
How Can We Build a Better World?

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Introduction

Just in case the reader hopes that, in the next few pages, I am somehow going to come up with the solution to the world's problems, as a conjuror might produce a rabbit out of a hat, let me confess at the outset: I do not have the solution to the world's problems. The title of my talk may even, I am afraid, be a bit misleading. For the question that I am going to address is not so much »How can we build a better world?« as »What kind of *inquiry* can best help us build a better world?« or »How ought we to go about *learning* how to build a better world?«

What, to begin with, do I mean by »a better world« – »a good world«? Nothing I think very surprising or controversial. A good world is a world in which everyone shares equally in the creating, sustaining and enjoying of what is of value in life, *insofar as this is possible*. A good world is a world in which there is peace, justice, democracy, freedom, and a sustainable economy. It is a world in which unnecessary human suffering and death are kept to a minimum, and the good things in life flourish: friendship, love, generosity of spirit, pleasure and happiness, concern for others, creative endeavour, great art and science.

In order to build such a world we need at least to solve the following six major global problems.

- (1) We need to end war and the threat of war. Our record during the 20th century is not good: 99 million people have died as a result of war in the 20th century so far.
- (2) We need to end third world poverty, and establish a more just distribution of wealth over the globe. As it is, one fifth of all people alive today live in conditions of abject poverty, mostly in the third world, without access to safe water, health care, education.

UNICEF estimates that 15 million young children die unnecessarily every year from malnutrition, or diseases related to malnutrition.

- (3) We need to eliminate tyranny everywhere, whether of the right or left, and establish world-wide democracy.
- (4) We need to end population growth.
- (5) We need to create an ecologically sustainable world industry and agriculture, so that depletion of vital natural resources, pollution, the destruction of tropical rain forests, the extinction of plant and animal species, and the threat of global warming are all brought to an end.
- (6) We need to create more rationally cooperative institutions and social arrangements, both locally and globally.¹

If I had been giving this talk a mere five years ago, I would have included a seventh global problem, the gravest of all, the problem posed by the cold war, the nuclear arms race, star wars, the apparent ever-increasing likelihood of nuclear armageddon. Almost miraculously, thanks largely to Mikhail Gorbachev, we do now seem to be overcoming this particular form of global insanity; and this surely ought to be a source of great hope. Released from the paralyzing insanities of the cold war, we may now begin to take seriously the possibility of resolving our other major global problems.

It is vital to appreciate that problems do not just arise in connection with how we are to realize the aim of building a good world; the aim itself is inherently problematic. We need to adopt a version of the aim that is both desirable and realizable: but both of these are profoundly problematic. The history of Utopian thought is very largely the history of impossible dreams which, if actively pursued, turn rapidly into all too possible nightmares. A good world ought to do justice to the

1 The best overall account known to me of our global problems is R. Higgins' *The Seventh Enemy: The Human Factor in the Global Crisis* (London, 1978). Even though published 12 years ago now and therefore, in some respects out of date, it still provides a lucid and vivid survey of our most urgent problems – the seventh enemy being that combination of blindness of individuals and inertia of political institutions which prevents us from coming to grips with our problems (and which the kind of inquiry I advocate here is intended to help us overcome).

conflicting interests of people: notoriously, this poses problems. And it is not just interests that conflict: ideals or values inevitably conflict as well. In a good world individual liberty will be restricted so that it can be maximized: this highlights dramatically the inherently problematic character of the goal. Again, in a good world, restrictions will be placed on the development of unjust inequalities; but of course if equality is pursued with too much zeal we end up destroying liberty, and equality as well, since a privileged class is needed to enforce »equality« on the majority.

Given the immensity and complexity of the problems that we need to solve in order to create a better world, and given our record of at best only partial success in creating a better world during the 20th century so far – given in particular our 20th century record of man-made tragedies and disasters involving millions – it seems reasonable to conclude that if we are to create a better world we need to *learn* how to do it. And this in turn suggests that we urgently need good traditions and institutions of inquiry rationally designed and devoted to helping us learn how to build a better world.

This brings me to the basic questions of my talk: What kind of inquiry can best help us create a better world? Or, a little more generally, perhaps: What kind of inquiry can best help us realize what is of value to us insofar as this is an aspect of, or is compatible with, a good world? What ought to be the overall character, the aims and methods, of organized inquiry – of science, technological research, social inquiry, the humanities, education – granted that the entire enterprise is rationally designed and devoted to helping us build a better world? How good are our existing traditions and institutions of learning, our schools and universities, when judged from this standpoint? What changes, if any, need to be made to academic inquiry if it is to be rationally designed to help us realize what is of value, a better world?

These are urgent and fundamental, if at present rather neglected problems that touch on almost every aspect of intellectual, cultural and practical life. They certainly ought to be regarded as central and fundamental to philosophy – and to the philosophy of inquiry, the philosophy of science.

A Kind of Inquiry Rationally Designed to Help Us Build a Better World

Without more ado, I now plunge into an attempt to answer these questions, proceeding from first principles, as it were. In order to do this, there are two vital preliminary points I must consider. First: What kind of *problems* do we need to solve in order to build a better world? And second: What is it to be rational? I take these in turn.

In order to build a better world the problems that we need to solve are fundamentally personal, social and global problems of *living*, problems of *action*, and not, primarily, problems of knowledge or technological know-how. The six global problems I indicated earlier are all of this type: they all require for their solution new actions, new ways of living, new policies, new economic and political programmes, new institutions, new philosophies of social life. We do of course also need to solve problems of knowledge and technological know-how, but only in a subordinate and secondary way, as an aid to the clarification of our problems of living, or as an aid to the assessment or implementation of proposed solutions to problems of living. In building a better world, to repeat, it is what we do, how we live, that ultimately matters: knowledge and technology make a contribution – in helping us solve medical or agricultural problems for example – only insofar as they enhance our capacity to act. In themselves, dissociated from life, solutions to problems of knowledge and technology are without value.

In a sense, this is true even when we pursue knowledge and understanding for their own sake. What matters here, ultimately, is the active personal striving to see and to apprehend, the active intellectual exploring of this mysterious and beautiful world in which we find ourselves, the sharing with others of what we have discovered. Impersonal knowledge and understanding are of value insofar as they help personal knowing and understanding but otherwise, dissociated from life, are without value. As Einstein once put it »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«.²

2 A. Einstein, *Ideas and Opinions* (London, 1973), p. 80.

We can say quite generally, then, that in order to realize what is of value the problems that we need to solve are, fundamentally, problems of living; impersonal problems of knowledge and technology are subordinate and secondary.

There is an additional important point to be made about the nature of the problems we need to solve in order to build a better world. Many of our most serious global problems can be regarded as arising from our failure to live together cooperatively. This is true of war and the threat of war; it is true of tyranny; and it is true of the grossly unjust way in which wealth continues to be distributed over the globe. We can say, then, that in order to build a better world it is essential that we discover how to resolve our conflicts, our problems of living, in more cooperative ways than we do at present. The task of creating more rationally cooperative institutions and social arrangements, locally and globally, is perhaps the key to solving our other global problems.

A group acts cooperatively insofar as all members of the group freely share responsibility for what is done, and for deciding what is done, proposals for action, for the the resolution of problems and conflicts, being judged on their merits from the standpoint of the interests of members of the group, or the interests of the group as a whole, there being no permanent leadership or delegation of power. There are of course degrees of cooperativeness, from all out violence at one extreme, through the settling of conflicts by means of threat, agreed procedures, or bargaining, to all out cooperativeness at the other extreme. Competition is not opposed to cooperation if it proceeds within a framework of cooperation, as it does, perhaps, within science.

A basic task of a kind of inquiry designed to help us build a better world will be to help us resolve our conflicts in increasingly cooperatively rational ways.

This brings me to my second preliminary point: what is it to be rational? The conception of rationality that we require appeals to the idea that there are general methods, rules or strategies which, if put into practice, whatever we may be doing, give us our best chances, other things being equal, of solving our problems, of realizing our aims.³ These methodological rules of rationality are fallible and non-

3 It is important to note that this conception of rationality includes the

mechanical: they do not determine our choices for us, but rather enhance our capacity to choose well.⁴ All problem-solving is aim-pursuing, and in a certain sense all aim-pursuing is problem-solving. The methods of reason may be formulated either as methods for solving problems, or as methods for realizing aims. I shall formulate methodological rules of reason in both ways.

Here then, to begin with, are four extremely elementary rules of problem-solving rationality, uncontroversial, I hope, to the point of banality. (I take banality, in the present context, to be a virtue and not a vice.)

Rule (1): Articulate, and try to improve the articulation of, the problem to be solved.

Rule (2): Propose (or try out) diverse possible solutions, and critically assess their adequacy.⁵

methods of *empirical testing* as a special case. One important way of critically (i. e. rationally) assessing an idea is to put it into practice and assess it in the light of *experience*. Reason, as understood here, includes, and is not opposed to empiricism. (See K. Popper, *The Open Society and Its Enemies* (London, 1966), vol. 2, ch. 24, for a discussion of the importance of characterizing reason in such a way as to include empiricism.)

4 Reason, as understood here, is doubly non-authoritarian. First, it is fallible; and second, it does not *determine* (even fallibly) choices for us, but helps us to make good choices for ourselves. The strategies of reason tell us what we need to try to do in order to put ourselves into a good position to solve our problems or realize our aims, but do not decide for us what choices we ought to make. The doubly non-authoritarian character of reason is vitally important when it comes to assess the desirability of living a rational life or creating a rational society. As long as we uphold authoritarian or oracular conceptions of reason (according to which, ideally, reason decides for us), to live rationally is to become enslaved to reason, and the rational society can only be authoritarian, even totalitarian. In terms of emphatically non-authoritarian conception of reason advocated here, however, the rational life and the rational society is that life, and that society which, other things being equal, is most conducive to the realization of what is of most value (including freedom, democracy and justice). For a more detailed discussion of these issues see K. Popper's *The Open Society and its Enemies*, and N. Maxwell, *From Knowledge to Wisdom: A Revolution in the Aims and Methods of Science* (Oxford, 1984), especially pp. 85–91.

5 Or, as Popper has put it: »... the one method of all critical discussion ...

These two rules I take to be absolutely basic to rationality. No enterprise can hope to be rational which systematically violates them.

Many problems are however too intractable to be solved by means of this direct approach alone. In these cases we need to put into practice a third methodological rule of rational problem-solving, namely:

Rule (3): When necessary, break up the basic problem to be solved into a number of preliminary, simpler, analogous, subordinate, specialized problems (to be tackled in accordance with rules (1) and (2)), in an attempt to work gradually towards a solution to the basic problem to be solved.

The danger in putting this third rule into practice is that the activity of solving preliminary, subordinate or specialized problems may obliterate all concern for the original, basic problem we seek to solve. We need therefore a fourth rule to counteract this danger, namely:

Rule (4): Interconnect attempts to solve basic and specialized problems, so that basic problem-solving may guide, and be guided by, specialized problem-solving.

The three methodological rules of aim-pursuing rationality that I shall appeal to are as follows.

Rule (A): Whenever our aims are problematic, as aims often are (because of unforeseeable undesirable consequences, because of conflicts between aims, or because the realizability of an aim is in doubt), we need to try to improve our aims, and associated methods, as we act, as we proceed; we need to explore imaginatively, and assess critically alternative possible aims and methods in an attempt to improve our solution to the problems associated with the aims we are attempting to achieve.

Rule (B): We need to try to ensure that we do not misrepresent or repress the aim that we are actually pursuing – especially if this aim is problematic.

is that of stating one's problem clearly and of examining its various proposed solutions critically« (K. Popper, *The Logic of Scientific Discovery* (London, 1959), p. 16). It deserves to be noted that Popper's conception of scientific method as consisting, in essence, of conjecture and refutation, amounts to a detailed development of rule (2) in the context of science. Some of the limitations of Popper's philosophy of science, as we shall see, arise from his relative neglect of other elementary rules of reason.

Rule (C): In an attempt to improve a problematic aim, we need to ask *why* we are pursuing the aim, both in the historical sense of how we came to be pursuing it, and in the »rationalistic« sense of what more general or more distant aim we hope to achieve.

These rules of aim-pursuing rationality are especially important granted that our aim is to try to help build a better world for, as I have already emphasized, this aim is inherently and profoundly problematic. If we neglect Rule (A) the chances are that we shall try to create a world that is undesirable, unrealizable, or both together, all our efforts resulting in hell on earth. One consequence of the neglect of rule (A) in political thought and practice is that the very idea of trying to make progress towards a better world has become discredited. A kind of Utopian politics that takes rule (A) seriously has scarcely been attempted. Rule (B) is relevant because it is just when our aims are problematic that we are most likely to misrepresent them. If we do misrepresent our aim, then the more rationally we pursue what we take to be our aim, the worse off we shall be: rationality becomes a hindrance rather than a help. Rule (C) is important in that, if implemented, ideas may emerge as to how a given aim can be improved.

Rules (A), (B) and (C) are what may be called *metamethods*. The presumption is that we pursue many different sorts of aims in life, employing many different more or less appropriate methods more or less successfully. The metamethods (A), (B) and (C) specify how to go about improving our diverse aims and methods in the diverse contexts of life; they are methods for the improvement of methods – that is, metamethods. *Rationality thus requires that there is diversity of methods corresponding to our diverse aims but unity of method at the metamethodological level.*⁶

So far I have said something about the kind of problems we need to solve in order to build a better world, and something about rational problem-solving or aim-pursuing. I now put two things together in an attempt to answer the basic question of my talk: What will be the general character and features of a kind of organized or institutionalized

6 For a more detailed discussion of aim-pursuing rationality see N. Maxwell, *What's Wrong With Science?* (Frome, England, 1976), Chs. 5–10, and *From Knowledge to Wisdom*, Ch. 5.

inquiry that is rationally designed to help us build a better world? The basic idea is straightforward enough: inquiry will be so organized and institutionalized that the problems we need to solve in order to build a better world are tackled in accordance with the rules of rationality.

The central and fundamental intellectual tasks of inquiry will be to articulate, and to improve the articulation of, personal, social and global problems of living, and to propose and critically assess possible cooperative *actions*. These tasks, at the heart of inquiry as a whole, undertaken in accordance with rules (1) and (2) of rational problem-solving, will be performed by social inquiry and the humanities. Thus the various branches of social inquiry and the humanities – economics, politics, sociology, psychology, anthropology, human geography, history, philosophy, cultural studies – are not primarily *sciences* at all; nor is their primary concern to improve knowledge of social phenomena, to solve problems of knowledge. Their primary intellectual task is to invent and explore imaginatively diverse possible cooperative *actions*, diverse possible ways of living, policies, economic and political programmes, institutions, philosophies of social life, and to assess these critically from the standpoint of their desirability and feasibility, their capacity, if implemented, to help us make progress towards a good world. Social inquiry and the humanities are primarily concerned to improve our ideas about how to live rather than our ideas about what is the case. Pursued in this way, social inquiry and the humanities form the central, fundamental part of inquiry as a whole; they are intellectually more fundamental than the biological, physical or technological sciences.

Notoriously, many problems of living are too intractable to be solved by means of the direct approach alone, by means of the use of rules (1) and (2). Taking this into account, inquiry implements rule (3), and breaks up the fundamental problems of living into a host of subordinate, specialized problems of knowledge and know-how, thus creating many subordinate, specialized disciplines. Social inquiry itself needs to acquire knowledge of relevant social phenomena, in order to aid the articulation of problems of living and the assessment of proposed solutions. The technological sciences – medicine, agriculture, engineering, artificial intelligence – tackle diverse technical problems of know-how, thus enhancing our power to act, to solve our problems of

living, to achieve what is of value. The physical and biological sciences tackle problems of knowledge and understanding of diverse aspects of the natural world, thereby improving our knowledge and understanding of what it is possible and not possible to do in order to solve our problems of living. Mathematics, by contrast, is not concerned to improve knowledge of anything actual at all. It is concerned rather to develop, systematize and unify abstract problem-solving methods; it is concerned with the exploration of *problematic possibilities*. As a result, mathematics is able enormously to enhance our problem-solving powers even though it does not in itself embody knowledge of any aspect of the actual.

The danger in setting up all this subordinate, specialized problem-solving is of course that it becomes dissociated from and unrelated to our fundamental problems of living – the latter even being lost sight of amongst the welter of specialized problem-solving. In order to counteract this danger, rule (4) of rational problem-solving needs to be built into the institutional structure of inquiry in addition to rules (1), (2) and (3). Thus the tackling of fundamental problems of living and specialized problems of knowledge and know-how are interconnected so that each may guide and be guided by the other. This does *not* mean, however, that only those subordinate or specialized problems are tackled which are seen to have an immediate relevance to our problems of living. In the first place, knowledge and understanding can be of value in their own right. Second, it often, though by no means always happens that solutions to practical problems emerge from entirely unexpected quarters, as a result of research into apparently unrelated matters. The use of X-rays in medicine is an obvious example. Third, we do not altogether know what our problems of living are: we need research into matters not immediately related to our present social concerns in part to help us discover the existence of problems of living of which we are not aware. Fourth, future problems of living may differ from our present problems, for a variety of reasons: specialized research not obviously needed from the standpoint of our immediate concerns may well be worth supporting because of a possible relevance to future concerns. A basic aim of inquiry is to build up our general problem-solving power, and this requires research to be undertaken that is not obviously related to immediate human need. None

of this, however, counteracts the fundamental importance of ensuring that rule (4) is built into the organizational structure of inquiry.⁷

For each one of us, the most important and fundamental inquiry going on in the world is the thinking that we engage in, on our own or with others, as we live – the thinking that guides or influences our actions. And from the standpoint of building a better world it is of course this kind of personally and socially active thought guiding personal and social life that really matters. The basic task of institutionalized or academic thought is to help enhance, by cooperatively rational means, the power and quality of personally and socially active thought so that it may all the better enable us to realize what is of value to us insofar as this is compatible with a good world. The intellectual progress of socially active thought is what ultimately matters, and the intellectual progress of academic thought is but a means to that end.

In a sense, academic inquiry as whole is a specialized part of the more fundamental socially active inquiry which we all engage in as we live. To the extent that this is correct, the relationship between academic thought and thought in the rest of the social world needs to comply with rule (4): each needs to learn from the other.

It is absolutely essential that organized inquiry is entirely without political power, and is non-authoritarian in character. There can be no question of academics deciding for the rest of us what our problems are, how they should be solved, how we should live or what is of value. Far from depriving us of the power to decide for ourselves, the task of organized inquiry is to help us enhance our power to decide for ourselves well by providing us with good ideas, proposals and arguments for our consideration. Academics will advocate, argue and criticize, but will have no power or authority to determine the thoughts and decisions of others. Academic inquiry will be a sort of people's civil service, doing openly for the public, with exemplary intellectual

7 For a more detailed discussion of the important role rule (4) has in counteracting that disease of the academic world today, specialism (the view that only specialist intellectual standard matter) see N. Maxwell, »Science, Reason, Knowledge and Wisdom: A Critique of Specialism«, *Inquiry* 23 (1980), pp. 19–81.

honesty and integrity, what actual civil services are supposed to do, in secret, for governments. (Academic inquiry must of course retain its independence, and must not degenerate into merely serving the special interests of government, industry, the nation, or public opinion.)⁸

So far I have characterized the basic task of social inquiry as being to help us to solve our problems of living by helping us to exploit the four rules of rational problem-solving. In addition to this, and on a somewhat longer-term basis, social inquiry has the task of helping us make progress towards a good world by implementing the three metamethods of aim-pursing rationality in all our diverse institutional endeavours. The idea here is that in order to build a better world the aims and methods of all our institutions need to be progressively improved – institutions associated with government, the law, industry, agriculture, medicine, international relations, education, and even scientific and technological research. Conceived of in this way, social inquiry is again not science but rather *social methodology* or *social philosophy*. What the philosophy of science, properly conceived, is to science, so economic inquiry is to economic activity, and political inquiry is to politics. On this view, the sociology of science and the philosophy of science are one and the same thing – that part of scientific enterprise concerned to improve the aims and methods of science so that science may all the more effectively contribute to the task of building a better world. (I will have more to say about this in a moment.) On this view, then, *there is unity of method, at the metamethodological level, throughout all of inquiry: nevertheless, social inquiry is entirely different from natural science, social inquiry being*

8 The academic world needs just sufficient power and authority to retain its independence, but no more. Academic inquiry needs to be non-authoritarian, not only about questions of value and how we are to live, but also about questions of fact and knowledge. All academic pronouncements, however expert and specialized, need to be open to challenge from non-experts. If we are to believe the pronouncements of experts, this should be because there are good reasons to do so, and not because experts possess some unassailable authority of expertise – being a sort of latter-day priesthood attending the oracle science. The non-authoritarian character of science, and of academic inquiry more generally, has much to do with the adoption of a non-authoritarian conception of reason.

social methodology or social philosophy, and not primarily science at all.

Learning how to build a better world, from what I have said so far, may well seem to be entirely a matter of discovering how to tackle problems more rationally: I have said nothing about learning from experience.⁹ In fact learning from experience has a vital role to play in the kind of inquiry I have been trying to describe. What we learn as a result of attempting to put into practice some proposed solution to a problem of living is of course all important for learning how to build a better world. A vital task for inquiry will be to monitor the successes and failures of attempts in practice to implement various proposed solutions to problems of living so that we can learn from such attempts; and a vital task for history is to see what can be learned from the successes and failures of our past attempts at solving problems of living. As far as possible, of course, we should try to keep our failures to social experiments that we perform *in imagination* rather than *in practice* in the real world, so that we only suffer the consequences of failure in imagination and not in reality. But however vivid, far-seeing and accurate our imagination may be, failure in practice will always happen, and we should seek to learn all we can from it for future actions. To this extent, what I am advocating can be described as a kind of empiricism, a kind of learning from experience. In two crucial respects however it differs from what is usually meant by empiricism. First, what is learned is how to do things, how to realize what is of value, how to live, and not, primarily, what we learn in the context of science: knowledge of fact. And second, »experience« means something like »what we acquire as a result of attempting to do things, attempting to achieve what is of value«, and not, primarily, what it means in the context of science: observation and experimentation.

I must emphasize that the kind of inquiry that I have been attempting to depict is designed to help us to see, to know and to understand, for their own sake, just as much as it is designed to help us solve our practical problems of living. It might seem that social inquiry, in articulating personal and social problems of living and proposing

9 See however note 3.

possible solutions, has only a severely practical purpose. But engaging in this intellectual activity of articulating personal and social problems of living is just what we need to do if we are to develop a good empathic understanding of our fellow human beings (and of ourselves) – a kind of understanding that can do justice to our humanity, to what is of value, potentially and actually, in our lives. In order to understand another person as *a person* (as opposed to a biological or physical system) I need to be able, in imagination, to see, desire, fear, believe, experience and suffer what the other person sees, desires, fears, believes, experiences, suffers. I need to be able, in imagination, to enter into the other person's world; that is, I need to be able to understand his problems of living as he understands them, and I need also, perhaps, to understand a more objective version of these problems. In giving intellectual priority to the tasks of articulating problems of living and exploring possible solutions, social inquiry thereby gives intellectual priority to the development of a kind of understanding that people can acquire of one another that is of great intrinsic value. Indeed, I would argue that it is essential to the development of our humanity, even perhaps our human consciousness, that we are able, when young, to acquire this kind of empathic understanding of others. Our being able to understand each other in this kind of way is also essential for cooperatively rational action. And it is essential for science. It is only because scientists can enter imaginatively into each other's ways of seeing things, each other's problems and research projects that objective scientific knowledge can develop. In attempting to improve our own understanding of something as utterly impersonal as the physical universe it is important to have some empathic understanding of what such a quest meant for Einstein, let us say, or for Faraday or Galileo: if we do not, our own understanding, however technically brilliant, is likely to be impoverished. In integrating the personal and impersonal aspects of science, the kind of inquiry I have tried to sketch is able to do full justice to the value of science when pursued for its own sake.¹⁰

10 For a more detailed discussion of this point, see my *What's Wrong With Science?*, especially Chs. 5–7; »Science, Reason, Knowledge and Wisdom«; and *From Knowledge to Wisdom*, especially Chs. 1, 5, and 9, and pp. 181–189.

According to the kind of inquiry I am attempting to describe, the arts have a vital *rational* contribution to make, as revelations of value, as imaginative exploration of possibilities, desirable or disastrous, or as a vehicle for the criticism of fraudulent values through comedy, satire or tragedy. Literature and drama also have a rational role to play in enhancing our power to understand empathically our fellow human beings as a result of identifying imaginatively with fictional characters – literature in this respect merging into biography, documentary and history. Literary criticism bridges the gap between literature and social inquiry, and is more concerned with the *content* of literature than the means by which literature achieves its effects.

It deserves to be noted, finally, that the metamethodological conception of rationality outlined above makes it possible to hold that our feelings, our desires and fears, are in general vital ingredients of rationality, even though more relevant in some contexts, such as being with friends and those we love, than others, such as experimental science or logic. (But even here, a *feeling* that an experiment is inadequately designed, or an argument inadequately put together, may well be worth attending to.) If we are to discover for ourselves what is of value, we must attend to our feelings and desires. But not everything that feels good is good, and not everything we desire is desirable. Rationality requires that feelings and desires take fact, knowledge and logic into account, just as it requires that priorities for the pursuit of knowledge take feelings and desires into account. In insisting on this kind of interplay between feelings and desires on the one hand, knowledge and understanding on the other hand, the kind of inquiry I have sketched helps us to acquire what we need if we are to help create a better world: mindful hearts and heartfelt minds.

This concludes my description of some broad features of a kind of inquiry designed to help us build a better world (see diagram 1).¹¹

At this point I must consider a possible objection to what I have argued for so far. It may be objected that before we can articulate and tackle problems of living we must first acquire relevant knowledge, simply in order to know what our problems of living are. Thus the

11 For a more detailed discussion of this point, see works referred to in note 10.

INQUIRY RATIONALLY DESIGNED TO HELP US BUILD A BETTER WORLD

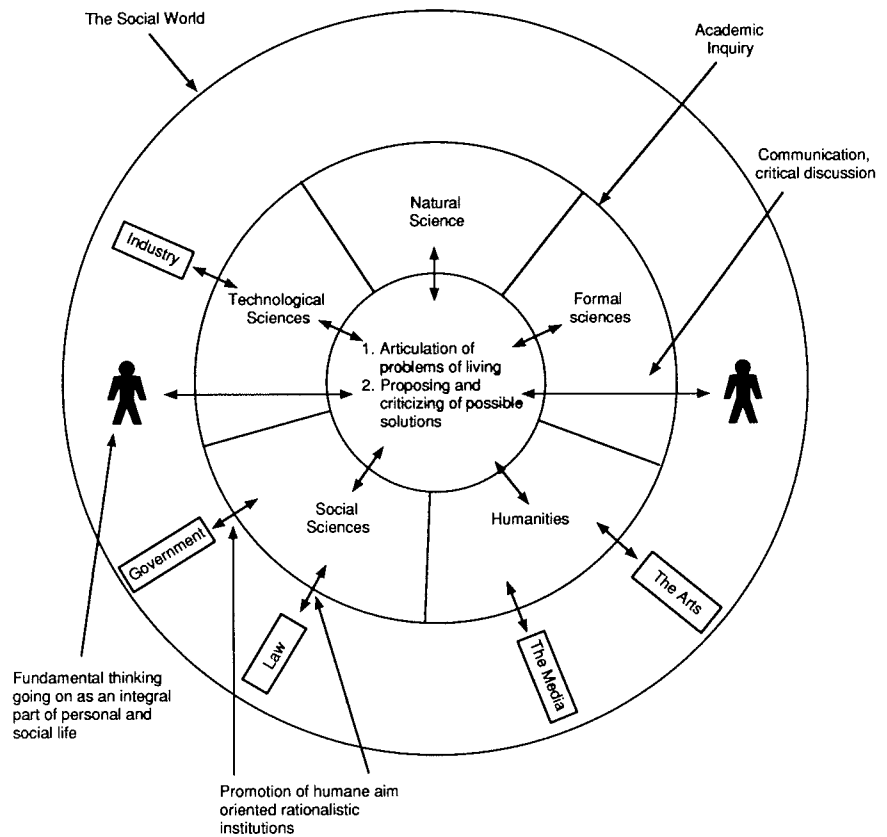


Diagram 1: *Inquiry Rationally Designed to Help us Build a Better World*

task of acquiring knowledge must be intellectually more fundamental than the task of tackling problems of living, and not the other way round, as I have maintained.

I have six staccato replies to this objection. *First*, even if the objection were valid, it would still be vital for a kind of inquiry designed to help us build a better world to include rational exploration of problems of living, and to ensure that this guides priorities of scientific research,

and is guided by the results of such research, roughly in accordance with rule (4) of rational problem-solving. But *second*, the validity of the objection becomes dubious when we take into account the considerable success people met with in solving problems of living in a state of extreme ignorance, before the advent of science. We still today often arrive at solutions to problems of living in ignorance of relevant facts. *Third*, the objection is not valid. In order to articulate problems of living and explore possible solutions we need to be able to act in the world, imagine possible actions and share our imaginings with others: insofar as some common sense knowledge is implicit in all this, such knowledge is required to tackle successfully problems of living. But this does not mean that we must give intellectual priority to acquiring new relevant knowledge before we can be in a position to tackle rationally our problems of living. *Fourth*, simply in order to have some idea of what kind of knowledge or know-how it is *relevant* for us to try or acquire, we must *first* have some provisional ideas as to what our problem of living is and what we might do to solve it. *Fifth*, relevant knowledge is often hard to acquire; it would be a disaster if we suspended life until it had been acquired. Knowledge of how our brains work is presumably relevant to all that we do but clearly, suspending living until this relevant knowledge has been acquired would not be a sensible step to take. It would, in any case, make it impossible for us to acquire the relevant knowledge. Scientific research is itself a kind of action carried on in a state of relative ignorance. *Sixth*, the capacity to act, to live, more or less successfully in the world, is more fundamental than (propositional) knowledge. All our knowledge is but a development of our capacity to act. Dissociated from life, knowledge stored in libraries is just paper and ink.¹²

How Good is Contemporary Academic Inquiry at Helping us Build a Better World?

To what extent does inquiry, as it actually exists in universities, research institutions and schools, correspond to the ideal kind of inquiry that I

12 For a more detailed exposition of these arguments, see *From Knowledge to Wisdom*, pp. 171–181.

have just outlined? How good, how rational, are contemporary science, scholarship and education when judged from the standpoint of helping us learn how to build a better world?

There can be no doubt that many academics struggle to do work that accords more or less with the conception of inquiry that I have outlined, and that scientific and scholarly work is produced that is of the highest quality when judged from the standpoint of helping us build a better world. But it is also the case, I suggest, that the dominant conception of inquiry that is built into the institutional structure of the academic enterprise differs sharply and dramatically from what I have outlined. For this dominant conception restricts academic inquiry to the pursuit of knowledge and technological know-how. If academic inquiry does help us build a better world, it does so by restricting itself, in the first instance at least, to acquiring factual knowledge. First knowledge is to be acquired; then it can be applied to help solve practical problems of living. On this view, social inquiry is social science, and is of course intellectually less fundamental than natural science.

It is vital to appreciate that insofar as the academic enterprise really does conform to this knowledge-pursuing conception of inquiry,¹³ it must be deemed to be disastrously irrational when judged from the standpoint of helping us build a better world. *Three* of the four elementary rules of rational problem-solving are violated. Rule (3) is put into practice to splendid effect in that a host of specialized problems of knowledge are tackled with brilliant success. But rules (1) and (2) are violated completely, in that the intellectual domain of inquiry excludes consideration of problems of living, and restricts itself to tackling problems of knowledge. And it follows at once that rule (4) is violated as well.

What has gone wrong? How can it be that our dominant conception of inquiry, exerting a profound influence over the course of science,

13 For a discussion of the question of the extent to which academic inquiry today is dominated by this knowledge-pursuing view, see *From Knowledge to Wisdom*, Ch. 6: »Present Domination of the Philosophy of Knowledge in the Academic World«.

scholarship and education, is so grossly and damagingly irrational when judged from the all-important standpoint of helping us build a better world?¹⁴

The Enlightenment and the Counter-Enlightenment

The idea for the kind of inquiry that I am advocating here can be traced back at least to the Enlightenment of the 18th century. For the basic idea of the philosophes of the Enlightenment – Voltaire, Diderot, Condorcet and the rest – was just to devote reason to the task of creating a more enlightened world. And the best of the philosophes did indeed actively strive, in their lives, to put into practice the sort of principles of rationality that I have indicated above. They fought dictatorial power, authoritarianism, superstition, dogma, mere tradition, injustice and intolerance with weapons no more lethal than those of argument and wit. They gave their support to the virtues of tolerance, diversity of opinions, openness to doubt, readiness to learn from criticism and from experience. They even lamented the development of specialized science and knowledge dissociated from the central task of achieving enlightenment. Courageously and energetically they laboured to promote rationality in personal and social life (in a sense of rationality which is close to the way I have characterized it above). The philosophes lived and breathed rational inquiry devoted to achieving a better world.¹⁵

Granted all this, and granted the considerable influence of the Enlightenment on subsequent history, why was the central idea and inspiration of the philosophes betrayed? Why is it still betrayed, by and large, in the character and institutional structure of our schools and universities today? Why are not academics up in arms against a kind of inquiry that is irrationally restricted to the pursuit of knowledge? Why do academics today even oppose vehemently the suggestion

14 For a discussion of the damaging consequences of this irrational knowledge-pursuing view, see *From Knowledge to Wisdom*, Ch. 3.

15 For a magnificent account of the Enlightenment along these lines, see P. Gay, *The Enlightenment: An Interpretation* (London, 1973).

that academic inquiry ought to take up its proper Enlightenment task of helping humanity learn how to tackle its problems of living in more cooperatively rational ways.¹⁶

An important part of the reason for all this, I suggest, is that the philosophes, in developing their basic idea in a little more detail, unfortunately made two serious intellectual mistakes. The result was that the Enlightenment programme came to be developed in a seriously defective form. The two mistakes of the Enlightenment were never subsequently put right. Even today there is no general recognition and understanding of what precisely these mistakes are, and what needs to be done to put them right. The result is a general blindness to the possibility of developing a genuinely rational kind of inquiry devoted to helping us make progress towards a good world.

In developing their basic idea, the philosophes sought to discover how humanity could learn from scientific progress how to make social progress towards an enlightened world. Immensely impressed with the way in which natural science progressively improved knowledge of Nature, they sought to apply the methods of natural science to social, economic and political problems, hoping that it would thereby prove possible to make social progress towards an enlightened world. The philosophes assumed that the proper way to implement this programme

16 Something of this opposition manifested itself on the occasion when this talk was delivered in Bonn on the 27th June 1990. To my amazement, I was accused of advocating totalitarianism, or at least advocating a kind of inquiry which would tend to lead to totalitarianism. We are to suppose, evidently, that academics do not actively explore, imaginatively and critically, possible solutions to urgent human problems because to do so would tend to usher in totalitarianism. How extraordinary! In my view, the matter is all the other way around. A kind of inquiry which restricts itself to the pursuit of knowledge is much more conducive to totalitarianism, and can coexist much more comfortably with totalitarianism, simply because inquiry conducted in such terms cannot be in a position to criticize the actions of governments professionally, as it were, as long as governments do not distort the truth. The kind of inquiry I am advocating, however, is professionally obliged to scrutinize critically the actions and policies of governments, and would thus be actively opposed to the development of totalitarianism (and would be abolished by a totalitarian government in power).

was to develop social science alongside natural science. Francis Bacon had already stressed just how vital it is to improve knowledge of the natural world in order to achieve social progress. The philosophes generalized this, holding that it is just as vital to improve knowledge of the social world. If it is social progress that we are after, then it is perhaps above all the laws of social change that we need to know; improving knowledge of social phenomena may be even more important than improving knowledge of natural phenomena.

Thus the philosophes set about creating the social sciences: history, anthropology, political economy, psychology, sociology. The idea was to create social science so that it would be as similar as possible to natural science – apart of course from the one big difference that whereas natural science studies the natural world, social science would study the human world. To the philosophes it seemed vital to bring into existence the rational, scientific study of humanity, so that knowledge of ourselves might improve in the spectacular way in which scientific knowledge of Nature had improved. Improving social knowledge seemed the essential first step to improving social life.

The Enlightenment effort to broaden the scope of science in this way, so that it included the study of the social world in addition to the natural world, had an immense impact. Throughout the 19th century the diverse social sciences were developed very much in accordance with the basic Enlightenment idea. By the mid twentieth century, all the diverse branches of the social sciences, as conceived of by the Enlightenment, were built into the institutional structure of Universities all over the world, as recognized academic disciplines. And today, the Enlightenment conception of social science continues to exercise a massive influence over academic thought and work.

The basic Enlightenment idea – to achieve a better world by means of science and reason – was of course opposed. It was opposed by Romanticism. The Enlightenment stressed the supreme importance of science, knowledge, reason, method, objectivity, logic, impersonal observation and experimentation. Romanticism found all this restrictive, oppressive, impersonal. It seemed to deny too much of what is of value in life. Thus Romanticism – stemming from such figures as Rousseau, Blake, Wordsworth, Beethoven, Goethe and many others – stressed the supreme value of art, self-expression, personal feelings and intui-

tion, imagination, spontaneity, individuality, creativity, sympathetic understanding, inspiration, genius, vision. For the rationalistic Enlightenment, intellectual integrity had to do with acquiring knowledge: it meant that one should attend to logic, to evidence – emotions and desires, hopes, fears and values all being disregarded in the quest for Truth. In contrast, for Romanticism, intellectual integrity had to do with emotional and motivational honesty, rather than honesty concerning fact or objective truth. Whereas for the rationalistic Enlightenment intellectual integrity demands the suppression (or ignoring) of emotion and desire, for Romanticism intellectual integrity demands, above all, the expression, the honest acknowledgment, of emotion and desire. Standards of intellectual integrity are diametrically opposed.

As far as the intellectual study of society is concerned, Romanticism leads to anti-scientific conceptions of social inquiry and the humanities, to what Isaiah Berlin has called »the Counter-Enlightenment«.¹⁷ What is important is the empathic study of the imaginings of humanity – the dreams and fantasies, the religion, the arts and ideas, the hopes and fears, the values. Whereas Nature is unaffected by our ideas about it, we *are* affected by our ideas about ourselves: this alone ensures, so figures of the Counter-Enlightenment argue, that the study of human culture and society cannot be like natural science.

The Counter-Enlightenment does not object to the idea that social inquiry and the humanities should have the basic intellectual aim of improving knowledge and understanding of our human world: it objects simply to the Enlightenment thesis that the kind of knowledge sought, and methods employed, ought to be as similar as possible to that of the natural sciences.

Academic inquiry today is by and large an uneasy, confused mixture of what we have inherited from the Enlightenment, from Romanticism and from the Counter-Enlightenment. And the net result is that the immensely important unifying idea of the Enlightenment of devoting reason to creating a better world has been lost sight of.

17 I. Berlin, *Against the Current* (London, 1979), Ch. 1.

The Two Intellectual Blunders of the Enlightenment

In opposing the Enlightenment, Romanticism, unfortunately, entirely missed the point. For what is wrong with the Enlightenment is not too much reason but, quite to the contrary, not enough. The traditional Enlightenment is a characteristic kind of *irrationality* masquerading as reason. Indeed, in stressing the fundamental importance of emotional and motivational honesty and integrity, Romanticism came closer, in some ways, to capturing what is most essential about rationality than did the ostensibly more »rationalistic« Enlightenment. The Romantic movement ought to have understood itself to be improving on the defective conception of rationality upheld by the Enlightenment: in this case a more coherent culture might have emerged, a rationalistic romanticism or a romantic rationalism, which might well have sought actively to create and develop inquiry along the lines I have indicated here – inquiry rationally devoted to helping us realize what is of value. Unfortunately, Romanticism naively took the Enlightenment to be what it claimed to be: the embodiment of Reason. Opposition to the Enlightenment thus became misinterpreted as opposition to Reason, and in welcoming irrationalism Romanticism itself suffered insofar as it became dogmatic, prone to being convinced by emotion alone, incapable of learning from criticism.

The basic idea of the philosophes of devoting reason to the attainment of enlightenment is, as I have already indicated, magnificent. And the slightly more specific version of this idea, that we should try to learn from scientific progress how to achieve social progress towards enlightenment, is also magnificent. And if the philosophes were impressed by the progress made by natural science in the early 18th century, we ought to be all the more vastly impressed by the extraordinary progress made by natural science throughout our 20th century. *Of course* we should try to learn all that we can about how to achieve social progress from this great example of scientific progress. But in order to do this it is absolutely essential that we get two key matters right. *First*, we must capture correctly the progress-achieving methods of science, responsible in practice for scientific progress. And *second*, we must generalize these progress-achieving methods correctly, and

apply them correctly to the task of achieving social progress. The Enlightenment got both of these crucial points disastrously wrong.

In order to specify the progress-achieving methods of science correctly, we must solve the problem of induction, the problem of exhibiting science as rational. Solving the problem of induction is something like an elementary test of adequacy for any view as to what the aim and methods of science are: any view of scientific method which fails this test cannot be adequate. Notoriously, the philosophes failed to solve the problem: they failed to specify adequately the progress-achieving methods of science. And from their time to ours, most scientists and philosophers of science have stubbornly held on to conceptions of science which render a solution to the problem of induction impossible.

Karl Popper, it is true, does claim to have solved the problem of induction;¹⁸ but, as we all know, his claim is a hollow one. He has not solved the problem.¹⁹ A solution to the problem of induction is however available. As I have argued for a number of years now, in order to solve the problem, and arrive at a rational conception of science, it suffices to adopt something like Einstein's mature view of science.²⁰

18 »I think that I have solved a major philosophical problem: the problem of induction.« K. Popper, *Objective Knowledge* (London, 1972), p. 1.

19 See for example: N. Maxwell, »A Critique of Popper's Views on Scientific Method«, *Philosophy of Science* 39 (1972), pp. 131–152. See also: I. Lakatos, »Popper on Demarcation and Induction«, in: *The Philosophy of Karl Popper*, ed. P. A. Schilpp (La Salle, Illinois, 1974), vol. 2, Ch. 5; A. Grünbaum, »Is the Method of Bold Conjectures and Attempted Refutations Justifiably the Method of Science?«, *British Journal for the Philosophy of Science* 27 (1976), pp. 105–136.

20 See my »A Critique of Popper's Views on Scientific Method«; »The Rationality of Scientific Discovery«, Parts I and II, *Philosophy of Science* 41 (1974), pp. 123–153 and 247–295; *What's Wrong With Science*, Ch. 6; »Articulating the Aims of Science«, *Nature* 265 (January 6, 1977), p. 2; *From Knowledge to Wisdom*, Ch. 9; »Induction and Scientific Realism: Einstein versus van Fraassen«, forthcoming.

How to Solve the Problem of Induction

The decisive point to appreciate is that in doing science we are rationally entitled to assume, as an item of scientific knowledge, that the universe is comprehensible, in some way or other. A comprehensible universe is a universe throughout which there exists *something*, invariant and unchanging, at all times and places, which in some sense controls, determines or is responsible for all that which changes, and in terms of which diversity and change can in principle be explained and understood. The omnipotent, omnipresent, unchanging *something* of the comprehensible universe might be God; it might be some kind of cosmic plan or purpose; or it might be a unified pattern of physical law. We are rationally entitled to assume that the universe is comprehensible in some way or other because *making this assumption helps, and cannot sabotage, the progress of science*. If the assumption is completely false, and the universe is wholly incomprehensible, then it is unknowable, and science and knowledge are impossible whatever we assume. (We would not be here to assume anything.) If, on the other hand, the assumption is only slightly false in the sense that the universe is approximately but not perfectly comprehensible, then the only way we can discover this fact is to assume that the universe is perfectly comprehensible, set out to discover in what precise way it is perfectly comprehensible, and fail in the attempt. Thus in assuming that the universe is comprehensible in some way or other we have everything to gain and nothing to lose.

The basic aim of science of discovering in what precise way the universe is comprehensible is profoundly problematic. At any given stage in its development, science quite properly makes some more or less specific metaphysical assumption about the way in which the universe is comprehensible and pursues the more or less specific *aim* of transforming this assumption into precise theoretical knowledge, adopting *methods* appropriate to this aim. Almost certainly, however, the metaphysical assumption is false, and the aim is unrealizable. It is thus essential that science seeks to *improve* its more or less specific aims and its methods as it proceeds, adopting the aim and methods which accord best with the basic comprehensible assumption, and which seem to lead to the greatest growth in empirical knowledge.

Since Galileo and Kepler, and the scientific revolution of the 17th century, physical science has in practice pursued evolving versions of the empirically progressive idea that some kind of unified pattern of physical law runs through all phenomena. Science has in practice implemented the metamethodology²¹ of seeking to improve aims and methods with improving knowledge, within the framework of the enduring assumption that the universe is comprehensible in some way or other – even though this has not been properly understood. With Einstein, however, all this becomes explicit. In his search for theoretical unity, Einstein tackled the most severe problems of unity associated with the theoretical knowledge of his time. Thus he tackled the problems of the clash between the field and the particle, the clash between continuous electromagnetic radiation and discrete matter, the clash between Maxwellian electrodynamics and Newtonian mechanics, and the clash between special relativity and Newtonian gravitation. From a consideration of these severe problems inherent in the aim of seeking unity, Einstein put forward principles that any new theory capable of resolving the problems of unity would have to satisfy, principles such as the restricted principle of relativity, the light postulate, Lorentz invariance, the principle of equivalence. These are both physical and methodological principles; they led Einstein to discover the special and general theories of relativity. Thus Einstein's new way of doing physics – his metamethodology of tackling severe problems associated with

21 The idea of characterizing scientific method on the »metamethodological« level was first introduced by me in 1974 in »The Rationality of Scientific Discovery«. Thus on page 260 I remark: »The reason why it is absolutely obligatory to characterize scientific method on what up until now has been thought of as the »metamethodological« level ... can be brought out quite decisively from the following elementary consideration ... *different aims give rise to different rational rules of theory acceptance*«. The outer trappings of the idea, and some of the terminology, have been taken up subsequently, without acknowledgement, by L. Laudan: see, for example, L. Laudan, »Normative Naturalism«, *Philosophy of Science* 57 (1990), pp. 44–59. In Laudan's account, however, the central idea of committing science to the assumption that the universe is comprehensible is missing. There is thus no solution to the problem of induction, and the basic *raison d'être* for characterizing science in terms of progressively *improving* aims and methods disappears.

the aim of seeking unity, and developing new methods as the aim evolved – met with quite extraordinary scientific success. And as a result, Einstein's new methodology informs the whole of theoretical physics today. This is apparent in the immense importance given to invariance and symmetry principles such as global and local gauge invariance; it is apparent in the search for unity, the emphasis given to the search for solutions to the problems of unification.²² There is, throughout theoretical physics today, a constant fruitful interplay between evolving aims and methods, and evolving knowledge. And the point is quite general: throughout the whole of natural science, aims and methods evolve with evolving knowledge – new methods being not just new theoretical principles of the kind put forward by Einstein, but also new experimental methods, new methods for preparing material in biology, for example, or new methods of observation in astronomy. To improve aims and methods is to improve knowledge about how to improve knowledge. Thus, as knowledge improves, knowledge about how to improve knowledge improves as well. This positive feedback between improving knowledge and improving knowledge about how to improve knowledge (i. e. improving aims and methods) helps to explain the explosive growth in scientific knowledge. It is of the very essence of scientific rationality, the nub of the progress-achieving metamethods of natural science. All this illustrates, I hope it will have been noticed, the metamethodological rules of aim-pursuing rationality that I indicated earlier, rules (A), (B) and (C). The progress-achieving metamethods of natural science are a strikingly successful special case of the general conception of aim-pursuing rationality indicated above.

22 For Einstein's own account of his life-long attempt to discover the unified structure of the universe see A. Einstein, »Autobiographical Notes«, in: *Albert Einstein: Philosopher-Scientist*, ed. P. A. Schilpp (La Salle, Illinois, 1949), pp. 3–94. For a delightful, non-technical account of the role of symmetry principles in modern theoretical physics see A. Zee, *Fearful Symmetry* (New York, 1986). For a somewhat more technical discussion of gauge theory see K. Moriyasu, *An Elementary Primer for Gauge Theory* (Singapore, 1983). For a discussion of achievements and problems of unification before the advent of string theory see A. Salam, *Ideals and Realities* (Singapore, 1984), pp. 299–362.

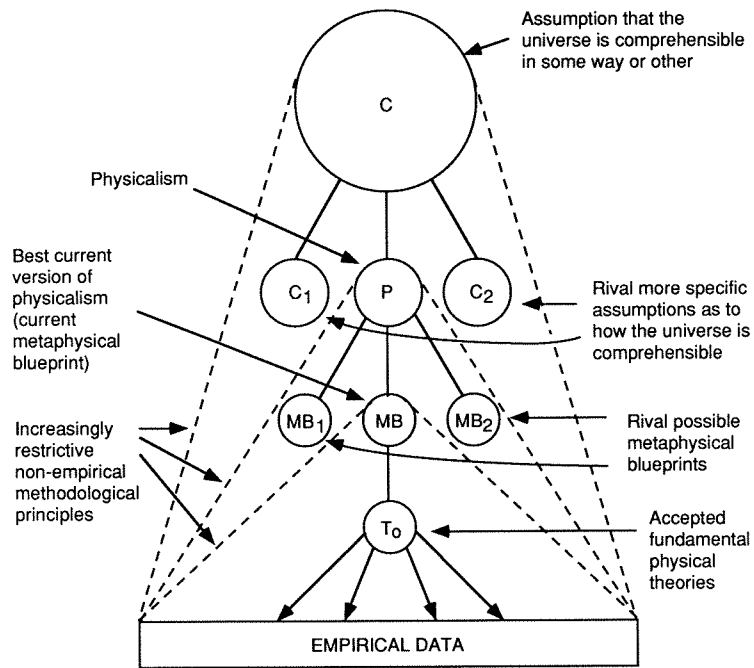


Diagram 2: *Aim-Pursuing Empiricism*

If we adopt this *aim-pursuing empiricist* conception of science (as we may call it²³), the problem of induction vanishes without trace. Any theory must, quite properly, satisfy two kinds of criteria in order to be acceptable: empirical criteria, and criteria that have to do with the theory being compatible with the best available version of the basic assumption that the universe is comprehensible (see diagram 2).

What creates the problem of induction is the stubborn rejection of the idea that natural science as a whole is entitled to have as its aim to discover in what precise way the universe is comprehensible, it being assumed that the universe is comprehensible in some way or other. For suppose we do reject this problematic aim, and accept instead the

23 Elsewhere called by me aim-oriented empiricism: see works referred to in note 20.

apparently unproblematic aim of improving knowledge about the world, nothing being presumed about the nature of the world whatsoever. We will then be obliged to uphold some version of *standard empiricism* (as it may be called), a conception of science which holds that science proceeds in accordance with 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«. ²⁴ At once we are confronted by the problem of induction in all its apparent horrifying insolubility. For how can it be possible to choose any theory in science on the basis of evidence alone when there will always be infinitely many easily formulated rival theories which will fit the evidence just as well as, if not better than, our chosen theory? How can it conceivably be rational constantly to give preference to simple theories over complex theories – which is, of course, what we do in practice in science – when the evidence is neutral between these choices or even, as is often the case, actually favours theories that are complex rather than simple? No answer can be forthcoming to these questions as long as we hold onto Popper's principle of empiricism, and the aim for science with which it is associated. What this shows, then, is that the problem of induction arises because scientists and philosophers of science have persistently *misrepresented* the proper basic aim of science. The profoundly problematic aim of discovering how the universe is comprehensible has been repressed, and replaced with the apparently unproblematic aim of improving knowledge of empirical truth. It is this that creates the problem of induction. In other words, the problem arises because of the failure to put rule (B) of aim-pursuing rationality, mentioned earlier, into practice in our thinking about science.

This, then, constitutes the first intellectual blunder of the Enlightenment: a failure to characterise accurately the progress-achieving metamethods of natural science – the metamethods of aim-pursuing empiricism, itself a strikingly successful special case of aim-pursuing rationalism.

It deserves to be noted that for aim-pursuing empiricism, discussion of rival views about aims and methods for science – that is, *the*

24 K. Popper, *Conjectures and Refutations* (London, 1963), p. 54.

philosophy of science – has an important role to play within science itself. The capacity of science to improve its aims and methods as it proceeds is absolutely essential if science is to make progress, and is behind the so far ever-accelerating rate of scientific progress. Thus, for aim-pursuing empiricism, the philosophy of science, properly understood, is an integral part of natural science itself, closely bound up with the progressive character of science. It is important to appreciate, however, that »philosophy of science« here means discussion of aims and methods of science carried on in the main by scientists themselves as an integral part of scientific research. It does not mean contemporary academic philosophy of science. This latter, on the whole, has little to offer science for the simple reason that, in the main, it takes versions of standard empiricism for granted. It misrepresents the basic aim of science and thus, in accordance with Rule (B), tends, if anything, to hinder rather than help scientific progress. The interplay of evolving knowledge and evolving aims and methods, such an important feature of the progress-achieving metamethods of science according to aim-pursuing empiricism, more or less disappears from view if we see science from the perspective of standard empiricism and orthodox academic philosophy of science. In brief, bad philosophy of science, shared to some extent by both scientists and philosophers of science, obscures the essential role that philosophy of science, properly conceived, has in helping science make progress.

How the Solution to the Problem of Induction Helps Social Inquiry

It might seem that failure to capture correctly the progress-achieving methods of natural science ought not to be too damaging for the Enlightenment project of learning from scientific progress how to achieve social progress towards a good world. Natural science, after all, has succeeded pretty well in making progress even though a good understanding of the nature of the metamethods which have made this progress possible has not, it seems, been generally available. Why should social life require a good understanding of the nature of progress-achieving metamethods if natural science does not? And in any case, is it not precisely the social sciences, at their most sterile, which

include interminable methodological discussions, whereas empirically progressive natural science avoids such discussion like the plague?²⁵

What matters, from the standpoint of making progress, is the methods we use in practice, not the methods we *think* we use, or the methods we discuss. Natural science has achieved such astonishing progress because it has *in practice* used good progress-achieving methods, even if they have not always been adequately understood. In these circumstances, inadequate understanding does not matter too much.²⁶ However, if we seek to implement the Enlightenment idea of learning from scientific progress how to achieve social progress, self-consciously seeking to transport progress-achieving methods from science to social life, it becomes a matter of vital importance that we do possess a good understanding of the methods of science that really are in practice responsible for scientific progress. Paradoxically, having a good understanding of scientific method is more important for the task of learning from science how to achieve social progress than it is for doing science itself.

Some more specific reasons for the importance of capturing the progress-achieving metameethods of science accurately for the basic

25 My answer to this question, I hope it is clear, is that the sterility of much methodological discussion associated with the social sciences is due to the fact that this entire discussion takes for granted precisely what ought to be rejected: namely that social inquiry is a kind of *science*, or at least has as its basic task to improve knowledge of social phenomena. As for the suggestion that natural science avoids discussion of methodological issues, this does not quite do justice to the actual situation: new methods, in the form of new symmetry or invariance principles, or new experimental methods, are indeed developed, assessed, and sometimes rejected in natural science (a dramatic case of rejection being provided by the downfall of parity and charge conjugation).

26 Elsewhere I have tried to explain how general acceptance, by the scientific community, of standard empiricism as opposed to aim-pursuing empiricism has had some damaging consequences for science itself: see *What's Wrong With Science?; From Knowledge to Wisdom*; »Methodological Problems of Neuroscience«, in: *Models of the Visual Cortex*, ed. D. Rose and V. G. Dobson (Chichester, 1985), pp. 11–21; and »Quantum Propensiton Theory: A testable Resolution of the Wave/Particle Dilemma«, *British Journal for the Philosophy of Science* 39 (1988), pp. 1–50.

Enlightenment project will emerge as we proceed. But even taking these into account, I must admit that the first intellectual blunder of the Enlightenment was not perhaps in itself too damaging for the task of creating a kind of inquiry designed to help build a better world. It was the second blunder that had the more damaging consequences.

This second more serious blunder was to try to apply scientific methods, not to social life itself, but to the task of improving *knowledge* of social life, to the task of creating social science. The outcome, as I have already indicated, was the subsequent development of social inquiry as social *science*. This in turn had the consequence that academic inquiry as a whole, as it developed during the 19th and 20th centuries, came to be restricted, by and large, to the pursuit of knowledge. Instead of the rational exploration of problems of living being put at the intellectual heart of academic inquiry as a whole, discussion of problems of living was excluded from the intellectual domain. What inquiry most needs to do if it is to devote itself to helping us build a better world becomes a prohibited academic activity. Furthermore, academic inquiry develops the wrong kind of relationship to the rest of the social world: social science *studies* aspects of the social world but cannot devote itself to helping us learn how to resolve our human conflicts and problems of living by cooperatively rational means. In addition, social inquiry develops the wrong kind of relationship to natural science; instead of being, in an important sense, intellectually more fundamental and central than natural science, social inquiry developed as social science, becomes less fundamental, peripheral, of dubious intellectual standing.

The Counter-Enlightenment proposal that social inquiry should be developed as the *non-scientific* study of society and culture does not really help. For this concedes what ought not to be conceded: that the main task of social inquiry is to improve knowledge and understanding of the human world. And even worse, the Counter-Enlightenment fails to appreciate that what is wrong with the Enlightenment is not the basic idea of learning from scientific progress how to achieve social progress; what is wrong, rather, is the bungled way in which this profoundly important idea was implemented.

What the philosophes *ought* to have done, of course, was to have tried to get an appropriately generalized version of the progress-achiev-

ing metamethods of science applied to *social life itself*. If our concern is to learn from scientific progress how to make social progress then it is above all our social lives, our actions, our attempts to solve our problems of living which need to exploit and implement appropriately generalized intellectual standards and methods of natural science.²⁷ We need to build these standards and methods into the structure of all our institutions, so that government, the law, the media, industry and so on may make progress towards the problematic aim of enlightenment in the kind of way in which the institution of science makes progress towards the problematic aim of understanding the universe (the latter aim being itself an aspect of enlightenment). In brief, social inquiry needs to be developed in the way I have already indicated, as social metamethodology or social philosophy, having as its goal to help us put the rules of aim-pursuing rationality into practice in our lives, rules that have been exploited so successfully by science.

27 The modern philosopher who has come closest to developing the version of the Enlightenment idea that I am advocating is undoubtedly Karl Popper. The conception of scientific method advocated in *The Logic of Scientific Discovery*, namely *falsificationism*, has been generalized by Popper to form a general notion of rationality, namely *critical rationalism*, which has in effect been applied, to striking effect, to fundamental problems of political thought in his *The Open Society and Its Enemies*. It is quite clear that Popper himself sees his own work in these terms (even though he does not make the point, to my knowledge, that this can be regarded as an improved version of the basic Enlightenment idea). Thus having remarked, in *The Logic of Scientific Discovery*, that the objectivity of scientific statements lies in their inter-subjective testability, Popper adds a footnote, in the English edition first published in 1959, which asserts: »I have since generalized this formulation; for inter-subjective *testing* is merely a very important aspect of the more general idea of inter-subjective *criticism*, or in other words, of the idea of mutual rational control by critical discussion. This more general idea [is] discussed at some length in my *Open Society and Its Enemies*, chapters 23 and 24« (p. 44). There are however three basic defects in the way Popper implements the basic idea. First, as we have seen, and despite his claims to the contrary, Popper does not succeed in solving the problem of induction; his conception of scientific method is seriously inadequate, and this leads to an inadequate conception of rationality. (Criticism is important, but it is not the whole of reason; in some circumstances, such as personal relationships, uncritical criticism can do

If the philosophes had managed to develop their basic idea in the way that I have just indicated, avoiding the two blunders that I have discussed, then they would have begun to create, in the 18th century, what we still lack today: a kind of organized inquiry rationally designed to help us build a better world.

In order to put the basic Enlightenment idea into practice successfully, it is important, for a number of reasons, to take as one's starting point the Einsteinian, aim-pursuing empiricist conception of scientific method that I have outlined above. For it stresses that in order to pursue diverse aims we need to adopt diverse methods – unity of method being found at the metamethodological level; it stresses the profoundly problematic character of the basic aim of science, and the need, therefore, to improve aims and methods as one proceeds; and it emphasizes the central importance of acknowledging problematic aims honestly, and not mis-

more harm than good. There is, besides, in Popper's work no recognition of the important point that rationality needs to be characterised at the metamethodological level, rationality being bound up with how we go about *improving* our aims and methods.) Second, Popper fails completely to advocate that social inquiry should be developed as *social methodology*, having as its basic task (in Popperian terms) to apply critical rationalism (generalized from falsificationism) to the problems of social life. On the contrary, in *The Open Society and Its Enemies*, and in the companion volume *The Poverty of Historicism* (London, 1957), Popper defends a thoroughly traditional conception of social inquiry as *social science* with methods essentially similar to those of natural science. To this extent, Popper commits the two blunders of the Enlightenment. Third, in these two books Popper presents us with a stark choice between piecemeal and Utopian social engineering, thereby in effect annihilating the very possibility of a *third* kind of radical, long-term, global cooperative action, which only becomes possible if there is prior sustained, imaginative and critical exploration of possible global actions or strategies, radical and long-term in character. (Cooperative action of this type would be neither piecemeal nor Utopian social engineering.) It may well be that we need to develop the capacity to engage in just such long-term cooperative action in order to overcome some of our global problems – environmental problems, for example. One important task for the kind of inquiry I have advocated here is to make such cooperative action possible (excluded *a priori* by Popper). For a more detailed discussion of this important point, see my *From Knowledge to Wisdom*, pp. 189–199.

representing them. All these points, important for science, are vitally important for life – and especially for tasks associated with trying to help build a better world.

There is an additional point. Social inquiry, I have argued, is fundamentally social philosophy, having the same sort of relationship to social life that philosophy of science has to science. But there is an obvious objection to this proposal. Philosophy of science has contributed little, if anything, to the progress of science: why should we expect philosophy of social life to contribute anything more to the progress of social life? As long as we hold onto the orthodox standard empiricist conception of science, which misrepresents the basic aim of science and fails to solve the problem of induction, this objection must be deemed to be valid. But, as we have seen, the moment we accept the more adequate aim-pursuing empiricist conception of science, which represents honestly the profoundly problematic basic aim of science, solves the problem of induction and thus succeeds in exhibiting science as rational, then it becomes clear that philosophy of science, properly conceived, has an important role to play in helping science make progress. We thus have good grounds for holding that social philosophy, too, will have a role to play in helping social life make progress towards an enlightened world. In both cases, because profoundly problematic aims are being pursued, we need to improve aims and methods as we proceed, as we live. In brief, we require the solution to the problem of induction in order to bring into sharp focus the way in which the paradigmatic core of social philosophy (namely, philosophy of science) helps that fragment of social life with which it deals (namely, natural science) make progress towards enlightenment (in this case that aspect of enlightenment that has to do with human knowledge and understanding of the natural world).

In important respects, the kind of inquiry that emerges as a result of implementing the Enlightenment idea in the way indicated here unifies the best aspects of Rationalism and Romanticism, the traditional Enlightenment and the Counter-Enlightenment. Motivational honesty, honesty about aims and desires, is central to the Romantic conception of intellectual integrity: it is just this that we need, as we have seen, in order to solve the problem of induction, the fundamental problem of the rationality, the intellectual integrity of science. Romanticism stresses

the value of imagination, art, the expression of feelings, desires, aspirations: all these have a vital role to play within the kind of inquiry I have advocated here. This kind of inquiry does full justice both to the value of trying to understand the nature of this utterly impersonal, mysterious cosmos we find ourselves in, and to the value of trying to improve our empathic understanding of our fellow human beings, these two kinds of understanding being interlinked. I have, in effect, in principle, solved C. P. Snow's problem of the two cultures,²⁸ and in just the sort of way Snow would have approved of: by indicating a kind of inquiry which calls upon both science and art, knowledge and empathic understanding in order to help us realize what is of value in our lives, a good world.²⁹

What I have been advocating can, then, be summed up like this. In order for that which is of most value actually and potentially in existence to flourish, we need to try cooperatively to improve our aims and methods as we live – attempting, in this way, to put aim-pursuing rationality into practice in our world. Cooperative aim-pursuing rationality 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 theories are critically assessed with respect to each other, 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, 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

28 C. P. Snow, *The Two Cultures and A Second Look* (Cambridge, 1964).

29 Unifying what is best in traditional Rationalism and Romanticism is one of the central themes of my *What's Wrong With Science?* and *From Knowledge to Wisdom*.

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 can arise, 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 may uncritically reinforce each other to produce dogmatism. The solution in both cases is to consider a number of rival ideas (theories or philosophies) there being tripartite assessment between idea, idea and experience. For this to occur, in science or in life, empathic 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.

As a result of putting aim-pursuing empiricism into practice in science we have achieved explosive growth in our knowledge and understanding of the natural world. Is it possible that, as a result of putting aim-pursuing rationalism (the generalized version of aim-pursuing empiricism) into practice in our personal, social and global lives, thus bringing into existence the kind of inquiry I have been advocating here, we could achieve a comparable kind of explosive growth in personal and global enlightenment? Could social progress towards a good world accelerate rapidly, rather like scientific progress, so that war, tyranny, third world poverty, environmental degradation rapidly become no more than nightmarish memories from the past?

Who can say? I, for one, would not like to place limits on what humanity could achieve by way of enlightenment. There are, however, important differences in what is involved in achieving scientific and social progress. In order to achieve scientific progress all we require, strictly speaking, is sufficient funds, and a hundred thousand or so highly talented, motivated and rewarded individuals to do the research; for social progress towards enlightenment, we need almost everyone to be involved. In doing natural science we are in the main free to perform endless experiments without worrying too much about what

the outcome will be; if things do not turn out the way we expect, no one will suffer. In social life, experiments that do not turn out as expected can cause great suffering, and may have irreversible adverse consequences for subsequent life. In natural science it is usually fairly clear whether a theory has been corroborated or refuted by an experiment: in social life it is by no means so clear whether a proposed policy, or political or economic programme has met with success or failure when the attempt is made to put it into practice. Again, there is usually agreement as to what is to count as success or failure in natural science: in social life it is just here that substantial disagreements arise. These differences make it much more difficult to implement problem-solving rationality and aim-pursuing rationality in personal and global life than in science. But this ought not to discourage us from making the attempt. Quite to the contrary, we have here all the more reason for taking very seriously indeed the task of attempting to put the strategies of reason into practice in life.

Conclusion

In order to create a better world, we need to *learn* how to do it. We need to learn how to resolve our conflicts and problems of living in more cooperatively rational ways than we do at present. And in order to do this, we need traditions and institutions of learning rationally devoted to this end. In our vast, complex, diverse, rapidly changing world, charged with conflict and injustice, there is very little hope indeed that we will discover how to resolve our conflicts and problems in more peaceful, just and cooperative ways than at present unless we have good traditions and institutions of inquiry designed to help us learn how to do this. Cooperative action is scarcely conceivable without cooperative discussion. When viewed from this standpoint, what we have at present – academic inquiry devoted primarily to the pursuit of knowledge and technological know-how – is seriously inadequate and damagingly irrational. We urgently need a new and more rational kind of inquiry which gives intellectual priority to the tasks of articulating our problems of living, and proposing and critically assessing possible cooperative solutions, problems of knowledge and technolog-

ical know-how being tackled in an intellectually subordinate way. In order to develop this urgently needed kind of inquiry we will need to change almost every branch and aspect of the academic enterprise. Above all, we will need to change social inquiry and the humanities so that they take up their proper task, at the intellectual heart of inquiry, of helping humanity resolve its conflicts and problems of living in increasingly cooperative ways – the tasks of the natural and technological sciences being subordinate and secondary. This new kind of inquiry would do better justice to both practical and cultural dimensions of inquiry.

Zusammenfassung

Um eine bessere Welt zu schaffen, muß man erst *lernen*, wie das zu machen ist. Wir müssen lernen, wie wir unsere Konflikte und Lebensprobleme auf eine kooperativere und rationalere Weise lösen können, als es heute der Fall ist. Um dies durchzuführen, brauchen wir Traditionen und Organisationen im akademischen Bereich, die sich diesem Zweck rational widmen. In unserer großen, komplexen, mannigfaltigen und sich schnell verändernden Welt, voll von Konflikten und Ungerechtigkeiten, gibt es nur sehr wenig Hoffnung, daß wir unsere Konflikte und Probleme auf eine friedlichere, gerechtere und kooperativere Weise lösen können, es sei denn, wir hätten gute Traditionen und Forschungsinstitutionen, die konzipiert wären, um uns diesbezüglich zu befähigen. Eine kooperative Arbeit ist ohne eine kooperative Diskussion kaum denkbar. Von diesem Standpunkt aus betrachtet, ist unser aktuelles System – akademische Forschung, die an erster Stelle der Suche nach Wissen und technischem Können dient – sehr mangelhaft und auf gefährliche Weise unüberlegt. Wir brauchen dringend eine neue und rationalere Forschungsart. Diese sollte sich in erster Linie mit den Aufgaben befassen, unsere Lebensprobleme zu artikulieren und mögliche gemeinschaftliche Lösungen zu entwickeln und kritisch zu bewerten. Währenddessen kann den Fragen vom Wissen und vom technischen Können auf einer intellektuell untergeordneten Weise nachgegangen werden. Um diese dringend nötige Forschungsart zu entwickeln, wird es nötig sein, fast jedes Gebiet und jeden Aspekt des aka-

demischen Bereiches zu ändern. Vor allem müssen wir die Sozial- und Geisteswissenschaften so ändern, daß sie ihren wahren Aufgaben nachgehen können, um als zentraler Teil der Forschung die Konflikte und Lebensprobleme der Menschheit auf eine zunehmend kooperative Art zu lösen – die Aufgaben von den Naturwissenschaften und technischen Wissenschaften sind dann sekundär und untergeordnet. Diese neue Art von Forschung wäre den praktischen und kulturellen Dimensionen der Wissenschaft angemessener.