual inquiry exhibits a little more life, a little more innovation and creativity, pre-existing laws are likely to be disrupted. In the field of educational psychology, for example, laws governing learning and education are only likely to be successful as long as certain standard, fixed aims and methods continue to be put into practice. A more creative, aim-oriented rationalistic approach to learning and education would quickly render pre-existing 'knowledge' of educational psychology redundant. Likewise, a more aim-oriented, rationalistic approach to politics might quickly render pre-existing 'knowledge' of political science redundant. (For 'aim-oriented rationalism', see chapter 5.)

This reveals both the triviality, and the profoundly anti-humanitarian character, of the kind of theoretical knowledge sought by social 'sciences' conceived of in accordance with the philosophy of knowledge. The social 'sciences' are only likely to meet with progress if there is no human progress. The more creative and innovative people are in their lives, the more rapidly will any 'laws' of the social 'sciences' become redundant. Far from helping to promotes human progress and rational, humane, social inquiry, the social 'sciences', in order to meet with success, actually require people to be obediently incapable of innovative thought and action. We are being invited to conceive of ourselves as incapable of reason and creativity.

6 *There are undesirable consequences for all human endeavour due to the creation of general distrust of reason.* As a result of masquerading as rational inquiry when in fact, without this being realized, it exemplifies a profoundly damaging kind of irrationality, organized inquiry, pursued in accordance with the philosophy of knowledge, will tend to create entirely unwarranted general distrust of reason, thus causing further harm. There are two ways in which this will come about. First, general acceptance of the philosophy of knowledge as constituting 'rational thought' must tend to have the consequence that, when such inquiry leads to diverse undesirable consequences, of the kind discussed above, many people will conclude that reason itself is to blame, reason being somehow

inherently defective, from a moral or human standpoint. Science and technology, making possible widespread suffering, injustice and death (see 2 above), even colluding with national and commercial actions that cause suffering, injustice and death (see 3 above) will be taken to demonstrate an inherent defect in the 'scientific' or rational approach to human problems. Second, as a result of reason becoming identified with the methods, the intellectual standards, of science as conceived of by the philosophy of knowledge, 'reason' becomes peculiarly ill-equipped to help us tackle personal and social problems of living. For this irrational, philosophy-of-knowledge conception of reason requires that, in order to be rational, we must exclude all consideration of feelings, desires, aims, values, personal experience and imagination, and attend only to impersonal, objective fact, evidence and logic. Even in the natural sciences such a conception of rationality is, as we have seen, irrational and damaging. But in most of the rest of life – and especially in connection with cooperative action and relationships of friendship and love – it is absolutely disastrous. The outcome of all this is that rationality comes to seem severely damaging if employed unreservedly in personal and social life, rationality apparently only being fruitful in limited domains such as natural science and mathematics. The programme of developing more rational ways of life, a more rational world, comes to be vehemently opposed in the interests of sanity, freedom, individuality, civilization. Precisely those who ought to be most concerned to help promote cooperative rationality in life – those who are most concerned to help create a more humane, a more just, a happier, more loving and wiser world – come to be the most vehemently opposed to it. Instead of rationality being understood to be essential for the flourishing of humanity, friendship, freedom, justice, love, civilization, it is seen to be, in many ways, the enemy of these things. (The mistake in all this, let me repeat, is to identify reason with the *irrationality* of the philosophy of knowledge. It is not reason itself that many self-confessed anti-rationalists oppose, but rather something that they have been fooled into taking to be reason, a characteristic kind of irrationality, long upheld by self-styled 'rationalists' to be reason itself.)

Opposition to science, reason, and the ideal of a rational world, based on the understandable misconceptions just indicated, is widespread and influential in the modern world. It is a major, enduring theme in literature, various expressions of which are to be found in the writings of Blake, Dostoevsky, Kierkegaard, Barzun, Ellul, Roszak, Zamyatin, D. H. Lawrence, Frisch, Laing, Cooper, Barrett, Feyerabend, and many others, as I have already indicated in chapter 2.

This concludes my survey of some of the ways in which socially influential inquiry, pursued in accordance with the philosophy of knowledge, must inevitably come to have damaging repercussions for all aspects of life as a result of the basic irrationality of the philosophy of knowledge.

Chapter Four
The Philosophy of Wisdom

The philosophy of wisdom is designed to overcome the fundamental and profoundly damaging defects of rationality inherent in the philosophy of knowledge. It differs radically from the philosophy of knowledge. All aspects of inquiry, all intellectual disciplines and the way these are related to each other and to the rest of society, are affected as we move from the philosophy of knowledge to the philosophy of wisdom. There is, however, nothing arbitrary about the basic principles of the philosophy of wisdom. These principles, as set out below, are necessarily what they are in order that the basic objective may be achieved: a kind of inquiry that is devoted, in a genuinely rational way, to enabling people to realize what is of most value to them in life. Whereas inquiry pursued in accordance with the philosophy of knowledge violates the three elementary rules of reason, (1), (2) and (3), indicated at the beginning of chapter 3, inquiry pursued in accordance with the philosophy of wisdom puts these three rules of reason into practice in a thoroughgoing way, throughout its whole structure and organization: it is solely this which accounts for the dramatic differences between the two kinds of inquiry.

Even though it has manifold repercussions, the basic idea of the philosophy of knowledge is, as we have seen, extremely simple. It is that inquiry can best help us realize what is of value in life by devoting itself, in the first instance, to achieving the intellectual aim of improving knowledge, in a way which is dissociated from life and its problems, so that knowledge thus obtained may subsequently be applied to helping us solve our problems of living.

It is just this basic, simple idea that the philosophy of wisdom rejects as damagingly irrational. It holds instead that inquiry, in order to be rational, in order to offer us rational help with realizing what is of value, must give absolute intellectual priority to our life and its problems, to the mystery of what is of value, actually and potentially, in existence, and to the problems of how what is of value is to be realized. Far from giving priority to problems of knowledge, inquiry must, quite to the contrary, give absolute priority to the intellectual tasks of articulating our problems of living, proposing and criticizing possible solutions, possible and actual human *actions*. The central and basic intellectual task of rational inquiry, according to the philosophy of wisdom, is to help us imbue our personal and social lives with vividly imagined and criticized possible actions so that we may discover, and

perform, where possible, those actions which enable us to realize[1] what is of value – happiness, health, sanity, beauty, friendship, love, freedom, justice, prosperity, joy, democracy, creative endeavour, cooperation and productive work – it being understood, of course, that knowledge and understanding can in themselves be of value in life, and are vital dimensions to almost all that is of value in life.

Far from serious, prestigious inquiry being primarily scientific or academic, it is according to the philosophy of wisdom, if anything, all the other way round: for each one of us the most important and fundamental inquiry is the thinking that we personally engage in (on our own or with others) in seeking to discover what is desirable in the circumstances of our life, and how it is to be realized. Institutionalized inquiry is simply a development of our personal and social thinking, having as its basic task to help us rationally develop our own personal and social thinking and problem-solving, so we may all the better realize what is of value to us in our personal and social lives. Whereas for the philosophy of knowledge the fundamental kind of rational learning is acquiring knowledge, for the philosophy of wisdom the fundamental kind of rational learning is learning how to live, learning how to see, to experience, to participate in and create what is of value in existence.

The central task of inquiry is to devote *reason* to the enhancement of *wisdom* – wisdom being understood here as the desire, the active endeavour, and the capacity to discover and achieve what is desirable and of value in life, both for oneself and for others. Wisdom includes knowledge and understanding but goes beyond them in also including: the desire and active striving for what is of value, the ability to see what is of value, actually and potentially, in the circumstances of life, the ability to experience value, the capacity to help realize what is of value for oneself and others, the capacity to help solve those problems of living that arise in connection with attempts to realize what is of value, the capacity to use and develop knowledge, technology and understanding as needed for the realization of value. Wisdom, like knowledge, can be conceived of, not only in personal terms, but also in institutional or social terms. We can thus interpret the philosophy of wisdom as asserting: the basic task of rational inquiry is to help us develop wiser ways of living, wiser institutions, customs and social relations, a wiser world.

What ought we to mean by 'reason'? What is involved in tackling problems 'rationally'? Reason, according to the philosophy of wisdom, appeals to the idea that there are entirely general rules or methods of

[1] The phrase 'to realize what is of value' I use throughout to mean both 'to become aware of what is of value' and 'to make real or actual what is of value potentially'.

problem-solving which, when put into practice, other things being equal, give us our best chances of successfully solving our problems. It is essential to the conception of reason employed here that reason cannot, and is not intended to, dictate decisions to us. In acting and thinking in a wholly rational fashion we do not in any circumstances forego our freedom, or reduce freedom to the one free decision to act and think in accordance with the rules of reason: on the contrary, by acting and thinking in accordance with the rules of reason we maximize our freedom, our capacity to decide for ourselves *well*. (The basic task of reason is indeed to maximize freedom in the sense of freedom to achieve what is desirable and of value to us – all but identical to wisdom.) But there is of course nothing infallible about reason: however rationally we may act and think, we may still unnecessarily fail.

Two rules of rational problem-solving (already mentioned) are absolutely basic: (1) articulate, and try to improve the articulation of, the problems to be solved; (2) imaginatively propose and critically assess possible solutions. In devoting reason to the enhancement of wisdom, academic inquiry gives absolute priority to these two rules of rational problem-solving.

Here we are, alive for a while, and then we die. How can we make something significant and of value out of our lives during the few decades that are, at most, available to us? How can we develop an ecologically sustainable world in which people do not die unnecessarily for lack of food, sanitation, medical care – a world in which there is a much more just distribution of land, resources, and wealth amongst people than at present? How can we put a stop to the spread of armaments throughout the world, and to war both within and between nations? How can we get rid of dictatorships everywhere, and establish instead traditions of democracy and personal liberty? How can we develop more cooperative ways of working and acting together, so that ownership and responsibility are shared amongst adults, and so that our best, our noblest impulses may flourish?

These are the kind of problems, already referred to in chapters 1 and 3, which need to be put at the heart of the academic enterprise. These are the problems that academic inquiry should be centrally concerned to help us solve.

Not everyone, I imagine, will agree with my list of fundamental personal and social problems of living. For a number of reasons, what we take our problems to be is itself controversial and problematic.[2] Different people,

[2] There is, for example, a considerable difference in the way the Brandt Commission saw global problems of economic development, and the way some of its critics understand such problems. See Brandt et al. (1980) and Encounter (1980). Richard Barnet (1972) has

different groups of people, encounter different problems. One person's solution may be another person's problem. Not all human failure and suffering constitute problems. We must recognize that some suffering is unavoidable, inherent to life. We all, at best, grow old and die. Problems arise when we suffer, when we fail to achieve what is desirable and of value, and our suffering, our failure could have been avoided. This makes the identification of our problems doubly problematic. In order to know what our problems are we need to know both what it is possible for us to do, and what it is genuinely desirable and of value for us to achieve.

It is just this inevitably *problematic* character of our problems which makes it essential for academic inquiry to devote considerable attention to the task of *improving the articulation* of our problems of living. The wide range of ways in which people, with different interests and beliefs, see their problems must be represented within academic inquiry, together with the cooperative endeavour to improve the formulation of these problems. It may seem that admitting such a plurality of interests and problems into the academic enterprise must inevitably destroy its coherence – so that it fragments into hostile, non-communicating factions. Such an outcome is possible: but it is not inevitable. At least there ought not to be any serious intellectual difficulty in establishing a common ground for the cooperative discussion of conflicting interests and problems within academic inquiry. Insofar as academic inquiry has, as its basic task, to devote reason to helping humanity achieve that which is of value in life, we can agree that inquiry must be committed to helping people resolve their problems in a cooperative and just fashion – to the extent that this is possible.

Solutions to personal, social problems of living are essentially personal, social *actions*. Thus, according to the philosophy of wisdom, academic inquiry is centrally and fundamentally concerned to propose and assess critically possible and actual personal, social actions, from the standpoint of their capacity to help us achieve what is of value in life. The task of proposing and criticizing possible actions is actually and intellectually more fundamental than the task of proposing and criticizing claims to knowledge.

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analyzed brilliantly the different ways in which United States administrations and third-world revolutionary movements perceive and understand problems of the third world. See also Stiglitz (2002). The inevitably problematic character of problems is perhaps the main consideration which leads me to develop aim-oriented rationality in chapter 5.

There are, of course, many rules of rational problem-solving in addition to the two basic rules already mentioned.[3] In tackling a complex problem it is often helpful to break the given problem up into a number of subordinate, specialized problems, which we tackle one by one, the solutions then being put together to solve our original, overall problem. It may be helpful to begin by tackling easier, analogous problems in an attempt to develop helpful methods of attack. In order to develop good ideas for a solution to our given problem it is often helpful to look at solutions to analogous, already solved problems.[4] Quite generally, in fact, solving a new problem involves discovering how to relate the new problem to analogous, already solved problems.[5] As a result of putting into practice these kinds of additional rules

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[3] The best book, to my knowledge, on rational problem-solving is Polya (1957); it is also one of the simplest and most delightful. Polya is concerned with how to go about solving elementary mathematical problems: he makes it clear however that strategies that arise in connection with solving mathematical problems are relevant to discovery and problem-solving in general. Also of interest in this context are Hadamard (1954) and Lakatos (1976). De Bono's tireless efforts to promote practice in, and a sense of the importance of, problem-solving also deserve to be mentioned. See, for example, de Bono (1972, 1974).

[4] Given that we seek to solve a problem P, and that a different but vaguely analogous problem P_1, has a known solution S_1, we may seek gradually to modify P_1 in the direction of P, at the same time appropriately modifying S_1, so as to be a solution to the modified problem until eventually it becomes a solution to P itself.

[5] An important additional rule is: try reformulating the problem P to be solved, and try reformulating the reformulation, and so on, in this way building up a network of reformulated versions of P, any one of which, if solved, leads to a solution to P (immediately, or without too much difficulty) in this way endeavouring to arrive at a solvable distant cousin of P. This rule helps to explain why problem-solving may actually be a more methodical, less irrational process than it is often thought to be. In support of problem-solving being irrational it is sometimes argued that solutions often come in a flash, in a moment of inspiration, almost unsought, often when methodical searches for a solution have persistently failed. What the above rule suggests is that this common phenomenon may well be deceptive. The result of applying this rule methodically and laboriously to some problem P may be the discovery that if P_N can be solved, so can P. In a flash it may be recognized that P_N is easy to solve. The high excitement of at last discovering how to solve P may fool one into supposing that the discovery that P_N can be solved is a moment of inspiration, of high intellectual achievement. Actually it may be nothing of the kind. P_N may be genuinely very easy to solve. The achievement lies in the laborious methodical discovery that a solution to P_N enables one to solve P. Thus appearances to the contrary, all the really difficult and substantial work involved in discovering how to solve P was actually performed in a slow, progressive highly methodical way. (This point arises in connection with other rules as well.)

of rational problem-solving, we develop a *tradition* of problem-solving, which enables us progressively to build up, to enhance our problem-solving power. Rational problem solving involves quite essentially the progressive development of problem-solving power in this way.[6]

Two further rules of rational problem-solving ought perhaps to be mentioned. First, in attempting to solve any given problem, always be ready to *change the problem.* The given problem P may be unsolvable, and may need to be changed to P_1, the most desirable solvable problem close to P. Alternatively it may be undesirable to solve the given problem P despite first appearances to the contrary: it may be desirable to change P to P_1, a desirable, solvable problem close to P. Second, in attempting to solve P, always be ready to consider, and reconsider, P as subordinate to some larger, more general, or more fundamental problem P_2, the solution to P being sought in order to help solve P_2. New approaches to solving P_2 may require P to be modified; or may render it unnecessary, or even undesirable, to solve P (in which case persisting in the attempt to solve P becomes irrational). These two rules are basic to aim-oriented rationality, to be expounded in the next chapter. It may be noted that, from the standpoint of the philosophy of wisdom, the philosophy of knowledge is irrational because it prohibits inquiry from putting these two rules into practice. We seek to solve problems of knowledge because, more fundamentally, we seek to solve problems of living. The two rules just indicated require that attempts to solve problems of knowledge be rationally responsive to attempts to solve more fundamental problems of living. This, demanded by the philosophy of wisdom, is prohibited by the philosophy of knowledge, in a misguided attempt to preserve the 'objectivity', the 'rationality' of science.

[6] These rules of reason presuppose that we can already successfully solve problems in the world; they are designed merely to help us marshal our already existing problem-solving power in order to solve new problems. All that reason can accomplish is to help us to reorganize what we can already do – solutions to problems that we can already solve – so that they become a solution to the new problem P that we initially do not know how to solve. This point has an important bearing on a basic tenet of the philosophy of wisdom (to be discussed below) that successful action in the world comes before, and is presupposed by, thought, reason and knowledge. The point is also important in connection with the Humean problem of rational action – the problem of how there can be any such thing as rational action in the world. And finally the point explains why it is important, of such value, to tackle our problems rationally: in doing so, we give ourselves, other things being equal, the best chances of progressively enhancing our problem-solving powers. According to this view, in acting rationally we act in such a way as to give ourselves the best chances of successfully developing and extending what we can already do. The basic task of reason is to help us to establish traditions of learning, of making progress. This immensely important point – that in a sense we only ever improve on what we can already do – might even be enshrined in another rule: in learning how to do something entirely new, which you cannot at present do at all, begin by *doing it* (in some non-destructive way) and then set about progressively improving your performance. It is

All this, according to the philosophy of wisdom, is exploited by academic inquiry. The basic task of academic inquiry is to help us build rules such as these into our habits of thought, feeling and action, into our personal and social life, and into our institutions, so that we may tackle our problems of living in such a way as to give ourselves the best chance of realizing what is of value to us, thus progressively enhancing our powers to realize value in life — progressively enhancing our freedom, our creativity, our capacity to love, our wisdom. In particular of course, academic inquiry itself puts these rules into practice, in rationally searching for solutions to problems of living. This does not just involve individual scientists and scholars putting these rules into practice in their own individual research work; in addition it involves these rules being built into the whole intellectual/institutional structure of the academic enterprise – thus influencing such things as the way that disciplines are related to each other and to the human world beyond; decisions concerning what is to be published; decisions concerning what research is to receive financial support; academic appointments; the content and style of education – of seminars and lectures, degree courses, examinations.

Emerging out of, and feeding into, the central concern with our personal and social problems of living (in accordance with rule (3) as stated at the beginning of chapter 3), academic inquiry quite properly creates and explores a wide range of subordinate, specialized intellectual problems, academic work on these subordinate problems all being designed, in one way or another, to help us achieve what is of value in life. Thus the *technological sciences* – *engineering, medicine, artificial intelligence* – seek to solve, and to develop techniques for solving, those technical problems that need to be solved if we are to realize desirable life-aims such as prosperity, health, release from repetitive, soul-destroying work. *Mathematics* seeks to develop, systematize and unify abstract problem-solving methods, applicable to as wide a range of circumstances as possible. Pure mathematics is concerned with significant, problematic possibilities, and not with anything actual at all. *The physical* and *biological* sciences seek to solve subordinate problems of knowledge and understanding concerning diverse aspects of the natural world. The *humanities* and the diverse branches of *social inquiry* have the fundamental intellectual task of articulating our problems of living, proposing and critically assessing possible solutions. In sharp contrast to the philosophy of knowledge, the philosophy of wisdom holds social inquiry to be intellectually more

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this rule that young children put into practice so successfully in learning to speak: first they babble, then gradually transform this babbling into speech.

fundamental than the natural sciences, just because social inquiry is concerned with primary problems of living whereas natural science is concerned with subordinate and secondary problems of knowledge. (From the standpoint of the philosophy of wisdom, the proper term is social *inquiry* rather than social *science* – the latter being a typical philosophy-of-knowledge misnomer and misconception.) Insofar as the humanities and the diverse branches of social inquiry seek to improve our knowledge and understanding of people and societies, this is undertaken as a subordinate intellectual enterprise in order to aid the fundamental task of helping us to realize what is of value in life. Thus *economics* has as its primary task to propose and criticize possible solutions to economic problems – the economic aspects of our problems of living – contributions to economic theory and knowledge being intellectually subordinate and secondary. *History* and *anthropology* have, as their basic tasks, to acquaint us with the successes and failures that people have encountered in seeking what is of value in life in the past and in other places, in other cultures, so that we may learn from their example. Keeping a record of our past problem-solving efforts is essential to reason. History and anthropology thus make an essential contribution to reason. *Psychology* has, as its primary task, to help us to articulate our personal and interpersonal problems of living, and to propose and criticize possible solutions, as we live, thus helping us to resolve rationally our most personal, emotional, intimate problems. *Sociology* has, as its basic task, to help us to propose and criticize possible solutions to social and institutional problems, in relevant social and institutional contexts, so that we may gradually improve our capacity to resolve such problems in a cooperatively rational way. *Political inquiry* has, as its basic task, to help us to articulate our diverse political problems, and problems associated with government, and to propose and criticize possible solutions, thus helping us gradually to discover how we may tackle these problems in a more cooperatively rational way than we do at present. The *study of international relations* has, as its basic task, to help humanity to articulate its international, global problems, and to propose and criticize possible solutions, so that gradually we may discover how to resolve these problems in a more humane, just, cooperatively rational way than we do at present. Yet again, *philosophy* has two inter-related tasks: to promote the rational tackling of our most general and fundamental problems of knowledge, understanding and living; and to articulate and assess critically views as to what ought to be the aims and methods of our diverse pursuits – art, literature, politics, theatre, education, industry, commerce, law, science. Philosophy is thus in part a severely practical endeavour, insofar as its task is to help us improve the aims and methods of our various pursuits, as we act. It is 'philosophy' in just this

sense that I am engaging in here, in expounding and critically assessing two rival views about what ought to be the basic aims and methods of the academic enterprise, the 'philosophies' of knowledge and wisdom.

The primary intellectual aim of the humanities and social inquiry, quite generally, is to help us to realize what is of value to us in our personal and social lives. What ultimately matters is personal and social progress towards enlightenment and wisdom: all academic progress is but a means to this end.

Academic inquiry may develop an intricate maze of subordinate, specialized academic disciplines and problems: it is however vital – according to the philosophy of wisdom – that work within these specialized disciplines on these specialized problems be undertaken in such a way that the overall outcome of, and reason for, this work is our enhanced capacity to solve our fundamental problems of living. In other words, specialized problems must be tackled as rationally subordinate to our fundamental problems of living (in accordance with rule (3) of chapter 3).[7] Only in this case can academic inquiry hope to be rational, intellectually rigorous and of maximum human value. The intellectual progress and success of academic inquiry is to be judged in terms of the extent to which academic work produces and makes available ideas, proposals, arguments, discoveries, techniques that help people achieve what is of value in life in a cooperative and just way.

The fact that scientific problems are tackled as aspects of, and in a rationally subordinate way to, intellectually more basic personal and social problems of living does not mean that only those scientific problems ought to be tackled whose solutions have immediate and obvious technological applications. There are two reasons for this. First, science can contribute to the quality of life, to the enhancement of wisdom, directly by enhancing our knowledge and understanding of significant aspects of the world around us (such knowledge, perhaps, having no technological applications). Second, problems are often solved in an unexpected, indirect way, as an unforeseen consequence of a solution to an apparently unrelated problem – a familiar phenomenon in both science and mathematics. If it was always obvious what scientific research programmes need to be pursued in order best to help us solve social problems of living, there would be little point in stressing the need to interrelate imaginative and critical discussion of social and scientific problems. It is precisely because this point is not obvious that the interrelation between social and scientific problems needs sustained, explicit, critical attention.

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[7] This point is developed further in Maxwell (1980).

According to the philosophy of wisdom, all the intellectual problems and aims of all science and scholarship are fundamentally personal and social in character. This does not mean, however, that the only kind of value that inquiry is recognized to have is a practical value. Quite to the contrary, the philosophy of wisdom seeks to emphasize the profound value that inquiry can have when pursued for its own sake, and not only as a means to some other end. Realization of value (the aim of all inquiry) includes the seeing, appreciation and understanding of what is of value, in people, in art, in the world, as well as the active endeavour to cherish and help grow what is of value, potentially and actually, in existence. The philosophy of wisdom insists, however, on the profoundly *personal* and *inter-personal* (or social) character of inquiry pursued for its own sake. Our own personal endeavour to see, to understand, what is of value in existence as we live is, for each one of us, pure inquiry, inquiry pursued for its own sake, at its most fundamental and important. In order to appreciate just how precious we hold such personal inquiry to be, consider why we would be so distressed to discover we are about to become blind. In part this distress would be due to the prospect of being deprived of the practical value of seeing; but far outweighing this, surely, would be the distress that we would feel at the prospect of being deprived of sight for its own sake. A whole precious dimension of personal inquiry – discovering and experiencing the visual aspect of things – would be cancelled. The extent of the distress we would experience at the prospect of being deprived of sight for its own sake gives us an indication of how highly we value our own personal inquiry pursued for its own sake. (We are all perhaps inclined to devalue, even to ignore, our own personal participation in inquiry pursued for its own sake because we tend to identify such inquiry with expert academic inquiry, our own thinking being depreciated as a result for failing to comply with the intellectual standards of the philosophy of knowledge. This is one way in which the philosophy of knowledge harms inquiry pursued for its own sake.)

According to the philosophy of wisdom, the whole *raison d'etre* of academic inquiry, from a purely intellectual standpoint, is to promote and aid personal inquiry, pursued for its own sake, as an integral part of life. According to this view, even an academic discipline as apparently remote from human concerns as *cosmology,* has a profoundly personal, social and creative aim: to enable people to improve their own personal knowledge and understanding of this cosmos in which we live.

As the argument of this book unfolds, I shall be concerned to stress that the philosophy of wisdom does better justice to *both* the practical *and* the intellectual aspects of inquiry than does the philosophy of knowledge. Thus,

the philosophy of wisdom stresses the intellectually fundamental character of articulating problems of living, proposing and criticizing possible solutions. At first sight this has only a practical value. I shall argue, however, that such imaginative exploration of people's problems is precisely what we need to do in order to acquire what may be called person-to-person understanding of other people – a kind of understanding (promoted by great literature) that it is essential to acquire if we are to appreciate what is of value in other people's lives, the value-discoveries of others enriching our own. This kind of person-to-person understanding is, I shall argue, fundamental to our humanity, essential for reason, and even for science. And yet the philosophy of knowledge debars it from rational inquiry for failing to satisfy its (misconceived) intellectual standards of 'objectivity' and impersonality. Yet again, much actual academic inquiry ostensibly pursued only for its own sake, may actually be pursued for quite different, all-too-human reasons: to further academic careers or win fame or status. As we shall see, the intellectual standards of the philosophy of wisdom can help us put right such perversions of science and scholarship – whereas those of the philosophy of knowledge cannot help.

The transition from the philosophy of knowledge to wisdom changes dramatically the whole way in which the two aspects of inquiry – 'pure' and 'applied' – are conceived. From the standpoint of the philosophy of knowledge, the two aims of inquiry (knowledge for its own sake, for the sake of its technological applications) seem to be quite distinct, even if a contribution to knowledge may be of value in both ways. From the standpoint of the philosophy of wisdom, the two aims ought to be intimately interrelated. In pursuing inquiry for its own sake we seek to discover what is of most value in existence. Even a meagre appreciation of what is of most value in existence can scarcely be had without some awareness of just how terrible it is that people – millions of people – should be needlessly deprived of their one opportunity to experience and participate in what is of value in life. Thus there develops the active concern to help people resolve their practical problems of living. The motive for this concern, however, ought to be to make it possible for more people to enjoy what is of value in life for its own sake. The two aims of inquiry are united in love.[8]

The transition from the philosophy of knowledge to the philosophy of wisdom changes dramatically the relationship between academic inquiry and politics. According to the philosophy of knowledge, political programmes and problems ought to have no place in the intellectual domain of inquiry,

[8] This point is developed further in Maxwell (1976b). See also Maxwell (2001, ch. 2).

which is concerned only with questions of fact and knowledge. According to the philosophy of wisdom, the intellectual domain of inquiry is concerned fundamentally with political programmes and problems. The distinction between the two spheres is based, not on subject matter, but on aims and methods. Academic inquiry is concerned to promote imaginative, critical thought, rational, cooperative, political action: it is not concerned to wield power, to legislate, or to persuade and manipulate, as are many of those who engage in political activity.

Again, the transition from the philosophy of knowledge to the philosophy of wisdom changes dramatically the relationship between science (or academic inquiry) and religion. According to the philosophy of knowledge, religious ideas and problems have no place within the intellectual domain of inquiry. According to the philosophy of wisdom, academic inquiry is concerned fundamentally with religious ideas and problems. If 'religion' is characterized in a broad way as 'concern for what is of most value in existence' then academic inquiry, as construed by the philosophy of wisdom, is essentially a religious enterprise. If 'God' is characterized in a sufficiently open, unrestricted way – as it ought to be according to many religious traditions – as that unknown something that is of supreme value in existence, then inquiry, as conceived of by the philosophy of wisdom, has as its overall goal to help us to realize 'God'. Inquiry as conceived of by the philosophy of wisdom is, however, opposed to the authoritarian and anti-rationalist elements present in most world religions. In particular, the idea that 'God' can be a supreme person, all powerful, all knowing and all loving, is rejected as a logical, moral and religious obscenity. Such a God would be knowingly responsible for all human suffering and death engendered by natural causes, and a participant in all suffering and death caused by people (since this invariably requires collaboration from Nature). Such a God would be a torturer and murderer of all mankind – infinitely more criminal than a mere Hitler or Stalin. All traditional attempts to excuse God's torturing and killing of people are similar to, and are on the same intellectual and moral level as, attempts to excuse the torturing and killing perpetrated by a Hitler or Stalin. To call such a cosmic tyrant a being of love is the most blatant inconsistency imaginable (unless one has monstrously perverted ideas about love). To advocate publicly that an all-powerful, knowing and loving God exists, as if this is a consistent possibility (let alone a known certainty) is, from the standpoint of the philosophy of wisdom, profoundly damaging in that it strengthens the impression that reason does not apply where it most needs to be applied: to the problem of what is of supreme value to us in existence.

Where it is most important for us to be rational we become carelessly and destructively irrational.

The mistake is to identify *power* and *love*. 'God' in the sense of cosmic *power* is the unified pattern of physical law that runs through all phenomena, to be discussed in chapter 9: it knows nothing of human suffering and cannot love. 'God' in the sense of cosmic *love* is that which is best, most loving, potentially, in human life, to be discussed in chapter 10: though potentially profoundly loving, it is at present often both ignorant and powerless – and hence the need for inquiry pursued in accordance with the philosophy of wisdom.[9]

It is not hard to understand why there should be such a great temptation to believe in the blatant inconsistency, a God of love who tortures and murders. We want our all loving God to be all powerful and knowing because we want to be assured that God can care for us, resurrect us after death, put to right all wrong. But in order to be comforted in this way we must abandon reason. Authentic religion seeks to help us confront realities however disturbing: it does not seek to console us with comforting illusion.

The transition from the philosophy of knowledge to wisdom also changes dramatically the relationship between academic inquiry and art. According to the philosophy of knowledge, art itself, like politics and religion, has no rational place within academic inquiry – even though of course factual knowledge about art, politics and religion does have such a place. According to the philosophy of wisdom, literature, drama, music, dance, painting, sculpture and other forms of art can make major and fundamental *rational* contributions to inquiry – as revelations of value in the world, and as imaginative explorations of life-problems and their possible resolution. Ancient Greece and the Italian Renaissance provide striking illustrations of how fundamental the contribution of art can be to inquiry if the latter is not hermetically sealed off from such learning as a result of observing the edicts of the philosophy of knowledge.

The two philosophies have radically different implications for education. Academic inquiry shaped by the philosophy of knowledge inevitably leads to education being of two kinds, often at odds with each other. On the one hand there is academic learning; on the other hand there is learning about how to live. If academic inquiry is shaped by the philosophy of wisdom, this dichotomy disappears. Academic learning *is* then learning about how to live. The philosophy of wisdom intelligently put into practice in schools and universities would change education beyond all recognition. Many current conflicts, difficulties, failings, would disappear. All education would be what

[9] See Maxwell (2001) for a development of this theme, especially ch. 1.

children instinctively want it to be: learning about how to live, learning about how to realize what is of most value to us in the circumstances of our lives.[10]

The two philosophies uphold requirements for rationality, for intellectual rigour, that are in important respects diametrically opposed. Thus, far from it being necessary for inquiry to be dissociated from life and its problems in order to be rational, it is, according to the philosophy of wisdom, all the other way round: inquiry can only rationally and effectively perform its basic task of helping us realize what is of value insofar as it is an integral part of our lives – even academic and scientific inquiry needing to be in close contact and communication with persons and institutions in the non-academic, non-scientific world in order to be able rationally to aid the realization of value in life. Again, far from it being necessary to banish desires and feelings from the intellectual domain of inquiry in order to preserve its rationality, it is all the other way round: desires and feelings must form an integral part of the intellectual domain of inquiry, at the most fundamental level (our own personal thinking) if inquiry is to be rational – capable, that is, of achieving rationally or effectively its basic task. Not everything that feels good is good, and not everything that we desire is desirable: but devoid of our feelings and desires we can make no value discoveries of our own: we can but echo or mimic the value discoveries and achievements of others. Thus, if inquiry is to help us realize what is of value, it must attend to our feelings and desires: the very articulation of our problems of living requires the expression of feelings and desires. According to the philosophy of wisdom, in fact, reason – rational action – is essentially so interrelating action, experience, feeling, desire, aim, imagination and doubt that we give ourselves the best chances, other things being equal, of realizing what is of value. Only by bringing together desires, aims, feelings, deeds and objective facts imaginatively and critically can we hope to be rational, and come to appreciate something of the value of what there is in the world. Whereas the philosophy of knowledge seeks to shield inquiry from an irrational world in order to preserve intact its rationality, the philosophy of wisdom, by contrast, gives to inquiry the basic task of helping us gradually develop more rational lives, a more cooperatively rational human world.

One assumption that tends to lie behind the philosophy of knowledge is that rational action only becomes possible once relevant knowledge has been obtained. This assumption is rejected absolutely by the philosophy of wisdom (see reply to objection 6 of chapter 8 for grounds for this rejection). What is absolutely fundamental is life itself, our doing things more or less

[10] For a sketch of wisdom education for five year olds see Maxwell (2005c).

successfully in the world, and our capacity so to do things. Our lives, our actions, are rational to the extent that we are able to exploit to our best advantage what we can already do in order to do new things so as to solve new problems. Being able to imagine possible actions can enormously increase our rational problem-solving power – if only because of the advantages to be accrued from trying out diverse actions in our imagination only, and not in the real world. Propositional knowledge and science are but developments of these more fundamental capacities, explorations in effect of what we can and cannot do, actually or in principle.

According to the philosophy of wisdom, the physical and biological sciences have an enormously important role to play within academic inquiry as a whole. Knowledge and understanding of the natural world are vital dimensions of wisdom. The crucial point, however, is that if the scientific search for knowledge and understanding is to be undertaken rationally, within inquiry as a whole, then it is essential that it be rationally subordinated to the intellectually more fundamental search for value in life (problems of knowledge being rationally subordinated to problems of living, natural science being rationally subordinated to social inquiry).

None of this means, let it be noted, that accepting the philosophy of wisdom rather than the philosophy of knowledge leads to a greater tendency to accept as true that which is highly desirable if true, or to reject as false that which is highly undesirable if true. Quite the contrary, as a result of taking human desires and aims into account in assessing contributions to knowledge, as the philosophy of wisdom requires, we put ourselves in a better position to correct any tendency to suppose that desirability implies truth, undesirability falsehood. The philosophy of wisdom provides us with a more intellectually rigorous conception of science, and of inquiry, than does the philosophy of knowledge, and upholds a more exacting, and more widely applicable, conception of reason. Stalin's imposition of Lamarckism on Soviet biology is no more in accordance with the philosophy of wisdom than it is in accordance with the philosophy of knowledge. Indeed, this argument for the philosophy of knowledge badly backfires: whereas philosophy-of-knowledge science flourished in the Soviet Union, despite lack of free speech, philosophy-of-wisdom science would not have been tolerated. This indicates how restricted, how tame, philosophy-of-knowledge intellectual standards really are. (It should be noted, in addition, that the philosophy of wisdom fully recognizes the elementary point that valuable contributions to inquiry can be made by those who pursue bad aims, and that trivial or harmful contributions can be made by those who pursue good aims.)

Values, of one kind or another are, inevitably and quite properly, inherent in the scientific enterprise. Science seeks *important* truth, not truth *per se*. In order to be accepted as a part of scientific knowledge, even accepted for publication in a scientific journal, it is not enough that a potential contribution be new and sufficiently well established; it must also be judged to be sufficiently interesting, significant or important. The philosophy of wisdom acknowledges the role of values in science, and seeks to subject values to rational, that is critical, control. The philosophy of knowledge, by contrast, denies that values are inherent in science (in the context of justification), and thus cannot subject these values to rational assessment.

We live, it seems, in an impersonal universe. Insofar as there is anything of value in the universe it has to do with life and especially, for us, with our own lives here on earth, the way in which we are in this impersonal cosmos. It is this holy mystery, this miracle of our existence with all its potentialities, embedded in the cosmos, that rational inquiry has as its task, its charge, to help us to cherish and grow, in an adult and responsible fashion, so that what is of value in our lives flourishes. Awareness of our surroundings and of ourselves is certainly a part of what is of value: but in order to be fully of value this awareness needs to inform our lives, our deeds, and not be cut off from life as impersonal scientific knowledge. The supreme thing, perhaps, is to live life lovingly, insofar as we can, lovingness certainly including every attention to the reality of what is objectively of value in the world, in others, in oneself. It is just such an objectively loving way of life that the intellectual standards of the philosophy of wisdom can promote, and that the intellectual standards of the philosophy of knowledge must sabotage.

Inquiry as conceived of by the philosophy of wisdom is perhaps best understood as being similar in character to, even though a rational development of, animal inquiry, animal learning. Animal learning is learning how to act, how to do, how to live. It is precisely in this way that we need to see our finest ideal of rational human learning, of rational human inquiry. A major difference is that whereas animals learn how to act to discover how to survive and reproduce, we may demand that we learn how to act in order to discover how to realize additional goals of value to us, such as justice, democracy, understanding, friendship and love. There is for us the possibility that we can exercise some influence over the quality of what survives and is reproduced – over what we can become and be and what our children can become and be.

It should be emphasized that, from the perspective of the philosophy of wisdom, all the diverse defects of the philosophy of knowledge follow from one single but fundamental error: a profound and disastrous

misrepresentation of the basic intellectual aim of inquiry. All the other defects of the philosophy of knowledge – its failure to characterize adequately such things as the proper relationship between inquiry and life, the aims and methods of social inquiry, the aims and methods of the natural sciences, the proper relationship between social inquiry and natural science, the nature of intellectual progress, the place of human desires and feelings in inquiry and their relevance for rationality, and so on – all these diverse failings stem from the simple, basic failure to specify the proper overall intellectual aim of inquiry.

A major task of this book is indeed to get across *both* (1) a sense of just how simple, how elementary, the basic proposal is – to change the overall aims and methods of inquiry, from knowledge to wisdom, in order to enhance simultaneously the rationality and the potential human value of inquiry; *and* (2) a sense of just how diverse and wide-ranging the repercussions of this simple proposal are – to such an extent that were we to take the proposal seriously, no aspect of our personal, social, political, intellectual or cultural life would remain unaffected.

The simple, elementary character of the basic proposal is exhibited in figure 3.

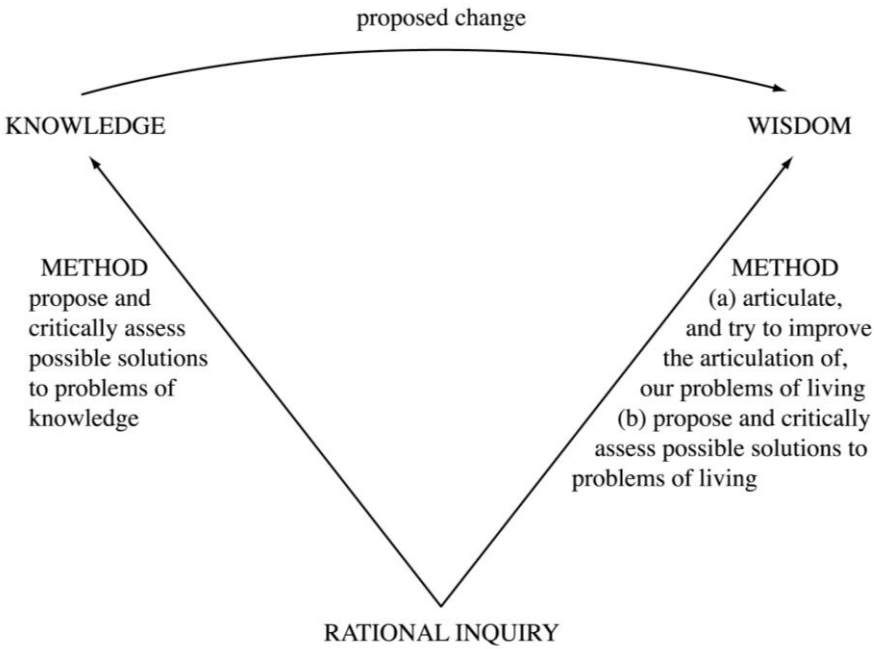

Figure 3 Proposed change in aims and methods of inquiry

It is of course not the job of scientists and scholars actually to decide for the rest of us what our problems are, how they should be solved, what we should do with our lives, and what is of value. On the contrary, the proper job of scientists and scholars is to help the rest of us to reach our own decisions – decisions that we do really want to make. In other words, the proper task of academic inquiry is not to deprive us of our power to choose and decide but to enable us to enhance our power to choose and decide well. The most important and fundamental kind of thought that there is in the world exists as an integral part of our personal and social life.

Subordinate, specialized aspects of thought are *delegated* to academics, our servants not our masters. At the most fundamental level of all, there is life, actions, the ability to do things. Out of this there emerges conscious thought, imaginative problem-solving, as an integral part of personal and social life. This in turn gives rise to institutionalized, professional thought – science and scholarship – devoted in turn to helping to promote rational, enlightened thinking in life.

The way in which life, and personal and social thought, ought to be related to academic thought – according to the philosophy of wisdom – can perhaps be illuminated by considering an analogy drawn from music. Associated with music there are all sorts of highly specialized skills and fields of expertise, to do with such things as playing and making musical instruments, musical composition, musicology, teaching: in the end, however, the whole rationale for engaging in all such specialized, technical pursuits is to further the creation, enjoyment and appreciation of performed music. In an analogous way, and more generally, we may argue, the whole rationale for engaging in the diverse specialized, technical pursuits of academic inquiry is to further the creation, enjoyment and appreciation of value in life.

From the standpoint of the philosophy of wisdom, public organized inquiry is perhaps best understood as arising primarily in response to – and to help us to solve – the problems of acting cooperatively in a vast, complex, diverse, interconnected human world of the kind we live in today. When humanity lived in small hunting and gathering tribes, this problem did not exist. It is at least possible for a tribe of some fifty people, who all speak the same language and share the same culture, skills and values, to solve problems cooperatively, without any elaborate institutional organization. Informal tribal meetings can be held to decide matters of concern to all, with everyone being able to have their say without major logistic problems being encountered. But then the hunting and gathering way of life gives way to more settled agricultural ways of life; tribes grow in size and begin to trade

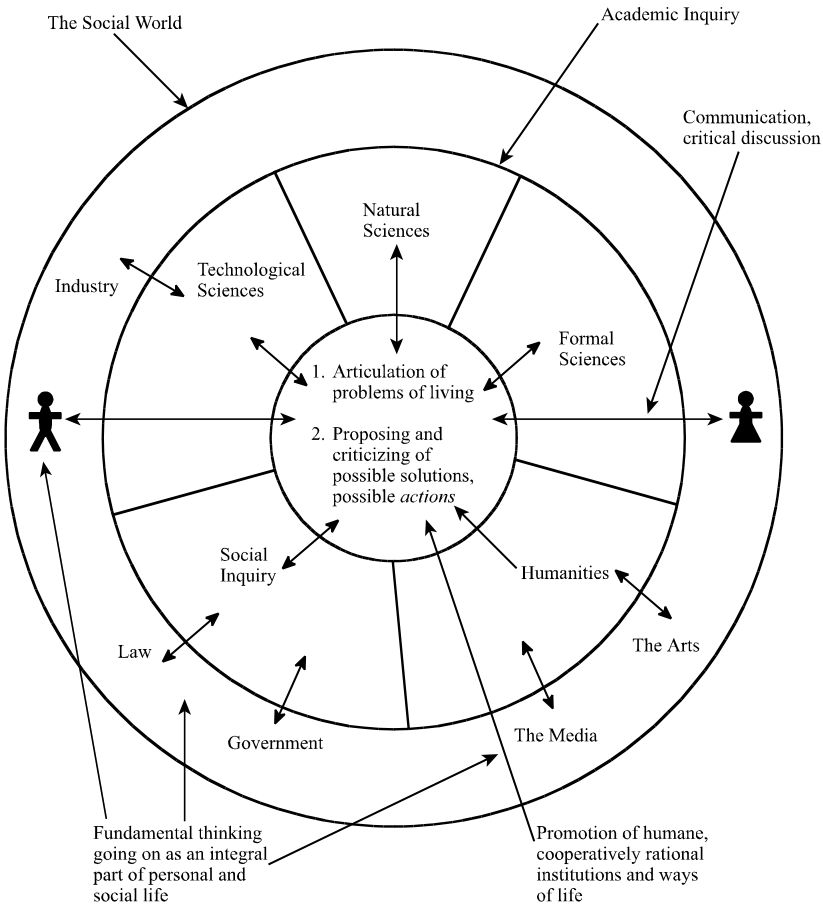

Figure 4 Relationship between the Intellectual Domain of Inquiry and the Social World according to the Philosophy of Wisdom

with neighbouring tribes; cities are built, tribes coalesce; divisions of class, work, skills, culture and values grow within societies of increasing size; modern science, technology and industry develop, and with them the further development of diverse, vital esoteric skills and expertise. Modern methods of communication, travel and trade have the effect of interconnecting most people, at least to some extent, to form a vast, complex interdependent and interacting global society. A tribal meeting of humanity has become a logistic impossibility – thus making cooperative action extraordinarily difficult to

achieve. (Cooperative action is here to be understood to imply action engaged in by a number of people who share responsibility for what is done and for deciding what shall be done, to their general benefit, there being no permanent leadership, or delegation of responsibility.)[11] Cooperative action – whether performed by groups of friends or colleagues, local communities or at the national or international level – is only possible if there exists the means for those acting to discuss problems, objectives, conflicts and diverse possible actions.

The most basic and urgent task of public, organized inquiry – academic inquiry – is, according to the philosophy of wisdom, to provide the means for such discussion ultimately at the global level – thus providing a sort of intellectual/institutional substitute for a tribal meeting of humanity. Intellectual standards governing the articulation and discussion of problems of living need to serve this end; standards of clarity, simplicity, truthfulness, justice, and cooperation. Contributions to academic inquiry – articulations of problems, proposals for action intended to solve human problems – must be assessed on their merit and not merely in terms of the expertise, authority or power that a person or group of people making the contribution may possess, or may be held to possess. The ability of people with power, wealth or talent to influence the way problems of living are discussed in public, in the media and elsewhere, must be counteracted by academic inquiry so that the interests and problems of the powerless, the poor and the inarticulate receive their due attention and representation, and are not neglected. On this view, academe is a sort of people's civil service, doing openly for people what the civil service is supposed to do for government (as I said in the introduction): see figure 4.

In order to perform this task properly it may be necessary for academic inquiry to develop esoteric discussion and vocabulary – in natural science and mathematics – impenetrable to most people; it is also absolutely essential, however, that the most important problems and discoveries be formulated clearly, simply and non-technically, so that even twelve-year-old children can understand them.

Popper has placed great emphasis on the importance of recognizing the existence of a relatively autonomous 'World 3' of ideas, propositions, theories, problems and arguments, which interacts with the material 'World 1' via the psychological 'World 2' (Popper, 1972; Popper and Eccles, 1977). From the

[11] Cooperativeness may include competitiveness, if generally deemed to be desirable: it is not automatically the opposite of cooperation. Force, threat of force, manipulation: these are what negate cooperation.

standpoint of the philosophy of wisdom, this Popperian view is a philosophy-of-knowledge mystification of the intellectual domain of inquiry. In reality there are, in this one world, the deeds, imaginings, proposals, intentions, arguments, suggestions and convictions of *people* – all aspects of personal and social life. It is, however, of vital importance that we develop in our human world a tradition of treating possible actions, proposals for action, problems, arguments, philosophies, theories, as if they are entities existing more or less independently of who expresses them, for what motive, in what language, when and where. We need to do this so that we may make possible something like a rational, cooperative tribal discussion of humanity, which cuts across barriers of language, time and place, and gives due emphasis to what is best, irrespective of the dictates of mere power or wealth.

If this intellectual domain of discussion is to perform its proper function, however, it is vital that we see it as a human fiction, created for a vital human purpose, an immensely valuable part of the social fabric designed to make possible just, cooperative action: it is vital that we do not become so dazed and inflated by this social creation of ours that we begin to imagine, with Popper, that it amounts to an autonomous realm of being.[12]

A basic task of inquiry, according to the philosophy of wisdom, is to help us live more rationally in all that we do, and to help us develop a more rational social world. I have argued that many who apparently oppose reason, actually oppose its opposite – a characteristic kind of extremely damaging irrationality, associated with the philosophy of knowledge, which falsely masquerades as reason. Many I am sure will not be entirely convinced by this argument: they will continue to find the very idea of living a wholly rational life in a wholly rational world thoroughly objectionable.[13] I therefore now

[12] For a discussion of how one can do justice to a basic candidate for a world 3 entity – the proposition – without invoking Popper's world 3, see Maxwell (2007a, section 7). See also the discussion of the biological, neurological and evolutionary origins of thought below, in chapter 8, replies to objections 6 and 7 – this evolutionary, 'action suspended' theory being further developed in Maxwell (2001).

[13] This is a major theme of romantic literature, art, thought, politics, psychiatry and education, to be found in the writings of Blake, Rousseau, Dostoevsky, Nietzsche, Kierkegaard, D. H. Lawrence. (Russell gives a hilarious account of Lawrence's attempt to convince him to throw away reason and the intellect; see Russell, 1956, pp. 106-7.) For a delightful fictional portrayal of the horrors of the perfectly 'rational' society see Zamyatin (1972). For a novel portraying the breakdown of a 'rational' individual, see Frisch (1974).

add a few remarks intended to make clear that it is always desirable to be rational in all that we do.

The central point is simply this. Acting and thinking in a wholly rational way cannot possibly in general go against our best interests, whatever we are doing, just because to act and think in this way is simply to do so in accordance with certain general methods or strategies which, other things being equal, give us our best chance of solving our problems, realizing what is of value to us. If reason leads us systematically astray then, by definition, it cannot be reason.

Consider the way in which attitudes towards reason have come down to us from the past. If we take what reason meant to Descartes, Leibniz, Hobbes or Spinoza as our starting point, and consider the subsequent development of the concept, then two kinds of decline in the concept become apparent. There is first of all a decline in what is taken to be the power, the efficacy of reason. For seventeenth-century Rationalists, reason had almost unlimited power in that, if properly used, it could decide issues with absolute certainty, beyond all doubt. Gradually since then this extreme confidence in the power of reason has been eroded. Sceptical arguments such as those of Hume led Rationalists to conclude reluctantly that reason may only be able to function negatively, in establishing the falsity of some claims to knowledge, finally even this negative power being called into doubt as the view develops that experimental refutations of theories in science are rarely decisive. Even the power of reason to establish results beyond all doubt in mathematics is called into doubt, as the paradoxes of set theory are discovered, introducing uncertainty even into the foundations of mathematics, the notion of rigorous proof becoming uncertain. The development of intuitionist logic, which rejects the law of excluded middle 'p or not p' indicates that uncertainty has even reached the inner sanctum of reason, logic.

The second kind of decline that reason has suffered since Hobbes and Spinoza is a decline in what is taken to be the proper domain of application, the scope, of reason. For Hobbes and Spinoza, reason seems to have unlimited scope in that it is applicable not just to questions of fact and knowledge, but also to moral issues, to problems of politics and religion. Subsequently, in part as a result of the development of the Romantic movement, this has seemed to many to be far too wide an application of reason, impossible to carry out, or undesirable to carry out even if possible. Thus religious faith is placed beyond the reach of reason, morality is held to be an affair of the heart and not of the mind, not something to which reason can be applied. Art, love, friendship, enjoyment, happiness may all be held to

be beyond the domain of reason. The perfectly rational life or perfectly rational society become not ideals to be striven for, but nightmares, ultimate horrors, to be adamantly opposed. Finally, as we have seen, it is even argued, in the twentieth century, that reason has no place in science!

My claim is that the first kind of decline is all to the good, and should have been welcomed, rather than reluctantly conceded, whereas the second kind of decline is a disaster. Furthermore, what has been responsible, in the end, for the second kind of decline in the scope of reason, has been the misguided resistance to the first kind of decline in the power or authority of reason.[14] As long as reason is conceived of in authoritarian or oracular terms as a set of rules which deliver indubitable, unchallengeable decisions to us, thus taking our power to reach our own decisions away from us, then it is entirely understandable that it should seem highly desirable (or inevitable) that the scope of reason should be severely restricted. It may be acceptable that logicians and mathematicians, and even perhaps scientists in the context of justification, should be in a sense deprived of the power to decide what to accept and reject, being obliged to comply with the dictates of reason. That this should happen in our personal lives is surely horrendous. We should become slaves to reason.[15]

What ought to have been clearly recognized and acknowledged long ago within the rationalist tradition is that the authoritarian conception of reason is a perversion of reason.[16] The whole point of reason is to help us to act and

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[14] Hayek, for example, has failed to understand this simple but crucial point. Hayek argues for the need to restrict the scope of reason; but his arguments only have any validity insofar as reason is presupposed to be powerful, capable of delivering authoritative decisions. Reject this authoritarian conception of reason, and Hayek's reasons for restricting the scope of reason collapse (Hayek, 1967, ch. 5).

[15] An argument along these lines, designed to show that the more rational, the more rigorous we become, so the greater our loss of freedom, is expounded by O'Connor (1973, pp. 44-6).

[16] Demolishing authoritarian conceptions of reason (and of science) as irrational and non-humanitarian is one of the central tasks and achievements of Popper's great works, (1959, 1969). See, in particular, Popper's introduction, (1963): 'On the Sources of Knowledge and of Ignorance'. The present book develops further this theme of Popper's work. Reason itself (almost by definition) is valuably applicable to all that we do: if 'reason' seems to lead us systematically astray, or leads to undesirable results such as enslavement, or a regimented, unspontaneous, uncreative, unimaginative, insensitive or unloving way of life, a despotic society, or a cruel political programme, then it is not reason which thus leads us astray, but some form of irrationality masquerading as reason. It is vital that rationalists be highly critical of any conception of reason – especially as it is applied in practice – just

decide as we really do want to act and decide: it is to enhance *our own capacity* to act and decide as we really want, not to wrench the capacity from us or to reduce it to the one decision to obey henceforth the dictates of reason. This is true in life; it is true in science; and it is even true in mathematics, and in logic. Confronted by a proposition P, which he cannot prove or disprove, the mathematician does not know whether he really wants to accept P or reject it. What a proof or disproof does is to reduce the big, uncertain decision to accept (or reject) P to a number of small decisions the mathematician knows he really does want to make (steps of the proof). A proof thus enhances the mathematician's power to make decisions that he really does want to make. It is of course obvious that mathematical thinking or problem solving – and especially the best, the most creative and original – cannot proceed in a way that is determined by explicitly stated rules. The most that could be said is that in mathematics precise rules determine what is to count as a rigorous *proof*. But it must also be said that mathematicians actually become *irrational* if they merely slavishly obey the rules. For rigour requires that such rules be constantly criticized and revised when found wanting. And in fact one finds that as mathematics has developed, standards of rigour have been again and again revised and improved. Furthermore – and this point is entirely general – no slavish, uncritical obedience to any set of explicit rules, however good they may be, can conceivably amount to being rational simply because adoption of such rules can only be sensible and helpful in certain circumstances and for certain purposes. Slavish following of explicit rules is bound to lead us to follow the rules in contexts and for purposes beyond their domain of fruitful application, the outcome being a characteristic kind of irrational action (a point of great importance to be developed in the next chapter).

The fear that to become wholly rational in one's living and thinking is to become a slave to reason, losing one's freedom and one's soul, can perhaps

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because deformations in the conception of reason embodied in a person's life, in an institution (such as science) or in a society, result in deformations in the life itself, in the institution, in the society. My criticisms of Popper are thus very much in the rationalist, and the Popperian, tradition. A minor point of criticism (alongside the major criticisms) is that an element of authoritarianism lingers on in Popper's conception of reason, of method. For Popper's method, ideally, determines for us, in a fallible way, the best choice. It chooses for us, as it were. The view developed here is that putting into practice the heuristic methods of reason enhances our capacity to choose as we really desire: it enhances desirable spontaneity, creativity, freedom, and does not reduce freedom to the one decision to proceed in accordance with the methods of reason.

best be put wholly to rest by the following consideration. The rules of reason (as reason is being conceived in this chapter), in order to satisfy the requirements of being relatively few in number and completely general, must be high-level meta-rules, which cannot tell us precisely what to do but can only tell us the sort of things we might try to do. Reason presupposes that we can already successfully put into practice a multitude of extremely diverse mostly implicit rules, methods, strategies in performing all the diverse actions we do perform in life – moving about in the world, perceiving, talking, writing and reading, cooking, earning a living, bringing up children and so on. All that the generally applicable meta-rules of reason can accomplish is to indicate – in a way that is open to infinitely many different specific interpretations – how almost infinitely diverse particular rules or methods already being put into practice can best be coordinated or marshalled so as to give us the best chances of solving our problems as we really want, thus achieving what is of real value to us. All of our thinking, feeling, desiring and doing inevitably proceeds in accordance with (mostly implicit) rules or methods of one kind or another whether we acknowledge this or not. This cannot be avoided. Acting spontaneously, or deciding to act spontaneously, does not alter this. The difference between living rationally and irrationally is thus not at all the difference between living in accordance with rules and in violation of rules. Rather, the difference is between exploiting strategies which offer us the best general help with realizing what is of value to us in life and failing to exploit such strategies (or systematically violating such strategies). And finally, it must be remembered that the rules of reason formulated above all tell us what to *attempt,* and do not necessarily specify an invariably performable action. There can thus be no question of slavishly obeying the above rules.

As long as reason is conceived of in quasi-oracular terms as, ideally, a set of methods which can be mechanically applied, scientific discovery, and creativity in general – inherently non-mechanical – are bound to seem beyond the scope of reason, as Reichenbach and Popper have both claimed. But the moment reason is conceived of in terms of non-mechanical methods which, if put into practice, give us the best chance of success (but which do not mechanically reach decisions for us), rational discovery and creativity become possible. In fact, not only do rational methods for creating good possible solutions to problems exist in science and in life (as the methods of rational problem-solving spelled out in this chapter make clear): methods of discovery, and methods of assessment, amount to two equally important, interdependent aspects of reason. Without good, explicit or implicit, methods of discovery, we will have no possible solutions to our problem to assess:

without good methods of assessment, we will fail to choose well between good possible solutions.

The point can be put even more strongly: any conception of rationality which restricts itself to methods of assessment must be defective or irrational. A conception of reason can be said to be defective or irrational (as opposed to merely incomplete) if it can be shown to lead us *systematically* astray. The crucial point is now this. Granted that we tackle a problem P, defined in terms of an aim A, and methods M for assessing possible solutions (which specify what is to count as a solution to P), then, in general, it is to be expected that as we proceed our understanding of P will improve, our aim A will improve, and so too our methods M, our idea as to what is to count as a solution. As long as we adopt the philosophy-of-wisdom idea that (heuristic) methods of discovery H are fundamental to reason, this process of *improving* problems, aims and methods of assessment as we proceed can be quite naturally incorporated into the basic conception of reason: it is implicit in rules (1) and (2). Identifying rules of reason solely with any set of methods M for the assessment of possible solutions cannot allow for this necessary *modification* of such methods: and thus is to be rejected as irrational.

It is worth noting that even if the philosophy-of-knowledge view of science was correct, science having the fixed aim of improving knowledge of truth as such, and having fixed methods of assessment M, nevertheless a strong case could be made out for holding that scientific rationality cannot adequately be conceived in terms of methods of assessment alone. For, as Feyerabend has argued in a classic essay (1965), in order to assess a given theory well empirically, we actually need good rival theories to indicate severe tests. Thus empirical assessment of the given theory (in terms of methods M) actually requires good methods H of discovery to generate good rival theories to indicate severe tests!

The case becomes overwhelming once it is recognized that the philosophy-of-knowledge conception of science is unacceptable. The aim of science is not to discover truth *per se,* but rather *explanatory* truth, and more generally *valuable* truth. It is to be expected that as we proceed, our aims and our methods M of appraisal will improve. In order to be rational in pursuing science we must allow heuristic methods of discovery H, influencing our ideas concerning what our basic scientific aims ought to be, to modify our methods M of assessment. Any attempt to characterize the rationality of science solely in terms of fixed methods M of assessment must be defective or irrational. It must miss the essential thing: the way in which improving scientific knowledge enables us to improve our knowledge about how to improve knowledge. Both in science and in life, our basic aims are

profoundly problematic: rationality requires that we improve aims and methods as we proceed (there being constant interplay between methods of discovery H and methods of assessment M).

I judge this point to be of such great and general importance that it is developed in the next chapter into a general 'aim-oriented' conception of reason.

Chapter Five
Aim-Oriented Rationalism

The philosophy of wisdom can be formulated in a somewhat more general, and perhaps more adequate way by appealing to a more general notion of *aim-oriented rationality*. Instead of characterizing reason in terms of rules which help us to solve problems we can, more generally, characterize reason in terms of rules of action which, when put into practice, give us, other things being equal, our best hope of achieving what is genuinely desirable and of value. All problem-solving is aim-pursuing, but not all aim-pursuing is (conscious) problem-solving, since there is much that we do – even of value – that we do effortlessly, instinctively. A problem is a failed action, perhaps a deliberately over-ambitious action, an attempt to do in some new domain what has already proved to be successful elsewhere. (More specifically, any problem can be construed to be an aim A, a provisional route R to the realization of A – the initial deed designed to realize A – and a barrier B which blocks the attainment of A along R. All real life problems – as opposed to problems set in exam papers – come with initial inadequate solutions, which may only need to be changed somewhat in order to become adequate.) Problem-solving, then, is a special case of aim-pursuing. It is this which enables us usefully to modify our earlier notion of rational problem-solving to form a more general notion of *aim-oriented rational action*. Aim-oriented rationalism has the added advantage that it brings out much more explicitly and generally the important points concerning rationality made towards the end of the last chapter.

The basic idea of aim-oriented rationalism is extremely simple. It can be put like this. Whatever we are doing, our aims are quite likely to be more or less problematic. Contrary to what we may suppose, aims we are striving to realize may not be realizable, or may not be desirable (or may not be as realizable or desirable as somewhat modified aims we might pursue). Thus, whatever we are doing, in order to act rationally we must be able and ready, as the need arises, to improve our aims and methods as we act. Any conception of rationality which does not include this requirement concerning the need to *improve* aims and methods as we act must systematically lead us astray, fail to help us realize what is of most value to us (on all those occasions when we pursue unrealizable or undesirable aims). All such conceptions of rationality must thus be rejected. Quite generally, in order to be rational, we must be ready to look critically and imaginatively at our aims, to give ourselves the opportunity to discover how to pursue more desirable

or more realizable aims; we must be ready to ask *why* we are pursuing the aims we are pursuing – in both the rationalistic, and historical or causal senses of 'why' – so that we may discover ways in which our aims can be improved; above all we must do all that we can to ensure that we are not *misrepresenting* to ourselves what aims we are pursuing – since if we *misrepresent* our aims to ourselves, our capacity to realize our actual aims rationally and successfully is seriously undermined. All this applies to whoever or whatever is doing the aim-pursuing, whether it be an individual person, a group of people, or an institution or social organization.

Any action that we perform – whether it be the action of an individual, of a group of people, or of an institution – has an aim and exemplifies a methodology. Thus, in improving our (personal and social) aims and methods of living we are improving what we do and are, our personal and social lives. It is of course always possible that a new, or an improved, method may first be discovered as a particular, successful new action – the more general method implicit in this action only subsequently being exploited by a variety of analogous actions. Even when a common set of methods inform a range of our actions, these methods mostly in practice remain implicit, it being perhaps impossible for us to formulate explicitly methods we successfully put into practice in doing such things as teaching, playing the violin, moving our limbs, constructing grammatical and meaningful sentences (all of which we must learn).

The prescription 'endeavour to improve your aims and methods as you live' is important just because it is simple, universal in its application, fundamental, and widely neglected.

In an attempt to demonstrate just how important, and how much neglected, how poorly understood, this simple idea of aim-oriented rationality is, I propose now to apply it to one important human endeavour, one important institutional enterprise, namely *science* – and more generally *academic inquiry*. What I propose to do is to begin with science as conceived of by standard empiricism and the philosophy of knowledge and show how *four successive applications* of aim-oriented rationalism transform standard empiricist science into philosophy-of-wisdom inquiry – into a version of this kind of inquiry, indeed, that constitutes a clarification and improvement of the version formulated in the last chapter. My claim is that the outcome of each of these four applications of aim-oriented rationality is a kind of science, a kind of inquiry, that is both of greater rationality and of greater human value – the end product thus being very much more rational and valuable than what we begin with. Each of these conceptions of inquiry – represented in figures 5b to 5e – upholds a different basic, overall intellectual *aim* for inquiry,

and thus different basic, overall *methods*. Each of these conceptions of inquiry thus upholds a different conception of *intellectual progress,* and a different conception of the nature of the *problems* that rational inquiry is concerned to solve. Each conception of inquiry is more intellectually rigorous, more rational, than its predecessor, in the straightforward sense that it explicitly articulates, criticizes, and thus seeks to improve by rational means, assumptions that are substantial, influential and problematic but unacknowledged and thus only implicit in the conception of inquiry that is its predecessor. In other words, each step in the argument acknowledges more honestly what the aims of inquiry actually are, and what they ought to be: at the same time this involves acknowledging, what before was suppressed, namely the profoundly problematic character of the more honestly represented basic aims of inquiry. Acknowledging explicitly the *problematic* character of the actual aims of inquiry is important because, as a result, the problems associated with these aims can be explicitly discussed as an integral part of inquiry itself, this in turn holding out the hope that improved solutions to the problems can be rationally developed, the outcome being progressive improvement of aims and methods of inquiry as inquiry proceeds (the essence of aim-oriented rationalism).

Here, then, is the argument, set out in four steps. (1) to (4), each step exhibiting the same pattern of argumentation.

We begin with natural science conceived as having the basic intellectual *aim,* in the context of verification, of improving knowledge about the world, no presuppositions being made about the world, the basic *method* being to assess empirically testable conjectures about the world entirely impartially with respect to their empirical success and failure alone (see figure 5a). There is a fixed aim and a fixed method; there are essentially just two domains of discussion, namely that of (a) observational and experimental results and (b) theory. An idea, in order to enter the intellectual domain of discussion of science, must at least be an empirically testable claim to knowledge (observational or theoretical).

1 This standard empiricist view seriously *misrepresents* the true intellectual aim of science. The aim of science is not merely to discover truth *per se,* nothing being presupposed about the nature of the truth to be discovered. A basic aim of science is to improve our *understanding* of the world. Science seeks *explanatory truth*. Even in the context of verification, the whole enterprise of natural science must presuppose (conjecturally, no proof or experimental verification of this being possible) that the universe is comprehensible to us, in some way or other.

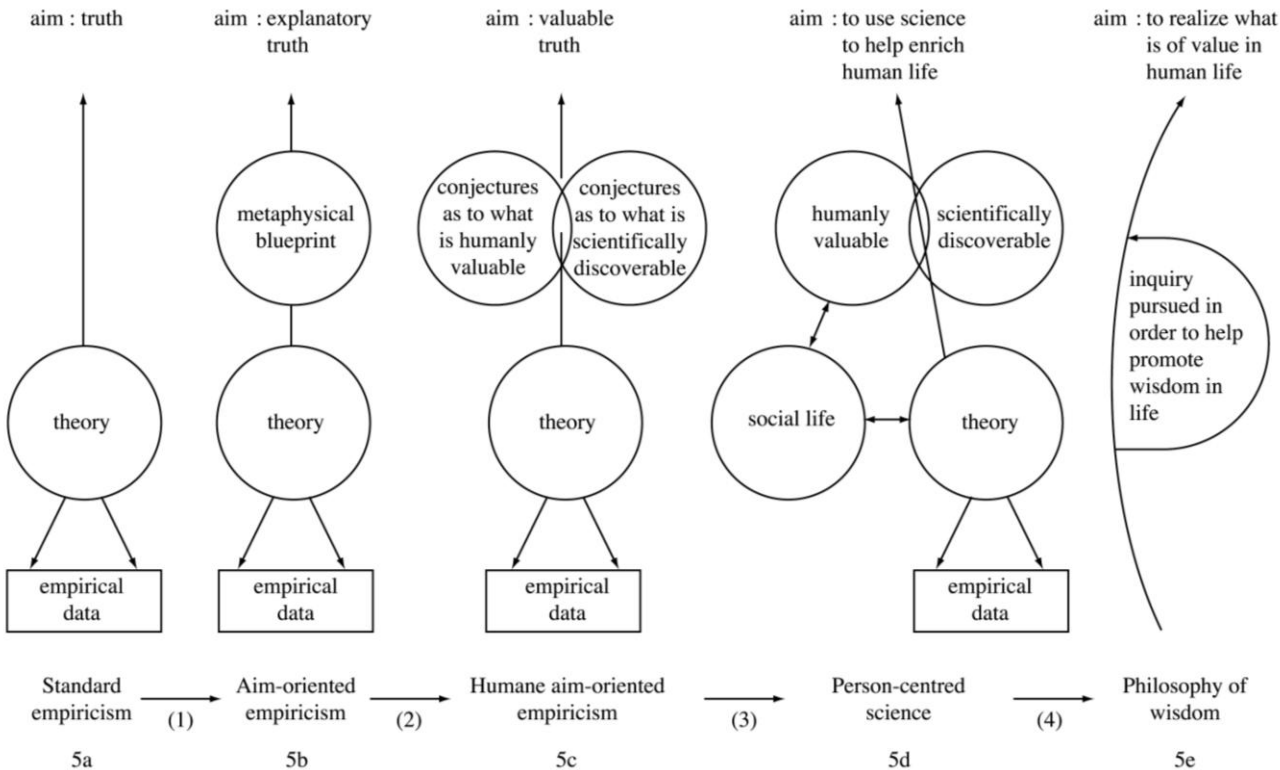

Figure 5 Aim-oriented rationalism applied to academic inquiry

More specifically, modern natural science presupposes that there exists some kind of unified pattern running through all natural phenomena, it being a basic *aim* of physics to articulate this pattern as a testable, comprehensive, unified theory. In the absence of some such presupposition it is impossible to choose between the potentially infinite number of rival possible theories, equally acceptable from an exclusively empirical standpoint, that can always readily be formulated. In practice in science this situation is avoided by giving preference to those few theories that simplify and unify – so much so that empirically successful systems of propositions which do not simplify and unify do not count as 'theories' at all. This in practice commits science to the presupposition that unity exists in Nature to be discovered – a crucial point that will be established in chapter 9 (and discussed further in chapter 14).

The aim of discovering some kind of unified, comprehensible pattern in the world, in terms of which phenomena can be explained and understood, is however profoundly *problematic*. Precisely what unified pattern does exist? In broad outline, what kind of pattern exists? What does it mean to assert that some kind of unified comprehensible pattern exists in Nature? Why should any such pattern implicit in the physical universe be comprehensible to us? Why should the universe be comprehensible at all? In terms of what concepts, what invariance and symmetry principles, is any such unified pattern to be characterized? What modifications need to be made to existing fundamental physical theories, and fundamental physical concepts (having to do with such things as space, time, energy, force, particle, field) in order that a clearer picture may be given of the conjectured unified pattern than at present? What grounds can we have for holding conjecturally that some kind of unified pattern is implicit in all phenomena? How can it be rational to commit science to such an article of faith, such a 'miracle-creed'? If some kind of unified pattern is implicit in all phenomena, more or less like patterns postulated by the fundamental theories of physics, how is it possible for there to be consciousness in the world? How is it possible for us to exist, experiencing, feeling, enjoying and suffering beings? How can there be colours, sounds, smells as experienced by us? If all that we are and do conforms to a fixed pattern of physical law, how can there be any free will? How can we be responsible for any of our actions, our thoughts, desires and decisions? How can there be purposiveness in the world? How can there be life? How can our lives have any meaning or value?

If science is to pursue its basic intellectual aim of improving our understanding of the world in a rational way – in a way which gives us the best hope in general of making progress towards the realization of this aim – then it is essential that these *problems,* associated with this aim, receive explicit

rational discussion as an integral part of science itself. The intellectual domain of science must include *three* interrelated departments of discussion: (a) discussion of observational and experimental results; (b) discussion of testable theories; and (c) discussion of problems associated with the basic aim of improving understanding. This third department of discussion must seek (i) to improve the articulation of the problems just indicated and (ii) to propose and criticize possible and actual solutions, in an attempt to improve the basic *aim* of science. Untestable, metaphysical ideas need, in other words, to be proposed and criticized (in the light, of course, of current scientific empirical results and theories) from the standpoint of their capacity to improve solutions to problems that arise in connection with the search for scientific understanding, as an integral part of science itself, in an attempt progressively to improve the overall aim of science actually being pursued. And as the overall aim of science is improved, so too the methods of science can be improved. In short, as we improve our scientific knowledge and understanding of the world, we improve our (conjectural) knowledge and understanding of the domain of our ignorance; this enables us to improve the aims and methods of science; we thus improve our knowledge about how to improve knowledge – a vital feature of scientific method which helps to account for the relatively recent explosive growth of scientific knowledge. The philosophy of science (the enterprise of articulating aims and methods of science) thus turns out to be a vital part of science itself, which must evolve as an integral part of the evolution of scientific knowledge, if science is to be rational. (Standard empiricist philosophy of science, seeking to understand the rationality of science, but pursued as a discipline distinct from science, actually helps thereby to undermine the very thing it seeks to understand.)

As a result of correcting a serious misrepresentation of the basic aim of science (this in itself an application of aim-oriented rationality) scientific inquiry is revealed to exemplify, in a striking way, the basic idea of aim-oriented rationality (as briefly characterized above). I shall call this conception of science *aim-oriented empiricism* (see figure 5b).

According to aim-oriented empiricism, untestable ideas about how the world is comprehensible inevitably exercise a profound influence over what theories are accepted in science, and what research aims are pursued. Thus, if science is to proceed rationally, it is essential that such untestable ideas be articulated and criticized as an integral part of science itself, within the intellectual domain of science. Standard empiricism, on the other hand, demands precisely the opposite. Untestable ideas must be excluded from the intellectual domain of science. Scientists are, of course, permitted to

propound and criticize untestable conjectures in the domain of discovery, to themselves and to each other, unofficially over coffee, as it were. What standard empiricism does not permit is the publication and criticism of untestable proposed solutions to problems inherent in the aim to understand in the official scientific literature, in the context of verification. This prohibition arises from a misguided attempt to preserve the rationality of science (misguided as a result of the basic aim of science being misrepresented to be discovery of truth *per se).* Actually the prohibition serves only to undermine rationality. Once the intellectual aim of science is acknowledged to be to improve understanding, it is clear that in order to pursue this aim *rationally* it is essential that we explicitly propose and criticize rival possible solutions to problems inherent in this aim, within the intellectual domain of science, in an attempt to improve the aim as we proceed. It is just this which we cannot do if we adopt the misconceived intellectual standards of standard empiricism.

Inevitably, if the scientific community pursues science in accordance with standard empiricism, scientific progress towards improved understanding of the world must tend to suffer as a result of the characteristic irrationality of standard empiricism. The scientific community as a whole will fail to improve the aim of understanding the world, in an explicit, cooperative way, as a result of the failure explicitly to articulate and criticize diverse possible solutions to the problems inherent in this aim. A few individual scientists (such as Darwin, Faraday or Einstein) may individually improve their research aims in this way, and as a result they may well make many significant (testable) discoveries: they will be unable, however, to communicate to their fellow scientists how they have made such discoveries (since standard empiricism prohibits such communication). Many scientists will be brain-washed by standard empiricism and hence will fail to discover anything of much significance from the standpoint of improving our understanding of the world. As a result of the failure of the scientific community to articulate and criticize diverse possible solutions to the problems inherent in the aim to improve our understanding of the world, it is quite likely that the scientific community will accept, as a body, in a dogmatic and uncritical way, some set of answers to such problems, for a time science almost being defined in terms of these dogmatic answers, so that all research and all theorizing proceeds within the framework of these answers. Eventually *empirical* problems – clearly recognized by standard empiricism – may become so overwhelming, that a new, empirically more successful, comprehensive theory may be developed, violating the old solutions to problems of under-standing but being in accordance with some new implicit set of possible

solutions. It will not be possible to discuss the transition from the old to the new theory *rationally,* just because problems of understanding cannot be explicitly discussed within science. The transition will thus be made in the irrational way so brilliantly depicted by Kuhn (1962). Scientists, and historians and philosophers of science may even hold, with Kuhn (1970), that this is the way science ought to develop; it will not be seen as the unfortunate consequence of the irrational suppression of sustained imaginative and critical discussion of problems of scientific understanding, within the intellectual domain of science itself.

Failure to articulate the scientific aim of improving understanding may well lead science to degenerate into nothing more than the enterprise of predicting more and more phenomena more and more accurately. Those few scientists who prize the search for understanding above all else, and who protest, will tend to be dismissed as unscientific metaphysicians or philosophers. The united, cooperative endeavour to improve understanding is likely to disintegrate into fragmentary, disorganized, specialized research endeavours, with aims unrelated to each other, and often obviously defective, even though the specialized scientists who pursue these aims will not realize this. The vital task of attempting to interconnect these diverse, disorganized research aims will fall into disrepute as philosophical and unscientific. The scientific aim of improving our understanding of the world may even itself fall into disrepute. It may be declared unscientific. The world may be judged to be incomprehensible. An intellectual disaster will have overtaken science (from the standpoint of improving understanding) and most scientists will not even notice as long as much specialized knowledge, however trivial, is being accumulated.[1]

The outcome of rejecting standard empiricism and adopting instead the more intellectually honest philosophy of science of aim-oriented empiricism might be, at first, for many scientists, disconcerting in that suddenly a wide range of intellectual defects of science leap to the eye that were before invisible. The eventual outcome would be, however, the (gradual) transformation of standard-empiricist science into something more closely resembling the natural philosophy of the seventeenth century (see chapters 9 and 14; see also Maxwell, 1998; 2002b; 2004b; 2005b; 2007a).

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[1] Something a bit like this has occurred as a result of the general acceptance of orthodox quantum theory, restricted as it is to making predictions about the results of measurement: see Maxwell (1998, ch. 7).

2 Given the profoundly problematic aim for science of seeking to improve knowledge of explanatory truth, of seeking to improve our understanding of the world, we next need to ask, according to aim-oriented rationality: why are we seeking to realize this aim? What more general or more fundamental intellectual aim do we, and ought we to, seek to realize by its means? There is, I submit, an obvious general answer to this question. We seek to improve our knowledge of explanatory truth because, more generally and fundamentally, in doing science we seek to improve knowledge of *humanly valuable truth,* of value culturally or practically, explanatory truth being one kind of valuable truth. In pursuing science we seek to discover knowledge that is of the greatest value to humanity, of greatest value from the standpoint of developing a healthier, richer, more just, more civilized world. Above all our concern should be, so we may hold, to develop knowledge that is of most value to those whose needs are greatest – the poor, the ill, the suffering. But in any case, quite properly, scientific progress is assessed in terms of the extent to which knowledge of valuable truth is increasing, growth of knowledge of trivial truth only, however extensive, being not progress but rather stagnation and decadence. Thus, in assessing a potential contribution to science (in order to decide whether it deserves to be published in a scientific journal for example), value and truth factors must both be taken into account. A contribution almost certainly true (and thus representing knowledge) may legitimately be rejected on the grounds of its triviality. A contribution almost certainly false may be accepted for publication (and even accepted as a great contribution to science) because of its potential value, its potential fruitfulness perhaps (even though false), or its value if by chance true. Thus considerations of value and truth cannot, and ought not to be, dissociated from one another even in the assessment of scientific results, and certainly not in the assessment of aims for research.[2]

The aim of improving knowledge of *humanly valuable truth* is, if anything, even more profoundly problematic than the aim of improving knowledge of explanatory truth, the aim of improving understanding. What is of value? Whose values, whose needs and desires, ought to be given priority? What is there potentially of value in the domain of our ignorance, awaiting discovery by us, capable of being discovered and exploited in desirable ways by means of present methods? What will be of value to humanity in ten, fifty, one hundred years time? How can science do justice to the value of acquiring

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2 This is the very opposite of holding that we should accept as true what it would be of value for us to believe *if* true, as I pointed out in the last chapter: see also the discussion of O'Hear in ch. 13.

knowledge and developing technology, including medicine, that is most needed by the world's poorest people, and at the same time do justice to the value of improving knowledge and understanding of the universe for its own sake – especially as such knowledge is often esoteric, remote from the concerns of most people, and unlikely to lead to the development of new technology of any kind, let alone of the kind most urgently needed, as in the cases of astronomy, cosmology, high-energy physics? How can science contrive to give priority to the needs of the world's poor when scientific research is mostly financed by, and thus presumably responsive to the interests of, the world's wealthy? Does science do as government, industry or popular opinion bids: or does it seek to acquire knowledge of truth deemed by the scientific community itself to be of value? What research aims and priorities are to be taken up, who is to decide, and how (in terms of what criteria)?

The prescription of aim-oriented rationalism is essentially just the same as that already discussed in (1) above. If science and technology are to pursue their basic intellectual aim of improving knowledge of humanly valuable truth in a rational way then it is essential that the problems associated with this aim receive explicit rational discussion as an integral part of science itself. The third department of the objective intellectual domain of science, recognized by aim-oriented empiricism, devoted to the discussion of problems associated with the aim to understand, will need to be broadened to include discussion of problems associated with the aim to improve knowledge of *valuable truth*. In order to choose aims rationally it is essential that we bring together discussion of factual (but possibly untestable) conjectures as to what exists to be discovered, and evaluative conjectures as to what it is genuinely desirable to try to discover, in this kind of way. And, furthermore, since the scientific community cannot claim to possess any special expertise which enables it to determine what is of human value better than the rest of us, the non-scientific community must be encouraged to take part in the sustained imaginative and critical discussion of aims for science within the intellectual domain of science. Whoever makes the actual decisions as to what research aims are to be pursued – individual scientists, heads of research laboratories and university departments, or grant-giving bodies – these decisions need to be informed and critically assessed by open discussion in scientific literature, and elsewhere. As a result of imaginatively and critically discussing the profound problems associated with the aim to improve knowledge of valuable truth, in an open, cooperative way, as an integral part of science – and as a result of actively promoting, and responding to, such discussion in society – the scientific community may be

able, as it proceeds, gradually to improve its actual overall aims and methods, its priorities, from the standpoint of discovering truth of most value to humanity. This view of science may be called *humane aim-oriented empiricism* (see figure 5c).

Standard empiricism excludes discussion of values from the intellectual domain of science in an attempt to preserve scientific rationality. Actually this serves only to undermine rationality, in that it places influential and problematic ideas (concerning what is of value) beyond criticism within science.

Inevitably, if the scientific community does proceed in accordance with standard empiricism, scientific progress towards acquisition of knowledge of most value to humanity must tend to suffer in characteristic ways as a result of the characteristic irrationality of standard empiricism. The scientific community, together with the non-scientific community, will fail progressively to improve the aim to discover truth of most value to humanity in an open, explicit, cooperative way, as a result of the failure to articulate and criticize diverse possible solutions to the problems inherent in this aim. A few individual scientists may seek to improve their individual research aims in this way. Unfortunately, as they do so, they are increasingly likely to fail to get funds to make such research possible. Furthermore, they will be unable to communicate to their fellow scientists the need for the scientific community as a whole to improve the basic intellectual aim of science in this way, since standard empiricism prohibits such communication. Indeed, the attempt of such scientists to convince their fellow scientists of the need to pursue science more rigorously, by articulating and criticizing possible solutions to the problems inherent in the aim of discovering *valuable* truth will be vehemently opposed by the majority who, accepting standard empiricism, will see the intrusion of moral and political ideas and problems into science as a threat to the objectivity, the rationality, the intellectual integrity of science. In seeking to preserve the intellectual integrity of science, with the best of intentions, they will be preserving a characteristic kind of irrationality in science. The failure of the scientific community as a whole actively to promote open, imaginative and critical discussion of problems associated with the aim of discovering *valuable* truth will almost inevitably result in that community pursuing research aims and priorities that merely reflect the interests and the values of those sufficiently rich and powerful to pay for scientific research – industrial concerns and governments of wealthy nations. The scientific community must tend to fail to develop and pursue research priorities that reflect the interests of the world's poor, hungry and suffering; general adoption of standard empiricism will shield the scientific community

from an awareness of the extent of their betrayal of humanity. Indeed, the scientific community may well, with a clear conscience, and with a full sense of scientific righteousness, pursue goals that are only of much value to the scientific community itself, because the truth discovered happens to be of interest to some scientists (even though otherwise relatively uninteresting and useless) or of value because of the gladiatorial interests inherent in science: Nobel prizes are won, scientific reputations are made, careers are advanced. In choosing research projects, scientists may well be influenced more by their concern to advance their position in the pecking order of international scientific reputations than by their concern to help alleviate the harsh conditions of life experienced by millions in central and south America, in Africa and in Asia, by relevant technological, medical or agricultural research. Furthermore, the scientific community must tend to fail to organize the awarding of scientific honour and status in such a way that these are to be achieved by successful research of maximum benefit to humanity – even though not necessarily scientifically fashionable or glamorous. Occasionally, dramatic improvements in scientific understanding may also lead to dramatic advances in technology of just the kind most needed by those whose plight is the greatest. But as scientific understanding has improved, the likelihood of scientific discoveries being of value in both these ways becomes progressively less and less. A major problem arises: how to balance the value of improving understanding against the value of relieving suffering. Standard empiricism, obscuring the need to give sustained intellectual attention to this agonizing problem, must tend to produce a science that merely predicts more and more phenomena more and more accurately, thus failing both to improve understanding and to lead to the development of the kind of technology most urgently needed. Scientific and technological research will come to suffer from all the defects discussed in chapter 3.

One point must be emphasized. I am not arguing that these failings of science arise because scientists are wicked, cowardly or selfish. And nor am I arguing the other side of the coin to this: that if science is to avoid these failings scientists need to be nobler, more compassionate and courageous. The argument is entirely different. It amounts to this. The aim of discovering truth of most value to humanity is inherently profoundly problematic. If science is to make good progress towards achieving this aim, then diverse conjectures about how to solve the problems of the aim must be persistently articulated and criticized within the intellectual domain of science, every attempt being made to interconnect this discussion concerning aims with research aims actually being pursued. The scientific community does not only need to learn at the level of empirical data and theory: in addition it needs to

learn at the level of aims, by means of the standard procedure of conjecture and criticism. If the scientific community excluded from the intellectual domain of science the open proposing and criticizing of theories, decisions about what theories science is to accept being reached by committees, it would occasion no surprise if, as a result, science manifested a profoundly defective theoretical knowledge. Likewise, if the scientific community excludes from the intellectual domain of science the open proposing and criticizing of conjectures about aims, decisions about what research aims are to be pursued being reached by (grant-giving) committees, it should occasion no surprise if, as a result, science manifests a profoundly defective choice of aims. Thus any scientific community which adopts standard empiricism, and which thus excludes discussion of aims from the intellectual domain of science, must inevitably come to pursue profoundly defective aims, even though the community is made up of reasonably 'noble' individuals. If standard empiricism is indeed built into the institutional structure of modern science, then it should occasion no surprise whatsoever if modern science also exhibits intellectual and humanitarian failings of the kind just described. In this case what needs to be done is to remould the intellectual/institutional structure of science in accordance with *humane aim-oriented empiricism*. In this way we may be able to institutionalize an intellectually and morally nobler, more compassionate and courageous science, without it being necessary to make unrealistic assumptions about scientists. Some recent steps in this direction will be discussed in chapter 12.

3 Given the (problematic) aim for science of seeking to improve knowledge of *valuable truth,* we next need to ask – according to aim-oriented rationality: why are we seeking this aim? Why ought we to be seeking this aim? What more general or more fundamental aim do we seek to realize by its means? Once again, in general terms the answer is surely obvious. Science seeks to improve knowledge of valuable truth in order to make it available to people so that it may be used by them in order to help enrich the quality of their lives. Knowledge and understanding, however potentially significant or important, mean nothing as long as they remain in scientific journals not used or appreciated by anyone. This is obviously the case as far as technological discoveries are concerned: but it is also true of contributions to 'pure' science. What ultimately matters, from this latter standpoint, is the curiosity, wonder, knowledge, and understanding achieved by, and shared between, people. Pure science is of value insofar as it *is* this, or contributes to this.

As before, however, this aim of science is profoundly – and notoriously – problematic. Science has unquestionably been successfully used by millions

of people to enrich their lives. Scientific and technological discoveries have made it possible to create the industrially-advanced modern world, to be found in Europe, the USA and elsewhere, with all its amenities, freedoms, health and longevity – incomparably more wealthy a way of life than anything to be found in earlier times. And furthermore, the immense advances in scientific knowledge and understanding achieved during the last century or so enable each one of us to explore imaginatively this mysterious cosmos in which we find ourselves – thus enriching our life – to an extent far beyond what was possible in earlier times. The immense diversity of living forms on earth, their diverse character and ways of life; the miracle of the slow evolution of life on earth during some 3 billion years at least; the far reaches of space, with its stars, pulsars, galaxies, quasars, black holes; the far reaches of time, stretching back even to the first few moments of the cosmos; the ultimate nature of matter, the strange domain of the quantum, and the unifying patterns of natural law embedded in all phenomena: these extraordinary and remote aspects of our world have been thrown open to our personal inspection by modern science.[3] But alongside these successes, there are the notorious (already discussed) problems and failures associated with the aim of using science and technology to enrich life – problems associated just as much with the cultural as the technological aspect of science. Thus non-scientists who are otherwise highly educated often profess ignorance of and hostility towards science, as C.P. Snow once reminded us (Snow, 1964). Most people in industrially-advanced countries, though surrounded by the products of science, are probably merely bemused by and somewhat resentful of the esoteric, autocratic mysteries of modern science, while most of those who live in the third world can have few opportunities to learn about science. It is by no means clear, in any case, how the scientific vision of the world can enrich our lives. According to this vision, so it would seem, everything – including ourselves – is made up of a few different sorts of fundamental particles interacting in accordance with precise law. Our freedom, our consciousness, our individuality, all the colour, richness, meaning and value of life, seem to fade away entirely, leaving nothing but leptons and quarks! It is not obvious that such a vision of the world is life-enhancing.

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[3] Excellent non-technical expositions of these topics are to be found in Weinberg (1977); Davies (1979); Silk (1980); Attenborough (1981); Mulrey (1981); and, more recently, in Adair (1987); Gould (1989); Thorne (1994); Guth (1997); Greene (1999); Benton (2003); Penrose (2004), to refer to a few – the Penrose somewhat more technical than the others, but a magnificent work of natural philosophy.

The prescription of aim-oriented rationalism is essentially just the same as that discussed in (1) and (2) above. In view of the profoundly *problematic* character of the aim of enriching life with science, it is essential, if science is to pursue this fundamental aim in an intellectually rigorous way, that science does all it can to promote and sustain explicit, imaginative and critical discussion of problems associated with the aim, both within science, and in the community as a whole – a conception of science that may be called *person-centred science (*see figure 5d).

As before, standard empiricism and the philosophy of knowledge, in excluding discussion of problems associated with the aim to enrich human life from science, undermine both the rationality of science, and the capacity of science to enrich human life.

4 We come now to the crux of the entire argument. Given the (problematic) aim of exploiting science in order to enhance the quality of our lives, we once again need to ask – according to aim-oriented rationalism – precisely as before: why are we, and ought we to be, seeking to realize this aim? What more general or more fundamental aim do we seek to realize by its means? Once again, the answer, in general terms, is obvious. We endeavour to realize this aim because, more generally and more fundamentally, we endeavour to realize what is of value to us in life, as we live. The pursuit of science and technology is but an aspect of, a tributary to, our central and fundamental pursuit of value in life.

The vital point that now needs to be recognized is that this fundamental aim of realizing what is of value in life, is, if anything, even more inevitably and profoundly *problematic* than the previous aims for inquiry discussed in (1), (2) and (3) above. What ultimately is of value, given the brevity of life, given that all that we do, experience and suffer in the end comes to nothing, and given that the world really is more or less as modern science tells us it is? What is it in life that we should seek to attend to, to realize, to cherish and to love?

In a changing world that we only partly know and understand, inevitably our personal and social aims must have their problematic aspects. Quite generally, aims that we are pursuing may not be in our best interests because – despite appearances to the contrary – they are unrealizable in principle, unrealizable in practice, not as desirable or as realizable as modified or different aims available to us, not the most desirable or realizable means to more general or distant goals we seek to realize, undesirable because of unforeseen, undesirable consequences, undesirable as a result of being inadequate resolutions of conflicts between desires or aims.

Thus, quite generally, in order to pursue aims rationally, in such a way that we give ourselves the best chances of realizing what is really of value to us, it is essential that we acknowledge the inherently problematic character of our aims, and the possibility that we may have misrepresented to ourselves the problematic aims we are actually pursuing. It is essential that we imaginatively articulate and critically assess possible solutions to problems inherent in the aims we pursue, as an integral part of our aim-pursuing, in an attempt to improve our aims and methods as we proceed. In endeavouring to help us realize what is of value in life, the fundamental intellectual task of organized inquiry – of science, technology, scholarship and education – is to help us, individually and cooperatively, to improve our aims and methods in this way.

It is above all the humanities and the diverse branches of social inquiry that have this fundamental intellectual task of helping us improve our personal and social aims and methods in life. The social 'sciences' are not *sciences* at all. They are social *methodologies* or social *philosophies* – concerned to articulate, and to criticize diverse actual and possible aims and methods for our diverse personal and institutional endeavours. What (aim-oriented) *scientific methodology* is to science, so *economic methodology* is to actual *economic endeavour* in the real world, *political methodology* is to *politics, sociological methodology,* more generally, is to our diverse *institutions* and *social endeavours*. On this view, the *sociology of science* is precisely the same thing as the *methodology of science* (or the philosophy of science). This book thus itself exemplifies philosophy-of-wisdom social inquiry. What I attempt here for the academic enterprise, other social inquirers need to attempt for other institutions – government, industry, the media, the law, international relations. Economics, political philosophy, sociology, psychology, history, anthropology, the study of international relations, philosophy, the study of industrial relations, education studies, the sociology or philosophy of science, of art, of literature, of drama, of religion: all are concerned to help us build cooperative aim-oriented rationality into our diverse personal and social endeavours, thus giving ourselves better opportunities to realize what is of value to us in life. Insofar as these diverse academic disciplines seek to acquire knowledge, this is acquired in order to further the fundamental intellectual task of helping us in practice improve aims and methods in life. All these diverse disciplines may be regarded as aspects of *Utopian studies* — the enterprise of imaginatively articulating and severely criticizing possible and actual aims and methods for humanity as a whole.

On this view, then, those who hold that the methods of the social sciences are similar to those of the natural sciences (the pro-naturalists) and those who hold that they are different (the anti-naturalists) are both wrong.

Social inquiry (not *science* at all) seeks to establish unity of method throughout all social endeavour, including science. It seeks to help enhance wisdom in life, by helping to build aim-oriented rationalism into the fabric of personal, institutional and social life.

The outcome of this entire argument (1) to (4) above, is thus rational inquiry as conceived of by the philosophy of wisdom (see figure 5e). As a result of four successive applications of aim-oriented rationalism, standard empiricist science has been transformed into philosophy-of-wisdom inquiry.[4] This new version of the philosophy of wisdom incorporates, and in some respects improves on, everything depicted in chapter 4. It clarifies what ought to be the aims and methods of natural science and of social inquiry. It clarifies how natural science and social inquiry ought to be related to one another, and to personal and social life. And the entire argument establishes, in outline, that this improved version of the philosophy of wisdom depicts a more intellectually rigorous and a more humanely valuable kind of inquiry than that depicted by the philosophy of knowledge.

It deserves to be noted that, just as at stages (1) and (3) above, so now at stage (4) of the argument, it can be shown that the attempt to pursue inquiry in accordance with standard empiricism and the philosophy of knowledge is damaging in both intellectual and human terms. This attempt leads economists, sociologists, psychologists and others, whether pro- or anti-naturalists, to pursue social inquiry as the academic enterprise of improving knowledge about diverse aspects of social phenomena, in a way that is more or less dissociated from society itself. The chief intellectual task becomes to solve sociological (or economic or psychological) problems of knowledge, not practical problems encountered by people in life. Thus the vital social and methodological task of social inquiry of helping us build aim-oriented rationality into our personal, institutional and social lives, demanding for its fulfilment active involvement with social life, is prohibited.

Among other advantages, aim-oriented rationality is more helpful than 'problem-solving' rationality when it comes to resolving conflicts between people. The way we formulate our problem depends on what we take our aim to be. Thus two people, caught up in some common enterprise, but with conflicting aims, will formulate their common problems in different ways. As a result, each may regard the other as illogical, merely self-interested, engaging in trickery, bluff, propaganda. This does not help cooperative rationality to develop. By contrast, putting aim-oriented rationality into

[4] A dramatization of this argument can be found in Maxwell (1976b). It is restated in Maxwell (2004b).

practice enables us to avoid such unnecessary, destructive misunderstandings, and helps us – if we so wish – to develop gradually more cooperative ways of resolving our conflicts. In roughly increasing levels of desirability, conflicts between people are settled by: force, threat, manipulation, some more or less arbitrary procedure (such as tossing a coin or voting), bargaining, the cooperative discovery of the most desirable, just resolution. The general adoption of the aim-oriented conception of reason is in all our long-term interests in that it offers us the best hope of increasing our capacity to resolve our conflicts in rather more desirable ways – even though, of course, it provides no magic procedure for resolving conflicts.

Aim-oriented rationality can be regarded as a kind of empiricism, in that it specifies a general methodology for 'learning from experience'. However 'experience' must be understood here in commonsense terms as that which is acquired through action, doing things, living, actively engaging in some enterprise. And what is learnt is how to do things, how to live, how to achieve that which is desirable and of value, varieties of wisdom.

Looked at in this way, aim-oriented rationality transforms philosophy into an essentially empirical, practical enterprise. In actively engaging in some endeavour, in pursuing aims, and adopting methods in order to *realize* aims, we in effect put into practice a 'philosophy' of our endeavour, whether we are aware of this or not. Other things being equal, we give ourselves our best chances of learning from experience if we articulate, imaginatively develop, and scrutinize philosophies that we put into practice in our various endeavours in life. Above all, by developing a *tradition* of accurately articulating our actual philosophies, our actual aims and methods in life, we make it possible for us to learn from each other's experience, from each other's successes and failures. This essentially is what aim-oriented rationality amounts to: clearly it gives to 'philosophy' the practical task of helping us to improve our aims and methods as we live – of enhancing our capacity to learn from experience. In order to be successful, of course, it is essential that we are able to be honest, to ourselves and to each other, about what we are doing, what our actual aims and methods are, what it is we desire and feel. A basic task for inquiry, according to the philosophy of wisdom, is to help us to develop a society, a world, in which such honesty is encouraged to flourish as opposed to being penalized.

One feature of the above argument, (1) to (4), is that it repeatedly establishes that academic inquiry seriously misrepresents the basic intellectual aim of inquiry. The real but profoundly problematic aims for science of

discovering *explanatory* truth, or *valuable* truth, are misrepresented by standard empiricism to be the apparently unproblematic aim of discovering truth *per se.*

In honour of Freud, any aim-pursuing endeavour, whether personal or institutional, that misrepresents its aims in this way may be said to be suffering from *rationalistic neurosis.*[5] Thus, according to the above argument, theoretical physics, and science more generally, suffer from rationalistic neurosis, as depicted in figures 6a and 6b.

Quite generally, in order to act rationally, it is essential to be open to the possibility that one's aim-pursuing endeavours suffer from rationalistic neurosis. This is because rationalistic neurosis – misrepresentation of aims – is almost bound to occur, and once established, can be profoundly damaging.

It might be thought that the assertion that people and institutions tend to suffer from misrepresentation of aims, from rationalistic neurosis, must amount to a highly speculative, dubious psychoanalytic theory, Freudian or post-Freudian, of uncertain scientific standing. This entirely misses the point. Representing to oneself or others the goal one is pursuing is itself a goal-directed endeavour, which may succeed or fail like any other. Furthermore, it is often highly problematic to represent or characterize accurately the goals actually being pursued by animals, people, or institutions. We cannot declare merely that the goal is what results as the outcome of the being's actions – since this leaves open the question of precisely how any outcome is to be characterized, and in any case does not take into account the important possibility that the being may pursue some goal G and may fail to achieve it. In attributing a goal to any being, we invariably offer an interpretation of the being's actions; in many cases, it may be hard to choose between a number of such rival interpretations, rival attributions of goals to the beings in question. One principle can be employed in deciding what goal or goals a being is in fact pursuing: a being is to be interpreted as in fact pursuing that goal (or goals) which, if postulated, makes the best overall sense of what the being *does* (taking internal 'imaginings' etc.. and misunderstandings into account), and which, at the same time, accords best with what the being has done in the past, and how it has come to be. It is something like this principle, implicitly understood, which leads us after Darwin, to attribute to animals the overall goals of survival and reproduction.

It is thus an uncertain, theoretical matter to characterize correctly goals in fact being pursued by animals, people and institutions, even when the person in question is oneself. Goals may be misrepresented for Freudian reasons, as

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[5] This important methodological notion of rationalist neurosis was first introduced in Maxwell (1976b, pp. 206-221). It is further developed in Maxwell (2004b).

a result of the repression of problematic aims (as in the case of science). Equally, goals may be misrepresented merely because the difficult task of representing goals accurately has not yet been accomplished. In holding that people misrepresent the goals they are pursuing, whether for Freudian or non-Freudian reasons, we do not need to believe in the existence of such Freudian entities as the id, the ego and the superego. In holding that institutions misrepresent goals they are pursuing, we do not need to imply, in any illegitimate sense, that there exist entities such as the institutional 'mind', 'unconscious' or 'id'. Even if we always sought with absolute intellectual integrity to represent accurately, to ourselves and others, the aims we pursue, it is to be expected that we will fail on occasions to represent goals we are actually pursuing correctly. We can make mistakes about what it is our actions are directed towards bringing about, just as we can make mistakes about any other factual matter about some aspect of the world. It behoves us, as rationalists, to suspect we have misrepresented our aims just as it behoves us to suspect that we have misrepresented other factual matters about the world: we need to adopt this sceptical attitude in order to make it possible for us to improve our representations of these factual matters.

Granted that we seek to live rationally, and develop rational institutions, it is especially important that we recognize that misrepresentations of goals are bound to happen, because of the tendency of misrepresentations of goals to sabotage reason – transforming it from something useful into something counterproductive. This will happen whenever aims are misrepresented in the characteristic manner of *rationalistic neurosis* as illustrated in figures 6a and 6b.

Rationalistic neurosis is damaging in a number of ways. The more 'rationally' the declared aim C is pursued – that is, the more thoroughgoing the corresponding methodology M_C is put into practice – the worse off the person or enterprise is. from the standpoint of realizing the genuinely desirable aim A. Even though B rather than C is pursued, B must nevertheless be pursued somewhat furtively and ineffectually in order to maintain the fiction that C and not B is being sought.

Failure to acknowledge the actual aim B means that problems associated with B cannot be recognized, as a first step to their resolution, and the pursuit of A. Instead of recognizing and seeking to solve those problems that need to be solved if A is to be pursued rationally and realized, the person or enterprise, as a result misrepresenting the aim to be C, recognizes and tries to solve a number of what may be called *neurotic problems* – problems which, if solved, only make matters worse. There are all the neurotic problems

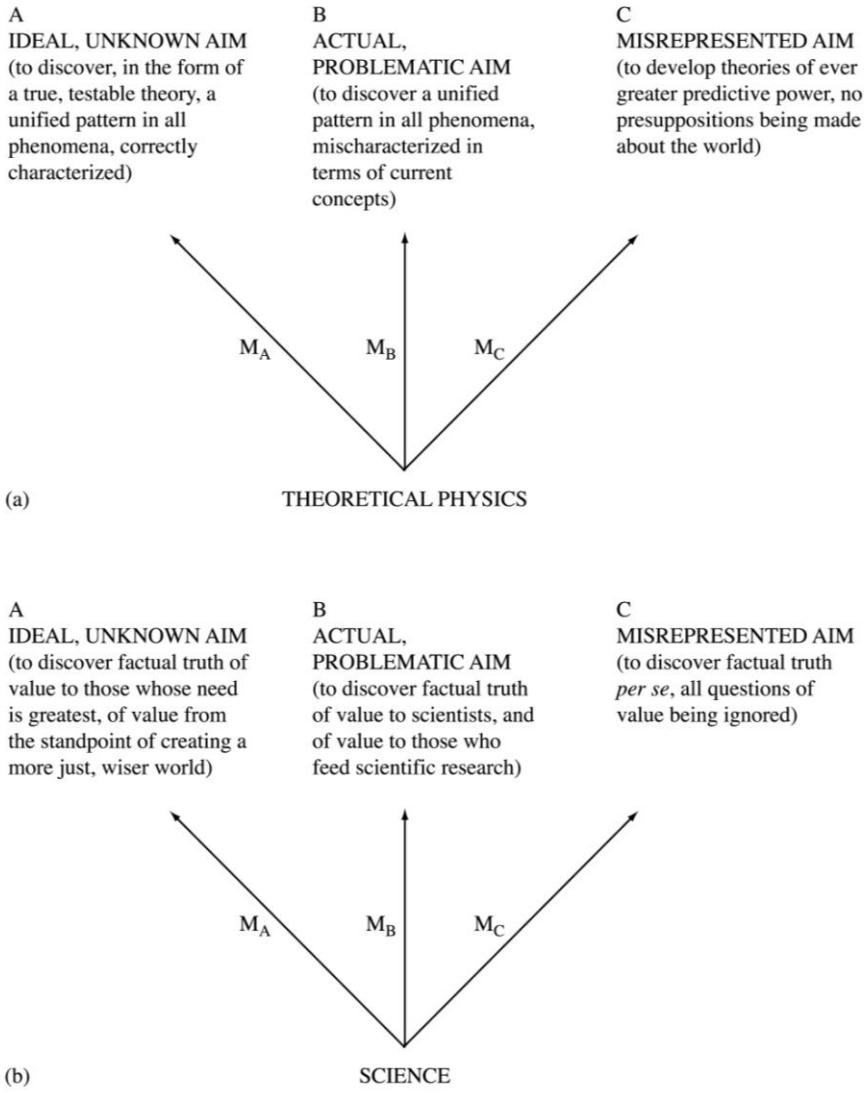

Figure 6 (a) Rationalistic neurosis of theoretical physics
Figure 6 (b) Rationalistic neurosis of science

associated with pursuing and realizing the declared goal C, defining and putting into practice the methodology M_C best designed to help realize C. There are the neurotic problems of explaining and understanding how real

success – steps towards B or A – can be steps towards C, taken in accordance with M_C. In all these ways, reason is counterproductive. The more rationally the declared aim C is pursued, the more nearly problems associated with realizing the declared aim C are solved, so the worse off the person or enterprise is, from the standpoint of realizing what is really of value, namely A. As long as the rationalistic neurosis persists, the vital activity of articulating and attempting to improve aims and methods – the essence of reason – becomes counterproductive, or at best sterile.

This counter-productivity of reason is likely to have a further damaging consequence: the experience of reason being useless or even harmful may lead the person or enterprise suffering from rationalistic neurosis to hold that reason deserves to be ignored: and as a result it becomes very much less likely that the rationalistic neurosis will be detected and overthrown – since this does require reason, authentic reason. As a result, rationalistic neurosis, once established, is very likely to persist, just because it has the peculiar capacity to discredit the very tools that are needed to overcome it.

It deserves to be noted that even pure mathematics, traditionally held to exemplify reason, intellectual rigour, at its finest, actually suffers from severe, damaging rationalistic neurosis. As a result of being pursued within the context of the philosophy of knowledge, pure mathematics is traditionally interpreted to have the aim of increasing knowledge of mathematical truth. At once problems arise as to what mathematics can be knowledge *about*. Empiricism fails to explain how mathematical results can be *proved*. Platonism fails in this respect even more lamentably: since no one has ever observed Platonic mathematical entities, all knowledge of such entities ought – so one would suppose – to be irredeemably speculative, and not capable of being proved at all. Logicism and formalism fail even more dismally. Logicism holds mathematics to be merely ever more intricate elaborations of logical truisms: formalism holds mathematics to be devoid of meaning altogether. If either were true, mathematics would be the intellectually disreputable enterprise of discovering ever more intricate ways of asserting *nothing*.

These, and other related, traditional problems in the philosophy of mathematics are all *neurotic* problems arising from a misrepresentation of the aim of mathematics. Viewed from the perspective of the philosophy of wisdom, the idea that pure mathematics has the aim of acquiring knowledge of anything actual can be dismissed out of hand. Rather, pure mathematics has the aim of contributing to wisdom by developing, systematizing and unifying problem-solving methods applicable to as wide a range of important actual problems as possible. Mathematics explores significant, problematic possibilities, not anything actual at all. Mathematics exists because apparently

very different real-life problems are solved by means of common or analogous methods: mathematics develops and unifies such common, widely applicable methods.

As a result of being pursued explicitly in accordance with the philosophy of wisdom rather than knowledge, mathematics would be transformed. Teaching, research, comprehensibility and availability of mathematics would be improved. Mathematics would be able to contribute far more effectively, more rationally, to the task of applying reason to the realization of what is of value in life.

The above theory of aim-oriented rationality and rationalistic neurosis provides us with a radical reinterpretation of psychoanalytic thought. Instead of regarding Freud, Adler, Jung and the diverse post-Freudian contributors to psychoanalytic thought as potential scientists, seeking to contribute to psychology, to our knowledge and understanding of the human psyche, we may instead regard them as *methodologists,* making contributions to the theory of aim-oriented rationality. A major claim of this book is that the social and humanistic disciplines quite generally – economics, political philosophy, the study of industrial relations, philosophy/sociology of education, of art, and of inquiry – need to be pursued and understood not as sciences at all, but rather as methodologies of our diverse social endeavours, helping us pursue these endeavours more rationally and successfully. Interpreting psychoanalytic theory along these lines is then just a special case of the general thesis.

Psychoanalytic theory interpreted in this aim-oriented rationalistic way has several advantages over psychoanalytic theory interpreted as an empirical theory about human nature, an intended contribution to knowledge. It vastly enhances the scope of psychoanalytic ideas, in that such key notions as repression, rationalization and neurosis become applicable to any sufficiently sophisticated aim pursuing entities – in particular, as we have seen, to institutions – and not just to people. It vastly increases the acceptability, the epistemological status, of key psychoanalytic ideas. Instead of the general theory of neurosis being an empirical theory about human nature of dubious scientific standing, it becomes essential to rationality to suspect aim-pursuing endeavours of having succumbed to neurosis, this having happened even to science itself!

The proposed reinterpretation frees psychoanalytic theory quite naturally from some dubious claims of Freud – in particular his theory of the id, the ego and the superego, his theory of the death wish, and his view of the paramount importance of the sexual drive in human life (this probably having more to do with general hypocrisy about sexual matters in Freud's

time rather than anything as basic as our biological nature, derived from evolution and our animal past). The proposed reinterpretation makes it much easier to see how psychoanalytic theory could quite naturally become a part of biology, Darwin's theory of evolution, and psychoneurology, all major hopes of Freud. It is reasonable to hold that a major factor in human evolution is the evolution of self-consciousness. A major part of self-consciousness may reasonably be held to be the representation to oneself of the pattern of goals one pursues in life – animals, by contrast, at most only representing to themselves goals actually being pursued. Our discussion above makes it quite clear that as such sophisticated representation of goals develops, so misrepresentation of goals is almost bound to develop as well. Neurosis may thus be maintained to be an almost inevitable teething problem of the early growth of self-consciousness – a point not without implications for psycho-neurology. In order to make intelligent guesses as to how self-awareness and self-misawareness may have developed, within a framework of ethology and Darwinian theory, it becomes essential to attend not only to anthropological studies of hunting and gathering tribes, but also to such studies as those of Jane Goodall (1971) and her followers into the way of life of the chimpanzee – which may be taken to be close to pre-self-conscious human life. Such an approach to understanding human nature – via its biological and historical evolution – is bound to lead to a view of ourselves and our problems different from that of Freud. Observation of chimpanzees for example, may lead us to suspect that male competitiveness is as important a (misrepresented) basic drive in human affairs as sex. The aim-oriented rationalistic interpretation of psychoanalytic theory also has implications for psychotherapy: it implies that the basic task of therapy is to develop better strategies for living rather than to uncover the underlying 'causes' of the neurotic problems. Indeed, one consequence of putting the philosophy of wisdom into practice is so to change both therapy and education that the distinction between the two all but disappears.

Our discussion has revealed a situation rich in irony. Psychoanalytic theory has been criticized for failing to attain the high intellectual standards of science (Popper, 1963, ch. 1; Cioffi, 1970, pp. 471-99; Grünbaum, 1984). It turns out that the thing is all the other way round. It is science that fails to attain the high intellectual standards of post-Freudian rationality, in that science suffers from rationalistic neurosis. On the other hand, it must be admitted that proponents of psychoanalytic theory have themselves misrepresented and misunderstood quite radically the goals and nature of their own discipline, in that they have construed it to be a branch of knowledge rather than a branch of aim-oriented rationality. The experts of

misrepresentation have succeeded in seriously and damagingly misrepresenting the goals of their own discipline. Psychoanalytic theory itself suffers from rationalistic neurosis! (For a more detailed development of this argument concerning the rationalistic neurosis of science and the methodological reinterpretation of psychoanalytic theory see Maxwell, 2004b.)

This aim-oriented rationalistic version of the philosophy of wisdom can be regarded as being derived from many sources: Lao Tzu, Socrates, Jesus of Nazareth, Einstein, Freud, Darwin, J.S. Mill, Kropotkin, Dewey, Fromm, Popper, amongst others. Some may regard Marx as an important source, especially in the light of that aspect of Marx's thought summed up in his eleventh thesis on Feuerbach: 'The philosophers have only *interpreted* the world, in various ways: the point, however, is to *change* it.' The two most important general sources are however: the Rationalist Enlightenment movement associated with such figures as Bayle, Voltaire, Diderot, Condorcet, Hume, Kant; and the Romantic movement, associated with such figures as Vico, Rousseau, Goethe, Beethoven, Blake, Wordsworth, Keats, van Gogh, William Morris, Tolstoy, and many contemporary and later writers, artists, musicians. An important achievement of the aim-oriented rationalist version of the philosophy of wisdom is that it provides us with a synthesis, a unification of these two great, but conflicting, humanitarian movements. The Rationalism of the Enlightenment upheld versions of the philosophy of knowledge. It thus upheld associated intellectual ideals such as anti-authoritarianism; scepticism; belief in the value of reason; objectivity; method; logic and evidence; impersonal observation and experimentation; science and scholarship; the pursuit of impersonal, progressive factual knowledge. Rationalism tended to be suspicious of imagination, subjectivity, spontaneity, instinct and inspiration, personal experience, personal feelings and desires, passion. The Romantic movement rebelled against what was taken to be this Rationalist disparagement of much of value in human life. Romanticism thus sought to uphold and celebrate the value of that which Rationalism undervalued: imagination, inner experience, personal feelings and desires, spontaneity, instinct and inspiration, self-expression in art, in literature, in music, in love – and in all of life. It sought to celebrate the basic value of life as it is actually experienced and lived. Unfortunately, Romanticism made the mistake of assuming that Rationalism did indeed stand for what it claimed to stand for – namely genuine reason. As a result, in celebrating the central value of personal experience and life, Romanticism took itself to be celebrating the non-rational or even the irrational: it even on occasion *advocated* irrationalism. A disastrous split developed between the Rationalist

movement – associated with science, technology, scholarship, the universities and much education, and the Romantic movement – associated with much literature, drama, art, music, and some education, psychotherapy, politics and religion, and manifesting itself, in this and the last centuries, in the form of existentialism, postmodernism, anti-science views and New Age movements. Both Rationalism and Romanticism have suffered as a result – both being more or less irrational and undesirable. What ought to have been realized long ago is that the Rationalist espousal of the philosophy of knowledge is actually irrational precisely because it excludes Romantic intellectual ideals of motivational and emotional honesty, truth to personal experience, imagination employed in the exploration of possibilities of value. The philosophy of wisdom is intellectually more rigorous than the philosophy of knowledge precisely because it incorporates such vital Romantic intellectual values. Aim-oriented rationalism heals the traditional split between Rationalism and Romanticism – the split between Snow's two cultures. It puts the two together, very much improving each as a result, the two uniting to form a coherent intellectual-cultural movement (Rational Romanticism or Romantic Rationalism), capable of devoting itself far more effectively to the cooperative realization of value in life.

If any one person in history deserves to be credited with discovering, practising and advocating the philosophy of wisdom, that person is Socrates. The following *eight* points about Socrates' thought and life constitute, in particular, especially striking grounds for making this claim. (1) Socrates' basic problem was this. What is the good life? How ought we to live? What is genuinely of value in life and how is it to be achieved? (2) For Socrates, this basic problem of living was more fundamental and important that the problems of cosmology or natural philosophy – the standard problems of the Presocratic philosophers. (3) Basic to Socrates' thought and life is his claim that we are all more or less ignorant as to how to live well, he, Socrates, in particular, sharing in this common ignorance. As Socrates tells us in Plato's *Apology* – generally presumed to be reasonably accurate historically – Chaerephon, an impetuous friend of his, had the audacity to ask the oracle at Delphi whether anyone was wiser than Socrates. The oracle replied that no man was wiser. After pondering this judgement for some time, and after questioning others who claimed to possess wisdom, in an attempt to refute the oracle, Socrates came to the conclusion that what the oracle meant was that he, Socrates, was wiser than others in that he at least knew full well that he lacked wisdom. Socrates decided to become, as he put it, a gadfly, stinging his fellow Athenians into recognizing the inadequacy of their claims to wisdom, so that they might at least possess the wisdom of acknowledged

ignorance. In other words, Socrates' central discovery and insight is just the fundamental presupposition of the philosophy of wisdom. (4) Socrates sought to devote reason, critical discussion, to the task of attempting to discover what is of value in life and how it is to be realized. (5) In particular, Socrates can be interpreted as putting into practice a conception of reason close to the aim-oriented rationalism advocated here, in that his basic endeavour was to subject to critical scrutiny diverse views as to what our aims in life ought to be, diverse views about such ideals as justice, goodness, courage, friendship, happiness, love. This is not, of course, the standard interpretation of Socrates' methodology. From Aristotle down to the present day, Socrates has been interpreted as holding the absurd view that in order to be virtuous it suffices to have *knowledge* of virtue, this to be acquired by arriving at correct definitions of key moral terms. The need to accommodate Socrates within the framework of the philosophy of knowledge has in this way grotesquely distorted what he actually sought to achieve: to get into personal and social life the habit of looking critically at actual and possible life aims and ideals in an attempt to improve them by rational means. Nothing could indicate more strikingly our tragic failure, even today, to have understood and taken seriously even the most elementary points Socrates sought to communicate. (6) A basic feature of Socrates' life and work, as revealed in Xenophon's Socratic writings and in the early, more historically accurate dialogues of Plato, is just the fundamentally practical, social and moral character of Socrates' concerns. Socrates did not seek primarily to make an intellectual contribution to thought (like Aristotle, and most modern academics); rather he sought primarily to make a practical, social and moral contribution to Athenian life. He sought to bring about a social and moral revolution – one which led to Socratic doubt and inquiry becoming a standard part of Athenian life. He sought to promote wisdom in life by rational means – and not mere intellectual wisdom or knowledge. (7) One kind of wisdom was recognized by Socrates to exist, namely the wisdom, the skill, of craftsmen. It is hardly too much to say that Socrates' central concern was to discover how skills employed and learned by craftsmen in creating things of value could be generalized to become skills that enable us to realize what is of most value in life. There is here a further indication of the practical character of Socrates' concerns, and an indication even that Socrates took action and the problems of action to be more fundamental than knowledge and the problems of knowledge. (8) In opposition to the Sophists, Socrates firmly rejected any mere subjectivist or relativist conception of value. What is of value is to be discovered, it is not simply what we decide. It is this

rejection of subjectivism and relativism that makes Socratic ignorance possible.[6]

Socrates might be interpreted to be arguing along the following lines: There is something here, implicit in our lives in Athens, that is of immense desirability and value, of profound grandeur, significance and beauty. This is to be seen in the world around us, but above all in ourselves, in our souls and in our civilization – in our crafts, our art, sculpture, poetry, drama, philosophy, in our freedom, democracy and justice. There are, however, in our souls and civilization devastating flaws – war, tyranny, injustice, violence, almost psychopathic ambition, deception, vanity and self-deception, self-annihilation of the soul. Our task is to discover how to help let that which we glimpse – of such supreme value, grandeur, significance and beauty – to grow, to come progressively into existence throughout our shared life, our *polis.* Fundamentally what we need to do is to improve appropriately our aims and methods in life – our actions, our lives, the movements of our souls. It is towards the accomplishment of this task that we need to devote our *thinking,* our rational inquiry and our education.'

It is in this way that we need to interpret Socrates' own account of his life-work:

So long as I breathe and have the strength to do it, I will not cease philosophizing, exhorting you, indicting whichever of you I happen to meet, telling him in my customary way: Esteemed friend, citizen of Athens, the greatest city in the world, so outstanding in both intelligence and power, aren't you ashamed to care so much to make all the money you can, and to advance your reputation and prestige – while for truth and wisdom and the improvement of your soul you have no care or worry?

[6] Since writing this sometime before 1984, I have modified my views about the historical Socrates somewhat as a result of reading I. F. Stone's fascinating book about Socrates (see Stone, 1989).

Chapter Six
Present Domination of the Philosophy of Knowledge in the Academic World

My claim is that it is the philosophy of knowledge – and not the philosophy of wisdom – that is the generally adopted, official view as to what ought to be the aims and methods of academic inquiry in universities throughout the world. The philosophy of knowledge, I claim, powerfully influences almost every aspect of the academic enterprise, and especially the best work being done within academia. It influences the aims and methods of the formal, natural and social sciences; the way different disciplines are interrelated; the way in which decisions are reached about research priorities and the funding of research; intellectual values and priorities; style and content of contributions to academic journals, monographs, textbooks, lectures and seminars; criteria adopted by editors and referees in deciding what is to be accepted and rejected for publication; academic success and failure; academic appointments and promotions; decisions concerning the awarding of academic honours, and the composition of academic elites and power groups; style and content of university degree courses; the whole way in which the academic enterprise is related to the rest of society – to industry, politics, international affairs, religion, education and so on. An academic world which upheld and sought to put into practice the philosophy of wisdom would differ profoundly, in a multitude of ways, intellectual and institutional, from what we have at present, inquiry pursued more or less in accordance with the philosophy of knowledge.

By no means all scientists and scholars accept the philosophy of knowledge in its entirety, as outlined in chapter 2. Increasingly during the last few decades aspects of the doctrine have been subjected to criticism, as we saw in chapter 2. In some fields, where anti-rationalist or postmodernist thought prevails, such as in culture studies, and some branches of social inquiry, philosophy, and history of science, rather different ideals for inquiry prevail. Furthermore, recent work of a number of people in diverse fields may well be held to be attempts to put something like the philosophy of wisdom into practice. As I shall argue in chapters 11 and 12, the intellectual/institutional revolution that I advocate in this book – from knowledge to wisdom – is already to some extent under way. Despite this, at the time of writing it is still overwhelmingly the philosophy of knowledge – and not anything like the philosophy of wisdom – which predominates over most aspects of the academic enterprise. The philosophy of knowledge is still,

over two decades after this book was first published, the only well-known ideal of rational inquiry.

How, it may be asked, does the philosophy of knowledge exercise its potent influence over so many aspects of the academic enterprise? The essential point is this. The philosophy of knowledge – like any philosophy of inquiry – specifies what is to count as a contribution to inquiry, what is to be meant by intellectual progress, and in particular what is to be judged to be intellectually important. This is a matter that potently affects and concerns everyone associated with the academic enterprise, directly or indirectly. Scientists and scholars desire passionately to contribute to inquiry, to have their contributions published, accepted and valued. This passionate desire may spring from the noblest of motives: to contribute to human understanding, to help lessen human suffering or otherwise enhance the quality of human life. Or it may spring from less noble – though by no means necessarily less passionate – motives: to achieve a kind of immortality by making a lasting contribution to thought; to become famous, establish a reputation, become honoured by colleagues; to further a career or simply earn a living. In order to realize any of these ambitions, in whatever proportion of the noble and less noble, scientists and scholars are obliged to present their contributions in a form that renders them understandable and acceptable, in a form that complies with the current philosophy of inquiry (at present the philosophy of knowledge). They must do this even if they do not agree personally with the current philosophy of inquiry. Likewise editors of journals, referees and academic publishers must ensure that work accepted for publication is good, or at least acceptable, with respect to the current philosophy of inquiry. Reputations, careers, appointments, tenure, scientific and scholarly honours, entrance to scientific or academic elites, all depend on the production and publication of work which conforms to the currently adopted philosophy of inquiry. The public face of inquiry thus tends to conform to the officially accepted philosophy even if privately many individuals may have their reservations. Students, introduced to this public face of inquiry in their education, will if anything come to believe in the current philosophy of inquiry even more strongly than their predecessors (rarely if ever encountering work which fails to conform to the official philosophy, and not realizing the extent to which this conformity is the result of pressures to publish and win recognition). The lesson will be all the more powerful for being implicit. Science students do not encounter critical discussion of the aims and methods of science as a normal part of their scientific education, precisely because standard empiricism excludes such discussion from the intellectual domain of science. The philosophy of

knowledge is not expounded and taught at all: it is simply presupposed by everything that is taught. Students thus come to accept the philosophy of knowledge as a result of a process that is closer to unconscious indoctrination than to education.

By means of these mechanisms, the philosophy of knowledge comes to be accepted firmly and unthinkingly by most scientists and scholars. It is passionately and tenaciously upheld just because it determines something of great importance to scientists and scholars – what is to count as intellectual progress, what is to count as an important contribution to science and scholarship.

Let us now examine some aspects of academic work to see to what extent the philosophy of knowledge is indeed the dominant view in practice. In what follows I consider six different aspects of academic inquiry. In each case I leave unchanged my findings and comments made in 1983 for the first edition of the book, and then add comments about the state of affairs in 2003, or thereabouts, for the present edition of this book. I am attempting to provide two snapshots of academia, two decades apart, in the hope that this will provide some indication of whether or not there has been some movement, during this period, away from the philosophy of knowledge and towards the philosophy of wisdom.

1 Literature on the Nature and Purpose of Universities and Higher Education

1983

Books on the nature and purpose of universities and higher education agree almost universally that the aim of universities both is and ought to be to create and promote knowledge. Typical quotations are the following. 'A university is a corporation or society which devotes itself to a search after knowledge for the sake of its intrinsic value' (Truscott, *Red Brick University,* 1943, p. 45). 'We think then of a university as a community of men and women engaged in a common task, namely the pursuit of knowledge' (Seeley, *The Function of the University,* 1948, p.6). 'The university is a community of scholars and students engaged in the task of seeking truth' (Jaspers, *The Idea of the University,* 1960, p. 19). 'One common theme in research activities is that all relate to the accumulation of knowledge, whether scientific or unscientific, theoretical or practical' (Corwin and Nagi, *The Social Contexts of Research,* 1972 p. 2). 'All . . . [universities] set themselves to advance learning and knowledge

by teaching and research for the benefit both of their students and of the community, and in general to give students the benefits of a university education. Some, notably the new technological universities, also pledge themselves to pay attention to the application of knowledge for the benefit of industry and commerce' (Venables, 'The study of higher education in Britain', 1972, p. 29). '. . . it will be commonly admitted that nowadays our expectations of universities are at least twofold: they must provide training and they must foster the preservation and advancement of knowledge' (Robbins, *Higher Education Revisited,* 1980, p. 6). Some authors even defend that extreme version of the philosophy of knowledge, according to which universities should pursue knowledge exclusively for its own sake, and not in order to help solve social problems. See, for example, Nisbet, *The Degradation of the Academic Dogma: The University in America 1945-70* (1971).

In amongst this chorus of agreement one does very occasionally come across the odd discordant voice. Thus Roszak, introducing *The Dissenting Academy* (1969), remarks of the contributors to the volume that they are convinced '. . . that the proper and central business of the academy is the public examination of man's life with respect to its moral quality. It is, from first to last, the spirit of Socrates that broods over the 'dissenting academy' this volume comprises' (p. 9). There is here, however, no disagreement with what is being maintained in this book. Quite to the contrary: what Roszak and his fellow dissenters argue is that most academics betray this Socratic ideal in their pursuit of academic careers and success based on the procurement and dissemination of specialized knowledge.

2003

An enormous amount has been published on universities since 1983. I have not found a single book, however, apart from my own, advocating anything like the philosophy of wisdom, or holding that universities actually put such a philosophy into academic practice. The standard, unthinking presumption is that universities, quite properly, seek to acquire knowledge, and impart knowledge to their students. Thus Gibbons *et al* (1994) argue that a change is taking place in the way knowledge is produced; it is becoming less hierarchical, more 'socially accountable and reflexive' and includes 'a wider, more temporary and heterogeneous set of practitioners, collaborating on a problem defined in a specific and localized context' (p. 3). But even though changes are taking place in the way knowledge is being produced, according to Gibbons *et al*, nevertheless what is being produced is still knowledge. Becher (1989) sets out to examine the diverse methods and approaches of

diverse academic disciplines from the natural sciences to the humanities but, again, takes it for granted that throughout the basic aim is the acquisition of knowledge.

Some authors are fiercely critical of aspects of modern academic life. Anderson (1992) is scathing about what he sees as the utter trivia of so much specialized academic work, the result, according to Anderson, of academics being protected from the rigours of the free market by tenure and peer review. 'The primary calling of professors' declares Anderson, 'is to teach – the sacred responsibility to impart information and ideas to the young' (p. 14). Anderson takes the philosophy of knowledge for granted. Other writers on higher education express a growing cynicism about the character of university life, apparent even in their titles. Examples of such apparent cynicism are: Readings (1996), *The University in Ruins*; Barnett and Griffin (1997), *The End of Knowledge in Higher Education*: Barnett (2003), *Beyond All Reason: Living with Ideology in the University*.

In chapter 12 we will see that some workers, more optimistically, hold that there is a place for 'teaching for wisdom' within universities.

2 Philosophy and Sociology of Inquiry

1983

In order to discover in a little more detail what philosophy of inquiry at present prevails in universities, the next obvious place to look is at current philosophy/sociology of inquiry. If academic inquiry puts anything like the philosophy of wisdom into practice, the philosophy/sociology of inquiry would be, straightforwardly enough, the imaginative and critical discussion of actual and possible aims and methods of inquiry, carried on as an integral part of inquiry itself, the basic presupposition being that the fundamental intellectual aim of inquiry is to help us enhance our capacity to realize what is of value in life, help us to devote reason to developing wiser ways of life, a wiser world. There would be no dissociation between the intellectual and social aims and aspects of inquiry: intellectual aims and problems are subordinate to our fundamental aims and problems of living.

The philosophy/sociology of inquiry as it exists at present differs from all this in just the ways one would expect granted that academic inquiry proceeds, and is held to proceed, in accordance with the philosophy of knowledge. Current philosophy/sociology of inquiry is made up of (1) epistemology (2) the philosophy of science (3) the history of science (4) the sociology of knowledge (5) the sociology of science (6) the study of science

policy. All these sub-disciplines accept, without question, that the basic intellectual aim of inquiry is to improve knowledge and understanding (thus, it is to be hoped, enriching the quality of life). There is not even the faintest whisper of the idea that we might need a different kind of inquiry, a more rational kind of inquiry, devoted to enhancing wisdom. Even the most radical critics of the scientific and academic *status quo* – Feyerabend (1978), Easlea (1973), Rose and Rose (1976), Roszak (1970), Ravetz (1971) and others – all fail to argue that what we need is a more intellectually rigorous kind of inquiry devoted to enhancing wisdom. Nothing remotely approaching this possibility receives any mention or consideration whatsoever by Passmore in his book *Science and its Critics* (1978).

One does find, it is true, some criticism of some aspects of the philosophy of knowledge. Kuhn, for example, has expressed some dissatisfaction with 'the very influential contemporary distinction between 'the context of discovery' and 'the context of justification' ' even though he does suppose that '. . . appropriately recast . . . [it does] have something important to tell us' (1962, p. 9). Despite this, the picture of science that Kuhn paints for us is very obviously a version of the philosophy of knowledge. Indeed, in some ways it is a highly reactionary version of the philosophy of knowledge in that, for Kuhn, criticism of fundamental assumptions has no rational role to play in normal science, a discipline only becoming authentic mature science when philosophical discussion of fundamentals is abandoned. In this way, Kuhn provides a rationale for scientists to pursue specialized puzzle-solving dissociated from all concern for philosophical and social problems of living. A basic tenet of the philosophy of knowledge is that the intellectual domain of inquiry must be decisively split off from, and shielded from being influenced by, broader social and cultural factors. For Kuhn, this is an essential feature of mature sciences. Thus he emphasizes '. . . the unparalleled insulation of mature scientific communities from the demands of the laity and of everyday life' and elsewhere asserts '. . . compared with other professional and creative pursuits, the practitioners of a mature science are effectively insulated from the cultural milieu in which they live their extraprofessional lives' (Kuhn, 1977, p. 119). He argues that '. . . the insulation of the scientific community from society permits the individual scientist to concentrate his attention upon problems that he has good reason to believe he will be able to solve' (1962, p. 164). Kuhn even comes close to endorsing the view that science should give priority to the pursuit of specialized knowledge for its own sake in passages such as the following. 'A part of normal theoretical work, though only a small part, consists simply in the use of existing theory to predict factual information of intrinsic value.

The manufacture of astronomical ephemerides, the computation of lens characteristics, and the production of radio propagation curves are examples of problems of this sort. Scientists, however, generally regard them as hack work to be relegated to engineers or technicians. At no time do very many of them appear in significant scientific journals'. (1962, p. 30).

In recent years, some philosophers of science have explored the possibility that there may be a rational, if fallible, method of discovery in science (Nickles, 1980). This ought, but does not seem, to involve the rejection of standard empiricism since, as I shall argue in chapter 9, such a rational method of discovery is only possible if aim-oriented empiricism is accepted and standard empiricism is rejected.

Some other philosophers of science have called into question the value-neutrality of some aspects of science (Rudner, 1953, pp. 186; Rescher, 1965, pp. 261-76; Gaa *et al,* 1977, pp. 511-618); some historians of science have argued that political and ideological issues run throughout science (Graham. 1981); some others have argued for the need to develop a kind of science and scholarship devoted to socialist objectives, to helping us develop a freer, more just and more beautiful world, or to examining critically the moral life of man (Rose and Rose, 1976; Easlea, 1973). Overall, however, current philosophy/sociology of inquiry presupposes the philosophy of knowledge. Many philosophers, historians and sociologists of science may wish to reject some minor points of detail. Very few might wish to reject major aspects of the view. No one seems to advocate putting anything like the philosophy of wisdom into practice.

Failure to adopt anything like the philosophy of wisdom and aim-oriented rationalism is also strikingly apparent in the way different branches of the philosophy/sociology of science are related to science itself, and to each other.

Aim-oriented rationality, and the philosophy of wisdom, require that the philosophy/sociology of science – that is, sustained imaginative and critical discussion of actual and possible aims and methods of science – be an integral, influential part of science itself. Standard empiricism and the philosophy of knowledge require, to the contrary, that the philosophy/sociology of science, in this sense, be excluded from the intellectual domain of science, just because discussion of this kind cannot amount to contributions to knowledge, let alone contributions to empirically testable knowledge. At present, academic philosophy and sociology of science are indeed excluded from the intellectual domain of natural science, precisely in accordance with what is required by the philosophy of knowledge, and grotesquely at odds with what is required by the philosophy of wisdom.

By and large, natural science just ignores academic philosophy and sociology of science. Failure to put anything like the philosophy of wisdom into practice is also strikingly indicated by the split that exists within present-day academic philosophy/sociology of science, between the philosophy of science on the one hand, and the sociology of science on the other hand. This split mirrors the split demanded by the philosophy of knowledge between intellectual and social aspects of science. Academic philosophy of science concerns itself almost exclusively with the intellectual aspect of science. Science is presumed to have, as its basic intellectual aim, to improve knowledge of factual truth *per se;* the philosophy of science restricts itself almost entirely to considering problems that this presupposition gives rise to, such as the problem of how knowledge is possible, and the problem of what methods ought to be adopted granted we seek to acquire such knowledge. The sociology of science – a branch of sociology, and thus of social science – seeks to develop factual, scientific, sociological knowledge about science conceived of as a sociological phenomenon, an aspect of society. Both these subordinate disciplines presuppose the philosophy of knowledge. Despite this, it is almost as if they inhabit different worlds of thought between which there is scarcely any communication. Insofar as any communication does take place, it is more or less confined to ineffective, intellectual sniping. Sociologists of science can dismiss the philosophy of science for not being a part of science itself, in that it is concerned with normative questions about how science ought to proceed, and not with factual questions about how science does proceed. Philosophers of science, on the other hand, can point out dismissively that sociologists of science must presuppose some sort of philosophy of science in order to identify science itself, and in order to pursue their own discipline scientifically. This extraordinary mutual dismissiveness and lack of communication is a direct consequence of the fact that the two disciplines concern themselves with aspects of science – the intellectual and the social – between which there *ought* to be a decisive split, according to the philosophy of knowledge. The inability of philosophers and sociologists of science to speak to each other is a further striking illustration of how subtly and profoundly influential the philosophy of knowledge is on present-day academic thought.

From the standpoint of the philosophy of wisdom, this split between the philosophy and sociology of inquiry – entirely understandable granted the philosophy of knowledge – is both absurd and disastrous. According to the philosophy of wisdom, the basic task of sociology, quite generally, is to help us improve institutional aims and methods by promoting imaginative and critical discussion of actual and possible aims and methods as an integral part

of the life of institutions. The basic task of the sociology of science in particular, then, is to help improve the aims and methods of science by promoting imaginative and critical discussion of actual and possible aims and methods as an integral part of science itself. This *is* the philosophy of science. According to the philosophy of wisdom, the philosophy and the sociology of science are one and the same thing: there is no distinction between them. If science is to serve humanity rationally and well, it is vital that it has associated with it imaginative and critical discussion of human or social aims for science – intellectual aims and problems being pursued and understood as subordinate to more fundamental aims and problems of people in life. This means in turn that the philosophy/sociology of science (a) is both a part of public discussion and scientific discussion (b) combines discussion of social and intellectual issues. All this is sabotaged by the current splitting up of science, philosophy and sociology of science. The sociology of science concerns itself with science as part of the human world, but cannot look critically at science from this perspective because of its concern to be factual and scientific. Thus Barnes remarks: 'It should be emphasized that the discussion is centred upon the sociologist's concern to understand and explain beliefs about nature and their variation. It does not seek to advocate or to criticize the beliefs discussed, nor is it concerned with their justification', (1974, pp. vii-ix). The philosophy of science on the other hand does leave open the possibility of criticism, since it is concerned with questions about what ought to be the aims and methods of science. Despite this, contemporary philosophy of science is quite unable to look critically at what modern science contributes to human life, just because it restricts itself to concern with the intellectual aspect of science, and seeks to portray science as a rational enterprise within the framework of the philosophy of knowledge. The net result is a general failure to discuss imaginatively and critically urgent problems concerning the capacity of modern science to help us realize what is of value in life. Such discussion cannot be an orthodox part of academic inquiry as it is at present constituted, because it does not amount to contributions to knowledge.

2003

It is hard to discern any movement towards the philosophy of wisdom – or 'wisdom-inquiry' – in those academic disciplines devoted to the study of science, during the two decades 1983-2003. To begin with, apart from reviews (see chapter 13), there has been scarcely any discussion whatsoever

of the thesis and argument of this book.[1] There has certainly been no active campaign to transform academia so that it comes to put the philosophy of wisdom into practice. Historians, philosophers and sociologists of science have not come together to consider afresh the fundamental question: 'What kind of science, and academic inquiry more generally, can best help humanity create a better world?'. If that had happened, some awareness of the urgent need to develop a more rigorous and humanly valuable kind of inquiry might have dawned. Instead, historians, philosophers and sociologists of science have continued to be disdainful of each other's work, as I indicated in the Introduction to this edition of this book. Most sociologists of science are convinced that science has to be viewed purely as a social phenomenon, there being change of scientific belief but no such thing as scientific progress, knowledge and rationality (see Barnes and Bloor, 1981; Bloor, 1991; Barnes, Bloor and Henry, 1996; Shapin and Schaffer, 1985; Shapin, 1994; Pickering, 1984; Latour, 1987). Many historians of science have accepted this view and, as a result, have abandoned the fundamental problem of their discipline – How has scientific progress occurred? – and instead have concentrated on social factors associated with science. Philosophers of science, instead of demolishing the absurd views of historians have, if anything, made concessions to these views. Despairing of making any headway with what was once perceived to be the central problem of the discipline, namely the problem of induction, philosophers of science have, during the period in question, presided over the fragmentation of the discipline into diverse specialized pieces: philosophies of physics, biology, computer science, geology, mathematics, astronomy, psychology, economics, sociology, and so on. As I indicated in chapter 2, postmodernists, feminists and others have attacked what they have perceived as the dangerous pretensions, the spurious objectivity, the menace of modern science, an attack which provoked a counter-attack by scientists, historians and philosophers of science seeking to defend science: see Gross and Levitt (1994), Gross, Levitt and Lewis (1996), Koertge (1998) and Segerstrale (2000). This debate received a great deal of publicity after the publication of Alan Sokal's brilliant spoof article 'Transgressing the boundaries' in a special issue of the cultural studies journal *Social Text* in 1996 entitled *Science Wars*: see Sokal and Bricmont (1998).

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[1] Two exceptions are Harré (1986) and O'Hear (1989): see chapter 13 for my critical assessment of what they have to say. Others, from other fields of study, have commented on and made use of the ideas of this book: see, for example, Midgley (1989); Dixon (1988); Burrows (1991); Elms, (1989); Harris (1979); Chisholm (1999); Deane-Drummond (2006).

Both sides of this 'science wars' debate missed the point. Those who attacked, and those who defended, scientific rationality attacked and defended, not scientific rationality, but a characteristic kind of *irrationality* masquerading as rationality. Those attacking science ought to have argued almost precisely the opposite: what we need is *more* scientific rationality and objectivity, not less. Instead of arguing, absurdly, that 'the natural world has a small or non-existent role in the construction of scientific knowledge' (Harry Collins) or that 'reality is the consequence rather than the cause' of the so-called 'social construction of facts' (Bruno Latour and Steve Woolgar)[2], it would have been better to argue that science, and academic inquiry more generally, need to be transformed so that they become rationally designed to help humanity make progress towards a civilized world. On the other hand, those defending science ought to have appreciated that what they were defending is defective when judged in both intellectual and human terms. Instead of defending versions of standard empiricism and the philosophy of knowledge – which is what Gross, Levitt, Koertge and company have done[3] – they ought rather to have criticized both for harmful lack of rigour, and argued for the need to adopt and implement the more rigorous and desirable conceptions of aim-oriented empiricism and the philosophy of wisdom. The 'science wars' debate was the wrong debate, conducted in the wrong place about the wrong issues – a modern version of the old debate between Rationalism and Romanticism. It served to distract attention away from what really did need to be debated: how to develop a science, and an academic inquiry, rationally devoted to helping humanity learn how to create a better world.

One development that has taken place during the period we are considering that might be considered to be a small step towards wisdom-inquiry is the growth of departments of science and technology studies (STS). STS have their roots in the sociology of science and science policy, but can include philosophy of science, history of science and studies concerned with science and the public as well. My department at University College London, once a history and philosophy of science department, is now a department of STS that includes science policy and science communication. The philosophy of wisdom takes philosophy of science, sociology of science and science policy to be the one integrated discipline of the philosophy of science – a consequence, of course, of taking the intellectual aim of science to be, not

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2 Both quoted in Sokal (1998, p. 11).

3 I must exempt Alan Sokal from this accusation: he accepts aim-oriented empiricism (personal communication) but not, I think, the philosophy of wisdom.

just truth, but *valuable* truth, knowledge contributing to what is of value in human life, and thus inherently *social* in character. This integrated discipline takes the basic aim of science to be to contribute to enhancing what is of value in life by means of scientific and technological discovery, and seeks to help improve scientific methods and institutions, priorities of research, choice of problems, relations between science and the public, science and the rest of academia, so that the basic aim may be all the better achieved. STS, in bringing sociology and philosophy of science and science policy together in one department might be regarded as a step towards this integrated wisdom-inquiry discipline. But this institutional step does not seem to be reflected in the intellectual situation. The philosophy of science is still at odds with the sociology of science, and is different from science policy. Philosophy of science is normative but narrowly intellectual, while the sociology of science, being a branch of sociology, sees science in factual, sociological terms.

Another development that might be construed to be a small step towards wisdom-inquiry is the birth of a new discipline: social epistemology. This sets out to study the social dimension of knowledge and science while seeking, at the same time, to do justice to the normative character, the rationality, of both. The discipline is usually said to have been founded by Alvin Goldman (1987, 1999) and Steve Fuller (1987, 1988), although many authors contributed to what may be deemed to be social epistemology at earlier times. A particularly incisive and somewhat neglected contribution was made by Karl Popper when he argued for the inherently social character of science (Popper, 1969, vol. II, pp. 216-223), and for the necessity of science to be pursued within an appropriate institutional structure (Popper, 1961, pp. 154-157). Popper also makes the related, more general point that 'Reason, like language, can be said to be a product of social life' (Popper, 1969, vol. II, p. 225).[4]

Social epistemology might be regarded as a step towards wisdom-inquiry in that both stress the social character of science, and of rational inquiry more generally. Both ought to follow Popper in appreciating that the social or institutional dimension of science, far from undermining the rationality of science, its objectivity, authenticity and capacity to make progress, is

[4] Many historians and sociologists of science deny that science provides us with authentic knowledge because, having seen the inherently social character of science, they think this precludes science from being rational. What they fail to appreciate is that Popper solved this problem decisively decades ago when he argued that the vital *critical* aspect of science depends on its *social* aspect; the social dimension of science, far from negating, is actually *necessary* for its rationality.

absolutely essential for these things. Unfortunately, social epistemology takes us no further: it fails to stress that the proper fundamental aim of rational inquiry is to seek and promote wisdom, a revolution in science and academia being required if this aim is to be properly pursued.[5]

3 Pronouncements of Scientists

1983

Do scientists themselves accept and advocate the philosophy of knowledge? The overwhelming majority of scientists do, I suggest, unquestioningly accept the philosophy of knowledge, in one or other of its versions. Despite this, full and careful formulations of the philosophy of knowledge by scientists are hard to come by, partly, I suggest, because most scientists assume the matter to be too obvious to need formulation, partly because the philosophy of knowledge puts the task of providing such a formulation outside science.

There is, however, one widely recognized spokesman for the scientific community in this respect. In contrast to the normal attitude of indifference merging into contempt that most scientists have towards the philosophy of science, the work of one philosopher of science, Karl Popper, is taken very seriously indeed by very many scientists (for example by such eminent and diverse scientists as Medawar, Bondi and Eccles). The extent of Popper's acceptance by the scientific establishment is strikingly indicated by the fact that Popper was a fellow of the Royal Society – a rare honour for a philosopher of science indeed. However, as I have already remarked in chapter 2, Popper's contributions to the philosophy of science amount to a powerful defence of one version of the philosophy of knowledge. It is very natural to interpret Popper's solution to his basic problem – the problem of demarcating science from non-science – as affirming a central tenet of the philosophy of knowledge, namely the need to restrict the intellectual domain of science to testable, factual propositions (and arguments concerning the acceptability of such propositions). Thus, the almost unprecedented way in which Popper's philosophy of science has been accepted and endorsed, almost as representing the official view, by the scientific establishment, can

[5] Wisdom-inquiry, in holding that the proper intellectual aim of inquiry is the social, humanitarian one of helping humanity learn how to realize what is of value in life, incidentally strengthens enormously the case for social epistemology.

be taken to be a striking indication of the extent to which the philosophy of knowledge is upheld by the scientific community.

Furthermore, scientists do on occasions themselves affirm – what is generally, I claim, held to be obvious – that everything apart from testable, factual propositions is excluded from the intellectual domain of science. Thus Einstein wrote:

". .. all scientific statements and laws have one characteristic in common: they are 'true' or 'false' (adequate or in adequate) . . . the scientific way of thinking has a further characteristic. The concepts which it uses to build up its coherent systems do not express emotions. For the scientist, there is only 'being', but no wishing, no valuing, no good, no evil – in short, no goal. As long as we remain within the realm of science proper, we can never encounter a sentence of the type: 'Thou shalt not lie'. There is something like a Puritan's restraint in the scientist who seeks truth: he keeps away from everything voluntaristic or emotional. Incidentally, this trait is the result of a slow development, peculiar to modern western thought." (1953, p. 779)

A somewhat similar affirmation of the value-neutrality of pure science has been made more recently by Professor Sir Ernst Chain, FRS, in the following terms:

"science, as long as it limits itself to the descriptive study of the laws of Nature, has no moral or ethical quality, and this applies to the physical as well as the biological sciences. No quality of good or evil is attached to results of research aimed at determining natural constants, such as that of gravity or the velocity of light, or measuring the movements of stars, describing the kinetic properties of an enzyme, or describing the behaviour of animals (whatever our emotional attitude towards it may be) or studying the metabolic activities of a microbe, whether harmful or beneficial to mankind, or studying physiological function or pharmacological and toxic action.

"No quality of good or evil can be ascribed to studies aimed at the elucidation of the chemical structure of substances of whatever nature, be it the harmless sodium chloride, the curative quinine or penicillin, or highly lethal poisons such as the botulinus toxin (a protein produced by the anaerobic *Clostridium botulinum* causing a deadly form of food poisoning, which will kill susceptible animals in amounts of fractions of a microgramme), be it the nucleic acids, the substances concerned with genetic transmission, or any other natural product, however important for life and reproduction its physiological action may be, and however potent its toxic effect on Man, animals and plants." (1970, p.166)

One version of the philosophy of knowledge insists that values are essential even to pure science – namely values that have to do with the cooperative search for truth. This position is expounded by Bronowski in his book *Science and Human Values* (1956), and later and independently by Monod in the last chapter of his *Chance and Necessity* (1974). For anyone hoping for an exposition of something approaching the philosophy of wisdom, or at least a criticism of orthodox conceptions of science, Bronowski's book seems at first sight promising. Bronowski begins by describing his arrival at Nagasaki in 1945 soon after the explosion there of the atomic bomb. It was this experience, he tells us, which prompted him to write the book. His basic problem might be put like this. What can be wrong with western science and western civilization that they can have led to the horrors of Hiroshima and Nagasaki? Any hope that Bronowski might be provoked to condemn the search for truth dissociated from the search for what is of value in life is, however, soon dashed. Bronowski merely reaffirms the traditional view that the basic aim of science is truth, the scientific search for truth making a vital contribution to civilization, the implicit assumption being that since science does have this value-neutral intellectual goal, it cannot be blamed in any way for what happened at Hiroshima and Nagasaki.

One of the most recent – and one of the clearest, fullest and most thoughtful – expositions of the philosophy of knowledge that I have come across was given by Professor Sir Andrew Huxley in his 1977 presidential address to the British Society for the Advancement of Science. Huxley sets out to defend the central tenets of standard empiricism and the philosophy of knowledge. Political, moral, religious and ideological judgements have no role to play in science; if allowed to infiltrate into the intellectual domain of science they can only serve to subvert scientific progress. The value of science resides in its capacity to acquire reliable knowledge of fact based on evidence alone. General, simplifying and unifying principles – such as the principle of conservation of energy in physics, or the principle that organisms are well adapted in biology – may provide good clues for new knowledge, but they are no substitute for evidence. In the end evidence alone, and not general principles, human hopes and fears, political, moral, religious or ideological views, must decide what is to be accepted and rejected in science. Opposition to Darwin's theory of evolution in the 1860s on religious grounds, opposition to Mendelian genetics and the adoption of Lysenko's Lamarckian views in the Soviet Union in the 1950s on ideological grounds, and more recent opposition to scientific research into questions concerning inheritance, intelligence and race in the 1970s on moral and political grounds, all constitute illegitimate and potentially damaging intrusions of human hopes

and fears into the intellectual domain of science, where fact and evidence alone ought to prevail. Huxley concludes:

"My message then is that neither clues nor motives are permissible substitutes for evidence firmly based on experiment and observation. There are temptations, on aesthetic grounds, to give too much weight to broad unifying principles which deserve to be used only as clues for suggesting further inquiry, and there is another set of temptations, on moral grounds, to pay too much attention to what we hope the social consequences of our discoveries will be – a large part of the motive of most scientists in carrying out their work. Although I have drawn attention to several cases where motive, political or ideological, is impeding or distorting the advance of science, I am not despondent about the future. I believe that, at least in the western countries, there are enough people around, both scientists and laymen, who appreciate that *in the long run the value of science depends entirely on its conclusions being independent of wishes and fears about their practical application,* and who will rally to the defence of science if the pressures that I have spoken of become severe." (1977; my italics)

2003

I have not been able to find any reasonably prominent natural scientist casting doubt on standard empiricism or the philosophy of knowledge, let alone advocating aim-oriented empiricism or the philosophy of wisdom, during the period in question. Occasionally, as I have mentioned elsewhere (Maxwell, 2004b, p. 6), scientists say something that looks at first sight like a rejection of standard empiricism. Thus John Barrow acknowledges that science makes presuppositions, but then goes on to argue that modern science has shown them to be false (Barrow, 1988, pp. 24-26). And again, Lewis Wolpert says of Barrow's presuppositions 'These assumptions may not be philosophically acceptable, but they are experimentally testable' (Wolpert, 1993, p. 107). Thus both uphold standard empiricism.

In other respects, however, changes have taken place in natural science which can be regarded as a slight shift towards some aspects of wisdom-inquiry. New inter-disciplinary research centres and institutions have been created which coordinate work being done in different departments or even in different universities, the research being directed towards helping to solve complex human problems, such as those associated with environmental problems, or problems of climate change more specifically. Some of these new centres, furthermore, take it to be an important part of their task to engage with the public, by holding seminars and public lectures, and by

interacting with relevant and influential representatives of the public such as members of parliament and parliamentary committees. More generally, there has been a change in the relationship between science and the public – or, at least, a change in the attitude of many scientists towards what that relationship should be. When the first edition of this book appeared, the scientific community, by and large, thought it sufficed to promote 'public understanding of science'. They thought communication should go in one direction only, from science to the public. Nowadays there seems to be a much wider appreciation that dialogue and debate is required; communication should go in both directions (as wisdom-inquiry requires). Scientists may even have things to learn from members of the public.

These changes have been brought about partly by dawning scientific awareness of the sheer urgency of some of our global problems, especially those associated with environmental degradation and climate change. Global warming in particular cannot be tackled by means of science and technology alone: it needs us to change our industry, our transport, our whole way of life – especially those of us living in developed and rapidly developing countries. Here is a pressing global problem of living which can only be resolved by enlightened and scientifically and technologically informed globally coordinated *action*. The unprecedented nature of the problem has forced parts of the scientific community, at least, to proceed in new, rather more wisdom-inquiry ways.[6]

Later on, in chapter 12, I will have more to say about these small but significant shifts towards wisdom-inquiry. Here, I conclude this section with some quotations from, and comments on, a rather remarkable Royal Society Presidential address in 2005 (a leap of two years into the future from 2003), given by the then president, Lord Robert May (2005). A number of the points that May makes echo basic themes of the 1984 edition of this book.

May begins by observing that 'The most important unanswered question in evolutionary biology, and more generally in the social sciences, is how co-operative behaviour evolved and can be maintained in human or other animal groups and societies' (p. 1). This book sought, in 1984, to contribute to the solution of just this problem by specifying how our institutions of learning need to be changed so as to help humanity learn how to tackle its conflicts and problems of living in increasingly cooperatively rational ways.

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[6] In Britain, the BSE disaster, and the GM crops rumpus, were perhaps contributory factors in prompting scientists to think again about what ought to be the relationship between science and the public.

May goes on to point out that cooperation in small groups is not too hard to understand, but that it poses much more of a problem when people began to live in big communities and cities from the beginnings of agriculture some 10,000 years ago. I made precisely this point in chapters 4 (pp. 95-8) and 6 (pp. 216-220). I went on to argue that a basic task of inquiry implementing the philosophy of wisdom is to help us learn how to act cooperatively in our big, diverse, complex, rapidly changing world.

May suggests that Enlightenment values – toleration of diversity, respect for individual liberty, holding ideas open to criticism and empirical assessment – may be at odds with social factors that have traditionally enabled large societies to persist. May does *not* suggest, however (as I have done), that blunders in the way the traditional Enlightenment has been developed and implemented are responsible for our failure to develop a kind of inquiry rationally designed to help us learn how to act in increasingly cooperative ways.

May is quite excellent in stressing that, because of modern science and technology, we live in 'the best of times and the worst of times' (p. 1) – or at least 'on the brink of the worst of times' (p. 3). May recognizes, in other words, that both good and bad comes from science. Having indicated the enormous benefits to humanity that have come from science and technology, May goes on to point out that these very successes have all sorts of initially unforeseen, potentially disastrous consequences. Recent rapid increase in population and associated industrial and agricultural developments (made possible by science) have led to habitat loss and extinction of species, and to such impending threats as food scarcity and global warming. There is a clear recognition, here, that modern science and technology have created a situation unprecedented in human history: science makes possible both great good and great harm. There is a new emphasis, a new even-handedness: in the past, scientists were much more ready to hold science responsible for all the good that comes from it, and humanity responsible for all the bad that comes from it. May clearly recognizes that the unprecedented powers to act that we have acquired from science and technology have created unprecedented dangers for humanity. This point is appreciated also by Martin Rees, Robert May's successor as President of the Royal Society. Rees has argued that this may be our final century because of powers bequeathed to us by modern science: see Rees (2003).

May goes on to give an excellent discussion of three such impending global dangers: global warming, the collapse of biological diversity, and the threat posed by existing and new infectious diseases. Throughout, May makes it clear that the real difficulty is to reach agreement on appropriate

action, and to implement such action. At one point May remarks that 'fisheries are a particularly telling example of the gulf that yawns between clearly identifying a problem and taking effective action' (p. 13). And he goes on to quote the fisheries scientist Daniel Pauly, who remarked that the relationship between fisheries science and management 'resembles a splendid and well-equipped hospital where patients' problems are diagnosed accurately, but where nobody receives treatment!' (p. 13).

When it comes to the nature of science, May makes clear that he upholds standard empiricism: he says at one point that 'the merit of ideas should be assessed on the strength of the evidence that supports them' (p. 2) – no hint, here, of aim-oriented empiricism. But May also declares emphatically that 'important aspects of science, in the widest sense, are indeed laden with values' (p. 19), and he goes on to clarify sensibly what he means. More strikingly, perhaps, May stresses that scientists should engage with the public in dialogue and debate: it is not just a question of scientists explaining science to the public. He says:–

"In everything I have said above there is the implicit, but hugely important, assumption that the scientific community has an obligation to explain itself – its agenda, its achievements, and their potential applications – to the public. This means individual scientists engaging more with wider society, explaining what they do and why, and responding through dialogue and debate to the interests, concerns and aspirations of the public. Such engagement is not always easy, in part because it often requires simplifying things (usually painful to researchers for whom the details can be entrancing), and must always avoid distortion. This dialogue between researchers and the general public – or, more accurately, the many and varied 'publics' – has in recent years been seen as an integral part of the scientific process. The UK has, I believe, been a leader in this, partly as the result of unfortunate earlier experiences (BSE in particular). The Royal Society hopes that, through its 'Science in Society' programme and other activities, it has been creative in its exploration of such engagement" (p. 20).

Such 'engagement with the public' is a vital component of the philosophy of wisdom, as I spelled it out in 1984. Indeed, I dramatized it earlier, in 1976, in the form of a debate between a scientist, a philosopher and others, in my first full exposition of wisdom-inquiry: see Maxwell (1976b).

What is very striking about Robert May's Presidential address is that there is a very clear recognition of the *problems* which wisdom-inquiry is designed to help us solve, and the beginnings of a shift in understanding of what the relationship between science and society ought to be towards what wisdom-inquiry requires, but not the faintest hint of an awareness that we urgently

need to bring about a revolution in academia if we are to give ourselves the best chances of tackling our immense, impending global problems in increasingly cooperative ways.

May concludes by declaring 'The Enlightenment's core values, which lie at the heart of the Royal Society – free, open, unprejudiced, uninhibited questioning and enquiry; individual liberty; separation of church and state – are under serious threat from resurgent fundamentalism, West and East' (p. 23). True. But there is another point to be made as well. The immensely influential Enlightenment programme of learning from scientific progress how to achieve social progress towards an enlightened world has been developed and implemented in academia in a damagingly bungled form. At the heart of what May, and so many others, take to be reason there is unreason. This cripples our attempts to tackle our global problems cooperatively, and is in part responsible for the gulf between science and action, of which May so tellingly writes. The Enlightenment core values, properly exemplified in the form of wisdom-inquiry, are threatened, not just by religious fundamentalism, but by a kind of scientific fundamentalism which fails to see the structural intellectual and humanitarian defects inherent in knowledge-inquiry, in the *status quo*, and thus fails to see the need to create a genuinely rational kind of inquiry devoted to seeking and promoting enlightenment. It is not just that Enlightenment core values are threatened; in an important sense, they have not yet been recognized and tried out.

There is a certain irony here. May speaks eloquently of 'the gulf that yawns between clearly identifying a problem and taking effective action' (p. 13). A major problem with science, and with academia more generally, was identified over twenty years ago, with the publication of the first edition of this book in 1984. It even received something like official scientific recognition with a glowing review in what is almost the official organ of establishment science, the journal *Nature*. The reviewer declared at one point that 'there are altogether too many symptoms of malaise in our science-based society for Nicholas Maxwell's diagnosis to be ignored' (Longuet-Higgins, 1984). But ignore the diagnosis is just what the scientific community has done. No attempt has been made to take up wisdom-inquiry, let alone implement it in academic practice. Here, too, a gulf yawns between the clear identification of a problem and effective action. May's address reveals recognition of some of the global problems that confront us, but *no recognition of the problems inherent in academia* which, if unsolved, inhibit our capacity to resolve the global problems cooperatively. Small, piecemeal steps towards wisdom-inquiry have been made, but there is still, two decades later, not even

the beginning of awareness in the scientific community of the scale of what so urgently needs to be done.

But now, let us return to 1983, and to what I found out then about scientific practice, viewed from a slightly different perspective.

4 Science Abstracts

1983

In the end what matters, of course, is the philosophy of inquiry that is actually put into scientific practice rather than the philosophy of inquiry that is consciously believed by scientists (by no means necessarily one and the same thing). Does science in practice embody and institutionalize the philosophy of knowledge rather than the philosophy of wisdom?

In order to answer this question, it is essential to identify the intellectual domain of science in a practical and institutional sense. The obvious way to do this is in terms of science's own identification of its intellectual domain, by means of 'science abstracts'. Not only do the various 'science abstracts' provide a record of what various scientific disciplines decide constitute contributions to science: in addition, this record plays an important role in actually shaping future science – the function of 'science abstracts' being to put scientists in touch with relevant published material.

I have therefore examined the contents of various 'science abstracts' for the year 1980.

In that year *Physics Abstracts* records 109,577 contributions to physics in published papers and books. Of these, just thirty-eight are devoted to 'philosophy of science'; and only fourteen are devoted to problems of 'science and society'. Furthermore, all the recorded contributions to the philosophy of science either presuppose or defend some version of standard empiricism and the philosophy of knowledge. Not one contribution listed in the 'Philosophy of Science' or 'Science and Society' subsections reveals the faintest glimmering of an awareness that discussion of social problems and possible and actual social goals for physics might have some relevance for the philosophy of physics. There is nowhere the faintest hint of the idea that if physics is to be both intellectually rigorous and of real human value then it is absolutely essential for the intellectual domain of physics to include some imaginative and critical discussion of actual and possible human or social goals for physics and how these influence choice of research problems in physics. Neither the technological aspect of physics, nor the cultural aspect (theoretical physics pursued as a vital part of our endeavour to improve our

understanding of the universe and our place in it) show any signs of being consciously pursued in accordance with aim-oriented empiricism or aim-oriented rationalism (as characterized in chapter 5).

It must be admitted that one or two of the fourteen (out of 109,577) contributions listed in the 'Science and Society' subsection can be interpreted as developing highly specific applications of the general argument developed in this book. Thus E. Woollett summarizes his paper, entitled 'Physics and Modern Warfare: the Awkward Silence' as follows: 'General education students enrolled in courses in physics or physical science are ill-served by an almost total lack of discussion of the intimate links between progress in science and 'progress' in weapons systems. The author discusses in detail the great dependence of the present arms race on a healthy physics enterprise and the pervasive connections between pure and applied science and military needs.'

General failure of physics courses to discuss the intimate links between scientific and weapons research is, I claim, one highly specific – and extremely important – example of something far more general: the failure of science to give intellectual priority to the discussion of problems of living, so that problems of knowledge are tackled as rationally subordinate to problems of living. Institutionalization of the philosophy of wisdom would automatically demand that all science courses give intellectual priority to a consideration of human problems. Institutionalization of the philosophy of knowledge automatically excludes discussion of human problems of living from the intellectual domain of science.

The contents of Woollett's paper, and of one or two other papers with related themes, powerfully confirm the basic claim of this chapter. At the same time, the mere existence of such papers, however few in number, recorded in *Physics Abstracts,* can perhaps be interpreted to be the first minute, fragmentary signs or hints of a possible future dramatic change in the overall philosophy of physics, from knowledge to wisdom. It deserves to be noted that the subsection 'Science and Society' in *Physics Abstracts* was introduced only in 1977; before that the 'General' section only had subsections: 'Communication'; 'Education'; 'History'; 'Philosophy'. In *Physics Abstracts* in 1965, there are just four papers on the philosophy of physics out of a total of 34,000 contributions to physics: no paper has a theme remotely touching on any 'Science and Society' topic.

One qualification must be made to the above. In the year 1980, *Physics Abstracts* has sixty-two main sections. So far just one of these has been discussed, namely the one entitled 'Communication, Education, History and Philosophy' (section 01.00). There are however two sections that are

concerned with technology related to human problems, namely: 'Energy Research and Environmental Science' (section 86.00), and 'Biophysics, Medical Physics, and Biomedical Engineering' (section 87.00). Of course, the mere existence of technological research that is devoted to helping to solve human problems does not in itself favour either of the philosophies of inquiry. The 'Energy Research and Environmental Science' section does, however, include some contributions which explicitly discuss environmental problems, and problems of energy policy. These contributions comply more with the intellectual standards of the philosophy of wisdom than with those of the philosophy of knowledge. It is however significant that this section appears to have only been introduced into *Physics Abstracts* in the year 1979; I have been unable to find any trace of the section in earlier years.

There are, in short, a very few recent contributions to the intellectual domain of physics (as defined by *Physics Abstracts)* which can be interpreted as specific, limited attempts to discuss problems of living related to physics. The great bulk of contributions to physics conforms, however, entirely to the edicts of the philosophy of knowledge. There is no hint of the philosophy of wisdom or of aim-oriented empiricism in the way the intellectual domain of physics is organized, into sections and subsections, as depicted by *Physics Abstracts.*

The few, marginal, scattered hints of some aspects of the philosophy of wisdom that are to be found in a few contributions listed in *Physics Abstracts,* disappear altogether, however, when one turns to *Chemical Abstracts.* Here there is no philosophy of science subsection and no science and society subsection: instead there is one section (out of a total of thirty-four) entitled 'History, Education, and Documentation'.

Abstracts for other disciplines in the physical sciences and technologies by and large confirm this general picture. Somewhat exceptional, perhaps, are *Electrical and Electronic Abstracts,* with such subsections as 'Administration and Management' and 'Planning', and *Computer and Control Abstracts,* with such subsections as 'Philosophical Aspects', 'Economic, Social and Political Aspects', and sections such as 'Systems Theory and Cybernetics' and 'Administrative Data Processing'.

As one moves from the physical sciences to the biological, medical and human sciences, so, as one might expect, more and more concessions are made to the need to discuss human problems of living. There are few signs of deviation from the philosophy of knowledge in *The Zoological Record:* in *Biological Abstracts,* however, there are sections which include contributions that discuss social problems, such as 'Public Health' and 'Psychiatry'. *Geo Abstracts* devotes entire sections to contributions to geography that are, in

one way or another, concerned with human problems: for example, 'Economic Geography C'. 'Social and Historical Geography D', and most strikingly 'Regional and Community Planning F' – many contributions to geography listed in this section being concerned with just the kind of problems at the centre of this book. *Psychological Abstracts* too, not surprisingly perhaps, includes, amongst its sixteen sections, some that are concerned with problems of living – such as 'Social Processes and Social Issues', 'Educational Psychology' and 'Applied Psychology' (although one cannot help but note that the very term 'applied' as used here, presupposes the philosophy of knowledge). In the fields of psychiatry, government, political science, economics, sociology and international affairs, it is of course standard for social problems, of one kind or another, to receive attention. There are academic journals in these fields more or less devoted to the discussion of social problems of one kind or another: for example, *Applied Economics, Development, Economic Impact, Human Relations, International Affairs, International Relations, Journal of Black Studies, Journal of Conflict Resolution, Journal of Development Studies, Journal of Social Policy, New Society, Political Science Quarterly, Radical Philosophy, Science and Society, Social Policy and Administration, Social Problems*

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Since 1980, physics abstracts, and abstracts of other scientific disciplines, have come online, which makes it very difficult to employ the abstracts test to compare contributions to science in the year 2000 with those for 1980. (*Physics Abstracts* for the year 1980 came in the form of a large volume which one could hold in one's hands.) INSPEC, the online system for physics, provides one with a variety of search methods to pick out specific contributions to physics. Some of these yield different answers for 'physics', 'philosophy of science' and 'science and society'. One such method yields, for the year 2000, 192,426 entries for physics, 25 for Philosophy of Science (of which only 12 can be said really to be philosophy of science published in physics journals), and 52 for Science and Society. But other, apparently equally different INSPEC search methods yield different numbers of entries – sometimes dramatically different numbers. And whatever method is used, a considerable number of items turn out to be published in journals that could not conceivably be regarded as physics. All this makes it impossible to compare 1980 with 2000 using *Physics Abstracts*. Similar difficulties beset comparisons in terms of abstracts of other scientific disciplines. Progress, as usual, has its drawbacks. I suspect, nevertheless that, during the period we are considering, natural science has in reality devoted more attention to the

philosophy of science, and to science and society issues and, to this extent, there has been a shift in the direction of wisdom-inquiry. I return to this question in chapter 12.

5 Social Inquiry

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One might be inclined to conclude from the above list of academic journal titles alone, that the various branches of social inquiry at least – economics, sociology, psychology, political science, the study of international affairs and anthropology – do put the philosophy of wisdom into practice (if only the version outlined in chapter 4). In fact, quite to the contrary, these academic disciplines all presuppose the philosophy of knowledge (in one or other of its versions). This holds even when, as in the case of economics or psychiatry, the basic rationale for the discipline is clearly recognized by everyone to be to help solve a group of important human problems of living.

Thus modern economics from the outset, with the work of James Steuart, Adam Smith, Malthus, Ricardo and J.S. Mill, conceived of itself, in the spirit of the Enlightenment, as a *science,* a discipline devoted to the acquisition of *knowledge* about economic phenomena which, when applied, would help solve practical economic problems (or show that some cannot be solved). Even Marx upheld this conception of economics. In the preface to *Das Kapital,* Marx makes it quite clear that he is making a contribution to knowledge, to the science of political economy. He tells us 'It is the ultimate aim of this work to lay bare the . . . law of motion of modern society' (1921, I, p. 14) and he quotes with approval the following remarks of a reviewer of an earlier edition of the book: 'Whilst Marx sets himself the task of following and explaining from this point of view the economic system established by the sway of capital, he is only formulating, in a strictly scientific manner, the aim that every accurate investigation into economic life must have. The scientific value of such an inquiry lies in the disclosing of the special laws that regulate the origin, existence, development, and death of a given social organism and its replacement by another and higher one' (p. 24). This tradition continues through the work of figures like Jevons, Menger, Marshall and Keynes down to the present day. There are, of course, disagreements and developments within economics: but these are disagreements about, and developments in, what is taken to be theoretical economic *knowledge.* Any standard history of economic thought – such as Blaug (1968) – is a history of economic *science,* a history of the endeavour to improve knowledge of the economic aspect of

life. J.N. Keynes, father of *the* Keynes, in his introductory remarks to his *Scope and Method of Political Economy* (1890), makes it quite clear than he subscribes to this general view. Thus he remarks *'Political economy* or *economics* is a body of doctrine relating to economic phenomena . . . the purpose of the following pages is to discuss the character and scope of this doctrine, and the logical method appropriate to its development. In seeking to define the *scope* of any department of study, the object in view is primarily to determine the distinguishing features of the phenomena with which it deals, and the kind of knowledge that it seeks concerning these phenomena' (p. 2). Friedman again asserts 'Positive economics is in principle independent of any particular ethical position or normative judgements. As Keynes says, it deals with 'what is', not with 'what ought to be'. Its task is to provide a system of generalizations that can be used to make correct predictions about the consequences of any change of circumstances' (1968, p. 509). Robbins, in his significantly titled *An Essay on the Nature and Significance of Economic Science,* ponders, in the first chapter, the problem of what it is that economic science seeks to acquire knowledge about, the assumption that economics does seek knowledge being so entirely taken for granted that it is not even explicitly stated. In the second chapter Robbins remarks '. . . Economics is entirely neutral between ends; that is, in so far as the achievement of any end is dependent on scarce means, it is germane to the preoccupations of the economist. Economics is not concerned with ends as such' (1935, p. 24). Joan Robinson, more recently, remarks 'Economics . . . consists . . . of imperfectly tested hypotheses – about how an economy works, why one economy differs from another, what consequences are to be expected from any particular events or particular policies' (1960, pp. xv-xvi). Worswick more recently still, in an essay entitled 'Is progress in economic science possible?' remarks '. . . we conceive progress in economic science as consisting of a dialogue, or interaction, between fact and theory, the latter being strengthened or modified according as new data come to light and according as they agree or disagree with hypotheses deduced from theory' (1972). Worswick makes clear that 'theory' here is to be understood as factual, explanatory theory 'as in any science'. Finally Hollis and Nell in their *Rational Economic Man* remark, 'we share the view advanced here that economic theory is a branch of a more general tree of knowledge' (1975, p. 251).

There are signs of a growing sense of malaise amongst economists about the nature and status of their discipline – due in part to an awareness of recalcitrant economic problems in the real world, and also to a more general feeling of unease amongst social scientists about the nature and success of social inquiry. This sense of malaise finds expression in books such as Ward,

What's Wrong with Economics (1972); Hutchison, *Knowledge and Ignorance in Economics* (1977); Bell and Kristal (eds), *The Crisis in Economic Theory* (1972). I have hunted in vain, however, in the economic literature, for signs of the philosophy of wisdom being advocated and consciously put into practice in economic inquiry. The task of helping us to put aim-oriented rationality into practice in our economic activities in a cooperative and just fashion, so that we all benefit, is not the central intellectual concern of economic inquiry. Methodology and philosophy are discussed by economists far more seriously than by natural scientists: see for example, works by von Mises (1960), Hutchison (1978) and Blaug (1980). But what is invariably discussed is the methodology and philosophy of *economic inquiry,* conceived to have the basic intellectual aim of acquiring knowledge of economic phenomena, of use for economic activity itself. What economists do not do is to endeavour to apply the methodology and philosophy of aim-oriented rationalism to economic activity itself, to economic pursuits – economic inquiry concerning itself with the problems that such an endeavour gives rise to. This disastrous intellectual and professional failure of economists during the last two centuries is not unrelated to the very serious economic problems that confront humanity today – wealth and poverty distributed amongst people and nations in an appallingly unjust way, rapid depletion of capital in the form of finite natural resources, world recession, unemployment, the failure to develop the free market system so that it functions in such a way that we all enhance our capacity to realize what is genuinely of value to us in life.

Analogous states of affairs prevail in the other branches of social inquiry – in sociology, psychology, history, theoretical psychiatry and psychotherapy, political science, anthropology, the study of international affairs, development studies, conflict studies and so on. In all these fields, the basic intellectual aim is to improve factual knowledge – and understanding – of different aspects of social phenomena, of the social world. This central intellectual aim goes back to the birth of the discipline, even when the discipline can be traced as far back as the Enlightenment of the eighteenth century. Social inquiry as science, as inquiry with the basic aim of improving knowledge of the human world, was indeed in a sense the creation of the Enlightenment. Almost all those in the eighteenth, nineteenth and twentieth centuries recognized by academic social inquiry itself to have made the most important contributions to the field have advocated, and have sought to put into practice in their work, some version of the philosophy of knowledge. This is true, for example, of such diverse major figures in sociology as Montesquieu and Ferguson in the eighteenth century, Comte, Mill and Marx in the nineteenth century, Pareto, Durkheim and Weber in the late

nineteenth and early twentieth centuries. It is true of more recent writers in the field, such as Talcott Parsons, Alfred Schutz, R. Nisbet, Raymond Aron, Erving Goffman, Donald MacRae, John Rex, Alan Ryan and Anthony Giddens. In psychology it is true of such diverse figures as Wundt, James, Gallon, McDougall, Fechner, Kohler, Koffka, Wertheimer, Pavlov, Watson, Piaget, Burt, Skinner, Eysenck, Hudson.

In all these diverse fields of social inquiry, both classic texts and introductory textbooks, assume or assert that the discipline has, as its basic intellectual aim, to improve knowledge and understanding of the relevant aspect of the human world. Thus in sociology, a major figure such as Weber declares that 'Sociology . . . is a science which attempts the interpretative understanding of social action in order thereby to arrive at a causal explanation of its course and effects' (1947, p. 80). The following quotations are typical of the introductory remarks to be found in elementary sociology textbooks. 'Sociology is one of the social sciences. Its purpose is the scientific study of human society through the investigation of the social behaviour of man' (Giner, 1972, p. 9). 'Sociology is the scientific study of human interaction. It is also the body of knowledge about human interaction resulting from such study' (Cairns and Dressier, 1973, p. 3). 'As a scientific field, sociology is both academic and applied. Like all scholars, sociologists try to be exact. As a result, they have developed ways of studying social life that may seem remote from urgent human concerns. This does not mean that sociologists do not care. Most want to help solve social problems, but if research is to be socially useful, it must be sound and objective. Thus, the sociologist is pulled in two directions: towards careful, well-designed studies and toward efforts to solve pressing social problems. The tension between these two priorities is expressed in a debate that has been going on for more than a generation' (Broom *et al.,* 1981, p. 7).

Analogous quotations can readily be accumulated from the classic works, and from the introductory textbooks, of the other fields of social inquiry – psychology, anthropology, history, political science, and so on.

As in economics, and as in sociology (as the last quotation indicates), so too in the other fields of social inquiry there is a general concern to help solve personal and social problems of living experienced by people in life, social inquiry seeking, in this way, to contribute to the promotion of human welfare. The fundamental idea that prevails, however, throughout the diverse branches of social inquiry, is that the proper way for each discipline to contribute towards the resolution of personal and social problems of living is, in the first instance, to acquire relevant factual, theoretical and explanatory knowledge, and then apply this knowledge to helping to solve social

problems. A distinction is maintained between *social* problems and *intellectual* problems of knowledge and understanding of each discipline – the primary intellectual task of each discipline being to solve these latter problems of knowledge and understanding. As one sociological textbook remarks 'Social problems are not the same thing as sociological problems' (Worsley *et al.,* 1970, p. 51). All this exemplifies the philosophy of knowledge, and echoes the analogous situation to be found in the physical and biological sciences (such as the distinction between technological and applied scientific research on the one hand, pure scientific research on the other hand).

Social 'scientists' do of course disagree amongst themselves about the relative merits of 'pure' and 'applied' social inquiry; they disagree as to where priorities ought to lie, in terms of money and research effort. Furthermore, they disagree in their views as to what the most urgent and important human problems are in the world today, and what needs to be done in order to help resolve them. Some social 'scientists' are politically conservative, others uphold liberalism, others democratic socialism, and others again some form of revolutionary socialism. All this echoes analogous states of affairs to be found in the physical, biological and technological sciences, and corresponds to what one would expect to find, granted that those fields are dominated by the philosophy of knowledge.

Another major long-standing disagreement that runs through most branches of social inquiry – and one which has more to do with the nature of social inquiry itself – is the disagreement between the pro-naturalists and the anti-naturalists.[7] Pro-naturalists such as Popper, Friedman, Blaug, Harris, Hutchison, Skinner, Eysenck, Wilson, Keat and Urry, can trace the naturalist tradition in diverse branches of social inquiry back to the Enlightenment. Anti-naturalists of one kind or another – Winch, Laing, Berlin, Goffman, Giddens, Shotter, Habermas, Foucault, Gadamer, Betti – can trace their tradition back equally far, to what Berlin has called the Counter-Enlightenment. Via hermeneutics, phenomenology and existentialism – via the writings of such figures as Schutz, Collingwood, Merleau-Ponty, Sartre, Heidegger, Husserl, Nietzsche, Kierkegaard, Dilthey, Schleiermacher – this tradition can be traced back to early nineteenth- and late eighteenth-century Romantic thought and literature, to Herder and to Vico. However, insofar as this long-standing debate is about whether or not the kind of knowledge and understanding to be sought in social inquiry, and the kind of methods to be employed, are similar to the knowledge, understanding and methods of the

[7] Pro-naturalists hold that social science is similar to natural science, anti-naturalists hold that it is different.

natural sciences, this debate is internal to the philosophy of knowledge. It is a debate about what version of the philosophy of knowledge to adopt for social inquiry. The existence of the debate is itself a striking confirmation of the thesis that the philosophy of knowledge – in one or other version – prevails in social inquiry. For, of course, from the standpoint of the philosophy of wisdom, the debate disappears.

According to the philosophy of wisdom, the aim of all of inquiry – including natural science – is to help us to live more aim-oriented rationalistic lives and develop more aim-oriented rationalistic institutions. Philosophy/sociology of science is a special case of this. Social inquiry is social methodology, very similar to scientific methodology, but of course quite unlike scientific inquiry itself. There is unity of aim and method throughout all of inquiry even though social inquiry is quite different from natural science. The philosophy of wisdom reveals with striking clarity that pro-naturalists and anti-naturalists are both partly right and partly wrong, and that the debate itself is entirely misconceived. The persistence of the debate amongst scholars pursuing social inquiry indeed helps to maintain the philosophy of knowledge by distracting attention away from the vital need to pursue social inquiry as reason devoted to the growth of wisdom in life, and by creating artificial divisions, conflicting intellectual interests and concerns, so that social inquirers fail to engage in the massive *cooperative* venture of putting the philosophy of wisdom into practice throughout all of social inquiry – indeed throughout all of inquiry and of life.

There is, it is true, a great deal of methodological discussion in social inquiry, not only in economics, but also in sociology, psychology, anthropology and elsewhere. But this methodological discussion is almost entirely devoted to the problems concerning the methodology of the diverse branches of social inquiry conceived to have the basic intellectual aim of improving knowledge and understanding of aspects of the human world. It is not at all concerned with problems arising from the endeavour to put aim-oriented rationalistic methodologies into practice in our diverse personal, institutional and social enterprises in life. It thus presupposes versions of the philosophy of knowledge throughout, and fails disastrously to be social inquiry pursued in accordance with the philosophy of wisdom.

The philosophy of knowledge has such a vice-like grip on the minds of social inquirers, that on occasions it leads them to project it onto the human world, and even onto the biological world. Thus Kelly, sensibly and correctly enough, recognizes that people pursue inquiry in their lives: but because the philosophy of wisdom is not available to him, he cannot make the elementary points made here – namely that personal thinking is fundamentally

concerned with problems of action, with realizing various desired goals in life, being pursued, ideally, in an (aim-oriented) rational way – all academic inquiry having, as its basic rationale, to promote personal inquiry in life so that we may realize what is of value to us. Kelly, gripped by the philosophy of knowledge, is obliged to interpret the personal inquirer as a sort of scientist, seeking knowledge. Personal construct theory then itself seeks to develop academic psychological knowledge about the knowledge acquiring, or construct building, endeavours of individuals (Kelly, 1955; Bannister and Fransella, 1971).

A second example of the phenomenon is to be found in the interpretation of evolution recently put forward by Plotkin and Odling-Smee (1981). These authors, quite properly, deplore the tendency of some workers on the theory of evolution, such as Dawkins (1978), to interpret evolution primarily in mechanistic or physical terms – so that the theory of evolution is used to explain purposiveness away, rather than to help us understand how and why purposiveness has gradually evolved in the world. All this is excellent. But then Plotkin and Odling-Smee go on to make the disastrous assumption that the goal of life can be taken to be knowledge-gain, it thus being legitimate to interpret evolution as the evolution of knowledge, learning being interpreted, in philosophy of knowledge terms, as acquiring knowledge, and not, in philosophy of wisdom terms, as learning how to live. It is almost as if the authors conceive of all of life as striving to do what academic scientists and scholars do – acquire knowledge. In fact, of course, though knowledge-acquisition is implicated in animal and human living and learning, both animals and humans pursue many goals, and learn how to pursue many goals besides that of acquiring knowledge.

As in economics, so in other branches of social inquiry, there is a constant sense of malaise, a sense that there is something fundamentally wrong in the way each discipline is pursued and conceived: this is, for example, indicated in the very titles of works such as: Gouldner, *The Coming Crisis in Western Sociology* (1970); Joynson, 'The breakdown of modern psychology' (1970); Brown (ed.), *Radical Psychology* (1973); Dyal *et al., Readings in Psychology: the Search for Alternatives* (1975); Heather, *Radical Perspectives in Psychology* (1976), all referred to in Westland, *Current Crisis of Psychology* (1978). So far, however, this sense of crisis has not led to a general recognition that all this is but part of the general crisis that confronts humanity today due to its longstanding failure to put the philosophy of wisdom into practice in personal and social life – its long-standing failure, indeed, even to conceive of the urgent need to attempt to do this.

The central, and tragic, intellectual mistake (according to the philosophy of wisdom) that has bedevilled social inquiry ever since the Enlightenment is illustrated in miniature in an especially graphic and simple way in a book by Barbara Wootton entitled *Testament for Social Science* (1950). Its subtitle – 'An Essay in the Application of Scientific Method to Human Problems' – might lead one to believe that the book expounds and defends the philosophy of wisdom. But if the first few sentences of the book strengthen this belief, what follows must quickly dispel the idea. The book opens as follows:

"The contrast between man's amazing ability to manipulate his material environment and his pitiful incompetence in managing his own affairs is now as commonplace as it is tragic. The world of atomic energy and nylon is for millions still the world of poverty, hunger, misgovernment, crime, domestic unhappiness or personal frustration. And mastery over earth and air and sea and atom has brought us only to daily fear of sudden death of our own making. No one has any doubt how that mastery has been won. It is by vigorous devotion to scientific method that we have made our conquests over the material environment . . . It is no less obvious that this method, which has been so brilliantly successful in the natural sciences, is not normally applied to the field of our most disastrous failures. The personal relations of human beings, individual and collective, are conducted in a quite different way: these are for the most part governed by a medley of primitive impulses, kindly or harsh, sometimes even noble, modified by rules of thumb, and set in a framework of a traditional morality which varies from place to place and from age to age. In these matters science plays little part and commands but meagre respect . . . experience (falls) into two sharply divided sections – that in which science speaks with authority, and that in which she whispers furtively, or is dumb. This contrast surely seems to point a simple moral – that one ought seriously to ask whether the tool that has worked such wonders in the one job could not be used for the other. More than a century has passed since Auguste Comte said that the rational reform of society must be brought about by the application of scientific method to social problems. If not very much has happened since to prove him right, certainly even less has happened since to prove him wrong. In the intervening century scientific method has marched from victory to victory in the field of natural phenomena, while those human problems which have not enjoyed its attention remain as intractable as ever. It is, therefore, the first purpose of this essay to ask how far these problems also might be tackled by the methods of science . . I hope to show (that) the potential contribution of science in this field is far greater than anything we have yet seen: the

differences between the material of the social and the natural sciences are differences of degree, rather than of kind." (Wootton, 1950, pp. 1-3)

Any lingering doubts one might have as to what Wootton is advocating vanish altogether with her second chapter entitled 'Scientific Method in the Social Sciences'. The chapter begins: "'The stages of the scientific progress are now generally familiar. There is first the accurate observation of data; then the formulation of an hypothesis; and finally the promotion of the hypothesis by empirical verification to the status of a law. Scientific method 'is simply the attempt to acquire knowledge of general laws directly or indirectly by experience, by the use of our five senses' A.D. Ritchie, *Scientific Method,* Kegan Paul, p. 189). Our problem is thus to determine how far a parallel attempt can be made to acquire knowledge about human relationships" (1950, p. 6).

In short, Wootton is not advocating that methods that have proved successful in the cooperative endeavour of science be generalized and exploited in our other cooperative endeavours in life, with their diverse aims – government, industry, art, literature, marriage, love, and so on. She is not arguing that social inquiry be pursued as social methodology – with the task of helping us to develop (aim-oriented) rationality in life. In absolute contrast to this, she assumes, without a flicker of doubt, that the task of applying 'scientific method to human problems' involves applying scientific method to *the enterprise of improving knowledge about social phenomena* – the central task being to develop social science on analogy with natural science. From the standpoint of the philosophy of wisdom, this is perhaps the basic intellectual disaster of the philosophy of knowledge.

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We saw above that, during the years leading up to 1983, some economists expressed doubts about economic orthodoxy. This continues during the two decades that follow. Donald McClaskey (1994) argues that economics is a science that uses stories like geology, and metaphors like physics (which does not, however, break with the idea that economics is a science). Michael Perelman, rather more radically, in his *The End of Economics* (1996), declares 'I can no longer accept the validity of prevailing orthodoxy' (p. 1), and goes on to say: 'I hope to expose economics as a pseudo-science that stands in the way of human betterment' (p. 7). George Brockway, in his *The End of Economic Man* (1995), argues against economics as a science like physics and for the view that economics is 'one of the 'moral sciences' which considers

the proper conduct of life' (p. 10), and goes on to remark that 'There is no point of view from which the solar system can be criticized, but if any economics system cannot be criticized, economics is an empty pastime' (pp. 10-11). Amartya Sen (1987), earlier, argued for the view that economics should be construed to be a branch of moral philosophy. Sen suggests economics has two origins: one rooted in ethics going back to Aristotle, and the other rooted in 'engineering' and being restricted to tackling technical economic problems. Sen goes on to argue that the 'engineering' approach needs to be absorbed into the more fundamental ethical approach, modern economics having been 'substantially impoverished by the distance that has grown between economics and ethics' (p. 7). This theme is taken up by, among others, Daniel Hausman and Michael McPherson in their *Economic analysis and moral philosophy* (1996).

One enormously important development during the period under consideration is the founding of green economics by David Pearce and others (Pearce *et al.*, 1989, 1991). As I write, in the Autumn of 2006, with signs of global warming all too apparent, the UK government is at last beginning to take seriously the idea of a carbon tax for the emission of carbon dioxide. It is sobering to realize that a few bold economists were discussing the idea as long ago as in the late 1980s: see Barrett (1991).

In order to see whether there has been any significant movement towards wisdom-inquiry in economics during the period we are considering, I decided to look at introductory textbooks in economics. I looked at 35 such textbooks[8] published during the years 1984 to 2006, paying attention, in particular, to how economics is defined. Wisdom-inquiry might define economics as follows:

Economics, like other branches of social inquiry and the humanities, is concerned to help humanity realize what is of value in life (in sustainable, just, and increasingly cooperatively rational ways). Economics is concerned with those aspects of this fundamental problem that relate (or ought to relate) to monetary value – to the 'economic' aspects, in other words. Or, put in slightly more familiar terms, economics seeks to help humanity create, sustain and justly distribute wealth. Economics does this by, in the first instance, (a) articulating, and seeking to improve the articulation of, the economic problems that need to be solved, and (b) proposing and critically

[8] Selected by typing 'introduction' as a title key word and 'economics' as subject in an advanced search of the catalogue of the British Library, and carrying on until exhaustion set in.

assessing possible and actual solutions – actions, policies, economic and political programmes, philosophies of life insofar as these are relevant to the economic aspect of our fundamental problem of living. Economics also seeks to help build into economic and other social institutions and patterns of living problem-solving rationality and aim-oriented rationality.

None of the books I examined came close to characterizing economics in that way. Most characterize economics in a way that has scarcely changed since Robbins (1935). Indeed, one of the books I consulted quotes Robbins' definition with approval: 'Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses' (Robbins, 1935, p. 16; quoted in Ricketts, 2003, p, 4). This does not differ much from the following. 'Economics is the study of how societies use scarce resources to produce valuable commodities and distribute them among different people' (Samuelson and Nordhaus, 2001, p. 4). '*Economics* studies how individuals, firms, governments, and other organizations within our society make *choices*, and how those choices determine the way the resources of society are used' (Stiglitz, 1997, p. 9). '*Economics*. The science which deals with the decisions involved in the creation and allocation of goods and services for the satisfaction of human wants under conditions of scarcity' (Weidenaar and Weiler, 1983, p. 465). 'ECONOMICS IS THE STUDY OF HOW MEN AND SOCIETY CHOOSE TO ALLOCATE SCARCE RESOURCES BETWEEN ALTERNATIVE USES IN THE PURSUIT OF GIVEN OBJECTIVES' (Hill, 1990, p. 5). Almost all the books I consulted characterized economics in a similar way. The rare and slight exceptions will be discussed in a moment.

These are knowledge-inquiry conceptions of economics because the presumption is that the task of economics is, in the first instance, to study, to improve knowledge and understanding of, that aspect of the social world to which economics is devoted. First, knowledge is to be acquired; then it can be used to help solve economic problems. And it is not just that economics is defined in this way; the books go on to exemplify this conception of economics throughout the rest of the text. Rohlf (1999), for example, makes the point like this 'Before economists can recommend policies for dealing with economic problems or achieving goals, they must understand thoroughly how the economic system operates' (p. 19).

Some books distinguish 'normative' from 'positive' economics. Thus Craven ((1990, p. 13) distinguishes three types of economic statements:–

(a) *Positive*: predictions of the model.

(b) *Prescriptive*: what can be done (according to the model) to achieve a particular aim.

(c) *Normative*: what should be done (based on predictions of the model).

The model – embodying economic knowledge of the economic system – is clearly a prerequisite for making all three kinds of statements. Normative economics is just as much knowledge-inquiry as positive or prescriptive economics. As it happens, Craven goes on to say 'normative statements have no place on our analysis' (p. 14).

Some books demur at calling economics a science. Thus Robinson (1986, p. 6) admits that 'Economics may not be a science (because of limitations of its capacity to predict)'. Economics is still, however, unquestioningly construed in knowledge-inquiry terms. Thus Robinson declares 'the whole purpose of economics is to help us to understand the real world, the world in which we live and the economy in which we operate' (p. 3), and 'Economic theory is an attempt to explain people's behaviour in certain specific circumstances' (pp. 3-4).

Knowledge-inquiry economics does not just seek to develop knowledge about how economies work (by the construction, in the main of economic models); it also aims to help solve real-life economic problems – just as any branch of natural science with practical applications (physics or biology) would as well. Dobson and Palfreman (1999, p. 3) spell out how economists tackle a real-life economic problem as follows: '(i) state the problem (ii) apply an economic model (iii) use that model to identify solutions (iv) use economic tools and some objectives to evaluate each of these solutions, and (v) choose and implement a solution (it is normally policy makers who make the final choice).' Economic knowledge plays an essential role here, in steps (ii) and (iii) and, to some extent, in step (iv). Wisdom-inquiry economics would differ from this in emphasizing the priority of (i) articulating, *and seeking to improve the articulation of*, the problem to be solved, and (ii) proposing and critically assessing possible solutions – possible actions or policies. Steps (i) and (ii) would take into account relevant ideas, discussion and debate in other branches of social inquiry, and in the public domain outside academia. Steps (i) and (ii) would be undertaken in such a way that no economic model whatsoever is presupposed. Presupposing some specific economic model (as knowledge-inquiry does) may well damagingly restrict both steps (i) and (ii). Both formulations of the problem, and possible solutions, that deserve to be considered, may well make assumptions, even correct assumptions, that clash with those of some presupposed model. The role of models within wisdom-inquiry is as an aid to the critical assessment of proposed solutions, very

different from their role – or their declared role – within current knowledge-inquiry economics.

In only two places did I find economists expressing unease about the way economics combines acquiring knowledge about the economy with attempting to help change it by helping to solve economic problems. One put the matter like this. 'The supreme irony resident within economics is that we tend to adopt an official positivist methodology which presumes that the economy is independent and transcendental to man, as is in part the case with the physical and chemical world while at the same time we seek policy implications and recommendations – to make or influence policy – which presume that the economy is not independent and transcendental to man and which have the effect of contributing to the (re)production of the economy' (Samuels, 1998, p. 360). But Samuels does not recommend that economics should be changed; he concludes merely that economists should be a bit more diffident in their policy recommendations. A second expression of unease is implicit in the statement '*economic analyses and the theories which shape them are themselves weapons in the conflicts which they analyze*' (Stretton, 1999, p. 11). Stretton's book struck me as intelligent, and more aware than most of the flaws inherent in the whole idea of positivist economics, but he does not advocate or do wisdom-inquiry economics.

One book is an introduction to Marxist political economy (Onimode, 1985), but this follows Karl Marx himself in taking a thoroughly knowledge-inquiry conception of economics for granted. Onimode declares that 'Marx's objective was to discover the economic law of motion of modern society' (p. 26), and goes on to say 'Marxist political economy is the science which studies how specific systems of economic relations in given historical epochs originate, develop, function and change' (p. 27).

Another book has the title 'The Challenge of Radical Political Economy' (Sawyer, 1989), but does not go so far as to challenge the knowledge-inquiry conception of the subject. 'Radical political economy', Sawyer declares, 'is a multi-paradigm study of the economy which emphasizes features such as the distribution of income, the dynamic rather than static nature of capitalist economies, capital accumulation and the generation of uses of economic surplus' (p. 3).

Yet another book discusses the relevance of moral philosophy to economics (Vickers, 1997), but does not break with the general knowledge-inquiry conception of the subject.

Prychitko (1998) is a collection of essays entitled *Why Economists Disagree*. In it no one disagrees with the idea that economics should be concerned, primarily and in the first instance, to acquire knowledge. Frances Woolley (pp.

309-332) has some criticisms to make of neoclassical economics from a feminist perspective, but her criticisms do not challenge the knowledge-pursuing nature of economics. Thomas Weisskopf (pp. 275-305) argues for democratic market socialism, not dissimilar in one of its forms from the cooperativism I argue for here, but there is no hint of the advocacy of wisdom-inquiry economics (even though this is what he is doing, in a sense, in his article).

Three books are about environmental economics. These stress interactions between the economic system and the environment and the important role that economics can have in helping to tackle environmental problems, but are otherwise orthodox in their conception of economics. That environmental economics began at such a late date, and only, it seems, after Schumacher's (1973) belated cry of outrage about the matter, is in itself an indication of just how dangerously irrational it is to restrict economic thinking to the blinkered vision of abstract economic models, the basis of knowledge-inquiry economics.

Three books are about developmental economics. Two are orthodox, but one is more interesting. Norton and Alwang (1993) begin with the sentence 'One of the greatest challenges facing the world is to find solutions to problems of hunger and poverty in less-developed countries' (p. 3). And the book goes on to give an account of the problems faced by the poor, problems of hunger, food production, agriculture and development. We are at once in touch with the real world as it is experienced by billions today, in a way in which we are not in almost all the other books I consulted. The book goes on to outline an approach which seems to me to be the closest to that of wisdom-inquiry economics of all the introductory texts I looked at. Thus, at one point Norton and Alwang declare 'Rather than a general theory of development, what has emerged [recently] is a set of alternative strategies based on earlier theories and experience and the recognition that the developing world is far from homogeneous. Debates have begun to center around appropriate strategies for particular countries given their stage of growth, resource base, asset-ownership patterns, and institutional structures' (p. 105).

A magnificent example of wisdom-inquiry economics in practice is Professor Muhammad Yunus's creation of the Grameen Bank in Bangladesh in 1976, for which Yunus received the Nobel peace prize in 2006. The bank provides credit to the poorest of the poor, mostly women, without collateral, thus enabling them to lift themselves out the direst of poverty. In May 2006 it had 6.74 million borrowers, 97 percent of whom are women. Muhammad

Yunus is indeed, in many ways, exemplary of what someone engaging in wisdom-inquiry should seek to do.

A similar search of forty-four introductory books on sociology, published between 1985 and 2006, produced a result similar to that obtained from the economic textbooks. Sociology, typically, is defined as 'the scientific study of human society and social interactions' (Tischler, 1996, p. 4), as 'the *systematic, sceptical study of human society*' (Macionis and Plummer, 1997, p. 4), as having as its basic aim 'to understand human societies and the forces that have made them what they are' (Lenski et al., 1995, p. 5), or as involving 'the study of people – their beliefs, behaviour, interaction, institutions, and so forth' (Neuman, 2006, p. 7). Some books take issue with the idea that sociology is the *scientific* study of society, or protest at the male dominated nature of sociology (Abott and Wallace, 1990, p. 3 and p. 1). Nowhere did I find a hint of the idea that a primary task of sociology, or of social inquiry more generally, might be to help build into the fabric of social life progress-achieving methods, generalized from those of science, designed to help humanity resolve its conflicts and problems of living in more cooperatively rational ways than at present.

6 Philosophy

1983

Finally, the philosophy of knowledge has exercised a profound influence over the entire field of modern *philosophy,* from Descartes to the present. Indeed, one might almost say that the central problems of philosophy, in this tradition, have been problems posed by 'the philosophy of knowledge'. What can we know? How can we acquire knowledge – whether common-sense knowledge or scientific knowledge? How are arguments which seem to show we cannot acquire knowledge to be refuted? Of what can we be certain? What methods need to be employed in order to improve knowledge? In terms of what criteria do we assess the progress of scientific knowledge? Can philosophy provide us with a special kind of non-scientific metaphysical – or phenomenological – knowledge? What are the limits of the knowable? These epistemological, methodological and metaphysical problems – and associated problems to do with perception, causation, the relationship between the mind and the brain, knowledge of the past and of other minds – may well be held to be the central problems of philosophy since Descartes. Those thinkers generally held to have made the most substantial contributions to this tradition of philosophy are generally understood to have been centrally

preoccupied with these problems: Bacon, Descartes, Locke, Spinoza, Leibniz, Berkeley, Hume, Kant, Mill, Whewell, Bolzano, Brentano, Husserl, Mach, Bradley, McTaggart, Frege, Peirce, James, Moore, Russell, Whitehead, Poincaré, Meyerson, Cassirer, Duhem, von Mises, Campbell, Hanson, Polanyi, Lewis, Schlick, Reichenbach, Carnap. The same holds for more recent philosophers: Hempel, Nagel, Ayer, Popper, Ryle, Strawson, W. Kneale, Quine, Grünbaum, Körner, Feigl, J.J.C. Smart, Kuhn, Agassi, Lakatos, Watkins, Hesse, Harré, Shimony, Madden, Salmon, Sellers, Suppes, Hacking, Toulmin, Black, Putnam, Levi, Mackie, Quinton, Scriven, Feyerabend – and many others. There are of course exceptions. Eighteenth-century philosophers such as Voltaire, Diderot, Condorcet, Paine, Godwin and Wollstonecraft, passionately concerned to devote reason to the growth of enlightenment – to the growth of tolerance, justice, happiness, love, individual liberty, democracy – do not fit very well into the general picture of philosophy as a part of the pursuit of knowledge: their role in the history of western philosophy is appropriately downgraded. Machiavelli, Hobbes, Rousseau, Hegel, Schopenhauer, Marx, Kierkegaard, Nietzsche, Wittgenstein, Sartre, Burtt, Hayek do not perhaps, in their very different ways, entirely conform to the general pattern. On the whole, however, philosophy is centrally concerned with problems of knowledge; and even where other branches of philosophy are pursued – moral, political, aesthetic, religious or educational – nevertheless the central intellectual concern remains to make a contribution to knowledge.

All this is quite startling when one considers that 'philosophy' traditionally means 'the love of wisdom'. Modern philosophy, entirely self-consciously, stems from ancient Greece, from Socrates, his contemporaries, predecessors and successors, most notably Plato and Aristotle. For Socrates and his contemporaries, philosophy was understood to be 'the pursuit of wisdom'. However, with Plato, and increasingly with Aristotle, 'wisdom' becomes 'knowledge'. It is this aspect of ancient Greek philosophy that became prominent, in the sixteenth and seventeenth centuries, with the birth of 'natural' and 'experimental' philosophy – that is, science. This tendency is continued with the development of social science from the eighteenth century to today. Ancient Greek philosophy – pursued as the love of wisdom – is transformed, from the seventeenth century onwards, into the scientific pursuit of knowledge, and academic philosophy, increasingly, is confronted with the problem of discovering how it can make some contribution to the general pursuit of knowledge. Over the centuries, academic philosophy has seemed to become increasingly impoverished, as whole areas of 'philosophy' have departed to become respectable empirical sciences: physics, astronomy,

sociology, psychology, logic, economics, political science, cosmology, linguistics.

It is of course the central thesis of this book that all of inquiry – mathematics, natural science, social science, technology, scholarship – needs to give intellectual priority to the task of promoting the growth of wisdom in the world. In order to be of maximum intellectual rigour and value, and of maximum human value, all inquiry needs to develop, and be an institutionalization of, what Socrates did, advocated, lived and died for. The terrible human disasters of the past and present are intimately linked with the great intellectual disaster involved in developing cooperative inquiry in such a way that intellectual priority is given to the growth of *knowledge,* rather than the growth of *wisdom in life* – an intellectual disaster that can be traced back to Descartes, to Francis Bacon, and to Aristotle and Plato.

All of those of us who are in one way or another involved with modern science, technology, scholarship or education must take some share of responsibility for the persistence of this intellectual and human disaster, that is such a fundamental and pervasive aspect of the modern world. Those of us who are in one way or another involved with academic philosophy, however, bear an especially heavy burden of responsibility. For the disaster is basically *a philosophical* disaster, a persistent misrepresentation of what ought to be the basic aim and methods of all rational inquiry. It is the central professional concern of academic philosophy to develop a good generally acceptable philosophy of rational inquiry – a view as to what the basic aim and methods of inquiry ought to be. What academic philosophy ought to have done, over the decades and centuries, building on what is best in the life and work of Socrates and the Sophists, and the 'philosophies' of the Enlightenment, is to have advocated that all of inquiry needs to give intellectual priority to the growth of wisdom. Furthermore, academic philosophy should have done everything in its power to help develop all of science, technology, scholarship and education in this way. And not content with this, academic philosophy should have sought to help promote imaginative and critical discussion of aims and methods in all other human endeavours as well – politics, industry, law, the media and so on – thus helping us quite generally to put cooperative aim-oriented rationalism into practice in personal, social and institutional life, so that we may all the better realize what is of value to us as we live. From this standpoint, of course, philosophy and social inquiry are one and the same thing: just as the philosophy and the sociology of inquiry are identical, so too are economic philosophy and economic 'science', political philosophy and political 'science', social philosophy in general, and sociology, psychological philosophy and psychological 'science'. All these branches of

philosophical/social inquiry are fundamentally *methodological* in character, and need to be an integral, influential part of that aspect of life with which they deal (in order to promote aim-oriented rationality, and wisdom, in life). As a result of being developed in this way, academic philosophy would have had an important, fruitful contribution to make to modern science and scholarship, and to modern life.

Instead of all this, academic philosophy, by and large, has taken it for granted that the basic intellectual aim of all of inquiry – and of philosophy in particular – is to improve knowledge. And as a result, in part, we have failed all too often to develop traditions of improving personal, social and institutional aims and methods towards the realization of what is of most value to us in life. We have failed to develop organized inquiry in such a way that it gives intellectual priority to the task of promoting wisdom in life.

All of life, and all of inquiry, has suffered to a greater or lesser extent as a result of the intellectual failure of philosophy to give priority to the task of promoting wisdom in life. And incidentally, as it were, academic philosophy has suffered as well. For whereas as the pursuit of *wisdom* philosophy has much to offer, as the pursuit of *knowledge* philosophy is reduced to an absurdity or a triviality. In the first place, it cannot solve its fundamental problem – the various aspects of the problem of knowledge. For, in order to solve this problem, it is essential to construe the pursuit of knowledge as an aspect of *life,* an aspect of the pursuit of wisdom in life. It is essential to give intellectual priority to action, to life, and to the capacity to act more or less successfully in the world. Traditional philosophical problems of knowledge – such as the problem of induction, the problem of the rationality of science – have resisted resolution precisely because these problems cannot be resolved within the framework of the philosophy of knowledge. They are indications of the *irrationality* of this framework. Only within the framework of the philosophy of wisdom can they be resolved. In the second place, philosophy fails to make any significant contribution to knowledge. For there is no peculiarly philosophical kind of knowledge distinct from scientific or common-sense knowledge: the attempt to provide such distinct knowledge leads either to absurdities, as in Hegel, Bradley, McTaggart or early Wittgenstein, or to sterile trivialities, as in much ordinary language philosophy and conceptual analysis.[9] Furthermore, the attempt to pursue

[9] For criticism of ordinary language philosophy and conceptual analysis, see Popper (1969, vol. 2, pp. 9-21; 1959, pp. 15-23; 1963, pp. 66-96); Gellner (1959); Maxwell (1976b, pp. 31-51).

philosophy as a branch of knowledge sabotages the one thing of great value philosophy might offer – help with the rational growth of wisdom in life. Precisely in order to retain the status of a branch of knowledge, academic philosophy of morality, of politics, of art, of religion, of science, of education and so on, is obliged to draw a sharp distinction between itself, a meta-discipline, and the enterprise it studies. Thus, in this vein, Melden, introducing a textbook on moral philosophy or ethics, remarks: '. . . theoretical interest in the subject matter of ethics, whatever the conditions of its origin may be, must not be confused with the practical interest of moral beings. The theoretical interest is concerned with knowing; the practical interest is concerned with doing' (1960, p. 3). Again, Quinton, introducing a collection of papers on political philosophy, remarks: 'A comparatively definite place has now been marked out for philosophy within the total range of man's intellectual activities . . . Very briefly, philosophy has the task of classifying and analysing the terms, statement and argument of the first-order disciplines' (1968, p.l). This sharp division between 'philosophy of X' and 'X', whatever human endeavour X may be, made in the interests of philosophy being a contribution to knowledge, annihilates at a stroke the only thing of value philosophy can have to offer, namely to help build aim-oriented rationalism into X itself. For, in order to do this, it is vital that the philosophy of X – the enterprise of articulating and criticizing actual and possible aims and methods for X – must be an integral, influential part of X itself.

All of inquiry, as a result of being deemed to pursue knowledge, can be regarded as suffering from rationalistic neurosis, with all the attendant defects this state of affairs incurs, discussed in chapter 5. Rationalistic neurosis is however especially acute in academic philosophy. From the standpoint of the 'philosophy of wisdom' indeed, academic philosophy pursued as a branch of knowledge might almost be characterized as a depository of neurotic problems that arise as a result of failing to give intellectual priority quite generally to the growth of wisdom. During the course of this book I hope to show how major philosophical problems which have long resisted resolution, and which have not led to fruitful work, are either solved or are at least transformed into fruitful problems as a result of being set within the context of the philosophy of wisdom: for example, the problem of induction, the problem of free will and determinism, the problem of mind and brain, the problem of fact and value.[10]

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[10] See also Maxwell (1998; 2001; 2004b; 2005b; 2005e; 2005f; 2006a; 2007a; 2007b).

2003

Elsewhere I have said what I think needs to be said about the state of academic philosophy in recent years (Maxwell, 2001, pp. 4-5; 2004b, pp. 103-110). I restrict myself, here, to making just one remark. Despite the fact that the argument for the need for a revolution in academia has been in the public domain for 30 years (since the publication of Maxwell, 1976b, and numerous publications since), it has been almost entirely ignored by academic philosophers of whatever persuasion. The blindness of philosophers to the need to transform academia so that it takes up its proper task of seeking and promoting wisdom was, perhaps, just about excusable before 1976; the argument for the need for this transformation did not then exist. Since 1976, and even more since 1984 (the date of the publication of the first edition of this book) this excuse has no longer applied. Academic philosophers have evaded their profound intellectual and humanitarian responsibilities during the last twenty years in a way that is, quite simply, inexcusable. It is not just science, technological research, social inquiry, the humanities and education that suffer from the philosophical disaster of failing to implement wisdom-inquiry: humanity suffers as well. We live in such a profoundly unphilosophical age that it sounds slightly mad to say humanity suffers from a *philosophical* disaster. Everyone knows philosophy has no *practical* significance whatsoever – a misconception reinforced, of course, by what academic philosophers make of the discipline. I sometimes think that if ever awareness of the need to transform academic inquiry does arise, the last academic group to learn of it will be the philosophers.

Chapter Seven
Assessment of the Basic Argument

During the twentieth century humanity made extraordinary progress in scientific knowledge, and in technological and industrial development. During the same period, humanity committed horrifying crimes against itself, in that millions upon millions of people suffered and died as a result of war, tyranny, concentration camps, mass executions, economic exploitation and increasingly unjust distribution of the world's resources. A major reason for this glaring discrepancy between what has been achieved in knowledge and in life is that during the last two or three centuries – and especially during the twentieth century in the developed world – humanity succeeded only in developing socially influential organized inquiry in accordance with the philosophy of knowledge, and has thus failed to develop organized inquiry in accordance with the philosophy of wisdom. As a result, specialized knowledge has flourished, but social wisdom in the world has faltered. If we are to progress towards a wiser world it is essential that science, technology, scholarship and education in schools, universities and research establishments throughout the world be transformed to accord with the edicts of the philosophy of wisdom. If organized inquiry is developed in this way, then we may reasonably hope to make gradual progress towards a more just, humane, cooperative – and even loving – world. This, in outline, is the central argument of this book.

I put this argument forward in all seriousness, in the hope that it will be taken up and used to help change the actual institutional structure of academic inquiry, from knowledge to wisdom. My intention is to help establish a new intellectual/institutional orthodoxy in which the philosophy of wisdom is taken for granted and built into research aims and priorities, into intellectual values and ideals, into criteria for publication and acceptance, into teaching and administration, into funding, appointments and careers, in universities and research establishments throughout the world. This is a tall order indeed. What I am proposing will be fiercely resisted – or will be, much more effectively, blandly ignored – for a variety of good and bad reasons. For I am advocating nothing less than that the basic aims and methods, the whole character, of the academic enterprise be changed.

It might be thought that nothing very disturbing or threatening can come from a field that is as abstract and theoretical as the philosophy of inquiry. But this is only true as long as the philosophy of inquiry restricts itself to attempting to provide a rationale for the *status quo* – some version of the

philosophy of knowledge – as it traditionally has done. The moment the philosophy of inquiry comes up with radically new proposals as to what the basic aims and methods of inquiry ought to be, inevitably such proposals will be held by many to be threatening indeed and will meet with fierce resistance. For, as I explained in the first section of the last chapter, the philosophy of inquiry that is in practice accepted and built into the institutional structure of the academic enterprise, determines something that is of importance to everyone, but above all of immense importance, of passionate concern, to every scientist and scholar – namely what is to count as intellectual progress, what is to count as a contribution to inquiry, and what is to count as intellectually important. This is something that is of passionate concern to all scientists and scholars not only for the very noblest of reasons, but also for reasons that are somewhat less noble: scientific and scholarly reputations, membership of scientific and scholarly elites, academic careers and appointments, academic prizes and degrees, all depend on what the academic community in practice judges to constitute important, or acceptable, contributions to inquiry. Advocate a change in the basic intellectual aims and methods of inquiry, a change in the standards used to judge the intellectual importance or acceptability of contributions to inquiry, and one advocates something that threatens to annihilate established scientific and scholarly reputations, cancel the importance of lifetimes of scientific and scholarly work, alter the rules of the game whereby Nobel prizes are won, professorships are acquired, careers advanced, degrees attained. It is not to be wondered at that any proposal along these lines, however intellectually obvious and urgently needed it may be, will meet with fierce resistance.

There are other sources of resistance as well. The philosophy of knowledge is built into the habits of thought and work of countless scientists and scholars; and it is built into the institutional structure of the academic enterprise. In order to put the philosophy of wisdom into practice, it is just these habits of thought and work, and these institutional realities, which will need to be appropriately changed. But, of course, habits of thought and work, built up over a lifetime, are notoriously difficult to change, and become all the more difficult the older one becomes. Institutions, again, are notoriously difficult to change. This is especially true of academic institutions, as anyone who has attempted to bring about any institutional change in universities, however minor, will know only too well.

If I am to make any headway against this massive wall of resistance, I must formulate the basic arguments in support of the philosophy of wisdom, and against the philosophy of knowledge, in an overwhelmingly obvious,

simple, clear and decisive fashion. This I now strive to do. If, in doing this, I labour the obvious, I apologize in advance.

There are two additional points to be taken into account. The first was briefly indicated in chapter 2. Standard empiricism and the philosophy of knowledge, as a result of being already built into the institutional structure of science, and of academic inquiry more generally, tend to exclude from the intellectual domain of discussion precisely the kind of arguments developed in this book, even though these arguments are entirely valid. Thus, according to standard empiricism, essentially only empirically testable ideas can enter the intellectual domain of science. In this book I put forward the idea that this standard empiricist conception of scientific rigour is seriously defective, as a result of misrepresenting the basic aim of science: this idea is, however, not itself testable, and thus will be excluded by standard empiricism from the intellectual domain of science! In this way, standard empiricism, as a result of controlling the flow of ideas and arguments in scientific journals, texts, lectures, degree courses and so on, effectively ensures that criticisms of standard empiricism of the kind developed here, however valid, are excluded from scientific discussion. Again, according to the philosophy of knowledge, more generally, essentially only claims to knowledge, and criticisms of such claims, may enter the intellectual domain of academic discussion. But what I put forward in this book is a criticism of the intellectual enterprise of giving intellectual priority to the search for knowledge and I put forward a counter proposal, namely that intellectual priority be given to realizing what is of value in life, and to proposing and criticizing possible actions designed to help achieve this. These arguments and counter proposals are not even intended to be contributions to knowledge: they are thus of a type that the philosophy of knowledge will exclude from academic inquiry.

The second point that needs to be taken into account is this. In seeking to assess the relative merits of the two philosophies of knowledge and wisdom we seek to decide an issue of immense and general importance: what the overall aims and methods of organized inquiry are to be, and how these are to be related to the aims and methods of life. We seek to decide nothing less than the question of how in general humanity should seek to think about and tackle its common problems.

All this serves to underline the importance of assessing the arguments for and against the rival philosophies of inquiry with the very greatest care and thoroughness.

As the above outline indicates, the central argument of this book can be interpreted as solving Wootton's problem of how to apply scientific method

to social problems, so that something comparable to the extraordinary technical progressive success of natural science and technology may also be achieved in personal and social life. At the same time the argument offers a simple and general explanation as to why there has been such a glaring discrepancy between the rapid technical progress of science and technology, and the only faltering progress of humanity towards a better world.

In order to solve Wootton's problem, what we need to do is to apply directly to our problems of living appropriately generalized rational methods that have proved to be so extraordinarily successful in solving problems of knowledge in science, so that we come to solve our problems of living in the characteristically progressive way in which problems of knowledge are solved in science. This is in essence the philosophy of wisdom. Wootton makes the disastrous mistake of attempting to apply scientific method not to problems of living, but to problems of social knowledge. Rational methodology is applied not to social life but to social science. This is the basic mistake of the philosophy of knowledge. It is the long-standing persistence of this mistake which in part at least explains the glaring discrepancy between humanitarian and scientific progress. For this glaring discrepancy is due precisely to our long-standing failure to resolve in cooperatively rational ways our problems of living in the kind of way in which problems of knowledge are at present resolved in science.

It was of course the great hope of the 'philosophes' of the Enlightenment – the great hope of men like Voltaire, Diderot and Condorcet – that *scientific* progress might contain the key, the vital clue, to how humanity might achieve personal and social progress towards enlightenment (Gay, 1973). Indeed, the programme of learning from the progress of science how to achieve progress in life towards the realization of value may well be called *the Enlightenment programme.*

In order to implement this programme, however, two vital preliminary problems must be solved. First of all, a correct characterization must be given of the methodology that is actually in practice exploited by science and responsible for scientific progress. Second, this methodology must be appropriately generalized so that it becomes fruitfully applicable to all worthwhile, problematic human endeavours, and not just the one endeavour of improving knowledge. In seeking to realize what is of value to us in life there are many goals that we seek to realize besides knowledge – such as health, happiness, friendship, love, justice, cooperative creative work, and so on. The crucial generalization, then, that needs to be made to scientific methodology so that it becomes a universally applicable progress-achieving

methodology is to generalize the *aim* of the methodology, from the growth of *knowledge* to the growth of *value in life* in general.

The way to solve these two preliminary problems is spelled out in chapters 3, 4, 5 and 9. In terms of the argument of chapters 3 and 4, the progress-achieving methodology of science amounts in essence to (a) articulating, and trying to improve the articulation of, the basic problems to be solved, and (b) proposing imaginatively and assessing critically possible solutions. The way in which the philosophy-of-knowledge version of this progress-achieving methodology needs to be modified and generalized so that it becomes fruitfully applicable to all that we do (including science) was spelled out in chapter 4. In terms of the argument of chapters 5 and 9, the progress-achieving methodology in fact exploited in science with such astonishing success (even though this has rarely been understood) is *aim-oriented empiricism*. In order to become fruitfully applicable to all that we do, this needs to be generalized to become *aim-oriented rationalism*. The basic task of the Enlightenment programme is to help us cooperatively exploit problem-solving rationality, or aim-oriented rationality, in all that we do. This is the task of the diverse branches of social inquiry. As a result of implementing this Enlightenment programme, we may well expect to achieve in life a degree of progress towards what is of value to us that is comparable to the remarkable progress that has been achieved in science (in improving knowledge). In seeking to make progress towards a better life the vital lesson to be learned from science is a *methodological* lesson. What science has discovered about the world is of course important: but the *manner* in which these discoveries have been made is perhaps of even greater importance. The cooperative rational progress achieved in science at its best has much of value to teach us about how to achieve cooperative rational progress in personal and social life.

The 'philosophes' of the Enlightenment might have succeeded in clearly articulating and advocating this programme in the eighteenth century. If they had, the unprecedented seventeenth- and eighteenth-century stream of scientific progress might well have burst its banks and flowed into all human endeavours throughout the eighteenth, nineteenth and twentieth centuries, thus becoming an unprecedented flood of social, humanitarian and spiritual progress. What began as a rapid growth of knowledge would have broadened into a rapid growth of social wisdom throughout the world. The present-day discrepancy between scientific success and human failure would not exist.

But this did not happen. The 'philosophes' of the eighteenth century disastrously misunderstood the Enlightenment programme. Instead of endeavouring to apply aim-oriented rationalism to personal and social life,

thus developing social inquiry as social methodology, they sought to apply scientific method to the task of developing *social science*. And as we have seen in chapter 6, this disastrous perversion of the Enlightenment programme has persisted down to the present day. Thus, the human disasters of the twentieth, and early twenty first, centuries are due to our failure to put right an intellectual disaster of the eighteenth century.

Is this conclusion correct? Granted that we have indeed failed to put right a disastrous intellectual mistake of the eighteenth century, would correcting this mistake really have such extraordinary consequences?

At least three reasons can be given as to why it is more or less inevitable that nothing comparable to the rapid, accelerating technical progress of modern science and technology can be introduced into the rest of human life, to enable us all to make rapid human progress towards a more just, free, civilized and loving world.

First, for rapid scientific and technological progress to be made it is not necessary that we all take part: it is only necessary that relatively few, highly talented and motivated people be trained in scientific and technological research, and be given the opportunity, by funding and so on, to take part in such research. By contrast, if we are to make real human progress towards a better, more humane, more civilized world, then we all (or almost all) need to take part, the intelligent and highly motivated, and the stupid, the unmotivated, the power-mad, the careerists, the manipulative, the criminal, the hopeless and dispossessed. We cannot expect a relatively small army of paid experts to solve the world's problems for us in the same way as we may expect such an army to solve for us our scientific and technological problems.

Second, natural science and technology have the immense advantage of being able to employ the method of experimentation, of relatively uncostly trials. In these fields, we can perform experiments, and build and test material models, in order to try out our scientific and technological ideas, without our having to pay the price of widespread suffering, injury and death when our trials fail (assuming reasonable precautions are taken). There are of course limits to what we can do, and ought to seek to do, even in the domains of the physical and technological sciences, let alone the biological and medical sciences. Nevertheless, freedom to try ideas out painlessly is immense in comparison to what is possible for us in human affairs – in politics, education, industry, the media, commerce, international relations, bringing up children, our own personal lives. Here, inevitably, people are involved: to experiment, to try out possibilities here is to experiment with people's lives. Failure is not just six months' paid work down the drain: it may involve appalling suffering, wasted lives, and may be irreversible in that

human experiments, once started, may be impossible to stop even if obviously undesirable.

Third, in science and technology failure is often obvious and uncontroversial: theories are constantly refuted experimentally, and prototypes for new technology can often readily be seen to be failing to work as expected. What counts as success and failure is relatively unproblematic. In our personal and social lives, success and failure is rarely as obvious and unproblematic. Even if, in real terms – in terms of our original aspirations, or in terms of what is of real value to us – we are failing miserably, nevertheless we may all too successfully conceal from ourselves the fact of our failure – or, alternatively, in connection with political or institutional action, failure may be ignored by those with power. And not only is it more difficult to detect failure in human affairs than in science and technology: in addition what is to count as success and failure is much more problematic, and differs from person to person, and from group to group.

In short, we cannot reasonably expect to be able to learn from our mistakes in life in anything like the rapid, progressive way in which we learn from mistakes in science and technology – partly because in life we cannot hire clever experts to do our thinking for us, partly because in life we cannot deliberately and painlessly make lots of mistakes from which to learn, and partly because in life mistakes are often difficult to detect and agree about.

However, the fact that learning and progress in human life are inevitably more difficult and problematic than narrow intellectual and technical learning and progress in science and technology, as these three considerations indicate, does not provide us with any good reason for not attempting to introduce into life the progress-achieving methodologies already so successfully exploited in science. It is all the other way round. Just because especially severe difficulties do arise in putting into practice progress-achieving methodologies in life, the whole endeavour deserves all our attention and care. Thus, for example, sustained attention needs to be given to the multitude of problems generated by the following basic question: How can we build into our political life and institutions methodological principles designed to help us discover and achieve generally desirable personal and social or political objectives, such as freedom, justice, prosperity, opportunities for cooperative work and endeavour, the capacity to resolve conflicts in just and mutually beneficial ways? And quite generally, sustained attention needs to be given to the multitude of problems that arise in connection with analogous questions to be asked about all the other aspects of our personal and communal lives. In doing this we must of course take into account that we are a mixture of the dedicated and idealistic, the ambitious and

unscrupulous, the intelligent and stupid, noble and criminal, rich and poor, well and ill. It will be especially important to develop education for everybody in the form of discovering how to put into practice and develop progress-achieving methodologies in life, in diverse personal and interpersonal pursuits, so that we may realize what is of value to us. Just because we cannot experiment in personal and political life in the free way in which we can in scientific and technological research, it becomes all the more important that we learn all we can from the variety of actions people do perform, and have performed, in attempting to resolve problems of living; and it becomes all the more important that we create vividly and accurately imagined trials, possible deeds, so that we may learn from what we imagine ourselves doing and not doing, in addition to learning from what we actually do. Just because what is of value in life is problematic, and different for different people and different groups of people, success and failure in life being problematic and diverse, all the more do we need to attend, imaginatively and critically, to questions about what kind of success we really do want to achieve, what kind of failure we want to avoid. And just because failure is often difficult to recognize in life – painful to acknowledge – we need to give sustained attention to the task of developing traditions and habits that help us to recognize and acknowledge failure when it happens (most of the time!).

In brief, we only have a reasonable chance of successfully and progressively realizing what is of value in all the diverse pursuits and aspects of life if we inherit and can make use of a tradition of organized inquiry and education that gives absolute priority to the tasks of developing and helping us to put into practice methodologies designed to enable us to achieve such success and progress in life.

At this point it may be conceded that having a tradition of rational inquiry devoted to the growth of wisdom is a *necessary* condition for developing a wiser world; and yet the importance of trying to develop such a tradition of inquiry within research and educational institutions may nevertheless be denied. In support of this denial, the following arguments may be produced: (1) social factors external to universities and schools would inevitably make it impossible to put the philosophy of wisdom into practice: it would never be permitted by governments, public opinion, religious authorities; (2) internal factors would make it impossible to put the philosophy of wisdom into practice: scientists, scholars, teachers, administrators would never be able to agree sufficiently amongst themselves about political, moral, social, religious or ideological issues for a good enough consensus to develop to make

cooperative, rational, intellectually productive exploration of personal and social problems of living a possibility; (3) even if it did prove possible to put the philosophy of wisdom into practice intelligently and fruitfully in universities and schools, this would nevertheless have only a negligible influence on the rest of the world, as good proposals for action emerging from universities would be systematically ignored; (4) there is no real need to put the philosophy of wisdom into practice in universities since there are no substantial intellectual problems about what we need to do in order to solve our major social problems. What we need to do is obvious; the problem is the political one of persuading others of the need to do the obvious (sometimes against their own short-term interests).

My reply to these arguments is as follows. (1) In many places in the world political and religious power may well at present make it impossible to put the philosophy of wisdom into practice in universities and schools. The extent to which the thing can be done in secular, democratic nations can only be discovered by trying. (2) Granted that external pressures do not prohibit putting the philosophy of wisdom into practice, it may well be difficult to develop and sustain traditions of tolerance, of cooperative, open and critical discussion, of learning from opponents, about potent political, moral and ideological issues, within universities and schools, so essential for putting the philosophy of wisdom into practice. It is not always easy to imagine schools and universities in which Marxists and Tories discuss political issues together, intelligently and with a common good will. It is all too easy to imagine schools and universities being taken over by some one ideological, political or religious doctrine, or by some one powerful group with its own special interests, by means of an appropriate policy of appointments and redundancies, operating behind a mask, perhaps, of 'open, critical, pluralistic inquiry'. The internal difficulties confronting the task of developing a genuinely rational kind of inquiry (devoted to the growth of wisdom) are considerable even in democratic societies, but not, I maintain, insurmountable. As I indicated in chapter 5, general acceptance and implementation of aim-oriented rationality would greatly help those with conflicting aims and ideals to resolve their differences in fruitful and cooperative ways. By representing problematic and contested aims as a hierarchy which becomes increasingly unproblematic and uncontested as one goes up the hierarchy, aim-oriented rationality makes it possible for those with conflicting aims and ideals to discover common, agreed aims within whose framework more specific disagreements may progressively be resolved. (3) It is entirely proper that universities should have influence but not power, or just sufficient power to retain independence (otherwise one has a modern

version of Plato's Republic). Inevitably many valuable proposals and criticisms emerging from universities (devoting reason to the growth of wisdom) will be ignored or rejected by society, by the political and economic system (an inevitable consequence of lack of power). Nevertheless, the existence of a vocal, active tradition of rational discussion of social problems can still profoundly influence thought, policy and action in the broader social world – via education, lectures, books, articles available and understandable to non-academics, journalism, the multitude of formal and informal points of contact that exist between universities and society. The activity of articulating and exploring proposals for action in public can in itself help make possible social action that would otherwise be impossible. A society in which there is a tradition of rational discussion of its problems has open to it all sorts of desirable possibilities – in particular the possibility of democratic, non-authoritarian, cooperative action – not open to a society in which there is no such tradition. (4) It is admittedly often held that no problems arise as to what needs to be done in order to solve our social problems, realize what is of value in life, all the problems having to do with persuading or forcing others to act appropriately. Amongst those who hold this view, however, one finds an incredible diversity of views about what does need to be done in order to resolve our social problems. When one takes into account the immense complexity of these problems, the almost inevitable capacity of social action to have all sorts of unforeseen consequences, the immense diversity of character, circumstances, capacities, aspirations, desires and fears of people in society, it is difficult not to regard the idea that social problems have obvious solutions as utterly idiotic, an absurdity scarcely worth mentioning were it not for the fact that it is such a widely held and dangerous illusion. Perhaps one should rather say that any adequate solution to a social problem, requiring many people to act cooperatively, must almost inevitably be of such complexity, requiring such a diversity of actions from those who participate, that no one person can hope to have anything but the vaguest notion of what the 'solution', the 'action' amounts to. Human life is so rich and diverse that even our own experiences and actions are beyond our full comprehension, let alone those of many people taken together. One important initial contribution that inquiry, pursued in accordance with the philosophy of wisdom, would be in a position to make, would be just to render commonplace the Socratic idea that we are all more or less ignorant of what is of most value in life and of how it is to be realized, learning in this domain being both possible and supremely desirable (an idea that is at present almost a commonplace within science with respect to knowledge).

In order to highlight the difference in *method* advocated by the philosophies of knowledge and wisdom for social inquiry, let us consider briefly the contrasting approaches to the following somewhat humdrum social problem: the progressive dereliction of city centers (an issue very much in the news when this book was first published in 1984).

A social scientist taking the philosophy of knowledge for granted, but anxious nevertheless to make some kind of contribution to our problem, will proceed more or less along the following lines. Armed with some kind of provisional understanding of the social problem in question (without which one would not be in a position to proceed at all) he will seek to gather social data which he deems to be in some way relevant to the problem. If he is relatively sophisticated methodologically, he may well put forward a conjecture designed to explain the progressive dereliction of city centres, which he then proceeds to attempt to refute or corroborate empirically. He may even attempt to gather data designed to decide between two conflicting theories. He may make a comparative study of two cities, one of which only is deemed to be progressively decaying. In any case, he will gather data, of a statistical character, by carrying out a survey, getting a randomly chosen sample of the population to fill in carefully prepared questionnaires. He will arrive at certain empirical conclusions, which may include a corroborated theoretical explanation of the social, cultural, economic, legislative factors which cause decay of city centres, but he will not come up with a proposal as to how the problem can be solved. His task is to solve a sociological problem – a problem of knowledge and understanding – not a social problem. Apart from the obvious criticism that such a social scientist does not even attempt to help solve the basic social problem, there is a further serious criticism to be made. The whole way in which the social scientist proceeds, the data he seeks to gather, the empirical theories he is prepared to consider, will be profoundly affected by his initial understanding of the underlying social problem, the way he formulates it, the kind of policy-measures he is prepared to consider as reasonable. And yet, just because such a social scientist seeks to solve a *sociological* problem, not a social problem, no explicit analyses or discussion of the underlying social problem is likely to be given. Implicit assumptions concerning human, social priorities and political options may well profoundly affect the kind of data that the social scientist seeks to gather: and yet these assumptions will not be explicitly articulated and critically assessed. As a result, the data that the social scientist gathers may well only be of interest to those who agree with the way the underlying social problem has been understood. Even worse, presenting the data as objective, value-neutral, politically-neutral, empirical results, when in fact value-judgements

and political judgements are bound to be implicit in decisions as to what sort of data are significant and relevant, will have the effect of influencing the reader of the eventual report to accept uncritically the underlying understanding of the social, political problem. Implicit, covert presuppositions are always much harder to challenge and resist than explicit assumptions. Thus the social scientist's work has the effect of obstructing the one thing that ought to be promoted – explicit, critical articulation and analysis of the underlying social, political problem.

Finally, of course, the social scientist's results can only aid manipulative social, political action. As a result of discovering that people are influenced to act in such and such ways by such and such factors, one basis is provided for enacting new legislation, for example, designed to influence the people involved to act differently.

A social inquirer who approaches the problem from the perspective of the philosophy of wisdom will proceed in an entirely different way. His approach will be much more like that of a good journalist than that of an orthodox academic social scientist.[1] His basic task is not to improve empirical knowledge of social phenomena at all:[2] rather it is to engage in, and to help promote, rational approaches to solving the basic social problem.

It is important to recognize that a major problem that confronts any attempt to resolve the social problem in a cooperatively rational fashion arises simply from the number of people that are involved. If an analogous problem confronted a tribal village, in that the centre of the village was suffering from progressive decay, it would always be possible to hold a meeting, which all members of the village could attend, at which the problem could be discussed, and an agreed policy be decided upon. Modern cities

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[1] It is my impression that much of the best work produced in the field of social inquiry is indeed produced by writers who proceed as good journalists – such as Daniel S. Greenberg, Anthony Sampson, Tony Parker, Richard Barnet, Ronald Blythe.

[2] I am not, of course, arguing that social inquiry should not seek to acquire factual knowledge at all. Rather, I am arguing that factual knowledge should be sought as a secondary, subordinate intellectual task, both within social inquiry, and within inquiry as a whole, subordinate to the fundamental intellectual tasks of articulating problems of living, proposing and criticizing possible solutions, possible actions designed to help us realize what is of value in life. In doing this, we make use of, and improve as needed, our already possessed highly sophisticated knowledge and understanding of ourselves, each other, institutions, social structure, the material world. As long as intellectual priority is given to the promotion of rational, cooperative problem-solving, the development of predictive knowledge of human behaviour is not harmful, and may well be beneficial – insofar as such knowledge is used to promote cooperative problem-solving in life.

cannot cope with their problems in an analogous fashion. An important long-term problem is precisely to develop institutional machinery which enables us, as far as possible, to overcome this obstacle to rational, cooperative social problem solving.

In the absence of a solution to this long-term problem, our social inquirer must do the best that he can. His task will be to enlist the help of some of those involved in the problem, in one way or another, in an attempt to improve his understanding of the problem, and improve his ideas as to what policies might be developed which, if put into practice, would help solve the problem. His concern will be to provoke people into putting forward suggestions, proposals, and into criticizing the suggestions and proposals of others. In addition he will himself discuss and criticize the ideas of the people he interviews. It will be essential for him to interview people involved in the problem in different ways: house owners, flat dwellers, property developers, businessmen, government officials, politicians, social workers, the police, pressure groups, shopkeepers. His task will be to probe beneath rhetoric to underlying aims, actions and motivations. And finally, in writing up his report, his concern will be to leave a record of his attempt to find a possible solution to the problem, publishing the ideas, arguments and responses of those he interviews, as well as an account of their actions. He will be concerned to make his report as clear, and interesting to read as possible, and have it published in a generally available form, so that it in turn may stimulate more enlightened public discussion of the issues involved.

The result of the failure of social inquiry to give intellectual priority to the task of promoting and sustaining cooperative, rational, problem-solving in the world is of course that such problem-solving fails lamentably to flourish. Consider, for example, the extent to which we succeed at present in tackling our economic problems in a cooperatively rational fashion.

At present the economic system that prevails in free market democracies is such that most adults receive treatment appropriate to children. There is, for most, no opportunity to take part in a jointly-owned, cooperative venture, where ownership, management, risk and responsibility are shared by all those who work for the venture. Instead, most people work for a wage, as instructed, without responsibility, part-ownership, or managerial influence, for the profit of the employer or share-holders. If indifferent work, unrealistic wage claims and strikes result, this should occasion no surprise. Treated by the system as an irresponsible child, it is scarcely surprising if one responds in kind.

One exception to all this is the extraordinarily successful cooperative movement of Mondragon in Spain. And the striking fact about this

movement is that it began with some practical (aim-oriented rationalistic, philosophy of wisdom) economic inquiry initiated by a priest, Jose Maria Arizmendi. As a result of his own research into earlier cooperative movements – such as that of Robert Owen – and as a result of discussions held during adult educational courses, the first Mondragon workers' cooperative, ULGOR (a manufacturer of domestic appliances) started up in 1956 with twenty-three people. In 1984, when the first edition of this book was published, there were over sixty cooperatives in the region employing some 15,000 people. Since then, the movement has gone from strength to strength. In the 1980's, the various companies joined together as the Mondragon Cooperative Corporation – now the Basque Country's largest corporation, and the seventh largest in Spain, the largest worker cooperative in the world. Here is an astounding example of just how socially efficacious and fruitful economic inquiry (and social inquiry more generally) of the appropriate kind can be.[3] If an amateur economist working amongst illiterate peasants in a severely repressed economy and a fascist dictatorship could help to bring about such profound changes, is it too much to ask similar things of our army of professional economists in educated and democratic Britain, let us say, or Europe, or the USA? Unfortunately, it almost certainly is too much to ask, as long as economists give priority to economic science, to the task of improving academic economic knowledge, as opposed to offering help with those practical economic problems that we need to solve in order that our lives be genuinely enriched.[4]

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[3] In chapter 6, towards the end of section 5 on social inquiry, I considered very briefly the work of another philosophy-of-wisdom economist, namely that of Professor Muhammad Yunus in creating the Grameen Bank in Bangladesh in 1976.

[4] Some work on cooperatives and industrial democracy has however been done by some writers in recent years. See, for example, Oakeshott (1978); Elliot (1978); Thornley (1981). For more recent works on cooperatives see, for example, Restakis and Lindquist (2001); Merrett and Walzer (2001); Fairbairn and Russell (2004). For the Mondragon cooperative movement see: MacLeod (1997); Cheney (2002). For a critical look at Mondragon see Kasmir (1996).

Chapter Eight
Objections to the Philosophy of Wisdom

Despite the arguments of the previous chapters, there may be some who wish to hold on to the view that the basic intellectual aim of inquiry should be to improve knowledge and not wisdom. The following arguments may be given in support of the philosophy of knowledge and against the philosophy of wisdom. (These are, as it were, hypothetical objections. In chapter 13 I respond to criticisms that were actually made to the first edition of the book.)

1. All rational inquiry, all rational thought has, as its aim, to establish or improve knowledge. Even in the context of action, the rational component of thought is devoted to establishing knowledge of the truth or falsity of various conditional propositions such as 'if X is performed, Y will result' or 'in order to realize A in the easiest and quickest way, B must be performed'.

2. Some reasons have previously been given for holding that, in order to give science a good chance of serving humanity, discussion concerning the aims and priorities of research, and the social use of results of research, need to be rationally related to discussion of social problems of living. This does not however in any way undermine the central tenet of standard empiricism and the philosophy of knowledge which asserts only that the *results* of research, namely claims to knowledge, must be assessed with respect to truth alone, independently of all consideration of aims of science and of life, human needs, desires, feelings, objectives, values.

3. The scientific pursuit of knowledge may be undertaken merely in order to acquire knowledge, and not at all in order to help promote human welfare. In this case, all the arguments against divorcing scientific problems from social problems – against standard empiricism and the philosophy of knowledge – become irrelevant.

4. The philosophy of knowledge is only vulnerable to criticism because it has been put forward in a grotesquely inflated form as a theory of *all* of rational inquiry. Reinterpreted more modestly, as a theory only of that part of inquiry that is devoted to the acquisition of knowledge, it becomes entirely acceptable, and immune to the criticisms that have been levelled against it.

5. It is essential that universities restrict themselves to devoting inquiry to the acquisition and improvement of knowledge. By doing this, universities can perform a vital service to the community, and indeed to humanity, while at the same time having some chance of retaining their intellectual independence from government. The moment universities adopt as their official intellectual aim to help develop a more rational world they must lay themselves open to charges of political, religious, moral and ideological bias, given the wide range of interpretations that the idea of a 'rational society' is open to. Universities risk becoming dominated by government, and may well become the slaves of political, religious, moral or ideological dogma, or the servants of those with power and money in the community; or they may become a battleground of sterile controversy between rival factions within universities, the vital, more modest task of improving knowledge being neglected. The enterprise of helping to develop a more rational, cooperative and humane world is a vital one: but it is a political or moral enterprise, which needs to be pursued in the world, outside the groves of academe. The enterprise of helping to develop a more rational, a wiser world, conducted within universities, can have little impact on the world itself, and only a destructive impact on proper university activities having to do with scientific research, scholarship and education.

6. Acquisition of relevant knowledge is an essential prerequisite for the rational tackling of problems of action, problems of living. Far from action, and problems of action, being intellectually more fundamental than knowledge and problems of knowledge, as the philosophy of wisdom maintains, it is all the other way round. Acquisition of knowledge is intellectually prior to and more fundamental than the rational tackling of problems of action. Without knowledge, action becomes impossible. Without knowledge it even becomes impossible to imagine possible actions, and thus to engage in the rational tackling of problems of action. Thus, the central assumption – the whole basis – of the philosophy of wisdom is untenable, and is to be rejected.

7. The philosophy of wisdom – with its emphasis on solving practical problems of living – cannot do justice to the intellectual value of inquiry, the value of inquiry pursued for its own sake without ulterior motive. Only the philosophy of knowledge can do justice to this aspect of inquiry.

8. The philosophy of wisdom advocates a species of 'Utopian social engineering' – a kind of social planning that has been decisively criticized by Popper.

9. The philosophy of wisdom, in committing inquiry to *cooperative* social problem-solving, commits inquiry to a programme of action that constitutes a massive infringement of individual liberty. The philosophy of wisdom is thus to be opposed in order to protect individual liberty from being drowned in an ocean of 'cooperativeness'.

My reply to these counter doctrines and arguments is as follows:

Reply to objection 1

In order to tackle our problems of living rationally, we need at the very least, according to the philosophy of wisdom, to engage in the intellectual activity of imagining and criticizing possible actions from the standpoint of their capacity to solve our problems. This involves, but is certainly not equivalent to, considering propositions of the form 'if X is performed, Y will result'. The intellectual excellence of our thinking is to be judged in terms of how good our imagined actions are, and how well assessed, from the standpoint of their capacity, if enacted, to solve our problems in such a way that we realize what is of value to us. For intellectual excellence, it is essential that propositions considered of the form 'if X is performed, Y will result' are not only *true,* but are also *relevant,* conducive to a good resolution of the problem in hand. Thus, according to the philosophy of wisdom, for inquiry to be rational, it is vital that it is not reduced merely to the consideration of claims to knowledge of the type 'if X is performed, Y will result'. To this one might add that in order to be rational, the intellectual activity of imaginatively exploring possible actions needs to be linked to and motivated by the desire and the capacity to *act,* when a good action is discovered: otherwise cogitation will be in vain. Rationality demands, in other words, that thought and action be interlinked in a certain way, whereas of course the philosophy-of-knowledge conception of reason demands that inquiry be divorced from action if inquiry is to be rational!

Reply to objections 2, 3 and 4

These objections claim, in various ways, that standard empiricism and the philosophy of knowledge are acceptable as long as they are interpreted sufficiently modestly as doctrines about how to acquire knowledge only, with no import as to how knowledge is to be applied so as to promote human

welfare. At least three arguments have, however, already been developed against such 'modest' versions and defences of the philosophy of knowledge, (a) The philosophy of knowledge misrepresents the basic intellectual aim of science, in that it fails to do justice to the search for *understanding,* (b) In addition, it fails to do justice to the aim of improving knowledge of *valuable truth.* Values inevitably, and quite properly, exercise a major influence over the intellectual domain of science, over estimations of scientific progress. Standard empiricism and the philosophy of knowledge, in banishing discussion of untestable ideas, and conjectures and problems about what is of value, from the intellectual domain of science, serve to undermine the intellectual rigour and success of science, (c) Whatever else it may be, science is an expensive and influential human enterprise: it needs therefore to be assessed as a human enterprise, a social or institutional activity with typical social or institutional aims, achievements and problems. In insisting that science be conceived and assessed in exclusively intellectual terms, the philosophy of knowledge illegitimately deflects valid criticism, of a social and moral character, away from science. (Arguments against an even more 'modest' version of the philosophy of knowledge are developed in the next chapter.)

Reply to objection 5

As long as the enterprise of improving knowledge can be conducted rationally when intellectually and institutionally dissociated from rational discussion of social problems of living, a defence of the view that universities ought to restrict themselves to improving knowledge is at least possible. But the arguments of chapters 3-5 have shown that the pursuit of knowledge must in important respects cease to be rational, if dissociated from concern with social problems. Hence the above view becomes indefensible. It may well be that attempting to put the philosophy of wisdom into practice in universities faces greater dangers and difficulties than putting into practice the philosophy of knowledge. This does not in itself constitute sufficient grounds for not making the attempt. In the past men like Bruno, Copernicus, Kepler, Galileo, Descartes might well have decided that the dangers and difficulties associated with the rational pursuit of knowledge of nature were too great for the thing to be attempted: and as a result, no doubt, modern science as it is today would not have come to be.

Reply to objection 6

The argument that the acquisition of relevant knowledge must precede, and be intellectually more fundamental than, rational action is perhaps the

central argument in support of the philosophy of knowledge. Widespread conviction in the validity of this argument is perhaps responsible, more than any other intellectual factor, for the persistent domination of the philosophy of knowledge in academic institutions. I therefore now devote some space to demonstrating that this argument is invalid, and that, quite to the contrary, the philosophy of wisdom is absolutely correct in insisting that more or less successful action in the world, and rational tackling of problems of action, are prior to knowledge. I have four main points to make.

1. Even if the objection were valid, this would still not undermine a central contention of this book, namely that rational inquiry devoted to promoting human welfare must devote much attention to articulating problems of living, proposing and criticizing possible solutions, this being intellectually integrated with science, the pursuit of knowledge.

2. The validity of the objection becomes extremely doubtful when one reflects on the extraordinary extent to which practical problems have been successfully solved in the past in a state of extreme ignorance – and when one reflects on the inevitability of ignorance. Endlessly many examples can be cited – from social life, from technology, and from medicine – of problems of action being successfully solved in the absence of what can only be regarded as relevant knowledge. Much of the basic technology possessed by mankind, upon which the modern world is founded, was developed by primitive man in prehistory, long before adequate theoretical knowledge and understanding of the relevant phenomena had been developed. Hunting, agriculture, irrigation, husbandry, fire, cooking, smelting, metalwork, pottery, clothing, building, transport, medicine: basic discoveries in all these fields were made by people who were convinced that the natural world is animated by gods, and who thus severely lacked relevant knowledge. And indeed, not only does technology come before science: the subsequent development of science is scarcely conceivable without the prior development of some basic technology. Even after much scientific knowledge has been accumulated, technological discoveries have continued to be made before adequate relevant theoretical knowledge and understanding has been achieved, especially perhaps in medicine. And even today we have every reason to believe we are still profoundly ignorant of the nature of the ultimate constituents of the world, of the fundamental laws of nature, and of how our brains function: in a sense our whole life is conducted within an ocean of ignorance about the world, ourselves, our immediate environment. Our capacity to acquire relevant knowledge before we act must inevitably be

severely limited – and even the process of acquiring knowledge itself requires that we act in a state of ignorance.

3. This objection is not valid. In order for rational tackling of life problems to become possible it is not knowledge that we need so much as *conjectures,* and the capacity to learn. We can then acquire knowledge about our environment and ourselves as needed, as we tackle our problems of living. Proposing and criticizing possible actions in the absence of knowledge *is* possible, and is in fact essential for rationality. Indeed, in practice, we are almost all the time obliged to tackle our life problems in a profoundly ignorant state about all sorts of matters of possible relevance to our actions – for example, the intentions, the future actions, of other people. It would actually be appallingly irrational to attempt to acquire knowledge of all relevant factors before tackling problems of action – simply because the endeavour would be never-ending, the prescription thus leading to complete paralysis. Indeed, if Popper is correct in holding that, strictly, we cannot have knowledge at all, but at best only well-tested conjectures, to wait for knowledge before one is prepared to act is simply to cease to act forever.

This argument can be put in a particularly forceful way as follows. There is scarcely any part of our environment which exercises such a profound influence over our conduct as our own brains. Therefore, if we take seriously the principle that knowledge of relevant factors must first be acquired before rational action becomes possible, we must first acquire knowledge of our brains before we act (if we are to be rational). Such advice is clearly absolutely disastrous, since we still do not really know how brains work even in broad outline, and probably cannot ever know, even in principle, for logical reasons, what is going on in detail in our own brains. Furthermore, if we were to take the advice seriously we would actually forego forever the possibility of improving our neurological knowledge, since neurology, like the rest of science, can only be pursued if we can act successfully in the world – a strand of the argument to be developed below.

But the argument needs to be developed even more forcefully than this. It is not just that in practical contexts rational action always proceeds in a state of enduring ignorance. Even when our prime concern is to improve knowledge, to do science, nevertheless priority still ought to be given to practical problems of action, to questions of what we want to do, what we want to achieve: for only in this case can we be in a position to know what new knowledge and technology it is relevant for us to try to develop, in order to make possible, or reveal the impossibility or undesirability of, proposed actions.

The argument here is essentially analogous to Popper's argument in support of hypothetico-deductivism as opposed to that version of inductivism which holds that evidence must first be accumulated before sound scientific theorizing can begin. Popper argues, in effect, that it is only if we give intellectual priority to articulating and criticizing *theories* in the natural sciences that we can know what observations and experiments it is relevant to make in order to test our theories. Accumulating evidence without prior theorizing leads only to a mass of trivial, useless results (Popper, 1959, pp. 106-8). My point is essentially analogous to this. It is only if we begin with proposals for action that we can know what scientific knowledge and technology it is relevant to try to develop in order to assess critically, or implement, these proposals. The pursuit of knowledge and technology without prior critical thought about what it is that we want to achieve is likely to lead to a mass of trivial, useless results when judged from the standpoint of achieving what is most humanly desirable. Both points are applications of the elementary point that rationality demands giving absolute intellectual priority to articulating and criticizing possible solutions to the problems to be solved. Popper, concerned primarily with inquiry devoted to solving theoretical problems in the natural sciences, stresses the intellectual priority of proposing and criticizing *theories* – possible solutions to these problems. I, concerned primarily with inquiry devoted to helping to solve problems of living, stress the intellectual priority of proposing and criticizing possible and actual *actions* – possible solutions to these problems. Consideration of our life problems, in short, constitutes a proper rational spur for the development of relevant knowledge and technology.

4 But in addition to this, the argument (that knowledge must first be possessed before rational action becomes possible) is wrong in an even more radical way – if by 'knowledge' is meant explicit propositional knowledge, rather than merely the capacity to act successfully in the world. In fact, I wish to argue, the thing is all the other way round. It is only insofar as we can act successfully in the world, and can propose and assess possible actions, that we can be in a position to possess or acquire explicit propositional knowledge. Action, and the ability to imagine oneself performing possible actions, must first come into the world before there can be explicit propositional knowledge. 'Knowledge', in this sense, is a development of, and not a prerequisite for, rational action.

I suggest the evolution of thought and knowledge in the world need to be conceived of in the following terms. To begin with there develops the capacity to act successfully in the world, the capacity to realize life-goals, solve problems of action. Fish, beetles, ants and spiders must be able to eat,

to avoid being eaten, and to fertilize eggs or mate, in order to reproduce. The ability to reproduce depends crucially and fundamentally on the ability to realize successfully such goals, solve successfully such problems of action. Natural selection operates primarily on this ability. But at this primitive level there is no conscious thought or knowledge. Insofar as primitive life forms can be said to possess implicit knowledge of the environment and to possess the capacity to acquire such knowledge, this must be understood in terms of the ability to act, to *realize* life-goals, to solve in practice problems of action.

At the most primitive level, an organism's repertoire of possible actions is largely genetically determined. However, even at the most primitive level, there must be some flexibility in action, in that action is a product of genetic determination and environment, and the environment varies with time and place. At a somewhat higher level there is learning, a flexible rather than fixed repertoire of actions plus the ability to learn being genetically determined. Animal learning is, however, essentially learning how to act more successfully. Even perceptual learning about the environment – which seems somewhat like the acquisition of knowledge – must be understood as an aspect of action, an aspect of solving problems of action. Enhanced powers of perception have survival value insofar as they enhance the ability to act successfully. At a still higher level there is imitative, social or cultural learning rather than only learning at the individual level: lion cubs learn hunting skills through play, through practice on half dead animals caught by parents for that purpose, and through imitating adult lions while hunting. The schools and academies of Nature are firmly based on the assumption that the purpose of education is to learn how to act, to live, to solve problems of living.

At a still more sophisticated level, there develops the capacity to *imagine* that actions are being performed. A lion hunting a zebra, for example, imagines various possible lines of action: various possible routes to the zebra are rehearsed in the imagination. Consider two apes confronted by the kind of problem with which Köhler once used to plague apes: bananas hanging out of reach can only be obtained by piling three boxes on top of each other. The first ape, let us suppose, tackles the problem by trying out all sorts of more or less unsuccessful actions until eventually the problem is solved. The second ape, at the other extreme, tries out all sorts of more or less unsuccessful actions *in its imagination* until the problem is eventually solved, and the ape swiftly puts the solution into practice. This second way of solving the problem seems on the face of it much more impressive as an intellectual performance than the first way: we may be misled, like Kohler and others, into invoking some mysterious mental act of insight to explain

the apparently sudden emergence of the completed solution. The greater apparent intelligence or rationality of the second ape is, however, in a sense deceptive: it is due simply to the fact that the second ape blunders about stupidly in the privacy of its imagination, thus rendering its nearly random, stupid efforts to solve the problem invisible to our eyes. There is thus no need to appeal to 'insight' here – or rather 'insight' can be fully explained by the assumption that the second ape is able to learn from what it does in its imagination while the first ape is restricted to learning from what it does in reality.[1]

The extent to which animals are in fact capable of this kind of imaginative problem-solving is at present no doubt a controversial issue. At this point I wish only to stress the following points. Being able to try out possible solutions to problems of action in the imagination – as opposed to being restricted to trying them out in practice – clearly has great potential survival value, especially for hunting. Thus there is no problem in understanding why natural selection should favour the development of such an ability. The hypothesis that the higher animals are capable of imagining actions, at least to some extent, provides an explanation for the otherwise puzzling phenomenon of dreaming. The evolutionary advantage of dreaming is generally conceded to be problematic. From the standpoint of the present hypothesis, however, dreaming may be understood as Nature's way of enabling an animal to develop its ability to imagine it is performing actions not actually being performed: this, after all, is the crucial feature of dreaming. This hypothesis thus explains why dreaming does have survival value. The fact that only the higher animals appear to dream supports the hypothesis – in that one would expect the ability to imagine actions to develop only with the higher animals.[2] Finally, the ability to imagine one is performing actions that one is not performing must be understood as a development of the prior ability to act. In imagining that it performs certain actions, an animal in effect arranges to have occur in its brain neurological processes that in certain relevant respects resemble those neurological processes that would occur in its brain were it actually performing the imagined actions. Both the potential survival value, and the meaning, of a process of imaginative thought, of the kind being considered here, require that imaginative thought be interpreted

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[1] See Köhler (1927) for his own account of insight in apes.

[2] The theory of dreaming proposed here is diametrically opposed to Sagan's theory, according to which the dream-state involves the activation of primitive systems in the brain, inherited from our reptilian past, which are repressed when we are awake: see Sagan (1978, pp. 149-51).

as the occurrence of inner processes analogous to inner processes that would be involved in the control of the imagined actions, were these actions actually to be performed. These inner processes are, we may legitimately conjecture, brain processes. To imagine, to think and to dream is to act with action suspended: what is being done makes essential reference to action, and to the prior capacity for action. Thus the ability to imagine, to think, must be understood as a development of the ability to act. The theory of imagination just outlined might be called the 'suspended action' theory of imagination, of thought.[3]

At the neurological level, the ability to imagine action can perhaps be understood as a development of the particular way in which action is neurologically programmed and controlled in order to facilitate learning.

Complicated actions performed by a spider, for example, in making a web, are, we may suppose, specified and controlled neurologically step by step. Nothing prompts the spider to engage in *general* web-making behaviour, and nor is the outcome dependent on prior web-making behaviour, on what has been learned. On the contrary, the spider is induced to perform a specific sequence of actions which result in the construction of a web of a certain definite structure which is neurologically, and ultimately genetically, predetermined. Variations in webs built by spiders of the same species are to be explained in terms of variations of the environment, and the accidents of construction.

A kitten, however, is induced to engage in hunting behaviour in a quite different way: there is an impulse, a desire, to act out general hunting behaviour, the actual performance thus depending crucially on prior practice. From a neurological standpoint, we may suppose that something in the kitten's brain induces neurological processes to occur which are somewhat analogous to those which occur when the kitten crouches and pounces. The kitten begins to imagine, to dream while waking, that it is hunting, and thus is induced, in a flexible way, to act out what is imagined. The imagination, on this view, is an internal arena within which desired actions are performed, thus inducing actual actions in a flexible fashion, in a fashion which facilitates

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[3] This 'suspended action' theory of imagination has subsequently received considerable support from observations made with new brain imaging techniques that became available during the 1990's (Carter, 1998). It has been discovered that when subjects are asked to perform specific tasks of imagination that involve colour, motion, or the form of some known object, areas in the visual cortex become active that correspond to those that would be active were the subject actually seeing what he imagines he is seeing (see Posner, 1993).

learning. The capacity to imagine actions is, on this view, a development of action being neurologically controlled in a way which facilitates learning, by means of desire, motive or instinct which flexibly, rather than precisely, determine action.

Be this as it may, the point that I wish to stress is that the development of the ability to imagine action – itself a development of the prior ability to act successfully in the world – is the crucial step in the development of thought, understanding of others and oneself, self-awareness, rational problem-solving (as defined above), propositional knowledge and science.[4]

Being able to imagine action makes possible an enormous extension in space and time of the context, the environment, in which an animal or person acts – in that possible actions at distant times and places can be imagined or conceived of. Long-term imaginative planning becomes possible – and also (of fundamental importance for the shaping of humanity, the shaping of the human predicament) realization of the inevitability of death. In addition, understanding of others becomes possible. If a person can imagine that he is in a context very different from his actual context, acting, seeking goals, having desires, feelings, experiences very different from his present actual actions, goals, desires etc., then, by extension of this, a person can imagine that he is another person, with that other person's goals, problems, context, desires, feelings, experiences. Imaginative understanding of others becomes possible. It is this, I suggest, which leads to self-consciousness, self-awareness. As a result of imagining that we are other people, we become aware of ourselves as imagined by others. We see ourselves from outside ourselves, through the eyes of others: we become aware of discrepancies between the way others conceive of us, and our own experience of ourselves. It is this which produces self-consciousness, self-awareness. Self-consciousness is, on this view, essentially social in character, an outcome of seeing ourselves from the standpoint of others.[5] Thus, our ability to plan, to understand others, and to be self-aware all arise out of the ability to imagine action, to dream while awake.

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[4] The view, put forward here, that imagination is fundamental to human consciousness has been developed subsequently and independently in much greater detail by others: see for example Baars (1988). See also Maxwell (2001).

[5] This 'empathy' or 'social' theory of self-consciousness runs into the difficulty that it implies that autistic people, who lack empathic understanding of others, to that extent lack self-consciousness. If autistic people have self-consciousness – empathic understanding of themselves, as it were – far in excess of their empathic understanding of others, then this would refute the empathy or social theory of self-consciousness outlined in the text.

Finally, with the development of language, inter-personal or social imagining becomes possible. A group of people can cooperatively imagine, propose and assess, possible actions. And out of this develops the possibility of the cooperative development of shared knowledge and understanding – myth, commonsense, literature, propositional knowledge and science. Factual propositions about the world are, as it were, truncated proposals for action, truncated imagined actions, with the imagining, acting subject ignored or suppressed. According to this view, acquiring scientific knowledge about the world, and acquiring empathetic, imaginative understanding of others, are but two sides of the same coin, each dependent on the other. To imagine oneself to be another person successfully demands, amongst other things, the ability to imagine that the world is as experienced and conceived of by the other person. Just this imaginative entering into alternative possible worlds is at the heart of the natural sciences: in understanding rival physical or cosmological theories we in effect imagine ourselves to be people who conceive of the world in terms of these theories; what we do is a development of the kind of empathetic, imaginative understanding of others, which we all, to a greater or lesser extent, practise in life, and which is practised in a professional way by good psychiatrists, teachers, biographers, historians, anthropologists, actors and novelists. Understanding physical theory, and understanding people, are both, equally, developments of the basic ability to imagine oneself to be performing diverse possible actions.

It may be objected that the theory of the evolution of thought, consciousness and knowledge just sketched is highly speculative, and therefore not a very good basis for refuting the philosophy of knowledge. The following points, however, should be noted:

(a) Even if speculative, the just sketched biological, evolutionary theory of the development of thought, reason, consciousness, meaning and knowledge in the world is at least itself coherent, intelligible. The decisive feature of this theory is that the ability to act successfully in the world, and to tackle rationally problems of action (at least to some extent) come into the world before explicit propositional knowledge – propositional knowledge instead being a development of the ability to act and to imagine actions. Thus it is certainly at least *intelligible* to assert that rational action is a precursor of, and not dependent upon, knowledge.

(b) A major implication of the above theory is that animal learning is, in one important respect at least, from a methodological standpoint, more *rational* than our best official ideas about human learning – standard empiricism and the philosophy of knowledge. The philosophy of knowledge assumes that the prime task for inquiry is to acquire knowledge, which can

then be applied to solving problems of living. Animal learning proceeds on the more enlightened principle that it is problems of action, of living, that are central and fundamental. However, we ought not to be surprised that we have much to learn from the methodology of animal learning. The ability to learn is clearly of great potential survival value: it is to be expected that the process of natural selection will select out only the most efficient, the most rational, learning methods.

(c) The account given above of the development of knowledge is fully in accordance with our present scientific knowledge – in particular our knowledge of the physical universe and of evolution. In this respect it has a clear advantage over those accounts of the development of knowledge which take the philosophy of knowledge for granted, and thus require an essential break between animal and scientific learning. Thus Popper, presupposing a version of the philosophy of knowledge, is led to postulate an autonomous realm of propositions – his 'world 3' – which nevertheless somehow interacts with the brain; all this is difficult to accommodate within existing scientific knowledge of the physical universe and of life.

(d) The above action-suspended theory of the development of thought, consciousness, meaning and knowledge in the world, though speculative, may well be true. If true, it provides a basis for arguing that it is not just that, as a matter of fact, the capacity to act and to imagine action have temporal priority over the capacity to develop propositional knowledge: rather these things are, as it were, rationally prior to the development of knowledge, necessary preconditions for knowledge to exist, so that the nature of knowledge cannot be understood at all unless understood as arising out of the capacity to act and to imagine action. Or, to put this more succinctly: if all our knowledge as a matter of fact has the character specified by the above theory, this provides a basis for maintaining that all knowledge must be conceived of as a development of the capacity to act successfully in the world.

But it must of course be admitted that the above theory, being empirical and speculative, does not provide a very good basis for arguing that all knowledge *must* be conceived as an aspect of, a development of, the capacity to act, to realize goals in the world. In seeking to establish this doctrine we must argue rather that all alternative views run into insuperable philosophical problems which this doctrine successfully resolves. In support of the doctrine, here are three further arguments.

(e) The ability to acquire knowledge requires the prior existence of the ability to act successfully in the world, and to imagine possible actions, since only in this case can there be any possibility of making observations and experiments needed in order to develop propositional knowledge (the

presumption here being that knowledge-acquiring observation inevitably involves an element of action). On an individual level, it is only when we have discovered how to act successfully in the world as very young children, that there can be any possibility of acquiring propositional knowledge.

(f) Knowledge only exists if there exist conscious, self-aware, knowing persons; in order to be a conscious person it is essential to be able to act successfully in the world: hence successful action in the world is an essential precondition for the existence of knowledge. It might seem that a conscious but completely paralysed person constitutes a counter example to this argument. We may hold, however, that a completely paralysed person is only conscious insofar as it is legitimate to interpret his brain activity as being sufficiently analogous to brain activity which would occur if the person *could* act. Successful action in the world is even here hovering in the background, as it were, as a conceptual necessity for the person to be conscious.

(g) Propositional knowledge can only exist if people understand propositions: understanding propositions is itself, however, to be understood as a development of imagining actions (a development of imagining the environment for an action with the actor ignored); imagining possible actions in turn requires the prior existence of successful action. Thus an essential precondition for propositional knowledge to exist in the world is the existence of successful action.

The viewpoint developed here is fully in accordance with, and is backed up by, the arguments of chapters 4 and 5 concerning rationality, designed to establish that rational solving of problems of action presupposes a prior capacity to solve problems of action, to act successfully in the world, or that rational aim-pursuing presupposes a prior capacity to pursue aims.[6]

Life, action, and the problems of life, of action, are not only historically and rationally (or conceptually) prior to knowledge and the problems of knowledge: in addition, I wish to argue, they are evaluatively prior. What really matters is what we do, what goes on in our lives. Knowledge is of importance insofar as it contributes to, and participates in, life.

1 conclude that life, and the problems of life, are more fundamental than knowledge and the problems of knowledge, and that inquiry, rationally devoted to helping us achieve value in life must give priority to problems of life over problems of knowledge – a basic requirement which inquiry pursued in accordance with the philosophy of knowledge fails to satisfy.

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[6] The account of the evolution of human consciousness, knowledge and understanding sketched here is developed in greater detail in Maxwell (2001). See also chapter 10 below.

Finally it may be objected that it is not at all clear what it means to assert that action and the problems of action are more fundamental than knowledge and problems of knowledge. Does this amount to a kind of idealism, somewhat like doctrines advocated by Hegel or Schopenhauer, according to which the world is somehow the product of our actions? The answer is no. The stars exist independently of our actions, our perception and knowledge of them. Indeed, in chapter 9 I argue that physical entities such as electrons and protons exist and have properties independent of our acts of observation and measurement. I advocate physical realism, even a doctrine that might be called conjectural essentialism. It is not the physical universe that is the product of human action: rather it is our knowledge of the physical universe that is such a product. All human knowledge, however ostensibly impersonal and formal, is the outcome of human action, presupposes successful action in the world, and needs to be pursued and communicated in such a way as to help enhance the value of life. Impersonally recorded knowledge, in libraries, exists in order to promote personal acts of knowledge, apprehension, exploration. Dissociated from life, it is just paper and ink.[7]

Reply to objection 7

As to the objection that the philosophy of wisdom, unlike the philosophy of knowledge, cannot do justice to the value of inquiry pursued for its own sake, I claim the thing is all the other way round. Inquiry, pursued for its own sake, *is* people, individually and cooperatively, seeking to discover, understand and appreciate that which is of value in existence, significant aspects of the world, as an end in itself. It can scarcely be distinguished from life itself. A life devoid of this dimension of searching for its own sake would be impoverished indeed. In chapter 4 I suggested that we can get an estimation of how highly we value our own personal inquiry pursued for its own sake by considering how highly we value our capacity to *see* for its own sake. To a very great extent, for all of us, the value of life is bound up in the value of exploring and discovering significant aspects of the world around us, for its own sake. It is just this that formal inquiry, pursued for its own sake, properly conducted, emerges out of and seeks to encourage. Science and scholarship, pursued for their own sake, are personal and passionate aspects of life, essential to the value of life. Even explorations into the furthest

[7] What this argument amounts to, as Mary Midgley (1986, p. 426) has remarked, is that 'knowing how' is intellectually more fundamental than 'knowing that', to use terminology introduced by Ryle (1949, ch. II).

reaches of space, into the first few seconds of the cosmos, or into the fundamental laws of nature, into aspects of the world as far removed as possible from our customary human world and its concerns, nevertheless are of value insofar as they enrich human life.

All this can flourish, and can be understood, as long as inquiry is pursued in accordance with the philosophy of wisdom, since this viewpoint stresses the fundamentally personal and social character of inquiry, and stresses that inquiry has as its basic aim the realization of value in life – 'realization' including both discovery and creation. It cannot however flourish, or be adequately understood, as long as inquiry is pursued in accordance with the philosophy of knowledge, since this viewpoint dissociates the intellectual from the personal and social, and gives to inquiry the aim of acquiring impersonal knowledge of value-neutral fact. General adoption of the philosophy of knowledge even leads to a general blindness to the way in which modern science and scholarship, as a result of becoming institutionalized, professionalized, specialized, even industrialized, betray what is best, potentially, in inquiry pursued for its own sake, namely the shared, passionate quest of individuals into aspects of the mystery that surrounds us, and of which we are a part.

Our understanding and appreciation both of Nature and of ourselves (and of the interrelation between the two) are adversely affected by general adoption of standard empiricism and the philosophy of knowledge. In chapters 5 and 9 I attempt to demonstrate and illustrate how pursuing physical science in accordance with standard empiricism leads to a neglect of problems of understanding, the aim of science degenerating into the task merely to predict more and more phenomena more and more accurately. As a result, important problems of understanding that may not be all that hard to solve if put into the context of the endeavour to understand the nature of the physical universe – such as interpretative problems of quantum theory – remain neglected, misconstrued and unresolved.[8]

Far more serious is the injustice that adoption of the philosophy of knowledge does to our understanding of each other, and of ourselves. As I have already pointed out, one can discern, in the history of thought, two apparently very different conceptions of understanding. On the one hand there is 'understanding' as this arises in the context of the physical sciences: here, we 'understand' some phenomenon to the extent that we can predict, and thus explain, the phenomenon by means of a comprehensive, unified and, ideally, true physical theory (the theory, at the very least, being

[8] For the case of quantum theory, see Maxwell (1998, ch. 7).

predictively successful elsewhere). To explain, and thus to 'understand', means here to fit into a comprehensive pattern: and precise prediction is a necessary, though not a sufficient, condition for understanding to be achieved. On the other hand there is what might be called 'person-to-person' understanding, achieved when one person can accurately imagine himself to be another person, with that other person's feelings, desires, experiences, problems, beliefs, values. (Ideally, perhaps, this kind of person-to-person understanding is to be conceived of as a mutual affair, achieved by two people of each other.) It is this kind of person-to-person understanding that we find ourselves to be achieving of imaginary people, to a high degree, as we read or view great works of literature or drama, such as those of Shakespeare, Tolstoy or Chekhov. It is in this way that we seek to improve our understanding of our acquaintances, our friends, those we love. All good biographies, autobiographies, and some history, seek to improve our understanding of people, in this sense of 'understanding'. In contributing to the tradition of 'hermeneutics', 'verstehen' or 'empathetic understanding', thinkers as diverse as Vico, Herder, Ast, Wolf, Schleiermacher, Droysen, Dilthey, Croce and Collingwood have emphasized the importance of this kind of 'person-to-person' understanding, or have discussed or pursued aspects of it in their work. In recent times Laing (1965) and Sacks (1976) in particular have emphasized its importance for psychiatry and medicine. (For a more detailed discussion of person-to-person understanding than that given here see Maxwell, 2001, pp. 103-112.)

However, as long as the philosophy of knowledge is accepted, 'person-to-person' understanding seems not only quite different from, but also intellectually inferior to, 'scientific' understanding. 'Scientific' understanding, it can be agreed, is (a) objective (b) impersonal (c) factual (d) rational (e) predictive (f) testable and (g) scientific, in that there is an objective, impersonal, factual theory, which predicts the phenomenon to be understood, and is independently testable, and so capable of being appraised scientifically and rationally. Any genuine example of 'person-to-person' understanding, based on one person imagining himself to be another person and thus seeing the other person's situation, experiences, problems, from his own point of view, is liable to lack all of the above features. Such an act of understanding is, it may well be argued, (a) subjective (b) personal (c) emotional and evaluative (and thus non-factual) (d) intuitive (and thus non-rational) (e) non-predictive (f) untestable (g) unscientific. In seeking to achieve 'person-to-person' understanding of another person in some particular context, I seek to put myself, imaginatively, into that person's shoes: I try to imagine myself to be him. In doing this inevitably I make all sorts of guesses about such things

as the other person's feelings, desires, thoughts, problems, circumstances, beliefs and so on. But my primary aim is not to construct a *theory* at all, but rather to achieve an act of imaginative identification – to create within myself, in imagination, feelings, desires, aims, beliefs and so on, analogous in relevant respects to those experienced in reality by the other person. I may genuinely achieve this, and yet be unable to articulate a *theory* about the person in question, let alone a predictive, testable theory. Furthermore, I may not be able to predict the other person's actions or, if I am able to do so, I may well make a false prediction: and yet my 'person-to-person' understanding might be very good. I might, for example, realize that the other person faces a certain problem which he may seek to solve by doing one or other of three possible actions: all this might be entirely correct except that the other person hits upon a fourth action which had not occurred to me but which, once enacted, confirms my understanding of the person in that it constitutes an even better, or more typical, resolution of the problem confronting the person than the three possible solutions I had imagined.

Thus as long as something like the intellectual standards of the philosophy of knowledge are upheld, person-to-person understanding must be judged to be both quite different from, and vastly inferior to, 'scientific' understanding, from an intellectual standpoint. This ought to be conceded even by those who, like Winch (1958), Bauman (1978), Outhwaite (1975), Giddens (1976) and Hesse, seek to defend the legitimacy or importance of some version of person-to-person understanding for the social sciences within the general framework of some version of the philosophy of knowledge. And, of course, any such apologetic defence of person-to-person understanding plays straight into the hands of those who argue that only 'scientific' understanding has any real intellectual merit. All this is entirely explicit in the work of those psychologists who write about what they call 'folk psychology': see, for example, Churchland (1981), Stich (1983), Greenwood (1991 and von Eckardt (1994). As Churchland says quite explicitly 'The term 'folk psychology' is . . . intended to portray a parallel with what might be called 'folk physics', 'folk chemistry', 'folk biology', and so forth' (Churchland, 1994, p. 308), and he then goes on to argue that 'folk psychology' is false and needs to be replaced by scientific knowledge and understanding (pp. 310-11).

When viewed from the standpoint of the philosophy of wisdom, however, all this is radically changed. The first point to note is that, in effect, according to the philosophy of wisdom, a central and fundamental task of inquiry is to promote the development of good person-to-person understanding between people in the world. For, according to the philosophy of wisdom, the central and fundamental task of inquiry is to articulate problems of living

experienced by people in their lives, and to propose and critically assess possible solutions – and to promote the doing of this in a cooperative way as an integral part of life. It is to help articulate and scrutinize life-goals, thus helping aim-oriented rationalistic ways of life to develop in the world. But it is just these things that we need to do in order to achieve person-to-person understanding of each other. If I am to enter imaginatively into another person's life, so that I imaginatively see and experience things from his point of view, the essential thing that I need to do is to recreate imaginatively the other person's life-problems and their possible solutions. I need to imagine that I have the other person's life-goals, shaped by his circumstances, past, temperament, skills, feelings, desires, values and beliefs, and that I am seeking to realize these goals in the kind of way in which he might (to put the matter in a rather more 'aim-oriented rationalistic' way). Thus the philosophy of wisdom in effect puts the development of person-to-person understanding at the heart of intellectual inquiry: inquiry pursued in accordance with the philosophy of wisdom is designed specifically to help us improve our person-to-person understanding of each other in life.

The second point is that the philosophy of wisdom, unlike the philosophy of knowledge, is able to do full justice to the supreme value of person-to-person understanding from both human and intellectual standpoints, here as elsewhere intellectual value reflecting and promoting that which is of human value. It is hardly too much to say – especially in the light of the arguments developed above – that almost everything of value in life depends in an essential way on people developing person-to-person understanding of each other. Certainly friendship and love depend on this. But more generally, we may argue, all cooperative action, and in particular all cooperatively *rational* action, depends upon people being able to develop this kind of understanding of each other. For if people are to be able to do things together cooperatively, taking joint responsibility for some shared, common enterprise, it is essential that those involved can 'understand' – can enter imaginatively into and identify with, at least to some extent – each other's problems, proposals, desires and goals. Without this, cooperative rationality cannot begin. The extent to which we can, or cannot, develop person-to-person understanding of each other is thus potentially of great social, political and moral importance. As the social world in which most of us find ourselves becomes increasingly vast, complex and diverse – so that increasingly we interact with others very different from ourselves – it becomes all the more important that person-to-person understanding can develop across such differences. Promotion of individual liberty, and thus of diversity of choices and ways of life at the individual level (itself a

cooperative enterprise) is hardly likely to succeed or be sought if individual people cannot empathetically understand those different from themselves. The great danger is that in a vast, complex and diverse world people, instead of being enriched by diversity, will merely come to feel threatened and isolated by it, and will as a result hunger for some form of collectivism or nationalism (of the left or right) which banishes individual liberty and diversity. Freedom, justice, cooperative rationality, peace, active and effective compassion, friendship and love: all these depend quite essentially on the existence of person-to-person understanding between people. And, so we may argue, person-to-person understanding is essential to the realization of value in life in even more basic ways than this, in that it is essential to all communication between people, essential to our development as persons, essential to the development of self-consciousness. Quite generally, we are able to realize things of value in our life because of imaginative identification with the value-realizing endeavours of others.

As long as the intellectual merit of understanding is assessed in terms of criteria appropriate to the philosophy of knowledge, in terms of such things as the factual content and predictive power of impersonal, objectively formulated theories, person-to-person understanding cannot amount to very much from an intellectual standpoint, whatever its human value may be. Acceptance of the philosophy of wisdom, however, leads us to assess the intellectual merit of 'understanding' in a different way, in terms of how important and central it is in helping us to realize what is of value in life. Assessed in this way, person-to-person understanding emerges as being of profound intellectual value and merit. In connection with the above seven apparent intellectual defects and disadvantages of person-to-person understanding, the philosophy of wisdom enables us to say the following. (a) Person-to-person understanding can be, and ought to be, wholly objective in that it does full justice to the actual (objectively existing) feelings, desires, beliefs, aims and values of the person to be understood. In addition it can be, and ought to be, objective in that it includes knowledge of the person's actual circumstances, actions and aims, and knowledge of what is genuinely of value to him, actually and potentially, in his circumstances, as opposed merely to what the person himself believes about all this. One does not understand another person merely by sharing that other person's illusions and delusions. (b) Person-to-person understanding is certainly a *personal* (and inter-personal) kind of understanding, but none the worse for that. For certain purposes some personal aspects of thought may be neglected: it ought always to be remembered, however, that all thought is in the end personal and inter-personal, pursued for personal and social ends. (c) Person-to-person

understanding certainly does in general include the imaginative sharing of feelings and desires: but far from being non-factual, such understanding, to be any good, must involve knowledge and understanding of facts having to do with the circumstances of the person to be understood; and feelings and desires imaginatively experienced must in fact be similar to those of the person being understood. Our understanding of others *ought* to involve imaginative experiencing of their feelings and desires, and the capacity – or at least the concern – to see what is of value, potentially and actually, in the circumstances of their lives. Our mutual development of this kind of understanding of each other enriches us all: in its absence we are all impoverished. (d) Being able to achieve good understanding of others is to some extent a skill which, like other skills – such as those involved in speaking a language, for example, or in doing scientific research – can be acquired as a result of desire and practice. In successfully using an acquired skill we may act spontaneously, instinctively or intuitively; that is, without reflection, and not quite understanding why we act as we do: and yet our performance may be entirely rational, learnable and in accordance with sound general (but implicit) principles or methods – in this case the principles of the philosophy of wisdom. (e) Person-to-person understanding is not of intellectual value insofar as it involves seeing apparently disparate phenomena as aspects of a precise, comprehensive pattern, and is not of practical value because it enables us to predict, and therefore control: rather, it is of intellectual and practical value insofar as it involves seeing ourselves in others (and others in ourselves), thus making possible cooperative rational action, communication, sympathy, friendship and love. Person-to-person understanding is not encapsulated in impersonally formulated predictive and explanatory theories: but this does not in itself cast doubt on the intellectually sound, important and fundamental character of this kind of understanding since, according to the philosophy of wisdom, problems of living and their possible solutions (possible actions) are at the heart of rational inquiry rather than problems of knowledge and their possible solutions (theories). (f) In seeking to improve our person-to-person understanding of others it is essential to proceed in a conjectural and critical way, 'testing' our understanding by means of communication, listening to the testimony of others, considering possible implications for ourselves, for our own actions, of having certain feelings and desires, checking the network of factual assumptions implicit in such understanding. (g) It is absurd to condemn person-to-person understanding as non-rational and unscientific since such understanding is essential to all cooperatively rational pursuits, and in particular essential to the cooperatively rational pursuit of science. In order

for 'scientific' understanding of natural phenomena to be developed, it is essential that scientists can develop person-to-person understanding of each other. Impersonal, public, scientific knowledge, resting on a multitude of agreements among scientists about meanings, methods and results, is the outcome of a long history of individual scientists seeking to 'understand', in a person-to-person sense, each other's ideas, problems, projects, objectives, proposals. It is in this way that 'scientific' understanding of natural phenomena is based on person-to-person understanding between scientists. That which one scientist seeks to achieve in attempting to understand the theoretical or experimental work of a colleague is not essentially different from what a historian of science seeks to achieve in attempting to understand the work of Galileo, Newton or Darwin; and this in turn is not essentially different from what anyone seeks to achieve in acquiring person-to-person understanding of the work, life or actions of any other person, living or dead. Imaginatively reorganizing the way I see the world so that it more nearly resembles the way you see the world (which I must do if I am to be able to have person-to-person understanding of you) is not essentially different from imaginatively reorganizing the way I see the world so that it more nearly resembles the way Einstein saw the world, or intended us to see the world, in propounding the special or general theory of relativity. In both cases I am concerned with possible imagined visions of the world (more or less comprehensive, precisely formulated, testable, empirically successful, and so on). In both cases the emphasis of my interest may be *personal* (I wish to improve my understanding of you, or of Einstein), or *impersonal* (I wish to improve my understanding of the world). However, these two sorts of interest ought not to be, and strictly cannot be, severed from one another. If in pursuing theoretical physics we lose sight of the fact that our ostensibly 'impersonal' understanding of natural phenomena is the outcome of persons sharing, criticizing and developing each others' personal imaginings (and thus an outcome of person-to-person understanding between physicists), we are likely, as we have already seen, to betray the intellectual heart of the whole enterprise of natural philosophy: if in enhancing person-to-person understanding of each other we abandon our best cooperative efforts at improving our knowledge and understanding of the world, and give equal validity to all world views, we descend into mere relativism and subjectivism, and abandon the means to distinguish between sanity and insanity, justice and injustice, democracy and totalitarianism.

The two kinds of understanding dovetail together, being interdependent. Only the philosophy of wisdom can do justice to both kinds of

understanding, and to their interdependence, in a unifying way, both being essential to wisdom.

Reply to objection 8

Utopian social engineering, as described and criticized by Popper, 'aims at remodelling the "whole of society" in accordance with a definite plan or blueprint; it aims at "seizing the key positions" and at "extending the power of the State . . . until the State becomes nearly identical with society", and it aims, furthermore, at controlling from these "key positions" the historical forces that mould the future of the developing society' (1961, p. 67). Popper formulates the basic argument appealed to by Utopians to support this plan of action as follows:

"Any rational action must have a certain aim. It is rational in the same degree as it pursues its aim consciously and consistently, and as it determines its means according to this end. To choose the end is therefore the first thing we have to do if we wish to act rationally; and we must be careful to determine our real or ultimate ends, from which we must distinguish clearly those intermediate or partial ends which actually are only means, or steps on the way, to the ultimate end. If we neglect this distinction, then we must also neglect to ask whether these partial ends are likely to promote the ultimate end, and accordingly, we must fail to act rationally. These principles, if applied to the realm of political activity, demand that we must determine our ultimate political aim, or the Ideal State, before taking any practical action. Only when this ultimate aim is determined, in rough outline at least, only when we are in possession of something like a blueprint of the society at which we aim, only then can we begin to consider the best ways and means for its realization, and to draw up a plan for practical action. These are the necessary preliminaries of any practical political move that can be called rational, and especially of social engineering." (1969, vol. 1, pp. 157-8)

There are, perhaps, some Utopian aspects to what is being advocated in this book. I do seek to help remodel the 'whole of society'. I do, after all, propose that we should seek to develop a cooperatively rational world society. As a means to this end, I propose a sweeping, holistic change in the overall aims and methods of institutionalized inquiry and education, from knowledge to wisdom. Organized, academic inquiry, I am arguing, needs to take as its basic intellectual aim to help us develop a cooperatively rational, world society. Furthermore, I have argued that the holistic, Utopian institutional change that I am advocating in connection with academic inquiry can be taken as a model, for all of social inquiry, as to how we should seek to transform all other institutions in the world.

Despite this, it is also clear that what is advocated in this book is quite different from Utopian social engineering, as characterized by Popper. Cooperative, rational, social problem-solving, as characterized here, involves action that is very different from 'seizing the key positions', or 'extending the power of the State until the State become nearly identical with society'. Aim-oriented rationalism involves articulating aims certainly: but it does not assert that 'to choose the end is the first thing we have to do if we wish to act rationally'. Quite the contrary, aim-oriented rationalism asserts that if we wish to act rationally we must seek to improve our aims and methods as an integral part of *what we are already doing*. We act rationally when we add to the aim-pursuing we are already engaged in the activity of imagining and criticizing possible and actual aims and methods, in an endeavour to discover how our actual aims and methods may be improved, little by little, as we proceed.

Popper contrasts Utopian engineering, which he rejects as irrational and disastrous, with piecemeal social engineering, which he advocates as rational and humanitarian. In *The Poverty of Historicism* Popper characterizes piecemeal engineering as follows:

"Even though he (the piecemeal engineer) may perhaps cherish some ideals which concern society 'as a whole' – its general welfare perhaps – he does not believe in the method of re-designing it as a whole. Whatever his ends, he tries to achieve them by small adjustments and re-adjustments which can be continually improved upon. His ends may be of diverse kinds, for example, the accumulation of wealth or of power by certain individuals, or by certain groups; or the distribution of wealth and power; or the protection of certain 'rights' of individuals or groups, etc. Thus public or political (piecemeal) engineering may have the most diverse tendencies, totalitarian as well as liberal . . . The piecemeal engineer knows, like Socrates, how little he knows. He knows that we can learn from our mistakes. Accordingly, he will make his way, step by step, carefully comparing the results expected with the results achieved, and always on the look-out for the unavoidable unwanted consequences of any reform; and he will avoid undertaking reforms of a complexity and scope which make it impossible for him to disentangle causes and effects, and to know what he is really doing." (1961. pp. 66-7)

And Popper adds 'Once we realize . . . that we cannot make heaven on earth but can only improve matters a *little,* we also realize that we can only improve them *little by little'* (1961, p. 75). In *The Open Society and its Enemies,* the aim of piecemeal engineering is characterized somewhat differently, in that Popper there asserts that the piecemeal engineer "will be aware that perfection, if at all attainable, is far distant, and that every generation of men,

and therefore also the living, have a claim; perhaps not so much a claim to be made happy, for there are no institutional means of making a man happy, but a claim not to be made unhappy, where it can be avoided. They have a claim to be given all possible help, if they suffer. The piecemeal engineer will, accordingly, adopt the method of searching for, and fighting against, the greatest and most urgent evils of society, rather than searching for, and fighting for, its greatest ultimate good." (1969, vol. 1, p. 158)

Granted that a choice must be made, then clearly cooperative, rational, social problem-solving, as characterized in this book, is very much more like piecemeal than Utopian social engineering, especially as piecemeal engineering is characterized in *The Open Society and its Enemies*. But there are still important differences between the two (even if the moral neutrality of piecemeal engineering as characterized in *The Poverty of Historicism* is ignored). The method of piecemeal engineering does not appear to be, fundamentally, a method of *cooperative,* social problem-solving – that is, a method whereby many people cooperatively take responsibility for and guide social problem-solving together. The method of social engineering fails to emphasize the vital need to try out many possible individual and social actions in the individual and social imagination, so that unforeseen undesirable consequences of actions may be discovered in the *imagination,* and not in reality. In particular, the method of social engineering fails to emphasize the vital need for individuals cooperatively to articulate and criticize proposals for perhaps quite radical cooperative social change, so that many individuals can discover for themselves what the consequences would be, for their own individual lives and problems, of taking part in the cooperative *enactment* of such proposals; and so that personal problem-solving can acquire a cooperative social context and perspective. For a group of people, or a society, to carry out cooperatively some plan of action, it is not necessary for any individual, or small group of individuals to be able to understand and anticipate all the detailed problems for individuals that carrying out the plan would create: all that is necessary is that individuals can anticipate and tell others when such problems become insoluble or insufferable, so that in these cases the overall plan can be appropriately and cooperatively modified. It is just such potential rich resources of cooperative rational problem-solving as this that Popper's piecemeal engineering ignores. In so harshly criticizing Utopian engineering Popper leaves no room for the imaginative proposing and criticizing of *proposals* for radical but cooperative social change: it is perhaps this, more than anything else, which differentiates Popper's piecemeal social engineering from cooperative, rational, social problem-solving as described and advocated in this book.

Cooperative, rational, social problem-solving is, I suggest, in the light of these considerations, a *third* method for solving social problems, distinct from and superior to both Utopian and piecemeal social engineering, and not envisaged by Popper.

Two important general points deserve to be made about radical, cooperative social change – the kind of change Popper fails to envisage, or holds we should not attempt to make.

In the first place it is important to understand why it is so especially difficult for modern societies – for the modern world – to bring about generally desired and desirable changes of this kind. It is important to understand, that is, the nature of the problems that arise in an especially acute form for us in the modern world in seeking to implement radical cooperative social change. Engaging in cooperative social action is, of course, confronted by many problems today that are essentially the same as the problems that have always confronted such social action throughout history and pre-history: how to combat the powerful, the criminal and the deceiving who seek to oppose or subvert cooperative action; how to reach general agreement about what policies to adopt, what changes to attempt to make. Even a tribal society, a relatively small group of people, may be confronted by problems of this nature. The problems that confront us in the modern world, in an especially severe form, in addition to the traditional problems of cooperative action, are, I suggest, essentially of a *logistic* character. Modern societies are very much bigger, more complex, specialized and diversified than societies of the past. In pre-historical times, people lived in small cohesive hunting and gathering tribes. No logistic problem arises in calling a tribal meeting to discuss and to decide how to solve, in a cooperative manner, some problem that confronts the tribe. Furthermore, in comparison with the modern world, many features of tribal life will tend to promote in individuals attitudes conducive to assuming shared, cooperative responsibility for the welfare of the tribe. All the members of the tribe are known to each other personally. Relationships of mutual interdependence are experienced daily, on a personal basis, in hunting, gathering food, and so on. Obligations and responsibilities towards fellow members of the tribe can be experienced in a personal, emotional way, in terms of known individuals in much the same way as we can experience responsibilities towards members of our family today. All members of the tribe have a common outlook on things, a common cosmology, religion, system of values: thus barriers to intimacy, to mutual understanding, do not arise as a result of differences of outlook and values. Because of the relative smallness of the tribe, each individual makes a personal impact on the life of the tribe as a whole, and can be well aware of

this impact. The tribe, as it were, acknowledges the existence, value and potency of the individual, and is clearly affected by the actions of the individual. It will be normal, and not abnormal, for the individual to suppose he or she has sufficient personal importance in the life of the tribe to have a role in taking cooperative responsibility for the life of the tribe.

Time passes; agriculture is invented; trade grows and tribes coalesce; work becomes more and more diversified and specialized; modern methods of travel and communication are invented and developed. As history unfolds, the tribe becomes the nation, and the nation the world.

As a result of these rapid changes (incredibly rapid if put into the context of the hundreds of thousands of years mankind has been in existence on earth), the problems of acting cooperatively on a world-wide basis have become immense. From the standpoint of the world-wide community of humanity, each individual is surrounded by millions upon millions of complete strangers, most of whom speak languages, live lives, uphold beliefs and values that are more or less incomprehensible. Almost all individuals are utterly powerless to have any sort of impact on national life, let alone on world society. Opportunities for discovering how to take cooperative responsibility for the modern world will for most simply not arise. Almost everything tending to promote cooperative action in tribal life has, in the modern world, disappeared. Indeed some of the developments we most prize, such as the development of specialization, and diversity of ways of life, the freedom of individuals to adopt beliefs, values, modes of being different from others, serve to increase the severity of the problem.

There are, in short, utterly obvious, wholly unmysterious, essentially *logistic* reasons as to why cooperative action has become so severely problematic in the modern world.[9]

As I indicated in chapter 4, a fundamental task of organized inquiry, according to the philosophy of wisdom, is to help provide solutions to the logistic problems of engaging in cooperative action in the modern world. According to the philosophy of wisdom, we need to develop academic inquiry as a sort of institutional surrogate for open, cooperative tribal meetings of humanity.

The second general point to note about radical, cooperative, social change is that circumstances can develop which make it all but imperative to make

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[9] Popper might agree with some of this. His failure, however, to envisage the possibility of the philosophy of wisdom, making cooperative action possible even in an 'open' society, leads him to hold that 'the strains of civilization' must be endured rather than resolved.

such changes, irrespective of the size of the community involved. A small tribal society may need to change quite drastically a way of life that has been established for generations – because of some change in the circumstances of life such as a change of climate, the advent of hostile neighbours or a new disease, or dwindling traditional food resources. Equally, a vast, complex, world society may need to change, relatively suddenly and drastically, complex, diverse ways of life that have been established for generations – because of a relatively sudden, world-wide change in the circumstances of life.

We are confronted today by just such problems, on a worldwide basis. The population explosion, the world-wide endeavour to industrialize and achieve rapid economic growth, the resulting rapid depletion of finite natural resources and rapid destruction of plant and animal life, and the development of the nuclear bomb, taken together constitute a sudden, dramatic change in the worldwide circumstances of life for humanity.[10]

In the light of these points, it is clearly a matter of extreme urgency that academic inquiry, on a world-wide basis, begins to give intellectual priority to the tasks of (1) improving the articulation of these problems and (2) putting forward and criticizing proposals as to how these problems might be cooperatively resolved – thus promoting cooperative rational, social problem-solving in the world by putting the philosophy of wisdom into practice.

But this violates the principles of Popper's piecemeal engineering. We have here an indication of the drastic inadequacy of what Popper prescribes.

What can have induced Popper to overlook the possibility of attempting to develop cooperative, rational, social problem-solving? The explanation is simple enough and will by now be very familiar. Popper upholds the philosophy of knowledge. For him, social inquiry is to be developed as social *science,* on analogy with natural science. Popper does, it is true, emphasize the importance of attempting to help solve practical problems in developing social science: but, as he goes on to point out, this does not differentiate social science from physical or biological science, since here too a concern for practical problems is important, not only because solving practical problems may be important in itself, but also because concern for practical problems may stimulate progress in theoretical scientific knowledge in that it is 'invaluable for scientific speculation, both as a spur and as a bridle'. Thus 'Pasteur's reform of the biological sciences was carried out under the stimulus of highly practical problems, which were in part industrial and agricultural' (1961, p. 56). Popper goes on to argue that social science needs to be

[10] Since this was first written, in 1983, the point has become all the more urgent because of global warming.

developed as *piecemeal social technology,* an adjunct to piecemeal social engineering, as a collection of scientific social laws which specify what *'cannot be achieved'* (1961, p. 61). Popper gives a list of candidates for social laws of this type, and argues that the laws and theories of natural science are of precisely the same form, in that 'every natural law can be expressed by asserting that *such and such a thing cannot happen* . . . For example, the law of conservation of energy can be expressed by: 'you cannot build a perpetual motion machine' ' (1961, p. 61). And Popper remarks that 'the significance of our analysis lies in the fact that it draws attention to a really fundamental similarity between the natural and the social sciences' (1961, pp. 61-2).

Thus for Popper, the task of social science is to improve our knowledge of testable social laws prohibiting certain sorts of social actions, the hope being, presumably, that it will be possible, eventually, to develop unifying testable social theories, from which a wide range of social laws will be derivable just as a wide range of natural laws are derivable from the unifying, testable theories of physical science. From this standpoint, social science must be judged to be in an incredibly primitive state. This is stressed by Popper, as when he remarks 'My point about the technological approach might perhaps be made by saying that sociology (and perhaps even the social sciences in general) should look, not indeed for "its Newton or its Darwin", but rather for its Galileo, or its Pasteur' (1961, pp. 59-60).

It is especially the primitive state of social science, of social technology, so necessary, from Popper's standpoint, for piecemeal social engineering, that leads Popper to condemn the attempt to make radical, far-reaching social changes, and to condemn even, by implication, the enterprise of putting forward and criticizing proposals for such social changes. Thus he says: 'At present, the sociological knowledge necessary for large-scale engineering is simply non-existent' (1969, vol. 1, p. 162).

Popper's line of argument has the effect of prohibiting the one social change that is now so urgently needed if humanity is to discover, little by little, how to tackle its common problems in more cooperative and humane ways – namely a change in academic inquiry, and above all in social inquiry, from knowledge to wisdom. Popper fails to make the two general points about radical, cooperative, social change stressed above, namely that such radical social change may well be urgently needed, and that the peculiar difficulties that confront us in seeking to bring about such social change are essentially logistic in character (as this was interpreted above). Failing to construe the problems in this way, he also fails to put forward the solution advocated here – namely that organized inquiry should be developed precisely in order to promote cooperative, rational, social problem-solving in

the world. His allegiance to the philosophy of knowledge blinds him to this even as a possibility. His view that social problem-solving is at present severely hampered by the primitive state of social science, and his resulting advocacy of the urgent need to develop social inquiry as the pursuit of social knowledge, as social science (on analogy with natural science) actually serves to prohibit the one thing that most urgently needs to be done if we are to solve our common problems of living in a more humane fashion: namely develop academic inquiry in accordance with the edicts of the philosophy of wisdom. His arguments against Utopianism in our present state of sociological ignorance, has the effect of prohibiting just the kind of thinking we need to engage in if we are to solve our problems more cooperatively, rationally and humanely.

According to the philosophy of wisdom, it is a fundamental intellectual obligation of every teacher, every social inquirer, every scientist and scholar, in his or her professional work, to put forward and criticize proposals for cooperative action intended to promote the realization of what is of value in life and to encourage others to do this. These proposals may be limited and specific; or they may be as unrestricted as one can imagine, in that they may well amount to proposals as to how humanity as a whole is to achieve 'heaven on earth'. The vital point is to promote in society the *habit* of putting forward and criticizing proposals for action intended to help achieve what is of value. Only a society which has this habit can engage in cooperative, rational, social problem-solving and action. A reader of Popper's diatribes against Utopianism, however, is likely to conclude that to put forward for consideration and criticism sweeping, holistic proposals for social action – as I have done in this book – is to commit the deplorable intellectual, political and moral sin of advocating Utopian social engineering. He or she will thus refrain from putting forward such proposals, and will discourage others from doing so. In this way, Popper's critique of Utopianism has the effect of sabotaging the enterprise of developing a more cooperatively rational society, even if this is not exactly the effect Popper intended. It should be noted that Popper here joins company with Marx: for Marx's diatribes against Utopian socialism must tend to have exactly the same effect.

From the standpoint of the philosophy of wisdom, Utopians are objectionable in so far as they put forward Utopian proposals that exclude cooperative living and problem-solving; dogmatically uphold such proposals; attempt to implement such proposals in uncooperative ways. Many Utopians commit some, if not all, of these sins. But this must not lead us to the conclusion that all Utopians must commit some, if not all of these sins. For then we prohibit cooperative reason.

Yates (1989) criticized the first edition of this book for its Utopianism. For an account of this criticism, and my reply, see chapter 13.

Reply to objection 9

The view that individual freedom and cooperativeness are inherently at odds with one another is a mistake. Quite to the contrary, freedom of individuals and cooperation between individuals are mutually interdependent. Cooperation between individuals depends on individual freedom simply because without individual freedom there can be no cooperation (as 'cooperation' is understood in this book). Individuals only cooperate with one another if they do so freely, as a result of their own desire and decision to do so: to the extent that one group is tricked, bullied or brainwashed into acting 'cooperatively' by another group, to that extent cooperation does not take place. Cooperation is essentially free and mutual: for A and B to cooperate it is essential that A cooperates freely with B and B with A. Then again, freedom of individuals depends on cooperation between individuals, for two general reasons. First, in our crowded, complex and interdependent world, without cooperation, freedom of individuals will collide. One person's exercise of freedom will demand another person's loss of freedom. One group's exercise of freedom will demand another group's loss of freedom. Second, our individual freedom depends on our capacity to realize what is of value in life. Much that is desirable and of value in life – such as friendship and love – is itself cooperativeness going on between people of various kinds. Thus freedom requires cooperation. In short, cooperation is implicated in almost everything of value (and thus necessary to freedom) both as a *means* to the realization of things of value, and as something of supreme value in itself, an *end* in itself.

It follows that if our concern is to help enhance individual liberty (as a part of our concern to help achieve a more civilized world), it is essential that we strive to help develop a more cooperative world. A basic defect of traditional liberalism is that it has failed to emphasize the fundamental importance of developing a more cooperative world and has even, in some ways, actually opposed this. Traditional liberalism sees the problem of individual freedom as, fundamentally, the problem of defending the weak individual against the strong group, government or nation. From this standpoint, either we do what the group or government wants us to do, thus cooperating and abandoning our individual freedom, or we stand up against cooperation, thus preserving freedom. The programme of developing a more cooperative world is thus understood to be a programme for the annihilation of individual freedom, a programme for some kind of collectivist

enslavement. It is correct to see the problem of protecting the weak individual against the strong group or state as a basic problem of individual liberty. It is correct to oppose 'cooperation' in the sense of enslavement to group will. The *mistake* is to adopt, and restrict one's attention to, anti-liberal, collectivist conceptions of 'cooperation' alone. The moment cooperation is understood as presupposing individual freedom, it becomes clear that developing a more cooperative world, in this liberal sense, is not only compatible with, but is actually essential for, the development of individual freedom. The freedom to cooperate, one might almost say, is *the* freedom. In a world in which mutual cooperation and good will can be taken for granted, individual freedom is vastly enhanced for all of us, in all sorts of ways. Protecting weak individuals against strong groups or governments must itself be a cooperative act. Without cooperativism, liberalism collapses, intellectually and in practice.

Chapter Nine
Refutation of Minimal Standard Empiricism: From Science to Natural Philosophy

If organized inquiry is to help us realize what is of value in life in a good, rational way, then its overall character must be, in many respects, much closer to the philosophy of wisdom than to the philosophy of knowledge. This much at least, I trust, has now been established.

However, even granted this, a last ditch attempt at a defence of a highly modest version of the philosophy of knowledge may still be made. 'It is a grotesque mistake' (so it may be argued in support of this defence) 'to interpret the philosophy of knowledge as applying to the *whole* of rational inquiry. Such an interpretation was never intended in the first place. Properly understood, standard empiricism and the philosophy of knowledge are to be interpreted as applying, much more modestly, only to an *aspect* of that fragment of rational inquiry that is devoted to the acquisition of knowledge. Much of rational inquiry may well have other goals and may be concerned with problems of action: insofar as the acquisition of knowledge is not here at issue, such branches of inquiry lie beyond the intended scope of the philosophy of knowledge.

'It is true' (so the argument may be continued) 'that some modifications must be made to the somewhat primitive doctrine expounded in chapter 2, in the light of some of the arguments of subsequent chapters. It is, for example, wrong to exclude discussion of aims and priorities of research from the intellectual domain of science – from journals, lectures, textbooks and so on. There may well be fallible methods of discovery in science, which cannot be properly exploited if possible aims for research do not receive explicit scientific discussion. Again, the important role that *values* play in science needs to be recognized: of course science endeavours to discover what is significant or useful, and not only what is true, however irredeemably trivial.

'Despite such concessions to the philosophy of wisdom, the basic tenets of standard empiricism and the philosophy of knowledge, properly interpreted, remain entirely valid. In science questions about the *value* of a potential contribution to science must be sharply dissociated from questions about *truth,* about verification and falsification. As far as the latter questions are concerned, ultimately only empirical considerations must be allowed to determine what potential contributions are to be accepted and rejected as scientific knowledge – plus considerations having to do with the simplicity, unity or explanatory character of theories. Science does not, and must not,

make permanent untestable (metaphysical) assumptions about the universe, all theories which clash with such assumptions being rejected (whatever their empirical success might be). And more generally, only matters having to do with truth, fact, knowledge, must be allowed to influence what is accepted and rejected as constituting knowledge. All personal and social aims, problems, feelings, desires, experiences and values must be ruthlessly ignored when it comes to deciding what is to be accepted and rejected as constituting knowledge. To this extent, the basic tenets of standard empiricism and the philosophy of knowledge remain wholly valid.'

In this chapter I set out to establish that even this highly modest version of the philosophy of knowledge, which makes so many concessions to the philosophy of wisdom, is unacceptable, and must be rejected. Empirical considerations alone cannot decide what theories are to be accepted and rejected in science; considerations of simplicity, unity or explanatory power are required in addition to empirical considerations. But this means that choice of theory in science is persistently and permanently biased in favour of theories that are simple, unified, explanatory; and this in turn means that science permanently makes the metaphysical presupposition that the universe is comprehensible (to some degree at least). Two considerations govern choice of theory in science: the extent to which a theory is empirical successful, and the extent to which it is compatible with the metaphysical assumption that the universe is comprehensible (which flagrantly contradicts standard empiricism). Furthermore, ignoring all personal experiences, feelings and desires when it comes to the assessment of claims to knowledge severely restricts what we can acquire knowledge about. If knowledge is not to be severely restricted, we must attend to our personal feelings and experiences in assessing many claims to knowledge. I discuss this last point first.

A major objection to the modest version of the philosophy of knowledge just outlined is that it necessarily excludes from inquiry all knowledge of just that aspect of the real world which gives life its meaning and value. As I shall argue in chapter 10, what is of value in existence is to be associated with what may be called the 'experiential' aspect of reality. The experiential has to do with all that we personally experience – with what we see, hear, smell, touch, feel, with what we become aware of through our own sensory and emotional responses to things. It includes the sensory and aesthetic qualities of things and works of art, and the personal and moral qualities of people and their actions. In chapter 10 I expound a view which I call *experiential realism,* according to which these experiential qualities of things and people

do really exist in the objective world of fact, even though we can only become aware of them via our own personal experiences, our own personal sensory and emotional responses to things. Thus a rose really is red, as perceived by us, even though in order to become aware of this perceptual quality it is necessary oneself to experience the visual sensation of redness, it being impossible for a person blind from birth to know what sort of quality experiential redness is. Similarly, personal qualities of people we inadequately characterize by means of such terms as shyness, cynicism, courage, generosity, deviousness and so on, are real qualities of people and their deeds, even though we only become aware of these qualities via relevant personal experience.

In contrast to the experiential, there is the non-experiential dimension of reality – that aspect of things which one can know and understand without it being necessary oneself to have any special kind of experience. Thus, in order to understand what physical properties such as mass or elasticity are, it is certainly necessary to have had some experiences, simply in order to be conscious and capable of knowing and understanding anything at all. There is, however, no particular sort of experience that it is necessary to have had. A congenitally blind or emotionless person may be able to understand all of physics just as well as a sighted or feeling person can.

All knowledge of this experiential realm – so vital from the standpoint of realizing what is of value in life, from the standpoint of acquiring wisdom – is however excluded *a priori* from inquiry pursued in accordance with the modest version of standard empiricism and the philosophy of knowledge as outlined above (and from the immodest versions expounded in chapter 2). In order to know experiential facts about things or people it is essential to attend to one's own personal sensations and emotions. According to standard empiricism and the philosophy of knowledge, however, personal sensations and emotions have no role to play whatsoever in assessing claims to knowledge. Only impersonal, de-sensorized, de-emotionalized observation and experimentation are relevant to the assessment of claims to knowledge. Thus inquiry pursued in accordance with the philosophy of knowledge (in its modest or immodest version) is necessarily restricted to improving our knowledge of non-experiential fact. Knowledge of experiences, feelings and sensations of people may of course be acquired in psychology, anthropology, sociology or history: this will however be knowledge of *non-experiential* aspects of experiences, sensations and feelings.

When the world is viewed through the spectacles of the philosophy of knowledge, all that is of value, all that makes life worth living, mysteriously vanishes, and leaves not a wrack behind. The beauty of a summer's day, the

joy of a child's laughter, the miracle of being alive and conscious: all such things of value fade into mere non-experiential fact. The miracle of value realized, and the tragedy of value unrealized – whether through death, poverty, suffering, disease, enslavement or other misfortune – dissolve into non-miraculous, non-tragic, non-experiential fact. That there is anything of value in existence – anything miraculous or tragic – can only be, from the standpoint of even the modest version of the philosophy of knowledge, some kind of subjective illusion, an absurd hallucinatory reaction having nothing to do with objective reality, with objective fact.

When we are young (and if we are fortunate), the world is rich with value, charged with sensory and emotional significance, vivid with colour, taste, feel, smell, mystery, joy, terror, pleasure and pain. We are raw and open to the experiential dimension of reality.

According to the philosophy of wisdom, the basic task of rational inquiry and education is to help us strengthen and deepen this precious childish rawness and openness to the experiential dimension of reality. The task is to help us develop our childish capacity to realize what is of value so that it gradually becomes more sensitive and realistic, more knowledgeable and understanding, more creative, cooperative and responsible, more loving. In so far as this involves improving knowledge and understanding, it is above all knowledge and understanding of what is of value in the experiential realm that is of importance.

All this stands in sharp contrast with what is achieved by rational inquiry and education pursued in accordance with the philosophy of knowledge (even in its modest version). Knowledge and understanding acquired in accordance with precepts of the philosophy of knowledge do not strengthen and deepen our childish openness to the experiential: quite to the contrary, if anything, they quietly annihilate it. For, such knowledge and understanding carries with it the implicit and powerful message that our personal sensory and emotional reactions to things are irrelevant when it comes to a determination of objective fact. Becoming knowledgeable and educated actually involves becoming blind to all that is of value in the experiential realm – in that this realm is excluded from the world of objective fact and knowledge. Awareness of what is of value in the experiential realm is relegated to the merely subjective and personal, the illusory and non-rational, thus being decisively split off from the realms of objective knowledge and fact.

From the standpoint of the philosophy of wisdom this represents, not education, but a kind of intellectual corruption, the progressive inculcation of extreme intellectual blindness and schizophrenia. Is it to be wondered at if

such value-blind knowledge, increasingly influential throughout the twentieth century, should on occasions be associated with such human horrors as the nuclear devastation of Hiroshima and Nagasaki, or the Vietnam war? Is it to be wondered at that those who are subjected to such value-blind education should end up puzzled and uncomprehending as to what really is of value in existence, and how it is to be cooperatively achieved?

Proponents of the philosophy of knowledge are, of course, absolutely correct to hold that our personal sensations, feelings and desires can often mislead us about the real objective nature of things. The disastrous mistake is to hold that our personal sensations, feelings and desires *always* mislead us about the real, objective nature of things. If we are to improve our knowledge and understanding of what is of value in existence (let alone improve our capacity to help realize what is of value), it is vital that we educate our sensory, emotional and motivational reactions to things, so that gradually these reactions may come to represent more faithfully to us objective experiential facts as opposed to emotional (or value) illusions and hallucinations. It is just this rational education of feeling and desire which becomes incomprehensible once one adopts the philosophy of knowledge view that all personal emotional and motivational responses to things are subjective and illusory.

Our personal capacity to realize what is of value in our life depends vitally on our own instinctive personal, emotional, and motivational responses to things becoming educated to reveal to us what is genuinely of value (to us) in existence. Likewise, our common capacity to develop a more civilized world depends vitally on our common emotional and motivational responses to things becoming educated to reveal to us what is genuinely of value in existence. The philosophy of wisdom encourages, and the philosophy of knowledge discourages, this vital kind of emotional and motivational learning.

In reply to these points, proponents of the philosophy of knowledge may deny that the experiential realm really does exist: the truth of experiential realism may, in other words, be denied. (Arguments in support of experiential realism are developed below in chapter 10.) Alternatively, it may be conceded that the precepts of the philosophy of knowledge apply only to the acquisition of a highly restricted kind of knowledge, namely knowledge of non-experiential fact. I turn now to a refutation of this excessively modest version of the philosophy of knowledge in a domain as remote as possible from the experiential, where it ought to meet with its greatest success – namely the domain of theoretical physics.

According to the excessively modest, last remaining fragment of, standard empiricism and the philosophy of knowledge, now under consideration, theoretical physics at least obeys what Popper has called *'the principle of empiricism,* which asserts that in science, only observation and experiment may decide upon the *acceptance or rejection* of scientific statements, including laws and theories' (1963, p. 54). Insofar as the simplicity, unity or explanatory character of a theory influences decisions concerning its acceptability, this influence must be exercised in such a way that this does not amount to assuming that the universe itself is simple, unified or comprehensible (i.e. such as to render phenomena amenable to explanation). The basic aim of physics, in the context of justification, is to increase knowledge about the physical universe, no permanent presupposition being made about the nature of the universe. (If physics were to make any kind of permanent presupposition about the nature of the universe, then all those theories in conflict with this presupposition would be rejected out of hand, whatever their empirical success might be: this would involve violating the principle of empiricism, in that evidence *alone* would not decide what theories are to be accepted or rejected.) Optimistically, we may hope that knowledge of truth is gained at the *theoretical* level – successive theories drawing closer and closer to the truth; pessimistically, we may only require, for scientific progress, that knowledge of truth is gained at the *empirical* level – successive theories merely predicting more and more phenomena more and more accurately.

It should perhaps be noted that most contemporary scientists and historians and philosophers of science, including Carnap (1966), Hempel (1965), Nagel (1961), Popper (1959, 1963), Kuhn (1962), Lakatos (1970), Holton (1973), Hesse (1974), Grünbaum (1974), Salmon (1966) and Laudan (1977, 1984) uphold versions of standard empiricism as here characterized, in that there is general agreement that science does not, and ought not to, make any permanent metaphysical presuppositions about the nature of the world. This is echoed, in one way or another, by Glymour (1980), van Fraassen (1980), Cartwright (1983), Watkins (1984), Hooker (1987), Hull (1988), Howson and Urbach (1993), Kitcher (1993), Musgrave (1993), Dupré (1993), and McAllister (1996). Dupré (1993, p. 1) asserts that 'It is now widely understood that science itself cannot progress without powerful assumptions about the world it is trying to investigate, without, that is to say, a priori metaphysics', which sounds like the announcement of the demise of standard empiricism. But Dupré goes on to say that 'empirical inquiry ... provides the evidence on which such assumptions must ultimately rest' (Dupré, 1993, p. 2), which is to affirm standard empiricism.

There are, I shall now argue, at least nine lethal objections to even this excessively modest version of standard empiricism.

At once it may be asked: But does aim-oriented empiricism fair any better? In the first edition of this book I restricted myself to arguing that aim-oriented empiricism does indeed fair better than standard empiricism in that it provides a more rigorous conception of science which solves the crucial problem of induction and the problem of how it has been possible for physicists to discover new fundamental physical theories. I failed to show that aim-oriented empiricism solves other problems standard empiricism fails to solve – in particular, problems concerning simplicity or unity, and the problem of verisimilitude (the problem of what it means to say that science makes progress given that it advances from one false theory to another). In this second edition, I have left my 1984 account, in this chapter, intact, and in the new chapter 14 I indicate how aim-oriented empiricism is able to overcome *all* nine lethal objections to standard empiricism. For a very much fuller account see Maxwell (1998).

Here, then, are the nine objections to standard empiricism.

1 Standard empiricism fails to solve the *practical problem of induction* (as it may be called), the problem, that is, as to how or why it can be rational to accept empirically verified or corroborated laws and theories of physics as a basis for *action,* via technological applications. Any law or theory of physics applies to infinitely many different empirical circumstances, but can only ever be 'verified' for finitely many of these. Thus, however much evidence is amassed in support of a law or theory of physics, we must always remain infinitely far away from verifying it empirically. Its probability, relative to established evidence, must remain zero. There can only be zero probability that the next standard application of our theoretical physical knowledge, however well verified, to building a bridge, aeroplane, radio or whatever, will meet with success. All knowledge of physical laws and theories must remain irredeemably speculative. How, then, can it conceivably be rational to base our actions on such improbable speculation?

It is worth noting that this practical problem of induction is only a part of the more general *problem of rational action* – the problem of characterizing, in general terms, what it is to act rationally, and of providing some kind of justification or rationale for the claim that action of this type does indeed deserve to be held rational.

It might be thought that standard empiricism only fails to solve the problem of induction insofar as rather strong claims are made for what science can achieve, namely that science can achieve theoretical knowledge sufficiently reliable to form a rational basis for action. Abandon entirely the

claim that science can achieve such reliable knowledge, and – so it might be thought – there remains no problem of induction which cannot be solved within the framework of standard empiricism.

This is not the case. Even if no claim whatsoever is made about the capacity of science to achieve knowledge sufficiently trustworthy to form a rational basis for action, there is still a problem of induction remaining which standard empiricism cannot solve. Standard empiricism cannot, in other words, even explain how physics can acquire irredeemably *speculative* knowledge. This leads us to:-

2 Standard empiricism fails to solve the *theoretical problem of induction* (as it may be called), the problem, that is, of how and why it can be rational to accept empirically verified or corroborated laws and theories of physics as constituting merely the best available *conjectures or speculations* about the physical universe, no claim being made to the effect that such speculations constitute reliable knowledge, a rational basis for action.

The problem of induction is often understood to be the problem of how we can know that the future will continue to resemble the past, the problem of how we can know that a theory verified in the past will continue to be verified in the future. It was roughly in this way that Hume formulated the problem. From our present point of view, however, there are several defects in this way of understanding the problem. The formulation of the problem needs to be improved.

In the first place we must remove any suggestion that the problem only arises if we presuppose an inductivist or verificationist conception of science, or presuppose that science can acquire reliable theoretical knowledge. Much more seriously than this, the *theoretical problem of induction* under consideration arises if we presuppose merely that it is rational to select theoretical *speculations* in science by means of observation and experiment alone. Not only inductivist and verificationist conceptions of science, but Popper's falsificationist conception of science too, all fail to solve the problem. In order to bring this out clearly, we can formulate the problem as follows.

Given any empirically successful physical theory T overwhelmingly corroborated in some inevitably finite space-time region R, we can easily construct endlessly many rival theories T_1, T_2, . . . $T_{1,000}$, . . , all of which agree with T in R but disagree with T elsewhere (in any way we please) at some other times and places. What rationale can there be, then, for preferring T (even as a mere speculation) on empirical grounds alone, since the theories T_1, T_2, . . . $T_{1,000}$, . . , are all equally well corroborated by the available evidence?

T might be, for example, Newton's law of gravitation plus his laws of motion (presumed for the sake of the argument to be unrefuted). A typical T_1 would be:

Up to the end of today, an inverse square law holds, $F = G.\frac{M_1.M_2}{d^2}$; from tomorrow onwards, an inverse cuble law holds: $F = G.\frac{M_1.M_2}{d^3}$.

Tomorrow, doubtless, T_1 will be refuted: endlessly many rivals to T will however remain unrefuted.

It deserves to be noted that the very absurdity of these endlessly many *aberrant* rivals to T (as they may be called) is a striking indication of the gravity of the problem confronting standard empiricism. If standard empiricism failed to provide a rationale for rejecting a fairly sensible rival to T, this would not constitute too serious an objection to standard empiricism. But the aberrant theories T_1, T_2, . . . $T_{1,000}$, . . , are absurd; no physicist would take any such theory seriously for a moment. Thus any methodology of physics which fails to provide a rationale for rejecting such ludicrous theories, thereby fails disastrously.

But the problem confronting standard empiricism is even more serious than this would suggest. Possible physical systems, to which any sensible physical theory T applies, do not only differ with respect to position and time; they also differ with respect to values of other physical variables, such as mass, shape, density, velocity, temperature and so on. Thus, in order to develop endlessly many aberrant rivals to T, all just as highly corroborated as T, it is not necessary to find some region of space-time in which T has not been tested: all one need do is find some range of values of other physical variables (mass, temperature, etc.) for which T has not been tested, and arbitrarily modify the equations of T within this range of values in any way one pleases whatsoever. Thus if Newtonian theory has not been verified for physical systems consisting of bodies of density greater than some value D_0, or with relative velocities greater than some value V_0, or for bodies further apart than d_0, endlessly many highly corroborated aberrant rivals to Newtonian theory may be formulated, differing from Newtonian theory only in the as yet unobserved range of values of physical variables (density, velocity or distance). For example, one may stipulate that

$$F = G\frac{M_1.M_2}{d^2} \text{ if } d < d_0, \quad F = G\frac{M_1.M_2}{d^r} \quad \text{if } d \geq d_0,$$

where r is any number between, let us say, 1 and 4 but different from 2.

It should be noted that the infinitely many aberrant rivals to Newtonian theory, here indicated, do not in any way postulate (what may be regarded as)

arbitrary changes of physical law at specific places or times. They all conform to the principle of uniformity of law in space and time; they are invariant with respect to space and time, or are 'strictly universal' in Popper's terminology (1959, pp. 62-70).

It is also important to note that there is no end to the ways in which physical systems, to which T applies, can be regarded as varying, and no end to the number of physical variables we can employ to distinguish different physical systems. These variables need not be referred to by T: it may simply be presupposed that variation of these variables leaves the applicability and success of T entirely unaffected. Thus the form of the equations of Newtonian theory not only remains invariant as we vary the place or time of a physical system, the mass, relative distance, velocity and acceleration of the bodies, their density and shape: the equations remain invariant as we change substance, temperature, colour, elasticity, smell. In order to formulate endlessly many empirically corroborated aberrant rivals to Newtonian theory, all we need do is specify, in universal terms (in terms of shape, substance, temperature, colour, smell or whatever) a kind of physical system, to which Newtonian theory applies, which has not yet been physically realized (perhaps because of its bizarre character): we then arbitrarily modify the Newtonian equations, in any way we please, for this specific kind of system. Thus we might stipulate: for two bodies, each of mass greater than two tons, each made of gold and shaped like a grand piano, adrift in space, an inverse cube gravitational law applies (but otherwise Newton's inverse square law applies).

Quite generally, given any theory T, in order to create endlessly many empirically equally successful, aberrant rivals to T, we need only take any kind of experiment E (however often repeated) and specify some bizarre, physically trivial but as yet untried physical modification of E, thus creating a nominally new kind of experiment E*. (The change from E to E* might involve such irrelevant modifications as painting the apparatus blue, placing an ounce lump of gold six feet from the apparatus, creating sound corresponding to middle C played on the violin.) Granted that T successfully predicts that E leads to outcome O, aberrant versions of T (T_1, T_2, . . . T_{10}^{10}, . .) agree with T everywhere except that for E* they predict O*, where O* is whatever we may please. Empirically successful aberrant theories of this type are of course ludicrous, and would never be considered for a moment within science: all the more disastrous, then, is the failure of

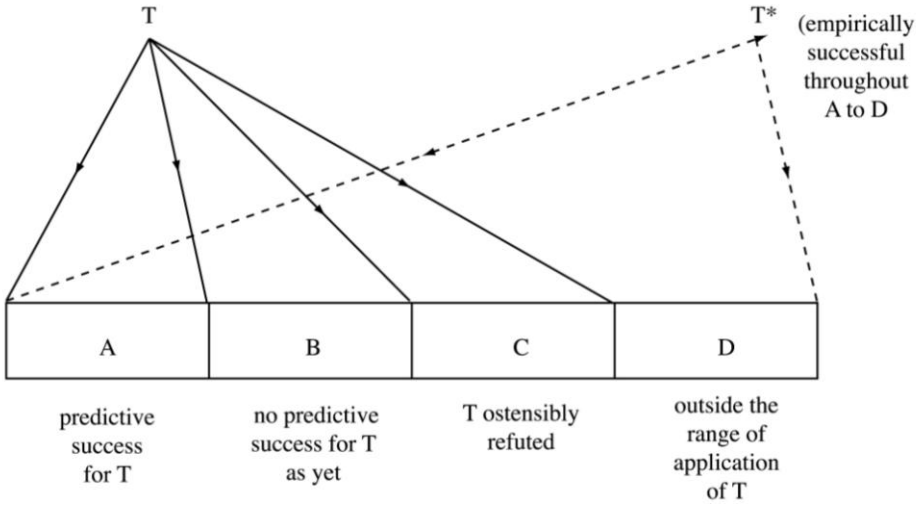

Figure 7 Refutation of claim that simplicity can be identified with empirical content

standard empiricism to provide a rationale for rejecting such ludicrous theories. (It is not physics that is here under attack, but rather a widely upheld, disastrously mistaken *philosophy of physics!*)

The problem confronting standard empiricism is still more serious than the argument so far indicates. Take any highly corroborated, sensible physical theory T: in practice T successfully predicts a range of phenomena A; it in principle applies to, but does not predict (because the equations of T cannot be solved) a range of phenomena B; it is ostensibly refuted by some recalcitrant, 'problematic' phenomena C; and it is entirely silent about a range of phenomena D (see figure 7). Here a phenomenon is understood, as it is in physics, to be a repeatable effect, a more or less observational or experimental law (which can of course always be sub-divided into as many sub-laws as we please). Consider now the rival theory T* which asserts: in A, everything occurs as T predicts; in B, everything occurs as T in principle predicts except for established laws L_B, which replace predictions of T; in C, and D everything occurs in accordance with the experimentally established laws L_C, and L_D. T* satisfies all the standard empiricist requirements demanded by Popper for a new theory to be more acceptable than its predecessor. T'* is not refuted whereas T is; T* has greater empirical content than T, and some of this excess content has been experimentally corroborated; T* successfully predicts all the empirical success of T; T* successfully predicts new phenomena, and given the infinite divisibility of

laws, the infinite nominal variability of experiments, T* can readily be shown to predict successfully hitherto 'unknown' phenomena. (And of course endlessly many aberrant alternatives to T*, equally preferable to T on empirical grounds alone, may also be constructed in the ways already indicated.)

In practice physicists persistently reject the most empirically successful (but horribly ugly) theories in favour of far less successful, and even ostensibly refuted, non-aberrant theories. The evidence persistently tells physicists that there are at most only patches of order in overall confusion, and physicists persist in believing in the existence of hidden order, despite all the evidence to the contrary, and even to the extent of rejecting empirically successful theories that postulate disorder. This scarcely accords with standard empiricism!

But the situation is even worse. It is not just that physicists persistently prefer beautiful, refuted theories to empirically far more successful, unrefuted but ugly theories: even worse, physicists habitually suppress evidence that clashes with established theoretical order. It is probably true to say that experimental results actually obtained in laboratories more often refute than corroborate established physical laws and theories: almost always experimentalists conclude that the experiment is at fault, and needs improving. It is nearly always extremely difficult, and a matter of great skill, to get apparatus used in even a fairly standard experiment to work properly. In practice, then, physical theory is used to refute experimental results at least as often as experimental results are used to corroborate or refute physical theory. In short, the conviction in physics that order exists in the world does not only lead to the rejection of empirically successful disorderly *theories:* it actually leads to the rejection of disorderly *evidence.*[1]

3 Standard empiricism fails to solve the problem of providing a rationale for preferring simple or unified theories to complex or disunified theories in physics.

One way in which one may seek to solve the above theoretical problem of induction is to stipulate that in physics simple, non-aberrant laws and theories are to be preferred to complex, aberrant laws and theories. Every proponent of standard empiricism acknowledges that considerations of simplicity or unity play an important role in determining choice of theory in physics. What no proponent of standard empiricism can do, however, is

[1] Later in this chapter I outline how aim-oriented empiricism solves the problem of induction – an outline developed, in order to deal with objections, in Maxwell (1998, ch. 5; 2004b, appendix, section 6), and in chapter 14 of the present work, section 6.

explain how physics can select theories impartially and solely with respect to empirical success and failure if preference is persistently given to simple or unified rather than complex or disunified theories (even to the extent, as we have seen, of the demand for simplicity and unity persistently overriding the demand for empirical success). Persistently to prefer less empirically successful but simpler theories to more empirically successful but more complex theories is precisely to *abandon* the principle of empiricism, according to which theories are to be chosen solely with respect to empirical success and failure.

The only honourable attempt that I know of to solve this problem within the framework of standard empiricism is Popper's attempt as set out in *The Logic of Scientific Discovery* (1959, ch. 7). Popper's argument might be reformulated as follows. If theories are to be selected in science solely with respect to empirical success and failure, in the best, most honest and rigorous way possible, then preference needs to be given, other things being equal, to those theories most amenable, most sensitive, to being selected in this way – that is, to those theories that are the most *falsifiable* empirically, or, in other words, to those that have the highest empirical content. *But high falsifiability (or high empirical content) equals high simplicity.* The more falsifiable a theory is, so the simpler it is (and *vice versa).* Thus preference is given in science to simple rather than complex theories precisely in order to put impartial empiricism into practice in the most honest, rigorous way possible. Superficially, giving persistent preference to simple over complex theories violates the principle that only empirical considerations must be permitted to determine the choice of theory: actually we can only honestly put impartial empiricism into practice by persistently preferring simple (i.e. highly falsifiable) theories.

This argument requires, quite essentially, that whenever we increase the empirical content or falsifiability of a theory, we increase its simplicity. But this crucial thesis is false. As we have seen above, it is all too easy to increase the empirical content (or falsifiability) of a theory and at the same time vastly *decrease* its simplicity, vastly increase its complexity, its degree of aberrance (see figure 7).[2]

Subsequently Popper has in effect recognized the inadequacy of the theory of simplicity expounded in *The Logic of Scientific Discovery.* For in *Conjectures and Refutations* he puts forward a requirement of simplicity for science that is wholly in addition to falsifiability. He argues that a new theory, in order to be acceptable, must 'proceed from some *simple new, and powerful,*

[2] For a slightly more detailed and careful refutation of this Popperian claim concerning simplicity see Maxwell (2007a, especially note 7).

unifying idea about some connection or relation (such as gravitational attraction) between hitherto unconnected things (such as planets and apples) or facts (such as inertial and gravitational mass) or new "theoretical entities" (such as field and particles)' (1963, p. 241).

Granted that it can be formulated sufficiently precisely, this new *'requirement of simplicity'* (as Popper himself calls it) is in a sense more adequate than the earlier one in that it does perhaps exclude empirically successful, highly falsifiable aberrant theories of the kind discussed above. But how can selecting theories in accordance with the new requirement of simplicity conceivably be compatible with standard empiricism, with the principle that *empirical considerations alone* are to govern the choice of theory? If this new requirement of simplicity is adopted in physics, then potential new aberrant theories which clash with the requirement must be rejected whatever their empirical success might be. This is precisely to abandon standard empiricism. Even if the physical universe is complex and aberrant, this fact cannot be discovered, at the theoretical level, by a science that puts Popper's new requirement of simplicity into practice, just because any theory asserting aberrance in the physical world would be rejected out of hand, whatever its empirical success might be.

In short, a standard empiricist rationale can at most only be provided for Popper's earlier, wholly inadequate requirement of simplicity (explicated in terms of falsifiability). As far as Popper's later, and perhaps more adequate, requirement of simplicity is concerned (which cannot be explicated in terms of falsifiability), no rationale can be provided for adopting the requirement within the framework of standard empiricism.

4 Quite apart from being unable to provide a rationale for adopting a principle of simplicity in physics, standard empiricism fails even to specify adequately what simplicity (or unity) is. Simplicity (or unity) cannot be defined merely in terms of the number of postulates a theory has, since number of postulates can always be reduced as much as we please, by logical means, even down to one postulate. Nor can simplicity be defined in terms of number of different sorts of entities postulated by a theory, since number of different sorts of entities can always be artificially reduced by stipulating that different sorts of entities are different states of *one* entity. For similar reasons, simplicity cannot be defined in terms of the number of different sorts of basic physical properties the theory attributes to physical entities. Simplicity cannot be defined merely in terms of the mathematical simplicity of the equations used to formulate the theory, since this depends on our choice of mathematical and conceptual conventions, a suitable change of terminology and concepts being sufficient to transform the 'simplest' theory

(in any such sense) into a highly complex theory (and *vice versa).* There are, it is true, standard empiricist attempts such as Goodman's (1972, ch. 7) to give a precise, formal explication of the notion of simplicity: these seem to have nothing to do with physics. There are also attempts to explicate the notion of unity of theory presupposing standard empiricism: see Friedman (1974), Kitcher (1981), Waktins (1984, pp. 203-13); and see Salmon (1989) for a review of the literature on the problem: but all these attempts fail (see Maxwell, 1998, pp. 56-68; 2004c; 2004d). In the circumstances it seems more honest for standard empiricists to confess, with Popper, that the *'requirement of simplicity* is a bit vague, and it seems difficult to formulate it very clearly' (1963, p. 241).

In order to make clearer what simplicity is in the context of physics, one crucial problem must be solved, namely the problem of distinguishing sharply mere *terminological simplicity,* of no account in assessing the acceptability of a theory, from *physical simplicity,* highly relevant to the assessment of a physical theory. Standard empiricism fails to solve this problem for at least one very good reason. Adoption of a *requirement of physical simplicity* in physics (physical simplicity here being clearly distinguished from mere terminological simplicity) would clearly and explicitly violate standard empiricism. In other words, clarification of the methodologically important notion of simplicity, as this arise; within physics, can only be done if standard empiricism is rejected.[3]

5 Not only does standard empiricism fail to provide a rationale for the acceptance and rejection of theories in the light of experimental results: it fails even to provide a rationale for the acceptance of experimental results themselves. As far as physics is concerned, an experimental result is a repeatable effect, a physical law. Any particular observational or experimental result, obtained at a particular time and place, can only become a part of physical knowledge, capable of corroborating or refuting physical theory, insofar as the particular result is construed to exemplify a repeatable, universal, law-like effect. Inevitably, however many experiments are performed, we must remain infinitely far away from verifying any such law-like experimental phenomenon. The two problems of induction, discussed above in connection with the acceptance of *theory* in physics, arise just as potently in connection with the acceptance of *experimental results,* as these are understood in physics.

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[3] See Maxwell (1998, chs. 2-4, and 2004b, appendix, section 2), and ch. 14 of the present work, section 2.

6 Standard empiricism fails to specify clearly and unambiguously the *methods* of physics – quite apart from its failure to provide a rationale for any such methods. Thus proponents of standard empiricism, such as Hempel (1965), Nagel (1961), Popper (1959, 1963), Kuhn (1962), Lakatos (1970), Hesse (1974), Salmon (1966) and Laudan (1977, 1984) disagree substantially about the nature of scientific method. Again, no clear, unambiguous formulation of the methodologically vital 'requirement of simplicity' is forthcoming, as we have seen.

7 Views about the aims and achievements of physics, the nature of physical theory, traditionally associated with standard empiricist philosophies of physics, fail to solve the problem of the *miraculous predictive success of physical theory* (as it may be called). According to these traditional standard empiricist philosophies, physical laws and theories essentially do no more than assert the existence of *regularities* in phenomena. At once the problem arises: why should phenomena observe these postulated regularities? A vast range and diversity of phenomena obey the regularities postulated by 'well-established' physical theory, to a quite extraordinary degree of accuracy. Unless something exists in the world which is, in some sense, 'responsible' for this extraordinarily widespread, accurate observance of postulated regularities – which in some sense 'controls' or 'determines' phenomena to obey these regularities – the continuing predictive success of physical theory can only be deemed to be utterly incredible, an enduring miracle. As traditionally interpreted, there is no scope for physical theory to assert the existence of anything in the world capable of being 'responsible for', in any sense, lawfulness or regularity. According to these traditional views, observed regularities can only be 'explained' by the discovery that they are a part of (and can be approximately derived from) even more widespread and accurate regularities (as when the regularities of Kepler's laws of planetary motion are discovered to be a part of the more universal regularities of Newtonian theory). This kind of 'explanation' only deepens the mystery: it serves only to make it all the more incomprehensible why phenomena should comply with such universal regularities to such an incredible degree of accuracy.

8 Standard empiricism fails to explain how it can be possible for theoretical physics to make progress. In attempting to discover new, better theories, physicists are, according to standard empiricism (as we have seen) confronted by infinitely many possible theories. The likelihood of formulating a theory that constitutes an improvement over existing theoretical knowledge would thus seem to be infinitely remote. Standard empiricism cannot provide even a fallible rational guide for the development of good, new physical theories, since according to standard empiricism the

only rational criteria that exist in physics for the assessment of ideas have to do with empirical success and failure of theories once they have been formulated. Theoretical physicists cannot even restrict their attention to new theories that are compatible with existing well-corroborated theories, since more often than not successful new theories are *incompatible* with pre-existing theories. (Thus quantum theory is incompatible with classical physics, Einstein's general theory of relativity is incompatible with his special theory of relativity, in turn incompatible with Newtonian theory, which is in turn incompatible with Kepler's laws of planetary motion.)

9 Not only does standard empiricism fail to provide a rationale for the claim that science makes progress (objections 2 and 3); not only does it fail clearly to characterize methods designed to achieve scientific progress (objection 6); not only does it apparently render the achievement of scientific progress little short of the miraculous (objections 7 and 8); but wholly in addition to all this, there is, if anything, an even more devastating failure: within the framework of standard empiricism there is no solution to the problem of what scientific progress *means*.

As long as science progresses by accumulating more and more truth, no problem arises as to what progress means. We have just seen, however, that science does not progress like this: new theories tend to correct their predecessors, thus revealing these predecessors to be, strictly, false. Physics in particular develops from one false theory to another: it is this which creates the problem of what scientific progress means.

The only conceivable solution to this problem, within the framework of standard empiricism, would seem to be the solution proposed by Popper: physics progresses if and only if successive theories T_1, T_2, T_3 . . ., though all false, nevertheless get progressively closer and closer to truth, in that they have progressively more and more truth in them, and/or less and less falsehood. A little more precisely, given any two false theories, T_1 and T_2, T_2 is closer to the truth than T_1 if and only if *either* (a) T_1 and T_2 have precisely the same false consequences but the true consequences of T_1 are less than, in the sense of being a proper part of, the true consequences of T_2; *or* (b) T_1 and T_2 have precisely the same true consequences but the false consequences of T_2 are less than, in the sense of being a proper part of, the false consequences of T_1; *or* (c) the true consequences of T_1 are less than those of T_2, and the false consequences of T_1 are greater than those of T_2 (case (c) being, as it were, an amalgamation of cases (a) and (b)) (Popper, 1963, pp. 231-7).

It turns out, unfortunately, that these apparently unproblematic conditions can never be realized for any false theories T_1 and T_2 (Tichy, 1974; Harris, 1974; Miller, 1974). If T_2 has more true consequences than T_1, then it

inevitably also has some false consequences in addition to those of T_1 (so case (a) cannot be realized). Alternatively, if T_1 has more false consequences than T_2, then it inevitably also has some true consequences in addition to those of T_2 (so case (b) cannot be realized). As neither case (a) nor case (b) can be realized, case (c) cannot be realized either.

For case (a), consider propositions p and q such that p is false, and is implied by T_1 (and T_2), and q is true, is implied by T_2 but not by T_1. In this case the proposition 'p & q' is false, is implied by T_2, and yet is not implied by T_1. Hence T_2 has false consequences that are in addition to those of T_1 , and case (a) cannot be realized. In connection with case (b), let r be a proposition that is false, that is implied by T_1 but not by T_2; let s be a proposition that is false and is implied by both T_1 and T_2. In this case the proposition 's ⊃ r' is true, is implied by T_1, but is not implied by T_2. Hence T_1 has true consequences that are in addition to those of T_2, and case (b) cannot be realized. Hence case (c) cannot be realized either, it thus being impossible to compare any two false theories with respect to their closeness to the truth. (See figure 8 for a representation of this essentially simple argument.)

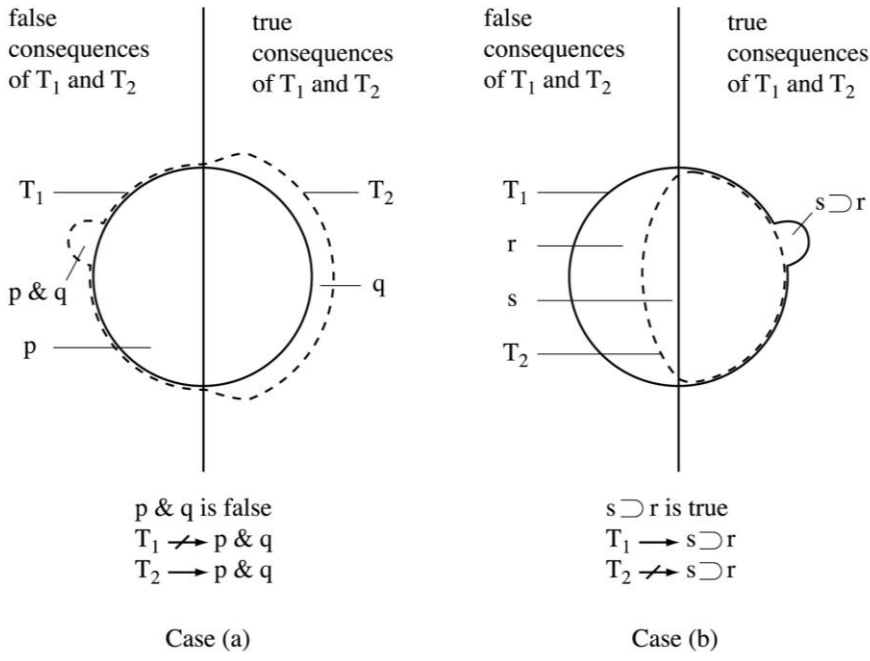

Figure 8 Impossibility of comparing verisimilitude of two false theories

If some kind of distinction can be made between atomic propositions (p, q, etc.) and molecular propositions (p & q, s ⊃ r, etc.), then it does become possible to compare some false theories with respect to their closeness to truth, in that for the purposes of the comparison, molecular propositions, the source of the problem, can be excluded from consideration. However, any distinction between atomic and molecular propositions must be relative to our choice of language. This has the consequence that the question of which of two false theories is closer to truth can depend on our choice of language. An acceptable solution to the problem cannot be language-dependent in this way.[4]

I conclude, in view of the above nine objections, that even the extremely modest version of standard empiricism formulated above is untenable, and must be rejected.

The mistake of standard empiricism is to misrepresent the basic intellectual aim of physics. The aim of physics is not, and cannot be, merely to improve knowledge about the world, nothing being presupposed about what sort of world this is. On the contrary, in the contexts both of verification and discovery, an essential aim of physics is to improve our understanding of the world, it being an unavoidable presupposition of physics that understanding is in principle possible, the universe being, in some way or another, comprehensible. There are many different sorts of ways in which the universe might be comprehensible. Modern physics, a highly sophisticated and successful development of humanity's long-lasting search for understanding, presupposes, more or less specifically, that the universe is intelligible in the sense that some kind of unified pattern runs through all phenomena. The vital general point, however, is that the pursuit of knowledge cannot be dissociated from the pursuit of explanation and understanding – from the presupposition that the world is such that explanations of some kind or other exist to be discovered. At first sight it might seem that the pursuit of knowledge can be, and ought to be, dissociated from any presupposition about the world whatsoever, including the presupposition that the world is comprehensible: it is just this standard empiricist thesis that has been shown to be untenable in the last section.

What is meant by the assertion that the universe is comprehensible? I wish to allow that there are many different ways in which the universe may be comprehensible and thus, in a sense, many different possible more or less

[4] The solution to the problem of verisimilitude I wish to advocate is spelled out in ch. 14, section 5 and appendix.

specific conceptions as to what comprehensibility is. Among the many ways in which the universe might conceivably be comprehensible, there are two – here called *personalism* and *physicalism* – which are of particular importance in the history of human thought.

According to personalism (sometimes called animism), the world is made up of beings – or gods – somewhat like persons, with purposes, desires, feelings, experiences, acting in response to experiences in order to satisfy desires, realize intentions. What goes on in the non-human world is the outcome of the actions of these gods rather as what goes on in human society is the outcome of the actions of people. Such things as the sun, the moon, the sea, the earth, forest, mountain, river, sky are – for versions of personalism – different gods, the characteristics and behaviour of these things being the expression of the intentions of these gods rather as the behaviour of a person is normally an expression of that person's intentions. Other versions of personalism hold that there is but one God, the whole world being the expression, the outcome, of God's will.

Physicalism, in sharp contrast to personalism, holds that the world is entirely impersonal in character. According to physicalism, although there appear to be very many different sorts of things and phenomena in the world, changing and interacting in apparently very many different and often arbitrary ways, in reality the world is made up of only a very few different sorts of things (atoms, point-particles, fields or whatever) which change and interact in only one, precise, fixed way. That which does not change, X, precisely determines the manner in which that which changes, Y, does change (both X and Y being properties of the basic physical entities out of which everything is composed). In the case of atomism, for example, X is the unchanging properties of space, time and atoms, whereas Y is the relative positions and motions of the atoms at any given moment.

There are other ways in which the universe may be conceived to be comprehensible. Personalism may be regarded as a special case of a more general view – *purposivism* – according to which the characteristics and behaviour of things can be understood in terms of purposes they realize (specific purposes contributing rationally perhaps to some overall cosmic purpose), these purposes not necessarily being conscious purposes, the purposes of person-like gods, or one God. Some versions of personalism accommodate aspects of physicalism – versions which hold that the material world behaves in accordance with physicalism but has been created to do so by God. Other versions of personalism may conceive of the material world as having been created by God to express thought and feeling, somewhat like a book or a work of art, the world thus being imbued with, and explicable in

terms of, Divine meaning or beauty. Alternatively, versions of personalism may conceive of God's intentions in creating the world more functionally, on analogy with a builder or architect.

These diverse conceptions of comprehensibility, despite their obvious differences, do have some things in common. They all agree that the comprehensibility of the universe has to do with its overall character, organization or structure. Each conception of comprehensibility takes some familiar, small-scale example of understanding – understanding a person, a persisting object or regularity, an artefact, a book or work of art, a pattern or machine – and projects this small-scale case of comprehensibility onto the entire universe. The character or organization of parts or bits of the world are taken to be indicative of the character or organization of the whole of the universe. Finally, comprehensibility may be held to be linked to *knowability*. To the extent that something is incomprehensible to us, we cannot readily acquire knowledge about it: to the extent that we do understand it, we can readily improve our detailed knowledge of it as needed. If we have a good overall understanding of a person, we can quickly come to know what the person is feeling, thinking, intending, doing, on particular occasions: if we lack such understanding, our ability to acquire such knowledge is much more limited and fallible. The diverse classic conceptions of comprehensibility may thus be said to take some familiar small-scale case where knowledge is successfully progressively acquired and improved owing to the existence of some kind of persisting order or structure; this small-scale order or structure is then projected onto the whole world.

It is, I suggest, in the light of these considerations, reasonable to hold that there is a general notion of comprehensibility, common to all the diverse, more specific notions. Quite generally, to say that the universe is (in some way or other) comprehensible – as the term is used here – is to assert that there is some kind of overall uniformity, lawfulness, order, coherence, pattern, meaning or plan to the universe, in terms of which particular things and events can in principle in some way be explained and understood. It is to deny mere disorder, incoherence, arbitrariness or randomness. It is to assert that characteristic reasons or explanations exist (even if not known) for the way things are, for the way things change (in terms of the underlying order or intelligibility pervading the world). To assert that the universe is in some way or another comprehensible is to assert that there exists something – the characteristic order or meaning of the universe – which pervades all that there is. In conceiving of a possible universe that is in some way comprehensible we at the same time, implicitly or explicitly, conceive of a vast number – usually infinitely many – possible universes that are not

comprehensible in this way, in that in them the characteristic kind of order or pattern involved does not pervade all that there is. In a comprehensible universe, in other words, all merely *aberrant* theories, of the kind discussed in the previous section, are false. To say that the universe is comprehensible is to make a substantial (but probably untestable or metaphysical) assertion about the overall character or nature of the universe: the more specific and precise the sense in which the universe is asserted to be 'comprehensible', so the more substantial and precise the assertion becomes (and *vice versa).*

Comprehensibility is such, so we may hold, that in a comprehensible universe any fragment of the universe contains clues or guidelines as to the character of the whole of the universe – somewhat as a small piece of patterned wallpaper contains clues as to the whole wallpaper. Any fragment of a comprehensible universe will exemplify the characteristic kind of uniformity, order, pattern or meaning that pervades the whole: it is this, exemplified in any fragment, which provides clues or guidelines to the nature of the rest of the universe.

I turn now to a consideration of the all important questions: to what extent is it true that the comprehensibility of the universe, in the sense indicated, is a necessary and sufficient condition for the universe to be *knowable,* for it to be rationally possible at least for genuine knowledge to exist and grow? Does this provide an adequate rationale for committing physics to the presupposition that the universe is comprehensible? Or can such a rationale be provided in some other way?

To begin with, it seems reasonable to hold the following. In a universe that is in some way comprehensible, in the sense indicated, there is at least a rational hope or possibility – as opposed to an infinite improbability – that knowledge can be improved. For, so we may argue, the uniformity, order, pattern, meaning or plan that pervades the whole of a comprehensible universe makes it rationally possible that even *imperfect* knowledge of a *part* of the universe can become more perfect knowledge of the whole universe. Since in any fragment of a comprehensible universe there exist clues or guidelines as to the nature of the rest of the universe, it is always reasonable to suppose that imperfect knowledge of such a fragment will itself contain, explicitly or implicitly, clues or guidelines as to the nature of the rest of the universe – clues or guidelines which make possible the progressive extension and improvement of knowledge. In such a universe, in other words, there can exist a body of knowledge which contains, explicitly or implicitly, heuristic and methodological rules (reflecting, perhaps imperfectly, the comprehensible character of the universe) designed to make possible, to promote, the improvement of knowledge.

On the other hand, we may also argue that in a wholly incomprehensible universe, knowledge cannot grow, or even exist. In a wholly chaotic universe, devoid of any kind of uniformity, order, pattern or meaning even to a limited extent in even a limited region, there cannot be persons, language, life, goal-pursuing or knowledge of any kind. For all these things exemplify and thus require for their existence at least some limited degree of order, pattern or meaning of some kind or other: and just this has been denied.

In a comprehensible universe, the growth of knowledge is a rational possibility: in a wholly incomprehensible universe, knowledge cannot even exist, let alone grow. These considerations rationally entitle us to commit science to the presupposition that the universe is in some way comprehensible, to some extent at least. Ultimately all our knowledge is conjectural in character. The assumption that the universe is comprehensible to some extent is no exception: it is a conjecture which we cannot prove to be true, or conclusively verify. On the other hand we can be quite certain that the opposite assumption 'The universe is wholly incomprehensible' cannot become a part of scientific knowledge, since if this is true there can be no science and no knowledge. We thus risk nothing in holding that 'The universe is comprehensible to some extent' is a solid, undeniable item of scientific knowledge. If this proposition can be asserted, it cannot be wrong.

This argument does not establish, however, that we are rationally entitled to commit physics to the presupposition that the universe is *wholly* comprehensible. It is logically possible that we might exist and possess knowledge, and science might meet with success, even though only a *part* of the universe is only *approximately* comprehensible. There are at least four general possibilities to consider. (1) Our space-time region of the universe is comprehensible, but elsewhere incomprehensible phenomena occur. (2) Comprehensibility extends throughout all regions of space and time, but is intermittently violated in that occasional incomprehensible, miraculous events occur. (3) Comprehensibility is restricted to the kind of phenomena that can be produced or observed on or from the earth: there are extreme phenomena that lie outside this range (at high energies, for example, or high densities of matter) that are incomprehensible. (4) The universe is only approximately or asymptotically comprehensible, in the sense that it is such that theoretical physics must always be infinitely far from its goal, infinitely many revolutionary theoretical developments being needed before a unified theory could be formulated which specifies precisely how the universe is

comprehensible.[5] Now, on what grounds do we reject these and accept instead the following? (5) The universe is in some way wholly and precisely comprehensible, without exceptions. It has been established that some degree of comprehensibility must be presupposed to exist by science. What rationally entitles us to commit science to the complete comprehensibility of (5), as opposed to the partial comprehensibility of (1), (2), (3) or (4)?

In answering these questions, one important preliminary point needs to be appreciated. On the face of it, it might seem more modest, more cautious, and therefore more rational, for physics to assume no more than partial and approximate comprehensibility, rather than (some unknown kind of) complete and precise comprehensibility, pervading all that there is. This impression of greater modesty is, however, somewhat illusory. In assuming only that the universe is at least in part approximately comprehensible, we thereby assume that the rest of what there is, even if not wholly comprehensible, nevertheless is sufficiently stable and well-behaved not to disrupt the partial comprehensibility in existence. Not just science, but even our most trivial, common-sense claims to knowledge contain, explicitly or implicitly, presuppositions about the entire cosmos, to the effect that all that exists of which we are ignorant is sufficiently stable and well-behaved not to disrupt the small bit of the universe we claim to know and understand. Insofar as we know anything about anything (and we cannot live or do science unless we possess some knowledge), we also know something at least about the entire cosmos, all that there is. And the more extensive and precise our scientific claims to knowledge are (however conjectural), so the more restricting and precise must be our assumptions about the character of all that exists of which we are largely ignorant.

What needs to be established, then, is that it is more rational for physics to presuppose, as conjectural knowledge, that the universe is, in some way, wholly and perfectly comprehensible, rather than only partly, approximately comprehensible, and elsewhere at least sufficiently stable to permit approximate comprehensibility to persist.

Here is my argument in support of this contention.

The refutation of standard empiricism above showed conclusively that physics, in order to progress, in order to develop (conjectural) theoretical knowledge, must persistently reject empirically successful, *aberrant* theories. This persistent rejection of empirically successful, aberrant theories means that physics implicitly or explicitly, presupposes that the world is non-

[5] I consider 20 different kinds of partially comprehensible universe compatible with modern science in Maxwell (1998, pp. 169-171).

aberrant – that it is, in some way or other, comprehensible. It is possible to pursue physics in such a way that this presupposition of non-aberrance or comprehensibility is left as a vague, implicit, even unacknowledged, but nevertheless profoundly influential assumption. Physics pursued in this way is, however, seriously irrational, seriously lacking in intellectual rigour, just because a substantial, influential and profoundly problematic assumption is not explicitly formulated and explicitly criticized. Quite generally, *an essential requirement for rationality, for intellectual rigour, is that substantial, influential and problematic assumptions be made explicit so that they are open to criticism and thus, one may hope, improvement.* Thus, if physics is to be genuinely rigorous and rational, it is essential that the substantial, influential and profoundly problematic assumption of non-aberrance or comprehensibility be formulated explicitly in as clear, bold, decisive, precise, extensive, extreme and contentful a way as possible, so that it may be subjected to the maximum degree of sustained criticism, in this way, we may hope, becoming progressively improved. Our best scientific conjecture as to how the universe is perfectly comprehensible, put forward at any stage in the development of science, is the tentative spearhead of research into the unknown, the probing searchlight we shine into the darkness ahead in the hope of lighting up our way. A conjecture postulating perfect, precise comprehensibility, as opposed only to partial, approximate comprehensibility, is to be preferred – is more rationally acceptable – (other things being equal) because it offers more, because it is potentially more helpful for the progress of physics, and because it is more *vulnerable to criticism,* more open to being found wrong (in ways to be discussed) should the universe be comprehensible in some other way.

In pursuing physics we are rationally entitled to adopt any (morally acceptable) procedure which can only help, and cannot hinder, scientific progress, the acquisition of genuine knowledge and understanding. Consider now the following procedure: formulate the best possible conjecture as to how the universe is perfectly and precisely comprehensible, in as explicit and definite a form as possible, in the light of all that is known and understood, this conjecture being *both* persistently criticized, *and* employed to assess the acceptability of empirically successful physical theories. Adopting this procedure, I claim, can only help, and cannot hinder, the progress of physics, whether the universe is in reality perfectly comprehensible, only partially comprehensible, or wholly incomprehensible. For if the universe is perfectly and precisely comprehensible, the procedure indicated can clearly only aid the progress of physics. If the universe is only partially comprehensible, the procedure indicated can still only aid, and cannot hinder, the progress of physics for in this case, in order to improve knowledge and understanding,

we can do no better than to seek for perfect comprehensibility, and fail. To assume only partial comprehensibility at any stage cannot help, and may well hinder progress in that there may exist more comprehensibility (and therefore more potential growth of knowledge) than we have allowed. We can only discover that the universe is only partially comprehensible (to the extent that this can be discovered) by persistently seeking for perfect comprehensibility, and failing in the attempt. Finally, if the universe is wholly incomprehensible, there can be no life, no knowledge and no science: thus even in this case, the procedure indicated cannot hinder scientific progress (since in this case there can be no science at all). Therefore in pursuing physics we are rationally entitled to adopt the procedure indicated.

I conclude that for physics to be intellectually rigorous and rational it is essential that physics explicitly formulates and criticizes, as a part of scientific knowledge, the best possible conjecture, at any stage, as to how the universe is wholly and precisely comprehensible, in the way just indicated. Furthermore, whenever there is some acceptable notion of degrees of comprehensibility, physics is entitled to assume that the universe is comprehensible to the highest degree, other things being equal.[6]

This argument, this proposed solution to the problem of induction, the traditional problem of the rationality of science, might be said to be Kant's attempt at a solution radically improved in the light of Popper's or, alternatively, Popper's attempt at a solution improved somewhat in the light of Kant's. The argument takes seriously Kant's central point that in order to

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[6] Some further aspects of this proposed solution to the traditional problem of the rationality of science can be found in Maxwell (1972a, pp. 131-52; 1974, pp. 123-53 and 247-95; 1979, pp. 629-53). Others have advocated conjectural, presuppositional approaches to solving the problem of induction. See, for example, Russell (1948, pt. VI); Burks (1977, ch. 10). My criticism of these attempts is that they attempt to justify as rational what is actually irrational, namely that science should make a too narrowly conceived, fixed metaphysical presupposition about the nature of the world, and should adopt fixed methods. They fail to characterize science as learning in what way the universe is comprehensible as it proceeds. They fail, in other words, to advocate aim-oriented empiricism. The solution to the problem of induction, that I proposed here in 1984, has been developed and refined subsequently to overcome objections to the above: see Maxwell (1998, especially ch. 5; 2004b, appendix). See also the present work, chapter 14, section 6.

solve the problem of knowledge, highlighted especially by Hume, we need to take into account what must be the case if knowledge is to exist at all, the necessary conditions for knowledge to be possible. But it also accepts Popper's point that all our knowledge is ultimately irredeemably conjectural in character. The metaphysical theory 'The universe is wholly and perfectly comprehensible' is the only proposition that might remotely exemplify Kant's key notion of a 'synthetic *a priori*' proposition – a proposition about the world known independently of experience. However, it exemplifies Kant's notion in only the weakest possible sense. The proposition is wholly conjectural in character (and not something that can be known to be true with certainty, as Kant would insist). Insofar as we have a good reason to accept this proposition as a part of our conjectural scientific knowledge *on grounds that are independent of experience,* this can only be done if the proposition is interpreted in the loosest, vaguest way conceivable – 'comprehensible' having only its very general meaning indicated above. In order to have good reasons to accept a more precise version of the proposition or theory – physicalism, for example, or some version of physicalism such as atomism – it is essential to appeal to experience, to the actual development of empirical science. (Thus nothing as precise as 'Space is Euclidean' can be a synthetic *a priori* proposition in even the weakest sense.) The above argument also accepts Popper's point that the problem of knowledge is fundamentally the problem of the *growth* of knowledge. Actually even the mere *possession* of knowledge – scientific or common-sense – involves in a sense the acquisition of new knowledge, as time passes, and things persist and change. Even for animals, the key problem is the acquisition or growth of knowledge, the capacity to detect, for example, the presence of other animals from the minutest of signs or evidence, whether these animals be predators, food, rivals, a mate or offspring. Thus Kant's idea is transformed into the idea of specifying the best possible conditions that we are rationally entitled to postulate in order to render the *growth* of knowledge as rapid as possible. In addition, the above argument incorporates Popper's point that in order to solve problems of knowledge created by the sceptical arguments of Hume and others, we must forego the attempt to specify some restricted body of knowledge that is wholly immune to scepticism, that is absolutely certain, beyond all doubt. Instead of seeking to defend reason and knowledge by attempting to *refute* scepticism in this way, we need to recognize that scepticism is absolutely essential to reason and the growth of knowledge. It is precisely by exposing our claims to knowledge to ruthless scepticism, to persistent, savage criticism, that we can best hope to make progress. The endeavour to delimit, defuse or rebut scepticism is actually profoundly

irrationalist, profoundly damaging to the growth of knowledge. Nothing is immune to doubt – certainly not the theory that the universe is in some way or other comprehensible. We need, indeed, to be sceptical even of scepticism itself, of its capacity invariably to aid the growth of knowledge. But we must not be totally (uncritically) sceptical about scepticism, its value, its capacity to aid the growth of knowledge and understanding, or there is a danger that scepticism will destroy itself and lapse into its opposite, dogmatism. If one doubts the value of scepticism to the point that one doubts that it is possible by means of criticism to improve knowledge at all, to assess correctly the relative merits of rival claims to knowledge, there is always the danger that one will end up accepting some ostensible body of knowledge as being as good as any other, criticism being ignored on the grounds that all criticism is ultimately pointless. In practice this is extreme dogmatism. Extreme scepticism of the value of scepticism is thus to be resisted as self-destructive and counterproductive. And more generally, whenever it can be shown that any application of doubt or scepticism can only hinder, and cannot aid, the growth of knowledge and understanding, then we are rationally entitled to abstain from this kind of doubt, on pragmatic grounds. (In this way we are *rationally,* and not just destructively, sceptical of scepticism.) Popper himself employs this kind of pragmatic argument when, for example, he argues that we ought not to doubt all our knowledge all at once, since this sabotages the very possibility of improving knowledge (1963, pp. 238-9). The argument I have outlined above is an example of just such a pragmatic, rational, critical delimitation of scepticism, in that the argument is designed to establish that to doubt that the world is in some way comprehensible cannot help, and can only hinder, the growth of knowledge. Again, the argument outlined above accepts Popper's contention that we need to put forward and prefer (other things being equal) those ideas, those theories, which make the boldest, the most ambitious, substantial and extensive claims about the world, because in doing this we give ourselves the best chances of discovering error and making progress. The argument accepts, that is, Popper's contention that – as one might put it – scientific rigour involves being constrained to accept (other things being equal) our wildest, furthest flung imaginings. It is Popper's hope that this principle suffices to solve the problem of selecting the best theory in the light of evidence, granted that there will always be infinitely many rival theories compatible with all available evidence to choose from. According to Popper, we choose the most ambitious theory, the theory which asserts the most, is the most falsifiable. We saw above that this proposal fails. It fails because it is too timid, not bold enough. With even greater daring than Popper envisages, we need to conjecture that the entire

cosmos is in some way perfectly and precisely comprehensible, this conjecture being upheld with almost reckless audacity throughout all the ups and downs of empirical research – even though, of course, every empirical hint as to the particular way in which the universe may be comprehensible is eagerly seized on and exploited. Invariably we choose those empirically successful theories which accord best with our best conjecture as to how the universe is comprehensible. We are rationally entitled to do this just because it gives us our best hope of improving our knowledge and understanding of the world. Confronted by infinitely many possible universes, we can do no better than explore first all the perfectly comprehensible universes that we can imagine: only when we have exhausted all our ideas about comprehensible universes will there be any point in considering those that are partially, approximately comprehensible. Finally, and most important of all, the above argument accepts, and arises as the outcome of putting into practice, Popper's key point that *criticism* lies close to the heart of *rationality*. Physics pursued in accordance with some version of standard empiricism such as Popper's falsificationism, in selecting from infinitely many empirically successful theories only those few that are non-aberrant, must inevitably, implicitly, be accepting a massive, profoundly influential metaphysical conjecture about the world, to the effect that the world is non-aberrant. This metaphysical conjecture is built into the actual (as opposed to the declared) methodology of physics – into the actual practice of rejecting empirically successful aberrant theories out of hand. This massive and profoundly influential conjecture about the nature of the world cannot however be criticized explicitly within science just because of the metaphysical (or untestable) character of the conjecture. The intellectual standards of standard empiricism, and especially falsificationism, are such that untestable, metaphysical conjectures are to be excluded from science. Aim-oriented empiricism, by contrast, insists that our best more or less specific conjecture as to how the universe is comprehensible or non-aberrant, in the light of present knowledge and methods, must be formulated explicitly as a vital part of scientific knowledge, precisely so that it can be criticized, and thus, we may hope, improved. Physics pursued in accordance with aim-oriented empiricism is thus more rigorous, more rational, than it is when pursued in accordance with any version of standard empiricism, including falsificationism, just because aim-oriented empiricism insists on, and standard empiricism prohibits, sustained, explicit criticism of a massive conjecture about the world inevitably exercising a profound influence over scientific thought. Furthermore, as we shall see, this enhancement of intellectual rigour, of scientific rationality, has important consequences for the way we do, teach

and understand science. Adopting aim-oriented empiricism encourages, and adopting standard empiricism discourages, improvement of aims and methods as science proceeds — vital for scientific progress.

In short, if the solution to the problem of induction offered here faintly echoes Kant's, it does so only by being even more radically Popperian than Popper's own attempt at a solution.

It is perhaps just worth noting that there are two additional ways in which the solution to the problem of induction offered here differs from Kant's. According to the solution offered here, it is ultimate reality, the world as it is in itself, remote from human experience – Kant's *noumenal* world – that we are rationally entitled to claim to know to be in some way comprehensible, in an *a priori* but conjectural manner. For Kant, all knowledge, including all *a priori* knowledge, is only about the *phenomenal* world, the world of possible experience, it not being possible to know anything about noumena except that they exist. The solution offered here thus rejects Kant's 'Copernican revolution' (Kant 1961, p. 22). The world is not comprehensible to us because what we call the world conforms to our minds: rather, it is only comprehensible to us if we allow it to teach us in what way it is to be understood. Instead of our mind shaping the (phenomenal) world to conform to our ideal of comprehensibility, we must seek to shape our ideal of comprehensibility to conform to the character of the (noumenal) world. Kant's mind-centred anthropomorphism (actually wholly non-Copernican in spirit) is replaced by the anti-anthropomorphism of modern physics. As the physicist John Wheeler has emphasized, in order to understand the world, we must be prepared to recognize how unfamiliar, how mysterious it is.

It is perhaps also just worth noting that other, somewhat less Popperian arguments may be given in support of the contention that we are rationally entitled to hold, as a part of scientific knowledge, that the universe is wholly and not just partially, comprehensible. Modern physics is based, so we may argue, on the presupposition that the universe is impersonal in character, there thus being nothing privileged about humanity's position in the cosmos, as far as the ultimate nature of the cosmos is concerned. This anti-anthropomorphic presupposition requires us to hold that comprehensibility (not necessarily precisely as characterized by us at present) is not confined to our particular region of space-time or phenomena, but is present throughout all places, times and phenomena. Again, we may argue that physics quite properly seeks understanding as being of value in itself, and not merely as an aid to the acquisition of knowledge. Granted that we seek understanding, we are entirely justified, on pragmatic grounds, in holding that what we seek exists, since the only way we can discover it does not exist, is to search for it

and fail to discover it. Postulates of partial comprehensibility are in this case even more obviously unhelpful. Finally, it may be argued that the possibility that the universe is only partially approximately comprehensible is to be rejected because of its wild implausibility. In any such universe, there can be no reason, no explanation, as to why limited chaos does not spread like a contagion to engulf all order. Once some inexplicable events occur, there can be no reason why all events should not be inexplicable. Sustained order could only be an infinitely improbable accident. Order of this kind, without any underlying *raison d'etre,* is too absurd to deserve a moment's consideration. (But for second thoughts on this issue see chapter 14 and Maxwell 1998, ch. 5.)

What all this amounts to, then, is that physics in particular, and natural science more generally, insofar as they have sought knowledge for its own sake, have suffered from rationalistic neurosis (in the sense indicated in chapter 5). The actual aim of natural science, quite properly, is to improve our knowledge and understanding of a world presupposed to be comprehensible. The declared aim (as far as most scientists and philosophers of science are concerned) has been to improve knowledge about the world, no presupposition being made about the nature of the world (theories being selected, in the end, solely with respect to empirical success and failure). This declared aim and method of science, standard empiricism, being widely upheld, has exercised a profound influence over science itself. Many physicists, and scientists more generally, do of course declare a personal faith in the comprehensibility of the universe: this is however at most a personal conviction, upheld in the context of discovery (where everything is permitted). The decisive point is that modern science as a whole does not hold the metaphysical theory 'The universe is comprehensible' to be a vital part of public, objective scientific knowledge, in the context of verification. The declared aim, in the context of verification, is the discovery of *truth,* and not *explanatory truth* (it being mistakenly held that in this context the truth cannot be presupposed to be explanatory).

This long-enduring rationalistic neurosis of science has had all the usual damaging consequences mentioned in chapter 5. The more 'rationally' or honestly science pursues its declared aim, so the worse off it is: scientific success seems to require some measure of irrationality. The attempt to construe science as rational, given its declared aim, can only be counter-productive. Such an attempt seeks to solve counter-productive neurotic problems (the problems indicated in chapter 5 during the discussion of rationalistic neurosis). This explains why so much orthodox philosophy of

science, presupposing standard empiricism, is so unfruitful from the standpoint of science itself, and is treated so dismissively by so many scientists. All this helps to ensure that the neurosis persists since reason (or critical concern for basic aims and methods) becomes discredited. In moving from standard empiricism to aim-oriented empiricism we transform the neurotic, insoluble, and scientifically sterile problems discussed above into problems whose solutions are potentially profoundly fruitful for science itself.

I now indicate twelve ways in which physics is transformed, and improved, as a result of rejecting standard empiricism and proceeding explicitly, and not just covertly, in accordance with aim-oriented empiricism.

1 Instead of there being just two (interacting) domains of scientific knowledge, there are three: (a) experimental results; (b) testable laws and theories; (c) metaphysical blueprints (at any stage the best idea as to how the universe is comprehensible). Instead of being separate from science, metaphysics and philosophy become a vital, integral part of scientific knowledge, essential to the intellectual rigour of science. Physics is transformed into natural philosophy.

2 Instead of having a fixed aim and fixed methods, physics has *evolving aims and methods*. This is essential to scientific rationality, and scientific progress. Even though we know (conjecturally) that the universe is comprehensible, we do not know in what precise way it is comprehensible. This we must find out, by discovering which conjectures about comprehensibility lead to the most rapid growth of knowledge when judged in terms of common, implicit criteria as to what constitutes knowledge. A change in aim, in metaphysical blueprint, can lead rationally to a dramatic change in methods. Given any version of personalism, whether multi-theistic or monotheistic, it is rational to attempt to know and influence the natural world by means of methods that are appropriately applied to powerful people – parents, chiefs, kings. Thus prayer, sacrifice and ritual constitute entirely rational methods. Rejection of personalism and adoption instead of some version of physicalism leads, rationally, to a dramatic change in methods. Furthermore, a change in the version of physicalism that is adopted is almost bound to lead to a change in methods. The relatively recent explosive growth of scientific knowledge and understanding during the last 200 years, or even during the last few decades, has only been possible, I suggest, because of the progressive improvement of the aims and methods of science: the adoption of a good aim for science by Kepler and Galileo, suggesting good methods and their

subsequent development. As knowledge has improved, so too knowledge about how to improve knowledge has improved.

3 Standard and aim-oriented empiricism lead to quite different views as to how science ought ideally to develop (see figures 9a and 9b). Our starting point is a tribe accepting without question some version of personalism – a(1) and b(1). According to standard empiricism, the ideally rational way to proceed is to divide knowledge up into the observational and theoretical, a(2), discard the theoretical, apart perhaps for heuristic purposes to suggest possible testable theories, a(3), and then to put forward and test testable theories, a(4), thus creating science. According to aim-oriented empiricism, the first major step is to develop a rival cosmological theory, b(2), or to create a society in which two rival cosmologies (or religions) coexist, perhaps as a result of two hitherto distinct tribes living together. Only when this has been achieved is there even the possibility of making a distinction between theory and observation, theory and common-sense (the latter being what is common to the two conflicting theories). The next major step is to discover that individuals can freely invent new cosmological theories and freely modify existing theories, b(3), a step first taken in history, it may be held, by the Presocratic philosophers – Thales, Anaximander, Anaximenes, Xenophanes, Heraclitus, Parmenides, Empedocles, Anaxagoras and Democritus. It is perhaps not surprising that this step should be associated with another, the discovery of the impersonal character of the world, the abandonment of personalism and the slow, laborious development of physicalism (the Presocratics being concerned above all with the central problems of physicalism, namely 'what does not change?' 'what does change?' 'how are the two interrelated?'). For, in a personalistic world, denial of the existence of gods, or of God, might be a dangerous matter, whereas in a physicalistic world, mere denial of the existence of atoms, for example, is harmless. This major step creates a major new problem. How can it be discovered which of a multitude of rival religions and cosmologies is true, especially as all are compatible with common-sense (almost by definition)? The solution to this problem is to stipulate that all acceptable cosmologies must postulate that the world is in some way comprehensible, in the sense explicated above, step b(4)). All rival cosmologies agree that in any bit of the world there are clues or guidelines as to the character of the whole, and that implicit in imperfect knowledge of a bit of the world there are clues or guidelines as to the character of the whole. Each cosmology attributes a different kind of comprehensibility to the world, and thus holds that a

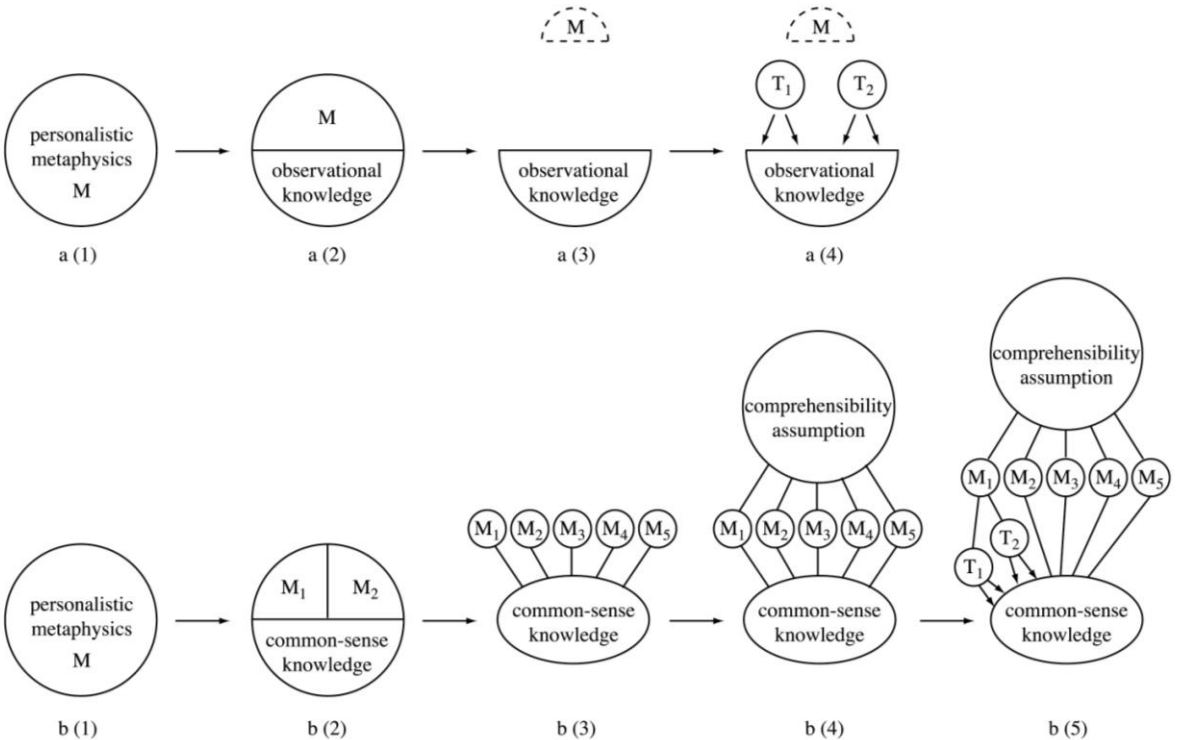

Figure 9 (a) Standard empiricist evolution of science
Figure 9 (b) Aim-oriented empiricist evolution of science

different kind of *methodology* needs to be adopted in order successfully to extend and improve (common-sense) knowledge.

The primary way to choose between rival cosmologies is to discover which associated methodology is best able to promote the growth of knowledge, when judged by means of implicit, common-sense terms. The discovery that there is one cosmology and associated methodology that has a far greater capacity to promote the growth of (common-sense) knowledge than any of its rivals is the discovery of science, step b(5). This discovery was made in the seventeenth century, above all by Kepler and Galileo. The cosmology is physicalism: that which is invariant is postulated to be such that it is capable of being characterized by means of some physically interpreted mathematics, from which (together with initial conditions), descriptions of that which varies can (in principle) be deduced. As Galileo put it, 'The book of Nature is written in the language of mathematics.' The method is to put forward mathematically precise conjectures as to what is invariant through change, for example motion, and to test these conjectures by means of precise observation, experiment, measurement. Kepler's laws of planetary motion, Galileo's laws of terrestrial projectile motion, and Newton's unifying laws of motion and gravitation, are the great early successes of this cosmology and associated methodology. Subsequent science continues to develop and select much more restrictive cosmologies and associated methodologies in much the same way, in terms of their capacity to promote the growth of knowledge, within the much more restrictive physicalist conception of comprehensibility. In this way the aims and methods of science improve as scientific knowledge improves.

Unlike the steps advocated by aim-oriented empiricism, those advocated by standard empiricism are either impossible or disastrous to take. Thus step a(2) cannot be done, as there can be no basis for distinguishing between theory and observation unless rival theories exist. Step a(3) is an intellectual disaster, as it involves abandoning all theoretical knowledge, together with the indispensable conjecture that the world is comprehensible. It is not in the least surprising that, after these intellectual disasters, step a(4) fails entirely to provide a basis for the rational growth of knowledge (as we have seen above). The traditional problems of the philosophy of science that arise if some version of standard empiricism is taken for granted are insoluble because these problems presuppose a context for science – step a(4) – which is itself the outcome of prior, usually unacknowledged, but absolutely disastrous intellectual developments, steps a(2) and a(3).

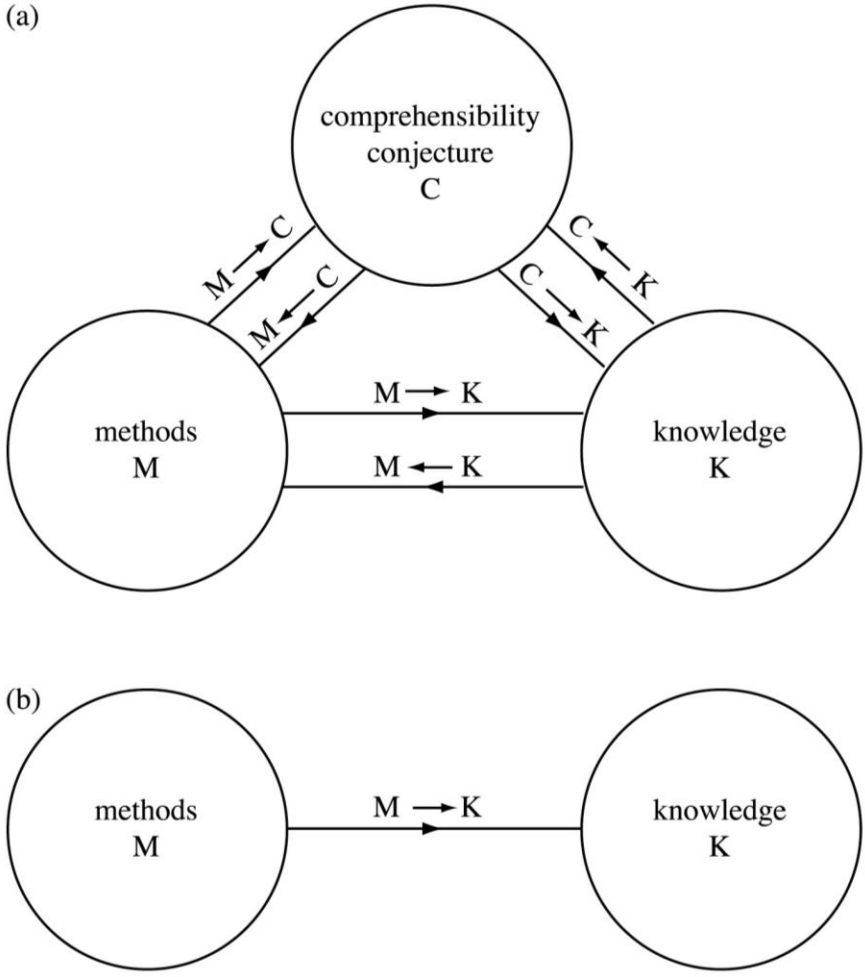

Figure 10 (a) Evolving aims, methods and knowledge interacting in accordance with fixed meta-methods of aim-oriented empiricism
Figure 10 (b) Fixed methods of standard empiricism

4 According to standard empiricism, the critical study of the aims and methods of science – the philosophy of science – is to be sharply distinguished from science itself, just because ideas as to what the aims and methods of science ought to be are not, in any straightforward sense, empirically testable theories. According to aim-oriented empiricism, if science is to be rational, it is vital that the attempt to improve the aims and methods

of science (the philosophy of science) be an integral part of science itself. In dissociating the study of aims and methods from science itself, and pursuing it as a meta-discipline, standard empiricism helps to undermine the very thing it seeks to understand: namely the rationality of science.

Natural philosophy (the discipline that emerges, or is recovered, as a result of integrating science and the philosophy of science in the way required by aim-oriented empiricism), can be represented as being made up of three interacting components. There is *knowledge* K (the sum total of accepted observational and experimental results, testable laws and theories); there is the best conjecture as to how the universe is *comprehensible,* C; and there are the currently accepted best *methods* of scientific inquiry, M. The fixed, meta-rules of natural philosophy specify how these three components ought to interact (see figure 10a): they specify how we should seek to modify each component to bring it into better accord with the others in such a way as to improve our knowledge and understanding of the world. These six meta-rules (to be established by critical practice and tradition in science) can be indicated as follows. K → M: modify explicit methods M so that they more accurately capture methods which have actually been responsible for the growth of K in history. M → K: modify K by putting M into practice in order to generate new knowledge, and assess existing knowledge. K → C: modify C so that C constitutes the best conjecture as to how the universe is ultimately comprehensible, in the light of all fundamental physical theories, their best interpretation, their common invariance principles, concepts and presuppositions, their inconsistencies and inadequacies. C → K: reformulate the fundamental theories of K in the light of C, so as to accord better with C. C → M: modify M so that M become the best methods to adopt in order to improve knowledge in a universe that is comprehensible in a C-type way. M → C: modify C so that according to C, the universe is comprehensible in just that way which make M the best methods to adopt in order to improve knowledge. The outcome of putting these six rules into practice will be to develop gradually alternative M's, C's and interpretations of K, until eventually alternative testable physical theories are developed. Crucial experiments are then to be performed (as all M's will insist). In comparison with this rich interplay of knowledge, methods and ideals of comprehensibility, the starkness of standard empiricism is glaringly apparent, with its one fixed set of methods M assessing knowledge, and excluding ideas of comprehensibility from the intellectual domain of science on the grounds that such ideas constitute unscientific, untestable metaphysics (see figure 10b).

5 The six meta-rules of aim-oriented empiricism just indicated provide natural philosophy with a rational, even if fallible and non-mechanical, method of discovery. No such thing is possible within the framework of standard empiricism, just because ideas of comprehensibility C are excluded from the intellectual domain of science. It has only been possible for science to make progress because some scientists have put the rational methods of discovery of aim-oriented empiricism into practice despite the prohibitions of standard empiricism. Widespread adoption of standard empiricism has had the consequence that the above six meta-rules of discovery have not been put into practice explicitly and cooperatively by the community of scientists with full understanding of their rationale. Instead only a few individual scientists have been able to exploit these rules in their work with success: the great theoretical innovators of science, and above all Einstein.

6 Most current versions of standard empiricism, after the work of Popper (1959, 1963), Kuhn (1962) and Lakatos (1970), recognize abrupt discontinuities in the development of science at the highest theoretical level. Aim-oriented empiricism, by contrast, recognizes continuity at the highest theoretical level of science. From Thales, Anaximander and Democritus, via the work of Kepler, Galileo, Newton, Dalton, Fresnel, Faraday, Maxwell, Darwin, Boltzmann and Planck to Einstein, Schrödinger, Watson, Crick, Salam, Weinberg and Gell-Mann, there is the gradual clarification and development of one basic idea, physicalism: there is that which does not change or vary, X, which determines (deterministically or probabilistically) how that which changes, Y, does change. All major theoretical developments of science can be interpreted as enabling us to understand, in ever greater detail and with ever greater precision, how more and more apparently diverse phenomena are the outcome of relatively few different sorts of entities interacting by means of ever fewer different sorts of invariant forces (at present described by the three or four fundamental dynamical theories of modern physics). Even Darwin can be interpreted as solving a major problem confronting physicalism namely: how is it possible for well adapted, purposive life to develop in a purposeless universe?

It should be noted that in order to make sense of the basic idea of physicalism, (that that which does not change *determines* change) we need to be able to interpret appropriate physical theories as attributing necessitating properties or powers to the physical entities they postulate. This in turn requires that Hume's famous analysis of causation, his denial of the

possibility of necessary connections between successive events, be rejected. Elsewhere I have shown how this can be done.[7]

7 On the face of it, however, there are abrupt discontinuities between successive more specific metaphysical blueprints of physics – for example, between the corpuscular blueprint of the seventeenth century, the point-particle blueprint of Newton and Boscovich, the particle/field blueprint of Faraday, Maxwell and Lorentz, the unified field blueprint of Einstein.

Each of these blueprints can, however, be interpreted as *generalizing* its predecessor (somewhat as Riemannian geometry generalizes Euclidean geometry), as long as they are all interpreted as specific versions of physicalism. Thus the corpuscle idea that there is an infinitely repulsive force located on the closed, rigid *surface* of each corpuscle is generalized by Boscovich into the idea that there is an alternatively repulsive and attractive force which varies in a fixed way throughout a *volume* about a central point-particle. This blueprint requires that changes be transmitted instantaneously from point-particle to point-particle through space. More generally, the velocity of such transmission may be *finite,* which means the state of the force-field around each particle will vary (depending on the past motions of the particle). In order to take this case into account, we may coalesce all the diverse force-fields of distinct particles together to form *one* force-field, created by, and acting on, point-particles. This point-particle/field blueprint, associated with Faraday, Maxwell and Lorentz, may in turn be modified by eliminating the particles and insisting that the field *interacts* with itself, small, intense regions of the field standing in for point-particles. This, in essence, is Einstein's unified field blueprint.[8]

The single theme of physicalism runs throughout this dramatic *evolution* of ideas: the basic physical entities (or entity), have invariant physical properties, specified by theory, which determine how the entities interact with one another, and evolve in space and time.

One harmful consequence of the standard empiricist prohibition on critical scientific discussion of such basic metaphysical ideas is that physicists have tended to hold onto outdated metaphysical ideas dogmatically, the best current theories being judged to be incomprehensible as a result (incomprehensibility even sometimes being held to be an inevitable

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7 See Maxwell (1968a), reprinted in Swinburne (1974, pp. 149-74), an anticipation of, and improvement over, ideas published subsequently by Dretske (1977), Tooley (1977) and Armstrong (1978, 1983). See also Maxwell (1998, pp. 141-55).

8 For a more elaborate version of this argument see Maxwell (1998, ch. 3, section 4).

consequence of theoretical advance – predictive success being the most that can reasonably be expected from physical theory, according to those who hold such *metaphysically* unenlightened views). Thus many of Newton's contemporaries (such as Huygens and Leibniz) condemned his law of gravitation as incomprehensible for failing to be a corpuscular, action-by-contact theory. In a sense even Newton took this view. Judged in terms of Boscovich's more general point-particle blueprint, however, Newton's theory is entirely comprehensible. Again, many nineteenth-century physicists sought to interpret the electromagnetic field theory of Faraday and Maxwell in terms of an underlying material substratum or aether, hoping thereby to make the theory comprehensible, appealing in effect to corpuscular or point-particle ideas of comprehensibility. In terms of Faraday's more general field blueprint, however. Maxwell's electromagnetic theory is comprehensible as it stands: the aether,if anything, can only undermine comprehensibility. An analogous situation has arisen in connection with quantum theory, which is often deemed to be inevitably incomprehensible, no more than an algorithm for predicting experimental results, insofar as it cannot, be understood in terms of outdated particle or field ideas.

8 It is above all in connection with Einstein's development of special and general relativity that the rational method of discovery of aim-oriented empiricism is first self-consciously put into practice in science with brilliant success. What we find in Einstein's work is precisely the subtle interplay between evolving theory, evolving methods and evolving metaphysical ideas of comprehensibility (or unity), stipulated in (4) above. As a result in part of Planck's quantum theory of blackbody radiation, Einstein became increasingly aware of a fundamental theoretical and blueprint inconsistency in classical physics: the problem of how continuous electromagnetic radiation can interact with discrete, particle-like matter – the problem of reconciling, as it were, Newton and Maxwell, Boscovich and Faraday. Einstein, meta-physically enlightened, accepted the aetherless, field blueprint interpretation of Maxwell's electromagnetic theory. He sought to clarify the nature of the conflict between Maxwell and Newton by extracting from each theory a basic principle, the two principles being mutually incompatible. From Newtonian theory he extracted the (restricted) *principle* of relativity: all laws have the same form with respect to inertial reference frames. This can be interpreted as a *methodological* principle, corresponding to the *metaphysical* principle that physical space is such that a material body cannot have a velocity relative to space itself but only to another material body (the decision as to which is in motion, which at rest, being purely terminological and therefore irrelevant

from the standpoint of the character of physical laws). This principle of relativity is only sensible to accept if the aether does not exist (since it is reasonable to suppose that rapid motion with respect to the aether would have physical effects, and would thus be detectable). From Maxwell's theory Einstein extracted the physical law: the velocity of light is a constant (it being, as we have seen, basic to the field-theoretical idea that there is some finite velocity of transmission of influences). Einstein's problem had thus become the problem of discovering how these two postulates can be made mutually compatible. Einstein discovered that the two postulates can be made compatible if Newton's ideas about space and time are appropriately modified. As a result of making measurement of length, time and mass frame-dependent, the velocity of light can be arranged to be the same in *all* reference frames. This is Einstein's special theory of relativity. This theory leads to a new *methodological* principle, Lorentz invariance, which Newton's theory of gravitation fails to satisfy. A relativistic theory of gravitation is needed. The restricted principle of relativity can be generalized to assert: the laws of nature have the same form with respect to all mutually accelerating reference frames. We can make sense of this idea if we postulate that the effects of acceleration, and gravitation at rest, are equivalent (so that an accelerating reference frame is transformed into a stationary reference frame *in a gravitational field).* We thus arrive at the *methodological* and *metaphysical* principle of equivalence of acceleration and gravitation (implicit in a limited form in Newtonian theory). Consider now a large, flat, rapidly rotating disc. According to special relativity, a rigid rod transported from the centre to the circumference will seem to shrink. The geometry of the disc, as measured by the rod, will thus be non-Euclidean. Acceleration affects geometry: hence, by the principle of equivalence, gravitation affects geometry. Mass – or, more generally, energy – may be postulated to affect the geometry of space-time. The field equations of general relativity are the simplest equations which encapsulate this idea, specifying precisely how energy modifies the geometry of space-time. Gravitation *is* the energy-induced curvature of space-time.[9]

After developing general relativity, Einstein abandoned his aim-oriented methodology of discovery in one respect: his (correct) conviction that

[9] For more detailed discussions of Einstein's scientific journey towards special and general relativity see Schilpp (1970); Holton (1973); Pais (1982). For a more detailed discussion of Einstein's exploitation of aim-oriented empiricism in discovering special and general relativity, see Maxwell (1993b, pp. 275-305).

orthodox quantum theory is unsatisfactory led him to ignore it from a heuristic standpoint, whereas he ought to have tried to extract basic, mutually incompatible principles from general relativity and quantum theory, in an attempt to develop a new unified general relativistic quantum theory.

9 Not only did Einstein invent and brilliantly exploit aim-oriented empiricism in his extraordinarily successful scientific work: he also advocated the view. Again and again in his writings he emphasized the importance of the conjecture for science that the universe is comprehensible, and the profoundly problematic character of this conjecture. Typical are the following remarks. 'The most incomprehensible thing about the universe is that it is comprehensible' (Hoffmann, 1972, p. 18). 'It is the very essence of our striving for understanding that, on the one hand, it attempts to encompass the great and complex variety of man's experience, and that on the other, it looks for simplicity and economy in the basic assumptions. The belief that these two objectives can exist side by side is, in view of the primitive state of our scientific knowledge, a matter of faith. Without such faith I could not have a strong and unshakable conviction about the independent value of knowledge' (Einstein, 1973, p. 357).

10 Einstein's aim-oriented empiricist way of doing physics exercises a profound influence over modern theoretical physics. The heuristic use of existing theories to develop new theories (special relativity and quantum theory being used to develop quantum field theory, quantum chromodynamics); the formulation of fallible symmetry and invariance principles, such as parity (rejected) and gauge invariance; the drive towards theoretical unity so striking in recent developments: all of this is thoroughly Einsteinian in character and, as Wigner has remarked (1970, p. 15), stems from Einstein's own work. Most contemporary physicists have not, however, renounced standard empiricism: as a result there is still no official place within physics to articulate and criticize metaphysical ideas of comprehensibility. The comprehensibility of the universe is taken to be an article of faith, not a central part of the content of current scientific knowledge. One consequence of this allegiance to standard empiricism is the blindness of most physicists to the inadequacies of orthodox quantum theory.

11 Orthodox quantum theory, lacking any consistent idea as to what sort of entity an electron or proton is in itself, is obliged to be a theory about the outcome of *performing measurements* on these entities. This means, as Bohr always insisted, that orthodox quantum theory is made up of two parts,

quantum postulates and some part of classical physics to describe the measurement process. Orthodox quantum theory is thus a severely *ad hoc* theory, being made up of two conceptually unharmonious parts. It is an *aberrant* theory, in that it postulates that something peculiar (and non 'quantal') occurs during the process of measurement. It lacks precision, just because the key notion of 'measurement' is somewhat vague. It lacks explanatory power, in that it must presuppose some part of classical physics, which thus cannot be explained, and it abandons physicalism.

All these defects can be overcome by developing a full micro-realistic, probabilistic version of quantum theory which attributes *propensities* to electrons, protons, etc., a propensity being a new kind of physical property which determines probabilistically how one entity, such as an electron, interacts with another, for example a positron. This notion of propensity generalizes classical, deterministic ideas of physical property and physical entity.

In order to implement this idea, it is essential to specify precisely the micro-realistic, quantum theoretic conditions for something probabilistic to occur (no reference being made to measurement). My suggested solution to this problem can be put like this. Consider a neutron decaying into a proton, electron and neutrino. Orthodox quantum theory predicts that, in the absence of measurement, the neutron persists in a superposition of the decayed and undecayed states – a state of indecision, as it were, as to what has actually happened. I suggest that, when the interaction is very nearly at an end (and I specify precisely what this means), the neutron abruptly jumps probabilistically into one or other state even in the absence of measurement. And more generally, whenever a composite quantum system evolves into a superposition of two or more interaction channels, each channel containing particles of different rest masses, the superposition collapses probabilistically into one or other channel when the interaction is very nearly at an end. Measurements that detect systems are just special cases of this.

This fully micro-realistic version of quantum theory has in principle all the predictive success of the orthodox theory, and none of the disadvantages. The theory is fully in accordance with physicalism (but not determinism). Furthermore, the theory is capable of being distinguished *experimentally* from orthodox quantum theory, certainly in principle, and possibly in practice (Maxwell, 1976a, 1982, 1994). Thus, for example, orthodox quantum theory predicts that decaying systems (such as neutrons) decay at a rate that differs slightly from an exponential rate for short and long times (Fonda *et al,* 1978), whereas the micro-realistic propensity version of quantum theory predicts

that there will be no deviation from the exponential rate for long times (due to the prediction of probabilistic jumps predicted not to occur by the orthodox theory).[10]

This simple solution to the problem of developing a fully micro-realistic, propensity version of quantum theory has been overlooked for historical reasons. After the advent of quantum theory, during the years 1925-1937, the physics community was polarized into two opposing camps concerning the status and interpretation of the theory. On the one hand, Bohr, Heisenberg and others held that micro-realistic determinism must be abandoned; on the other hand, Einstein, Schrödinger and others held that micro-realistic determinism must be retained (orthodox quantum theory thus being seriously incomplete). Both camps took for granted that *micro-realism* and *determinism* stand or fall together. As a result, no one at the time had the idea of *defending* one, and *rejecting* the other. No one sought to develop, or even put forward as a possibility, the view advocated here – *micro-realistic probabilism.* This in turn led everyone to overlook the *problem* that confronts micro-realistic probabilism – the problem, that is, of specifying precise micro-realistic, quantum-mechanical conditions for probabilistic events to occur. Thus no one put forward the simple *solution* to this problem indicated here.[11]

According to this micro-realistic propensity viewpoint, the mysteriousness of quantum objects, such as electrons and protons, is due to the fact that the basic physical properties of these objects – namely propensities – are of a kind quite different from the basic physical properties of objects we are familiar with from our experience of the macroscopic world, and from classical physics. Familiar macroscopic propensities are not *basic* physical properties. Thus the propensity of a die to be unbiased when tossed, can be explained away in terms of basic deterministic properties of the die and the environment, and probabilistically varying initial conditions from toss to toss. Quantum objects – or *propensitons* as I have called them elsewhere (Maxwell, 1988) – seem strange to us for reasons that are similar to (and no better than)

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[10] For further work on this version of quantum theory see Maxwell (1985a, 1988, 1993, 1994 and 1998, ch. 7).

[11] It is above all Popper who subsequently stressed the possibility of developing a realist, propensity interpretation of quantum theory: see his 'Quantum mechanics without the 'Observer' ' in Bunge (1967, pp. 7-44). Popper's propensity interpretation of quantum theory fails however to be micro-realistic, in that for Popper *'Propensities are properties of neither particles nor photons nor electrons nor pennies.* They are *properties of the repeatable experimental arrangement'* (p. 38): see Maxwell (1976a, pp. 667-73; 1985a, pp. 41-2; 2007a).

the reasons which led Newton and others to find the Boscovich point-particle mysterious, and Maxwell and others to find the Faraday field mysterious. An electron is a smeared out wave-packet: what is smeared out, however, is the propensity of the electron to interact in a (probabilistic) particle-like way, should physical, micro-realistic, quantum-mechanical conditions to do so arise. All this would have been obvious to the physical community long ago had it put aim-oriented empiricism into practice. Micro-realistic probabilism is at present unknown to the physics community because of its general adoption of standard empiricism, with its exclusion from physics of sustained imaginative and critical metaphysical thought about problems having to do with the overall *comprehensibility* of the universe (of the kind indicated here).

12 From a general intellectual and cultural standpoint, the most important contribution that science has to make is to our overall understanding of the world and our place in it. It is this that is emphasized as being of supreme intellectual importance by aim-oriented empiricism: the whole *raison d'etre* for natural philosophy, set within the context of the philosophy of wisdom, is to help people and societies to acquire such understanding as an integral part of life. Standard empiricism devalues, and even suppresses, such understanding for being philosophical, metaphysical, unscientific. As a result, standard empiricism does nothing to heal, and much to promote, the gulf that has grown up between esoteric, expert scientific knowledge on the one hand, unscientific personal understanding of the world on the other hand.[12]

I have not yet shown that aim-oriented empiricism can overcome *all* the nine lethal objections to standard empiricism. In particular, I have not shown how aim-oriented empiricism solves problems concerning simplicity and unity, and the meaning of progress in physics given that physics advances from one false theory to another. This, as I have already mentioned, I set out to do in chapter 14.

From the standpoint of the overall argument of this book, two implications of the present chapter are especially important. The first is that we must take seriously the thesis that the universe is perfectly comprehensible, as conceived of by physicalism (that is, more or less as conceived of by modern theoretical physics). For this is not a mere metaphysical or philosophical speculation: it is a secure part of scientific knowledge – a presupposition of all of physical science and thus more secure

[12] Maxwell (1976b, 1998, 2001 and 2004b) are all highly germane to this issue.

than any particular physical theory. But if the universe is perfectly comprehensible in this way, severe constraints are placed on what can be of value in existence and how what may be of value can be realized – so severe that it may be doubted that anything of value can exist at all (in that all of human life must comply with some unified pattern of physical law). But secondly, if the universe really is comprehensible more or less as modern physics conceives it to be, then one human endeavour, of great value, has been extraordinarily successful: the endeavour of improving scientific knowledge and understanding of the universe. This extraordinarily cooperative, progressive success has been achieved in part as a result of putting into practice aim-oriented rationalism (in the form of aim-oriented empiricism), even if this has not always been recognized. There is here an enormously important methodological lesson to be learned for all human endeavours – a point to be developed further in the next chapter. Just that which seems to pose the greatest threat to the very possibility of human freedom and value – the immense success of physics – actually holds a vital clue for the growth of human freedom and value in all that we do.

Chapter Ten
How Can There Be Life of Value in the Physical Universe?

The argument of the last chapter serves to intensify an already severe problem. If the world really is more or less as modern theoretical physics conceives it to be, if it really is comprehensible in the way that physics presupposes it to be, so that some version of physicalism is true – then how can human life have any real meaning or value? If we are all merely very complicated physical systems, made up of molecules in turn made up of electrons, protons and neutrons (electrons and quarks) which interact in accordance with a fixed, unified pattern of physical law, how can we also be people, sentient and conscious? How can it be that we feel, think, enjoy and suffer? How can we be responsible, in any degree for our actions? How can our lives be imbued with any meaning or value? What becomes of our minds and our souls? What becomes of the entire world of human experience if the universe really is more or less as modern theoretical physics seems to tell us it is? The aim to improve our understanding of the world, pursued in order to enrich life, seems to threaten to annihilate conceptually all meaning and value in life. How is this problem to be resolved?

This chapter is devoted to this problem. I begin with some remarks about what is of value in life. I then endeavour to show how that which is of value, which I associate with the experiential realm, may be accommodated within the physicalistic view of the world.[1]

I now endeavour to characterize, in the form of a theory, a *conjecture,* some general features of what is of value in existence. I have fourteen points to make.

1 All that which is of value in existence has to do with life and above all, for us, with our lives, with human life. We participate in that which is of value in our living, experiencing, doing. Insofar as non-living things are of value, whether natural or made by people, their value arises as a result of their association with life.

2 That which is of value in existence, associated with human life, is inconceivably, unimaginably, richly diverse in character. That which is of most value in one person's life is inherent in the rich pattern of particularities of the person's life, the extraordinarily intricate pattern of environment,

[1] See also Maxwell (1966, 1968a, 1968b, 1976b, and especially 2001).

deeds, perceptions, feelings, thoughts, desires, imaginings, relationships with others. The greatest poets, novelists and dramatists – Shakespeare, Chekhov, Tolstoy, Stendhal, Jane Austen, Dickens, D. H. Lawrence – can only hint at the rich diversity of value inherent in a person's life. In order to come to see and to understand something of what is of value in another person's life we need the empathetic, imaginative and creative resources of a great artist so that we may enter into the person's world and, in imagination, see, feel, experience, desire, fear, love and suffer what he or she does. We need to acquire deep person-to-person understanding of the other. We need to be an intimate friend at least. It is for this reason that each one of us can glimpse only a minute fraction of all that there is of value in the world in human life.

The richness and diversity of value is made possible, from a neurological standpoint, by the vast structured complexity of our brains, with their 10^{10} neurons. A casual perception, a fleeting thought or feeling, of any person in life has a beauty and profundity greater by far than that of even the greatest works of art, such as a tragedy by Shakespeare, a mass by Bach, a symphony by Mozart or Beethoven. What we are is greater by far than what can be expressed by even the greatest artistic skill. As we live we may deepen our awareness and appreciation of this wealth we inherit, in ourselves and in others: or we may become progressively deaf and blind to it.

In acknowledging the inconceivably rich diversity of what is of value in the world I am not acknowledging that peoples' diverse beliefs about what is of value are all as good as each other. I am not, in other words, putting forward a relativistic or subjectivist view about value. Quite to the contrary, the view that I am proposing is an objectivist, realist view of value. I seek to specify a few general characteristics of that which actually exists in the world, whatever anyone may believe or not believe, that is of value. The value-judgements of some do better justice to what really is of value in existence than the value-judgements of others. In endeavouring to assess the adequacy of any value-judgements, or systems of such judgements, whoever or whatever may uphold them, we need to try to assess how adequately the judgements correspond to the value-reality they are about. Fundamentally, it is not a question of assessing them in terms of some other set of value-judgements, upheld by some other person or being whether it be the majority, the government, the masses, society, culture, the church, history, posterity, the Bible, a prophet, or God.

It is most important to distinguish between diversity in value-judgements reflecting the diversity in what actually exists that is of value, and diversity in value-judgements reflecting disagreement as to what is of value. Some may adopt value-relativism because of a failure to make this distinction, rejection

of any system of value-judgements being interpreted, as a result, to be an undesirable refusal to recognize the rich diversity of value.

It should be noted that if what has been said above about the rich diversity of value is true, then it follows that all publicly communicable theories about the general character of what is of value in existence – such as the theory formulated here – must inevitably fail to do justice to what is of value in all its rich, diverse, particular actuality, as experienced by each one of us in our lives.

3 My reasons for rejecting value-subjectivism and value-relativism are extremely simple. To say that any set of value-judgements is as good as any other is, I suggest, equivalent to saying that in reality nothing is of value. For if in reality some things are of value, those value-judgements which recognize this must be better than those which do not; different value-judgements cannot all be equally good, and value-subjectivism and relativism must be false. For value-relativism and subjectivism to be true, in other words, it is necessary for nothing in reality to be of value.

Value-subjectivism and value-relativism also of course annihilate the possibility of there being genuine individual and cooperative *learning* concerning what is of value in existence. Inquiry pursued in accordance with the philosophy of wisdom, devoting research to learning about what is of value – what it is and how it is to be realized – becomes a nonsense. There can be a change in one's values, but no real learning about what is of value.

It deserves to be noted that there is a close association between value-subjectivism and Cartesian dualism. For of course Cartesian dualism implies that the objective, material world is denuded of value features, just as it is denuded of sensory features. From the standpoint of Cartesian dualism, our experiences of sensory and value features of things in the world around us are all hallucinations, since these features do not exist in reality, in the material world.

4 Our ability to achieve that which is of value must inevitably be limited. Some suffering, failure, injustice is intrinsic to life and cannot be avoided. However fortunate and wise we may be, inevitably in our life we will encounter limitations, failure, misfortune. And there will always be those less fortunate and wise than ourselves. The tragic dimension to life is permanent and unavoidable.

5 Our ability to achieve that which is of value is also limited in a more desirable way. Much that is of value has come into existence unforeseen and unintended. We cannot hold ourselves to be exclusively responsible for all that is of value. Indeed, even when we consciously create something of value, we do so only insofar as Nature, that which is not us, conspires with us to

bring about what we intend. The creation and development of human life – the supreme source of value – is almost entirely out of our hands. Our continuing existence, our simplest deeds and thoughts, require the cooperation of Nature in a multitude of ways of which we are ordinarily quite unaware, and even do not understand (in that we are unaware of, and do not understand, the workings of our brains). There may even be a sense in which Nature is wholly responsible for all that is of value in existence; for if, as I shall argue below, the whole human world is a part of Nature, then ultimately it is Nature which produces all that is of value.

6 The ability to experience, participate in and help create that which is of value does not arise abruptly, inexplicably, out of nothing; rather it gradually evolves in time. Sudden blossomings of value owe their existence to long periods of prior germination and growth. We owe our present ability to participate in that which is of value to the actions and efforts of millions of people who have gone before us. Almost everything of value is inherited from the past. Creation is the modification of what already exists. Our present ability to speak, to think, to be conscious and self-aware – our humanity, our self identity as persons – is, as it were, acquired from others: these things develop for us because they have already developed for others. Our existence today depends on a long process of past social and cultural evolution – and on a long process of natural evolution as a result of random variation and natural selection during millions upon millions of years. It is above all the consideration that we are a part of Nature which compels us to recognize that what is of value evolves gradually in time: abrupt creation of value out of nothing would be inexplicable, a miracle, a violation of natural law.

In seeking to discover and achieve what is of value, our task then is to develop that which is of value which already exists and has been inherited from the past. All attempts to create what is of value by means of abrupt revolutions or conversions which wholly repudiate the past are doomed to failure.

At first sight unprecedented, revolutionary achievements in the arts and sciences – achievements such as those of Shakespeare, Mozart, Beethoven, Newton or Einstein – may seem to tell against the point just made. Closer examination reveals that this is not the case. Shakespeare's plays required the prior existence of Elizabethan society, culture and language, an already developing tradition of poetry and theatre. And most of Shakespeare's plays are based on traditional or historical themes, and modify preexisting literature. Mozart and Beethoven both required for their work pre-existing musical traditions. Newton himself correctly declared: 'If I have seen further than

others, it is because I have stood on the shoulders of giants'. Newton achieved a grand synthesis of the work of Kepler, Galileo, Descartes and many others. Einstein's great contributions to science not only presuppose the whole framework of classical physics, the product of cooperative labour of many people over centuries but also his contributions owe much of their importance to the fact that they resolve problems buried deep in traditional classical physics and mathematics.

7 In order to detect and help create what is of value in existence we need all our personal resources of intelligence, experience, courage and generosity. Above all we need ourselves to feel and to desire. Devoid of feelings and desires, we can only parrot the value-discoveries of others. No scientific instrument or artefact can, as it were, detect the value aspect of reality. In order to be aware of the existence of what is meaningful and of value in the world, it is necessary to be a person responding emotionally to what exists. (This point was the great discovery of Romanticism.)

8 Just as the rich diversity of that which is of value should not lead us to abandon value-realism, so too the fact that it is necessary oneself to experience, feel and desire in order to perceive value in the world should not lead us to conclude that value is only subjective, not a part of objective reality. On the contrary, what is of value has to do with objective aspects of reality, of people and things in the world, which we perceive through our personal emotional responses to them. There is an analogy here with the perceptual qualities of things. Colours, sounds, smells, as perceived by us (rather than as described and understood by physics), require for their detection the having of appropriate sensations of colour, sounds, smells; but this does not mean that colours, sounds, smells merely are these sensations. On the contrary, these perceptual qualities are, I maintain, objective properties of things perceived by us via our sensations. Similarly, that which is of value exists objectively in the world; it is perceived by us through our emotional responses to things.

On the one hand, a blade of grass can be objectively green even though no one happens to perceive its greenness. On the other hand, a person may experience visual sensations of greenness and yet not perceive any green thing, in that he experiences merely an after-image, an optical illusion, a hallucination.

Analogously, on the one hand, a piece of music may be objectively beautiful even though no one happens to experience its beauty; a human action may be objectively noble or cruel even though no one happens to experience or perceive the action in this way – possibly not even the person who performs the action. On the other hand, things may seem to be

desirable, beautiful, cruel or ugly – they may be experienced in these ways – and yet in reality may not be these things. Just as our sensations, our inner experiences, can lead us to misperceive what is before our eyes, so too our emotional responses, our 'value-perceptions', may delude us about the significance and value of what goes in the world around us. Indeed, value illusions and hallucinations are probably far more common in life than illusions and hallucinations of a kind that would ordinarily be described as having to do with objects and facts.

We have to learn to see aspects of the world around us: stones, people, trees, sky. Equally, we have to learn to see meaning and value in the world around us, in our environment, in events, in human actions and lives. As I have already indicated, inevitably the full richness, significance and value of what there is in the world escapes human perception and understanding. A person dies. Something infinitely precious has ceased to exist. Almost certainly, however, no one is aware of the full significance of the person's life. Even an intimate friend, a lover, can only know of aspects of the value of the person's life. Even the person herself probably failed to appreciate adequately her own value. The full significance and value of the life is something that eludes us all: and yet it is something that did objectively exist in the world, in the realm of actuality.

9 I have already remarked that our feelings and desires, though necessary for the perception of value, are not infallible guides to what is of value. Equally, no prophet, religion, revelation, book, tradition or institution is an infallible guide to what is of value – just as none of these constitutes an infallible guide to truth, to knowledge. Our attitudes to traditional judgements concerning what is of value ought to be analogous to attitudes to traditional scientific judgements concerning truth encapsulated in our best scientific theories: these traditional judgements, even if the best we have, nevertheless are no more than fallible, imperfect conjectures, always open to development and improvement.

10 In addition – it almost goes without saying – there are no infallible methods or recipes for the achievement or creation of that which is of value. We cannot infallibly achieve value or know we have achieved it – even when the achievement has actually been made.

11 The inevitability of doubt about the meaning and value of our lives ought not to be the cause of despair – any more than the inevitability of doubt in science ought to be the cause of scientific despair. Acknowledging calmly the inevitability of doubt about the meaning and value of our lives makes learning and growth possible, just as in science. Repudiation of doubt, out of fear, obstructs learning and growth. We should not seek to *rebut* scepticism

about value: rather we should seek to *exploit* it in an endeavour to help increase value. As in science, so in life: we need to be so unrestrictedly sceptical, in our endeavour to realize what is genuinely of value, that we become sceptical even of the capacity of unlimited scepticism to promote the realization of value. As in science, so in life: total scepticism is to be rejected on pragmatic grounds; it cannot help. We are rationally entitled to assume that our lives here on earth are genuinely meaningful and of value, even though this cannot be verified or proved, just as we are rationally entitled to assume that the universe is, in some way, comprehensible even though this cannot be verified or proved. These are the only two basic rationally justifiable articles of faith – justifiable on pragmatic grounds, without one iota of scepticism being repudiated. Our problem is to reconcile these two basic tenets of rational, sceptical faith. How can there be life of value in this physically comprehensible universe? This is a *practical* problem (a problem of acting so as to realize value in this world), a *theoretical* problem (a problem of choosing the best answer from many possibilities) and a *conceptual* problem (a problem of discovering just one possible answer).

12 In order for that which is of most value actually and potentially in existence to flourish, we need to endeavour cooperatively to improve our aims and methods as we live – seeking, in this way, to put aim-oriented rationalism into practice in the world. Cooperative aim-oriented rationalism provides a framework within which diverse philosophies of value – diverse religions, political and moral views – may be cooperatively assessed and tested against the experience of personal and social life. There is the possibility of cooperatively and progressively improving *such philosophies of life* (views about what is of value in life and how it is to be achieved) much as *theories* are cooperatively and progressively improved in science. In science diverse universal theories are critically assessed with respect to each other, with respect to metaphysical ideas concerning the comprehensibility of the universe, and with respect to *experience* (observational and experimental results). In a somewhat analogous way, diverse philosophies of life may be critically assessed with respect to each other, with respect to relatively uncontroversial ideas about what is of value, and with respect to *experience* – what we do, achieve, fail to achieve, enjoy and suffer – the aim being so to improve philosophies of life (and more specific philosophies of more specific enterprises within life such as government, education or art) that they offer greater help with the realization of value in life. It is of course true that we understand and judge what we do, the extent to which we succeed and fail, even our enjoyment and suffering, in terms of our explicit or implicit philosophies of life. As a result, experience and philosophy may simply

reinforce each other to produce dogmatism, and failure to see even the need for learning. An analogous situation arises, however, in connection with science: observations and experiments are interpreted and judged in terms of theory, there thus always being the danger here too that experience and theory uncritically reinforce each other to produce dogmatism. The solution in both cases is to consider a number of rival universal ideas (theories or philosophies) there being tripartite assessment between idea, idea and experience. For this to occur, in science or in life, sympathetic person-to-person understanding needs to develop between individuals, and between theories and philosophies (or cultures). In this way, multiplicity of religions, philosophies, cultures, ways of life, can be enriching for us all (just as multiplicity of theories can enrich science), instead of such multiplicity being, as at present, a source of incomprehension, fear and conflict.

13 At present our thinking about what is of value, and our real-life capacity to realize what is of value, are seriously obstructed by a wide-spread tendency to run together doctrines that ought to be sharply distinguished. At its most extreme, this tendency may be delineated as follows. There is, to begin with, a tendency, in some religions and political doctrines to amalgamate the following five distinct doctrines: (la) it is not the individual that is of supreme value, but rather something else (one particular individual, some individuals, God, the state, the masses, humanity, or an ideal society in the future); (2a) the individual ought not to decide for himself what is of value; rather he should allow this to be decided for him by whatever it is that is of supreme value (a religious leader, a group of religious leaders, God, the church, tradition, history, a sacred book, the state, reason, science, the masses, the majority, humanity as a whole, future opinion); (3a) the individual should not value what he desires but on the contrary should recognize that his desires (or most of them) are in opposition to what is of value; he should not selfishly pursue his own interests, but on the contrary should seek to serve what is of supreme value rather than self; (4a) value exists objectively; (5a) what is of value in existence is beyond doubt: it can be known to be of value with absolute certainty.

Any doctrine which amalgamates these five doctrines may be called a version of *authoritarian objectivism.* Anyone who upholds liberalism in the broadest sense – in that they value individual liberty and tolerance, and reject authoritarianism and dogmatism in all its forms – must find authoritarian objectivism abhorrent. Failure to distinguish carefully the five different doctrines that go to make up authoritarian objectivism will however lead the anti-authoritarian liberal into rejecting all of them; as a result, he will come to uphold an opposing doctrine, which may be called *individualistic subjectivism,*

which amalgamates the following five opposing doctrines: (1b) it is the individual that is of supreme value; (2b) the individual ought to decide for himself what is of value, as far as he is concerned; (3b) the individual should value what he desires; he should seek to satisfy his own desires; he should selfishly pursue his own interests; (4b) value is subjective; (5b) what is of value for each one of us is a legitimate matter of doubt.

As an attempt at something better than authoritarian objectivism, individualistic subjectivism is a disaster, in that it is inconsistent and self-defeating. Almost all the disastrous defects of individualistic subjectivism come from the rejection of value-objectivism (4a), and the affirmation instead of value-subjectivism (4b).

Thus, upholding subjectivism (4b) sabotages just that which the tolerant liberal is most concerned to affirm, namely the *objective* supreme value of the individual (1b) – an objectivist conjecture about what is of value in the world. In short, (4b) is incompatible with (1b). If (4b) is true, and all value-systems are equally viable, then so too are those which deny (1b) and assert some form of authoritarian objectivism. The value-subjectivism of individualistic subjectivism annihilates all reasons for repudiating authoritarian objectivism.

It deserves to be noted that (1b) is also incompatible with (3b): for if what is of supreme value is the individual person, then individual persons ought surely to recognize this and act accordingly, by treating others as ends in themselves, as Kant, for example, advocates. They ought not to satisfy their own desires if this involves exploiting others.

Again, in advocating value-subjectivism (4b), the tolerant liberal may hope, in this way, to oppose those doctrines in authoritarian objectivism that are for him the most abhorrent – the authoritarianism and intolerance, the demand for submission, obedience and self-sacrifice, explicit or implicit in (la), (2a) and (3a). His hope is that if subjectivism comes to be generally accepted, it will lead to general tolerance, to respect for the diversity of values and ways of life, in that people will cease to claim that what they value is better than what others value, that they have a moral duty to conquer and convert others – and will thus cease to strive to conquer and convert in actuality.

The aim here is excellent, but the means chosen to realize the aim are disastrous. Value-subjectivism, far from helping to oppose authoritarianism and intolerance, actually destroys the only rational basis for opposing these things. For, from the standpoint of value-subjectivism (4b), one is obliged to hold that although these things may be immoral with respect to one value-system, they will nevertheless be highly moral with respect to other, equally good, value-systems.

Yet again, in advocating value-subjectivism, the tolerant liberal may hope to promote anti-dogmatism (5b) and oppose dogmatism (5a). But, once again, the thing is all the other way round. Value-objectivism (4a) provides the only rational basis for doubt about what is of value. Subjectivism annihilates the very possibility of doubt: it actually renders doubt meaningless, since subjectivism cannot make sense of the idea that a value judgement is in any sense wrong. Thus for subjectivism, there can be no valid role for reason or for criticism within the realm of value; and there can be no learning. In all these respects, individualistic subjectivism is actually worse than authoritarian objectivism. This latter doctrine can at least hold doubt to be meaningful (even if it deserves to be rejected); it can give a role to reason and criticism, and can acknowledge the possibility of learning. Thus, once again, the rejection of objectivism, and the adoption of subjectivism in its stead serves to annihilate the very values the tolerant liberal seeks to affirm.

Finally, of course, value-subjectivism implies that nothing is objectively of value. The failure to distinguish the five distinct doctrines of authoritarian objectivism has the disastrous consequence that we leave open for ourselves only the following extraordinarily unattractive choice: either we must accept the supreme value of something other than ourselves, to which we must sacrifice our intellectual independence and our individual freedom: or we must accept that everything is ultimately meaningless and valueless. This is not a desirable choice to be forced to make. Insofar as individualistic subjectivism is the official opposition to authoritarian objectivism, one might almost suppose that it had been deliberately cooked up by authoritarians to be as grim as possible to discourage as many people as possible from leaving the prison of authoritarianism.

In developing a better alternative to authoritarian objectivism than individualistic subjectivism, the decisive point to recognize is that value-objectivism, or value-realism, far from being naturally aligned with dogmatism and authoritarianism, is actually incompatible with these things, value-realism actually being essential to provide a rational basis for doubt, and for learning, in the realm of value. Furthermore, the five components of authoritarian objectivism need to be sharply distinguished, and dealt with separately, one by one. The result, I suggest, is the doctrine proposed here, in points (1) to (12) above. It might be called conjectural, cooperative, experiential realism, or experiential realism for short, it being understood that cooperativeness and individual freedom are interdependent as explained in chapter 8.

It deserves to be noted that this discussion of the relative merits and demerits of *authoritarian objectivism, individualistic subjectivism* and *experiential realism* is

highly relevant to the overall theme of this book. Following Plato, the academic enterprise is at present, one might say, committed to *authoritarian objectivism:* it is recognized, however, that committing the academic enterprise to this doctrine in the realm of value would be intolerable (because of its authoritarian, anti-liberal, Platonic consequences): hence the academic enterprise restricts itself to presupposing authoritarian objectivism in the realm of fact. This is married to acceptance of *individualistic subjectivism* in the realm of value – the division aided by past acceptance of *Cartesian dualism.*

I propose that both authoritarian objectivism for the realm of fact, and individualistic subjectivism for the realm of value be rejected, and that instead we adopt *conjectural physicalism* as far as the physical universe is concerned, and *conjectural experiential realism* as far as the human world is concerned – the world as lived and experienced by us, imbued with human enjoyment and suffering, human meaning and value.

The above account of some general features of what is of value in the world is put forward as providing a conjectural background framework for more specific and definite conjectural philosophies of value.[2] Do I have any such more specific philosophy of life to offer, compatible with but more definite than, the points (1) to (13) above? In an attempt to answer this question, I put forward the following slightly more definite conjectural philosophy of life.

14 The poles of value are life and love on the one hand, suffering and death on the other hand. The supreme good in existence is living life lovingly, actively loving that which is lovable in existence; and the supreme evils are suffering and death. Everything else of value in existence is organized around these two poles of good and evil.

We can help our love to grow, or to wither and die, by what we do, what we attend to, what we strive for and value. We cannot, however, authentically command ourselves to love X, or decide to love Y at will, since real love is too dependent on spontaneous, instinctive feeling and desire, out of our immediate control. We cannot therefore sensibly demand of ourselves, and of each other, that we should indiscriminately love our fellow human beings. We can however sensibly strive to create a world in which people, on the whole, treat each other, and do things together, in ways which are in

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[2] For an earlier and more passionate defence of value-realism see Maxwell (1976b, pp. 140-49). When I wrote the above I thought I was alone in arguing for value-realism in recent times, but I have discovered since that it has been advocated by others too: see especially the excellent book by Bond (1983); see also Brink (1989), the review article by Little (1994), and Maxwell (2001, ch. 2).

accordance with certain necessary conditions for love to exist. Thus we can strive to create justice, democracy, individual freedom, tolerance, cooperative rather than hierarchical social structures, traditions of resolving conflicts based on mutual understanding, good will and cooperation rather than on bargaining, manipulation, threat or violence. In this way, love can be held to be the supreme positive value, from which all others are, as it were, derived. Justice, peace, cooperativeness, democracy, health, prosperity, enjoyment, knowledge and understanding, reason, creativity, skill, imagination, courage, beauty, sensitivity, compassion, cherishing, active concern for one's own welfare and for the welfare of others, generosity, friendliness, freedom, passion, life itself: these are all of value insofar as they are necessary conditions for the supreme thing, love.

But in addition we may hold that suffering and death are evils in their own right, as it were, and not evil only insofar as they negate the possibility of love. We do not need to appeal to the value of love in order to provide a rationale for striving to avoid unnecessary suffering and death: these endeavours carry with them their own rationale. Attempts to cure and prevent disease, to end war, totalitarianism, torture, exploitation, poverty require no further *raison d'etre* than that of bringing avoidable suffering and death to an end.

Ideally, then, we live life lovingly, and in such a way as to minimize suffering. The fundamental purpose of academic inquiry is to help us develop a less suffering, more loving world.

With these preliminaries over, we come now to the central problem of this chapter. How can the rich world of human experience, full of colour, sound, love and hate, joy, tedium and pain, imbued with meaning and value – the world of Shakespeare, Tolstoy, Chekhov, Mozart, Bach, the Renaissance, the French Impressionists – be accommodated within the physical universe, as conceived of by modern physics? How can we be conscious, free and loving persons if we are merely electrons, protons, and neutrons interacting in accordance with precise physical law? How is it possible to reconcile *physicalism* and *experiential realism* (as set out in the above fourteen points)? This is an old problem. It goes back at least to Democritus. An important part of the problem can be put like this. If physicalism is correct, then the world is such that it is in principle possible to formulate a testable unified physical theory, which is both *true* and *complete*. This theory – let us call it T– would in principle apply to all that there is, and would in principle predict and explain all phenomena. (In practice, of course, it would be possible only to apply the theory to the very simplest of phenomena, to only a limited degree of accuracy.) The theory would give a precise specification of the physical

nature of the few fundamental physical entities of which everything is composed, and would specify precisely the nature of the invariant property (or properties) possessed by these entities, determining how what changes does change. Conceivably, there might be just one entity with one invariant property determining how what varies does vary, in space and time. T would unify all the forces of nature; it would unify general relativity and current quantum field theories of the electromagnetic, weak and strong forces. T, we are supposing, is true, complete and comprehensive: and yet, it seems, it could not predict the content of the world of human experience – colours, sounds, smells, tastes, tactile qualities of things, as experienced by us; our inner experiences, sensations, feelings, thoughts, states of consciousness; the vast diversity of human character and personality; the beauty, tragedy and value of human life; its joys and pains, its inner meaning.

Descartes sought to solve this problem by, in effect, conceding that (1) T gives a true, complete and comprehensive account of what there is in the world of matter; but at the same time postulating that (2) there is a distinct world of mind, which accommodates all that there is in the world of human experience which T fails to predict. For each one of us, our distinct, private 'world of mind' is linked in some way with our brain. These two postulates, (1) and (2), constitute the essence of Cartesian dualism. They do provide some sort of solution to the *physical universe/human world problem* (as we may call our original problem), but only at the expense of creating a number of severe new problems, such as (a) how is the mind related to the brain? (b) how can the mind influence the brain, if T is true? (c) granted that the mind cannot influence the brain, and that it is the brain that controls our bodies, how can there be free will? (d) if all we are ever aware of is our own private world of consciousness, our world of mind, how can we ever come to know anything of the external world of matter?

Since Descartes' time, the main effort of western philosophy has been to solve these and related problems that arise once some form of Cartesian dualism is accepted. Locke, Spinoza, Leibniz, Berkeley, Hume, Kant, Hegel, Schopenhauer, Mill, Mach, G.E. Moore, Russell, Wittgenstein, Schlick, Feigl, Ayer, Ryle, Popper, Smart, Armstrong, Fodor and many others, have been centrally preoccupied with aspects of the problems generated by Cartesian dualism even when, as has often been the case, dualism itself has been repudiated. One striking feature of this tradition of discussion is a tendency to lose sight of the nature of the original problem – the physical universe/human world problem – which Cartesian dualism sought to solve.

Instead of attempting to solve the problems generated by Cartesian dualism, what we need to do, I suggest, is return to the original physical

universe/human world problem, recognize clearly that Cartesian dualism fails to solve this original problem, and develop a better resolution of the problem.[3]

I suggest that both (1) and (2) above, of Cartesian dualism, are false. It is false that (1) T gives a true, complete and comprehensive account of what there is in the world of matter. Rather, T gives a true, complete and comprehensive account of what there is in the world of a very special, highly restricted kind. T provides us with what might be called a skeleton description of the world. Only properties of a very special kind are described by T – or by descriptions of states of affairs formulated in the vocabulary of T. In particular T leaves out all mention of the experiential qualities of things – their colour, sound, smell, feel, as experienced by us, and their beauty, ugliness, meaning and value as felt and experienced by us. T is specifically designed to omit all references to such qualities of things: thus the fact that T is silent about them provides us with no reason whatsoever for supposing that they do not really, objectively exist, as a part of the material world. *Physicalism and experiential realism can thus both be true.* The world of physics and the world of human experience dovetail together to form one unified material world. The very distinction between 'the physical universe' and 'the world of human experience' is, as it were, an artifact of our understanding rather than something that exists in reality.

Not only is (1) false; (2) is false as well. The basic reason for believing in (2) – for believing in a 'world of mind' distinct from the 'world of matter' – was belief in (1), namely that T is *entirely* complete and comprehensive about the world of matter, as it were, it therefore being necessary to postulate an additional world to accommodate all that T leaves out, namely the experiential. Thus the moment (1) is rejected – the moment it is recognized that T provides a true, comprehensive description of what there is in the world of *only a very special, restricted kind,* (silence about experiential qualities being no grounds for holding they are not a part of the material world) – the basic reason for believing in (2), in the existence of a distinct 'world of mind', collapses. I conjecture that no such distinct 'world of mind' exists. At a stroke,

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[3] In Maxwell (2001) I argue that the pre-Cartesian physical universe/human world problem is the most fundamental problem of understanding that there is, and I spell out in much greater detail the approach to the problem sketched in the present work. Others who have recognized the fundamental character of the physical universe/human world problem include Meyerson (1930, ch IX), Whitehead (1926), Sellars (1963, ch. 1), Feigl (1967), Smart (1963) and Chalmers (1996).

the Cartesian problems that arise from postulating the distinct 'world of mind' vanish as well.

In what way precisely is the *comprehensive* description of T highly *restricted*'? If T is ever formulated, it will have arisen as a result of the endeavour to *predict, explain* and *understand* all phenomena. T is designed specifically to specify the unified pattern running through all phenomena, however diverse, controlling how they occur. Thus T may be said to refer only to those *casually efficacious* properties which everything has in common with everything else.

The precise way in which T is both comprehensive, on the one hand, and highly restricted, on the other hand, can be specified as follows.

Given any bit of the world, isolated from the rest of the world, then for any instant t_0, there exists a true description D_0 of this bit of the world, formulated entirely in the vocabulary of T. D_0 specifies the instantaneous physical states of the basic physical entities that go to make up the bit of the world in question – their relative positions, velocities and so on, their masses, charges, momenta, energy or whatever. Furthermore, the true description D_0 is such that it, together with T, predicts (perhaps probabilistically) future states of the bit of the world in question at future times t_1, t_2, when described in precisely the same way, i.e. in terms of physicalistic descriptions D_1, D_2. T is comprehensive and complete in the sense that it refers to *all* the invariant physical properties determining change in *all* possible isolated bits of the world. But this does not mean that T, D_0, D_1 or D_2 tell us all that is true of the bit of the world in question. Only those properties will be referred to and described which need to be referred to and described in order that the above predictive tasks become (in principle) possible. Thus, if omission of all reference to experiential qualities does not in any way disrupt the capacity of T plus D_0 to imply D_1, then experiential qualities will not be described by D_0 or D_1. If, for example, the bit of the world in question contains a person, who sees a blue light at time t_0 which turns into a red light at time t_1, then D_0 and D_1 will describe the light and the person as light of such and such wavelengths, and electrons, protons and neutrons (which make up atoms, molecules and neurons of the person's brain) interacting in such and such ways: there will be no reference whatsoever, however, to the experiential qualities of blueness, redness, and no reference to visual sensations of blue and red, just because omitting all reference to these experiential qualities does not disrupt physicalistic prediction. T applies to all that there is, and predicts everything that occurs when described in terms of the highly specialized, highly restricted vocabulary of T: it does not, however, tell us all that is true of what there is. In particular, T tells us nothing whatsoever about the rich,

diverse experiential dimensions of reality (of which we only catch a glimpse in our own experiences).

A further decisive difference between physicalistic properties (of the kind referred to by the vocabulary of T) and experiential qualities, is the following. In order to come to know what sort of property any physicalistic property is, it is not necessary oneself to have any special sort of experience (although some experiences are obviously necessary if one is to be sentient and conscious, and thus capable of understanding anything). In order to understand the meaning of any physicalistic term – such as mass, electric charge or whatever – it is not necessary oneself to have had any special sort of experience. In sharp contrast to this, in order to know what sort of property an experiential property is, it *is* necessary oneself to have had certain sorts of experience. In order fully to understand the meaning of experiential terms such as red or blue, or love, despair, kindness etc., it is necessary oneself to have had certain sorts of experiences. Being blind from birth does not in itself debar one from understanding any physics: it does however debar one from understanding the visual part of the experiential domain. Physical colour can be understood; experiential colour cannot.[4]

At this point it may be asked: given that T has been formulated, why cannot one formulate a new, even more complete and comprehensive theory T*, which would consist of the postulates T plus additional postulates correlating physicalistic states of affairs and experiential qualities and states of being? The answer to this is that it might well be possible to formulate such a T*, but a terrible price would be paid. T* would be so grotesquely complex and *ad hoc* that it would be entirely *non-explanatory*. It would predict, but it would not explain. Just because of the incredibly rich diversity of the experiential world, and because of the incredibly complex way in which this connects up with the physical world (via our 10^{10} neurons, in different ways for each one of us), one would need to add endlessly many new postulates to form T*. (Endlessly many new postulates would need to be added with the birth and growth of every person.)

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4 This argument for the incompleteness of physics is usually attributed to Nagel (1974) and Jackson (1982, 1986). In fact the argument was first formulated by me – see Maxwell (1966, especially pp. 303-8; 1968b, especially p. 127 and pp. 134-7) – some eight years before Nagel's paper, and sixteen years before Jackson's first paper. When I recently drew Nagel's attention to these publications of mine, he remarked in a letter, with great generosity 'There is no justice. No, I was unaware of your papers, which made the central point before anyone else.' Jackson acknowledged, however, that he had read my 1968 paper.

We thus have here an *explanation* as to why there cannot be a good explanation as to why physicalistic properties and experiential qualities are correlated in the ways that they are.[5] I suggest that this solves one major part of the problem of reconciling the physical and the experiential. The experiential domain has always seemed profoundly mysterious from the standpoint of physicalism, just because of the apparent impossibility of explaining and understanding the experiential domain. What the above argument does is to explain, and thus demystify, the impossibility of giving good (scientific, physicalistic) explanations of the experiential domain.

Put another way, the above argument shows that in order to explain and understand phenomena in the way that physics enables us to do, by revealing underlying unified patterns in ostensibly diverse phenomena, a certain price must be paid. The rich particularity and diversity of the experiential aspect of reality must be neglected. This can always be put back in again, but only at the price of one's predictive theories becoming so grotesquely complex and *ad hoc* that they cease entirely to be explanatory. Physics is only able to delineate the unified skeleton of the world by leaving out the richly diverse experiential flesh.[6]

An explanation has been given as to why there can be no good explanation of how and why the experiential flesh of the world exists as it does amongst the physicalistic bones – no good explanation, that is, of the kind sought by physics. Might it not be possible, however, to explain and understand correlations between physicalistic and experiential aspects of the world in some other way, in terms of some other notion of 'explain' or 'understand'? We have already acknowledged the existence of two kinds of understanding – or rather, the existence of two interdependent aspects of understanding. On the one hand there is person-to-person understanding, achieved when one person can imaginatively recreate for himself the view of the world, aims, problems, experiences, desires, hopes and fears of another person, thus entering imaginatively and accurately into that other person's experiential world; on the other hand, there is scientific or physicalistic understanding, achieved when a group of people develop an empirically

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[5] Subsequently I have, however, put forward a possible explanation as to why brain processes and sensations are correlated in the way that they are: see Maxwell (2001, pp. 126-9; 2005f).

[6] For further aspects of this proposed solution to the physical universe/human world problem see Maxwell (1966, 1968a, 1968b, 1985c, 1998, and especially 2001).

successful theory which attributes a unified pattern to a range of ostensibly diverse phenomena.

I have emphasized that these two kinds of understanding are interdependent. There can be no successful person-to-person understanding without some sort of 'scientific' understanding of the environment in which the person to be understood exists, however primitively pre-scientific this understanding may be. And there can be no scientific or physicalistic understanding of the world without scientists being able to acquire person-to-person understanding of each other in the context of science, to the extent of being able to enter imaginatively into each others' scientific views of the world, research aims and research problems, and scientific experiences (observations and experiments). Scientific theory and knowledge – embodying our scientific understanding of the world – is itself the product of a multitude of past and present person-to-person understandings achieved by scientists of each other.

This interdependence of person-to-person and scientific understandings is not, however, itself generally acknowledged and understood, either by those concerned primarily with person-to-person understanding, or by those concerned primarily with scientific understanding. It never occurred to the 'philosophes' of the Enlightenment to divorce passionate concern for the inner life of man from passionate involvement with the imaginative and critical exploration of the natural world being undertaken by natural science. Romanticism created this divorce. Rousseau, Blake, Wordsworth, Keats, Tolstoy, Kafka, D.H. Lawrence and a multitude of other novelists, poets, dramatists and artists passionately pursued person-to-person understanding – exploration of the experiential world – in a way that was divorced from, if not actually hostile to, science. (Chekhov is a notable exception.) Natural scientists, on the other hand, in conformity with the philosophy of knowledge, developed scientific knowledge as if it were wholly impersonal, something quite distinct from person-to-person understanding. (Here, Einstein is a notable exception.)

The result of these intellectual and cultural developments is to create the impression of two disassociated 'worlds' – the world of physics, the physical universe, on the one hand, and the world of human experience and life, the experiential world, on the other hand. In immersing ourselves in science, we forget, or fail to realize, that the experiential flesh of the world has been deliberately excluded so that unified patterns of law may be discerned in the bare bones of the world: and in immersing ourselves in the experiential world, the world of person-to-person understanding, of history, biography, literature and art, we may find it necessary to repudiate the scientific vision of the

world in that it is (mistakenly) interpreted to annihilate the experiential domain.

And there is a further point. Failure to acknowledge the very different (through intellectually equally legitimate and interdependent) physicalistic and person-to-person modes of understanding may itself be a source of mystification concerning the 'comprehensibility' of the experiential. In an important sense, these modes of understanding proceed in opposite directions. What is for one mode the base line of comprehensibility (in terms of which everything else needs to be understood) is for the other mode wholly incomprehensible. Thus for person-to-person understanding, the base line of comprehensibility is made up, for each one of us, of our own personal elemental experiences and actions, in terms of which we seek to 'understand' experiences and actions of others. The physical properties of fundamental physical entities, and the patterns postulated by fundamental physical theory, being utterly remote from our personal experience, are utterly incomprehensible, from a person-to-person standpoint. In terms of physicalistic understanding, however, exactly the reverse of this holds. The physical properties of fundamental physical entities – the unified patterns postulated by physical theory – constitute the base line of physicalistic understanding, in terms of which we seek to explain and understand everything else. From the standpoint of this mode of understanding, it is our personal experiences and actions that are almost inconceivably incomprehensible, being the outcome of interactions of millions upon millions of fundamental particles organized in an incredibly complex and specialized way into the cells of our brain and body – the functioning of the brain, in particular, being profoundly affected in an incredibly intricate way by years of intricate, particular occurrences in the past. If now we fail to distinguish these two opposingly directed (but interdependent) modes of understanding, and as a result suppose that there is just one uni-directed mode of explanation and understanding, we will inevitably, as a result, be deeply mystified by both the experiential and the physicalistic. Our immediate experiences, in one way so utterly comprehensible, will also seem, in a wholly puzzling way, to be completely incomprehensible. Electrons, photons, protons, quarks, in one way entirely comprehensible, will also seem, in a wholly puzzling way, to be completely incomprehensible.

In order to resolve these puzzlements – a major part, I suggest, of puzzlement concerning how the physical and the experiential are interrelated – we need to recognize clearly the existence of the above two distinct, intellectually equally legitimate, interdependent but opposingly directed modes of explaining and understanding. The view that understanding does

have this character may be called the *duo-directional theory of understanding.* This theory must be an important part of our 'understanding' of how the experiential world of human life can be accommodated within the physical universe.

In order to improve further our understanding of how the experiential and physical worlds dovetail together, we need, I suggest, to take the following two steps.

1 We need to recognize that all human life, and indeed all life, is essentially purposive or aim-pursuing in character, person-to-person understanding being a form of purposive understanding, and itself, indeed, exemplifying purposiveness.

2 We need to develop a mode of inquiry that I shall call the *generalized Darwinian research programme.* This seeks to improve our knowledge and our (duo-directional) understanding of how purposiveness has gradually evolved in the world fully in accordance with the (presumed) fixed physicalistic structure of the universe.

I take these two points in turn.

1 We are what we do. All our human world (personal, social, cultural, intellectual, spiritual) is purposiveness, the exemplification of aim-pursuing. Our imagining, thinking, feeling, dreaming is activity, aim-pursuing, as explained in chapter 8. All meaning and value exists only in association with aim-pursuing. Scientific knowledge and understanding themselves constitute aim-pursuing. All this has been argued for throughout this book.

Not all our purposiveness or aim-pursuing is however human life, or indeed life of any kind. A simple feedback device such as a thermostat, or a somewhat more sophisticated feedback device such as a self-guiding rocket, can be regarded as an 'aim-pursuing' device, as the term is used here.

A device 'pursues an aim A' to the extent that almost all possible routes the device might take into the future in the given environment fail to realize the aim A, and yet almost always the device pursues one of those very special routes that do take it to A. A thermostat or self-guiding missile does this by means of feedback mechanisms which adjust the 'direction' of the device's activity in the light of environmental disturbances, so that one of those very rare paths to A is persistently pursued. In the case of a thermostat or self-guiding missile, no problem arises in understanding how aim-pursuing is compatible with physicalism: in these cases successful aim-pursuing actually requires that there is a fixed pattern of physical law which the feedback mechanisms of the device obeys.

Person-to-person understanding is a special case of *purposive* explanation and understanding. The latter explains the actions of an aim-pursuing entity essentially by showing how these actions fit into a pattern of hierarchically organized goal-pursuing. Short-term goals are pursued in a particular sequence in order to realize some overall goal. In this way, actions can be explained as solutions, or attempted solutions, to problems: reasons can be given for actions, and for changes of activity. In explaining and understanding the actions of a self-guiding missile, let us say, in this sort of way, there is however no suggestion that the missile is sentient, let alone conscious: thus person-to-person understanding is not involved. Purposive understanding might be described as a highly etiolated form of person-to-person understanding (the latter being a highly enriched version of the former, applicable to purposive beings that are sentient, conscious, and *thus persons).*

In order to understand how the experiential world of human life, imbued with meaning and value, can be a part of the physical universe, an important step is to acquire (necessarily etiolated) *purposive* understanding of human action and human life. This would include purposive understanding of those goal-pursuing actions that consist of one person acquiring person-to-person understanding of another person. It would also include purposive understanding of those cooperative goal-pursuing actions that consist of scientists improving scientific knowledge and understanding of the world. It would enable us to understand, in purposive terms, aim-pursuing associated with sentience, consciousness, communication and even love, between people: it would enable us to understand how these things can exist and proceed in a way that is in accordance with the physicalistic structure of the universe: and yet it would not itself embody person-to-person understanding. Person-to-person understanding would be, as it were, superimposed on top of purposive understanding of human life.

In this way, it becomes possible to set the sentience, consciousness, meaning, value, love and suffering of human life into a broader, intelligible context of purposiveness – a sort of neutral buffer zone between the experiential world and the physicalistic universe. It becomes possible, in principle at least, to develop purposive understanding of goal-pursuing that very gradually, over millions of years of evolution, becomes sentient, conscious, personal. In terms of physicalistic and person-to-person modes of understanding alone, this cannot be achieved.[7]

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[7] For a more detailed development of these points see Maxwell (2001, especially chs. 2-6).

2 In order to implement the programme I have just indicated, we need to adopt an historical approach. In particular, we need to improve our knowledge and understanding of the history of human and other life on earth by pursuing a mode of inquiry that I call the generalized Darwinian research programme.

All life is the embodiment of purposiveness. (Plants achieve their goals primarily by growth.) From the present standpoint, Darwin's great achievement was to provide an explanation as to how the vast diversity of forms of embodied purposiveness we find on earth can have come to be even though we live in a physicalistic universe. Darwin postulated two mechanisms: (a) random inheritable variation; (b) natural selection. We conjecture that billions of years ago, molecules developed that acquired the capacity to reproduce: possibly these consisted of crystalline rods which grew in length until they broke as a result of environmental buffeting. Those inheritable variations with the greatest capacity to grow and reproduce multiplied, while other variations died out: this process continued, leading in the course of time to the world as we find it today, including ourselves. Amongst the predictions of the theory are the following. (i) In its given environment, an animal pursues a pattern of goals, a way of life, organized so as to promote the overall goal of reproductive success. (ii) Its body is designed so as to facilitate the pursuit of these goals. (iii) In the past, the pattern of goals, and the body changed very gradually, possibly in step with a changing environment, in such a way that each small change enhanced reproductive success.

Two interpretations of Darwin's theory – or of neo-Darwinism – need to be distinguished. They may be called the *anti-purposive* and the *purposive* interpretations.

Anti-purposivism interprets neo-Darwinism in such a way that the theory helps us to eliminate purposiveness from Nature, the aim being to explain and understand the biological world in non-purposive terms, in terms of molecular biology, and ultimately in terms of the purposeless laws of chemistry and physics. The aim is to explain and understand ostensible purposiveness in the world by explaining it away, ultimately everything being explicable solely in physicalistic terms.

Purposivism, in contrast, interprets neo-Darwinism in such a way that the theory enables us to explain and understand how and why purposiveness has evolved in Nature, in a way that is in accordance with physicalism. The task of purposive neo-Darwinism is to enable us to explain and understand how the diverse purposive patterns exhibited by, and embodied in, plant and

animal life, have gradually come to be superimposed upon the fixed pattern of physicalistic law. Purposivism accepts physicalism but is anti-reductionist.

Insofar as we understand ourselves as purposive beings (at the very least) anti-purposivism creates an entirely artificial (and thoroughly non-Darwinian) hiatus between the purposeless biological world of Nature, and the purposeful human world of history and the present.[8] This hiatus is automatically avoided by purposivism, the interpretation adopted here.

The generalized Darwinian research programme accepts physicalism, and seeks to understand how and why all purposiveness has evolved in the universe – especially purposiveness associated with what we value most in human life, such as sentience, consciousness, person-to-person understanding, science, art, freedom, love. This programme of research brings together, into a coherent field of inquiry, aspects of such diverse fields of research as orthodox Darwinian theory (given its purposive interpretation), the study of animal behaviour, palaeontology, archaeology, history, anthropology, psycho-neurology, artificial intelligence, psychology, sociology, philosophy, linguistics, semantics, history and philosophy of science, and history and philosophy of inquiry more generally (the history and philosophy of ideas and culture). Person-to-person understanding of people in the past is embedded in a more general animal-to-animal understanding, so brilliantly displayed by Jane Goodall (1971), for example, in her almost 'anthropological' studies of chimpanzees. Animal-to-animal understanding involves not only endeavouring imaginatively to enter into the lives and experiences of animals: it also involves interpreting ourselves as animals – as close cousins of chimpanzees, for example. Animal-to-animal understanding is in turn embedded in the more general purposive understanding, this in turn being embedded in physicalistic understanding (which, however, is itself an evolution of person-to-person understanding).

In line with physicalism, this programme of research presupposes that goal-pursuing entities do not come abruptly into existence from prior purposeless states of affairs. There is no sponataneous generation of life. Furthermore, there is no abrupt initiation of new goal-directed activity, radically different from antecedent goal-directed activity, to such an extent that the new goal-directed activity is as inexplicable as spontaneous generation of life. All new goal-directed activity (it is presumed) can be

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[8] Monod and Dawkins both incline towards adoption of the anti-purposive interpretation of Darwinian theory: as a result, both hold that evolution of a new kind comes into existence with the cultural evolution of humanity. See Monod (1974, chs. 8 and 9); Dawkins (1978, ch. 11).

explained and understood as arising as a slight, intelligible modification of prior goal-directed activity. Where radically new goal-directed activity does genuinely arise, this is due to an already existing capacity for innovation, creativity, originality or learning, gradually and intelligibly developed in the past and suddenly given the opportunity to flourish in a new way by a small, intelligible change of circumstances. Of course, there are a multitude of goal-directed activities going on in the world, associated especially with human life, that seem radically different from previous activities. It is these innovative activities that pose the problems that the generalized Darwinian programme seeks to solve.

One important general problem confronting this programme is the problem of how purposive beings create new purposive beings. Four possibilities are (a) exclusively genetic or biological reproduction; (b) genetic plus educational or cultural reproduction; (c) manufacture; (d) manufacture plus education ((c) and (d) arising in connection with robots).

A momentous development in evolution is the transition from (a) to (b). It is this which makes cultural evolution possible – the evolution of new ways of life even in the absence of genetic evolution. New kinds of actions, initiated by individual animals, are learned by offspring, culturally inherited as it were, and progressively developed during the course of a number of generations. Social and cultural changes that have taken place throughout human history, and more recent scientific, technological and associated social and cultural changes – unprecedented in their radical character and ever accelerating rapidity of occurrence when put into the context of biological evolution as a whole – both exemplify, and depend upon the prior existence of, cultural evolution. Much that is essential to our humanity, to our identity as the individual persons we now are, such as language, personal relationships, customs, institutions, values, exist and persist because of a long prior process of cultural evolution.

How does cultural reproduction and evolution itself gradually evolve from almost exclusively genetic reproduction and evolution? In order for it to be possible for animals to reproduce and evolve culturally it is essential for animals to possess two capacities: (i) the capacity to learn individually, and (ii) the capacity to imitate (itself, perhaps, a special kind of learning). It seems likely that the development of cultural reproduction is, in addition, associated with the development of parental care. For it is primarily when offspring are cared for by parents for some time that learning through imitating others is likely to have survival value. We may postulate, then, the gradual development of (i) the capacity to learn, (ii) the capacity to care for young, and (iii) the capacity to imitate, by means of almost exclusively genetic

evolution. Parental care, for example, begins with care being taken to place eggs advantageously: this leads to guarding eggs; to moving and guarding newly-hatched offspring (performed by crocodiles), to feeding offspring (birds). When to what crocodiles and birds do there is added training in how to find food, hunt, or escape from predators – performed by many mammals – the conditions for cultural reproduction to occur are satisfied. In such conditions, mutations promoting the capacity to imitate and to learn from parental actions in youth will have survival value. Such mutations make cultural reproduction and evolution possible.

Whether a way of life is reproduced in an exclusively genetic way, or in a way that is in part genetic, in part cultural, is something that can in principle be determined empirically. Spiders spin webs and execute other aspects of a spider way of life entirely successfully even if reared in isolation from other spiders: here the way of life is passed on from body to body in an exclusively genetic way (in the given environment). In the case of many mammals, however, and especially the primates, this is not the case at all. Even if given the opportunity to survive and to learn how to survive in an isolated but otherwise carefully controlled environment, many mammals will, in these circumstances fail to develop the capacity to survive and reproduce if returned to their natural habitat. Young chimpanzees die simply from being deprived of the presence of their mothers.

The development of (b) genetic-plus-cultural-reproduction (from prior (a) exclusively-genetic-reproduction) changes profoundly the character of evolution. In particular, it makes it possible for non-genetic, exclusively cultural changes in an animal's way of life to be an essential part of the cause of subsequent morphological changes of descendants, changes that are genetically reproduced, as Hardy (1965) especially has emphasized.[9]

The great advantage of the generalized Darwinian research programme, just outlined, is that it provides a framework for understanding the deeds, achievements and experiences of people in a way that is compatible with the kind of knowledge and understanding achieved in the physical sciences, without being reducible to such knowledge and understanding. It promises to enable us to understand ourselves as a part of the biological domain without our humanity, our distinctive human value, being in any way denied: persons are not reduced to animals, and nor are animals misconceived to be persons. It holds out the hope that we can come to understand the human world as an integral part of the natural world without the meaning and value of the

[9] For a more detailed exposition of the interpretation of Darwin's theory of evolution sketched here, see Maxwell (2001, ch. 7).

human world being thereby conceptually annihilated. The programme specifies in general terms what we must seek to do in order to develop a coherent understanding of nature and of ourselves which does justice to the character of both.

From the standpoint of the philosophy of wisdom, of course, this programme of research provides no more than a background to the central task of rational inquiry: to help us develop our overall goal of seeking reproductive success, inherited from our evolutionary past, so that it becomes the goal of living life lovingly, cooperatively helping to develop a less suffering, more loving human world.

My claim is that the above discussion shows how physicalism and experiential realism can be reconciled, in an intellectually fruitful way. But how, it may be asked, can *free will* be reconciled with physicalism?

A major part of the problem here is to arrive at an acceptable definition of 'free will'. I suggest that an acceptable definition must be such that it is clear that 'free will', in the defined sense, is something that is of great value to possess, the more valuable the better the definition.

An important part of what we ought to mean by free will, or freedom, can, I suggest, be put like this: to be free is to have the capacity (and the opportunity) to realize what is of value in life. We are free to the extent that we do, or do not, possess this capacity (and opportunity). Clearly, it is of great value to have 'free will' in this sense.[10]

Granted that this conception of free will is accepted, then the above discussion, in showing how it is possible for there to be purposive human life of value immersed in a physicalistic universe, also shows how it is possible for there to be some degree of freedom associated with human life even though physicalism is true.

It deserves to be noted that freedom, in this sense, satisfies the Darwinian requirement of being something that can be understood to have developed *gradually,* in small steps, during the course of evolution. It develops gradually with the gradual development of the capacity to learn, to imitate, to dream

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[10] "Nicholas Maxwell (1984) defines freedom as 'the capacity to achieve what is of value in a range of circumstances'. I think this is about as good a short definition of freedom as could be. In particular, it appropriately leaves wide open the question of just what is of value. Our unique ability to reconsider our deepest convictions about what makes life worth living obliges us to take seriously the discovery that there is no palpable constraint on what we can consider" Dennett (2003, p. 302).

and to imagine, to be sentient and conscious, and to be able to communicate (all of which exists in chimpanzees, for example).

How free are we? From the standpoint of this book, our freedom is to be judged in terms of our capacity and opportunity to avoid suffering and death and live life lovingly. Clearly, when judged from this perspective, human freedom is severely restricted.

In order to increase our freedom, in this sense, we need, quite generally, to improve our aims and methods as we live in such a way that we realize what is of value to us. Rational inquiry, pursued in accordance with the philosophy of wisdom, has as its basic task, to increase freedom!

An argument in support of the contention that mankind does indeed have the capacity to be free emerges from just that which seems to threaten the possibility of freedom – namely the success of theoretical physics (or natural philosophy). The argument can be put like this. Suppose physicalism is true. Suppose, that is, that the universe really is comprehensible in the kind of way modern physics holds it to be. In this case one cooperative human endeavour of great value has been extraordinarily successful, namely the endeavour of improving our knowledge and understanding of the universe. Here then is a practical demonstration of human freedom (as defined above). If physicalism is true, in short, mankind definitely does have the capacity to be free. *The truth of physicalism, far from threatening, actually serves to establish the reality of human freedom.*[11]

The argument of this chapter might be summarized as follows.

Two important lessons are to be learned from the success of physical science: a view of the world, and a methodology. The view of the world is

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[11] As far as I know, this argument for the existence of free will has been entirely ignored by the flood of literature on free will and determinism that has appeared since the publication of the first edition of this book in 1984. (For an excellent discussion of much of this literature, from an incompatibilist perspective, see Kane, 1998.) Incompatibilists will, no doubt, find my characterization of free will as the capacity to realize what is of value in life (in a range of circumstances) as unacceptable, or perhaps as acceptable but grist to the incompatibilist's mill in that, in the absence of incompatibilist free will *nothing is of value*, not even scientific knowledge. Nevertheless, acceptance of this conception of free will, even if it does not resolve the debate about whether or not free will and determinism (or, better, physicalism) are compatible, might nevertheless introduce a fruitful problem-shift in the debate (which otherwise seems to have reached stalemate). The fundamental question becomes: What kind of free will does one need to have for life to have value? I might add that one philosopher has recently recognized the fundamental importance of the problem of *increasing* free will, even though the central argument of this book is ignored: see Dennett (2003).

physicalism, qualified by *experiential realism.* The methodology is *aim-oriented empiricism* generalized to become *aim-oriented rationalism.* The *generalized Darwinian research programme* holds out *the* hope of enabling us to improve our understanding of how cooperative aim-oriented rationalistic life of value might come to be in the physical universe. At the same time it emphasizes the fundamental importance of endeavouring to put cooperative aim-oriented rationalism into practice in our lives, within the framework indicated, so that we may come to develop a less cruel, more loving world.

Chapter Eleven
The Revolution is Under Way

This chapter is a record of what I said in the 1984 edition to indicate that the revolution, from knowledge to wisdom, was then already under way. In fact I was well aware, at the time, that I was giving a rather over optimistic account of things. I was trying to write the revolution into existence – my hope being that my words would become a self-fulfilling prophecy. In the next chapter, composed for this second edition, I say something about how I see the situation in 2006.

At present standard empiricism and the philosophy of knowledge predominate in science, and in universities, in a very obvious way, as chapter 6 makes clear. From this it might be concluded that there are few signs of change in academic inquiry, from knowledge to wisdom, and little hope that such a change will come to be in the foreseeable future, however urgent the need may be, and however decisive the reasons may be, for making such a change.

This gloomy conclusion is, I believe, a mistake. There is a growing groundswell of opinion and effort already devoted, in various ways, to bringing about changes in science, technology, scholarship, education, medicine, welfare, aid, politics, the media and elsewhere that can be interpreted as pioneer attempts to implement aspects of what has here been called 'the philosophy of wisdom'. These diverse efforts are, however, scattered and isolated. Individuals find themselves battling alone against general incomprehension and misrepresentation. Those concerned to develop academic psychology along rather more philosophy-of-wisdom lines are perhaps unaware of similar efforts being made in sociology, economics, philosophy, or education. There is a general failure to appreciate the need for a coordinated and comprehensive change in intellectual aims and methods throughout all of academic inquiry and education. Above all, the current prevalence of standard empiricist and philosophy-of-knowledge intellectual standards ensures that these efforts do not receive the attention, discussion, and publicity that they deserve. Intellectual standards inevitably, and quite properly, function as a form of censorship. Standard empiricism and the philosophy of knowledge are no exception. At present potentially excellent contributions to inquiry from the standpoint of the philosophy of wisdom do not get published – or when published do not get noticed – just because of a

failure to conform to the edicts of standard empiricism and the philosophy of knowledge. Academics are discouraged from giving intellectual priority in their work to the tasks of articulating problems of living, proposing and criticizing possible solutions, as they know full well that such work, however urgently needed and intellectually excellent, will not be accepted for publication in that it will not amount to potential 'contributions to knowledge'. In these ways the institutional illusion is created that academics universally accept the philosophy of knowledge, even though in fact there are many who hold that the currently adopted intellectual system – its aims, problems, priorities and values – is profoundly and damagingly defective.

I am myself well aware of just how potently the philosophy of knowledge, as a result of being built into the institutional structure of academic inquiry, operates to censor out of existence work that fails to conform to its edicts. I have encountered this again and again in my own work – above all in my attempts, during the past ten to twenty years, to communicate and publish the proposals and arguments of this book!

The themes of this book have their origins, for me, in my childhood. For as long as I can remember I have had the passionate desire to get to the bottom of things, to understand. Probably for all too human reasons, I have wanted to discover 'the secret of the universe', the riddle of life. To begin with this took the form of a desire to understand the ultimate structure of the physical universe. As a twelve-year-old, I read with fascinated incomprehension accounts of nuclear physics to be found in *Penguin Science News,* and Eddington and Russell on relativity and quantum theory. It was above all the mystery, the incomprehensibility, of this strange world of physics that appealed to my imagination. Here was this extraordinary world – of time slowing down and space contracting, of curved space-time, of particles that are also waves, of almost infinitely vast galaxies and infinitely minute atoms – apparently so different from the familiar world of stones, trees and people: and yet it was this other mysterious world that was the real world, the common-sense world being largely an illusion. With the customary unselfconscious audacity of the young, I decided that I would discover the secret of all this mystery, and thus reveal to the world the true meaning of existence.

With the onset of adolescence, however, I discovered literature. I plunged into the vivid, dramatic, extraordinary worlds of Dostoevsky, Kafka, Virginia Woolf, Chekhov, Stendhal, Shelley, Hazlitt, Fielding, D.H. Lawrence, Rex Warner, Emily Bronte. Here, I began to feel was reality: the vivid, dramatic and extraordinary inner world of human life, the inner world of imaginative

experience. I decided I would discover the innermost secret of this mysterious and passionate world of human experience by writing novels. I would create a living and breathing universe, so real in its dramatic intensity that it would all but engulf the real world.

My attempts to do this failed miserably. A failed theoretical physicist, mathematician and novelist, I decided, after a spell of national service, to return to university to do philosophy. My efforts 'at being a genius' (as I thought of it then) had failed, and were obviously absurd in any case. Without any great expectations – in order merely to indulge an interest in trying to understand how things fit together –1 became an undergraduate at Manchester University.

But after a year I made what seemed to me then a great discovery. As I put it then: 'the riddle of the universe is the riddle of our desires'. Philosophy devotes itself to the problems of knowledge, thus presuming, without question, that the basic aim of inquiry is to acquire knowledge. It is this presumption that is the mistake. The proper basic aim of philosophy is to help us resolve the riddle of our desires. It is not in the ultimate nature of the universe, nor in the ultimate nature of our inner life, that the answer to the riddle of life lies; it lies rather in what might be termed the region of overlap between the two – in the familiar miracle of this experienced world (its familiarity all too often, alas, dimming our perception of its miraculousness).

I had sought the answer to the riddle of life in the ultimate nature of the physical universe, and in the ultimate nature of our inner world. Actually the answer to the riddle of my life lay around me all the time, in the experience of living my life. I had striven to be a physicist and a novelist and had neglected to be what was for me the thing of most value, myself. The miracle upon miracle is this moment-by-moment experience of living, the outcome of the interaction between our unknown inner world and the unknown outer world. In discovering and participating in this experienced world of colour, sound, landscapes, people, beauty, we all exhibit passionate intellectual and imaginative resources that far outstrip what is revealed in the work of the greatest scientists and artists: but over-familiarity and misconceptions conspire to make us lose sight of the miraculous character of the worlds we inhabit. I wrote down these 'discoveries' of mine, submitted the manuscript for publication, and looked forward to telling the philosophers of Manchester University about the new territories for philosophy that I had stumbled across.

The manuscript was rejected; and I found that in seminars and lectures I could scarcely open my mouth. I became increasingly horrified by academic philosophy; to me, it seemed to be either totalitarian in motivation, or utterly

trivial. Great philosophers did not offer their intellectual visions of reality in an intellectually honourable way as *possibilities* – more explicitly articulated and scrutinized versions of the philosophies of reality we all create and discard casually, as we live. On the contrary, from Plato to Wittgenstein, they sought *to prove* the final and complete truth of their personal vision, thus in effect, as far as I was concerned, endeavouring to set up a sort of intellectual dictatorship, all other minds and lives to be faithful copies of their own, programmed by indoctrination masquerading as education. Apart from this, there was the triviality of ordinary language philosophy and conceptual analysis. No one seemed to be interested in the obvious and important endeavour of imaginatively articulating and scrutinizing the basic, problematic aims of life (presumably because academic philosophers never questioned their own aims). I began to suspect I was living in a new dark age.

Then I discovered Karl Popper, and especially *The Open Society and its Enemies,* and I heaved a sigh of relief. Here was a work passionately concerned with a profoundly important problem: how are we to achieve civilization? What are the *basic problems,* and what role does reason play in helping us to solve these problems? Popper had tackled these problems in a wholly responsible way intellectually and morally, and with a wealth of detailed scholarship. The covertly totalitarian character of philosophy was, with the discussion of Plato, brilliantly unmasked. With Popper's work, I concluded, the basic problems of epistemology, methodology, political and social philosophy had received their definitive solution.

Subsequently, however, as a result of pondering difficulties associated with Popper's claim to have solved the problem of induction (discussed in chapter 9), I came to the conclusion that the source of the trouble was that Popper, along with almost all scientists, seriously misrepresented the basic intellectual aim of science. The argument of chapter 5 unfolded itself before my eyes. My discovery of ten years earlier had re-emerged as the idea that all rational inquiry – and not just philosophy – should have as its basic task to help us to improve our aims and methods as we live so that we may realize what is of most value to us in life. I put pen to paper, and before long discovered that now that I had something important to communicate publication seemed to be all but impossible.

I am quite sure that many others have followed lines of argument not so dissimilar from those spelled out in this book. Doubtless they too have encountered what I have encountered: that the philosophy of knowledge, as a result of being already institutionalized, tends to block both criticism of itself and attempts to pursue inquiry along rather more philosophy-of-wisdom lines. There is a further point. It is at present especially difficult for

people without academic qualifications to speak up and be heard within academic contexts. During ten years of advocating the philosophy of wisdom to all and sundry, I have found that most non-academic women know what I am talking about straightaway, most academic men do not, with non-academic men and academic women falling somewhere in between.

There can be no doubt that during the last ten to fifteen years (leading up to 1984) a multitude of developments have taken place within and without the academic world that can be interpreted as disparate attempts to put into practice inquiry corresponding to what has here been called 'the philosophy of wisdom'. Here is an impressionistic indication of some of these developments – with the emphasis on those that have occurred in Britain.

During the period in question, there has been an enormous increase in concern about environmental and ecological problems: problems of pollution, depletion of finite natural resources, destruction of plant and animal life, growth of the world population. A number of books come immediately to mind: Rachel Carson's *Silent Spring* (1962); Barry Commoner's *Science and Survival* (1966); The Club of Rome's report; Meadows *et al., The Limits to Growth* (1974); Dubos and Ward, *Only One Earth* (1972); Maddox, *The Doomsday Syndrome* (1972); Goldsmith *et al.,* 'A blueprint for survival' (1972); Foley, *The Energy Question* (1976); Allaby, *Inventing Tomorrow* (1976); Ward, *Progress for a Small Planet* (1979); Alien, *How to Save the World* (1980); Eckholm, *Down to Earth* (1982). Groups such as the Friends of the Earth have had an effect in increasing public awareness of environmental issues. The problems have been increasingly discussed in the media. The *Ecologist* has published articles on themes closely related to that of this book (see, for example, Skolinowski, 1975, and the issue devoted to the 'Scientific straightjacket', 11, (1), 1981). Political parties have even been formed around ecological issues, with West Germany's Green Party winning seats in the Bundestag as I write.

There has been an increasing awareness of the plight of people living in the third world, as evinced by such things as Victor Zorza's 'Village Voice' column and the Third World Review, both in the *Guardian,* the rise of journals like *New Internationalist* and the publication of such books as George, *How the Other Half Dies* (1976); P. Harrison, *Inside the Third World* (1979); Brandt *et al., North-South: A Programme for Survival* (1980). Some authors have attempted to give a comprehensive survey of the most important global problems that confront mankind in the decades to come; for example Heilbroner, *An Inquiry into the Human Prospect* (1975); and most notably Higgins, *The Seventh Enemy* (1978).

Closely associated with these concerns, movements have arisen seeking to develop and promote alternative, intermediate and appropriate technology; see, for example Schumacher, *Small is Beautiful* (1973); Dickson, *Alternative Technology* (1974); Cooley, *Architect or Bee?* (1980). I have mentioned already renewed interest in Britain in cooperatives; in addition, see *In the Making: A Directory of Radical Cooperation* (1981).

There is also the movement for social responsibility of science, promoted for example by the British Society for Social Responsibility in Science, and by the society's journal *Science for the People*. Initially this movement began outside, or on the fringes of, universities and colleges of technology. Subsequently, it has had a considerable impact on courses and departments in universities and colleges of technology. Science in a Social Context (SISCON), guided by the wise stewardship of Dr Bill Williams at Leeds University, produced during the seventies over forty booklets designed to provide background material for courses in higher education on issues having to do with science, technology and society. Over twelve universities and colleges of technology in Britain now have departments or give courses devoted to such issues (although in 1982 some began to face severe difficulties due to cut-backs in expenditure on higher education). Analogous but much more wide-ranging and radical developments have taken place in the USA, as Heitowit (1977) shows. The UNESCO publication *World Directory of Research Projects, Studies and Courses in Science and Technology Policy* (1981a) list over 1,000 departments, institutes or units devoted to such issues. (See also UNESCO, 1981b).

Criticisms of modern science, technology and industrial society, conducted initially primarily from outside the scientific world by such writers as Jacques Barzun, Jacques Ellul, Theodore Roszak, and Ivan Illich, have gradually had a certain impact. Nowadays there are many distinguished scientists, Nobel prizewinners, Fellows of the Royal Society, pillars of the establishment, who are profoundly disturbed and concerned by the priorities of current scientific and technological research and by the use to which such research is put. Something of this can for example be detected in Dyson's recent scientific autobiography *Disturbing the Universe* (1981). It is to be found in the *Bulletin of the Atomic Scientists,* and in recent literature on the threat of the bomb, such as Ryle *Towards the Holocaust* (1981) and Rotblat (ed.) *Scientists, the Arms Race and Disarmament* (1983). And it is to be found in Jerry Ravetz's call for a more humanitarian and critical science, see his *Scientific Knowledge and its Social Problems* (1971).

Psychiatrists, emerging from a post-Freudian background, have kept alive a tradition of giving intellectual priority to problems of living we encounter in seeking that which is of value to us. In this context, most notable are the

numerous publications of Erich Fromm, such as *The Fear of Freedom* (1942), *The Sane Society* (1963), and *To Have or to Be?* (1979). There are also such works as Rollo May, *Love and Will* (1972); Axline, *Dibs: In Search of Self* (1971); Szasz, *The Myth of Mental Illness* (1961) and Laing, *The Divided Self* (1965). There is also Sacks' remarkable book *Awakenings* (1976) about people suffering from severe forms of Parkinson's disease. The book does full justice to the primacy of the problems of living suffered by the people in question, without in any way denying or blurring the physiological aspects of these problems.

Finally, there are eight books I wish to refer to which, in very different ways, pursue themes related to those of the present book. First Peter Gay's *The Enlightenment: An Interpretation* (1973), a magnificent evocation of the work and thought of the 'philosophies' of the eighteenth century, passionately devoted as they were to the progressive achievement of enlightenment through critical reason. Academics today might well regard the life-work of 'philosophies' like Voltaire and Diderot as paradigmatic of what academic work ought to be. Second, there is Brian Easlea's *Liberation and the Aims of Science* (1973), a serious and heartfelt exploration of problems concerning the aims of science, and how these might be transformed so that science offers more help with the task of building a less suffering, more loving world. Third, there is Robert Pirsig's *Zen and the Art of Motorcycle Maintenance* (1974), brilliantly exploring, partly in fictional or autobiographical form, themes closely related to those of this book. Fourth, there is Iris Murdoch's *The Sovereignty of Good* (1970), in which I seemed to find depicted something close to aim-oriented rationalism. Fifth, there is Mary Midgley's *Beast and Man* (1978), a book which has many interesting things to say about the problems of how our humanity has arisen from, and is related to, our animal past. Sixth, there is John Kekes' *The Nature of Philosophy* (1980), in which it is argued that 'it is the task of philosophy to show how to live well by the construction and rational justification of worldviews' (p. xii). Seventh, there is David Collingridge's *The Social Control of Technology* (1981), a book which has detailed, incisive and important things to say about 'one of the most pressing problems of our time – "can we control our technology – can we get it to do what we want and can we avoid its unwelcome consequences?" ' (p. 11). Finally, there is Colin Norman's *The God that Limps* (1981), already referred to, which considers the extent to which priorities of scientific and technological research succeed and fail to correspond to human need, in a global context.

The intellectual revolution, from knowledge to wisdom, is already under way. It will need, however, much wider cooperative support – from scientists,

scholars, students, research councils, university administrators, vice chancellors, teachers, the media and the general public – if it is to become anything more than what it is at present, a fragmentary and often impotent movement of protest and opposition, often at odds with itself, exercising little influence on the main body of academic work.

Chapter Twelve
The Revolution is Underway: Twenty Years Later

'The revolution is under way' I boldly declared in the final paragraph of the first edition of this book. I said this in the hope that saying it might help bring the revolution about. The contributions I discussed in that final chapter really did seem to me to be steps towards what I tend to call 'wisdom-inquiry' these days, but hardly amounted to the beginning of a recognition of the central thesis and argument of this book. I did however think that, by the end of the century, some kind of recognition of the validity and importance of the argument would have begun to dawn. How mistaken I was! Nothing of the kind has happened. Even in the tiny academic ghetto of philosophy of science, the case for wisdom-inquiry is still largely unknown. The wider academic community knows nothing of it.

But even though the case for wisdom-inquiry is still, at the time of writing, largely unknown within and without academia, there are nevertheless, as we shall see, grounds for optimism. A sea change is at present underway in universities, and this can be regarded as amounting to the first steps towards wisdom-inquiry. This seems to be most apparent in the academic response to environmental problems, and especially problems engendered by global warming.

But why has so little attention been paid to the case for wisdom-inquiry? As I indicated in the preface to this second edition, it is perhaps above all developments in history and philosophy of science that have conspired to block transmission of the message of this book. Just when aim-oriented empiricism promised to provide the solution to the problem of induction – the problem of the rationality of science – historians of science decided that the problem is insoluble, the claim of scientists and philosophers of science that science delivers authentic knowledge is fraudulent, *social factors*, not evidence, being what determines what is accepted and rejected as knowledge. The outcome has been disastrous. Historians of science have ignored the central, immensely important problem of their discipline – How has science made such spectacular progress in acquiring knowledge? – as an old fashioned pseudo-problem (since science does not make progress), and instead have discussed social and political factors supposedly determining developments in science. At a stroke, the ground beneath the first step in the argument of this book disappears. I mean the argument that we can learn from *scientific* progress how to achieve *social* progress towards a wise world.

Social constructivist history of science seems to demolish the first step in this argument, by denying that there is any such thing as scientific progress!

Philosophy of science has not helped either. It continues to take for granted that the basic intellectual aim of science is just knowledge of truth, but has become increasingly doubtful that a rational defence can be given of the claim that science does actually achieve this aim! Far from continuing to take seriously, and to tackle energetically, the problem of the rationality of science, in the manner of Popper, Kuhn, Lakatos and others in the 1960s and '70s, philosophers of science have retreated into scholastic specialization. Philosophy of science has split into a number of sub-disciplines: the philosophy of physics, biology, chemistry, computing science, anthropology, economics, and so on. Creating sub-disciplines in this way is always good for careers and funding, of course, but in this particular case it has meant that the one fundamental and really important problem of the discipline – How is science to be understood as a successful and rational endeavour? – has been neglected. Large questions about what the aims of science are, and ought to be – just the questions one needs to take seriously if one is to begin to discover and follow the argument of this book – fall into disrepute as old fashioned and somehow naive.

Historians of science rubbish standard empiricism and the philosophy of knowledge, and at the same time do nothing to put something better in their place, and make apparent nonsense of any attempt to do just that. Philosophers of science, without conviction, proceed as if they still believe in these doctrines, but in such a scholastic and fragmenting way that the simple and profoundly important point that these doctrines are damagingly defective is entirely hidden from view. The *status quo* is maintained.

What ought to have happened, of course, is that historians, philosophers and sociologists of science should have come to appreciate that science as it exists at present suffers from 'rationalistic neurosis', as I called it in chapter 5. Science profoundly and damagingly misrepresents its real, problematic aims in failing to acknowledge problematic assumptions, having to do with metaphysics, values and politics, inherent in these aims. Science suffers from a bad *philosophy of science*, a bad *view about what the aims and methods of science are and ought to be*, one which seriously misrepresents the real, problematic aims of science. This bad philosophy of science damages science itself, both its intellectual and humanitarian value, as a result of being taken for granted by most scientists, and as a result of being built into much of the institutional structure of science. And more generally, academic inquiry suffers from a bad philosophy of inquiry, one which seriously misrepresents what the overall aims of inquiry ought to be. Social inquiry and the humanities are especially

harmed. Above all, the outcome of these bad philosophies of inquiry – standard empiricism and the philosophy of knowledge – being built into the institutional structure of academia is that humanity fails to have what it so desperately urgently needs, a kind of inquiry rationally devoted to helping us make progress towards a good, wise, enlightened, genuinely civilized world.

We have solved the first great problem of learning – the problem of learning about the nature of the universe, and about ourselves as a part of the universe. We solved this problem of learning when we created modern science in the 16th and 17th centuries. But we have not solved the second great problem of learning – the problem of learning how to become civilized. The 18th century Enlightenment made a stab at it, but got it wrong. Their malformed stab at a solution is what we today have, and suffer from: academic inquiry overwhelmingly shaped by the philosophy of knowledge.

Solving the first problem without also solving the second one puts us into a situation of unprecedented danger. The solution to the first problem, science and technology, immensely increases our power to act: it makes possible industrialization, modern agriculture, modern medicine and hygiene, rapid population growth, modern armaments. But without also having in our hands the solution to the second problem, all this vastly increased power to act can result in human harm, danger and death as well as human benefit. Solving the first problem without also solving the second means we have an immensely enhanced power to act without also having a correspondingly enhanced power to act *wisely*. Thus, our science and technology have endless human benefits but also lead to global warming, to lethal technologies of war, to lethal *war*, to destruction of natural habitats, and to rapid extinction of species. All these menacing bi-products of modern science and technology are more or less *inevitable* granted that we solve the first great problem of learning without also solving the second one. Successfully acquire knowledge and technology in a way which is dissociated from a more fundamental concern to acquire wisdom, and there are bound to be all sorts of bad, initially unforeseen consequences.

In the circumstances there can hardly be anything more important for any of us to try to do, for the long-term well-being of humanity, than to help spread an awareness of the urgent need to bring wisdom-inquiry into existence, and to help bring this about. Above all, this ought to be the central professional concern of academic philosophers, and historians, philosophers and sociologists of science. For what we are suffering from, as we have seen, is a bad *philosophy of inquiry* built into the institutional structure of academia. Instead of joining forces to cry from the rooftops about the urgent need for change, philosophers, and historians, philosophers and sociologists of

science continue on their largely scholastic way, oblivious to what they ought to be doing. Philosophy, sociology and history of science are still pursued as *meta*-disciplines whose object it is to acquire knowledge and understanding about science, but not to contribute to the *improvement* of science. These disciplines are pursued in such a way that the task of urging scientists, and academics more generally, to take up wisdom-inquiry is excluded at the outset, in an *a priori* way, as it were.

If the 'from knowledge to wisdom' argument is to take root in academia, and begin to shape the way things develop, it may well be that those who take up the cudgels on its behalf will be natural and social scientists, and those concerned with social policy, peace studies and environmental issues, and not philosophers, and historians and philosophers of science at all. If ever the philosophical revolution begins to happen, philosophers will, I sometimes imagine (as I have already said in chapter six) be the last to hear the news.

The failure of rationalistic philosophers of science, and anti-rationalist historians of science, to catch even a glimmer of the urgent case for transforming academia is echoed by a failure in the wider community to catch onto the message. European culture – which, to a considerable extent means world culture – is still split into the two halves C. P. Snow wrote about all those years ago: the scientific rationalists, and the arts-based romantics (Snow, 1964). In order really to appreciate the rationale for, and the significance of, wisdom-inquiry, one needs to see it as a *synthesis* of Rationalism and Romanticism, an improvement over both. From traditional rationalism wisdom-inquiry takes a passionate belief in the profound importance and value of natural science, and the view of the world aim-oriented empiricist natural science has associated with it, namely physicalism. Wisdom-inquiry is passionately committed to rationality, to intellectual rigour and honesty, to objectivity, to the importance of trying to recognize the reality of cold fact however much it may go against our wishes and values. But wisdom-inquiry is also committed to more romantic values, to emotional and motivational honesty, honesty about aims, to the profound significance of art as revelations of value and as unmasking of false values in comedy and satire, to the significance of instinct, spontaneity and imagination, and the importance of attending to our emotional responses to things. This is no cobbling together of awkward opposites. On the contrary, romantic values are absolutely essential to the rationality, the rigour, of inquiry, if it is to be effective and rational in helping us realize what is of value in life.

Given this feature of wisdom-inquiry, its authentic synthesis of traditional rationalist and romantic intellectual ideals and values, one might think both

halves of our culture would welcome the viewpoint with open arms. So far, exactly the opposite has happened. Traditional rationalists take a superficial look at wisdom-inquiry and see science and reason being subverted by romantic ideals. Science must, they gather, incorporate metaphysics and philosophy, and, even worse, values and politics. Reason must include feelings, desires and values. The very term 'wisdom' is redolent, for traditional rationalists, of fraudulent romantic aspirations. And to cap it all, science is accused of being 'neurotic'. All this, clearly, amounts to yet another pathetic romantic attack on science and reason. Traditional romantics, on the other hand, looking superficially into the argument for wisdom-inquiry, see 'argument' continually being employed. They see appeals to reason, to rational problem-solving. They see the value and success of natural science being trumpeted. They see, even worse, the ultimate nightmare of physicalism being defended. Horror of horrors, the whole endeavour is supposed to usher in 'the rational society', an even greater nightmare. The ideal life, it seems, is the rational life, enslaved to reason. Reason is to have no restrictions on its scope of application at all. Here are the most brazen and idiotic claims and aspirations of rationalism being arrogantly proclaimed, outdoing even the most demented of the 18th century *philosophes*.

Thus, what should please everyone, ends up pleasing no one. The real difficulty, I believe, is that wisdom-inquiry, and the ideals it embodies, represents something really new. To appreciate wisdom-inquiry, at least as a viable possibility, is to experience the whole of our culture reorganized and realigned. It is to see our infinitely precious, fragile human world embedded in the physicalistic universe. It is to experience the miracle and the tragedy of human life immersed in the cosmos. It is to experience something of the hope, and the despair, which Pirsig expresses in *Zen and the Art of Motorcycle Maintenance*. It is to put together C. P. Snow's two cultures in a new way, so that each intermingles with the other, thus resolving problems and curing defects each cannot solve or cure in its own current terms, the gulf between the two dissolving away and disappearing. This amounts to a seismic shift in our intellectual and cultural landscape: it is not just a matter of adopting this philosophy of physics, or that philosophy of education.

So far, then, 22 years after publication of the first edition, there have been few signs of this seismic shift beginning to happen. There have been few signs of the basic argument catching on and beginning to exercise some influence over science, scholarship and education, or over academic policy. But major changes have occurred.

Let us consider, first, the big world beyond academia. In 1984, Margaret Thatcher was Prime Minister in Britain, and Ronald Reagan was President in

the USA. The cold war still existed. There was still the persistent possibility that, as a result of an accident, miscalculation or escalating crisis, the nuclear missiles would be unleashed and humankind, perhaps, would be for ever extinguished. Then the miracle occurred. The Berlin wall came down, the Soviet Bloc collapsed and, for a few years it almost seemed as if sanity might prevail, and a better world be created.

But then 9/11 happened. A monstrous crime, but not an act of war. After faltering initially, George Bush began to appreciate what an opportunity 9/11 presented him with. He could now do what he had wanted to do all along, invade Iraq. But first, Afghanistan had to be invaded. Then he was free to pursue his disastrous Iraq war, with its monstrous toll of deaths. Bin Laden planned 9/11, presumably, in order to provoke a reaction from the USA. He wanted to provoke a war between the Muslim world and the West. Bush's response must have vastly exceeded his expectations. It is almost as if George Bush has acted on instructions received from Bin Laden. But the great shame in all this, for the people of the USA and the UK is that after the horror of the Iraq war and subsequent hopelessly incompetent and brutal occupation, after all the political arrogance, stupidity and lies, both Bush and Blair were re-elected. Nothing could demonstrate more dramatically the need for adult education about the world's problems and what we need to do about them. Democratic governments are unlikely to be very much wiser than their electorates.

In short, if we are to tackle our global problems democratically, then it is the people of the world who need to know what these problems are, and what we need to do to resolve them. Wiser political leaders would be desirable, but what we really need is a wiser world population.

Over the 22 year period we are considering, academia did not rise to the occasion and put adult education at the forefront of its concerns. The scientific community has, however, increasingly come to emphasize the threat posed by global warming. Scientists now publicly declare global warming, not terrorism, to be the greatest threat to humanity – a declaration at the time of writing not heeded yet by the USA government, still in global warming denial. The prospects do not look good. Ice at the poles and in glaciers is melting at an alarming rate. As the polar ice melts, less sunlight is reflected back into space, which further contributes to global warming. And there are a number of other such 'tipping points'. Vast quantities of methane are trapped in permanently frozen ground in Canada and Russia, and under the sea. If global warming melts this ground, and the methane is released from the earth and sea, this will further accelerate warming, as methane is an even stronger greenhouse gas than carbon dioxide. Global warming might

turn tropical rain forests, already under threat, into deserts: the destruction of trees and other vegetation that this would involve would further contribute to carbon dioxide in the atmosphere, and to global warming. Millions of people may die as a result of drought, hurricanes, floods, and rising tides.

Scientists have done what they can to establish that global warming is taking place, almost certainly as result of rising levels of carbon dioxide in the atmosphere due to human activity, to industrialization. They have done what they can to warn governments and the public of the grave dangers we face. But have they done so soon enough, and effectively enough? And has academia done enough to develop and publicize policies that need to be implemented to decrease the amount of carbon dioxide being released into the atmosphere, before it is too late? It is of course inherent in the argument of this book that wisdom-inquiry is vastly more capable of getting across to the public and to government the dangers of global warming, the urgent need to respond before too much damage is done, than is knowledge-inquiry. Wisdom-inquiry is designed to alert the public to such matters, whereas knowledge-inquiry is not (as a glance at diagram 4 in chapter four will reveal). If wisdom-inquiry had been in place in the early 1960s, when reliable measurements of the steady increase in carbon dioxide in the atmosphere were made in Hawaii by C. D. Keeling, we might have been doing then what we are attempting to do now, forty years later, in 2006.[1]

There can be no doubt that some changes have taken place in academia since 1984 which can be regarded as steps towards putting wisdom-inquiry into practice (in complete ignorance, of course, of the argument of this book). Perhaps the most noteworthy change has been the creation of departments concerned with such things as social policy, environmental issues, peace, planning, transport and medical ethics. For example, a number of departments and research centres concerned with social policy have been created at my own university – University College London – during the years 1984-2006.

However, merely adding policy studies to knowledge-inquiry does not suffice to turn it into wisdom-inquiry. It may even be a step backwards. This is the case when governments employ academics to engage in policy research, but in such a way that there is no freedom to criticize policies determined by governments, or to improve policies or propose better alternatives. In this

[1] For an excellent account of the discovery of global warming, going back to John Tyndall's discovery that carbon dioxide is a greenhouse gas in 1859, and Svante Arrhenius's discovery of the possibility that we might cause global warming in 1896, see Weart (2003).

case, academia loses its intellectual independence and becomes no more than an arm of government. Natural scientists would not allow governments to determine for them what is to be accepted as scientific knowledge; equally, those engaged in policy research in universities cannot allow governments to decide for them what are the best available policies. It is vital that academic policy research has absolute intellectual independence from all external pressures, whether exerted by government, industry, the media, religious authorities or public opinion.

But for wisdom-inquiry we require more than flourishing policy research conducted in an atmosphere of intellectual independence. Policy studies need to be devoted to helping humanity resolve its most urgent conflicts and problems of living in increasingly cooperatively rational ways. They need to have, as their central aim, to help us realize what is of most value to us in life. Policy studies need to be at the heart of academia, influencing and being influenced by more specialized scientific and technological research, as indicated by figure 4 of chapter 4. For this to work, natural science itself needs to change; it needs to put humane aim-oriented empiricism into explicit scientific practice. Policy studies need to implement aim-oriented rationality, and need to take up the task of helping humanity implement aim-oriented rationality in personal, social, institutional and global life. Policy studies need to be pursued as the intellectually fundamental part of social inquiry which, in turn, needs to be pursued as the intellectually fundamental part of academic inquiry as a whole. The most basic task of academia needs to be to help humanity think and act in more cooperatively rational ways in life.

Thus – to give one small illustration of the kind of changes that are required – each nation ought to contain, within its university system, a shadow academic government, actively engaged in running the country in a purely virtual manner, proposing policies and legislation, taking decisions, in a way untainted by the inevitable corruption of power and office. The tasks of the virtual academic government would be, not simply to criticize the actual government, but to discover and broadcast what the actual government ought to be doing, were it to be acting in the best interests of its people and, insofar as this is possible, the best interests of the people of the world, unswayed by vested interests, pressure groups, public opinion, and the need to win the next election. The hope would be that actual and virtual governments would gradually discover how to learn profitably from each other, without this constituting a violation of democracy.

An obvious corollary of this point is that the university system of the world should create one or two virtual world governments, dedicated to

enacting in imagination what an actual world government ought to be doing, were it to exist. A related task for wisdom-inquiry would be to work out how a democratic world government can be created.

In short, adding policy studies to knowledge-inquiry does not suffice to create wisdom-inquiry – even though it may be a step in the right direction.

Some may hold that the growth of academic work and writing during the last twenty years (especially in north America) that are devoted to disadvantaged and unjustly treated groups – women, gays, Afro-Caribbeans, indigenous people of all kinds – is indicative of a move towards wisdom-inquiry. Without doubt wisdom-inquiry must be concerned to help solve problems of injustice (in a way which does not encroach upon freedom to an intolerable extent). Insofar as academic studies that are devoted to the interests of disadvantaged groups contribute towards this end, they do indeed indicate a move towards wisdom-inquiry. It is important, however, that such studies meet the high standards of rationality demanded by wisdom-inquiry. Contributions need, as a bare minimum, to get clearer about what the problems are, and to propose and critically assess possible solutions. Those who work in these fields need to be open to, and ready to learn from, criticism, whether it is friendly or hostile. What matters is the inherent intellectual value – and potential human value – of a contribution, and not the qualifications of the author, whether these have to do with academic degrees, personal background, skin colour, gender or ethnicity.

A case can be made out for holding that there is an especially strong potential link between wisdom-inquiry and feminism – a link not yet recognized by feminists themselves (or their critics).[2] For one can argue that today we have knowledge-inquiry, and not wisdom-inquiry, in our universities all over the world in part because, when modern science, and modern academic inquiry more generally, were being formed, from the 16th to the early 20th century, men were in charge. If women had been permitted to play an equal role in creating modern science and academia, things might have developed in a very different way, and we might today possess something more like fully fledged wisdom-inquiry.

It is often argued that men and women think differently, whether for genetic or cultural reasons, or for some combination of the two. Men are

[2] For a sympathetic survey of feminist literature on science and exploration of the issues see Schiebinger (1999); for criticisms of feminist philosophy of science see Pinnick et al. (2003). Both books ignore wisdom-inquiry entirely, and thus do not consider the possibility that the exclusion of women from science and academia may have played a role in suppressing the development of wisdom-inquiry.

more competitive, aggressive and argumentative, more concerned with objects than people, less ready to acknowledge emotion and uncertainty. Women are more cooperative in their thinking, more empathetic, more interested in people than objects, and much readier to acknowledge emotion and uncertainty. If these differences really do exist, on a statistical basis, then we have at once a partial historical explanation for the prevalence of irrational and damaging knowledge-inquiry. It is a consequence of men having played the dominant role in creating modern academe, women and female modes of thought having been excluded. Knowledge-inquiry is typically masculine in character: ostensibly impersonal, driven in part by unacknowledged competitive and aggressive emotions and motivations. Exclude women, and female modes of thought, from science and academia, and knowledge-inquiry is just what one might expect to emerge. Allow women, and female modes of thought to contribute on an equal basis with men and masculine thinking, and something closer to wisdom-inquiry might have been the outcome. For wisdom-inquiry requires a synthesis of masculine and feminine modes of thought. It needs the hard-edged, objective, aggressive, logic-constrained, fact-based style of masculine thought; but this needs to be combined with the more empathic, emotional and cooperative style of female thinking. Only when male and female thinking work together can wisdom-inquiry flourish.

I throw this suggestion out as a conjecture to be pondered. I am not sure how seriously it deserves to be taken. Many feminists will be hostile to the suggestion that there are inherent differences in the way men and women think, and so hostile to the idea that we have, so far, failed to develop inquiry rationally devoted to the realization of what is of value in life because women, in the past, have been excluded from the academic enterprise. But whether men and women, on a statistical basis, do or do not think differently, is a factual question. It damages the feminist cause to decide such a factual question on ideological grounds. ('Different' does not, of course, mean that one mode of thought is superior to the other: from the perspective of wisdom-inquiry, both are of value, both are needed, both are defective when separated from the other: it is the synthesis of the two in wisdom-inquiry that is so urgently needed and desirable.) If it turns out that men and women do indeed tend to think differently along the lines indicated, then feminists might well take up the argument of this book as an important ingredient of feminist philosophy. (And the fact that the argument has been put forward by a man should not in itself tell against it in feminist eyes.)

The factual conjecture concerning gender and modes of thought, indicated above, may be false. If so, we could still label the two modes of

thought 'masculine' and 'feminine', and make the point that if inquiry is to be devoted in a genuinely rational way to helping humanity realize what is of value in life, then it is essential that these two modes of thought are married together in the way required by wisdom-inquiry. Wisdom, at a personal level requires, of course, a synthesis of these two modes of thought (or at least the capacity to call upon both as needed). It might then be argued that a part of the reason for our failure to develop wisdom-inquiry during the last two centuries or so has to do with the exclusion of devalued 'feminine' modes of thought from academic inquiry, the outcome being what we have today, knowledge-inquiry embodying 'masculine' modes of thought.

In the first edition of this book, I confined myself to one feminist remark, namely: 'During ten years of advocating the philosophy of wisdom to all and sundry, I have found that most non-academic women know what I am talking about straightaway, most academic men do not, with non-academic men and academic women falling somewhere in between'. I did so, well aware that to say more would be the intellectual equivalent of wandering onto a minefield. No one seems to have noticed the implications of that brief remark.

I should perhaps add that if sociological reasons are required for our failure to develop wisdom-inquiry over the centuries, I would be much more inclined to stress the threat that wisdom-inquiry would pose to religious and secular authorities. Wisdom-inquiry would actively challenge these authorities in a way which knowledge-inquiry does not – and that would have been enough to ensure that wisdom-inquiry was not developed during the 17th, 18th and early 19th centuries. (It is interesting to note that those who created the Royal Society in Britain in the 1660s took pains to exclude political and religious issues from the remit of the Society.) Once standard empiricism and knowledge-inquiry are in place, their continued existence is all but ensured by the epistemological-institutional-motivational mechanisms discussed at the beginning of chapter six.

Progress towards wisdom-inquiry may have been held back in Britain, in particular, by the Research Assessment Exercise (RAE) introduced by Margaret Thatcher in 1986. This operates as follows. Every five years or so, the research output of every department in every university in the UK is subjected to assessment. Each subject has its own committee of assessors – deemed to be leading UK academics in the field. Each department submits its best research work to the committee to be assessed. The committee then grades each department from very good to very poor. Funds for research are then allocated on the basis of these results. Those departments that do well receive funds for research, while departments which do poorly receive little

research money. Members of such departments have to spend more time on teaching, and universities may even contemplate closing down departments which are given a poor grade by the RAE.

Donald Gillies, in a brilliant paper, argues that the RAE is almost bound to have an adverse effect on scientific progress, and on the quality of academic work more generally (Gillies, 2006). He considers the outstanding work of three individuals: Wittgenstein, Frege and Semmelweiss. Gillies argues that the RAE, if it had been in operation when these men did their important work, would have had bad effects on this work, and might even have stopped the work being done altogether.

Wittgenstein held a post at Cambridge University from 1930 to 1947, ending up as professor. During this time he worked on his *Philosophical Investigations*, regarded by many 'as the greatest philosophical masterpiece of the twentieth century' (Gillies, 2006).[3] It was published in 1953, after Wittgenstein's death in 1951. But during the 17 years while at Cambridge, Wittgenstein published nothing. If the RAE had been operating at that time, Gillies convincingly argues, it would have been difficult for Wittgenstein to continue at Cambridge University as a research philosopher.

In 1879 Frege published *Begriffsschrift* which, more than any other work, created modern mathematical logic. It was dismissed at the time by his fellow logicians as worthless (see Bynum, 1972) and, despite publishing further important work, with increasing difficulty, Frege got no real recognition for his work during his lifetime (see Gillies, 2006 for details). If the RAE had been in operation at the time, this lack of recognition would have made it even more difficult for Frege to continue with his research. His teaching and administration load would have increased, and he might have been urged to take early retirement.

A similar counterfactual story can be told in connection with Semmelweiss. During the years 1844 to 1849, Semmelweiss discovered that women were dying of puerperal fever after giving birth in the hospital where he worked because doctors did not disinfect their hands properly before carrying out examinations in the maternity ward. Semmelweiss's discovery was rejected by the medical profession, and it was only some twenty years later that the medical profession began to appreciate the importance of disinfection. Once again, if Semmelweiss had been subjected to assessment by the RAE, this might have made it impossible for him to do his research.

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[3] I do not myself hold this view.

Gillies also discusses the well-known case of Copernicus, who published nothing in his lifetime, and whose heliocentric theory was not appreciated for nearly a century after its posthumous publication.

Gillies goes on to point out that there are two kinds of error that can be made in funding research. A type I error is failing to fund good research that would lead to important results, while a type II error is funding research that produces nothing of value. The RAE concentrates exclusively on avoiding type II errors, but in doing so, as we have seen, it considerably increases the chances of committing type I errors. But from the standpoint of scientific progress, type I errors are much more serious than type II errors. The RAE is designed to impede scientific progress.

There are many other cases of people making important scientific or intellectual contributions and receiving no recognition for their work for twenty years or more. Thomas Young's discovery of the wave character of light via his interference experiment was initially dismissed by his peers. Gregor Mendel's discovery of some basic laws of genetics famously had to wait several decades before it received recognition. This was true, too, of Alfred Wegener's theory of continental drift, and John Waterston's contribution to statistical mechanics. Georg Cantor met with opposition when he developed set theory – of profound importance to the whole of mathematics. E. Stückelberg failed to receive recognition for his important contributions to quantum field theory. And Guy Callendar failed to convince when he announced in 1938 that increased emissions of carbon dioxide as a result of human activity was leading to global warming. These cases, I am sure, merely scratch the surface.

But how, it may be asked, may the RAE impede acceptance and implementation of wisdom-inquiry? To begin with, as long as knowledge-inquiry intellectual standards are in place, the RAE will make it even more difficult to do wisdom-inquiry research. The point was made to me in a striking way by Dr. Caren Levy, director of the Development Planning Unit at University College London. Her work and research, like those of others in her Unit, is concerned to help the poor tackle their problems of living in Africa and Asia. Here, if anywhere in academe, wisdom-inquiry is being put into practice. But this creates a dilemma. On the one hand, Dr. Levy can publish papers in relevant academic journals, which gain recognition by the RAE but may not lead to anything of value in the real world. On the other hand, reports produced by Levy, dealing with developmental problems in Africa and Asia, widely read by many grappling with these problems, taken up and implemented by the UN and other organizations, and having practical consequences of value in the real world, receive no recognition from the

RAE at all, because the relevant reports are not published in academic journals acknowledged by the RAE. In this way, the RAE increases the pressure on academics to produce orthodox , and often useless, knowledge-inquiry work, instead of really worthwhile wisdom-inquiry work – pressure, I hasten to add, which Levy resists (even if others in other departments do not).

Essentially the same point, in another context, is made by Mike Hulme, Director of the Tyndall Centre for Climate Change Research (discussed below). He writes 'A research paper in *Nature* or *Science* brings a very different set of rewards to the individual than being invited to give oral evidence in front of a Parliamentary Select Committee. Within the narrow confines of the academic establishment, the former is much more likely to bring career rewards than the latter. Yet in terms of impact … or for influencing decisions the latter may be much more significant' (Tyndall Centre, 20006, p. 6).

The RAE may have other adverse effects as well. It may well be especially difficult for a revolutionary idea like that of wisdom-inquiry to get a fair hearing in an academic world constrained by the RAE. The RAE puts additional pressure on academics to engage in safe research work that can be relied upon to result in acceptable publications within a period of two or three years. Taking seriously a wild, unknown idea like wisdom-inquiry provides no guarantee that the outcome will be quick, reliable publications. One might well be putting one's career at risk. And it is not just that wisdom-inquiry is a revolutionary idea; in addition, it leads to a change in the rules of the game. What counts as intellectual excellence differs, depending on whether knowledge-inquiry or wisdom-inquiry is accepted. Research work that is excellent if judged from the standpoint of wisdom-inquiry might well be unacceptable when judged in terms of the rather different criteria in place at the moment. Taking up wisdom-inquiry might mean one fails to get one's work accepted for publication at all. Furthermore, there is a strong interdisciplinary element inherent in wisdom-inquiry. A philosopher, as a result of being convinced by the arguments for wisdom-inquiry, might suddenly find that his education as an academic philosopher is seriously inadequate: he might find he needs to acquire some knowledge and understanding of work that is far from his field of expertise – in theoretical physics, cosmology, evolutionary biology, mathematics, psychology, anthropology, current affairs and politics. The serious work that is required to gain this new knowledge and understanding would not help his publication output. Furthermore, accepting wisdom-inquiry may lead to a dramatic change in *what* one should seek to publish, and *where* one should seek to publish it. It may become more important to communicate to the

public than to one's fellow academics. The RAE might not recognize such publications as worthy of assessment.

Near the beginning of chapter six I indicated the kind of factors that enable knowledge-inquiry to retain such a grip on so much of scientific and academic work. The RAE serves to strengthen all these factors. It tends to help preserve the *status quo*, whatever that may be, and make the introduction and adoption of revolutionary ideas more difficult – especially ideas that have an interdisciplinary character, and lead to changes in views as to what constitutes intellectual excellence.

Soon after I began my academic career as a young lecturer in philosophy of science at University College London, in the late 1960s, I realized I had a choice. Either I could have an academic career, or I could pursue my interests, but I could not do both. Just then, philosophy of science had become severely professional. The leading UK journal in the subject, *The British Journal for the Philosophy of Science*, had stopped publishing fascinating articles by scientists on subjects such as cosmology, and was instead full of dreary papers on confirmation theory written by professional philosophers of science. In order to further my career as a philosopher of science, I realized, I would have to work on such subjects myself. But to me, at the time, an academic career struck me as nothing compared to the opportunity I had to explore what I was interested in. I plunged into finding out about quantum theory, topology, the conscious brain, cosmology, the history of ideas – interests always related to the problems I was grappling with, but with no thought of eventual publication. (I had come to the conclusion, even then, that academic publication had more to do with furthering careers than communicating ideas and results.) I was also, of course, exposed to the influence of the 1960s. There was this wonderful new idea around that life was for *living*, not for jobs and careers, and we young people were going to inherit the earth and transform it immeasurably for the better, putting an end to war, poverty and misery. I was enthralled by the '60s, but also appalled by its anti-intellectual romanticism, its political naivety, and by its idea that authentic feeling was everything, and hypocrisy the ultimate sin. But it was because I had the freedom to step back from the grindstone of work then current in philosophy of science, and consider problems and issues of concern to none of my academic colleagues, that I was led to stumble across the ideas and arguments spelled out in this book. I must also confess that I was, in those days, ferociously ambitious, but ambitious to make a contribution to the human spirit, not to academic philosophy of science. I had decided, however, that Karl Popper had solved the main problems in the field: I explored aspects of a problem he had not then said anything about –

the problem of seeing how the human world could fit into the physical universe, discussed in chapter ten. I also began to ponder defects in Popper's falsificationism, and it was this that led me to aim-oriented empiricism and wisdom-inquiry.

I wonder whether it would be as easy today – as it was for me in the late '60s – for a young academic to step back from current research issues, forego an academic career, and explore questions of real interest wherever they might lead, with no thought of eventual publication? It is just this that a number of academics probably need to do today if wisdom-inquiry is eventually to become a viable option for academia.

In those far off days of the 1960s, the main idea behind university policy seemed to be to try, in the first place, to hire good people, and then give them the freedom to follow up their interests, in the hope that this would, in some cases at least, lead to work of real value. I was shocked that so few of my fellow academics seemed to seize the opportunities that were available to them. But all this has now changed, in the UK at least. Whether because of the RAE, or because of other factors, academic life seems to have become so much more pressured, busy, burdened with administration, preoccupied with careers, grants, publications and assessments.

Was I ever adversely affected by such pressures? Possibly I was. Applications for promotion were turned down, year after year. Finally I was told that the head of mathematics, who chaired the committee that decided these things, said 'When I consider Maxwell's work, I feel humble, and I can assure you it is very rare for me to feel humble'. This seemed to me to be a very strong recommendation indeed, especially coming from a Professor of mathematics, but the then Provost, who chaired the final, rubber-stamping committee, declared (I was told) 'If ever Maxwell's ideas are taken up, it would be the end of science as we know it, so we should not promote him'. When I complained, he informed me that I did not fit into the ethos of the place and I might do better if I looked for a job in the USA. His successor, a few years later, told me, when I complained to him about bullying I was being subjected to in my department, that my work 'seems to have taken a new direction'. This prompted me to reply 'Oh, have universities in Britain sunk so low one is now penalized for originality?'. My work was investigated, and it was whispered to me that I should apply for a readership. I did, and this time I was successful. But the bullying continued, my life in the department became impossible, and I took early retirement in 1994. If I had stayed on, I would probably have ended up a Professor, and that would have made it easier for me to get my ideas across. But I do not think I would have

been able to produce the body of work that I have produced since 1994 (but work largely ignored).

Frank Kermode recently had this to say about the state of universities and education in the UK: 'Universities are being driven by madmen. And education in general is being run by lunatics'.[4]

I turn now to the question of what has been done since 1984 that can be regarded as contributing, in a direct way, to the development of wisdom-inquiry.

Knowledge and wisdom inquiry are not mutually exclusive. They overlap. Many – perhaps most – excellent contributions to knowledge-inquiry will also be excellent contributions to wisdom-inquiry. In amongst the standard dross of scientific and scholarly work, there is a great deal of excellent work – excellent when judged from either standpoint. All this I now ignore, and concentrate on a few scattered contributions and developments known to me, mainly in Britain, that seem significant from the standpoint of a possible growing interest in wisdom-inquiry.

As I have already indicated, in my view the most significant developments that have taken place within universities during the last twenty years that may be construed as steps towards wisdom-inquiry are the creation of departments, institutions and research centers concerned with social policy, with problems of environmental degradation, climate change, poverty, injustice and war, and with such matters as medical ethics and community health. A number of departments and research centres concerned in one way or another with policy issues have been created at my own university of University College London since 1984.

At Cambridge University, there is a more interesting development. One can see the first hints of the institutional structure of wisdom-inquiry being superimposed upon the existing structure of knowledge-inquiry. As diagram 4 of chapter 4 indicates, wisdom-inquiry puts the intellectual tackling of problems of living at the heart of academic inquiry, this activity being conducted in such a way that it both influences, and is influenced by, more specialized research. Knowledge-inquiry, by contrast, organizes intellectual activity into the conventional departments of knowledge: physics, chemistry, biology, history and the rest, in turn subdivided, again and again, into ever more narrow, specialized research disciplines. But this knowledge-inquiry structure of ever more specialized research into diverse aspects of knowledge and technological know-how is hopelessly inappropriate when it comes to tackling our major problems of living. In order to tackle environmental

[4] Quoted in *The Guardian*, g2, 29 August 2006, p. 24.

problems, for example, in a rational and effective way, specialized research into a multitude of different fields, from geology, engineering and economics to climate science, biology, architecture and metallurgy, needs to be connected to, and coordinated with, the different aspects of environmental problems. The sheer urgency of environmental problems has, it seems, forced Cambridge University to create the beginnings of wisdom-inquiry organization to deal with the issue. The 'Cambridge Environmental Initiative' (CEI), launched in December 2004, distinguishes six fields associated with environmental problems: conservation, climate change, sustainable technology, natural hazards, society, and technology, and under these headings, coordinates some 87 research groups working on specialized aspects of environmental issues in some 19 different (knowledge-inquiry) departments: see http://www.cei.group.cam.ac.uk/. The CEI holds seminars, workshops and public lectures to put specialized research workers in diverse fields in touch with one another, and to inform the public. There is also a CEI newsletter.[5]

A similar coordinating, interdisciplinary initiative exists at Oxford University. This is the Oxford University Centre for the Environment (OUCE), launched in 2005. It is based in the department of geography and in the Environmental Change Institute and the Transport Studies Unit, all at Oxford, and has links with the UK Climate Impacts Programme, also based at Oxford and founded in 1997, and the UK Energy Research Centre, founded in 2004 and concerned with research into sustainable energy. The OUCE links up at least 34 specialized research groups or centres under the heading of five 'research clusters'.

Even more impressive, perhaps, is the John Tyndall Centre for Climate Change Research, founded by 28 scientists from 10 different universities or institutions in 2000. It is based in six British universities, has links with six others, and is funded by three research councils, NERC, EPSRC and ESRC (environment, engineering and social economic research). It 'brings together scientists, economists, engineers and social scientists, who together are working to develop sustainable responses to climate change through trans-disciplinary research and dialogue on both a national and international level – not just within the research community, but also with business leaders, policy advisors, the media and the public in general' (http://www.tyndall.ac.uk /general / about.shtml). It is clear from the Centre's own account of its work

[5] A somewhat similar, if more modest, coordinating entity was created at University College London in 2003/4. Called the 'Environmental Institute', it seeks to link 20 research groups in some 13 departments.

(see Tyndall, 20006), that innovations in this work are strikingly in accordance with basic features of wisdom-inquiry. We have here, perhaps, the real beginnings of wisdom-inquiry being put into academic practice.

A similar organization, modeled on the Tyndall Centre, is the UK Energy Research Centre (UKERC), launched in 2004, and also funded by the three research councils, NERC, EPSRC and ESRC. Its mission is to be a 'centre of research, and source of authoritative information and leadership, on sustainable energy systems' (http://www.ukerc.ac.uk/). It coordinates research in some twelve British universities or research institutions. At the time of writing, UKERC is about to launch the National Energy Research Network (NERN), which will seek to link up the entire energy community, including people from academia, government, NGOs and business.

Another possible indication of a modest step towards wisdom-inquiry is the growth of peace studies and conflict resolution research. In Britain, the Peace Studies Department at Bradford University has 'quadrupled in size' since 1984 (Professor Paul Rogers, personal communication), and is now the largest university department in this field in the world. INCORE, an International Conflict Research project, was established in 1993 at the University of Ulster, in Northern Ireland, in conjunction with the United Nations University. It develops conflict resolution strategies, and aims to influence policymakers and others involved in conflict resolution. Like the newly created environmental institutions just considered, it is highly interdisciplinary in character, in that it coordinates work done in history, policy studies, politics, international affairs, sociology, geography, architecture, communications, and social work as well as in peace and conflict studies. The Oxford Research Group, established in 1982 (just two years before the period we are considering), is an independent think tank which 'seeks to develop effective methods whereby people can bring about positive change on issues of national and international security by non-violent means' (www.oxfordresearchgroup.org.uk/). It has links with a number of universities in Britain. Peace studies have also grown during the period we are considering at Sussex University, Kings College London, Leeds University, Coventry University and London Metropolitan University. Centres in the field in Britain created since 1984 include: the Centre for Peace and Reconciliation Studies at Warwick University founded in 1999, the Desmond Tutu Centre for War and Peace, established in 2004 at Liverpool Hope University; the Praxis Centre at Leeds Metropolitan University, launched in 2004; the Crime and Conflict Centre at Middlesex University;

and the International Boundaries Research Unit, founded in 1989 at Durham University.[6]

I conclude with a few remarks about particular developments known to me which may be regarded as steps towards wisdom-inquiry. Demos, a British independent think tank has, in recent years, convened conferences on the need for more public participation in discussion about aims and priorities of scientific research, and greater openness of science to the public: see Wilsdon and Willis (2004). This has been taken up by The Royal Society which, in 2004, published a report on potential benefits and hazards of nanotechnology produced by a group consisting of both scientists and non-scientists. The Royal Society has also created a 'Science in Society Programme' in 2000, with the aims of promoting 'dialogue with society', of involving 'society positively in influencing and sharing responsibility for policy on scientific matters', and of embracing 'a culture of openness in decision-making' which takes into account 'the values and attitudes of the public'. There is a growing awareness among scientists and others of the role that values play in science policy, and the importance of subjecting medical and other scientific research to ethical assessment. Even though academia is not organized in such a way as to give intellectual priority to helping humanity tackle its current global problems, semi-popular books appear, some written by academics, that tackle these issues: see Mason (2006), Monbiot (2006), Martin (2006), Homer-Dixon (2006). Since around 2000, emailing groups have been created which concern themselves with these matters, such as Crisis-Forum, and one that I founded, Friends of Wisdom. As I mentioned in chapter six, in recent years many departments of 'science, technology and society' have been created in the USA, the UK and elsewhere, the intention being that these departments will concern themselves with interactions between science and society. Put 'science technology and society' into Google, and up come 719,000 items. 'Environmental studies' yields 11.7 million, 'development studies' over 1 million, 'peace studies' just under a million, but 'wisdom studies' a mere 10,200 (although 'wisdom' has 90 million entries). These at least were the figures on the 19 November 2006. One of the top entries under 'wisdom' is Copthorne Macdonald's 'The Wisdom Page' – a compilation of 'various on-line texts concerning wisdom, references to books about wisdom, information about organizations that promote wisdom', and including a bibliography of more than 800 works on wisdom prepared by Richard Trowbridge. There are also details about Macdonald's own books on the subject. Sternberg (1990) provides an

[6] For an account of the birth and growth of peace studies in universities see Rogers (2006)

account of the work of nineteen researchers in universities in north America and Germany into the 'nature, origins and development' of wisdom. Subsequently, the editor of this book, Robert Sternberg, has done research on teaching for wisdom – the idea being that whatever else is being taught, physics, anthropology, economics, it should be taught in such a way that students also acquire wisdom. This has been taken up by some teachers and educationalists, mainly in the USA.

None of these developments quite amounts to advocating or implementing wisdom-inquiry. One has to remember that 'wisdom studies' is not the same thing as 'wisdom-inquiry'. Nevertheless, these developments can be regarded as indicating that there is a growing awareness of the need for our schools and universities to change so as to help individuals learn how to realize what is genuinely of value in life – and help humanity learn how to tackle its immense global problems in wiser, more cooperatively rational ways than we seem to be doing at present.

Chapter Thirteen
Replies to Criticisms

When first published, this book received many excellent reviews: see in particular Longuet-Higgins (1984), Collingridge (1985), Richards (1985), Sardar (1985), Easlea (1986), Midgley (1986), Foss (1986), Ravetz (1987), Hendry (1989) and Koertge (1989). But it also received some harsh criticism.

Steven Rose, writing in the *New Statesman*, declared that 'The trouble with [Maxwell's] neo-Benthamite approach is that it is stronger on morality than it is on politics. Suppose that what is of most value to me is the creation of a neutron bomb or a more lethal nerve gas, by what rule does the philosophy of wisdom say nay? Maxwell's exhortations are thus scarcely likely to raise even an alarm bell on the frontiers of Medawarian science' (Rose, 1985, p. 30). In my reply I wrote 'I am baffled. Throughout my book I argue that the proper basic task of the academic enterprise is to help humanity resolve its problems of living in more and more *cooperatively rational* ways. This programme of progressive cooperativism is of course in part moral – as well as being intellectual and methodological. But it is also, as I repeatedly emphasize, economic, political and international. Resolving conflicts in cooperatively rational ways quite obviously cannot involve employing neutron bombs and nerve gases So grotesque is Professor Rose's misrepresentation of my book that I can only conclude he has not bothered to read it' (Maxwell, 1985b). The philosophy of wisdom has no difficulty in supplying *moral* grounds for banning neutron bombs and nerve gases. There is of course, in addition, the specifically *political* problem of ensuring that no one uses or produces neutron bombs or nerve gases – a problem very much still with us at the time of writing. The only way to do this is to establish international agreements banning such 'weapons of mass destruction', backed up by world-wide inspection, and ultimately the internationally agreed use of force against any rogue state actively threatening to use such weapons (a strategy sabotaged, at the time of writing, by the USA). We need, in other words, international political cooperation, which is just one of the basic themes of this book. It is difficult to imagine that Rose would have a better policy. Either Rose did not read my book, or he did and deliberately set out to create the impression the book is a work of idealistic nonsense because it does not quite accord with his own left-wing convictions.

Rose also complained that I had failed to consider feminist criticisms of science. There was one implicit feminist remark in the book, in chapter 11, explosive in its implications (as I was well aware), quoted again in chapter 12.

Almost all the feminist criticism of science known to me misses the point in criticizing the *content* of science, when what it should criticize is the aims and methods, the philosophy. There do seem to be differences in the way men and women think. It may well be that this has a genetic basis, and is not just a matter of upbringing and culture. As I suggested in chapter 12, the philosophy of knowledge, with its stern exclusion of feelings, values and problems of living, its fierce competitiveness, may accord with male ways of thinking, while the philosophy of wisdom, with its emphasis on cooperative rationality, problems of living, feelings and values, includes female ways of thinking as well. The long-standing exclusion of women, until recently, from science, and from academia more generally, may well be related to the fact that it is the philosophy of knowledge, not the philosophy of wisdom, that has prevailed. But, be that as it may, our reasons for holding that we need to replace the philosophy of knowledge with the philosophy of wisdom must be that the philosophy of wisdom provides us with a more intellectually rigorous and humanly valuable kind of inquiry. That the philosophy of wisdom does better justice to female modes of thought cannot in itself justify preferring it to the philosophy of knowledge. What feminists who are critical of the *status quo* ought to do, in my view, is work towards implementing the philosophy of wisdom: this would introduce more female modes of thought into academia, and would at the same time provide a rationale for doing this, arising from the independent and objective grounds for preferring wisdom-inquiry to knowledge-inquiry. So far this has not happened, yet one more unfortunate consequence, perhaps, of the neglect of the first edition of this book. (Certainly no feminist reading Rose's review would be inclined to think this book carries a feminist message.) Instead of helping to bring about the revolution towards wisdom, feminists have engaged in ill-judged criticism of the *content* of science, or of what is intellectually excellent in current scientific and academic work, criticism which has badly backfired: see, for example, Sullivan (1998) and Patai and Koertge (1994).

Patrick Enfield, writing in the *Times Literary Supplement* (Enfield, 1985), had a number of complaints to make. (1) He complained that 'ideas so lucidly presented in the first chapter become progressively less clear during the course of the book'. (2) He complained that 'no indication is given' of how philosophers discussing questions about knowledge 'is supposed to show that the nineteen specific theses Maxwell associates with the philosophy of knowledge are generally accepted'. (3) Having characterized standard empiricism as the doctrine that 'acceptance or rejection of theories is determined by the empirical facts alone', Enfield then went on to assert that my 'claim that this is the received philosophy of science is false, and [my]

arguments against it are unoriginal, except with regard to some of the simplifications [I] introduce'. (4) Enfield goes on to complain that the 'distinction between the philosophies of knowledge and of wisdom is rather elusive because while the former admits that cognition is sought with the deeper aim of improving life through practical applications, the latter admits it as of value for its own sake. Maxwell argues that the philosophy of wisdom stresses the profoundly personal and social character of inquiry which is pursued for its own sake. But no explanation is given as to why the philosophy of knowledge cannot also be thus characterized.' (5) Enfield went on to say: 'One interesting idea in Maxwell's account is the proposal that reason should be brought to bear not merely on the selection of means to ends, but on the ends themselves, since the choice of social objectives is problematic. This idea does not seem required by the argument, since the book is motivated by uncontroversial social problems such as mass starvation in the Third World. Moreover, it is notoriously unclear how ends can be rationally discussed at all, and Maxwell fails to answer the Humean objection that reason can only help to further, and not to determine, our ends.' (6) Enfield concluded his critique by remarking: 'Workers in particular fields may be more or less directly concerned with practical applications, and there have long been fields whose concerns were more or less practical, such as medicine and engineering. There may be a need for reorientation of priorities in certain areas, as in the frequently cited case of the intensive arms-related research in physics. But are specific proposals of this kind part of a wholesale intellectual revolution?'

I take these points in turn. (1) Enfield may have found ideas becoming less clear during the course of the book because he misinterpreted these ideas to begin with, and failed to modify his conception of them in the light of subsequent discussion. His subsequent remarks, which indicate a series of misunderstandings, would seem to bear this out. (2) My grounds for declaring the philosophy of knowledge prevails over academia are to be found in chapter 6, of which Enfield makes no mention, and have little or nothing to do with the discussion of philosophers. (3) I made it quite clear that standard empiricism takes simplicity considerations into account in addition to evidence, and may ignore falsifying evidence for a time in the kind of way depicted by Kuhn and Lakatos. (This is clear even in Maxwell, 1974). Thus, in the first edition, while characterizing standard empiricism, I said '…in science, choice of theory may be biased in the direction of some untestable metaphysical conjecture about the world, some paradigm or 'hard core', in the kind of way described by Kuhn (1962) or Lakatos (1970)' (see

page 33 of this edition).[1] I went on to say 'It is generally agreed that scientists quite properly choose simple rather than complex theories, other things being equal' (p. 46), and I then indicated how twelve distinct contemporary views in the philosophy of science are all versions of standard empiricism (pp. 49-51). It is only with respect to his grotesquely distorted characterization of standard empiricism that Enfield is correct in declaring that it is false that this is the received philosophy of science. (4) My argument is that the philosophy of wisdom does better justice to inquiry pursued for its own sake than does the philosophy of knowledge, not the absurdity that the philosophy of knowledge has nothing to do with inquiry pursued for its own sake. The philosophy of knowledge may be characterized in personal and social terms, as I acknowledge: the difference between the two views is that the philosophy of wisdom stresses that our problems of living are intellectually fundamental, and that inquiry, at its most fundamental, is the thinking we engage in as we live – both points denied by the philosophy of knowledge. (5) Even tackling mass starvation in the third world involves problematic issues about aims. Many sincere attempts to tackle poverty and starvation have only succeeded in making matters worse, by undermining local agriculture, and by making people dependent on aid. There are also issues (perhaps not at present very pressing issues) arising from the question of how far democratic governments of wealthy nations are justified in going, in providing aid to the poor and starving. More important, even though a major motivation for my book may have been to alleviate third-world poverty, the fundamental and long-term hope is that academic inquiry will take, as its basic aim, to help us realize what is of value in life, help us make progress towards a good world. But both these aims are notoriously inherently problematic. Simplistic ideas about what would constitute a good world, influencing political ideologies and governments, have led to unspeakable horrors. One need only consider Marxism and communism. Enfield goes on to say I fail 'to answer the Humean objection that reason can only help to further, and not to determine, our ends'. But I took great pains emphasize that aim-oriented rationalism is designed to help us to improve our choice of ends, but does not, cannot, and should not, of itself, *determine* our ends: see pp. 98-132 of this edition. Thus, on pages 100-1, I said 'The whole point of reason is to help us to act and decide as we really do want to act and decide: it is to enhance *our own capacity* to act and decide as we really want, not to wrench the capacity from us or to reduce it to the one decision

[1] In referring to the first edition, I refer throughout this chapter, to the relevant page(s) of this 2nd edition.

to obey henceforth the dictates of reason'. (6) I recognize, of course, that philosophy-of-knowledge inquiry includes work concerned primarily to solve practical problems, as in medicine and engineering. The transition from knowledge to wisdom inquiry does not, however, just involve a 'reordering of priorities' in such fields. It involves transforming the nature of social science so that it becomes, primarily, social methodology or social philosophy, giving intellectual priority to the increasingly cooperatively rational tackling of conflicts and problems of living. It involves transforming the relationship between natural science and social inquiry, so that the latter becomes intellectually fundamental. It involves transforming the relationship between academia and society, so that academia becomes much more like an ideal public civil service, doing openly for the public what actual civil services are supposed to do in secret for governments. Enfield ignores all this, and ignores too the arguments I spell out in support of the thesis that the gross, structural irrationality of inquiry pursued in accordance with the philosophy of knowledge (when judged from the standpoint of helping humanity realize what is of value) is directly responsible for our failure to learn how to resolve our conflicts and problems of living more successfully, directly responsible, indeed, for many of our current global problems, which have arisen because of our new power stemming from modern industry and technology, associated with modern science. Harmful consequences of putting the philosophy of knowledge into academic practice instead of the philosophy of wisdom do not just stem from the wrong 'priorities' of technological research.

Enfield sniped, not at theses and arguments to be found in this book, but at hallucinations – at grossly distorted versions of these theses and arguments. But in doing so he no doubt ensured that those who read his review did not bother to consult this book.

John Kekes, contributing an essay-review published in *Inquiry*, began his attack with some praise (Kekes, 1985). He declared that 'The book is written in simple straightforward language' and that it raises 'an important and fundamental question'. 'It is a merit of the book', he said, 'that it forces one to think about the important issues it raises.' But he then went on immediately to say 'I remain totally unpersuaded that we need the revolution Maxwell advocates'.

Kekes's criticisms, like Enfield's, were unfortunately based on a misunderstanding of what this book argues. Kekes attacked gross distortions of what the book says; he even criticised the book for defending a doctrine which is discussed and – not defended – but *criticized* in the book.

Kekes summarized the book like this:

'The philosophies of knowledge and wisdom are rival interpretations of the Enlightenment program . . . Both are concerned with understanding scientific method, both regard scientific method as the paradigmatically rational method, and both aim to solve problems of living by applying scientific method to them. Maxwell's view is that the philosophy of knowledge radically misunderstands scientific method. This is much worse than an intellectual blunder, for its cost is paid by the endless suffering of victims of war, poverty, exploitation, and social injustice. If the correct scientific method were applied, these problems would be greatly diminished.'

From this, and from other passages in Kekes's article, it is rather natural to conclude that in arguing for the abandonment of the philosophy of knowledge and the adoption instead of the philosophy of wisdom, I am arguing for no more than a change in scientific method. But this, of course, is only a small part of the argument (as I have just indicted in my reply to Enfield). It is not just the methods of science that need to be changed, but the aims of science as well. It is not just science that needs to be changed, but the whole of inquiry – the most important and radical changes being made to those parts of inquiry that are not science: social inquiry, scholarship, the humanities, and education. Social inquiry needs to cease to be, at the most fundamental level, social science, or the pursuit of knowledge of social phenomena; instead it needs to have the quite different basic task of proposing alternative possible human actions, to be critically assessed from the standpoint of helping us, if implemented, to resolve our local and global conflicts and problems of living in increasingly cooperative ways. And this radically new kind of non-scientific social inquiry needs to be pursued as intellectually more fundamental than the natural sciences. Furthermore, it is not just all of academic inquiry that needs to be changed, but even more important, the whole way in which academic inquiry is related to the rest of the human world. We need to recognize that the most important and intellectually fundamental kind of inquiry going on in the world is that thinking we engage in as we live which guides our personal and social actions. Academic inquiry emerges out of this and, properly constituted has, as its basic task, to help enhance the intellectual quality and relevance of this personally and socially active thought. To reduce all this, as Kekes, does, to the thesis that what needs to be changed is scientific method is to miss out nine tenths of what the book advocates. Kekes in effect interprets the philosophy of wisdom to be advocating merely that both natural and social science should implement aim-oriented empiricism.

The philosophy of wisdom corrects *three* basic blunders inherent in the philosophy of knowledge, inherited from the Enlightenment, concerning (1) scientific method, (2) how this is to be generalized, and (3) how, once generalized, this is to be applied to social life. All the above changes involved in moving from the philosophy of knowledge to that of wisdom, apart from the initial change in scientific method, stem from correcting (2) and (3). Kekes, in ignoring (2) and (3), and considering (1) only, misses out on almost all of the above. And as a result, what Kekes criticizes is merely his own minimalist distortion of the message of the book.

Kekes has two basic criticisms to make. The first is that organized inquiry of the kind I advocate can only help us solve 'simple' not 'complex' problems. Kekes defines as 'simple', problems which require, for their solution, the removal of an obstacle in the environment. He defines as 'complex', problems which 'arise because we have conflicting wants, because we have to choose between undesirable or worse courses of action, because some of our wants are immoral or harmful, because we are often unsure about what we want, because the satisfactions we seek ought not to be sought, or because we may repress our wants'. And he remarks: 'Simple problem-situations require us to try to change or control the environment; complex problem-situations require us to try to change or control ourselves'. And Kekes's critical point is that whereas 'science is the best method we have for . . . finding solutions in simple problem-situations', it is hopelessly inappropriate when it comes to solving complex problems. And yet the problems that are the central concern of the book – war, poverty, exploitation, social injustice – are all 'complex', and thus unamenable to solution 'by the application of scientifically accredited techniques'.

Kekes's second criticism is that what I advocate founders because there are a plurality of values and ways of life in the world. In advocating a version of the Enlightenment programme I am in effect, according to Kekes, advocating one way of life based on Enlightenment values of liberty, equality, and fraternity, and on progress through reason and science. Yet my major concern is with the problems of living of the poorest people of the Third World. But it is above all here, in the Third World, that societies are organized in terms of traditional values and ways of life radically different from those of the Enlightenment. What I advocate thus amounts, according to Kekes, to the imposition of Enlightenment values on quite different Third-World cultures. Implementation of such a programme has disastrous consequences. What is needed 'is not the implementation of the Enlightenment program in alien contexts, but the nourishing of some native traditions'.

Kekes's two criticisms can be summed up by saying that what I advocate amounts, first to a disastrous kind of 'scientific imperialism' (Kekes's phrase), and second to a more general kind of European or Western cultural imperialism to be imposed upon the Third World.

These two criticisms strike me as not just invalid, but quite bizarrely at odds with what this book does argue for. The whole raison d'etre of the book is to develop a kind of inquiry which helps people everywhere, in all circumstances, to tackle for themselves their 'complex' problems of living, in increasingly cooperative ways, so that what is of value in life, in all its rich diversity, may be realized – and so that we may gradually develop a more cooperative and wiser world. This I see as the very opposite of imperialism – whether military, political, economic, cultural, or 'scientific'.

In developing his first criticism Kekes asserts:

'We look in vain for anything more than the most superficial remarks about what problems are. The closest Maxwell comes to a general characterization [of a problem] is: 'any problem can be construed to be an aim A, a provisional route R to the realization of A . . . and a barrier which blocks the attainment of A along R' (p. 105). The model implicit in this way of thinking leads us to view problems as obstacles. I find nothing in the book inconsistent with this understanding of problems, and there are countless passages that presuppose it.'

On this basis Kekes interprets me as conceiving of problems as 'simple' (involving removal of obstacles in the environment) rather than 'complex'. He neglects to point out that I nowhere identify 'obstacle' with 'obstacle in the environment'. He neglects to point out that the quoted sentence, which is in parentheses, is part of a paragraph concerned to establish that all problem-solving is a special case of active aim-pursuing in the world. Far from analysing problems in this introductory paragraph to chapter five, I am on the contrary explaining how and why the notion of problem-solving rationality, developed in the previous chapter, needs to be recast as a more general and useful notion of 'aim-oriented rational action', the key methodological idea of the book.

But what is really outrageous is that Kekes completely ignores the fact that in the very next three paragraphs I expound this key idea of aim-oriented rationality in such a way that it is abundantly clear it applies to 'complex' and not just 'simple' problems. The reader will find that the second paragraph of chapter 5 begins 'The basic idea of aim-oriented rationality is extremely simple. It can be put like this. Whatever we are doing, our aims are quite likely to be more or less problematic. Contrary to what we may suppose, aims we are striving to realize may not be realizable, or may not be desirable

(or may not be as realizable or desirable as somewhat modified aims we might pursue). Thus, whatever we are doing, in order to act rationally we must be able and ready, as the need arises, to improve our aims and methods as we act.'

I go on to formulate rules designed to help us improve our aims and methods as we live. I emphasize that 'above all we must do all that we can to ensure that we are not misrepresenting to ourselves what aims we are pursuing' – repression being a special case of misrepresentation. This is a basic theme of the chapter, and leads to a discussion and reinterpretation of Freud as a methodologist. And I go on to point out that 'Among other advantages, aim-oriented rationality is more helpful than 'problem-solving' rationality when it comes to resolving conflicts between people. The way we formulate our problem depends on what we take our aim to be. Thus two people, caught up in some common enterprise, but with conflicting aims, will formulate their common problems in different ways. As a result, each may regard the other as illogical, merely self-interested, engaging in trickery, bluff, propaganda. This does not help cooperative rationality to develop. By contrast, putting aim-oriented rationality into practice enables us to avoid such unnecessary, destructive misunderstandings, and helps us – if we so wish – to develop gradually more cooperative ways of resolving our conflicts.' The point is amplified subsequently in point 12 of chapter ten.

In these and other passages I make it clear that aim-oriented rationality is fruitfully applicable to all aspects of Kekes's 'complex' problems: all this Kekes ignores.

He also manages to ignore the sustained general discussion of the nature of our problems that is developed throughout the book. A central thesis, developed during the course of a number of chapters, is that personal and social problems of living, problems of action (requiring appropriate personal and social actions for their resolution) are intellectually, historically, and evaluatively prior to problems of knowledge, problems of science and technology. Problems of knowledge and understanding are interpreted as subordinate aspects of intellectually more fundamental problems of living – this being linked to the basic tenet of the philosophy of wisdom that 'problems of knowledge and understanding need to be tackled as rationally subordinate to intellectually more fundamental problems of living' (chapter 1). Human problems of living are interpreted as having evolved from animal problems of living; and this is linked to a discussion of animal and human evolution, the evolution of consciousness and culture, and to a discussion of how Darwinian theory needs to be reinterpreted to accommodate both biological and human evolution. The point is made that whereas animal

problems of living presuppose basic aims of survival and reproduction, human problems of living ought to be interpreted as presupposing basic aims of survival and reproduction of that which is of value. Our basic problem in life is to reinterpret and develop our aims and methods in life in such a way that mere survival and reproduction, inherited from our biological past, becomes survival and reproduction of that which is of value. Aim-oriented rationality is specifically designed to help us solve this problem of the progressive development of aims and methods in life. All this is ignored by Kekes.

He also ignores an extensive analysis of social and global problems (developed on pages 95-98 and 213-222) which interprets these problems as having arisen because of a breakdown in cooperation during the course of human history. When humanity lived in more or less isolated hunting and gathering tribes, tribal cooperation was relatively easy. In our vast, complex, diverse, interdependent modern world, the tribal cooperation of humanity on a global scale has become both essential and extraordinarily difficult. This analysis is important in that it supports the basic contention of the book, that the academic enterprise needs to be developed as an institutional substitute for a 'tribal discussion of humanity', a 'people's [global] civil service', having as a fundamental task to promote increasingly cooperative resolutions of global conflicts. These passages, ignored by Kekes, in themselves suffice to rebut his two criticisms.

As for the charge of 'scientific imperialism', Kekes ignores numerous passages in the book which make it clear that what I am advocating is, if anything, just the opposite. Far from arguing that thought guiding action should become more 'scientific' in character, what I actually argue is that scientific and academic thinking should, in important respects, become much more like the personal and social thinking we engage in as we live – an institutional amplification of such thinking, as it were. As I have so often stated in this book, the basic intellectual task of academic inquiry is to articulate our problems of living, propose possible human actions, and assess such possible actions critically from the standpoint of their capacity to resolve problems cooperatively and wisely if put into practice. This is the task of social inquiry – which is not social science, nor, fundamentally, even the (non-scientific) pursuit of knowledge. This drastically restricts the domain of the 'scientific'. I emphasize further that social inquiry, so conceived, needs to be construed as intellectually more fundamental than science – again quite the opposite of 'scientific imperialism'. I argue that rational inquiry, devoted to helping us realize what is of value, must express (and critically assess) human feelings and desires, human values and aspirations; and I argue that

the arts have a vital rational role to play within inquiry as 'revelations of value'. I argue for a non-authoritarian conception of reason, according to which the 'whole point of reason is to help us to act and decide as we really do want to act and decide: it is to enhance our own capacity to act and decide as we really want, not wrench the capacity from us or to reduce it to the one decision to obey henceforth the dictates of reason' (pp. 100-1). These and related points, which make nonsense of Kekes's criticism, are developed in chapters four, entitled 'The Philosophy of Wisdom' and devoted to expounding the central proposal of the book.

I also point out, in chapter five, that the philosophy of wisdom is a kind of synthesis of traditional Rationalism and Romanticism, something very different from Kekes's 'scientific imperialism'. And I argue for the fundamental importance of a non-scientific kind of person-to-person understanding, achieved through imaginative identification with the other person by means of the imaginative recreation of the other person's problems, feelings, desires, thoughts, context, beliefs, values. Whereas orthodox science and the philosophy of knowledge condemn such understanding as subjective, untestable, and unscientific, the philosophy of wisdom construes such understanding as being fundamental to our humanity, to our capacity to act and think rationally and cooperatively, and even to our capacity to do science. In sharp contrast to the philosophy of knowledge, the philosophy of wisdom places such understanding at the heart of rational inquiry, which is specifically designed to promote such vitally important understanding between people in the world. The passages which develop these points (on pages 72-5, 198-202, 206-213, 269-272, and 285-9) in themselves suffice to refute both of Kekes's criticisms: once again, these passages are ignored.

Kekes also ignores those passages which discuss possible and actual applications of the philosophy of wisdom (for example to problems of inner-city decay, and to the development of cooperatives in Mondragon in Spain) which also make a nonsense of his two criticisms.

All this is so extraordinary that it begs for an explanation. How could Kekes have got it so wrong? The answer lies in Kekes's basic misunderstanding of the message of the book, indicated above. Kekes chose to interpret the philosophy of wisdom as the thesis merely that natural and social science need to put aim-oriented empiricism into scientific practice. This would, as I have said, correct the first blunder inherited from the Enlightenment, but would leave the remaining two far more substantial blunders unaffected. Such a misinterpretation of the philosophy of wisdom leaves almost everything out (apart from the mere change in scientific

method). Given this interpretation, the philosophy of wisdom might, with some justice perhaps, be regarded as advocating a kind of 'scientific imperialism'. Kekes's criticisms at least make some sort of sense when directed against such a doctrine. But this doctrine is quite different from the one expounded and defended in this book.

Not only that. Incredible as it may seem, this version of the philosophy of wisdom – the Enlightenment programme – which Kekes attributed to me, I actually expounded, criticized, and *rejected* during the course of a discussion of Barbara Wootton's *Testament for Social Science* (see pages 164-5). (For a more detailed development of these points see Maxwell, 1986.)

Steven Yates, in an essay-review published in *Metaphilosophy* (Yates, 1989), also prefaced his attack with praise. He wrote 'Maxwell's is a very ambitious project. Were it entirely successful in its aims, it would be among the most important works in recent philosophical writing. I will argue that while it fails as a grand strategy, there is nevertheless much of value in this book which therefore merits more attention that it will probably receive'. Yates went on to give a good and detailed account of the argument.

Yates begins his criticism by pointing out that what is of value in life is highly diverse in character, it not being 'clear either that some can be reduced to others or that the same methods of achieving them will be successful'. I am not quite sure why Yates thinks this is an objection: I too emphasize that what is of value is, as I put it, 'inconceivably, unimaginably, richly diverse in character' (p. 269), although I also go on to say: 'The poles of value are life and love on the one hand, suffering and death on the other. The supreme good in existence is living life lovingly, actively loving that which is lovable in existence; and the supreme evils are suffering and death. Everything else of value in existence is organized around these two poles of good and evil' (p. 279). Perhaps what Yates has in mind is that if what is of value is highly diverse in character, then wisdom – the capacity to realize what is of value – must be highly diverse in character as well, and a kind of inquiry devoted to promoting wisdom would fragment into endlessly many disassociated sub-disciplines. But first, just this could, of course, be said with equal force of knowledge and the pursuit of knowledge. And second, this ignores what I say about rationality, the *methods* of the kind of inquiry I advocate. The rules of reason, designed to help us solve our problems, realize aims of value, are what may be called *meta*-rules: they 'presuppose that we can already successfully solve problems in the world; they are designed merely to help us marshal our already existing problem-solving power in order to solve new problems' (p. 83, note 6: see also pp. 80-87, 98-104, and chapter 5). Wisdom, likewise, can be regarded as a *meta*-capacity, one which enables us to marshal

our other more specific skills and capacities so that, in general, we can utilize them so as to realize what is of value, in diverse, specific contexts.

Yates objects that 'nowhere [do I] establish that 'values' have an objective existence in a sense in which they would hold in the same way for all people and hence could be called discoveries rather than inventions or creations'. This I hold to be impossible. What one can do, however, is formulate value-realism in such a way that it is immune to standard objections, and then demolish objections when directed against this view. This is what I set out to do in chapter 10. (The task is taken up in very much more detail in Maxwell, 2001.)

Yates's fundamental criticism, however, is that what I am proposing is neither 'realizable nor necessarily desirable'; it is Utopian and lacks 'a realistic strategy for putting [the] program into practice'. I have seven comments to make about this, which in sum constitute my rebuttal of Yates's objection.

1. In chapter three I show decisively that academic inquiry as it exists at present, dominated by 'the philosophy of knowledge', is very seriously irrational in a wholesale, structural way, granted that it has, as a basic humanitarian aim, to help promote human welfare by intellectual means. I demonstrate that academic inquiry violates three absolutely elementary, banal, uncontroversial rules of rational problem solving: (a) articulate, and seek to improve the articulation of, the basic problems we hope to solve; (b) propose and critically assess possible solutions; and (c) tackle subordinate problems in close association with our primary problems, so that subordinate and primary problems continue to be relevant to each other as we proceed (p. 60). Yates does not contest these claims; if anything, he endorses them. But if that is the case, I am at a loss to understand how Yates can oppose the program to put this structural irrationality to rights, and develop a more intellectually rigorous kind of inquiry, which is all that my 'very ambitious project' amounts to.

2. I go on, in chapter three, to point out that this wholesale, structural irrationality of academic inquiry is no mere formal matter; given the impact that scientific and technological research has on the social world, the irrationality of academia, being built into its institutional structure, has massive, long-term, damaging social repercussions (see pp. 62-77). It means that scientific knowledge and technological know-how are developed in a way which is dissociated from a more fundamental concern with how we can tackle our conflicts and problems of living in more just, humane, cooperatively rational ways (in violation of (a), (b) and (c)). As a result, the immense increase in our power to act that we have gained via modern science and technology is used, as often as not, to do harm, whether intended

or not, as it is to do good. 'In a world where there are immense injustices, persistent, violent conflicts between people, and where national and international politics are often conducted at the moral level of gang warfare, the products of scientific and technological progress, however nobly sought, and however potentially beneficial to humanity, will be used to imprison, enslave and kill' (p. 65). What is culpable, in other words, is not the pursuit of knowledge, but the damaging irrationality of the pursuit of knowledge *dissociated from a more fundamental active concern to help humanity learn how to resolve its problems gradually in more just, rationally cooperative ways*. Again, Yates does not contest this quite detailed thesis and argument of my book; if anything, he endorses it. But this, I would have thought, provides overwhelming grounds for trying to develop a more intellectually rigorous and humanly desirable kind of inquiry, less irrational and socially damaging, than what we have at present (which, again, is all my 'very ambitious project' amounts to). It is as if I have discovered some seriously false assumptions in current engineering, which are leading to the collapse of bridges, the crashing of aeroplanes, and the deaths of thousands of people, and a critic, agreeing with all this, nevertheless objects that it is Utopian to expect engineering to correct its mistakes, and I have failed to specify a strategy for persuading engineers to do this.

3. In arguing that my programme lacks a realistic strategy, and is Utopian, Yates conflates, or slides between, two quite different things: (i) the programme to change the overall aims and methods of academic inquiry (so that it puts the philosophy of wisdom into practice instead of the philosophy of knowledge); and (ii) the program of philosophy-of-wisdom academic inquiry itself to help humanity tackle its problems of living in more cooperatively rational ways than at present. For example, Yates writes at one point: 'What Maxwell nowhere gives us is a realistic strategy for putting his program into practice. Thus we are forced to conclude that Maxwell's call for intellectual revolution is the sort of proposal that looks good on paper but will not work in the world of flesh-and-blood human beings anymore than earlier proposals for Utopia from Plato down through Marx have worked.'

This criticism only looks plausible because it conflates, or slides between, the quite distinct programmes (i) and (ii). The reference to my 'call for intellectual revolution' sounds like (i) (to reform academic inquiry), but the comparison with Plato and Marx sounds like (ii) (to reform the social world). The crucial point, here, is simply this. As far as programme (i) is concerned, the only strategy available is the one that is everywhere apparent throughout my book: to spell out, in academic contexts, in books, papers, lectures, seminars and elsewhere, *what* needs to be changed, and *why* it needs to be

changed. Intellectual revolutions occur when enough scientists or academics become convinced that the proposed 'revolutionary' change is desirable and needed, the way forward, and proceed to teach, argue and do research in the new, revolutionary manner. Kuhn has familiarized us with the kind of battles, rearguard actions and circular disputes that take place when such revolutions occur, at least in science (Kuhn 1962). As far as programme (ii) is concerned – the programme of philosophy-of-wisdom academic inquiry of helping humanity resolve its conflicts and problems of living in increasingly cooperatively rational ways – it is a part of the task of the new kind of inquiry to work out how to do this. In general terms, what is required is quite clear: academia needs to engage in active public debate with the public, government, industry, and other influential institutions and aspects of social life. In addition, I have indicated the different approaches of the philosophies of knowledge and wisdom by contrasting the different ways a sociologist would tackle the problem of inner city dereliction (a problem much in the news when the first edition was published: see pp. 187-190). (More on this below, in point 6.)

4. Yates suggests at one point that the philosophy of wisdom is Utopian in the sense of Popper's 'Utopian social engineering' (Popper 1961, 67-93; Popper 1966, 157-68). But this is quite wrong (as even Yates himself goes on to acknowledge). Popper's 'Utopian social engineer' sets out to seize power and impose an overall plan, an ideology, on society by force. Philosophy of wisdom inquiry seeks to promote more cooperatively rational resolving of social conflicts and problems by means of imaginative and critical exploration of possibilities, debate, argument, learning from past successes and failures, the active engagement in and promotion of rationally cooperative discussion with the public and all those concerned. As I say at one point 'Granted that a choice must be made, then clearly cooperative, rational, social problem-solving, as characterized in this book, is very much more like [Popper's] piecemeal than Utopian social engineering' (p. 215). But I then go on to point out that Popper's distinction between the two is much too crude, and misses out a third strategy, namely the one argued for in this book of cooperatively rational problem-solving, employing aim-oriented rationality (see pp. 213-21). Yates's suggestion that what I am arguing for amounts to 'Utopian social engineering' in Popper's sense could not be more mistaken.

5. Yates creates the impression that the task for philosophy-of-wisdom inquiry of promoting wisdom and cooperative rationality in the social world is hopelessly unrealistic and unrealizable (and 'Utopian' in that sense) by making it an all-or-nothing affair. It is as if Yates imagines that I am arguing

that a transformed academic inquiry could create a wholly cooperative world overnight. But I make it dazzlingly clear in my book that what I am arguing for is that academic inquiry should seek to help humanity resolve its conflicts and problems of living gradually, step by step, in a somewhat more cooperatively rational way than at present, insofar as this is desirable and achievable. Thus, at one point, I write: 'putting aim-oriented rationality into practice enables us ... to develop gradually more cooperative ways of resolving our conflicts. In roughly increasing levels of desirability, conflicts between people are settled by: force, threat, manipulation, some more or less arbitrary procedure (such as tossing a coin or voting), bargaining, the cooperative discovery of the most desirable, just resolution. The general adoption of the aim-oriented conception of reason is in all our long-term interests in that it offers us the best hope of increasing our capacity to resolve our conflicts in rather more desirable ways – even though, of course, it provides no magic procedure for resolving conflicts' (pp. 121-2.)

Increasing our capacity, gradually, to resolve conflicts in more cooperatively rational, just ways, insofar as this is desirable and realizable (which is what I am advocating), does not seem to me to be an unrealizable, Utopian aim at all. By distorting what it is that I am advocating, Yates creates the (false) impression that what I am arguing for is unrealistic and unattainable.

Yates points out that I hold that cooperative rationality requires that people acquire empathic, or what I call 'person-to-person' understanding of each other. He then goes on to argue that it is not clear that person-to-person understanding 'will cross either personal or cultural divides', and that it is not clear that this latter divide '*should* be crossed: doing so might inevitably involve the imposition of one set of 'values' lifted from Western science on a culture which wishes no such imposition'. Here, again, Yates argues as if what is at issue is an all or nothing affair, whereas what I am arguing for is that our institutions of learning should be designed to help us progressively *increase* our capacity to understand each other in a person-to-person way. I find the suggestion that this would involve the imposition of values lifted from Western science extraordinary. (This is Kekes's accusation too, of course.) 'Western scientific culture' is surely not alone in valuing mutual understanding, cooperation, and justice. If aim-oriented rationality, arrived at by generalizing the progress-achieving methods of science, helps mutual understanding, cooperation and justice to flourish, surely the fact that modern science was first developed in Europe or 'the West' should not mean that other cultures are to be deprived of benefiting from employing aim-oriented rationality. The only way we can put a stop to Western culture

dominating and crushing out of existence all others, on our crowded planet, is to develop more cooperative modes of interaction, based on the development of mutual understanding and justice.

6. Yates complains that I fail to say how my "'third method for solving social problems" can acquire force, how the desires of the wealthy and powerful to preserve the status quo can be overridden.' I find this complaint extraordinary. If the few *philosophes* of the Enlightenment in the 18th century, without support from democracy, opposed by a powerful and largely hostile combination of Church and State, could have the immense impact they did have, so that our modern world of liberal, tolerant democracies is scarcely conceivable without their efforts, all this being achieved with nothing more powerful than argument and wit, then surely it cannot be beyond the powers of the mighty world of academia today to achieve as much employing similar means. The strategy I recommend to my fellow academics is clear: become an Enlightenment *philosophe* in the 21st century, but make sure you free yourself of the paralysing blunders we have inherited from the 18th century *philosophes*! Just this is what I said in the first edition. Almost at its conclusion I declared 'Academics today might well regard the life-work of 'philosophes' like Voltaire and Diderot as paradigmatic of what academic work ought to be' (see p. 303).

7. Yates asks rhetorically: 'Are the problems of third world nations, for example, *intellectual* problems? On the face of it, it would seem not. The situation in Ethiopia, to name just one obvious example, is not due to problems about the aims of inquiry but to a corrupt and incompetent government; if that government could be put out of its misery, the starvation there could be stopped within months or even weeks. This suggests that political action, and not intellectual programs attempting to redirect the aims of academic inquiry, is what is needed to redress the problems of third world nations.' *Of course* political action is needed to redress the problems of the third world. Yates writes here as if he thinks I am arguing that academic thought in itself can somehow solve problems of living. Nowhere do I suggest anything so absurd. On the other hand, if political action is to redress the problems of the third world, it needs to be appropriate political action, not just any old action, and it is here that appropriate academic inquiry may be relevant. The Ethiopian government to which Yates refers professed to be, I believe, a Marxist government; ideas, here (of the wrong kind) are clearly highly relevant to the suffering of the Ethiopian people. Yates's choice of Ethiopia is in fact extremely pertinent. The Marxist government to which Yates refers was defeated in 1991 after a 17 year long guerrilla war. Joseph Stiglitz, in his recent book (Stiglitz 2002, pp. 25-36), gives an account of how

he, as newly appointed senior vice president of the World Bank, visited Ethiopia in 1997, and met Prime Minister Meles Zenawi, generally regarded as intelligent, knowledgeable, honest and capable. Stiglitz describes excellent policies initiated by the Ethiopian government designed to relieve poverty and hunger, and promote agriculture (although, unfortunately, hunger persists in Ethiopia, and there have been serious civil rights violations in that country since 1997). But the IMF, because of its commitment to right wing, free market dogma, wanted to impose wholly inappropriate policies on the Ethiopian government, and sought to penalize the government for pursuing its sensible policies. And quite generally, in the IMF: 'Decisions were made on the basis of what seemed a curious blend of ideology and bad economics, dogma that sometimes seemed to be thinly veiling special interests. When crises hit, the IMF prescribed outmoded, inappropriate, if 'standard' solutions, without considering the effects they would have on the people in the countries told to follow these policies. Rarely did I see forecasts about what the policies would do to poverty. Rarely did I see thoughtful discussions and analyses of the consequences of alternative policies. There was a single prescription. Alternative opinions were not sought. Open, frank discussion was discouraged – there was no room for it. Ideology guided policy prescription and countries were expected to follow IMF guidelines without debate' (Stiglitz 2002 pp. xiii-xiv). And the consequences have been dire, often for the lives of the poorest people on earth, as Stiglitz makes clear. There could scarcely be a more lucid illustration of the way bad ideas, and irrational, dogmatic modes of thought, once they get a grip on powerful governments, institutions and bureaucrats, can have devastating human consequences. What Stiglitz says also makes clear just how profoundly important it is that those people and institutions who exercise such power in the modern world should put into practice the elementary principles of rationality of philosophy of wisdom inquiry.

In order to create a better world with as little human pain as possible, it is essential that bodies such as the IMF, World Bank and World Trade Organization, together with governments – especially governments of the powerful, technologically advanced first world – implement policies intelligently designed to move us towards such an end, policies openly discussed and critically assessed, especially against experience, in a way that is so deplorably absent in the IMF as depicted by Stiglitz. But this is unlikely to come about without appropriate education giving priority to the tackling of problems of living rationally – philosophy of wisdom education, in other words, rather than philosophy of knowledge education, which is what we largely have at present. And if all this is to be achieved democratically, with

the endorsement of the electorate, then it is not just the politicians, the business men, the civil servants and bureaucrats who need philosophy of wisdom education, but all of us, so that we may all come to have a better understanding of what our problems are, and what needs to be done to help resolve them.

We need an academic enterprise prepared to take up the educational-political task of actively campaigning for a more rational and just, a freer, a more democratic, a wiser world. This in turn requires that we have in existence the kind of academic enterprise that I am arguing for, devoted to the active promotion of global wisdom. What we have at present, a kind of academic inquiry devoted to the pursuit of knowledge, cannot engage in the kind of debate that is required, with government, public opinion, big business, the military, the media: to do so would violate the edicts of the philosophy of knowledge. As I have said, the intellectual revolution that I am arguing for is certainly not *sufficient* to bring about a better world, but it may be *necessary*. In order to create a better world humanity needs to learn how to do it, and for that humanity needs traditions and institutions of learning rationally designed and devoted to that end. This is what we do not have at present.

In conclusion, then, Yates's criticisms seriously misrepresent what I actually argue for in my book. Nothing that he says in any way undermines my claim that the revolution I argue for is urgently needed, wholly desirable, and entirely realizable and practical. So far, it is not the powerful and wealthy who have ensured that what I am arguing for has not got a hearing, but reviewers like Yates, who have so seriously misinterpreted my thesis and argument that it comes over as nonsense, or as hopelessly Utopian.

Another example of this is to be found in Anthony O'Hear's *An Introduction to the Philosophy of Science* (O'Hear, 1989, pp. 224-32). O'Hear begins his critique with the remark: 'The claim is that there is something inherent in the methods of science which make its practitioners peculiarly prone to go in for, say weapons research, as opposed to solving the world food problem and devoting themselves to working out ways in which we can all collectively decide on a better, wiser way of life for everyone on the planet. This, at any rate, is the claim of Nicholas Maxwell, in his influential book *From Knowledge to Wisdom*' (O'Hear, p. 224).

This is, at best, a grotesque distortion of what I do say in this book. Its basic thesis is about academic inquiry *as a whole*, and not just about *science*. Furthermore, the claim is that academic inquiry pursued in accordance with the philosophy of wisdom would better help humanity learn how to solve its problems of living than inquiry pursued in accordance with the philosophy of knowledge. This is very different from the claim attributed to me by O'Hear.

Nowhere in the book do I say anything remotely like 'there is something inherent in the methods of science which make its practitioners peculiarly prone to go in for, say, weapons research, as opposed to solving the world food problem'. I do argue that current conceptions of science, and of academic inquiry – standard empiricism and the philosophy of knowledge – which exclude discussion of aims and values from the intellectual domain of science, make it difficult for scientists to explore, within the context of science itself, questions about the aims and priorities of research, and the values that ought to be implicit in these aims and priorities. But this, again, is a claim very different from the one that O'Hear attributes to me.

O'Hear goes on: '*science* has (at some cost to landscape) provided us with the means to feed the total population of the world several times over. So it is a bit unfair to blame science for our failure to eradicate starvation' (O'Hear, p. 235). But nowhere in my book do I 'blame science' in this way. I simply argue that in order to learn how to solve our immense global problems we need a kind of academic inquiry rationally designed to help us learn how to do this – a kind of inquiry which we do not at present have. My target is not *science*, but the accepted *philosophy of science* and, much more importantly, the accepted *philosophy of academic inquiry*, built into the institutional and intellectual structure of academic inquiry as it exists in universities all over the world. Again, what O'Hear criticizes is quite different from anything that I actually say.

O'Hear goes on: 'Maxwell's criticisms of science ... derive from the very nature of science as an enquiry which prescinds from questions of value' (p. 225). Once again, this misses the point. I argue at length in my book, in my view decisively, that values, of one kind or another, are inevitably, entirely properly and desirably, built into the scientific enterprise in influencing choice of research aims, in influencing what scientists seek to develop knowledge about. Values ought not, of course, to influence what scientists decide is true or false, to be accepted or rejected: but values do, inevitably and quite properly, influence scientists in deciding to study these objects, phenomena or problems, rather than some other set of objects, phenomena or problems. (O'Hear completely ignores all this.) I do not, then, argue against the value neutrality of science, for there is no such thing; what I do argue against, again, is the official *philosophy of science* of standard empiricism which, falsely and damagingly, denies that values do play any legitimate role within science. Once again, O'Hear misses the point.

O'Hear goes on: 'Maxwell urges that knowledge is valuable only to the extent that it is knowledge of valuable truth, that is truth which helps us collectively to 'live life lovingly'' (p. 225). This is acceptable as a

characterization of what I do say as long as it is appreciated that 'living life lovingly' includes 'seeing, exploring, seeking knowledge and understanding of the world around us', knowledge being of value if it contributes to this. In my book I place great emphasis on what might be called the 'cultural', 'intellectual' or 'intrinsic' value of knowledge, something that is not at all clear from O'Hear's caricature of my position. But I do definitely argue that science does not, and ought not to, just amass knowledge of facts, however trivial and useless these facts are. O'Hear does not seem to be aware of these arguments. He does not, in any case, acknowledge them, criticize them, or provide any reasons whatsoever for supposing that what I argue for is wrong.

O'Hear goes on: 'Against Dr. Bronowski's position ... that we cannot blame science for what happened at Hiroshima and Nagasaki, Maxwell says that "our task, in engaging in rational inquiry is to see, participate in, and help to grow what is significant and of value in existence in the cosmos"' (pp. 225-6). As it happens, the quotation that O'Hear gives, here, from the first edition of this book, comes from page 19; my brief discussion of Bronowski is on page 147! To suggest that on page 19 I am arguing 'against Dr. Bronowski's position' is at best misleading. When I do discuss Bronowski, I make it clear that what I am against is Bronowski's espousal of a *philosophy of science* which denies that science has anything to do with what is, or is not, of human value. Suppose Heisenberg and his fellow physicists had energetically pursued research on the atom bomb in Hitler's Germany during the war, 'value-neutral' facts about how to construct such a bomb being made available to Hitler's staff. Could such scientific activity, pursued in such circumstances, be regarded as free from all blame? The 'physical facts' may be value-neutral; the *human actions* of doing the research and communicating the results are not. (But, in my view, it was thoroughly understandable that physicists worked on the bomb in the USA, given the belief that Heisenberg and co. had already been working for some time on the bomb in Germany. The decision to drop the bomb on Japan is another matter, especially when one takes into account that it was done in part, it seems, to impress Stalin. But in any case the decision to drop the bomb was not made by the scientists who developed the bomb.)

O'Hear goes on: 'Maxwell ... would make the assessment of the aims of scientific research internal to the scientific activity itself, and not something external to and separable from what is regarded as the purely scientific stratum of a scientist's work' (p. 226). I do indeed hold this; and it is in my view, entirely correct. Of course assessment of aims for research is, at least in part, internal to science itself. Insofar as aim-oriented empiricism emphasizes

this, and standard empiricism denies it, so much the better for aim-oriented empiricism, so much the worse for standard empiricism.

O'Hear then writes: '[Maxwell], like many other critics of science, insists that the implicit separation of the internal knowledge-producing aspects of science from enquiries into the use and value of the knowledge thus acquired provide all too easy an alibi for scientists to engage on harmful and destructive projects, and may indeed actually encourage the growth of such projects, in so far as those who work on them are ... trained to look at them in isolation from their wider social and moral effects' (p.226). Fair enough. But one must appreciate that values, of one kind or another, are in any case implicit in the knowledge-producing aspects of science, in connection with choice of subject-matter or research aims; the only question is whether these influential implicit values are made explicit and critically assessed, in the interests of rationality and objectivity, or denied and left implicit. Second, in my book I am not really concerned to criticize *science*; my concern, rather, is to argue for a new kind of *academic inquiry* rationally devoted to promoting wisdom. The idea for this new kind of inquiry – the philosophy of wisdom – arises in part from generalizing a new *philosophy of science*, namely aim-oriented empiricism, which stands in contrast to the orthodox philosophy of science of standard empiricism. It is fundamental to my argument to criticize the *philosophy of science* of standard empiricism; this is very different from criticizing *science itself*. Nowhere does O'Hear allude to these crucial points. Throughout he interprets my criticism of the philosophy of science of standard empiricism as a criticism of science itself. Perhaps O'Hear is so used to understanding science in terms of standard empiricism that he is unable – or was unable when he wrote the book – to see that there might be a distinction between the two.

One crucial point that O'Hear completely overlooks is that the entire argument of my book is based on the idea that we can learn something immensely important from science: we can learn from scientific progress how to achieve social progress towards a better world. In order to do this we need first to get clear about the nature of the progress-achieving methods of science; we need then to generalize these methods and apply them to *social life*. As I have already repeatedly stated, this involves rejecting standard empiricism and accepting aim-oriented empiricism in its stead; it then involves generalizing aim-oriented empiricism to form aim-oriented rationalism, which then needs to be applied to diverse institutions and other aspects of human life. Social inquiry emerges as social *methodology* rather than social *science*. The outcome is philosophy-of-wisdom inquiry. There is not a hint of any of this, fundamental to the argument of my book, in O'Hear's

critique. This basic argument of my book involves *learning from science*, not *criticizing* science. What is criticized is not *science*, but a defective and damaging *philosophy of science*!

O'Hear then goes on to link me with feminist criticisms of science and, in particular, the work of Sandra Harding. At this point, O'Hear's misunderstandings of my book intensify considerably.

O'Hear writes: 'Maxwell argues in his book that empiricist methodologies make it impossible to understand the progress of science and, by implication, that such methodologies will hold up scientific progress. This is because empiricism refuses to recognize that ultimately the universe is comprehensible in ways which will be revealed as we go along, and as we recognize ourselves as part of the evolution of nature towards states of greater value and greater love' (p. 227).

What I argue is that *standard empiricism* makes it impossible to understand scientific progress and would bring progress to an end if honestly put into scientific practice. But I do not argue that *empiricism* as such does this, because what I defend is a view that I call *aim-oriented empiricism*, very definitely a version of empiricism. Again, what I actually argue is that standard empiricism cannot acknowledge that the thesis that the universe is physically comprehensible *is a part of current scientific knowledge*, and this is the basic defect of the view (an argument spelled out at greater length in Maxwell, 1998 and 2004b). Not only does O'Hear fail to get what I do say properly into focus; he also fails to offer even a whisper of a criticism: he seems to think that it suffices to mis-state what I do argue for.

O'Hear goes on: 'it must be perfectly obvious, despite Harding and Maxwell, that the backing of a given set of political or metaphysical values is neither necessary nor sufficient for scientific truth' (p. 228). This leaves me completely baffled. Nowhere in my book do I say anything remotely like 'the backing' of any kind of 'value', political or otherwise, is necessary or sufficient for scientific truth. O'Hear goes on: 'The anti-Darwinists in the Soviet Union in the 1940's did not achieve scientific truth, despite being motivated in their theories by correct 'progressive' values. Nor is it clear that the realist Einstein, lauded as such by Maxwell, was correct in his opposition to the largely instrumentalist and non-realist proponents of the Copenhagen interpretation of quantum mechanics' (p. 228). I hope that " 'progressive' values" in this quotation is meant ironically: otherwise I suggest O'Hear reads Medvedev's account of the horrible things that went on during the disastrous imposition of Lysenko's ideas on biology and agriculture in the Soviet Union (Medvedev, 1969). The idea that anything said in the first edition of this book provides anything remotely like a justification for the view that facts ought to comply

with wishes, desires or values, 'progressive' or otherwise, is simply grotesque. As I have already stressed, and as I stressed in the first edition of this book, values inevitably, desirably and quite properly influence choice of subject matter or problems, but ought never to influence decision about what is true and false (unless negatively, in that we should be all the more critical of something we passionately want or need to be true). I find it rather astonishing, furthermore, that O'Hear should link the Lysenko case with Einstein's opposition to quantum mechanics. Morally the two cases are utterly different. But they are also entirely different intellectually. Einstein opposed the Copenhagen interpretation of quantum mechanics not, primarily on grounds of desires or values, but on grounds of *physics*, of *science*. And furthermore, Einstein was correct to do so! Precisely because of its lack of realism, orthodox quantum theory (OQT) is a very seriously *ad hoc* theory, in that it consists of two inconsistent parts, a quantum mechanical part (for a treatment of the quantum system) and a classical part (for a treatment of the measurement process): for a detailed exposition of this argument and further references see Maxwell (1998, chapter 7). No physical theory, as grossly *ad hoc* as this, is acceptable in physics, whatever its empirical success may be. This point goes to the heart of the argument of this book. If standard empiricism is accepted, it seems unacceptable to reject the immensely empirically successful OQT on the grounds just indicated. But if aim-oriented empiricism is accepted, it is entirely legitimate. My argument for aim-oriented empiricism and against standard empiricism is indeed that the latter view cannot begin to do justice to what actually goes on in science, in that endlessly many empirically successful, but grossly *ad hoc* rivals to accepted theories can always easily be concocted. Once again, O'Hear reveals that he has simply failed to read my book properly. As a coda to all this, I might add that many physicists today would agree with me that Einstein was right, and there is something seriously defective about OQT.

O'Hear goes on: 'one can say quite generally that such values as political emancipation (however conceived) and participatory realism are not achieved in advance of knowledge of the relevant aspects of nature' (p. 228). This again links together two issues that are entirely distinct. Of course we can't expect nature to comply with our wishes or values in the absence of knowledge. But this is quite different from maintaining that the scientific enterprise must make a hierarchy of assumptions concerning the comprehensibility and knowability of the universe, theories which clash with these assumptions being rejected whatever their empirical success might be. This has nothing to do with 'the value ... of participatory realism'; it has to do with the rationality of science, with science being possible at all. In running

these two quite distinct issues together (values and metaphysics), in this confused way, O'Hear in effect commits a version of the blunder of which he (falsely) accuses me – namely blurring the distinction between value and scientific fact.

O'Hear goes on: 'We cannot, as Harding and Maxwell appear to want us to do, assume in our scientific work one version of a specific value and then expect that nature is obligingly going to fit in' (p. 228). Nothing that I said in the first edition provides a basis for attributing to me the view that O'Hear here attributes to me; and much that I do say goes right against this attribution. First, to repeat the point already made, I argue only that values influence choice of subject matter or problem; nowhere do I argue that values ought to influence what we decide is true or false, acceptable or unacceptable in science. Second, I stress at length that 'the aim of improving knowledge of *humanly valuable truth* is, if anything, even more profoundly problematic than the aim of improving knowledge of explanatory truth' (see p. 113). The fundamental idea of the conception of science that I advance, aim-oriented empiricism, is that the *aim* of science is *fundamentally problematic*, and hence needs to be subjected to constant critical scrutiny as science proceeds, in an attempt to improve it. This is fundamental, too, to the notion of rationality that I arrive at by generalizing aim-oriented empiricism, namely aim-oriented rationality. Just as aim-oriented empiricism forms the methodological framework for science, aim-oriented rationality forms the methodological framework for philosophy-of-wisdom inquiry. All this is central to the book, and yet entirely overlooked by O'Hear. The point, in the present context, is that, as I made clear in the first edition, to say that the aim of acquiring knowledge of valuable truth is problematic is to say that we *cannot at all* expect facts to comply with values. Values legitimately influence what we look for, what we might hope to discover: but precisely because values are one thing, and facts another, we must also give sustained attention to trying to find out what is potentially scientifically discoverable that it would be of value to discover. This is argued for at length in the first edition: see for example, pages 113-7, and figure 5c on page 108.

One final comment. O'Hear remarks 'one has to be suspicious of Maxwell's aim of conflating the descriptive and explanatory role of science with questions of value, including those relating to the value of particular types of scientific inquiry' (p. 229). It is not I who conflates these things; if anything, as I have indicated above, it is O'Hear.

Rom Harré, in his book *Varieties of Realism*, devotes a section to a discussion of this book (Harré, 1986, pp. 26-32). Unfortunately, the ideas that Harré discusses I hardly recognize as having anything to do with the

book. Thus Harré declares 'Maxwell tries to subsume under the one 'philosophy of wisdom' both the morality of a science directed to the resolution of human problems, and the attitude to science as a road to a deep understanding of the world, displayed by such philosopher-scientists as Faraday and Einstein. . . His argument depends on a generalization of the concept of 'love', from a supervening moral attitude to interpersonal relations, to a general attitude of reverence for the physical universe as such. This fundamental premise of Maxwell's reasoning seems to me not well established.' But, once again, the philosophy of wisdom is a conception of *academic* inquiry, not just *scientific* inquiry. And I simply do not recognize the premise that is 'not well established', not as a 'fundamental premise' of my 'reasoning' at any rate. The components of my basic argument are a conception of rational problem solving (or aim pursuing), two rival views about what the aims and methods of academic inquiry ought to be, and the argument that one violates elementary rules of reason whereas the other does not. I do also go on to argue, of course, that the intellectual defects of the philosophy of knowledge have damaging consequences, both for the cultural and the practical aspects of inquiry. But it is quite wrong to say that my 'argument depends on a generalization of the concept of 'love", a 'fundamental premise' which turns out to be 'not well established'.

Harré goes on 'Apropos the philosophy of science Maxwell . . . makes the point that the standard 'problems' in the philosophy of science, such as the problem of induction, are consequences of adopting standard empiricism, not inevitable paradoxes of the search for a knowledge of nature. . . The failure of standard empiricism does not show that a better account of the knowledge-garnering ways of science cannot be formulated within the *moral* position Maxwell calls the 'philosophy of knowledge'. Maxwell seems to me to slip into the same error of reasoning that . . . undermines Feyerabend's position. Because some particular philosophy of science is objectionable – say, 'standard empiricism' – no other can be found which could guide scientists towards their moral ambition of obtaining trustworthy knowledge of the natural world. . . . [This error] leads Maxwell to suggest, despite his occasional comments to the contrary, that only by adopting a new moral order, the philosophy of wisdom, will the creative aspect of scientific thought be freed from the shackles of crude empiricism.' There are several oddities about this. First, I would hardly call the philosophies of knowledge and wisdom rival 'moral orders': they are rival views about what the intellectual aims and methods of academic inquiry ought to be. Second, in order to free 'the creative aspect of scientific thought . . . from the shackles of crude empiricism', as Harré puts it, what is needed, in my view, as I made

abundantly clear, is the philosophy of science of *aim-oriented empiricism* rather than the whole of the philosophy of wisdom. Third, far from making the error, which Harré attributes to me, of assuming that the downfall of standard empiricism means that 'no other can be found which could guide scientists towards' acquiring knowledge of nature, I put forward just such a philosophy of science which I claim can do just that: aim-oriented empiricism. And finally, this philosophy of science of aim-oriented empiricism, which Harré seems to overlook entirely, is able, I claim, to resolve the problem of induction, just that which standard empiricism cannot do.

Harré continues: 'Maxwell's criticism of the hegemonic position of the philosophy of knowledge (chapter 3) depends on a fundamental premise, that 'intellectual priority [should be] given to the task of articulating problems of living, proposing and criticizing possible solutions – problems of knowledge and technology being tackled as rationally related, subordinate, secondary problems.' Judged with respect to this principle science departments of schools and universities are defective according to Maxwell, because they pursue the subordinate rather than the superordinate end. But Maxwell nowhere shows that the Edwardian counsel of perfection for politicians could not be referred to the scientific elite, who, were they to adopt the traditional moral order of their own community, *impartial, co-operative problem-solving* would begin to move towards ways of solving the problems of living. Maxwell has done nothing to establish that a *new* philosophy of wisdom is called for.' First, a relatively minor point. My argument is not that science is defective because it pursues 'the subordinate rather than the superordinate end'. What is defective, rather, is that the scientific tackling of (subordinate) problems of knowledge and technology, the proper business of science, should be dissociated intellectually from the more fundamental *academic* tackling of problems of living. As for the main point that Harré makes here, I am baffled. I am not sure I understand it. Is he arguing that a new philosophy of wisdom is not needed for scientists – or for academics – to take up the task, at an intellectually fundamental level, of proposing and critically assessing possible solutions to problems of living in a cooperatively rational way? Whether new or old, just doing this amounts to putting the philosophy of wisdom (as characterized in chapter 4) into practice, and certainly conflicts with the philosophy of knowledge. Or is Harré arguing that all that is required is for *politicians* to engage in cooperatively rational problem solving, and for that a new philosophy of wisdom is not required? But my argument against that is, first, it is not enough for *politicians* merely, to do this, and second, it is unlikely that politicians and others will do it in the

absence of an academic enterprise that puts the philosophy of wisdom into practice.

A theme runs through the above criticisms. They all, in different ways, misrepresent the argument of this book, often in quite bizarre and extreme ways. One might think that this must be my fault; I have failed to make myself clear. But others, mentioned at the beginning of this chapter, understood very well what the book is about. Why, then, this catalogue of incomprehension? It could be that it is one aspect of the resistance, not necessarily conscious, that academia puts up when asked to consider new ideas. It may be that the training that academic philosophers receive equips them to engage in philosophical analysis, but not to understand new ideas. But perhaps the explanation for this cascade of incomprehension is simpler. Academics are so busy these days that they simply do not have the time to read books. All a critic can be expected to do is glance at a chapter or two, make a guess at the rest, and expound and criticize that guess. The prospects for launching new ideas in philosophy, in the humanities, do not look good.

So much for criticisms, not of this book, but of various serious misrepresentations and distortions of the book. I turn, now, to a consideration of something more worthwhile: criticisms of what is actually in the book.

David Collingridge, in an essay review published in *Social Studies of Science* (Collingridge, 1985), criticizes the proposal to bring about radical social change employing aim-oriented rationality for failing to be an acceptable 'mid-course between Utopian social change and the incremental changes which are open to Popper's piecemeal social engineer' (p. 767). He has four criticisms. (a) We are always ignorant of relevant information, and are thus all too likely, in seeking to bring about social change, to make mistakes. Attempts to bring about radical change are likely to involve big, costly mistakes. The attempt to anticipate such mistakes by the use of imagination is not likely to be successful as imagination is all too likely to fail when radical change is involved. (b) Once one accepts that aims are corrigible and all too likely to need revision, radical social change in pursuit of such aims becomes all the more likely to be mistaken. 'The kind of major social changes argued [for] by Maxwell are too risky' (p. 767). (c) Radical social change pursued in an aim-oriented rationalistic manner requires that people achieve person-to-person understanding of each other. But this involves a direct cost, since people 'could have been doing something else with their time' (p. 768); and it involves an indirect cost too, because such understanding takes time to achieve, which means there will be delays in reaching decisions which 'will prove very expensive' (p. 768). Collingridge continues 'The problems

identified by Maxwell are indeed urgent, but then that would seem to imply that they are unsuitable for his procedure of rational cooperative decision-making. One way in which our present institutions and customs have got around this problem is the device of compromise, where no party even attempts to see the other's point of view. . . This may be rough and ready but it has the advantages of being quick and inexpensive. . . compromise really only works for piecemeal changes; revolutionaries, by definition, hold it in contempt' (p. 768). (d) 'If there is a flourishing tradition of intellectual inquiry pursued according to the lights of the philosophy of wisdom, then people are able to make choices in a cooperatively rational way. But there is no such tradition: the field is held entirely by the philosophy of knowledge. In these circumstances, how are decisions to be made?' (p. 768). How can radical aim-oriented rational social action be possible?

To take the last point first, this book argues for the urgent need to bring about the revolution in academic inquiry, from knowledge to wisdom, in the main because this would make possible aim-oriented rational social action to an extent that is not possible in the absence of the revolution. It might almost be said that the whole point of the philosophy of wisdom is to promote aim-oriented rationalistic social action and ways of living. In recognizing that 'people are able to make choices in a cooperatively rational way' if 'there is a flourishing tradition of intellectual inquiry pursued according to the lights of the philosophy wisdom', Collingridge emphatically endorses the main message of this book.

As for Collingridge's other criticisms, I have two points to make. First, Collingridge recognizes that we face vast, complex, urgent, global problems engendered by population growth, modern industry and agriculture, modern war and technology of war, problems such as third-world poverty and global warming. To tackle such big, complex, urgent problems by means of nothing more radical than Popper's piecemeal social engineering aimed at removing specific, small-scale evils seems hopelessly inadequate; it is bound to lead to much future human suffering. Second, piecemeal social engineering may be interpreted in two very different ways: as a doctrine about *aims*, and as a doctrine about *methods*. Interpreted in the first way, the doctrine holds that policies for social change should have, as their aim, to remove quite specific, small-scale evils, in particular the evil of avoidable human suffering. (I ignore the morally neutral characterization of piecemeal social engineering of *The Poverty of Historicism*: see chapter 8 of the present book, reply to objection 8.) Interpreted in the second way, the doctrine holds that, in pursuit of aims, whether large-scale or small-scale, we should adopt the method of implementing social change bit by bit, in a piecemeal way, at least initially, so

that we can learn from mistakes as we proceed. Popper does not draw this distinction, although some aims he holds to be non-Utopian and achievable, such as creating international institutions to prevent war, may strike many as large-scale and highly non-piecemeal-like in character (see Popper, 1969, vol. 1, ch. 9, note 7, pp. 288-91). Aim-oriented rationalistic social action might almost be characterized as piecemeal social engineering given this second interpretation as a doctrine about *methods* and not *aims*. Interpreted in this way, piecemeal social engineering may have vast, complex, long-term aims, but adopts piecemeal methods. Aim-oriented rationality adds wisdom-inquiry and cooperative rationality to this, and adds also the vital requirement that such social action should include some imaginative and critical scrutiny of big, long-term aims and methods associated with piecemeal social action, so that problematic long-term aims may be held under review, and so that what is actually being done may be assessed from the standpoint of its success and failure in helping to bring about long-term aims. It would seem essential to adopt such an approach if we are to tackle successfully such vast, complex, long-term global problems as world hunger and global warming. The task of implementing social change in this way, adopting and improving on the *methods* of Popper's piecemeal social engineering, but having big, complex, long-term, global *aims*, is free of the defects Collingridge depicts in his first two criticisms, (a) and (b). Because social change is enacted bit by bit, at least initially, learning from mistakes becomes entirely possible. Collingridge's criticisms in effect assume that aim-oriented rationalistic action can only be assessed in terms of its results when its overall aim has been achieved, and monumental and very damaging mistakes might well have been made. But aim-oriented rationalistic social action is not like this at all, as I have made clear. In fact the situation is the reverse of what Collingridge depicts. Popperian piecemeal social engineering which is not improved by the addition of sustained critical scrutiny of long-term aims actually *suppresses* criticism, and thus goes against the spirit of Popper's critical philosophy.

As for Collingridge's stress on the importance of achieving compromise rather than person-to-person understanding, I would say this. Resolving conflicts in a cooperatively (aim-oriented) rational way by means of person-to-person understanding is in many cases likely to arrive at some kind of compromise: only rarely will both parties discover a resolution which gives everyone more than (or as much as) they hoped for. How good a compromise is achieved may well depend on how good a level of person-to-person understanding the parties have achieved of each other. Person-to-person understanding is never perfect; it is always a matter of degree. It may well, however, be held to be something good in its own right, as well as being

good as a means to the discovery of good resolution of problems and conflicts, resolutions which are at least good compromises.

Noretta Koertge, in a review published in *Isis* (Koertge, 1989), says 'I found Maxwell's exposition and critique of the current state of establishment science to be clear and convincing. I was less persuaded by his program for change. The idea of unifying epistemology and moral theory is an attractive one, but Maxwell may underestimate the difficulties and dangers of carrying it through. For me, the weakest aspect of his philosophy of wisdom was its tacit political theory. To base science policy on empathy and consensus, no matter how rationally arrived at, might indeed make science the servant of the people, but perhaps at the cost of taming its radical potential' (p. 146).

I have four things to say in response. First, in arguing for more cooperative rationality in social and political life, this book does not claim that all social and political decision-making can be done in a cooperatively rational way. Ultimately, political power, however democratic, will need to be delegated, and maintained by force. Otherwise there is no defence against those who seek despotic power by ruthless means. And cooperative rationality has its limits, in part because of its inefficiency and cumbersomeness in various contexts, in part because it may not permit individuals with exceptional talents, skills, knowledge and experience to make their full contributions, in the arts and sciences for example, from which we all benefit. The all-important point, however, is that much more of social and political life could be conducted in more cooperatively rational ways with great benefit to all, before we encounter the negative, the undesirable, aspects of cooperation. Second, and in line with the point just made, fund-giving committees will no doubt continue to make decisions about what scientific research projects are to receive financial support. The really important point is that such decision-making should be made within a context of sustained open debate about what can be discovered and developed, and what it is *desirable* to discover and develop, this debate being undertaken by both scientists and non-scientists. Furthermore, an important task for this debate will be to scrutinize and question decisions made by fund-giving committees. This debate should influence, and be influenced by, intellectually more fundamental explorations of our problems of living and their possible resolutions, which is engaged in by social inquiry, pursued in accordance with the philosophy of wisdom. Ultimately, of course, the task of academic inquiry as a whole, pursued in accordance with the philosophy of wisdom, is to promote in the public domain rational exploration of problems of living and their possible resolutions. But such public debate has, as its task, to produce good ideas, and to influence policy decisions, whether in the

scientific or political domain; its task is not to *make* the decisions. This is rather different from what Koertge apparently imagines this book advocates. Third, it is vital that science, and academic inquiry more generally, keep – or acquire – independence from the pressures of public opinion, governments, industry, the media, the wealthy and powerful, and do not lose this in a hopeless attempt to become the 'servant of the people', hopeless because the idea of '*the* people' is a myth, people having contradictory desires, needs, problems, values and beliefs, and hopeless because this would destroy creativity, work of rare, individual genius (from which, I have said, we all benefit). And fourth, sustained imaginative and critical scrutiny of the inherently *problematic* aims of science (problematic because of built-in assumptions about the unknown, and assumptions about values) ought, if anything, to make science more radical, less conservative, and less prone to mere fashion.

Chapter Fourteen
Aim-Oriented Empiricism since 1984

Since the publication of the first edition of this book I have made a number of important improvements to aim-oriented empiricism (AOE), and developed further arguments intended to show that the doctrine solves fundamental philosophical problems about science – such as the problems of simplicity and induction – which standard empiricism cannot solve.

1 Improved Versions of Aim-Oriented Empiricism (AOE)

In the first edition – in a deliberately simplified way – AOE represented knowledge in physics (and thus in natural science to some extent) at five levels. These are: (1) the thesis that the universe is comprehensible in some way (physicalism being a special case), (2) physicalism (the thesis that the universe is physically comprehensible), (3) best available metaphysical blueprint, (4) fundamental physical theory, and (5) empirical phenomena.

There is an obvious objection to the doctrine formulated like this. What if thesis (1) is false, and the universe is only imperfectly or partially comprehensible (in some way or other)? In chapter 9, I considered this possibility, and argued that science should reject it. I argued that science is justified in accepting that the universe is perfectly physically comprehensible because, if it is only imperfectly or approximately physically comprehensible, the best way we can acquire knowledge of this is to assume perfect physical comprehensibility, search for it and fail in the attempt. Even in an imperfectly or approximately physically comprehensible universe, in other words, the assumption of perfect comprehensibility is the most fruitful, heuristically and methodologically, to make. But I now think this need not be the case. The universe might be so constituted that infinitely many theoretical revolutions are required before we can arrive at the true physical theory of everything, it being the case, however, that after each revolution one more force needs to be postulated. In such an ultimately incomprehensible universe, science might still be possible. Furthermore, we might well, after two or three revolutions, cotton on to the point that the universe is such that the number of forces goes up after each revolution. In such a universe, this would be a more fruitful assumption to make than that of perfect physical comprehensibility. Considerations such as this add support to the point that AOE needs to take into account the possibility that the universe is not perfectly comprehensible.

And should we not, perhaps, consider much more extravagant possibilities? For all that we know for certain, ultimate reality may be very different from the way it is depicted by modern theoretical physics. Perhaps it is not physical at all. Perhaps physics is a sort of temporary illusion, and some time in the future physics will cease to apply to phenomena and we will find ourselves in a strange new world. Perhaps the universe is not even partially comprehensible. It might still be possible to live, and to acquire knowledge, even though of a kind very different from current scientific knowledge, nothing like modern science being possible in that its basic presupposition, imperfect or approximate physicalism, is false, and phenomena do not even occur as if it were true. Even in such bizarre and no doubt (in some sense) wildly improbably circumstances, there is one assumption about the world we would be entitled to make, namely that it is such that we can acquire some knowledge about our local environment sufficient to make life possible. If this assumption is false, we have had it, whatever we assume. In making this assumption we cannot, in any circumstances, endanger the pursuit of knowledge, and we may, quite possibly, aid it. This assumption of the *partial knowability* of the universe thus deserves to be a permanent item of scientific knowledge which is accepted, not because we have good grounds for holding it to be true, but because accepting it cannot obstruct, and can only aid, the pursuit of knowledge.

I have subsequently developed a number of more elaborate versions of AOE to take these possibilities into account. The first modification, the most elaborate, was spelled out in Maxwell (1998). This version of AOE has *ten* levels. The specific details of this version of AOE, low down in the hierarchy, might need to be modified if, for example, one is considering the history of science before Galileo, or if the future produces such dramatic changes in our conception of the universe that theses, low down in the hierarchy, need to be changed. In order to take such eventualities into account, I put forward *generalized aim-oriented empiricism* (GAOE): see Maxwell (1998, p. 101). GAOE holds that some kind of hierarchical view needs to be adopted, the top one, or more, levels being those of the ten-level version of AOE, but other, lower levels possibly being different. Subsequently I decided that the ten-level version of AOE was too elaborate, and I reduced the number of levels to *seven*: see Maxwell (2004b; 2005b). I have also suggested an alternative to physicalism – a sort of cosmological physicalism: see Maxwell (2004b, appendix, section 5, pp. 198-205). This amounts to a modification of the seven-level version of AOE. Then I complicated the picture again, and developed a version of AOE which takes, as the hierarchy of metaphysical theses, different versions of physicalism: see Maxwell (2004a). This version

of AOE, which presupposes that the universe is *physical* in character, can be embedded in the earlier, more accommodating versions of AOE which allow for the possibility that the universe might turn out to be non-physical. I have also spelled out how endlessly many much more restrictive versions of AOE can be developed which are applicable to different scientific specialities, each with its own restrictive presuppositions: see Maxwell (2004b, pp. 41-47). This is discussed below in connection with the problem of induction.

What is one to make of these different versions of AOE? I am inclined to think, now, that which one you choose to adopt depends on what your purpose is. If you want to solve the problem of induction, it may be necessary to consider the ten-level version. If you are exclusively interested in theoretical physics, and you are happy to assume that *some* version of physicalism is true, the version of AOE expounded in Maxwell (2004a) may suffice. Philosophers, anthropologists and others exploring wild cosmological possibilities might find it useful to do so within the framework of GAOE. Those concerned with specific scientific specialities – whether scientists, or historians or philosophers of science, will need to consider an appropriate specific version of AOE. These diverse versions of AOE are not *rivals*: they are more or less detailed exemplifications of a single basic idea.

In what follows, I first expound the ten-level version of AOE, and then, briefly, the seven-level version. Then, in the next section I tackle the fundamental problem of what the *unity* of a physical theory is. The solution to this problem provides us with eight distinct versions of physicalism. In the section after, I expound that version of AOE which exploits these eight versions of physicalism. I then tackle the problem of verisimilitude and, to conclude this chapter, I argue that AOE solves the problem of induction.

The basic idea behind the ten level version of AOE – see figure 11 – and the other versions, can be put like this. For science to proceed, and for the enterprise of acquiring knowledge to proceed more generally, an untestable, metaphysical assumption must be made about the nature of the universe. In order to give ourselves the best chance of achieving success we need to make an assumption that is fruitful and true, but it is more than likely that the assumption we make will be false. Granted this, in order to give ourselves the best hope of making progress in acquiring knowledge, we need to make, not just *one*, but a *hierarchy* of assumptions, these assumptions becoming increasingly insubstantial, and so increasingly likely to be true, as we ascend the hierarchy. We make those assumptions which seem to be implicit in our apparently most successful ventures at improving knowledge, and which seem to be inherently fruitful for improving knowledge, if true. The hierarchy, initially, simply makes explicit what is implicit in what seem to be our most

successful efforts at acquiring knowledge. We then *revise* metaphysical assumptions, and associated methodological rules, in the light of which seem to lead to the most empirically successful research programmes, but in such a way that we keep such revisions as low down in the hierarchy of assumptions as possible. Only when efforts at acquiring knowledge seem to be meeting with little success do we actively consider more radical revisions higher up the hierarchy. We assume, quite generally, that the top level 10 assumption, of figure 11, is true, and the bottom level 3 assumption is false. As we descend from 10 to 3, at some point we move from truth to falsity, and thus to an assumption which needs to be revised. Our hope is that as we proceed, and learn more about the nature of the universe, we progressively bring truth lower and lower down in the hierarchy. As our knowledge improves, assumptions and associated methods improve as well. There is positive feedback between improving knowledge and improving assumptions and methods – that is, knowledge-about-how-to-improve-knowledge. This positive feedback between improving knowledge, and improving knowledge-about-how-to-improve-knowledge is the *sine qua non* of scientific methodology and rationality. As science improves its knowledge and understanding of nature, it adapts its own nature to what it has discovered. The astonishing progressive success of science in improving our knowledge and understanding of nature owes much to the exploitation of this positive feedback, meta-methodological feature of AOE in scientific practice. (Even though the scientific community has officially upheld standard empiricism, fortunately its allegiance to this doctrine has been sufficiently hypocritical to make it possible to implement something close to AOE in scientific practice. Paying lip service to standard empiricism has nevertheless been damaging; freeing science of this hypocrisy, so that AOE becomes the official philosophy of science, would have beneficial consequences: see Maxwell, 1998 and 2004b, for further details.)

If we can be reasonably confident that the best available thesis at level 3 is false, we can be even more confident that accepted fundamental physical theories, at level 2, are false, despite their immense empirical success. This confidence comes partly from the vast empirical content of these theories, and partly from the historical record. The greater the content of a proposition the more likely it is to be false; the fundamental theories of physics, general relativity and the standard model have such vast empirical content that this in itself almost guarantees falsity. And the historical record backs this up; Kepler's laws of planetary motion, and Galileo's laws of terrestrial motion are corrected by Newtonian theory, which is in turn corrected by special and general relativity; classical physics is corrected by

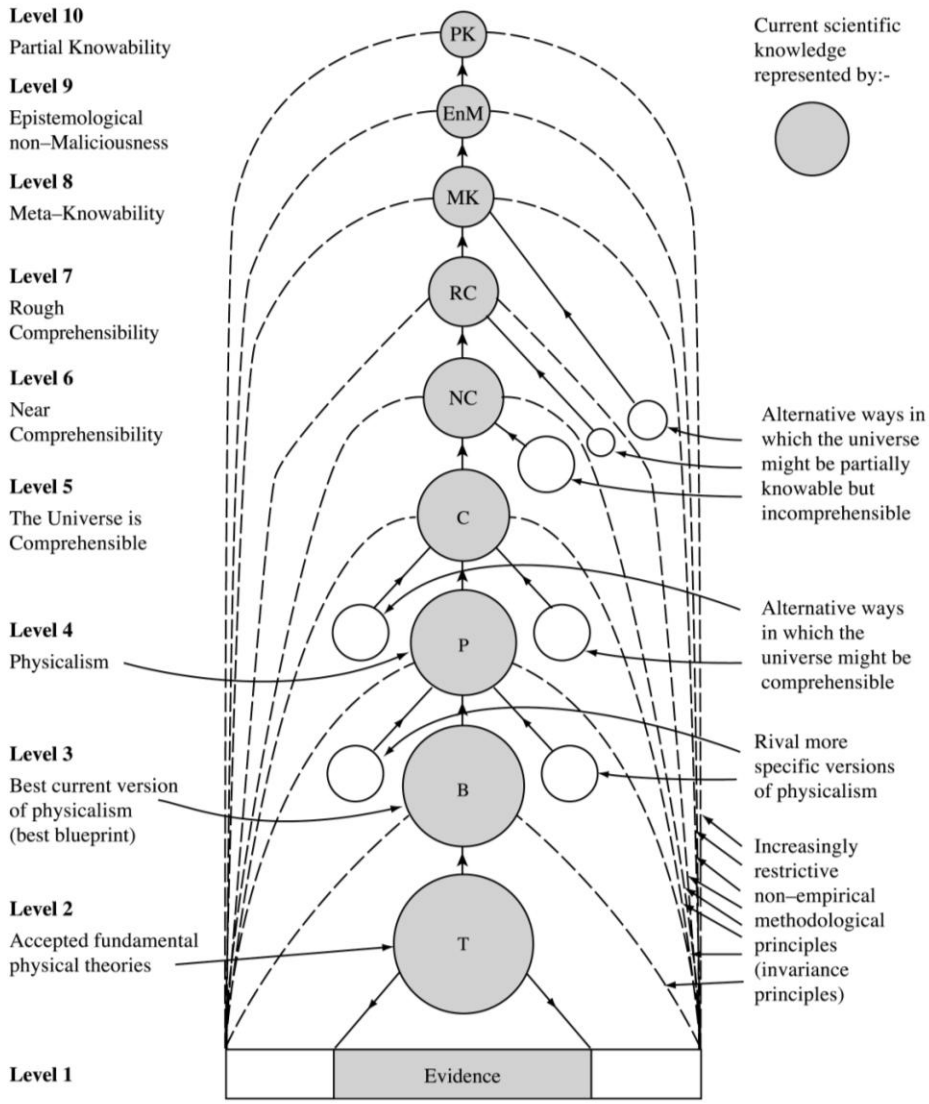

Figure 11: Aim-Oriented Empiricism (AOE)

quantum theory, in turn corrected by relativistic quantum theory, quantum field theory and the standard model. Each new theory in physics reveals that predecessors are false. Indeed, if the level 4 assumption of AOE is correct, then all current fundamental physical theories are false, since this assumption asserts that the true physical theory of everything is unified, and the totality

of current fundamental physical theory, general relativity plus the standard model, is notoriously disunified. AOE actually predicts that accepted fundamental physical theory, that is not both unified and (in principle) applicable to all phenomena, is false, whatever empirical success it may have.

In more detail, the ten-level version of AOE amounts to the following[1]: see figure 11.

Level 1: P1. Empirical data (low level observational and experimental laws).[2]

Level 2: P2. All accepted fundamental dynamical theories, or accepted laws governing the way physical phenomena occur if no dynamical theory has been developed that applies to the phenomena in question. In terms of current scientific knowledge, this level consists of the so-called standard model (SM) – the quantum field theory of fundamental particles and the forces between them – plus general relativity (GR).

Level 3: P3. *Best Blueprint*. The best available more or less specific metaphysical view as to how the universe is physically comprehensible, a view which asserts that everything is composed of some more or less specific kind of physical entity, all change and diversity being, in principle, explicable in terms of this kind of entity. Examples, taken from the history of physics are: the corpuscular hypothesis of the 17th century, according to which the universe consists of minute, infinitely rigid corpuscles that interact only by contact; the view, associated with Newton and Boscovich, according to which the universe consists of point-atoms that possess mass and interact at a distance by means of rigid, spherically symmetrical, centrally directed forces; the unified field view, associated with Faraday and Einstein, according to which everything is made up of one self-interacting field, particles of matter being especially intense regions of the field. Some might argue that the best available blueprint available today is the basic metaphysical idea of superstring theory, or M-theory as it is now called: the universe consists of

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1 The following scheme deliberately ignores vast tracts of scientific knowledge, such as: all of phenomenological physics, including such areas as solid state physics, thermodynamics and statistical mechanics; observational science carried on for its own sake, in astronomy, geology and elsewhere, and not in order to test fundamental physical theories; chemistry; biology; all of social science. For a justification of this neglect here, see remarks above and below, and Maxwell (1998, chapter 2, section 5). For my views about biology and social inquiry, all that which physics seems to miss out, see earlier chapters of the present work, and Maxwell (2001 and 2004b).

2 These are in the form of laws appropriately restricted in terms of range of application and accuracy, so as to stand a good chance of being true, and of being derivable, in principle, from appropriate theory.

minute quantum strings that move in 10 or 11 dimensions of space-time, all but four of which are curled up into a minute size, thus escaping detection. In Maxwell (1998, chapter 3) I argue, however, that the best available blueprint is a somewhat more general thesis that I call *Lagrangianism*.

Level 4: P4. *Physical Comprehensibility*. The more imprecise thesis that the universe is physically comprehensible in some way or other, everything being made up of just one kind of physical entity (or perhaps just one entity), all change and diversity being in principle explicable in terms of this one kind of entity. This thesis asserts that the universe is such that some as-yet-to-be-discovered unified physical 'theory of everything' (in the current jargon of theoretical physicists) is true. This is the thesis I have been calling 'physicalism'.

As we have seen, there are a number of ways in which the universe might be comprehensible even though physicalism is false. It might be that God exists, all natural phenomena being explicable in terms of the will of God. It might be that a society of gods exist, natural phenomena being the outcome of (and being explicable in terms of) the diverse, and sometimes conflicting, desires of the gods. It might be that, even though there is no God, there is some sort of overall cosmic goal, everything being explicable in terms of this cosmic goal (being required to fulfil the goal). Or it might be that there is some kind of cosmic programme, somewhat like a computer programme, which determines how events unfold; in this case events would be explicable in terms of the basic cosmic programme.

These conflicting views as to how the universe is comprehensible, together with physicalism, despite their diversity, all have something in common. They all hold that the universe is such that there is *something* (kind of physical entity, God, tribe of gods, cosmic goal, cosmic programme or whatever) which does not itself change but which, in some sense, determines or is responsible for everything that does change (all change and diversity in the world in principle being explicable and understandable in terms of the underlying unchanging *something*). This is the thesis at the next level.

Level 5: P5. *Comprehensibility*. The thesis (even more imprecise than physicalism) that the universe is comprehensible in some way or other, there being *something*, or an aspect of something (kind of physical entity, God, society of gods, cosmic purpose, cosmic programme or whatever) that runs through all phenomena, and in terms of which all phenomena can, in principle, be explained and understood. The thesis that the universe is comprehensible pushes to the limit the thesis that the universe is such that some phenomena can be explained and understood, to some extent at least: it asserts that the universe is such that *all* phenomena can, in principle, be

fully explained and understood (insofar as this is logically possible), all phenomena being explicable in terms of the one, unchanging *something*, present everywhere, at all times and places, throughout all phenomena, in an invariant form.[3]

Level 6: P6. *Near Comprehensibility*. The even more imprecise thesis that the universe is 'nearly comprehensible'. This means that the universe is sufficiently nearly comprehensible for the hypothesis that it is perfectly comprehensible to be more fruitful to adopt than any comparable assumption from the standpoint of the growth of knowledge.

Level 7: P7. *Rough Comprehensibility*. The even more imprecise thesis that the universe is 'roughly comprehensible' in the sense that the universe is such that there is some assumption of approximate comprehensibility (including the possibility of perfect comprehensibility as a special case) which is the most fruitful rationally discoverable [4] assumption to adopt from the standpoint of the growth of knowledge.

Level 8: P8. *Meta-Knowability*. The still more imprecise thesis that the universe is 'meta-knowable', which means that the universe is such that there is some rationally discoverable assumption about it which leads to improved methods for the improvement of knowledge.

Level 9: P9. *Epistemological Non-Maliciousness*. The universe is such that it does not exhibit comprehensibility, meta-knowability, or even mere partial knowability more generally, in our immediate environment only. However drastically phenomena at other times and places may differ from local phenomena, nevertheless the general nature of all such phenomena is such

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[3] In order to explain, in science, it is not sufficient to predict; it is necessary, in addition, to show that ostensibly diverse phenomena are diverse aspects of *one* phenomenon (or one kind of phenomenon), as when the diverse motions of terrestrial projectiles, the moon round the earth, the earth and other planets round the sun, double stars round each other, and stars round our galaxy are all aspects of the *one* kind of phenomenon of objects moving and interacting in accordance with Newton's laws of motion and law of gravity. The thesis that this is the proper way to understand scientific explanation will be developed in the next section: see also Maxwell (1998, chapter 4, and 2004b).

[4] The notion of 'rationally discoverable' is problematic. As I am using the phrase, no thesis about the universe is rationally discoverable if it is grossly *ad hoc*, and the *ad hoc* phenomena, postulated by the thesis, lie beyond our experience. (A thesis is grossly *ad hoc* if it is like the theories discussed in chapter 9 in that part devoted to the refutation of standard empiricism – points 2 and 3 – or like the most severely disunified theories considered in the next section of the present chapter.) Any thesis *ad hoc* in this way is one of infinitely many rivals, all equally arbitrary, there being no rationale to prefer the given thesis.

that it can in principle be discovered by us by developing knowledge acquired in our immediate environment. If inexplicable, arbitrary phenomena occur (phenomena specifiable only by some grossly *ad hoc* theory of the kind indicated in footnote 4 above), their occurrence is discoverable by us in our immediate environment.

Level 10: P10. *Partial Knowability*. The universe is such that we possess and can acquire some knowledge of our immediate environment as a basis for action.

Corresponding to each metaphysical assumption, at level r, where r runs from 3 to 9, there is a methodological rule (represented by sloping dotted lines in figure 11) which asserts: accept that level r-1 assumption (or collection of fundamental dynamical theories if r = 3) which best exemplifies the level r assumption, and which best promotes the growth of empirical knowledge (at levels 1 and 2), or at least holds out the greatest hope of doing this.

A few words of clarification concerning the principles at levels 3 to 10. They all assert, in different degrees, that the cosmos is more or less comprehensible or knowable. As we ascend, from level 3 to level 10, the theses become increasingly unspecific and contentless and thus, other things being equal, increasingly likely to be true. Theories at level 2 are burdened with massive precision and content; AOE predicts that, however empirically successful they may be, if, taken together, they clash with physicalism (as at present), then they are false. They are, in this case, fragmentary imperfect glimpses of an underlying unity. The best blueprint at level 3 is the best current attempt to do justice both (a) to theoretical knowledge at level 2, and (b) to physicalism at level 4. Ideally, it exemplifies physicalism in the sense that, what the blueprint postulates to exist that determines the way events occur must be (like what physicalism postulates) invariant throughout all phenomena. (If a blueprint is to exemplify physicalism perfectly, in other words, it must not add to physicalism in a patchwork way, for some, but not for all possible phenomena.) Level 3 blueprints have vastly less precision and content than current level 2 theory (SM plus GR); it is nevertheless reasonable to hold that all blueprints proposed so far are false, even if physicalism is true.

Each assumption, from level 3 to 6, asserts that the universe is comprehensible (to some degree at least), but with decreasing precision and content as we ascend from level 3 to 6. P_7, at level 7 asserts, still more modestly, that the universe is such that some assumption of partial comprehensibility is more fruitful than any rival, comparable assumption. It might be the case, for example, that the universe is such that there are *three*

fundamental forces, theoretical revolutions involving the development of theories that progressively specify the nature of these three forces more and more precisely. In this case, the assumption that there are three distinct forces would be more helpful than that there is just *one* fundamental force (required if the universe is to be perfectly comprehensible physically). Alternatively, it might be the case that the universe is such that progress in theoretical physics requires there to be a series of theoretical revolutions, there being, after each revolution, one more force: in this case, the assumption that the universe is such that the number of distinct forces goes up by one after each revolution would be more helpful for the growth of knowledge than the assumption that there is just one fundamental force. P8, even more modestly, asserts merely that the universe is such that existing methods for improving knowledge can be improved. These methods might involve consulting oracles, prophets or dreams; they need not involve developing explanatory theories and testing them against experience. P9 asserts, still more modestly, that the universe is such that local knowledge can be developed so that it applies non-locally;[5] and P_{10} asserts, even more modestly, that the universe is such that some factual knowledge of our immediate environment exists and can be acquired.

It is important to appreciate that these assumptions are to be understood in such a way that they presuppose some existing body of empirical knowledge (at levels 1 and 2), and existing methods for improving knowledge implicit in current practice. What is being asserted is that the universe is comprehensible, or meta-knowable, to *us*, with our current factual knowledge and implicit methods for improving knowledge.

The logical relationship between the propositions at the various levels is as follows. Let us suppose, initially, that the universe really is physically comprehensible, and the true theory of everything, T, at level 2, has been discovered. In this case, ideally, P_2 would entail P_1, and P_r would entail P_{r+1} for $r = 2,...8$. (P_9 does not entail P_{10} as we shall see below.) For $2 \leq r \leq 8$, we

[5] P_9 is a kind of 'principle of the uniformity of nature'. P_9 is, however, intended to be very much weaker than uniformity principles as these are usually formulated and understood. It does not assert that all phenomena are governed by the same laws everywhere, since the possibility of (some) arbitrary, '*ad hoc*' phenomena is conceded. Instead, P_9 asserts that if such phenomena occur anywhere they occur in our immediate environment. P_9 does not even assert that approximately lawful phenomena occur everywhere, but merely that whatever it is that makes our immediate environment partially knowable extends throughout the universe. We might live in a partially knowable world even though no laws strictly obtain, as the notion of law is understood in natural science.

may think of P_{r+1} as consisting of a statement of the form 'P_r or P_r* or P_r** or ...', where P_r*, P_r**, etc., are rival cosmological theses to P_r. In moving down from level r+1 to level r we adopt the factual conjecture that P_r*, P_r**, etc., are all false, and P_r is true.

For $5 \leq r \leq 8$, the above does not represent an idealization; in our present state of knowledge, P_r entails P_{r+1}. But for $r < 5$ the above is an idealization in many ways.

To begin with, even if we had discovered the true, unified theory of everything, T, this P_2 proposition at most entails P_1 propositions insofar as they are couched in the form: if such and such a state of affairs, S_1, exists at time t_1, then such and such a state of affairs, S_2, exists at time t_2. If T is comprehensive and true then it entails all true conditional statements of this type. However, our ability to extract detailed implications from T is bound to be severely restricted: the equations of T are likely to be solvable only for a few, extremely simple states of affairs; they may, indeed, not be solvable precisely at all, it being necessary to use approximation methods to extract predictions from T. This may involve making dubious additional assumptions, or simplifying assumptions known to be false. As theoretical physics has advanced, from Newtonian theory to general relativity and the standard model, so equations have become immensely more difficult to solve; it is reasonable to suppose that this trend will continue into the future.

Granted that we have discovered T (the true, unified theory of everything) no problem should arise in connection with P_2 implying P_3, P_3 in turn implying P_4, and P_4 implying P_5. But of course we have not discovered T (and may never do so, physicalism, perhaps, being false). Instead, we have at present at least two very different, even clashing, fundamental physical theories – the so-called standard model (SM) and general relativity (GR). This means P_2 conflicts with P_3. Even taken individually, currently accepted theories belonging to P_2 may clash with P_3 (as when Newtonian theory clashes with the corpuscular blueprint, or Maxwellian electrodynamics clashes with the Boscovichean blueprint). Furthermore, in trying to formulate P_3 in such a way that it does as much justice as possible to the theories of P_2, P_3 may conflict with P_4.

Although a theory, T, at level 2, may clash with a blueprint, B, it may also be a B-type theory, in the sense that it is a more or less disunified exemplification of B. Thus B might assert that the universe is made up of one kind of point-particle that interacts by means of one kind of force, and a theory, T, might postulate 2 (or more) kinds of point-particle, with different masses, perhaps, or charges. In this case, even though T is incompatible with B, it is nevertheless a B-type theory, a more or less disunified exemplification

of B. (Only theories which exemplify B perfectly imply B; theories which are more or less B-disunified are incompatible with B.) Analogous remarks concern the ways in which T may be related to physicalism, or B may be related to physicalism. In fact, quite generally, given theses P_r and P_s at levels r and s with $2 \leq r < s \leq 9$, P_r may be a more or less unified or adequate exemplification of P_s, even though P_r contradicts P_s.

An important non-empirical methodological rule of AOE asserts, in effect, that given two rival level r theses, P_r and Q_r, that one is to be preferred (other things being equal) which exemplifies the accepted level r+1 thesis, P_{r+1}, in the more unified, more adequate way.

The clashes (or disunities) between levels for $r < 5$, and clashes within levels, especially within P_2, serve to drive theoretical physics forward. These pose the problems that physicists try to solve. They are symptomatic of our ignorance. Progress in theoretical physics is to be assessed in terms of the extent to which a contribution promises to bring physics closer to the ideal state of affairs in which P_2 implies both P_1, and P_3 and P_4, P_2 being a candidate for the true, unified theory of everything.

In seeking to resolve clashes between levels, influences can go in both directions. Thus, given a clash between levels 1 and 2, this may lead to the modification, or replacement of the relevant theory at level 2; but, on the other hand, it may lead to the discovery that the relevant experimental result is not correct for any of a number of possible reasons, and needs to be modified. In general, however, such a clash leads to the rejection of the level 2 theory rather than the level 1 experimental result; the latter are held onto more firmly than the former, in part because experimental results have vastly less empirical content than theories, in part because of our confidence in the results of observation and direct experimental manipulation (especially after expert critical examination). Again, given a clash between levels 2 and 3, this may lead to the rejection of the relevant level 2 theory (because it is disunified, *ad hoc*, at odds with the current metaphysics of physics); but, on the other hand, it may lead to the rejection of the level 3 assumption and the adoption, instead, of a new assumption (as has happened a number of times in the history of physics, as we have seen). The rejection of the current level 3 assumption is likely to take place if the level 2 theory, which clashes with it, is highly successful empirically, and furthermore has the effect of increasing unity in the totality of fundamental physical theory overall, so that clashes between levels 2 and 4 are decreased. In general, however, clashes between levels 2 and 3 are resolved by the rejection or modification of theories at level 2 rather than the assumption at level 3, in part because of the vastly

greater empirical content of level 2 theories, in part because of the empirical fruitfulness of the level 3 assumption (in the sense indicated above).

It is conceivable that the clash between level 2 theories and the level 4 assumption might lead to the revision of the latter rather than the former. This happened when Galileo rejected the then current level 4 assumption of Aristotelianism, and replaced it with the idea that 'the book of nature is written in the language of mathematics' (an early precursor of our current level 4 assumption of physicalism). The whole idea of AOE is, however that, as we go up the hierarchy of assumptions, we are increasingly unlikely to encounter error, and the need for revision. The higher up we go, the more firmly assumptions are upheld, the more resistance there is to modification.

It deserves to be noted that something like the hierarchy of metaphysical theses, constraining acceptance of physical theory from above, is to be found at the empirical level, constraining acceptance of theory from below. There are, at the lowest level, the results of experiments performed at specific times and places. Then, above these, there are low-level experimental laws, asserting that each experimental result is a repeatable effect. Next up, there are empirical laws such as Hooke's law, Ohm's law or the gas laws. Above these there are such physical laws as those of electrostatics or of thermodynamics. And above these there are theories which have been refuted, but which can be 'derived', when appropriate limits are taken, from accepted fundamental theory – as Newtonian theory can be 'derived' from general relativity. This empirical hierarchy, somewhat informal perhaps, exists in part for precisely the same epistemological and methodological reasons I have given for the hierarchical ordering of metaphysical theses: so that relatively contentless and secure theses (at the bottom of the hierarchy) may be distinguished from more contentful and insecure theses (further up the hierarchy) to facilitate pinpointing what needs to be revised, and how, should the need for revision arise. That such a hierarchy exists at the empirical level provides further support for my claim that we need to adopt such a hierarchy at the metaphysical level.

Having expounded and defended this ten level version of AOE in great detail (see Maxwell, 1998), I decided subsequently that *ten* levels are perhaps excessive, and I reduced them to *seven* (Maxwell, 2004b): see figure 12. I then complicated the picture again somewhat, by introducing additional levels that explicate the different meanings that may be assigned to physicalism, and to unity of theory. I will say something about this in the next but one section, after I have first discussed the problem of what the unity of a theory might be.

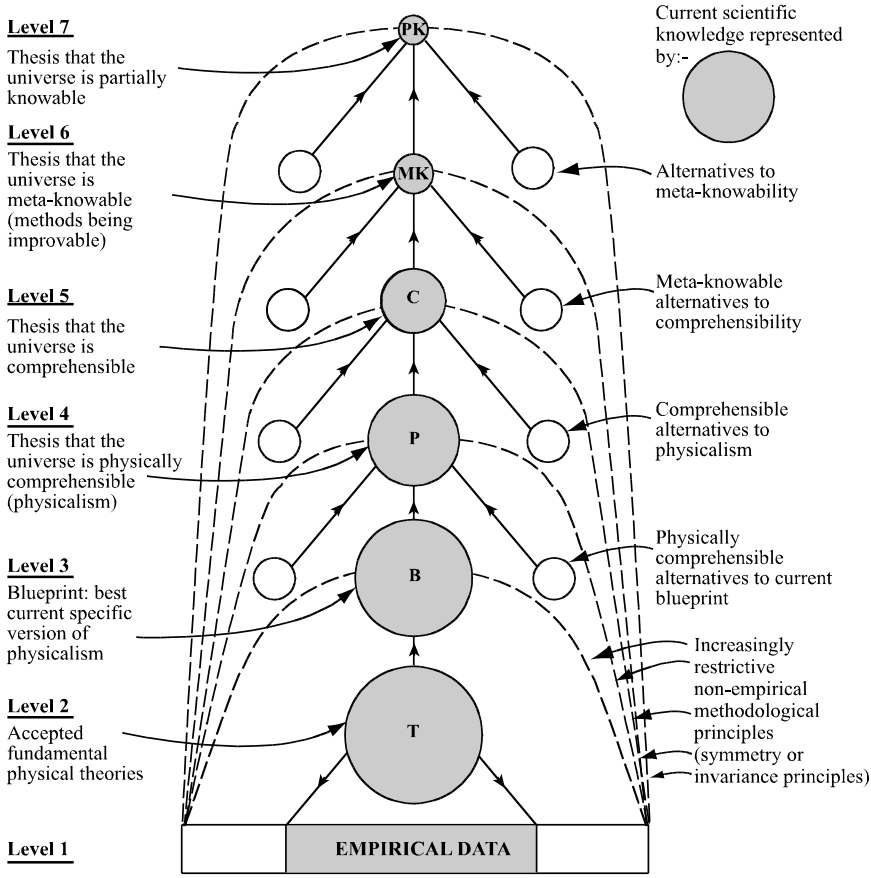

Figure 12: Aim-Oriented Empiricism

The extended version of AOE just indicated can be generalized, in line with the argument of chapter 5, to become an extended version of aim-oriented rationality. Not just in science, but in life too, aims can be profoundly problematic. Thus, generalizing from science, whenever the aim of any worthwhile endeavour is inherently problematic, it needs to be represented as a hierarchy of aims, these aims becoming less specific, more general, and thus less problematic as one goes up the hierarchy. In this way a framework of more or less unproblematic aims and associated methods is created within which much more specific and problematic aims and methods can be improved as the endeavour proceeds. Aim-oriented rationality, construed in this way, is especially relevant when there are conflicting aims and ideals: it enables those involved to distinguish agreement, high up in the hierarchy of aims, from disagreement, low down in the hierarchy, thus

facilitating resolution of conflict. Aim-oriented rationality is no magic cure for conflict, but in facilitating conflict resolution, it could help promote the desire for it by demonstrating that it is feasible.

Figure 13 depicts what this extended version of aim-oriented rationality might look like when applied to the fundamental and profoundly problematic aim and endeavour of creating a wise, civilized world.

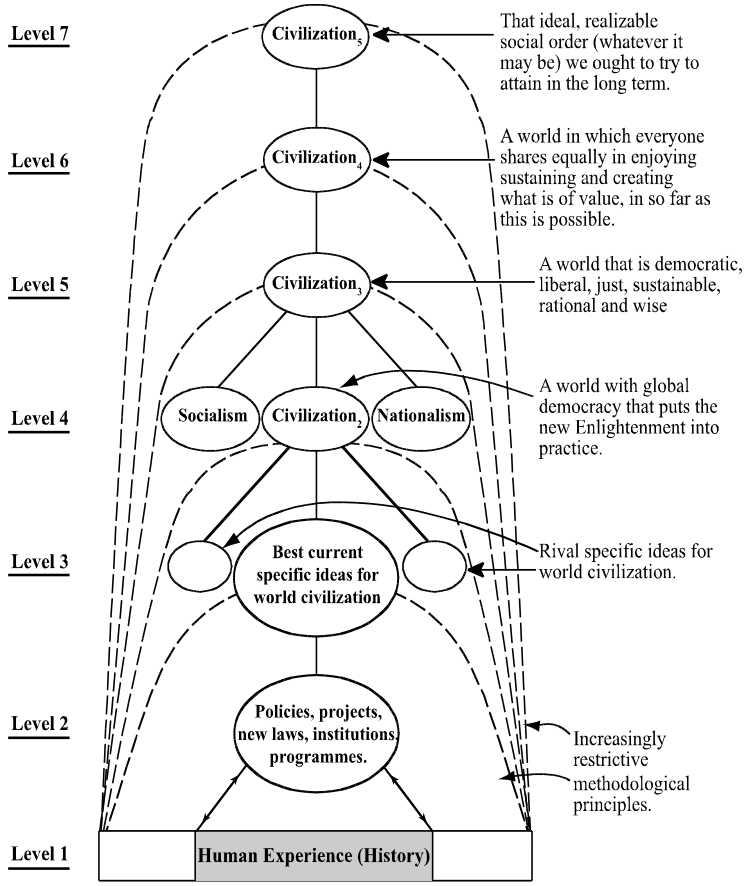

Figure 13: Implementing Aim-Oriented Rationality in Pursuit of Civilization

Figure 13 is the outcome of generalizing figure 12, and then applying the result to the task of creating civilization.

2 The Problems of Simplicity and Unity of Theory

A further improvement that I have made to AOE has to do with the simplicity, unity or explanatory character of theories. (In what follows I shall concentrate on 'unity'; I will then make a few remarks about 'simplicity'. The 'explanatory character' of a theory depends on just three things: unity, simplicity, and empirical content.)

Everyone recognizes that a theory, in order to be acceptable, must satisfy requirements of unity as well as requirements of empirical success. Horribly *ad hoc*, disunified, complex, aberrant theories, of the kind considered on pages 230-4 above, are just not considered in science, whatever their empirical success might be were they to be considered. But what is unity? Is there just one notion here, or several? How can one capture this notion of the unity of a theory when an apparently beautifully unified theory can always be reformulated so that it becomes horribly disunfied, and *vice versa*, a horribly disunified theory can be reformulated to become unified?[6] How are *degrees* of unity to be specified? And how is giving persistent preference to unified theories to be justified? Standard empiricism cannot answer these questions, as we saw on pages 234-7. The problem of 'simplicity' or 'unification', as this group of problems tends to be called, is widely understood to be a fundamental problem of the philosophy of science (Salmon, 1989). Even Einstein recognized the problem, acknowledged he did not know how to solve it, but said that it should be possible to solve (see Maxwell, 1998, pp. 105-6). Can AOE do better?

When I wrote the first edition of this book I was convinced that AOE must be able to solve these problems, in particular the problem of what theoretical unity *is*, but I did not know how it could be done. Four years later, while thinking about the problem in order to distract my attention away from an agonizing pain in my neck caused by a nerve squeezed between two vertebrae, the crucial insight come to me in a flash. Those who have attempted to solve the problem have been looking at entirely the wrong thing.

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6 Richard Feynman has provided the following amusing illustration of this point (Feynman et al. 1965, 25-10 - 25-11). Consider an appallingly disunified, complex theory, made up of 10^{10} quite different, distinct laws, stuck arbitrarily together. Such a theory can easily be reformulated so that it reduces to the dazzlingly unified, simple form: $A = 0$. Suppose the 10^{10} distinct laws of the universe are: (1) $F = ma$; (2) $F = Gm_1m_2/d^2$; and so on, for all 10^{10} laws. Let $A_1 = (F - ma)^2$, $A_2 = (F - Gm_1m_2/d^2)^2$, and so on. Let $A = A_1 + A_2 + \ldots + A_{10}10$. The theory can now be formulated in the unified, simple form $A = 0$. (This is true if and only if each $A_r = 0$, for $r = 1, 2, \ldots 10^{10}$).

They have been looking at the *theory itself*, its axiomatic structure, its number of postulates, its formulation, its characteristic derivations, the language in which it is formulated. But all this is wrong. What one needs to look at is not the theory itself, but what the theory says about the world, the *content* of the theory in other words. One needs to look, not at the theory, but at the world, or rather at the world as depicted by the theory. At a stroke the worst aspect of the problem of what unity *is* vanishes. No longer does one face what may be called the *terminological* problem of unity – the problem, namely, that the extent to which a theory is unified appears to be highly dependent on the way the theory is *formulated*. Suppose we have a given theory T, which is formulated in N different ways, some formulations exhibiting T as beautifully unified, others as horribly complex and disunified, but all formulations being interpreted in precisely the same way, so as to make precisely the same assertion about the world. If unity has to do exclusively with *content*, then *all these diverse formulations of T, having the same content, have precisely the same degree of unity*. The variability of apparent unity with varying formulations of one and the same theory, T, (given some specific interpretation) – which poses such an insurmountable problem for traditional approaches to the problem (see Salmon, 1989; Maxwell, 1998, pp. 56-68) – poses no problem whatsoever for the thesis that unity has to do with *content*. Variability of formulation of a theory which leaves its content unaffected is wholly irrelevant: the unity of the theory is unaffected.

But now we have a new problem: How is the unity of the *content* of a theory to be assessed? What exactly does it mean to assert that a dynamical physical theory has a unified content?

What it means is that the theory has *the same* content throughout the range of possible phenomena to which the theory applies.[7] Unity, in other words,

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[7] What is invariant throughout the range of phenomena to which the theory applies is what is asserted by the *differential equations* of the theory. A simple example of a differential equation is $dy/dx = 2x$. This represents an infinite family of curves (or functions), $y = x^2 + A$, each curve being given by assigning a different value to the constant, A. This is a general feature of differential equations: they represent infinitely many different curves or functions. In physics, these functions, the so-called 'solutions' of the differential equations of the physical theory, determine how the different physical systems (to which the theory applies) evolve in space and time. It is in this way that *one and the same* differential equation can apply to infinitely many diverse physical phenomena – diverse physical systems which trace out quite different paths through space with the passage of time. (What, it may be asked, is dy/dx? It expresses the rate at which y is changing with respect to changes in x. Equivalently, it expresses the slope of the tangent to the curve of the function in question. If dy/dx is big, a tiny change of x means a big change in y. If

means that there is just *one* content throughout the range of possible phenomena to which the theory applies. If the theory postulates *different* contents, *different* laws, for different ranges of possible phenomena, then the theory is *disunified*, and the more such *different* contents there are so the more *disunified* the theory is. Thus 'unity' means 'one', and 'disunity' means 'more than one', the disunity becoming worse and worse as the number of different contents goes up, from two to three to four, and so on. Not only does this enable us to distinguish between 'unified' and 'disunified' theories; it enables us to assign 'degrees of unity' to theories, or to partially order theories with respect to their degree of unity.[8]

All this can be illustrated by considering 'aberrant' and 'non-aberrant' theories of the kind discussed in chapter 9. Thus Newton's theory of gravitation, $F = GM_1M_2/d^2$ is unified in that what the theory asserts is *the same* throughout all possible phenomena to which it applies (all bodies of all possible masses, constitution, shape, relative velocity, distance apart, at all times and places). An aberrant version of this theory that asserts that this law is an inverse cube law after some definite time t_o, so that $F = GM_1M_2/d^2$ for times $t \leq t_o$ and $F = GM_1M_2/d^3$ for times $t > t_o$, is disunified because what the theory asserts is *not* the same throughout the range of possible phenomena to which the theory applies.

Note that special terminology could be introduced to make Newtonian theory look *disunified*, and the aberrant version of Newtonian theory look

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dy/dx is small, it needs a big change in x before there is much change in y. Just what one would expect granted that dy/dx is the slope of the tangent to the curve! Differential equations specify the fixed, unchanging way in which quantities change with respect to other quantities, these quantities being, as far as physics is concerned, such things as position, velocity, acceleration, time. Consider a function, such as $y = x^2$. Pick any point (x,y) on the curve of the graph of this function, and consider a second point very close to it $(x + \delta x, y + \delta y)$, where δx and δy are minute numbers. The fraction, $\delta y/\delta x$ is close to representing the slope of the tangent to the curve at the point (x,y). As δx and δy get closer and closer to zero, and $(x + \delta x, y + \delta y)$ gets closer and closer to (x,y), so $\delta y/\delta x$ gets closer and closer to expressing precisely the slope of the tangent at the point on the curve, (x,y). In the limit, as δx and δy approach zero, so $\delta y/\delta x$ approaches the true value of the tangent, dy/dx. It is not hard to show that in the case of the function $y = x^2$, $dy/dx = 2x$ for any point on the curve (x,y). In this case, as x gets bigger, so dy/dx gets bigger too, just as the graph of the function, a parabola, indicates.) For further details see Maxwell (1998, appendix).

[8] For earlier accounts of my proposed solution to the problem of unity of physical theory see Maxwell (1998, chs. 3 and 4; 2004b, chs. 1-2 and appendix; 2004a; 2004c; and 2007b).

unified. All we need do is interpret 'd^N' to mean 'd^N if $t \leq t_o$ and d^{N+1} if $t > t_o$'. In terms of this (admittedly somewhat bizarre) terminology, the aberrant theory has the form '$F = GM_1M_2/d^2$,' and Newtonian theory has the 'aberrant' form '$F = GM_1M_2/d^2$ for times $t \leq t_o$ and $F = GM_1M_2/d$ for times $t > t_o$'. But this mere *terminological* reversal of aberrance or disunity does not affect the *content* of the two theories: the content of Newtonian theory remains unified, and the content of the aberrant version (which looks unified) remains disunified. For unity, in other words, we require that the theory is *terminologically* invariant throughout the range of possible phenomena to which it applies when *terminology*, used to formulate the theory, is itself invariant throughout the range of possible phenomena (so that *terminological* invariance implies *content* invariance).

In practice in physics assessments of degrees of unity are somewhat more complex than I have indicated so far because of the following consideration. In assessing the extent to which a theory is disunified we may need to consider *in what way* different, or *how* different, from one another, the different contents of a theory are. A theory that postulates different laws at different times and places is disunified in a much more serious way than a theory which postulates the same laws at all times and places, but also postulates that distinct kinds of physical particle exist, with different dynamical properties, such as charge or mass. This second theory still postulates *different* laws for different ranges of phenomena: laws of one kind for possible physical systems consisting of one kind of particle, and slightly different laws for possible physical systems consisting of another kind of particle. But this second kind of difference in content is much less serious than the first kind (the kind that involves different laws at different times and places).

What this means is that there are different *kinds* of disunity, different *dimensions* of disunity, as one might say, some more serious than others. We can, I suggest, distinguish at least eight different kinds of disunity, as follows.

Any dynamical physical theory, T, can be regarded as specifying an abstract space, S, of possible physical states to which the theory applies, a distinct physical state corresponding to each distinct point in S. For unity, we require that T asserts that the same dynamical laws apply throughout S, governing the evolution of the physical state immediately before and after the instant in question. If T postulates N distinct dynamical laws in N distinct regions of S, then T has disunity of degree N.

Eight different kinds of disunity can now be specified. [These are numbered (8) to (1), rather than (1) to (8), because in the next section these

eight increasingly restrictive notions of unity will be incorporated into AOE.] In what follows, in connection with (3) and (2), there are a few physical and mathematical technicalities, which I attempt to explain. Some may find my explanations unhelpful; if so, I hope that (8) to (4) will be crystal clear, and will convey the general idea satisfactorily.)

(8) T divides space-time up into N distinct regions, $R_1...R_N$, and asserts that the laws governing the evolution of phenomena are the same for all space-time regions within each R-region, but are different within different R-regions.[9]

(7) T postulates that, for distinct ranges of physical variables (other than position and time), such as mass or relative velocity, in N distinct regions, $R_1,...R_N$ of the space of all possible phenomena, distinct dynamical laws obtain.

(6) In addition to postulating non-unique physical entities (such as particles), or entities unique but not spatially restricted (such as fields), T postulates, in an arbitrary fashion, N - 1 distinct, unique, spatially localized objects, each with its own distinct, unique dynamic properties.

(5) T postulates physical entities interacting by means of N distinct forces, different forces affecting different entities, and being specified by different force laws. (In this case one would require one force to be universal so that the universe does not fall into distinct parts that do not interact with one another.)

(4) T postulates N different kinds of physical entity,[10] differing with respect to some dynamic property, such as value of mass or charge, but otherwise interacting by means of the same force.

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[9] As I have formulated it here, (8) is open to two somewhat different interpretations. First, for N = 1 we require only that *the same* law operates throughout space in the sense that this would be true even if the law in question asserted that all objects experience a force directed at a unique point in space, and inversely proportional to their distance from that point. Second, for N = 1, we require that *the same* law operates throughout space in the sense that a mere change of position in space of an isolated physical system has no effect on the way the system evolves. An analogous distinction arises in connection with time. In what follows I adopt the second interpretation of (8), which means that a theory which is unified with respect to (8) exhibits symmetry with respect to spatial location, and time of occurrence. As far as the *ad hoc* version of Newtonian theory is concerned, N = 2 for both versions of (8).

[10] Counting entities is rendered a little less ambiguous if a system of M similar particles is counted as a (somewhat peculiar) field. This means that M particles all of the same kind (i.e. with the same dynamic properties) is counted as one entity. In the text I continue to adopt

(3) Consider a theory, T, that postulates N distinct kinds of entity (e.g. particles or fields), but these N entities can be regarded as arising because T exhibits some symmetry (in the way that the electric and magnetic fields of classical electromagnetism can be regarded as arising because of the symmetry of Lorentz invariance, or the eight gluons of chromodynamics can be regarded as arising as a result of the local gauge symmetry of SU(3)). If the symmetry group, G, is not a direct product of subgroups, we can declare that T is fully unified; if G is a direct product of subgroups, T lacks full unity; and if the N entities are such that they cannot be regarded as arising as a result of some symmetry of T, with some group structure G, then T is disunified.[11] (See note 11, and below, for clarification.)

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the convention that M particles, all the same dynamically, represents one kind of entity, rather than one entity.

[11] A few words of explanation. A homogeneous sphere exhibits symmetry in that it can be rotated through any angle about its centre, and it remains the same. Group theory is the mathematical theory of symmetry. Given any symmetric object, there will be a set of operations, a, b, c, … which, when performed on the object leave it unchanged. (In the case of the sphere, the operations are rotations about the centre.) These operations, a, b, c, … form a group, the symmetry group of the object. They must obey the following axioms. (1) There is the identity operation, i, which does nothing. (2) Any two operations, a and b say, can be combined to form a third, c, so that $a.b = c$. (3) Every operation, a, has an inverse, a^{-1}, so that $a.a^{-1} = i$. (4) Repeated operations are associative, so that $a.(b.c) = (a.b).c$. There are many different sorts of groups, finite, infinite, discrete, continuous. The symmetry group of the sphere is called SO(3).

A group G is a direct product of subgroups G_1 and G_2, written $G = G_1 \otimes G_2$, if the following three conditions hold: (a) G_1 and G_2 are subsets of G and groups in their own right, (b) $g_1.g_2 = g_2.g_1$, where g_1 is any member of G_1 and g_2 is any member of G_2, and (c) any member g of G is such that $g = g_1.g_2$, for some unique pair belonging to G_1 and G_2 respectively.

In theoretical physics, a symmetry arises when, given any isolated physical system (perhaps of some specific type) some specific kind of change is made to the system, and it evolves in time in just the same way, as if the change had not been made. Thus, given any isolated system, changing merely (a) its location in space, (b) its orientation in space, (c) its time of occurrence, or (d) its uniform velocity, leaves unaffected the way the system evolves. These are space-time symmetries, and apply to all dynamical physical theories (which presuppose that space-time is flat). Lorentz invariance is the name given to the symmetry, postulated by Einstein's special theory of relativity, which any physical system exhibits when its uniform velocity is changed.

There are, in addition, symmetries that apply to specific theories. Thus quantum field theories of electromagneticsm, the electoweak force, and the 'strong' force (which holds quarks together inside protons and neutrons) exhibit a symmetry called 'global gauge

(2) If (apparent) disunity of there being N distinct kinds of particle or distinct fields has emerged as a result of a series of cosmic spontaneous symmetry-breaking events, there being manifest unity before these occurred, then the relevant theory, T, is unified. If current (apparent) disunity has not emerged from unity in this way, as a result of spontaneous symmetry-breaking, then the relevant theory, T, is disunified.[12] (See below for clarification.)

(1) According to GR, Newton's force of gravitation is merely an aspect of the curvature of space-time. As a result of a change in our ideas about the nature of space-time, so that its geometric properties become dynamic, a physical force disappears, or becomes unified with space-time. This suggests the following requirement for unity: space-time on the one hand, and physical particles-and-forces on the other, must be unified into a single self-interacting entity, U. If T postulates space-time and physical 'particles-and-forces' as two fundamentally distinct kinds of entity, then T is not unified in this respect.

For unity, in each case, we require N = 1. As we go from (8) to (4), the requirements for unity are intended to be accumulative: each presupposes that N = 1 for previous requirements. As far as (3) and (2) are concerned, if there are N distinct kinds of entity which are not unified by a symmetry,

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invariance'. A feature of the physical state of the system, called the phase, can be changed by any fixed amount everywhere, at some instant, and the system evolves exactly as before. This can be transformed into a local symmetry, called 'local gauge invariance' as follows. At some instant, the phase is changed by different amounts at different places, and to compensate, the field is changed by different amounts at different places, but in ways that are determined by the (arbitrary) changes made to the phase: the result of these two compensating changes is that the physical system evolves as before, as if nothing had been changed. The symmetry groups of these local gauge symmetries of quantum electrodynamics, electroweak theory, and chromodynamics (the quantum field theories of electromagneticsm, the electroweak force, and the strong force respectively), are called U(1), U(1) ⊗ SU(2), and SU(3). Unlike U(1) and SU(3), U(1) ⊗ SU(2) is a direct product of subgroups, as the nomenclature indicates.

A somewhat more detailed, but still informal account of these matters is given in Maxwell (1998, ch. 4, sections 11 to 13, and the appendix, pp. 257-65). For rather more detailed accounts of the locally gauge invariant structure of quantum field theories see: Moriyasu (1983), Aitchison and Hey (1982: part III), and Griffiths (1987, ch. 11). For a delightful informal account of the role of symmetry and group theory in physics, see Zee (1986). For more technical introductory accounts see Isham (1989) or Jones (1990).

12 For accounts of spontaneous symmetry breaking see Moriyasu (1983) or Mandl and Shaw (1984).

whether broken or not, then the degree of disunity is the same as that for (5) and (4), depending on whether there are N distinct forces, or one force but N distinct kinds of entity between which the force acts.

(1) does not introduce a new kind of unity, but introduces, rather, a new, more severe way of counting different kinds of entity. (4) to (2) require, for unity, that there is one kind of self-interacting physical entity evolving in a distinct space-time, the way this entity evolves being specified, of course, by a consistent physical theory. According to (4) to (2), even though there are, in a sense, two kinds of entity, matter (or particles-and-forces) on the one hand, and space-time on the other, nevertheless N = 1. According to (1), this would yield N = 2. For N = 1, (1) requires that matter and space-time are no more than aspects of one basic entity (unified by means of a spontaneously broken symmetry, perhaps).

As we go from (8) to (1), then, requirements for unity become increasingly demanding, with (3) and (2) being at least as demanding as (5) and (4), as explained above.

One qualification ought, perhaps, to be added to the above. Isolated physical systems, that exhibit perfect symmetry related to the symmetries of the underlying theory, may evolve in accordance with a simplified version of the theory. Thus, given Newtonian theory (NT), two spheres of equal mass and dimension, rotating about a point equidistant between them, move in accordance with a simplified version of NT. They rotate uniformly in a circle whose centre is the mid point between the two spheres. This is not to be interpreted as a manifestation of disunity. (One could, of course, consider taking such anomalies seriously, and demand that a perfectly unified theory must be such that it does not permit physical systems which exhibit such symmetries perfectly, to exist.)

Let me now take (8) to (1) in turn, and give, in each case, an example of a theory with some degree of disunity.

(8) T asserts: Up to the last instant of the 21st century, NT holds; from the next instant on, a version of NT holds with the gravitation force repulsive instead of attractive. T, here, is disunified to degree N = 2, in a type (8) way.

(7) T asserts: everything occurs as NT asserts, except for the case of any two solid gold spheres, each having a mass of between one and two thousand tons, moving in otherwise empty space up to a mile apart, in which case the spheres attract each other by means of an inverse cube law of gravitation. T is again disunified to degree N = 2, in a type (7) way.

(6) T asserts: everything occurs as NT asserts, except there is one object in the universe, of mass 8 tons, such that, for any matter up to 8 miles from the centre of mass of this object, gravitation is a repulsive rather than attractive

force. The object only interacts by means of gravitation. T, here, is again disunified to degree N = 2, in a type (6) way.

(5) T postulates particles that interact by means of Newtonian gravitation; some of these interact by means of an electrostatic force $F = Kq_1q_2/d^2$, this force being attractive if q_1 and q_2 are oppositely charged, otherwise being repulsive, the force being much stronger than gravitation. T, here, is disunified to degree N = 2, in a type (5) way.

(4) T postulates particles that interact by means of Newtonian gravitation, there being three kinds of particles, of mass m, 2m and 3m. Here, N = 3, in a type (4) way.

(3) T postulates the classical electromagnetic field, composed of the electric and magnetic fields, obeying Maxwell's equations for the field in the vacuum. The symmetry of Lorentz invariance unifies these two fields (see below). Here, N = 1, in a type (2) way.

(2) T is Weinberg's and Salam's electroweak theory, according to which at very high energies, such as those that existed soon after the big bang, the electroweak force has the form of two forces, one with three associated massless particles, two charged, W^- and W+, and one neutral, W^o, and the other with one neutral massless particle, V^o. According to the theory, the two neutral particles, W^o and V^o, are intermingled in two different ways, to form two new, neutral particles, the photon, γ, and another neutral massless particle, Z^o. As energy decreases, the W+, W- and Z^o particles acquire mass, due to the mechanism known as spontaneous symmetry-breaking (involving another, hypothetical particle, not yet detected, called the Higgs particle), while the photon, γ, retains its zero mass. There appear to be two new, very different forces, the weak and electromagnetic. This theory unifies the weak and electromagnetic forces as a result of exhibiting the symmetry of local gauge invariance; this unification is only partial, however, because the symmetry group is a direct product of two groups, U(1) associated with V^o, and SU(2) associated with W^-, W+ and W^o.[13] This is type (7) unity.

(1) One might imagine a version of string theory without strings, different vibrational modes (perhaps) of empty, compactified six-dimensional space giving rise to the appearance of particles and forces, even though in reality there is only 10 dimensional space-time. Or one might imagine that the quantization of space-time leads to the appearance of particles and forces as only apparently distinct from empty space-time. In either case, N = 1 in a type (1) way: there is just the one self-interacting entity, empty space-time.

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[13] For further discussion see (Maxwell 1998, 131-40, 257-65 and additional works referred to therein.) See also notes 11 and 12.

In all eight cases, disunity arises because *different* laws govern different regions in the space of all possible phenomena predicted by the theory in question. This is obvious as far as (8) is concerned. In the case of (7), if laws are different depending on whether the value of some variable V is less or greater than some value V_O, then for those parts of the space of all possible phenomena, S, in which $V < V_O$, laws governing phenomena will be different from parts of S in which $V > V_O$. In the case of (6), regions of S in which the unique dynamic object is not present will be different from regions in which it is present. In the case of (5) and (4), regions of S in which only one kind of force or particle prevails will be governed by laws different from other regions in which a different kind of force or particle prevails.

As far as (3) is concerned, the point is perhaps best made by considering the particular case of classical theory of the electromagnetic field formulated so as to conform with Einstein's theory of special relativity. According to special relativity, the electromagnetic field is made up of two fields, the electric and the magnetic fields. On the face of it, there will be regions of S in which there is just an electric field, and other regions in which there is just a magnetic field, which means disunity. According to special relativity, however, a mere change of uniform velocity (with respect to a reference frame) cannot affect the way a system evolves: such a change leaves everything dynamically significant unchanged (as does a mere change of position or orientation in space). However, given any specific electromagnetic field, the way this divides up into an electric and magnetic field *is* changed by a change of uniform motion. In particular a pure electric field will become an admixture of electric and magnetic fields, and a pure magnetic field will become an admixture of magnetic and electric fields. Granted that a mere change of uniform relative motion does not change anything dynamically, or physically, significant, we are obliged to hold that the electric and magnetic fields cannot be separated out in the way required for disunity. There is one unified entity, the electromagnetic field, with electric and magnetic aspects. Both aspects are always present, although, for some quite specific fields, this will not be apparent relative to a reference frame in *one very specific state of motion only*.

The paradigmatic illustration of (2) is, as I have indicated, Weinberg's and Salam's theory of the electroweak force. On the face of it, the four particles of the theory are very different, and cannot be transformed into each other by means symmetry operations. The photon (associated with the electromagnetic force) is massless, whereas the particles associated with the weak force, W^+, W^- and Z^o, have mass (nearly 100 times the mass of the proton), and two of these particles are charged. The underlying electroweak

theory nevertheless possesses the local gauge symmetry of U(1) ⊗ SU(2). All these particles at high energies, soon after the big bang, are massless. As the universe cooled, a kind of asymmetry developed in the vacuum, associated with the as-yet undetected Higgs particle, and it is this which creates the asymmetry between the particles of the theory. Something analogous occurs when a uniform block of iron is gradually cooled. The lowest energy state involves the minute magnets associated with the atoms of iron aligning themselves so that there is an overall magnetic field in some specific direction. There is a loss of spatial symmetry – the symmetry is 'broken'. The underlying theory of electrodynamics does not, however, pick out any preferred direction. The theory has directional symmetry, even if the block of iron does not.

As far as (1) is concerned, if space-time and particles-and-forces are distinct then, presumably, one region (or possibly point) of S consists of nothing more than empty space-time.

Granted that theoretical physics is pursued in such a way that theories that fail to satisfy (8) to (6) are rejected, whatever their empirical success might otherwise be, it is clear that this means that physics thereby assumes that the universe is such that no physical theory which violates (8) to (6), with $N > 1$, is true. This accords with AOE but violates standard empiricism. Standard empiricism cannot solve the problem of what theoretical unity is because it cannot endorse the crucial point that unity applies to the *content* of theories (and at the same time hold that unity considerations may over-ride empirical considerations) because this would commit standard empiricism to holding that science permanently accepts a metaphysical thesis (no disunified theory is true), which contradicts a basic tenet of the doctrine. Standard empiricism can only solve the problem by becoming inconsistent!

That the problem of what unity is can be solved granted AOE, but cannot be solved granted standard empiricism, is an enormous success for the former view.

Is AOE *required* in order to solve the problem of unity? Could it not be argued that a view which acknowledges, merely, that science makes the metaphysical assumption 'the universe is such that no theory that is disunified in senses (8) to (6) is true' is able to solve the problem of unity? There are two objections to such a claim. First, this fails to provide a rationale for biasing choice of theory unified in senses (5) to (1). Second, such a conception of science lacks the rationality of AOE: it dogmatically upholds its one metaphysical assumption (which might after all be false), whereas AOE allows science to *modify* such assumptions in the light of the empirical progress achieved by the rival research programmes to which rival

assumptions lead. AOE is permanently committed only to assumptions required to be true for the enterprise of acquiring knowledge to meet with any success at all.

I have formulated the above eight requirements for unity as applying to the individual theory. Formulated in this way, there is an obvious objection. In the case of requirements (6) to (2), the methodological demand that an acceptable theory be unified can always be satisfied trivially: given a theory disunified to degree N = 6, let us say, this can always be split into six theories, each unified with N = 1. The way to cope with this objection is to interpret (8) to (1) as applying to the totality of fundamental physical theory, and to empirical laws if there is no theory which predicts and explains them.

I now consider briefly three questions that may be asked in connection with this proposed solution to the problem of unity of physical theory.

First, what of 'simplicity'? Is this the same as 'unity', or something distinct? The 'simplicity' of a theory can be interpreted as having to do, not with whether the *same* laws apply throughout the space of possible phenomena predicted by the theory in question, but rather with the *nature* of the laws, granted that they are the same. Some laws are simpler than others. In order to overcome the objection that simplicity is formulation dependent it is essential, as in the case of unity, to interpret 'simplicity' as applying to the *content* of theories, and not to their *formulation*, their *axiomatic structure*, etc. For details, see Maxwell (1998, pp. 157-9). It is a further great success of AOE that it succeeds in distinguishing sharply between these two aspects of the problem of what the explanatory character of a physical theory *is*, namely the *unity* aspect, and the *simplicity* aspect, and succeeds in solving both.

On the face of it, mere terminological simplicity can play no important heuristic or methodological role in physics at all because, given any *unified* theory, it can be made as simple or complex as we like by appropriate choice of terminology. But what is paradoxical about the role of simplicity in physics is that terminological simplicity does, in practice, seem to be highly significant heuristically and methodologically. How is this paradox to be resolved? A part of the answer is that what matters, for physics, is that a theory should be simple when formulated in terminology appropriate to a good, acceptable metaphysical blueprint – terminology that, for example, conforms to the symmetries of the blueprint. In addition, it is important that different laws and theories, applicable to different phenomena, should *all* be simple when formulated in the same appropriate terminology. The demand that all physical laws and theories should, as far as possible, be formulated in a common terminology appropriate to the best available blueprint means that terminological simplicity ceases to be something that can always be cooked

up artificially, and becomes something that is heuristically and methodologically significant. (For details see Maxwell, 1998, pp. 110-3.)

Second, does the question of whether laws governing a range of phenomena *remain the same* throughout their range of application have an unambiguous answer, in view of Goodman's 'grue' and 'bleen' paradox (Goodman, 1954)? Adapting Goodman's notions slightly, an object is grue if it is green up to the last moment of the 21st century, and blue thereafter; an object is bleen if it is blue up to the last moment of the 21st century, and green thereafter. Are not grue and bleen just as good predicates as blue and green? If the colours of objects change dramatically at the end of the 21st century, so that blue objects become green, and green objects blue, can we not, with equal legitimacy, say that there is no change, objects continue to be grue and bleen? This much discussed paradox is, in my view, very largely a red herring. On the face of it, the distinction made above, between formulation and content, suffices to dismiss the paradox. The sentence 'This object is grue' (S) may, as far as its written form is concerned, be invariant through the end of the 21st century, but what this sentence asserts, its content, is not invariant. To this, the reply may be made that the *content* of S may be regarded as being invariant. But this is not what is ordinarily meant by 'invariant' or 'remain the same': the above account of unity of theory appeals to the ordinary meaning of 'invariant' or 'remains the same', and not the perverse grue and bleen meaning. Two additional points. It should be noted that the Goodman paradox implicitly accepts the ordinary meaning of 'remains the same' in employing the terminology of 'grue' and 'bleen', terminology which remains the same, in the ordinary sense, throughout the end of the 21st century. Second, that the content of grue and bleen is not invariant with respect to the passage of time – unlike the content of blue and green which is invariant – is demonstrated by the point that if objects really are grue and bleen, and a person is convinced of this, then he can tell, by looking at grass and sky, whether or not the 21st century has ended, whereas the same is not true with respect to green and blue. Grue and bleen implicitly refer to a specific time in a way in which green and blue do not.

Third, Goodman's point concerning the ambiguity of 'remains the same' may seem to gain support from the mathematical notion of a function as a rule which takes one from one set of numbers to another. According to this notion, the two functions (1) $y = 3x$ for all x, and (2) $y = 3x$ for $x \leq 2$ and $y = 4x$ for $x > 2$, are equally good functions. Both functions 'remain the same' as x increases and passes through the value $x = 2$. Clearly, we need a narrower notion of function than this if we are to be able to distinguish between functional relationships which do, and which do not, 'remain the

same' as values of variables change. We need to appeal to what may be called 'invariant functions', functions which specify some fixed set of mathematical operations to be performed on 'x' (or its equivalent) to obtain 'y' (or its equivalent). In the example just given, (1) is invariant, but (2) is not. (2) is made up of two truncated invariant functions, stuck together at x = 2. Functions that appear in theoretical physics are *analytic*; that is, they can be represented as a power series (Penrose, 2004, pp. 112-4). Analytic functions are repeatedly differentiable. Such functions have the remarkable property that from any small bit of the function, the whole function can be reconstructed uniquely, by a process called 'analytic continuation'. All analytic functions are thus invariant. The latter notion is however a wider one, and theoretical physics might, one day, need to employ this wider notion explicitly, if space and time turn out to be discontinuous, and analytic functions have to be abandoned at a fundamental level.[14]

This concludes my discussion of what it means to say of a theory that it is simple and unified. AOE not only solves the problem of what simplicity and unity *are*; it also solves the problem of why it is *rational* for science persistently to accept only those theories that are sufficiently simple and unified (as well as being sufficiently empirically successful, of course).[15] Standard empiricism fails to solve both problems.

It deserves to be noted that (8) to (1), in addition to explicating what it means to say of a dynamical theory that it is unified, also explicates eight different meanings that can be given to *physicalism*. Physicalism(n), for n = 8, 7, … 1, can be interpreted to assert: the universe is such that the true theory of everything is unified in an (n) type way, with N = 1. This will be exploited in the next section. More generally, the above provides us with the means to throw a two-dimensional grid over all possible *partially physically comprehensible* universes. We can interpret physicalism(n,N) to assert: the universe is such that the true theory of everything is disunified in an (n) type way to extent N, with n = 8, 7, … 1, and N = 1, 2, … ∞.

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[14] For a fascinating discussion of the problems that arise in connection with the wider notion of what I have called 'invariant function', see Roger Penrose's discussion of what he calls the 'Eulerian' notion of function: Penrose (2004, 6.4).

[15] For further details concerning this solution to the problem of unity of physical theory, see Maxwell (1998, especially chs. 3 and 4 and the appendix; 2004b, chs. 1, 2, and appendix, section 2; 2004c; 2004d).

3 A Further Extension of Aim-Oriented Empiricism (AOE)

The above layered interpretation of physicalism makes possible another version of AOE, relevant specifically to physics and modern science since Galileo. The different versions of physicalism(n, 1), as n goes from 8 to 1 correspond, in this version, to increasingly substantial and restrictive metaphysical theses and associated methods: see figure 14. Physicalism(4-2) are on the same level since they are all but equivalent to one another. As we descend the hierarchy, from level (8) to (1), theses become increasingly specific, increasingly potentially fruitful for future progress in theoretical physics, but also increasingly likely to be false and in need of revision. The corresponding methodological requirements for unity, as explicated in the last section, become increasingly demanding, but also increasingly speculative and uncertain. The totality of physical theory, at any given stage in the development of physics (except when a candidate unified theory of everything has been proposed and accepted) will only satisfy these methodological rules partially; a new theory, in order to be an advance from the standpoint of unity, must lead to a new totality of theory satisfying the methodological rules better than the previous totality.

In figure 14, each version of physicalism is taken to assert that the true theory of everything is unified to the full extent (in that sense) with $N = 1$. This restriction could conceivably be relaxed if the search for unity persistently failed.

Even with the restriction relaxed, however, the version of AOE depicted in figure 14 may turn out to be false. If we exclude from consideration physicalism($n = 8$, $N = \infty$) which permits anything, AOE as depicted in figure 14 assumes that the universe is at least partially physically comprehensible in the sense that phenomena occur in accordance with physical laws which are more or less unified, the traditional distinction between laws and initial conditions being presupposed. But even though the universe is physically comprehensible, the traditional distinction between laws and initial conditions might not be observed. As we shall see in the next section, the true theory of everything might be cosmological in character, and might specify unique initial conditions for the universe.

This possibility, and other possibilities of this kind, could no doubt be accommodated within a modified version of the above view. But there are other possibilities, of philosophical interest even if of no interest to physics as at present constituted, which cannot be so accommodated. Perhaps God is ultimately responsible for all natural phenomena, or some kind of cosmic purpose or cosmic programme analogous to a computer programme (as has been suggested). In these cases the universe would be comprehensible but

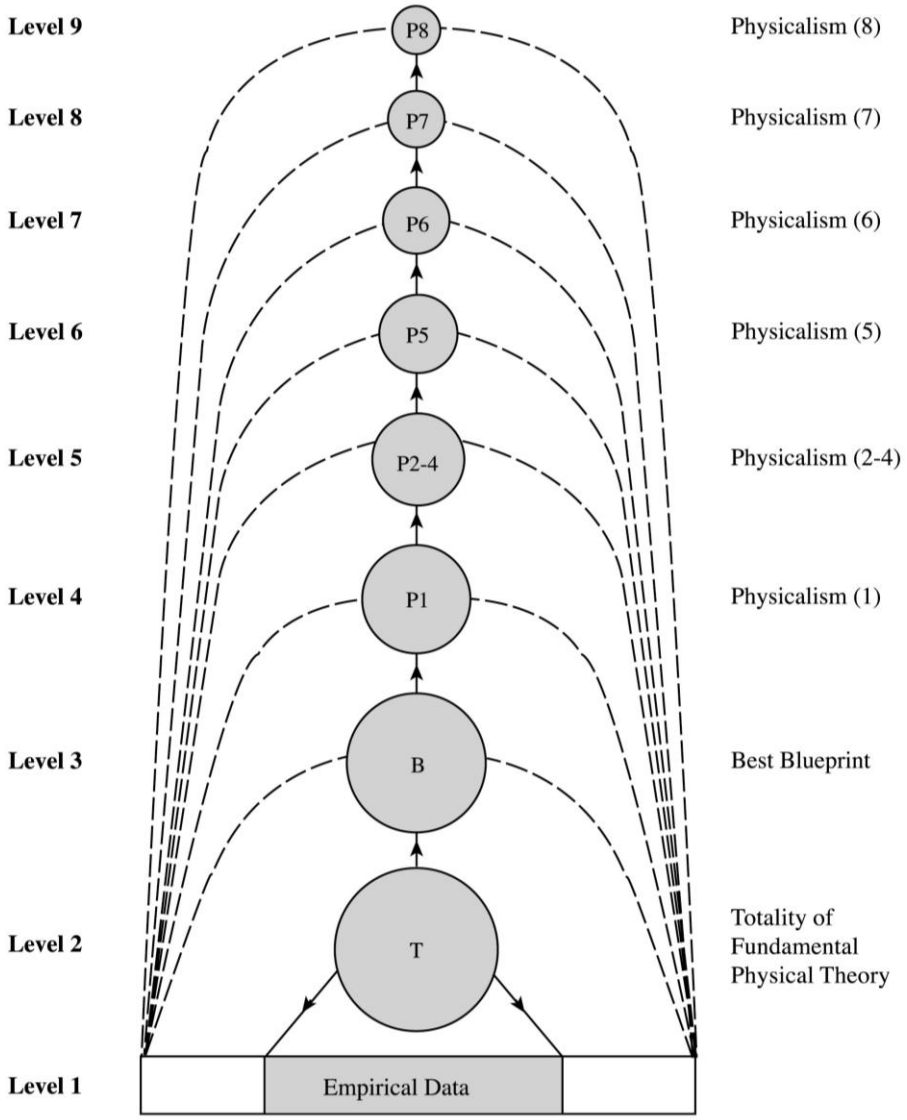

Figure 14: Another version of Aim-Oriented Empiricism (AOE)

not physically comprehensible – even though it might mimic a physically comprehensible universe.

In order to accommodate these, and other such, possibilities we need to embed the version of AOE depicted above in the version depicted in figures 13 or 12: see figure 15.

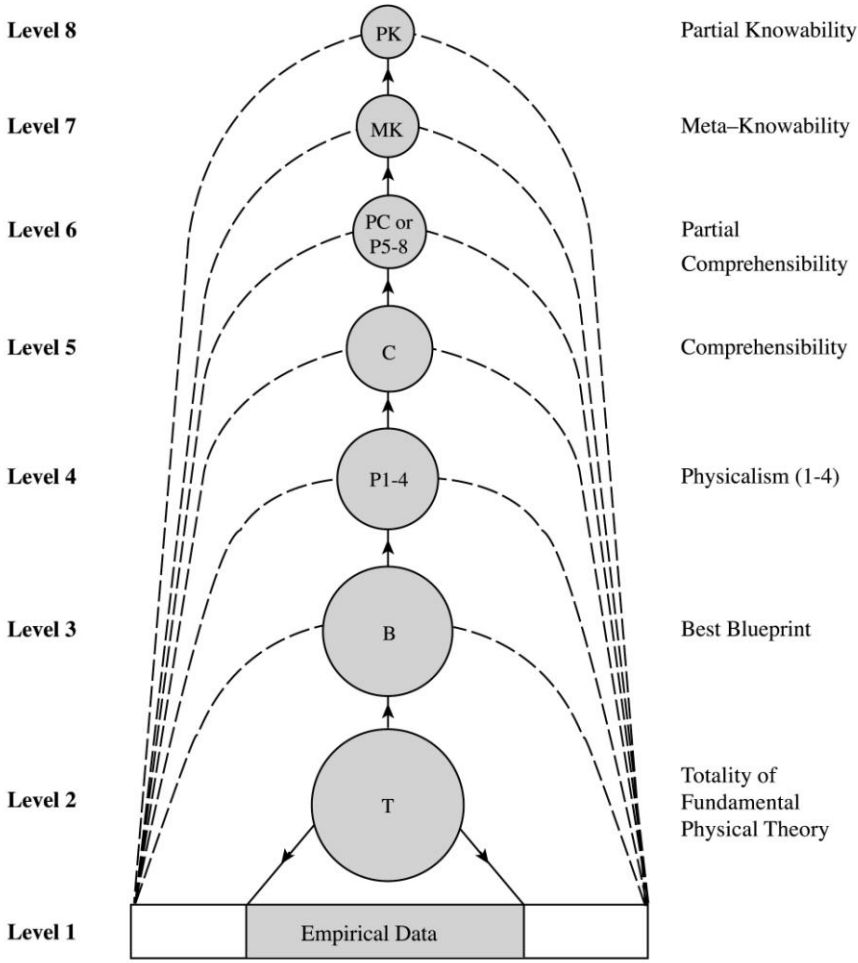

Figure 15 : Yet another version of Aim-Oriented Empiricism

4 Atomistic and Cosmic Physicalism

A basic motivation for making explicit metaphysical assumptions implicit in the methods of physics is that it provokes us into inventing new metaphysical possibilities, which we might not otherwise have considered. We are much more victims of *implicit* assumptions – of assumptions we deny

making – than of assumptions we make explicit. This consideration prompts the question: Are alternatives to physicalism(n,1) with n = 8 …1, conceivable?

The eight versions of physicalism depicted in figure 14 all hold that the physical universe, at any given instant,[16] is made up of two distinct aspects, which we may call U and V. U is what is depicted by the true physical theory of everything, T. It is inherent in all phenomena, everywhere, at all times. It does not itself change, but determines (perhaps probabilistically) the way that which changes does change. V, by contrast, is what does change and vary, from moment to moment, and from one place to the next. U and V together, at one instant, determine (perhaps probabilistically) V at the next instant.

This distinction between U and V can be traced back to atomism, the very first version of physicalism put forward by Democritus some two and a half thousand years ago. Given atomism, U consists of the unchanging properties of atoms and space, while V consists of the changing (relative) positions and motions of the atoms. As modern physics developed, ideas about the nature of U and V have changed, but the distinction itself has persisted up to the present. After Newton, rigid atoms interacting only by contact were transformed into point-atoms surrounded by rigid, centrally-directed fields of force. Here, U consists of the unchanging properties of the point-atoms and their surrounding fields of force (including the way the force falls off with distance and the affect it has on other point-atoms), while V consists of the changing (relative) positions and motions of the point-particles. Then it emerged, as a result of Maxwell's theory of the electromagnetic field and Einstein's theory of special relativity, that force fields are not rigid. Changes in the field take time to travel. This led to a new *unified field* version of physicalism, according to which everything is made up of an extended, self-interacting, unified field (matter being simply especially intense regions of the field). On the one hand there are changing, variable features of the field, V; and on the other, there are the unchanging features of the field, U, which determine how V changes, and which correspond to the laws of the true theory of the field. Subsequent developments have led to further changes in ideas as to what U and V are, but have not undermined the distinction itself.

It is no accident that the atomism of Democritus sharply distinguishes U and V. Atomism arose as an attempt to solve the problem of change, in particular the problem Parmenides posed with his argument that change involves a contradiction, and his view that the universe is a homogeneous,

[16] Or on any given spacelike hypersurface, looking at things from the standpoint of general relativity.

unchanging sphere.[17] Parmenides argued that change is impossible because the non-existent cannot exist, hence the world must be full, and hence there can be no room for movement or change. Democritus accepted the argument but rejected the conclusion. There is change, hence the non-existent must exist. The non-existent or, as we might say today, the void surrounds Parmenides's homogeneous, unchanging universe. Other Parmendian universes exist in the void. These can be shrunk down to a minute size, put in relative motion – and we have atomism. Each Democritean atom is a miniature Parmenidean universe. Atomism solves the problem posed by Parmenides by retaining as much as possible of the Parmenidean homogeneous, unchanging universe, but at the same time modifying this view just sufficiently to allow for change and diversity. Atomism solves the general problem of change – the problem of understanding how something can both remain the same *and* change – by segregating very precisely those aspects of atoms which do not change, and those which do change, the key to the distinction between U and V.

But there is another possible response to Parmenides. The universe as depicted by Parmenides – a homogeneous unchanging sphere – is a very special, uniquely unified state of the universe, the big bang state. This unified, initial state of the universe is unstable: spontaneous symmetry breaking occurs, and the universe evolves into a state made up of a great number of *virtual* big bang states. Today, every space-time point is made up of just one thing: a fleetingly existent, virtual big bang state.

Quantum theory can be interpreted as asserting that for very short intervals of time there is uncertainty of energy, and this permits so-called *virtual* particles to come into existence in the vacuum and almost immediately cease to exist. According to *cosmic physicalism* – the alternative to atomism as a response to Parmenides – every minute space-time region is composed, not of virtual particles, but of the virtual universe in its initial, unified, Parmenidean state. Before the big bang, unity is real and all disunity is virtual. After the big bang, disunity is real and unity is virtual. In a sense, there is only the big bang state. Variety and change come from the different ways in which instantaneously existent, virtual big bang states of the universe are inter-related.

There are, then, two distinct versions of physicalism which we may call *atomistic* and *cosmic* physicalism. They can be regarded as arising as a result of giving different responses to the challenge posed by Parmenides's impossible physically comprehensible universe.

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[17] This story has been told brilliantly by Karl Popper: see Popper (1998)

Atomistic physicalism takes the Parmenidean universe to depict U – that aspect of the universe which does not change and which determines the way that which changes, V, does change. Initially, U represents the properties of the atom. Subsequent developments in theoretical physics have transformed U, so that it may be taken to represent the invariant properties of a unified field, a quantum field, space-time of variable curvature, and so on. Despite these developments, the distinction between U and V persists, and it is this which is the hallmark of atomistic physicalism.

Cosmic physicalism, by contrast, takes the Parmenidean universe to be a special, uniquely unified state of the universe – the big bang state. According to cosmic physicalism, the true theory of everything, T, specifies the properties of the universe in this state. At a fundamental level, the distinction between U and V does not arise. The distinction only arises when spontaneous symmetry breaking has occurred, and the universe consists of momentarily existing virtual big bang states. V consists of the different, changing ways in which these momentarily existing big bang states are inter-related.

There are other striking differences between these two versions of physicalism. Cosmic physicalism is inherently *cosmological* in character, whereas atomistic physicalism is not. According to cosmic physicalism, T of itself specifies the initial state of the universe, whereas according to atomistic physicalism, initial conditions are required in addition to T to specify the initial state of the universe. Cosmic physicalism is inherently *probabilistic*, since spontaneous symmetry breaking is an inherently probabilistic process, whereas atomistic physicalism may be deterministic or probabilistic. Cosmic physicalism must be quantum mechanical to the extent, at least, of incorporating the quantum mechanical distinction between *actual* and *virtual*. Atomistic physicalism makes no such demand.

The two versions of physicalism specify very different conditions for underlying unity to become apparent in as simple a way as possible. According to atomistic physicalism, this happens when the physical system being considered is as simple as possible – the vacuum, or a one particle system or, somewhat more complex, a two particle system. According to cosmic physicalism, it is exactly the opposite: underlying unity is made manifest in a system consisting of *everything* – the entire universe in a very special state, the initial big bang state.

Theoretical physics so far has presupposed atomistic physicalism. But it is possible that cosmic, and not atomistic, physicalism is true. Elsewhere I have indicated a number of recent developments in theoretical physics, from the increasingly variable and dynamic character of space-time as suggested by

general relativity and quantum field theory, to the idea of spontaneous symmetry breaking and probabilism as suggested by the electroweak theory, which can be regarded as pointing in the direction of cosmic physicalism. For further details of the view, and arguments in support of the view, see Maxwell (2004b, appendix, section 5).

5 The Solution to the Problem of Verisimilitude

Physics advances from one false fundamental physical theory to another, and from one false level 3 blueprint to another. What, in this case, does it mean to say that physics is making progress? This is the problem of verisimilitude. Popper (1963, pp. 231-7) proposed a solution to the problem but, as we saw in chapter 9, this fails.

Philosophers of science, viewing the matter from a standard empiricist perspective, tend to regard the fact that physics advances from one false theory to another as having very negative implications for scientific progress. That physics will continue in this way has even been dubbed 'the pessimistic induction' (Newton-Smith, 1981, p. 14). But viewed from the perspective of aim-oriented empiricism (AOE), this manner of progression is actually to be expected, if physics really is making progress, and the universe really is physically comprehensible. For, if a theory, T_o, is precisely true throughout some restricted domain of phenomena D then, granted physicalism,[18] T_o must specify precisely what does not change, U, throughout all phenomena in D, and the way U determines how things change in D. But, according to physicalism, U exists unchanged throughout all phenomena. Thus, if T_o specifies the nature of U in D, it will be a straightforward matter to extend T_o so that it specifies U for all physically possible phenomena, T_o thus becoming the true theory of everything, T. Conversely, if T_o cannot be extended in this way to apply correctly to all phenomena, then T_o cannot be precisely true within D: T_o must be false. In brief, physicalism implies that a physical theory can only be precisely true of *anything* if it is (capable of being) precisely true of *everything*.

Granted, then, that physics proceeds, not by attaining T in one bound, but rather by developing a succession of theories that apply, with ever increasing accuracy, to ever wider ranges of phenomena until eventually a theory of everything is attained, it is inevitable, granted physicalism, that physics will

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[18] 'Physicalism' here, as elsewhere where there is no suffix, means 'physicalism(n,1) with $n \leq 4$'. We require a version of physicalism which asserts that there is an invariant **U** throughout all phenomena that are physically possible (according to that version of physicalism).

progress by the development of theories that are all *false* throughout their domains of application until the ultimate, unified true theory of everything is attained (which will be precisely true about everything).[19] Since physicalism predicts that physics will progress in this way, the fact that physics has so far thus progressed can only count in favour of physicalism: it cannot count against physicalism and AOE, as some have supposed.[20]

There is just one conceivable exception to this argument. It is possible that the form of T (or the nature of U) might be such that T reduces to an especially simple form for an appropriately simple or symmetric kind of system. Thus two spherical bodies of equal mass rotating about the point midway between them exemplify a law much simpler in form than Newtonian theory.[21] In having only what remains of T when it has been reduced to just such an especially simple form for some simple or symmetric system, one would have a true theory, but a theory *not* easily extendable to recover T. However, even if such a simplified version of T were to be formulated, it is most unlikely, before the discovery of T, that it would be correctly interpreted to apply only to appropriately symmetric kind of system. One would need to have T in order to know how to specify correctly systems to which the simplified version of T applies precisely. Interpreted to apply to a broader range of systems, the simplified version of T will not be precisely true.[22] It is in any case likely that the perfectly symmetrical system will not be a physical possibility in the actual universe. This is the case as far as the system consisting of two bodies rotating around each other, mentioned above, is concerned. However far away from other bodies this system might be, Newtonian theory, nevertheless, predicts that other bodies will slightly perturb the system, thus ensuring that it is not precisely symmetrical.

Given physicalism (and AOE), it is to be expected that physics advances by developing a succession of theories, T_o, T_1, T_2 ...T_n, which, though all false,

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19 Or rather, precisely true about that aspect of what exists which determines the way events evolve everywhere, at all times, throughout all phenomena.

20 See, for example, Laudan (1980), Newton-Smith (1981).

21 Given NT, precisely the right initial conditions, and nothing external to the system disturbing its evolution, the two spheres move in circular orbits with uniform speeds about the point midway between them. Interestingly enough, given GR, this is no longer the case: the rotating spheres radiate gravitational waves, and thus, very gradually lose energy. The spheres slowly spiral inwards – something that has been observed in the case of a double star system.

22 And on the other hand if it is precisely true when applied to all physically possible systems that differ slightly from the symmetric systems then, granted physicalism, it will be readily extendable to become T.

and though all mutually incompatible, nevertheless deserve to be regarded as getting progressively closer and closer to the truth, T. But what does it *mean* to speak, here, of T_o ... T_n getting 'progressively closer and closer to the truth'?[23]

AOE solves the problem as follows. T_o ... T_n get 'progressively closer and closer to the truth', T, if and only if: T_n can be 'approximately derived' from T (but not vice versa), T_{n-1} can be 'approximately derived' from T_n (but not vice versa), and so on down to T_o being 'approximately derivable' from T_1 (but not vice versa).

In order to explicate the key notion, here, of 'approximate derivation' let us consider a special case. Let us take T_o to be Galileo's version of the heliocentric theory (G), T_1 to be Kepler's laws of planetary motion (KL), T_2 to be Newtonian theory (NT), and T to be Einstein's theory of general relativity (GR). What does it mean to say that NT can be 'approximately derived' from GR, KL can be 'approximately derived' from NT, and G can be 'approximately derived' from KL? Let us take the case (considered briefly in previous chapters) of approximately deriving KL from NT.

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[23] It is important, in my view, to regard the problem of verisimilitude as being a problem that arises, in the first instance, and perhaps exclusively, in connection with progress in fundamental theoretical physics. This is where the problem arose in the first place, with the discovery, the realization, that theoretical physics advances from one false theory to another, and yet does genuinely seem to be making progress. The problem, interpreted in this way becomes, if anything, even more acute when it is appreciated that if physics really is making progress towards depicting the comprehensible structure of the physical universe, as AOE implies, then physics *ought* to make progress by advancing from one false theory to another. There must, it seems, be a solution to the problem: What can it mean to talk of progress in these circumstances? Some have interpreted the problem in a much wider way, as the problem of specifying what it can mean, quite generally, to say of a succession of false propositions, p_1, p_2, … that they get, progressively closer and closer to the truth. But it is not at all certain that there is a solution to this more general problem. We do not need to solve this more general problem to say what we mean by progress in parts of natural science outside physics. This is because in these other areas of natural science it does not happen, in the same way, that science advances, predictably and rigidly, from one false theory to another. Harvey's theory that the heart pumps blood around the body, put forward long ago, still seems true today, and it is not easy to see how it could ever turn out to be false. The idea that *all* natural science advances from one false theory to another is itself, quite simply, false. In my view, then, the fact that the solution to the problem of verisimilitude, proposed here, is restricted to theoretical physics does not mean that this proposal is limited or inadequate. The problem – and the solution – *need* to be restricted in this way. Theoretical physics is where the problem belongs.

This can be done in three steps. *First*, NT is restricted to N body systems interacting by gravitation alone within some definite volume, no two bodies being closer than some given distance r. *Second*, keeping the mass of one object constant, we consider the paths followed by the other bodies as their masses tend to zero. According to NT, in the limit, these paths are precisely those specified by KL for planets. In this way we recover the *form* of KL from NT. *Third*, we reinterpret this 'derived' version of KL so that it is now taken to apply to systems like that of our solar system. (It is of course this *third* step of reinterpretation that introduces error: mutual gravitational attraction between planets, and between planets and the sun, ensure that the paths of planets, with masses greater than zero, must diverge, however slightly, from precise Keplerian orbits.)

The approximate derivation of G from KL is even simpler: only two steps are required. *First*, KL is restricted to systems for which the elliptical paths of planets take the form of circles; and *second*, this restricted version of KL is then reinterpreted to apply to all systems to which KL applies.

The approximate derivation of NT from GR is, by contrast, somewhat more complicated. *First*, GR is restricted to systems of bodies with mass travelling along geodesics. *Second*, we consider the paths of the bodies as distances between the bodies are increased, relative velocities tend to zero, and the curvature of space-time tends to the limiting case of flat space and time. *Third*, the resulting laws are reinterpreted to apply to bodies of any mass travelling at any relative distance and velocity. In this way, we arrive at an instrumentalistic mimic of NT which asserts (in effect): bodies move *as if* there is a force of gravitation such that $F = ma$ and $F = Gm_1m_2/d^2$. According to GR, there is no force of gravitation; there is, rather, space-time that is curved by the presence of mass, or energy-density. Massive bodies travel along geodesics in this curved space-time, a geodesic being the equivalent of a straight line in curved space. The force of gravitation has disappeared. Since GR makes no reference to force, it is not possible to derive from GR a version of NT that asserts that the force of gravitation exists. It is possible, however, to derive a version of NT that makes precisely the same predictions as NT, which is all that we require.[24]

Quite generally, we can say that T_{r-1} is 'approximately derivable' from T_r if and only if a theory empirically equivalent to T_{r-1} can be extracted from T_r by taking finitely many steps of the above type, involving (a) restricting the range of application of a theory, (b) allowing some combination of variables

[24] For details see Schutz (1989: 205-208) or Rohrlich (1989).

of a theory to tend to zero, and (c) reinterpreting a theory so that it applies to a wider range of phenomena.

It is important – for this proposed solution to the problem of verisimilitude – that the true theory of everything, T, is not presupposed to be unified or comprehensible. We want the idea that successive theories get closer and closer to the truth to be applicable in as wide a range of possible universes as possible. We don't want this notion to be applicable in only physically comprehensible universes. The demand that the successive theories can all be derived from the true theory of everything, T, does place constraints on T, but it does not mean that T must be unified or comprehensible. The first step to be taken in approximately deriving T_n from T is to restrict the range of application of T to a specific kind of system. It is quite possible for T to be sufficiently unified as far as this specific kind of system is concerned to approximately imply the more or less unified theory, T_n, and yet for T to be seriously disunified for all other phenomena.[25]

This solution to the problem of verisimilitude can be exploited to solve the problem of what it means to say, of a succession of level 3 blueprints, B_0, B_2, ... B_n, that they get closer and closer to the true blueprint, B. Here, B is a blueprint of the true theory of everything, T. T implies B, but not *vice versa*. B, roughly, specifies the *kind* of entity precisely specified by T. B specifies symmetries which T must observe if it is to accord with B. Given B, T is the simplest theory there is compatible with B. Let T_0, T_2, ... T_n, be the simplest possible physical theories corresponding to B_0, B_2, ... B_n respectively. Then we may say that the blueprints, B_0, B_2, ... B_n get progressively closer and closer to B if and only if T_0, T_2, ... T_n get progressively closer and closer to T (in the way just explicated).

This proposed solution to the problem of what it means to say of a succession of blueprints that they are getting closer and closer to the true blueprint is likely to be misleading unless T is unified, and physicalism is true. Otherwise it would be possible for B_0, B_2, ... B_n to be progressively exemplifying physicalism more and more adequately, and at the same time getting closer and closer to B, even though B itself fails drastically to exemplify physicalism. To this extent (and to rule out this counter-intuitive possibility), physicalism and AOE do need, I think, here, to be presupposed.

It is worth noting just how ubiquitous 'approximate derivations' of the above type are in physics. When empirical predictions are derived from a

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25 This corrects Maxwell (1998, p. 214) where I said that this solution to the problem of verisimilitude 'requires AOE to be presupposed'. On the contrary, it is important that AOE and physicalism are *not* presupposed.

physical theory approximations are very frequently made during the course of the derivation. Higher order terms in some expansion are set to zero; complicated expressions reduce to simple ones as a result of the neglect of effects deemed to be sufficiently minute. All such 'approximate derivations', to be found everywhere in physics, are logically invalid in just the same way in which the derivations of KL from NT, and NT from GR, are invalid. It is legitimate to regard such 'derivations' as valid insofar as it is an easy, if pedantic, matter to turn them into valid derivations by replacing the precise conclusion with an approximate one. None of this ought to seem problematic to anyone with any first hand familiarity with physics.

In one important respect, the above solution to the problem of verisimilitude is unsatisfactory. If a series of theories, T_o ... T_n progressively approaches the truth, T, then, as we move from T_o to T_n, more and more of the form of T will be captured by the successive theories. This justifies regarding T_o ... T_n as constituting improving theoretical knowledge of the nature of the basic dynamic structure of the universe. Nevertheless, T_o ... T_n are all false. We do not have progress in knowledge in the sense of a progressive capturing of more and more empirical truth.

I have remarked above, however that, even though successive accepted physical theories are all false, we nevertheless regard them as making progress because they 'apply, with ever increasing accuracy, to ever wider ranges of phenomena'. This certainly seems to be true of the sequence G, KL, NT, GR, and of other such sequences of physical theories (from classical to quantum physics). Can a bit more precision be given to this idea that T_2 is 'closer to the truth' than T_1 because the predictions of T_2 are more accurate than those of T_1, and apply to a wider range of phenomena? It can.

The important point to appreciate, of course, is that accepted physical theories, despite being false, nevertheless make a vast amount of true *approximate* predictions. It is these true approximate predictions of T_2 and T_1 that we need to compare. Furthermore, the theories we are interested in make predictions about the way physical systems or states of affairs evolve in time. It is the true approximate predictions, made by T_1 and T_2, about how systems evolve in time that we need to compare. This we can do as follows.

We consider predictions that the theories – T_1, T_2 and the true theory of everything, T – make of any isolated system of the form:

[Theory + state of the system at time t_1] $\rightarrow$ state of the system at time t_2.

What is derived, here – the specification of the state of the system at time t_2 – is the prediction of the theory. T_1, T_2, and the corresponding specifications of the state of the system at time t_1, and the predictions – the derived specifications of the states of the system at time t_2 – are all false. But

these *false* specifications of the states of the system at times t_1 and t_2 imply true approximate specifications. In the case of Newtonian theory applied to the solar system, for example, such a true approximate specification would assert that each planet is located within such and such a region of space, having such and such a range of possible velocities (and would not give the precise position and velocity).

We can now declare that T_2 is closer to the truth than T_1 if:-

(a) The true approximate prediction of T_2 is more accurate, more precise, than the true approximate prediction of T_1;

(b) The true approximate specification of the initial state, at time t_1, associated with T_2, is at least as accurate, as precise, than the one associated with T_1.

(c) T_2 yields true approximate predictions of phenomena about which T_1 is silent (but T_1 makes no such predictions about which T_2 is silent).

If (a) to (c) hold, we can declare that T_2 makes more precise predictions than T_1 about more phenomena and is, in that sense, closer to the truth than T_1.

Why do we need clause (b)? Because we want to capture the idea that, if scientific progress is taking place, then increasingly accurate *predictions* are being made on the basis of specifications of initial states which at least do not decrease in accuracy. In fact these specifications of initial states will, no doubt, increase in accuracy as the predictions increase in accuracy. In the limit, when the true theory of everything is reached, T provides the means for true, precise specifications of initial and final states of the system (even though such specifications could not be made in practice).

In spelling out this second account of what it means to say of two false physical theories that one is closer to the truth than the other, I have slurred over some details concerned, in the main, with what it means, precisely, to say that one specification of the state of a system is more accurate than another. As these details are rather fussy and unilluminating, I have relegated them to an appendix to be found at the end of this chapter.

Even if (a) to (c) hold for T_1 and T_2, and T_2 is closer to the truth than T_1 in the sense just explicated, it still might be the case that T_2 makes wildly false predictions about phenomena about which T_1 is silent. In other words, T_2 might be much more accurate than T_1 about phenomena to which both theories apply and might make true approximate predictions about additional phenomena about which T_1 says nothing, but might, in addition, make wildly false predictions about further phenomena about which T_1 is silent. Even though having much more truth content than T_1, T_2 would also have much

more falsity content. If ever such circumstances arose in scientific practice, would we hold T_2 to be, nevertheless, an advance over T_1?

We might. T_2 might be accepted as a better theory than T_1, as long as it is restricted, in an *ad hoc* fashion, to phenomena for which it yields true approximate predictions. (Something like this is done when quantum theory is restricted in an *ad hoc* fashion so as not to apply to classical measuring instruments, for which it gives drastically false predictions.)

This second way of explicating what it means to say that T_2 is closer to the truth than T_1 would be characterized by Popper (1963, chapter 3) as 'instrumentalistic', in that it amounts to declaring that T_2 is a better *instrument* than T_1 for predicting phenomena (T_2 predicting more phenomena more accurately). This explication does not capture the idea that T_2 is closer to the truth than T_1 because T_2 is a more accurate characterization of the ultimate explanatory structure of the universe. But for that idea, we can turn to the first proposal, spelled out above. This second proposal is intended only to supplement the first. Taken together, the two proposals provide, I claim, an acceptable solution to the problem of verisimilitude as this arises in the context of theoretical physics.

6 The Problem of Induction

In chapter 9 I argued that AOE succeeds in solving the problem of induction, something which no version of standard empiricism can do. I have left unchanged what I said in that chapter of the first edition of 1984, since it is in my view essentially correct. But there have been developments since 1984, as I have already indicated, and some of these reveal the following inadequacies in the argument of chapter 9. To begin with, I argued (see page 247-8) that the best way we can improve knowledge in a partially comprehensible universe is to assume perfect comprehensibility, and fail to discover it. But, as I have already mentioned, situations might arise in which this is not correct. Again, the argument of chapter 9 fails to exploit properly the divergent, schematic accounts of the early evolution of natural science depicted on pages 255-7. Yet again, I argued that standard empiricism fails to solve the problem of verisimilitude, and the problems of simplicity, but I failed to explain how AOE solves these problems. Solutions to these problems (as we shall see) are required for the solution to the problem of induction. Again, the solution to the problem of induction sketched in chapter 9 makes essential use of the idea that AOE provides a framework for the *improvement* of false metaphysical assumptions – or blueprints – at the lowest level in the hierarchy of metaphysical assumptions, but no account is given there of what it *means* to say of two false metaphysical theses that one is

an 'improvement' over the other. Even more serious, and closely connected to the previous point, I fail to explain how the aim-oriented empiricist solution to the problem of induction overcomes what may be called the circularity objection. This objection is that it is invalidly circular to appeal to some metaphysical thesis in order to justify the success of science, and then appeal to the success of science in order to justify acceptance of the metaphysical thesis. AOE seems, if anything, to intensify this circularity objection, in that it is a proud boast of the view that it captures and facilitates positive feedback between improving theoretical knowledge, and improving accepted metaphysical theses and associated methods. Publications of mine subsequent to 1984 have to a considerable extent put right these inadequacies in the argument of chapter 9: see Maxwell (1998, especially chs. 4 and 5; 2002b: 2004a; 2004b, chs. 1 and 2, and appendix, sections 2 and 6; 2004c; 2005b; 2005e; 2006a; 2007a; 2007b). In what follows I draw the various threads of these arguments together to form a line of argument as strong and succinct as possible, and one that makes amends for the deficiencies of the account of 1984.

In chapter 9 I pointed out that there are two parts to the problem of induction, namely:

1. *The Theoretical Problem*: What grounds are there for holding that theories accepted in accordance with the methods of science embody knowledge, granted that our aim is to improve our theoretical knowledge and understanding of (aspects of) the universe?
2. *The Practical Problem*: What grounds are there for holding that theories accepted in accordance with these methods embody knowledge sufficiently reliable and trustworthy to form a basis for action?

To these two, a third part should be added:-

3. *The Methodological Problem*: What precise methods ought science to employ in accepting and rejecting theories in the light of evidence?

Problems 1 and 2 differ because they presuppose different aims or purposes for which theories are accepted. If our aim is to improve theoretical knowledge and understanding of the universe, as in problem 1, it may be more important that a theory we accept is fruitful, in suggesting further fruitful lines of research for example, than that its empirical predictions are reliable. Just the reverse is the case if the aim is that presupposed by problem 2. It would seem, on the face of it, that we have no reason to suppose that a theory accepted for the purposes of theoretical knowledge and understanding would invariably be the same as that accepted for practical purposes, for the sake of *technological applications* and *action*. In scientific practice, rather remarkably, these two very different purposes do often lead to the

acceptance of the same theory – although requirements that arise in connection with 2 may be more stringent than those that arise in connection with 1. (This latter point is not, perhaps, surprising. If a cosmological theory should turn out to be false, only some professional cosmologists may be disappointed; but if a theory employed in practical contexts, such as designing aeroplanes or developing drugs, should turn out to be false, people may well die. Naturally, in such *practical* contexts, we need to be more certain of truth, insofar as we can be, than in exclusively *theoretical* contexts.)

Whereas problems 1 and 2 both require that some kind of rationale or justification be provided for accepting theories in accordance with scientific method given one has such and such aim in mind, problem 3, by contrast, makes no such request for a rationale or justification. In order to solve problem 3, all one needs to do is specify the methods of science correctly.

If one looks at the history of attempts to solve the problem, one finds that most of the attention has been on problem 2. Problem 3 tends to be overlooked, the presumption being, it would seem, that that part of the problem can easily be solved. This attitude is a very serious mistake.

There is a vast literature on the problem of induction: see, for example (Kyburg, 1970; Swain, 1970; Watkins, 1984; Howson, 2002 – and references given therein). Most commentators hold that, despite this vast literature, the problem remains unsolved, and hardly any advance has been made towards its solution. Very few philosophers claim to have solved the problem, and when such claims are made, almost everyone else disagrees with them. Karl Popper is one of the few philosophers to have claimed to have solved the problem but, as he acknowledges himself, hardly anyone else agrees.[26] The problem has been around for over 250 years and has, it seems, stubbornly resisted endless attempts at solving it, so much so that, in recent years philosophers of science have grown weary of the problem, and no longer expect it to be solved, or indeed think it solvable. Given all this, why should my claim that AOE solves the problem be taken seriously for a moment?

It should be taken seriously because I can point to a *reason* why earlier attempts at solving the problem have failed. They have failed because they have presupposed (some version of) standard empiricism. Without even distinguishing problems 1 to 3, they have sought to justify acceptance of theories selected by methods prescribed by standard empiricism. *But this is to attempt to justify the unjustifiable.* Standard empiricism is, as we have seen, hopelessly unrigorous because it suppresses substantial metaphysical

[26] 'I think I have solved . . . the problem of induction. . . However, few philosophers would support the thesis that I have solved the problem' (Popper, 1972, p. 1).

assumptions made by science which influence what theories are accepted and rejected in addition to empirical considerations. In short, all earlier attempts at solving problem 2 have failed because invalid answers to problem 3 have been carelessly presupposed. The crucial first step in solving the problem of induction is to give the correct solution to problem 3. The correct solution is AOE. Previous attempts have failed because they have got this crucial first step wrong. The solution to be proposed here deserves attention because this crucial first step is got right.

Traditionally, the problem of induction is viewed as the problem of how claims to theoretical knowledge – especially theoretical scientific knowledge – can be justified given that no theory can be verified however much evidence may be accumulated in its favour. What the above considerations indicate is that the problem should be viewed in a quite different way. We should rather view the persistence of the problem of induction as an indication that there is something seriously wrong with the whole conception of science that is being presupposed by the way the problem is formulated. And we should formulate the problem, rather, like this: How do we need to change our views about the nature of science so that the problem of how theories are established on the basis of evidence no longer arises? The problem of induction is important because it provides a test for the adequacy of views about science. In order to be acceptable, a view as to what the aims and methods of science ought to be must lead to the solution to the problem of induction. We might also say: the task is not to *justify* science; rather, it is to see how science must be *changed* so that the problem of induction no longer arises.

Some of this is implicit in Popper's attempted solution. His proposed solution involved changing dramatically our whole conception of science, in that it is recognized that scientific theories can be *falsified* but not *verified* (a point now quite widely accepted, but once heresy). Popper's proposal, quite exceptionally, does make an important contribution towards solving the problem, precisely because it involves changing our view about science in a way that it needs to change, if the problem is to be solved. But Popper does not go far enough in this respect. Ultimately, his proposed solution fails. For, despite its revolutionary aspect, in one respect Popper's falsificationist conception of science is thoroughly conventional, in that it is a version of the untenable standard empiricism. Popper's proposed solution fails because it

does not even solve problem 3 above – the problem, merely, of specifying the methods of science.[27]

In order to solve the problem, we need to take matters one step further than Popper's falsificationism: we need to adopt AOE.

But does this suffice to solve problem 3? Even if AOE, with an appropriate choice of metaphysical blueprint at level 3 in the hierarchy of theses, solves the problem of specifying the methods of theoretical physics, does this suffice to solve the methods of natural science as a whole? I have three points to make in response to this question.

First, the problem of induction only arises in a pristine form in connection with theoretical physics. This is because all other branches of natural science presuppose relevant results of some other, explanatorily more fundamental natural science. Put crudely, biology presupposes chemistry which, in turn, presupposes physics. As a result, two kinds of consideration uncontroversially govern choice of theory in biology, let us say: empirical considerations from 'below', and relevant results of explanatorily more fundamental sciences, such as chemistry and physics, from 'above'. Thus, within biology – as should be clear even to a standard empiricist – evidence alone does not decide what biological theories are accepted and rejected: relevant parts of chemistry and physics play a role as well.[28] In order to

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[27] That Popper espouses standard empiricism is clear from his advocacy of his demarcation criterion: a theory, in order to be scientific, must be falsifiable (which renders unfalsifiable metaphysical statements unscientific). And, as I remarked in chapter 9, Popper defends the doctrine explicitly in defending 'the principle of empiricism, which asserts that in science, only observation and experiment may decide upon the acceptance or rejection of scientific statements, including laws and theories' (1963, p. 54). It might be thought that Popper's espousal of 'metaphysical research programmes' in his later publications represents a change of attitude towards the scientific status of metaphysics, but it does not. Although 'indispensable for science' such research programmes are, nevertheless, according to Popper 'more of the nature of myths, or of dreams, than of science' Popper (1982, p. 165). For a discussion of this point see Maxwell (2007a).

[28] Very occasionally, when a biological theory clashes with accepted chemistry or physics, it may happen that the biology is found to be correct and it is the chemistry or physics that needs to be revised. This happened when Kelvin, employing then current knowledge in physics, calculated that the earth could not have existed long enough for evolution to have occurred in the way described by Darwin, because if it had it would have cooled long ago to a temperature far below its present value. It turned out, subsequently, that Kelvin's calculations were incorrect because they ignored the heat generated by naturally occurring radioactivity associated with some of the constituents of the earth. Biology was right, physics was wrong. But this way of resolving such a clash is very infrequent; the norm is

confront the problem of induction in its naked, pristine form, we need to concentrate our attention on theoretical physics, since this is the only branch of natural science which does not have a more fundamental branch to presuppose. What this means, in turn, is that, as far as tackling the problem of induction is concerned, it suffices that AOE depicts the methods of theoretical physics; it does not need to specify the methods of natural science as a whole. (And given the explanatorily fundamental role of theoretical physics in the natural sciences, if the problem of induction can be solved for the former, this will suffice to solve it for the latter as well.)

Second, because theoretical physics is explanatorily fundamental in natural science, there is an important sense in which AOE, in being applicable to theoretical physics, as depicted above, in figure 12 let us say, is applicable to the whole of natural science.

But, third, there is a much more detailed and accurate way in which the general idea of AOE is applicable to the diverse methods of *all* the diverse branches of natural science. Different branches of natural science have different aims, and make different presuppositions, even if they are all inter-related in the way just indicated. Thus, a major aim of biology is to discover what survival value features of living things have – an aim that presupposes Darwin's theory of evolution. Such an aim and presupposition does not arise within the context of physics, cosmology or inorganic chemistry. Again, geology has the historical aim of discovering how various features of the earth's surface were created in the past: theoretical physics as it has been conducted up to the present does not have any such historical aim.[29] Specific aims and presuppositions of these types, made by specific branches of natural science, lead to the adoption of specific methods, designed to help achieve the specific aims, and corresponding to the specific presuppositions. These diverse methods of the diverse branches of the natural sciences, corresponding to diverse aims and presuppositions, can be accurately captured by the general idea of AOE. All we need to do is add one or more levels below level 3 of figure 12 to take into account specific presuppositions

for it to be resolved the other way round. All this is, of course, all but demanded by AOE, as I made clear when expounding the view.

[29] The idea of spontaneous symmetry breaking, if taken literally as an historical event which, in a sense, transformed manifest basic laws of physics, does give to theoretical physics a kind of historical aspect. This is apparent, too, in the emphasis given in theoretical physics to the study of conditions at or immediately after the big bang. If ever cosmic physicalism becomes the accepted blueprint, physics would acquire an even stronger historical character.

and aims of the specific natural science we are interested in, and associate relevant additional methods with these additional assumptions. In this way we can accurately capture the specific aims and methods of as wide a range of specific scientific specialities as we please, even to the extent of capturing accurately aims and methods of highly restricted scientific specialities. We can also, in this way, capture the *evolving* aims and methods of a scientific speciality by specifying the evolving specific presuppositions of that speciality. AOE is sufficiently flexible to capture both what is common to all of natural science and at the same time what is specific to diverse branches of natural science, however specialized they may be, and however much they may evolve with time.

Granted that AOE solves problem 3, it remains to be shown that it solves problems 1 and 2 as well.

In tackling problems 1 and 2, it is important not to formulate them in a way which renders them insoluble. Thus it is no good formulating the problem of induction as the problem of how physical theories can be verified by evidence, since such theories cannot be so verified. Nor should the problem be formulated as 'How can we have some grounds for holding that an empirically successful theory is true?', since the historical record tells us that even the most empirically successful physical theories turn out eventually to be false, and AOE tells us that all dynamical physical theories, not generalizable to all phenomena, are false. The above *theoretical* and *practical* problems of induction need to be reformulated slightly, along the following lines, to make this point explicit:-

1. *The Theoretical Problem*: What grounds are there for holding that a physical theory, accepted in accordance with the methods of science, embodies knowledge in the sense that, even though it may be false, it is a step towards the truth, granted that our aim is to improve our theoretical knowledge and understanding of (aspects of) the universe?
2. *The Practical Problem*: What grounds are there for holding that a physical theory, accepted in accordance with the methods of science, embodies knowledge in the sense that it will continue to yield true empirical predictions in standard regions of application, to standard degrees of accuracy, in a way that is sufficiently reliable and trustworthy to form a basis for action?

In tackling the problem of induction, it is important to appreciate just how strong the reasons are for holding that scientific knowledge makes presuppositions that are metaphysical and cosmological. There is the argument already encountered: in persistently failing even to consider endlessly many empirically more successful, disunified rivals to accepted physical theories, physics makes a persistent metaphysical and cosmological

assumption to the effect that the universe is such that no disunified theory is true. But even more striking, our most humble, prosaic, common sense claims to knowledge of things in our immediate environment make metaphysical and cosmological presuppositions. The proposition 'This chair on which I now sit will continue to exist and support me for the next 30 seconds' implies 'No cosmic convulsion is now occurring far away in the universe which will spread with near infinite speed to engulf and destroy the earth and everything on it, including me and my chair, in under 30 seconds time'. If the first proposition is true, then the second must be true as well, which means the first implies the second. Thus, if I know the first proposition, I at least implicitly know the second as well. Even our most trivial, common sense, observational claims to factual knowledge, which include knowledge of matters a mere second or two into the future, presuppose knowledge of metaphysical theses about the entire universe. If we deny that we have such cosmological knowledge we are obliged to deny, also, that we have trivial factual common sense knowledge of our immediate surroundings. Bereft of cosmological knowledge, we have scarcely any factual knowledge at all. These considerations can perhaps be taken in two ways: as establishing either extreme scepticism (we know nothing), or that we need to adopt a more conjectural conception of knowledge, one which is such that 'knowledge' of the entire cosmos does indeed become possible. But in any case, if even our most humble, limited, common sense items of particular, factual knowledge make presuppositions about the entire cosmos, it ought to occasion no surprise that our theoretical scientific knowledge, so vastly more burdened with empirical content, so much more precise and wide ranging in predictive power, makes such cosmological presuppositions as well.

It is, in a way, very odd that AOE has not been seen as the obvious view to adopt as the first step towards solving the problem of induction. Everyone agrees that evidence underdetermines theory. And yet, in practice, most of the time, very few theories contend for acceptance. Almost all of the infinity of rival theories that are compatible with the available evidence that always exist, never in scientific practice make their presence felt. It is entirely reasonable to conclude that this is because hidden, unacknowledged assumptions made by scientists, in addition to the evidence, exclude these infinitely many rivals. The obvious first step to take, in tackling the problem of induction, one would think, is to make these hidden, unacknowledged assumptions explicit. It is just this that one does if one is confronted by an invalid inference from correct premises to a correct conclusion: make explicit additional implicit premises which, once acknowledged, turn the invalid inference into a valid one. Why not take the analogous step in connection

with scientific 'inference' from evidence to theory (even if in this case, strictly speaking, no valid inference results)? It is just this which AOE does. It makes explicit implicit metaphysical assumptions concerning the knowability and comprehensibility of the universe which have the effect, when added to evidence, of tightly restricting theories that receive, and deserve, scientific attention (disunified rivals that are compatible with the evidence being excluded). There is the problem, of course, that the metaphysical thesis most effective in so restricting theories worthy of consideration – the metaphysical blueprint at level 3 – is most likely to be false: but the hierarchical framework of AOE is designed to help us put that right: it is designed to help us critically assess, and improve, this probably false thesis. Why has not AOE been adopted as the first step towards solving the problem of induction long ago? A version of the doctrine has been in the literature, after all, since 1974 (see Maxwell, 1974).

One reason may have to do with the demise of so-called 'rationalism'. Once upon a time some philosophers, the 'rationalists' – notably Descartes, Spinoza and Leibniz – held that some substantial theses about the nature of the universe could be established by reason alone. Some philosophers today may think that appealing to metaphysics in order to solve the problem of induction can only be successful if rationalism is correct, and the relevant metaphysical theses can be established by reason alone. Evidence cannot establish the truth of metaphysical theses so, if anything, it must be reason that one has to call upon to do the job. But rationalism is, nowadays, severely discredited. How could reason alone establish the truth of substantial theses about the universe? With the demise of rationalism – so the thought runs – comes the demise of the idea of appealing to metaphysical theses in order to solve the problem of induction.

But this objection collapses the moment one adopts a quasi-Popperian conception of knowledge, and acknowledges that all our knowledge is conjectural in character, it being just as impossible to justify the truth of scientific theories as scientific metaphysics, whether by an appeal to reason or evidence.

A more serious objection has to do with the apparent invalid circularity involved in appealing to metaphysical theses in order to solve the problem of induction – something that has already been alluded to. Such an approach would seem to involve justifying the success of science by an appeal to metaphysical principles, which are in turn justified by the success of science. But, as Bas van Fraassen has put it in a striking phrase (which I have quoted on other occasions), 'From Gravesande's axiom of the uniformity of nature in 1717 to Russell's postulates of human knowledge in 1948, this has been a

mug's game' (van Fraassen, 1985, pp. 259-60). How does AOE escape this charge?

The first point to note is that, quite independent of any claim to solve the problem of induction, a conception of science – call it presuppositionism – which acknowledges that science makes a persistent metaphysical assumption concerning unity is *more rigorous* than any standard empiricist conception which denies this. Intellectual rigour demands that assumptions (or conjectures) that are substantial, influential, problematic and implicit need to be made explicit. In persistently accepting simple, unified theories in preference to empirically more successful disunified theories, science thereby does make an (implicit or explicit) metaphysical assumption. Rigour demands that this assumption be acknowledged explicitly. Presuppositionism does this, but standard empiricism does not. This means that presuppositionism is more intellectually rigorous than any version of standard empiricism.

Attempts at solving the problem of induction, if they are to have any hope of success, must begin with the most *rigorous* conception of science available. It is clearly hopeless trying to justify the unrigorous, and therefore unjustifiable. This means that the actual situation is the exact opposite of what van Fraassen declares. The only hope we have of solving the problem of induction is to begin with presuppositionism, unless something better turns up; all views which reject presuppositionism, being inherently unrigorous, are doomed to failure.

A view that is even more rigorous than presuppositionism is available, namely AOE. This is more rigorous because it does not just rigidly and dogmatically accept some metaphysical thesis of unity, but instead accepts a hierarchy of theses, thus facilitating the critical assessment, and revision, of the more substantial theses in this hierarchy, those most likely to be false, in the light of the empirical success and failure of associated research programmes, and other considerations. AOE is more rigorous than presuppositionism because it focuses attention on those assumptions most likely to be false, and most likely to need revision and improvement, at the same time providing a relatively unproblematic framework within which such revision and improvement may proceed.

Granted all this, the conclusion is clear: attempts to solve the problem of induction must begin with AOE; all other approaches are doomed to failure.

But this does not solve the circularity problem. Indeed, it may even be judged to make this problem worse. For AOE has something like circularity built into it quite explicitly; it is even upheld as its greatest virtue and triumph. The whole point of the view, after all, as I have just emphasized, is to facilitate the critical assessment of theses low down in the hierarchy in the

light of the empirical success and failure of science. Successful theorizing may lead to a revision of level 3 blueprint ideas; such ideas constrain what is accepted at the level of testable theory (level 2). How, then, does AOE overcome the circularity objection?

In order to solve the problem, I shall argue, we need to see science as accepting a metaphysical thesis which, if true, renders the circularity of AOE legitimate, the reasons for accepting this thesis making no appeal to the success of science whatsoever.

It may be asked how AOE can in practice work at all if physical theories are both constrained by the current level 3 metaphysical thesis, and at the same time are able to modify this level 3 thesis. How can choice of theory both be influenced by, and influence, choice of level 3 thesis? The answer is that, as one goes up the hierarchy of levels of AOE, so the corresponding theses become more and more resistant to modification. Level 2 theories are only acceptable if sufficiently empirically successful, and sufficiently in accord with the best available thesis at level 3. But if attempts to develop theories in accordance with this level 3 thesis persistently fail, and a theory emerges that accords with the thesis at level 4 but clashes with the current level 3 thesis, then this thesis will be modified to accord with the new theory. Far greater persistent empirical failure would be required before this would legitimately lead to the rejection of the level 4 thesis of physicalism, and the adoption of some rival comprehensibility thesis, especially if this differed substantially from physicalism. Such a development would be dramatic and revolutionary indeed, for it would involve changing the whole nature of natural science. An intellectual earthquake would be needed before the level 5 thesis of comprehensibility deserved to be modified. It is the increasing resistance to modification as one goes up the hierarchy that makes it possible for theses accepted at one level both to be influenced by, and to influence, theses accepted at the next level up. The increasing resistance to modification that arises as one goes up the hierarchy is justified by the point that theses become increasingly contentless as one goes up the hierarchy, thus being increasingly likely to be true. It is also justified by the point that, as one goes up the hierarchy, theses become increasingly close to being such that their truth is required for science, or the pursuit of knowledge, to be possible at all.

As I have already pointed out above, a similar two-way influence takes place between theory and evidence. If a theory clashes with evidence then, in general, the theory will be rejected. This will occur especially if the clashing evidence consists of a number of different kinds of experimental result, each kind of experiment being repeated, and being subject to expert critical scrutiny. But the opposite also takes place in science. A clash between theory

and an experimental result may lead to the experimental result being rejected. Many experiments are very difficult to perform. It may take weeks before the apparatus involved works properly. Early experimental results that clash with established theory are regarded as indications that the apparatus is not working properly, and are rejected. In short, the two-way influence between theory and metaphysics, demanded by AOE, also takes place between theory and evidence, as every scientist would acknowledge.

Here, now, are nine further preliminary remarks concerning my proposed solution to the problem of induction, and the circularity problem in particular.

First, within the framework of AOE, no attempt is made to justify the truth of a physical theory by an appeal to a blueprint, the truth of the blueprint in turn being justified by an appeal to the empirical success of the theory. Physical theories, whatever their empirical success, and metaphysical assumptions, whatever their position in the hierarchy, and however fruitful in helping to generate empirical progress, remain conjectures. All our knowledge is presumed to be conjectural in character, even though we may conjecture that some parts are rather more conjectural than others.

Second, even though AOE provides no arguments for the *truth* of theses in the hierarchy, it does provide arguments for *accepting* these theses, granted that the aim of science is to acquire knowledge of the truth, insofar as this is possible. It is important to recognize just how different these two things are. To illustrate the point, Popper (1959) sets out to justify accepting that theory which has the greatest empirical content (other things being equal), even though that is the theory which is most likely to be false. He does so on the grounds that it is the theory with the greatest empirical content which we can most readily discover to be false (if it is false), discovering falsehood in this way being the means by which science makes progress. This Popperian justification for *accepting* a theory is diametrically opposed to any attempted justification of the *truth* of the theory, since the justification involves accepting that theory most likely to be *false*!

Third, and backing up the two points just made, it is important to remember that accepted physical theories (at level 2) and the best available blueprint (at level 3) will be *incompatible* with one another as long as no candidate theory of everything has been accepted (as at present). The circularity inherent in AOE can hardly be interpreted as any kind of attempt to justify the truth of the accepted blueprint by an appeal to the empirical success of accepted theories, in turn justified by an appeal to the blueprint, if these two are *incompatible* with one another.

Fourth, the rationale behind making explicit metaphysical theses implicit in the persistent scientific acceptance of unified theories is not to justify the truth of these theses. Quite the contrary, it is to make these theses available for sustained critical scrutiny (in the hope that they can be improved).

Fifth, it is vital to remember that it is not just theoretical knowledge in physics that presupposes and requires some metaphysical and cosmological knowledge. As we have seen, even our most trivial items of common sense knowledge about our immediate environment (that include knowledge of things mere seconds into the future) contain, implicitly, some knowledge about the entire cosmos. It hardly overstates the situation to say that we have no factual knowledge of anything if we do not have some knowledge, even if meagre, of everything. Failing to acknowledge the metaphysical, cosmological presuppositions of science cannot be anything other than intellectually dishonest. As I have stressed, merely acknowledging such presuppositions as an explicit part of conjectural scientific knowledge in itself enhances the intellectual rigour of science.

Sixth, it might seem, despite points one to four above, that any attempt to solve the problem of induction by appealing to some metaphysical or cosmological thesis *must* provide some grounds for holding that this thesis is true. But this seems hopeless: neither an appeal to evidence, nor an appeal to reason, could conceivably, it would seem, do the job. But what this demand neglects is that it presupposes an untenable, standard empiricist conception of science. It presupposes that science got going when it dissociated itself from metaphysics and concentrated on assessing claims to knowledge empirically. This view is hopeless, both as an historical account and as a prescription as to what ought to go on: see, for example, figure 9 of chapter 9 and associated text. As point five above makes clear, science cannot get going by dissociating itself from metaphysical presuppositions: instead, science gets going and proceeds by developing and preferring those metaphysical theses which seem best to promote progress in knowledge. The proper task, in other words, is not to provide arguments for the truth of some metaphysical thesis, but to provide arguments for the claim that one such thesis helps promote the growth of scientific knowledge better than rival theses. (But it is precisely arguments of this kind which introduce apparent invalid circularity.)

Seventh, the reasons that will be given for accepting metaphysical theses at the various levels of AOE are versions of the following: this thesis is the best to adopt, at its level of generality, given that the aim is to improve knowledge of the truth, insofar as this is possible. There are three reasons of this type, namely: this thesis (1) needs to be true for the pursuit of knowledge

to meet with any success at all; (2) holds out the greatest hope for progress in knowledge, if true; and (3) is in fact associated with progress in scientific knowledge – or what seems to be progress in scientific knowledge – in that it is the blueprint of the most empirically successful research programme.

Eighth, it is clear from point six, that there can be no knock-down, definitive solution to the problem of induction. This is because (a) one cannot list all possible theses that might be considered at each level, and (b) as science progresses, the thesis at level 3 is almost bound to change, and even theses higher up in the hierarchy may change.

Ninth, it is the *methodological* character of AOE that creates the circularity problem. If the methods of AOE, evolving in the light of the empirical success and failure of rival research programmes, had no more than *heuristic* force, suggesting merely that one kind of hypothesis might be sought rather than another, there would be no serious problem. What creates the problem is that these evolving methods of AOE have what may be termed *methodological* force: they influence (but in a fallible and revisable way) what theories are to be accepted and rejected, along with empirical considerations.

Quite enough preliminaries! I now sketch how, in my view, AOE solves the problem of induction, taking the version of AOE depicted in figure 12 above, beginning at the top of the hierarchy, and working down to accepted physical theories at level 2. What follows is only a sketch; a fuller account would be couched in terms of the version of AOE depicted in figure 11. For an account along these lines see Maxwell (1998, ch. 5).

Level 7: Partial Knowability. The universe is such that we possess and can acquire some knowledge of our immediate environment as a basis for action. If this is false, we cannot acquire knowledge whatever we assume. Accepting this thesis as an item of scientific knowledge can only help, and cannot sabotage, the pursuit of knowledge whatever the universe is like. We are justified in accepting this thesis as a permanent item of scientific knowledge even though we have no grounds for holding it to be true.[30] It should be noted that this is a thesis about the entire cosmos, and not just about our local environment.

Level 6: *Meta-Knowability*. The somewhat more precise thesis that the universe is 'meta-knowable', which means that the universe is such that there is some rationally discoverable assumption about it which leads to improved methods for the improvement of knowledge.

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[30] Sooner or later, this thesis will be falsified. Our current scientific knowledge tells us that, one day, the sun will become a red giant and engulf the earth; acquisition of knowledge, and life itself, will no longer be possible on earth.

As I have already acknowledged, the notion of 'rationally discoverable' is problematic. As the phrase is used here, no thesis about the universe is rationally discoverable if it is grossly *ad hoc*, like a theory that is disunified in a type 8 to 6 way, and the *ad hoc* phenomena, postulated by the thesis, lie beyond our experience. Any such thesis is one of infinitely many rivals, all equally arbitrary, there being no rationale to prefer the given thesis.

Meta-knowability brings us to my proposed solution to the circularity problem. Permitting metaphysical assumptions to influence what theories are accepted, and at the same time permitting the empirical success of theories to influence what metaphysical assumptions are accepted, may (if carried out properly), *in certain sorts of universe*, lead to genuine progress in knowledge. Meta-knowability is to be interpreted as asserting that *this is just such a universe*. And furthermore, crucially, reasons for accepting meta-knowability make no appeal to the success of science. In this way, meta-knowability legitimises the potentially invalid circularity of generalized AOE (GAOE), and of AOE.

Relative to an existing body of knowledge and methods for the acquisition of new knowledge, possible universes can be divided up, roughly, into three categories: (i) those which are such that the meta-methodology of GAOE or AOE can meet with no success, not even apparent success, in the sense that new metaphysical ideas and associated methods for the improvement of knowledge cannot be put into practice so that success (or at least apparent success) is achieved; (ii) those which are such that AOE appears to be successful for a time, but this success is illusory, this being impossible to discover during the period of illusory success; and (iii) those which are such that GAOE, and even AOE, can meet with genuine success. Meta-knowability asserts that our universe is a type (i) or (iii) universe; it rules out universes of type (ii).

Meta-knowability asserts, in short, that the universe is such that AOE can meet with success and will not lead us astray in a way in which we cannot hope to discover by normal methods of scientific inquiry (as would be the case in a type (ii) universe). If we have good grounds for accepting meta-knowability as a part of scientific knowledge – grounds which do not appeal to the success of science – then we have good grounds for adopting and implementing AOE (from levels 5 to 2). Meta-knowability, if true, does not guarantee that AOE will be successful. Instead it guarantees that AOE will not meet with illusory success, the illusory character of this apparent success being such that it could not have been discovered by any means whatsoever before some date is reached.

If AOE lacks meta-knowability, its circular procedure, interpreted as one designed to procure knowledge to the extent that this is possible, becomes

dramatically invalid, as the following consideration reveals. Corresponding to the succession of accepted fundamental physical theories developed from Newton down to today, there is a succession of severely disunified rivals which postulate that gravitation becomes a repulsive force from the beginning of 2150, let us say. Corresponding to these disunified theories there is a hierarchy of disunified versions of physicalism, all of which assert that there is an abrupt change in the laws of nature at 2150. The disunified theories, just as empirically successful as the theories we accept, render the disunified versions of physicalism just as scientifically fruitful as unified versions of physicalism are rendered by the unified theories we actually accept. The circularity inherent in AOE is invalid because it can be employed so as to lead to the adoption of *disunified* theories and metaphysical theses just as legitimately as it can be employed to lead to the adoption of *unified* theories and metaphysical theses. This is the case, at least, if AOE is bereft of meta-knowability. But if we have good reasons to accept meta-knowability as a part of scientific knowledge, then we have good reasons to reject *disunified* versions of physicalism: these lack the crucial requirement of rational discoverability. If we have good reasons to accept meta-knowability as an item of scientific knowledge, and these reasons make no appeal to the success of science, then the circularity inherent in AOE ceases to be invalid: meta-knowability asserts that the universe is such that empirical success achieved by implementing AOE will not be illusory *in a way which could not discovered by any means before a certain date*.

But what reasons have we for accepting meta-knowability that make no appeal to the success of science? One argument is simply this. As the pursuit of knowledge, and science, have developed over the millennia, GAOE and AOE have in fact been put into practice. Metaphysical presuppositions have been revised in the light of which seem to meet with the greatest empirical success – from myths, religious views, the ideas of the Presocratics, the ideas of Plato, Aristotle, Galileo, Boyle, Newton, and Boscovich, to field ideas, ideas associated with quantum theory and string theory, to physicalism. Even empirical methods have been revised in the light of metaphysical revisions. For example, given Aristotelian metaphysics, with its denial that precise mathematical laws govern natural phenomena, there is little point in performing precise experiments to decide between rival theories. This changes dramatically once Galileo's metaphysics is accepted, according to which 'the book of nature is written in the language of mathematics' (an early statement of physicalism). Suddenly, it becomes highly pertinent to perform precise experiments, of the kind performed by Galileo involving, for example, rolling balls down inclined planes, to try to determine what precise

mathematical law governs the fall of bodies near the surface of the earth. Granted, then, that GAOE and AOE have been put into practice over the millennia and right up to the present, science is more rigorous if the metaphysical assumption, implicit in this practice, is made explicit. This is the case even if this explicit thesis remains a conjecture *with no other reasons being given for its acceptance* over and above that it is implicit in scientific practice. No other justification for accepting meta-knowability explicitly in the hierarchy of GAOE and AOE is required (in order to render science more rigorous).[31]

Can anything more be said? I think it can. We can argue that, as a result of accepting meta-knowability, we may have much to gain and little to lose. In accepting meta-knowability we decide, in effect, that it is worthwhile to try to improve knowledge about how to improve knowledge. We take seriously the possibility that the universe is such that we can discover something rather general about its nature which will enable us to *improve* our methods for improving knowledge. Not only do we hope to learn about the world; we hope to learn about how to learn about the world, and we are prepared to implement a meta-methodology (GAOE) which capitalizes on this possibility should it turn out to be actual. To fail to try to *improve* methods for improving knowledge on the grounds that apparent success might prove to be illusory is surely to proceed in a cripplingly over-cautious fashion. Any attempt at improving knowledge may unexpectedly fail, including the attempt to improve methods for improving knowledge. But eschewing the attempt to learn because it may fail cannot be sound: such an excuse for not making the attempt always exists. In accepting meta-knowability we do not assume, note, that the universe *is* such that GAOE will meet with success. We assume, merely, that it is such that *if* GAOE or AOE appears to meet with empirical success, this success will not be illusory in a way which could not have been discovered prior to the illusory character of the success becoming apparent. But this is an entirely sensible assumption to make. Nothing is to be gained from foregoing the attempt to acquire knowledge because of the fear that future, inherently unpredictable changes in the laws of nature may occur which render knowledge acquired obsolete.

Neither partial knowability nor meta-knowability excludes the possibility that such inherently unpredictable events occur. Even though we accept

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31 No circularity is involved here, because no attempt is made to justify acceptance of meta-knowability by an appeal to the success of science. The argument is that we should make meta-knowability explicit even if AOE science is entirely unsuccessful. We should make explicit metaphysical assumptions implicit in our methodological practice whether this practice meets with success or not.

these theses, we might, nevertheless, still discover and accept that unpredictable changes in the laws of nature do occur (if they did occur). We might live, or come to live, in a world in which inherently inexplicable, unpredictable events occur quite often. Objects vanish, or abruptly appear; substances abruptly change their properties; bridges collapse, mountains vanish, houses turn into elephants, trees become daffodils. People die as a result, but life might nevertheless go on, and it might be possible, not just to improve knowledge, but to improve knowledge about how to improve knowledge. Meta-knowability asserts that, if we have had no such experience of them, such events do not occur. We are justified in ignoring the possibility that such events may occur in future in both science and life because, if they occur in the future nothing, in the nature of things, can be done to anticipate their occurrence, or evade the harm they may cause. It is this which provides the grounds for accepting meta-knowability as an item of scientific knowledge.

Hume, famously, argued that what exists at one moment cannot necessarily determine what exists at the next moment. If he is right, we may well feel that anything may happen at any moment – just because there can be nothing in existence now to *determine* (perhaps probabilistically) what will exist next. However, elsewhere I have shown that Hume is wrong, and it *is* possible that what exists at one instant necessarily determines what exists at the next moment (Maxwell, 1968; 1998, pp. 141-155). Since this is possible, it is, in my view, madness not to assume that what exists now *does* necessarily determine what exists next. Recognizing that Hume's arguments, here, are invalid is bound to affect ideas about how likely it is that utterly inexplicable, inherently unpredictable events will occur, as long as we do not seem to have had any experience of them.

Accepting meta-knowability, then, puts on record our decision to try to learn how to learn – to try to improve assumptions and associated methods in the light of improving knowledge and understanding, in the light of which seem best to promote empirical progress. This goes on, after all, in a thoroughly acknowledged and uncontroversial manner at the empirical level. New knowledge can give rise to new technology, new instruments and experimental techniques – from the telescope and microscope to the cyclotron – which are in turn employed to help create new knowledge. At the empirical level, uncontroversially and fruitfully, there is a kind of circular, positive feedback between improving knowledge and improving observational and experimental methods for the further improvement of knowledge. Something analogous has long gone on too, implicitly, in scientific practice, at the theoretical level. Science would be more rigorous,

and even more successful, if this latter was explicitly recognized and acknowledged.

I have argued that we are justified in ignoring the possibility that apparent success achieved as a result of implementing GAOE might turn out to be illusory in a way we could not possibly have discovered. Are we justified, however, in ignoring illusory apparent success of a less fiendish kind – apparent success which we could have discovered to have been illusory, if we had tried harder? Do not GAOE and AOE always carry the danger that they will actively create the illusion of success – metaphysical assumptions and methods being chosen to promote the illusion of success in the pursuit of knowledge?

GAOE and AOE are better equipped to defeat this danger than any other rival methodology for science.

Consider the best that any version of standard empiricism can do to defeat illusory success. First, accepted observational and experimental results can be subjected to sustained critical scrutiny. Experiments can be repeated in different laboratories by different scientists; and essentially the same experiment can be performed in different ways in an attempt to eliminate errors associated with one type of experiment. Second, accepted laws and theories can be severely tested, a variety of consequences being put to the test. Third, rival laws and theories can be developed in order to disclose crucial experiments which may falsify the accepted laws and theories, and which would not otherwise have been thought of: these crucial experiments can then be performed. These three standard empiricist procedures for detecting illusory empirical success are all important.

But AOE science can go further. In addition, it can subject the current best blueprint, and associated methodological principles, to sustained critical scrutiny. It can actively seek to develop improved versions of this blueprint. It can even criticize and develop alternatives to metaphysical theses higher up in the hierarchy, at level 4, and even higher (see figure 11). AOE comes with a framework that facilitates sustained critical scrutiny of current aims and methods, assumptions and methods; it provides meta-methodological machinery for the development of alternative possible aims and methods - alternative vantage points from which any illusory success of current aims and methods may be much more readily detected. Basic blueprint assumptions of a science do much to determine what kind of evidence is acceptable within that science. A change of blueprint may lead to a change in what constitutes acceptable evidence – a point illustrated above in connection with the transition from Aristotle to Galileo. There is always the danger that a science seems to make great empirical success and fails to

discover that this success is illusory because the evidence required to reveal this is declared illegitimate by the accepted blueprint. Thus the demand within physics that experimental result be repeatable prevents physics from discovering miracles – unique, unrepeated events – on empirical grounds. In order to discover the illusory character of such apparent empirical success it may be necessary to view matters from the standpoint of a modified blueprint, with modified standards for what constitutes an acceptable empirical result. AOE encourages the development of such modified blueprints, whereas standard empiricism does not even recognize the need for them. (Any view which specifies a fixed metaphysical assumption for science, on one level, is no better than standard empiricism in the respect just discussed.)

That AOE is better equipped to discover illusory empirical success than rival views provides a decisive rebuttal of the charge that there is an inherently invalid circularity in the manner in which AOE adjusts assumptions and methods in the light of empirical success and failure. On the contrary, AOE science is in a better position to detect such illusory success than science conducted in accordance with any rival view. AOE can modify its aims and methods, its assumptions and methods, in the direction of those which seem to produce the greatest empirical success – thus implementing something like positive feedback (and circularity). At the same time, AOE provides means for discovering when such apparent success is illusory in a way that is better, more effective, than any rival view.

This concludes my discussion of the solution to the circularity problem, and the reasons for accepting meta-knowability in preference to any rival thesis at this level.

Level 5: *Comprehensibility*. The thesis that the universe is comprehensible in some way or other, there being *something*, or an aspect of something (kind of physical entity, God, society of gods, cosmic purpose, cosmic programme or whatever) that runs through all phenomena, and in terms of which all phenomena can, in principle, be explained and understood. Almost all (perhaps all) cultures possess a myth, cosmology or religious view taken to explain natural phenomena, presupposed by attempts to improve knowledge. Almost all of these are personalistic, animistic or purposive in character: natural phenomena are explained in terms of the actions of gods, spirits, God, or purposes. Acceptance of some version of comprehensibility is often combined, however, with a clause that places strict limits on knowability (this clause being required, perhaps, to protect the thesis against criticism, and to explain away the lack of success of the view in promoting acquisition of knowledge). Thus God is said to be mysterious and unknowable. That the

universe is held to be (more or less) comprehensible in almost all cultures is not, however, a good reason to hold it to be worthy of acceptance. Grounds for this stem from the thesis one rung down in the ladder of theses at:-

Level 4: *Physicalism(1,1)*. The thesis that the universe is physically comprehensible, everything being made up of just one kind of physical entity (or perhaps just one entity), all change and diversity being in principle explicable in terms of this one kind of entity. This thesis asserts that the universe is such that some yet-to-be-discovered physical 'theory of everything' (in the current jargon of theoretical physicists) is unified in a type 1 way, and true.

Granted meta-knowability, we are justified in accepting that thesis, other things being equal, which holds out the greatest promise, if true, for progress in empircal knowledge. Physicalism(1,1) satisfies this requirement better than any rival thesis at this level, in that it places more demanding restrictions on any testable theory that is to be ultimately acceptable. (Such a theory must, in principle, predict and explain all physical phenomena, and must be unified in a type 1 way – the most demanding requirement for unity.) Physicalism(1) also indicates a path along which physics may proceed in order to improve empirical knowledge: testable theories need to be put forward and tested that, as far as possible (a) predict ever wider ranges of phenomena, and (b) are ever more unified. In order to develop good new theories, the attempt needs to be made to resolve clashes between existing empirically successful, unified theories. In short, physicalism(1,1), if true, indicates that AOE needs to be put into scientific practice.

But it is not just that physicalism(1,1) holds out the promise of progress; it has been associated, implicitly, with all the great advances in theoretical knowledge and understanding in physics at least since Galileo's time.

All advances in theory in physics since the scientific revolution have been advances in unification, in the sense of (8) to (1) above. Thus Newtonian theory (NT) unifies Galileo's laws of terrestrial motion and Kepler's laws of planetary motion (and much else besides): this is unification in senses (8) to (6). Maxwellian classical electrodynamics, (CEM), unifies electricity, magnetism and light (plus radio, infra red, ultra violet, X and gamma rays): this is unification in sense (5). Special relativity (SR) brings greater unity to CEM, in revealing that the way one divides up the electromagnetic field into the electric and magnetic fields depends on one's reference frame: this is unification in sense (3). SR is also a step towards unifying NT and CEM in that it transforms space and time so as to make CEM satisfy a basic principle fundamental to NT, namely the (restricted) principle of relativity. SR also brings about a unification of matter and energy, via the most famous

equation of modern physics, $E = mc^2$, and partially unifies space and time into Minkowskian space-time. General relativity (GR) unifies space-time and gravitation, in that, according to GR, gravitation is no more than an effect of the curvature of space-time – a step towards unification in sense (1). Quantum theory (QM) and atomic theory unify a mass of phenomena having to do with the structure and properties of matter, and the way matter interacts with light: this is unification in senses (5) and (4). Quantum electrodynamics unifies QM, CEM and SR. Quantum electroweak theory unifies (partially) electromagnetism and the weak force: this is (partial) unification in sense (2). Quantum chromodynamics brings unity to hadron physics (via quarks) and brings unity to the eight kinds of gluons of the strong force: this is unification in sense (3). The standard model (SM) unifies to a considerable extent all known phenomena associated with fundamental particles and the forces between them (apart from gravitation): partial unification in senses (5) to (2). The theory unifies to some extent its two component quantum field theories in that both are locally gauge invariant (the symmetry group being U(1)XSU(2)XSU(3)). All the current programmes to unify SM and GR known to me, including string theory or M-theory, seek to unify in senses (5) to (1).[32]

In short, all advances in fundamental theory since Galileo have invariably brought greater unity to theoretical physics in one or other, or all, of senses (8) to (1): all successive theories have increasingly successfully exemplified and given precision to physicalism(1,1) to an extent which cannot be said of any rival metaphysical thesis, at that level of generality. The whole way theoretical physics has developed points towards physicalism(1,1), in other words, as the goal towards which physics has developed. Furthermore, what it means to say this is given precision by the account of theoretical unity given in section 3 above.

In response to this claim it may be objected that theoretical physics could equally well be regarded as pointing towards a less restrictive version of physicalism – one which does not require matter and space-time to be unified, or one which demands only that the true theory everything is no more disunified than in a type 4 way to an extent N = 3, let us say (so that the true theory postulates three kinds of forces). What grounds are there for preferring physicalism(1,1) to physicalism(4,3), let us say? There are at least four, none of course decisive.

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[32] For further discussion see (Maxwell 1998, 80-89, 131-40, 257-65 and additional works referred to therein).

Fundamental to the whole argument for AOE is that physics needs to put the Principle of Intellectual Integrity into practice (and I have claimed that AOE can be construed as the outcome of successive applications of this principle). In considering what thesis ought to be accepted at level 4, then, we need to consider what is implicit in those current methods of physics that influence what theories are to be accepted on non-empirical grounds – having to do with simplicity, unity, explanatoriness. There can be no doubt that, as far as non-empirical considerations are concerned, the more nearly a new fundamental physical theory satisfies all eight of the above requirements for unity, with N = 1, the more acceptable it will be deemed to be. Furthermore, failure of a theory to satisfy elements of these criteria is taken to be grounds for holding the theory to be false even in the absence of empirical difficulties. For example, high energy physics in the 1960s kept discovering more and more different hadrons, and was judged to be in a state of crisis as the number rose to over one hundred. Again, even though the standard model (the current quantum field theory of fundamental particles and forces) does not face serious empirical problems, it is nevertheless regarded by most physicists as unlikely to be correct just because of its serious lack of unity. In adopting such non-empirical criteria for acceptability, physicists thereby implicitly assume that the best conjecture as to where the truth lies is in the direction of physicalism(1,1). The Principle of Intellectual Integrity requires that this implicit assumption – or conjecture – be made explicit so that it can be critically assessed and, we may hope, improved. Physics with physicalism(1,1) explicitly acknowledged as a part of conjectural knowledge is more rigorous than physics without this being acknowledged because physics pursued in the former way is able to subject non-empirical methods to critical appraisal as physicalism(1,1) is critically appraised, whereas physics pursued in the latter way cannot do this. Because physicalism(1,1) makes more definite, substantial claims than any rival version of physicalism, it is more open to critical appraisal than rival versions.

A second point to note is that it may well be that, even if some other version of physicalism(n,N) is true, with $n > 1$ and $N > 1$, nevertheless our best hope of discovering the truth may still lie in attempting to discover a theory that exemplifies physicalism(1,1), and failing in the attempt. As N becomes bigger, so the number of possible theories of everything compatible with that version of physicalism rapidly increases. (If N = 2, and the universe is made up of two distinct unified, dynamical patterns, there are, nevertheless, in general, infinitely many ways in which these two distinct patterns can be fitted together to make infinitely many different possible universes exemplifying just these two dynamic patterns. The step from one specified

unified pattern to two is the step from one possible universe to infinitely many!) It makes sense to seek the simplest, most discoverable possibility, and design our methodology accordingly. As I mentioned at the beginning of this chapter, one can imagine a universe in which we might have reasons for adopting a methodological rule different from: (A) in order to be ultimately acceptable, a theory must be comprehensive and unified in a type (1) way. An example is: (B) in order to discover the true theory of everything, there need to be infinitely many theoretical revolutions, the number of forces increasing by one at each revolution. We cannot, therefore, just argue that, even if some version of physicalism other than physicalism(1,1) is true, nevertheless our best hope of discovering the truth is to adopt (A), try to discover a theory that exemplifies physicalism(1,1), and fail in the attempt. But we can argue that, in our current state of ignorance, our best bet is to adopt (A), and revise our acceptance of physicalism(1,1) if some other version of physicalism should emerge as appearing to fit the progress of physics better. (A number of revolutions have taken place, and each time, the number of forces has gone up by one.)

There is another reason for preferring physicalism(1,1) to any other version, namely: only this version can do justice to the way general relativity unifies gravitation and space-time. This is a step towards type (1) unification in that, according to the theory, gravitation as a force disappears, and we are left with a dynamic theory of space-time. (Matter, or energy-density more generally, tells space-time how to curve: bodies then move along geodesics – the nearest things to straight lines in curved space-time.) It is above all general relativity which holds out the possibility that, not just gravitation, but all the forces and particles may be unified with space-time.

In short, physicalism(1,1) seems to be the best bet when one takes into account (a) its inherent promise of progress, (b) the manner in which it is exemplified in every accepted new fundamental theory in physics, (c) its greater fruitfulness for progress even if some other version of physicalism is true, and (d) the way in which it is suggested by general relativity.

Finally, it needs to be remembered that what we are discussing is reasons for accepting physicalism(1,1) at level 4 within the context of AOE. If physicalism(1,1) was a candidate for the only metaphysical thesis to be accepted by science, it might well be thought to be much too specific and risky to be regarded as a part of scientific knowledge. But the whole point of AOE is that, as we descend the hierarchy, theses become increasingly specific, risky, tentative, and likely to require rejection, or at least revision. Physicalism(1,1) is bound to have a much more dubious epistemological status than partial knowability, let us say.

This concludes my discussion of reasons for accepting physicalism(1,1) at level [4].

Level 3: Best Blueprint. The best available more or less specific metaphysical view as to how the universe is physically comprehensible, a view which asserts that everything is composed of some more or less specific kind of physical entity, all change and diversity being, in principle, explicable in terms of this kind of entity. As I have already mentioned, examples, taken from the history of physics include: the corpuscular hypothesis of the 17th century, according to which the universe consists of minute, infinitely rigid corpuscles that interact only by contact; the view, associated with Newton and Boscovich, according to which the universe consists of point-atoms that possess mass and interact at a distance by means of rigid, spherically symmetrical, centrally directed forces; the unified field view, associated with Faraday and Einstein, according to which everything is made up of one self-interacting field, particles of matter being especially intense regions of the field. Some might argue that the best available blueprint available today is the basic metaphysical idea of superstring theory, or M-theory as it is now called: the universe consists of minute quantum strings that move in 10 or 11 dimensions of space-time, all but four of which are curled up into a minute size, thus escaping detection. In Maxwell (1998, chapter 3) I argue, however, that the best available blueprint is a somewhat more general thesis that I call Lagrangianism. What one requires, of course, is a metaphysical idea which unifies key ideas taken from quantum theory and general relativity. My suggestion, along these lines, is probabilistic dynamic geometry of space-time (Maxwell, 1985a, pp. 40-41; 2006b, pp.240-1).

Level 2: Accepted fundamental Physical Theory. All accepted fundamental dynamical theories, or accepted laws governing the way physical phenomena occur if no dynamical theory has been developed that applies to the phenomena in question. In terms of current scientific knowledge, this level consists of the so-called standard model (SM) – the quantum field theory of fundamental particles and the forces between them – plus general relativity (GR). We are justified in accepting these theories because, better than any available rivals they satisfy the two requirements of (a) empirical success, and (b) unity, as explicated above, thus exemplifying (in the best available way) the best level 4 thesis – physicalism(1,1)[.33]

[33] Some theories clash with physicalism(1,1) more severely than others. What, it may be asked, can this mean? The account of theoretical unity, given above, provides the answer. Given two rival sets of fundamental physical theories, T_1 and T_2, each set aspiring to be comprehensive, T_1 clashes more severely with physicalism(1,1) than T_2 if T_1 is disunified

This concludes my discussion of reasons for preferring theses at levels 7 to 2 of the version of AOE depicted in figure 12.

It may, perhaps, be conceded that AOE solves the methodological problem of induction. And it might just about be conceded that the above discussion solves the theoretical problem of induction. But none of this, it may be objected, goes any way at all towards solving the practical problem of induction. The reason is very simple. None of the arguments given above for accepting theses at the various levels of AOE provide any grounds whatsoever for believing these theses are likely to be true. They are only reasons for accepting these theses granted that our aim is to improve knowledge, insofar as this is possible. What counts, in other words, is the fruitfulness of these theses from the standpoint of improving theoretical knowledge. But when it comes to action, what we require is reasons for holding relevant factual propositions are true. Conjectures, speculations, are not good enough when it comes to risking our own lives, or the lives of others.

I have three points to make in support of the claim that the above does succeed in solving the practical problem (insofar as it is capable of being solved).

The first is this. Accept AOE, accept that the level 4 thesis of physicalism is a part of our knowledge, and a sharp distinction can be drawn between certainty and speculation – a distinction that eludes Popper's account of the matter. Briefly, and roughly, factual propositions which are sufficiently well corroborated and sufficiently in accord with physicalism fall into the category of trustworthy knowledge; all other factual propositions that have not been falsified fall into the category of mere speculation. This, I claim, reflects the way we actually demarcate trustworthy knowledge from mere speculation. To take an example considered by John Worrall (1989), we do not jump off the top of the Eiffel tower, entrusting our life to the truth of the conjecture that we will float gently down to the ground because this conjecture fails to satisfy the two requirements for trustworthy knowledge. It is no doubt possible to concoct a theory that is more acceptable, according to the methodology of Popper (1959), than Newton's or Einstein's theory of gravitation – an *ad hoc* theory concocted to have greater empirical content and success than either – but such a theory would clash severely with physicalism. This demarcates trustworthy knowledge from speculation, but does not provide a justification

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in a more serious way than T_2. (Theories become increasingly seriously disunified as n goes from 1 to 8.) If T_1 and T_2 are disunified in the same kind of way, then T_1 clashes more severely with physicalism(1,1) than T_2 if T_1 has a greater degree of disunity than T_2.

for the distinction. For that, some kind of justification of physicalism is required. Is any forthcoming?

This leads me to my second point. Even our most humdrum, particular, factual items of knowledge about our immediate circumstances, presupposed by our ordinary actions in life, have a cosmological dimension, as we have seen. Cosmological assumptions, or conjectures, are an inevitable part of almost all that we take to be factual knowledge, whether commonsensical or scientific. The crucial question, in the context of practical life, is: which cosmological conjecture, of those available, is to be preferred? The only guideline we have available as to which is most likely to be true is: Which seems best to promote acquisition of empirical knowledge? The answer, as we have seen, is physicalism(1,1).

We have before us, let us suppose, a number of candidate cosmological theses: physicalism(1,1) and theses A, B, C,... (which might include the Aristotelian thesis that everything is to be explained in terms of some overall cosmological purpose, the thesis that natural phenomena exemplify a cosmological computer programme, and the thesis that phenomena occur as a result of the will of God). How should we choose? (We assume the theses are all consistent, and viable cosmological theses in that each can apparently accommodate everything that exists.) One consideration, clearly, is to see which is implicit in our everyday actions, and is presupposed by that part of what we take to be knowledge upon which we base our actions. Let us suppose all the candidates pass this test. The only remaining relevant consideration is: Which thesis holds out the greatest hope of empirical progress, if true, and is actually associated with what seems to be progress in empirical knowledge? An untestable, metaphysical thesis that holds out the promise of progress in empirical knowledge, if true, has a kind of quasi-testable status. If it is adopted as the blueprint of an actively pursued research programme, and this programme, even after decades or centuries of endeavour, makes no substantial progress, this tells against the blueprint. But if, on the other hand, the research programme seems to make rapid, even ever accelerating progress, this tells for that blueprint. What better indication could we have of the truth of the blueprint than that assuming it to be true is uniquely fruitful for the acquisition of knowledge? Given this way of assessing cosmological theses, the grounds for preferring physicalism(1,1) to all other candidates are overwhelming.

But the above argument has, of course, a built in circularity (which no doubt explains why philosophers ignore it). It is perfectly possible, in other words, for natural science to appear to achieve spectacular progress in empirical knowledge – this success being uniquely associated with science

presupposing physicalism(1,1) – and yet for physicalism(1,1) to be grossly false. The success might be illusory, either in a way which could in principle be discovered, or in a way which could not, even in principle, until some specific time in the future (when 'the laws of nature abruptly change').

This circularity problem was solved above. If apparent scientific progress is illusory in a discoverable way, well, AOE is uniquely equipped to discover it. The circularity feature of AOE (as far as discoverable illusory success is concerned) provides no grounds whatsoever for not implementing AOE, and accepting the results of AOE science as a basis for action, when these results are sufficiently well corroborated empirically. If, on the other hand, scientific progress is illusory in a way which is not discoverable (until all is revealed), then nothing can be done to guard us against such possible future disasters. Not just AOE science, but any methodology, any procedure or way of life, must be vulnerable to such undiscoverable illusory success. That AOE is vulnerable to it, and cannot guard against it, is thus no reason whatsoever for not accepting, as a basis for action, the well-established results of AOE science. Since nothing can anticipate, and protect us from, such unanticipatable disaster, it's foolish to blame AOE for being unable to anticipate, and protect us from, such disaster. There is here no reason not to accept well-established results of AOE science as a basis for action.

My third and final point is this. Before the scientific revolution, there was much more general awareness, than there is today, that what may be called cosmological circumstances could impact, in perhaps drastic and dreadful ways, on the ordinary circumstances of life. Evil spirits might cast spells and bring catastrophe, even death; comets might bring disaster; the gods might send drought, locusts, storm, the plague, and might even destroy the world. Then came science, and with it the assurance that the natural world is governed by impersonal, utterly reliable physical law. This, it seemed, had been securely established by Newtonian science. Had not Newton himself demonstrated how physical laws can be verified by induction from phenomena|? There remained the niggling philosophical puzzle as to how it is possible to verify laws by means of induction, but this irritating puzzle of induction is best left to philosophers to waste their time on.

This rather common attitude – common at least until recently (scepticism about science having recently become much more widespread) – rests on an illusion. Newton did not establish his law of gravitation by induction from the phenomena, as he claimed to have done. He could not have done this, because it cannot be done.

As it happens, Newton himself anticipated a basic feature of AOE. He recognized explicitly that scientific method makes presuppositions about

nature. Three of his four rules of reason, concerned with simplicity, quite explicitly make assumptions about the nature of the universe. Thus rule 1 asserts: 'We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances.' And Newton adds: 'To this purpose the philosophers say that nature does nothing in vain, and more is in vain when less will serve; for Nature is pleased with simplicity, and affects not the pomp of superfluous causes' (Newton, 1962, p. 398). Newton understood that persistently preferring simple theories means that Nature herself is being persistently assumed to be simple.

But this aspect of Newton's thought came to be overlooked. The immense, unprecedented success of natural science after Newton was taken to demonstrate that humanity had somehow discovered the secret of wresting truth and certainty from nature, and only the incompetence of philosophers prevented everyone from knowing exactly what this secret amounted to. Even today there are philosophers who think that the problem of induction will only be solved when this secret of how scientists manage to capture truth and certainty is laid bare for everyone to see and understand.

But this is an illusion. Even our most humdrum, particular, practical knowledge of aspects of our immediate environment, as we have seen, let alone the mighty claims to knowledge of science, contains a cosmological element which must remain conjectural. Modern science has, it seems, made a profound discovery about the ultimate nature of the cosmos, namely that it is physically comprehensible. Once AOE is accepted, it becomes clear that this thesis, despite its metaphysical and cosmological character, is one of the most firmly established theoretical propositions of science (in that physical theories, in order to be accepted, must accord with this proposition as far as possible, and theories which clash with it too stridently are not even considered, even though they would be much more empirically successful than accepted theories if considered). Given this cosmological thesis that the universe is physically comprehensible, the way we in practice distinguish trustworthy knowledge from mere speculation becomes clear. Nevertheless, despite its central place and role in science, the thesis remains inherently conjectural in character. Practical certainty has this usually unacknowledged conjectural and cosmological dimension inherent in it.

As it is, our attitude towards the thesis that the universe is physically comprehensible is highly hypocritical. The fundamental role that it plays in science, in technology, in our whole culture and way of life, is denied. Non-scientists deny it because they do not want to confront the grim implications the thesis has for the meaning and value of human life – the difficulty of seeing how there can be consciousness, freedom, meaning and value if the

universe really is physically comprehensible.[34] Scientists deny it, because they do not want to acknowledge that there is an element of faith in science. They confidently distinguish science from religion on the grounds that, whereas religion appeals to dogma and faith, in science there is no faith and everything is assessed impartially with respect to evidence. But this, as we have seen, is nonsense. There is an element of faith in science too. The real difference between science and religion – most dogmatic religions that is – is that whereas science subjects its articles of faith to sustained critical scrutiny, modifying them in the direction of that which seems most fruitful from the standpoint of the growth of knowledge, dogmatic religion does nothing of the kind. We are justified in accepting physicalism as a part of our knowledge, even in the context of practical action, because some such cosmological conjecture must be accepted, and physicalism has proved more fruitful for progress in knowledge than any rival. It is always possible that this success is illusory, and physicalism is no more than a kind of scientific hallucination. But if the success of science is illusory in a way we could not in principle discover, then this is a possibility we face whatever we assume; it is not something we can do anything about, and deserves to be ignored. If, on the other hand, the success of science is illusory in a way which can in principle be discovered, then AOE science provides us with the best means of unmasking the illusion. Either way, physicalism deserves to be accepted even in practical contexts.

A more honest recognition of the presence of cosmological conjectures inherent in science, and inherent even in our most humble items of practical knowledge would involve recognizing that all our knowledge is indeed conjectural in character without, thereby, destroying the distinction we make between practical certainty and speculation.

Popper has done much to create an awareness of the conjectural character of scientific knowledge – helped, of course, by the dethronement of Newtonian science with the advent of general relativity and quantum theory. But in one crucial respect, Popper helped sustain the Newtonian tradition, the status quo. He fiercely defended, to the last, the highly traditional, and mistaken, idea that the scientific character of science depends on it being dissociated from metaphysics.[35] Actually, it is all the other way round. If science is to be rigorous, it is essential that it acknowledge – and so throw

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[34] Elsewhere I have sought to show how consciousness, free will, the experiential world, meaning and value can exist even though the universe is physically comprehensible: see chapter 10 of the present work and Maxwell (1966; 1968; and especially 2001).

[35] For a discussion of this defect in Popper's work, see (Maxwell, 2005b and 2007a).

open to criticism and improvement – metaphysical and cosmological theses implicit in the persistent scientific selection of unified, explanatory theories. And that is just the start of one line of argument leading to the philosophy of wisdom: not just metaphysics, but values, and political committments too, implicit in the scientific endeavour, need to be made explicit, if science is to be rigorous, so that these problematic assumptions and committments can be criticized and, we may hope, improved.

For further arguments intended to show that AOE solves all three parts of the problem of induction, insofar as they can be solved, see Maxwell (1998, ch. 5; 2004b, appendix, section 6; and 2005e).

Appendix

In this appendix I say, in a little more detail, what it means to say that T_2 is closer to the truth than T_1 in the sense that it makes more precise predictions of more phenomena.

In order to do this, we need to consider, as before, the following paradigmatic kind of prediction that a dynamical physical theory makes. We have the physical theory, T_1 let us say, and any isolated physical system or physical state of affairs, S, which is such that T_1 predicts how S evolves in time. Let the specification of the initial state of the system at time t_1, formulated in terminology appropriate to T_1, be S^1_1, and let the specification of some later state of the system, at time t_2, be S^1_2. T_1 might be Newtonian theory, S^1_1 might be a specification of the state of the solar system at some moment t_1, S^1_1 specifying the instantaneous positions, velocities and masses of the sun and planets at time t_1, and S^1_2 specifying the state of the solar system, as predicted by Newtonian theory, at a later time t_2. We have that T_1 and S^1_1, taken together, imply S^1_2; i.e. $(T_1 \,\&\, S^1_1) \rightarrow S^1_2$. Newtonian theory plus a specification of the instantaneous state of the solar system implies specifications of future states – future positions and velocities of the sun and planets.[36]

[36] This simplifies what is, in scientific practice, much more complicated. The way initial and boundary conditions need to be formulated changes from one theory to another, as one moves from Newtonian theory to a field theory, such as classical electrodynamics, to general relativity and quantum theory. These complications are not relevant to the problem under discussion, and can be ignored. Orthodox quantum theory, lacking its own ontology, must appeal to a process of preparation, and cannot specify an initial state in purely quantum mechanical terms, but in this respect the theory is unsatisfactory: see Maxwell (1998, ch. 7).

In the case of a false theory, such as T_1, T_2 or Newtonian theory, the theory, T_1, and the corresponding specifications of state, S^1_1 and S^1_2, are all false. To take the example of Newtonian theory, it is not just that the theory is false, but the Newtonian specifications of the instantaneous states of the system will be false as well. All these Newtonian propositions presuppose that space is Euclidean, for example, but general relativity tells us that space is not Euclidean, and the true theory of everything, T, is likely to tell us this too. T may depart even more radically from the presuppositions of these Newtonian propositions in that it asserts that space, and perhaps time, are discontinuous, whereas Newtonian theory presupposes that both are continuous. Again, the Newtonian propositions assume that physical systems, such as the sun and planets of the solar system, are made up of classical particles with mass, and with definite positions and velocities at successive moments. But this is denied by quantum theory, and is likely to be denied, if anything, even more emphatically, by the true theory of everything, T.

Despite the presumed falsity of T_1, S^1_1 and S^1_2, T_1 can still issue in true predictions. S^1_2 can be specified in a looser, approximate fashion, S^1_2* say, so that S^1_2* asserts something like 'the state of S is S^1_2 within such and such a range of values of variables (such as relative positions and velocities)'. In the case of Newtonian theory, S^{NT}_2* tells us, not precisely where each planet is and what its precise velocity is, but rather specifies a volume of space within which such and such a planet is located, having a velocity of such and such a range of values. We stipulate that S^1_2* is concocted so that (a) $S^1_2 \rightarrow S^1_2$*, and (b) S^1_2* is compatible with T. (It may be asked: How is it possible for the false proposition S^1_2 to imply the looser, approximate *true* proposition, S^1_2*? A trivial example of this is the following statement, uttered on Wednesday: "'Today is Monday' implies 'Today is a weekday'".[37])

We may stipulate that S^1_2* is as precise as it can be without being false. We don't have to assume that there is just one true approximate specification of S at time t_2. Given one such specification, S^1_2*, it seems reasonable to suppose that others could be generated by making the range of values of one variable, specifying the state of S approximately, a little bit more precise, as long as the range of values of another variable is made, compensatingly, less precise. There may be infinitely many such different, true, minimally approximate specifications of the state of S at time t_2, corresponding to the

[37] A better example, avoiding context, would be 'This sphere is an ellipsoid', said of an ellipsoid that is not a sphere. (A sphere is a special case of an ellipsoid, so that all spheres are ellipsoids, but not all ellipsoids are spheres.)

precise, false specification $S^1{}_2$. (We require that there is no true $S^1{}_2$** such that $S^1{}_2* \rightarrow S^1{}_2$** but not $S^1{}_2$** $\rightarrow S^1{}_2*$.)

Just as there is a true minimally approximate specification of S at time t_2, namely $S^1{}_2*$, so too we can stipulate that there is a true, minimally approximate specification of the state of S at time t_1, namely $S^1{}_1*$. And, as before, there may be infinitely many such true approximate specifications.

We have, then, that $(T_1 \& S^1{}_1) \rightarrow S^1{}_2 \rightarrow S^1{}_2*$.

Everything stipulated about T_1, $S^1{}_1$, $S^1{}_1*$, $S^1{}_2$ and $S^1{}_2*$ is also stipulated to hold for T_2, $S^2{}_1$, etc. In the case of the true theory of everything, T, there are the two true precise specifications of the state of S at time t_1 and t_2, namely $S^T{}_1$ and $S^T{}_2$.

Now a few remarks about the relations between T_1, T_2 and T. Corresponding to any given pair, $S^1{}_1*$ and $S^1{}_2*$ there will be, we may presume, infinitely many different, true, precise specifications of possible states of systems, $S^T{}_1$ and $S^T{}_2$. Newtonian theory, applied to the solar system, need not take into account the positions of all the constituent atoms, or fundamental particles, of which the planets are composed. Infinitely many re-arrangements of atoms will not affect the way the solar system evolves, as predicted by Newtonian theory. Because the specifications of states of systems, formulated in terms of T, are so much more precise than specifications formulated in terms of T_1 or T_2, infinitely many different specifications of the former, will correspond to the same specification formulated in terms of T_1 or T_2. We may presume that, likewise, infinitely many different true approximate specifications of states formulated in terms of T_2 correspond to just one pair, $S^1{}_1*$ and $S^1{}_2*$. [38]

With these preliminaries over, we can now state the conditions that must be satisfied for T_2 to be closer to the truth than T_1. This will be the case if:-

(a) Given any predictive task such that both T_1 and T_2 make true approximate predictions, the prediction of T_2 is more accurate than that of T_1. That is, given that the two corresponding predictions are $(T_1 + S^1{}_1*) \rightarrow S^1{}_2*$ and $(T_2 + S^2{}_1*) \rightarrow S^2{}_2*$, then $S^2{}_1*$ is at least as precise as $S^1{}_1*$ and $S^2{}_2*$ is more precise than $S^1{}_2*$. (That is, $S^2{}_2* \rightarrow S^1{}_2*$ but not $S^1{}_2* \rightarrow S^2{}_2*$, and if $S^1{}_1* \rightarrow S^2{}_1*$ then $S^2{}_1* \rightarrow S^1{}_1*$.)

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[38] It is just possible that, for some range of phenomena to which T_1 applies, the theory is so badly false that it is difficult to see what true approximate predictive statements it implies. If this is the case, then this range of phenomena must be ignored, and only that range considered for which T_1 does yield true approximate predictions.

(b) There are many (presumably infinitely many) true approximate predictions of T_2 (implied by T_2 and appropriate specifications of initial conditions, statements of type S^2_1), to which there correspond no such predictions of T_1 (but not *vice versa*).

In other words, T_2 is closer to the truth than T_1 if (a) everything true that T_1 predicts, T_2 can predict with greater accuracy, and (b) T_2 makes true predictions about which T_1 is silent or can predict nothing true whatsoever.

Not only are the true approximate predictions of T_2 more precise than those of T_1; the true approximate specifications of initial conditions of T_2 are at least as accurate as those of T_1.

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Index

"Maxwell's theory of aim-oriented empiricism is the outstanding work on scientific change since Lakatos, and his thesis is surely correct. Scientific growth should be rationally directed through the discussion, choice, and modification of aim-incorporating blueprints rather than left to haphazard competition among research traditions seeking empirical success alone. . . Of the theories of scientific change and rationality that I know, Maxwell's is my first choice. It is broad in scope, closely and powerfully argued, and is in keeping with the purpose of this book, which is to see science in its totality. No other theory provides, as Maxwell's does in principle, for the rational direction of the overall growth of science."
Professor George F. Kneller, ***Science as a Human Endeavor***

"As Nicholas Maxwell has suggested, if we make one crucial assumption about the purpose of science, then the possibility arises that some paradigms and theories can be evaluated even prior to the examination of their substantive products. This one crucial assumption is that the overall aim of science is to discover the maximum amount of order inherent in the universe or in any field of inquiry. Maxwell calls this 'aim-oriented empiricism'. . . I agree with Maxwell's evaluation of the importance of coherent aim-oriented paradigms as a criterion of science. . . The time is ripe, therefore, to replace the incoherent and unconscious paradigms under whose auspices most anthropologists conduct their research with explicit descriptions of basic objectives, rules, and assumptions. That is why I have written this book."
Professor Marvin Harris, ***Cultural Materialism***

First Edition of From Knowledge to Wisdom

"Nicholas Maxwell (1984) defines freedom as 'the capacity to achieve what is of value in a range of circumstances'. I think this is about as good a short definition of freedom as could be. In particular, it appropriately leaves wide open the question of just what is of value. Our unique ability to reconsider our deepest convictions about what makes life worth living obliges us to take seriously the discovery that there is no palpable constraint on what we can consider."
Professor Daniel Dennett, ***Freedom Evolving***

'In this book, Nicholas Maxwell argues powerfully for an intellectual "revolution" transforming all branches of science and technology. Unlike such revolutions as those described by Thomas Kuhn, which affect

knowledge about some aspect of the physical world, Maxwell's revolution involves radical changes in the aims, methods, and products of scientific inquiry, changes that will give priority to the personal and social problems that people face in their efforts to achieve what is valuable and desirable.'
George Kneller, ***Canadian Journal of Education***

The Comprehensibility of the Universe: A New Conception of Science

"Maxwell performs a heroic feat in making the physics accessible to the non-physicist ... Philosophically, there is much here to stimulate and provoke . . . there are rewarding comparisons to be made between the functional roles assigned to Maxwell's metaphysical "blueprints" and Thomas Kuhn's paradigms, as well as between Maxwell's description of theoretical development and Imre Lakatos's methodology of scientific research programmes."
Dr.Anjan Chakravartty*, Times Higher Education Supplement*

"Maxwell ... has shown that it is absurd to believe that science can proceed without some basic assumptions about the comprehensibility of the universe . . . Throughout this book, Maxwell has meticulously argued for the superiority of his view by providing detailed examples from the history of physics and mathematics . . . *The Comprehensibility of the Universe* attempts to resurrect an ideal of modern philosophy: to make rational sense of science by offering a philosophical program for improving our knowledge and understanding of the universe. It is a consistent plea for articulating the metaphysical presuppositions of modern science and offers a cure for the theoretical schizophrenia resulting from acceptance of incoherent principles at the base of scientific theory." **Professor Leemon McHenry**, ***Mind***

"This admirably ambitious book contains more thought-provoking material than can even be mentioned here. Maxwell's treatment of the descriptive problem of simplicity, and his novel proposals about quantum mechanics deserve special note. In his view the simplicity of a theory is (and should be) judged by the degree to which it exemplifies the current blueprint of physicalism, that blueprint determining the terminology in which the theory and its rivals should be compared. This means that the simplicity of a theory amounts to the unity of its ontology, a view that allows Maxwell to offer an explanation of our conflicting intuitions that terminology matters to simplicity, and that it is utterly irrelevant. Maxwell's distinctive views about what is wrong with quantum mechanics grow out of his adherence to aim-

oriented empiricism: the much-discussed problem of measurement is for him a superficial consequence of the deeper problem that the ontology of the theory is not unified, in that no one understands how one entity could be both a wave and a particle. In response to this problem Maxwell finds between the metaphysical cracks a way to fuse micro-realism and probabilism, which leads him to a proposal to solve the measurement problem by supplementing quantum mechanics with a collapse theory distinct from the recent and popular one of Ghirardi, Rimini and Weber. Maxwell's highly informed discussions of the changing ontologies of various modern physical theories are enjoyable, and the physical and mathematical appendix of the book should be a great help to the beginner."
Professor Sherrilyn Roush, ***The Philosophical Review***

"Nicholas Maxwell has struck an excellent balance between science and philosophy . . . The detailed discussions of theoretical unification in physics - from Newton, Maxwell and Einstein to Feynman, Weinberg and Salam - form some of the best material in the book. Maxwell is good at explaining physics . . . Through the interplay of metaphysical assumptions, at varying distances from the empirical evidence Maxwell shows, rather convincingly, that in the pursuit of rational science the inference from the evidence to a small number of acceptable theories, out of the pool of rival ones, is justifiable . . . Its greatest virtue is the detailed programme for a modern version of natural philosophy. Along the way, Maxwell homes in on the notion of comprehensibility by the exclusion of less attractive alternatives. In an age of excessive specialization the book offers a timely reminder of the close link between science and philosophy. There is a beautiful balance between concrete science and abstract philosophy . . . In the "excellently written Appendix some of the basic mathematical technicalities, including the principles of quantum mechanics, are very well explained . . . Einstein held that 'epistemology without science becomes an empty scheme' while 'science without epistemology is primitive and muddled'. Maxwell's new book is a long-running commentary on this aphorism."
Dr. Friedel Weinert, ***Philosophy***

"some of [Maxwell's] insights are of everlasting importance to the philosophy of science, the fact that he stands on the shoulders of giants (Hume, Popper) notwithstanding . . . My overall conclusion is that *Universe* is an ideal book for a reading group in philosophy of science or in philosophy of physics. Many of the pressing problems of the philosophy of science are discussed in a lively manner, controversial solutions are passionately defended and some

new insights are provided; in particular the chapter on simplicity in physics deserves to be read by all philosophers of physics."
Dr. F. A. Muller, ***Studies in History and Philosophy of Modern Physics***

"In *The Comprehensibility of the Universe*, Nicholas Maxwell develops a bold, new conception of the relationship between philosophy and science…Maxwell has a metaphysically rich, evolutionary vision of the self-correcting nature of science…The work is important…An added benefit of Maxwell's analysis…is the possibility of a positive, fruitful relationship to emerge between science and the philosophy of science…his important and timely critique of the reigning empiricist orthodoxy…what does it mean to say simplicity is a theoretical virtue? And why should we prefer simple to complex theories? Maxwell provides an admirable discussion of these issues. He also provides a useful discussion of simplicity in the context of theory unification – simple theories are unifying theories – and illustrates his points with examples drawn from Newtonian physics and Maxwellian electrodynamics…It is hard to do justice to the richness of Maxwell's discussion in this chapter. I can only say that this is a chapter that will repay serious study…Maxwell turns his attention to issues surrounding the theoretical character of evidence, the idea of scientific progress and the question as to whether there is a method of discovery….The discussion of these matters – as with the other topics covered in this book – is conceptually rich and technically sophisticated. A useful antidote, in fact, to the settled orthodoxy surrounding these philosophical issues…Maxwell has written a book that aims to put the metaphysics back in physics. It is ambitious in scope, well-argued, and deserves to be seriously studied."
Professor Niall Shanks, ***Metascience***

The Human World in the Physical Universe: Consciousness, Free Will and Evolution

"Ambitious and carefully-argued...I strongly recommend this book. It presents a version of compatibilism that attempts to do real justice to common sense ideas of free will, value, and meaning, and...it deals with many aspects of the most fundamental problems of existence."
Dr. David Hodgson, ***Journal of Consciousness Studies***

"Maxwell has not only succeeded in bringing together the various different subjects that make up the human world/physical universe problem in a single

volume, he has done so in a comprehensive, lucid and, above all, readable way." **Dr. M. Iredale**, ***Trends in Cognitive Sciences***

"...a bald summary of this interesting and passionately-argued book does insufficient justice to the subtlety of many of the detailed arguments it contains." **Professor Bernard Harrison**, ***Mind***

"Nicholas Maxwell takes on the ambitious project of explaining, both epistemologically and metaphysically, the physical universe and human existence within it. His vision is appealing; he unites the physical and the personal by means of the concepts of aim and value, which he sees as the keys to explaining traditional physical puzzles. Given the current popularity of theories of goal-oriented dynamical systems in biology and cognitive science, this approach is timely. . . The most admirable aspect of this book is the willingness to confront every important aspect of human existence in the physical universe, and the recognition that in a complete explanation, all these aspects must be covered. Maxwell lays out the whole field, and thus provides a valuable map of the problem space that any philosopher must understand in order to resolve it in whole or in part."
Professor Natika Newton, ***Philosophical Psychology***

"This is a very complex and rich book. Maxwell convincingly explains why we should and how we can overcome the 'unnatural' segregation of science and philosophy that is the legacy of analytic philosophy. His critique of standard empiricism and defence of aim-oriented empiricism are especially stimulating"
Professor Thomas Bittner, ***Philosophical Books***

"I recommend reading *The Human World in the Physical Universe* . . . for a number of reasons. First, [it] … provides the best entrance to Maxwell's world of thought. Secondly, [it] contains a succinct but certainly not too-detailed overview of the various problems and positions in the currently flourishing philosophy of mind. Thirdly, it shows that despite the fact that many philosophers have declared Cartesian Dualism dead time and again, with some adjustments, the Cartesian view remains powerful and can compete effortlessly with other extant views"
Dr. F. A. Muller, ***Studies in History and Philosophy of Modern Physics***

"Some philosophers like neat arguments that address small questions comprehensively. Maxwell's book is not for them. *The Human World in the Physical Universe* instead addresses big problems with broad brushstrokes."
Dr. Rachel Cooper, ***Metascience***

"A solid work of original thinking."
Professor L. McHenry, ***Choice***

Is Science Neurotic?

"This book is bursting with intellectual energy and ambition...[It] provides a good account of issues needing debate. In accessible language, Maxwell articulates many of today's key scientific and social issues...his methodical analysis of topics such as induction and unity, his historical perspective on the Enlightenment, his opinions on string theory and his identification of the most important problems of living are absorbing and insightful."
Clare McNiven, ***Journal of Consciousness Studies***

"Is science neurotic? Yes, says Nicholas Maxwell, and the sooner we acknowledge it and understand the reasons why, the better it will be for academic inquiry generally and, indeed, for the whole of humankind. This is a bold claim … But it is also realistic and deserves to be taken very seriously … My summary in no way does justice to the strength and detail of Maxwell's well crafted arguments … I found the book fascinating, stimulating and convincing … after reading this book, I have come to see the profound importance of its central message."
Dr. Mathew Iredale, ***The Philosopher's Magazine***

"… the title *Is Science Neurotic?* could be rewritten to read *Is Academe Neurotic?* since this book goes far beyond the science wars to condemn, in large, sweeping gestures, all of modern academic inquiry. The sweeping gestures are refreshing and exciting to read in the current climate of specialised, technical, philosophical writing. Stylistically, Maxwell writes like someone following Popper or Feyerabend, who understood the philosopher to be improving the World, rather than contributing to a small piece of one of many debates, each of which can be understood only by the small number of its participants…. In spite of this, the argument is complex, graceful, and its finer points are quite subtle…. The book's final chapter calls for nothing less than revolution in academia, including the very meaning of academic life and work, as well as a list of the nine most serious problems facing the

contemporary world - problems which it is the task of academia to articulate, analyse, and attempt to solve. This chapter sums up what the reader has felt all along: that this is not really a work of philosophy of science, but a work of 'Philosophy', which addresses 'Big Questions' and answers them without hesitation…. I enjoyed the book as a whole for its intelligence, courageous spirit, and refusal to participate in the specialisation and elitism of the current academic climate…. it is a book that can be enjoyed by any intelligent lay-reader. It is a good book to assign to students for these reasons, as well - it will get them thinking about questions like: What is science for? What is philosophy for? Why should we think? Why should we learn? How can academia contribute of the welfare of people? … the feeling with which this book leaves the reader [is] that these are the questions in which philosophy is grounded and which it ought never to attempt to leave behind."
Margret Grebowicz, ***Metascience***

"Maxwell's fundamental idea is so obvious that it has escaped notice. But acceptance of the idea requires nothing short of a complete revolution for the disciplines. Science should become more intellectually honest about its metaphysical presuppositions and its involvement in contributing to human value. Following this first step it cures itself of its irrational repressed aims and is empowered to progress to a more civilized world."
Professor Leemon McHenry, ***Review of Metaphysics***

"Maxwell argues that the metaphysical assumptions underlying present-day scientific inquiry, referred to as standard empiricism or SE, have led to ominous irrationality. Hence the alarmingly provocative title; hence also-the argument carries this far-the sad state of the world today. Nor is Maxwell above invoking, as a parallel example to science's besetting "neurosis," the irrational behavior of Oedipus as Freud saw him: unintentionally yet intentionally slaying his father for love of his mother (Mother Earth?). Maxwell proposes replacing SE with his own metaphysical remedy, aim-oriented empiricism, or AOE. Since science does not acknowledge metaphysical presumptions and therefore disallows questioning them - they are, by definition, outside the realm of scientific investigation - Maxwell has experienced, over the 30-plus years of his professional life, scholarly rejection, which perhaps explains his occasional shrill tone. But he is a passionate and, despite everything, optimistic idealist. Maxwell claims that AOE, if adopted, will help deal with major survival problems such as global warming, Third World poverty, and nuclear disarmament, and science itself will become wisdom-oriented rather than knowledge-oriented--a good thing. A large

appendix, about a third of the book, fleshes the argument out in technical, epistemological terms. **Summing Up**: Recommended. General readers; graduate students; faculty."
Professor M. Schiff, ***Choice***

"*Is Science Neurotic?* … is a rare and refreshing text that convincingly argues for a new conception of scientific empiricism that demands a re-evaluation of what [science and philosophy] can contribute to one another and of what they, and all academia, can contribute to humanity… *Is Science Neurotic?* is primarily a philosophy of science text, but it is clear that Maxwell is also appealing to scientists. The clear and concise style of the text's four main chapters make them accessible to anyone even vaguely familiar with philosophical writing and physics… it is quite inspiring to read a sound critique of the fragmented state of academia and an appeal to academia to promote and contribute to social change."
Sarah Smellie, ***Canadian Undergraduate Physics Journal***

"Maxwell's aspirations are extraordinarily and admirably ambitious. He intends to contribute towards articulating and bringing about a form of social progress that embodies rationality and wisdom... by raising the question of how to integrate science into wisdom-inquiry and constructing novel and challenging arguments in answer to it, Maxwell is drawing attention to issues that need urgent attention in the philosophy of science."
Professor Hugh Lacey, ***Mind***

"Maxwell has written a very important book . . . Maxwell eloquently discusses the astonishing advances and the terrifying realities of science without global wisdom. While science has brought forth significant advancements for society, it has also unleashed the potential for annihilation. Wisdom is now, as he puts it, not a luxury but a necessity . . . Maxwell's book is first-rate. It demonstrates his erudition and devotion to his ideal of developing wisdom in students. Maxwell expertly discusses basic problems in our intellectual goals and methods of inquiry."
Professor Joseph Davidow, ***Learning for Democracy***