

IS SCIENCE NEUROTIC?

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Contents

Part I: Exposition

- 1 Rationalistic Neurosis
- 2 The Neurosis of Natural Science
- 3 Does the Neurosis of Science Matter?

Part II: Implications for Natural Science

- 1 Rational Scientific Discovery
- 2 The Philosophy of Science
- 3 Science and Values
- 4 Science and Politics

Part III: Implications for Social Inquiry

- 1 The Enlightenment Programme
- 2 The Damaging Neurosis of Social Science
- 3 Philosophy and Sociology of Science
- 4 Academic Neurosis
- 5 Philosophical Neurosis
- 6 Self-Preservation of Institutional Neurosis
- 7 The Neurosis of Psychoanalytic Theory and Practice
- 8 Conclusion

Notes

References

Is Science Neurotic?

Abstract

Neurosis can be interpreted as a methodological condition which any aim-pursuing entity can suffer from. If such an entity pursues a problematic aim B, represents to itself that it is pursuing a different aim C, and as a result fails to solve the problems associated with B which, if solved, would lead to the pursuit of aim A, then the entity may be said to be "rationalistically neurotic". Natural science is neurotic in this sense in so far as a basic aim of science is represented to be to improve knowledge of factual truth as such (aim C), when actually the aim of science is to improve knowledge of explanatory truth (aim B). Science does not suffer too much from this neurosis, but philosophy of science does. Much more serious is the rationalistic neurosis of the social sciences, and of academic inquiry more generally. Freeing social science and academic inquiry from neurosis would have far reaching, beneficial, intellectual, institutional and cultural consequences.

Is Science Neurotic? Part I: The Natural Sciences

1 Rationalistic Neurosis

It seems, on the face of it, absurd to suggest that science is neurotic. Some *people* may, perhaps, be neurotic; even the odd pet. But how can a vast, impersonal intellectual endeavour like science be called neurotic? Is not this to attribute a mind to science, an ego, id and superego? What could be more nonsensical?

But let us consider a classic example of neurosis: the Oedipus complex, for example. A boy loves his mother, and as a result is furiously jealous of, and hates, his father. But his father is big and powerful, and not easy to get rid of; and besides the boy also loves his father. So the hatred is suppressed (Freud 1962, 77-78, 125-126). Nevertheless it persists into adult life, and one day, purely by accident, while caring lovingly for his elderly and ill father, the son mixes a lethal dose of medicine, and finally succeeds in fulfilling his long-suppressed desire. But the act is rationalized away as a ghastly accident.

Put in a more abstract way, what one has here is something like the following. The son, whatever else he may be, is a being with aims, whether acknowledged or suppressed. There is a basic desire or aim, A: to love his mother. There is a secondary, highly problematic, suppressed aim, B: to kill his father. There is a third, declared, but somewhat unreal aim, C: to love, to care for, his father. The son supposes himself to be pursuing aim C, while in reality pursuing aim B: actions performed in pursuit of B (administration of a lethal dose of medicine) are rationalized in terms of the pursuit of C (it was an accident): see diagram 1.

The advantage of construing the Oedipus complex as a very special case of something much more general, namely the pursuit of problematic, repressed (or unacknowledged) aims under the smokescreen of apparently pursuing some unproblematic, acknowledged aim, is that it becomes possible to attribute neurosis to *anything* that can be construed (1) to pursue aims more or less successfully, (2) represent (to itself or to others) the aims it pursues, and (3) almost inevitably *misrepresent* the aims that it is pursuing.

The aim-pursuing thing might be a person; or it might be an animal, a robot, a group of people, or an institution or movement in so far as these can be construed to be aim-pursuing entities.

Neurosis, as I have sketchily characterized it above, is a condition that almost *any* aim-pursuing entity is likely to fall into, in so far as it is sufficiently sophisticated to represent, and hence misrepresent, the aims that it is pursuing. Neurosis, conceived of in this way, is not a sickness of the psyche, the mind or the id; it does not require that there are mental acts of repression and rationalization; it does not presuppose, even, that the thing that suffers from neurosis is conscious or has a mind, not even in the sense that animals can be said to be conscious, or at least sentient. All that is required is that the thing in question pursues aims, represents the aims that it pursues, and hence on occasion misrepresents its aims. (At the very least we require that the thing in question can legitimately be *construed* to be aim-pursuing in this way.)

The notion of neurosis that I have indicated might be called "rationalistic neurosis" to distinguish it from Freudian or psycho-analytic notions. Rationalistic neurosis is a methodological notion, a notion that belongs to the theory of rational aim-pursuing.¹ It is especially damaging from the standpoint of rationality because, as the term "rationalization" implies, it subverts reason. Once a being has fallen into the pattern of confusion of rationalistic neurosis, "reason" becomes a hinderance instead of a help. The more "rationally" the being pursues its declared, false aim C, the worse off it is from the

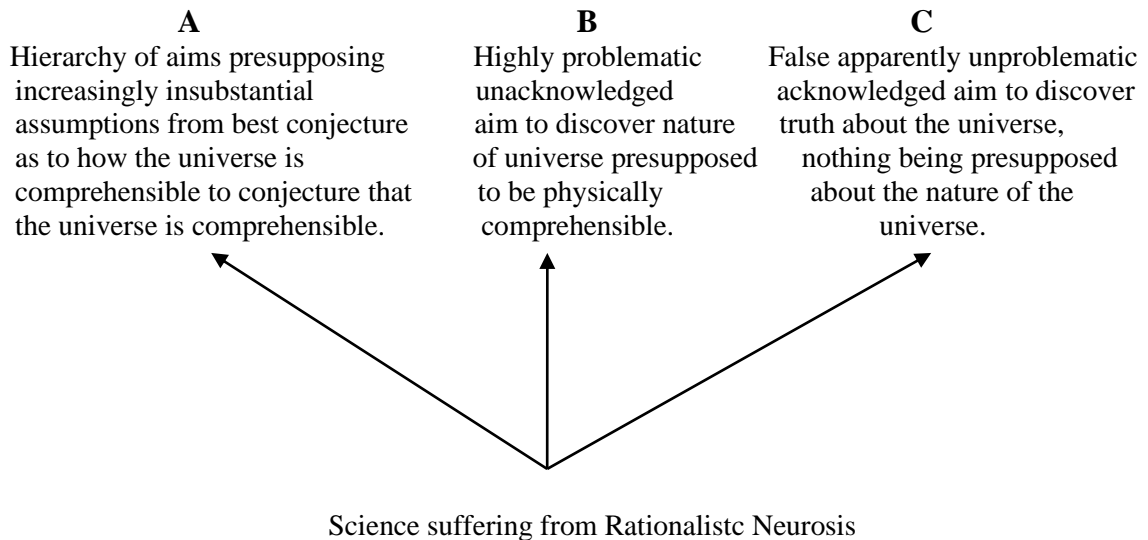
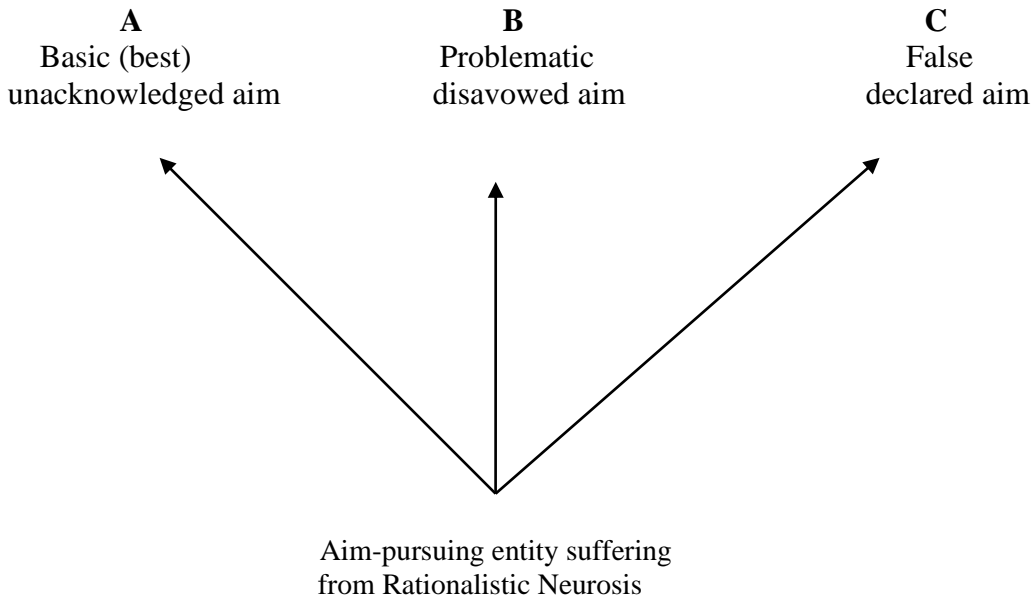
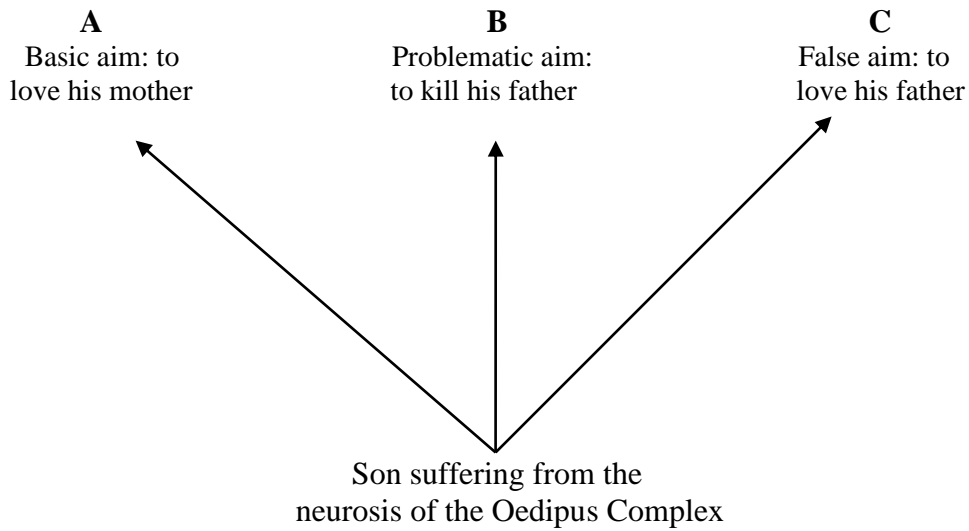


Diagram 1: From the Oedipus Complex to the Neurosis of Science

standpoint of pursuing its real, problematic aim B, the further away it is from *solving* the problems associated with the aim B, thus coming to pursue the really desirable aim A. The more "rationally" the being pursues its declared aim, the more unsuccessful, in real terms, it will be; in order to achieve real success the being must act "irrationally". Not only does this subversion of reason block progress; it has the added disadvantage of bringing reason into disrepute. Reason appears to block, rather than aid, progress.

Science is an institutional endeavour that pursues aims; it is certainly sufficiently sophisticated to represent its aims, both to itself and to the public, in terms of its official "philosophy" (a philosophy of science being a view about what the aims and methods of science are, or ought to be). Thus, in terms of our new notion, it certainly makes sense to declare that science suffers from rationalistic neuroses. But is it true? I now proceed to demonstrate that it is.

2 The Neurosis of Natural Science

For science² to suffer from rationalistic neurosis, all that we require is that the real, problematic aim of science, B, differs from the official, declared aim, C. Just this is the case. A real aim of science, B, problematic and hence repressed, is to discover in what way the universe is comprehensible, it being presupposed from the outset that the universe is comprehensible (to some extent at least).³ Acknowledging this aim involves acknowledging that science accepts, as an article of faith, that the universe *is* comprehensible (to some extent at least). But on what basis can this be known? To accept this substantial thesis about the nature of the universe as an article of faith makes science look more like a religion than the official view about the nature of science. The aim is too problematic to be officially acknowledged, and hence is repressed, or disavowed.

Instead, the scientific community holds, officially as it were, that the basic intellectual aim of science, C, is to discover factual truths about the universe, nothing being permanently presupposed about the nature of the universe independently of evidence.⁴ This declared, official aim seems unproblematic; adopting it does not commit science to making some vast assumption about the nature of the universe, independently of the evidence. Adopting this aim enables scientists to hold on to the official view that the essential thing about science - that which distinguishes science from religions and other enterprises - is that in science claims to knowledge, laws and theories, are accepted and rejected impartially on the basis of evidence, *no thesis about the nature of the universe being accepted permanently as a part of scientific knowledge independently of empirical considerations* (see diagram 1). According to this view, considerations that have to do with simplicity, unity or explanatory power may influence choice of theory, in addition to empirical considerations; this must not, however, commit science to making the permanent assumption that the universe itself is simple, unified or comprehensible.

But this official philosophy of science, which I shall call *standard empiricism* (SE), taken for granted by scientists and non-scientists alike, is untenable.⁵ Elementary considerations show that science cannot possibly have the specified aim, and cannot possibly proceed in the specified way.

Given any scientific theory, however well verified empirically, there will always be infinitely many rival theories, equally well supported by the evidence, which make different predictions, in an arbitrary way, for phenomena not yet observed. Thus, given Newtonian theory (NT), one rival theory might assert: everything occurs as NT asserts up till midnight tonight when, abruptly, an inverse cube law of gravitation comes into operation. A second rival theory might assert: everything occurs as NT asserts, except for the case of any two solid gold spheres, each having a mass of a 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. A third rival asserts that everything occurs as NT asserts until thirty tons of gold dust and thirty tons of diamond dust are heated in a platinum flask to a temperature of 500°C, in which case gravitation will instantly become a repulsive force everywhere. There is no limit to the number of rivals to NT that can be concocted in this way, each of which has all the predictive success of NT as far as observed phenomena are concerned but which makes different predictions for some as yet unobserved phenomena. Such theories can even be concocted which are *more* empirically successful than NT, by adding onto NT independently testable and corroborated laws, or by arbitrarily modifying NT, in this entirely *ad hoc* fashion, so that the new theory yields correct predictions where NT does not, as in the case of the orbit of Mercury for example (which very slightly conflicts with NT).

One can set out to refute these rival theories by making the relevant observations or experiments, but as there are infinitely many of them this may take some time. In short, if science really did take seriously the idea that theories must be selected on the basis of evidence alone, science would be swamped by an infinity of empirically equally successful rival theories; science would come to an end.

This does not happen in scientific practice because, in practice, given an accepted, well verified theory, such as Newtonian theory, quantum theory, or general relativity, almost all the infinitely many equally empirically successful rival theories are, in comparison, grotesquely *ad hoc* and disunified. Such theories are, in practice, excluded from scientific consideration on the grounds that they violate symmetry principles, they lack simplicity, unity or explanatory power.

Now comes the crucial point. In persistently excluding infinitely many such empirically successful but grotesquely *ad hoc* theories, science in effect makes a big assumption about the nature of the universe, to the effect that it is such that no grotesquely *ad hoc* theory is true, however empirically successful it may appear to be for a time. Without some such big assumption as this, the empirical method of science collapses. Science is drowned in an infinite ocean of empirically successful *ad hoc* theories.

The idea that science has the aim of improving knowledge of factual truth, *nothing being presupposed about the nature of the universe independently of evidence* is thus untenable.⁶ The academic discipline of the philosophy of science, in so far as it seeks to justify science in terms of this (declared, false) aim, is engaged in a deeply neurotic activity. It is providing, not reasons for the actions of scientists, but rationalizations. The more nearly science conforms to the edicts of such philosophers of science, the more unsuccessful science becomes. (This accounts for the uselessness of much academic philosophy of science for science itself, a point sometimes made by working scientists, as we shall see below.)

In order to do justice to scientific practice we must acknowledge the real intellectual aim of science: to improve knowledge about the universe presupposed to be physically comprehensible to the extent at least that there is some yet-to-be-discovered, true, physical theory-of-everything, T, which is at least not grotesquely *ad hoc* like the *ad hoc* versions of Newtonian theory considered above.⁷

Granted this aim, the problem of how and why empirically successful *ad hoc* theories are to be excluded from scientific consideration disappears. Such theories clash with the basic presupposition that the universe is physically comprehensible, at least to the extent that it is not grotesquely *ad hoc*, and are to be dismissed on that account. The problem of excluding empirically successful *ad hoc* theories from science - in effect the problem of induction - turns out to be a typically *neurotic* problem. It only arises because the basic aim of science is misidentified. If the aim of science is misidentified as that of improving knowledge about the universe, nothing being presupposed about the nature of the universe, then all theories equally successful empirically must be treated equally, infinitely many *ad hoc* rival theories must be taken seriously, all theoretical knowledge

disappears, and the full horror of the problem of induction destroys science. Identify the aim of science properly, and these neurotic horrors vanish.

But two big new problems emerge instead. First, granted that the intellectual aim of science presupposes that the universe is physically comprehensible to some extent, what exactly ought this (untestable, metaphysical) presupposition to be? Second, what possible justification can there be for just accepting, as a basic, permanent part of scientific knowledge, that the universe is physically comprehensible, in the chosen sense? I take these two problems in turn.

There is a vast spectrum of assumptions that may be made concerning the comprehensibility of the universe, from the rather vague and minimal at one end of the spectrum, to the much more precise and contentful at the other end. I have already indicated a rather weak assumption: the universe is such that the true theory-of-everything, T, is at least not grotesquely *ad hoc* in the senses indicated above (with N not too large). Even weaker assumptions are possible. One possibility is: the universe is such that local observable phenomena occur, most of the time, to a high degree of approximation, in accordance with some T that is not grotesquely *ad hoc*. Near the other end of the spectrum we have the assumption that the universe is perfectly comprehensible. For perfect physical comprehensibility we require that the universe is made up of just one physical entity (or one kind of entity⁸), a field perhaps, underlying all fundamental physical particles and forces; we require that this entity interacts with itself in a fixed, uniform way, the interactions resulting in the diverse, changing phenomena we observe, all such phenomena being, in principle, explainable and understandable in terms of the one, basic entity (plus variable initial conditions of this entity). In short, for perfect physical comprehensibility we require that T is unified in the sense that it attributes to the universe a unified dynamic structure.

As an example of perfect comprehensibility in this sense, consider a universe that consists of nothing but the classical electromagnetic field in the vacuum (there being no charged particles to create, or be acted on, by the field). In this rather bleak universe, change and diversity is restricted to varying values of the electric and magnetic fields, which differ from place to place and time to time. The physical *something* that does not change, but which determines all change, is that property of the electromagnetic field, the same everywhere, which determines that the electric and magnetic fields change in accordance with Maxwell's equations for the classical electromagnetic field in the vacuum.

Why is it legitimate in this case to regard the field as one *unified* entity, the electromagnetic field, and not two distinct entities, the electric field and the magnetic field? In part unity arises from the *symmetrical* way in which changes in the electric field produce a magnetic field, and changes in the magnetic field produce an electric field. But even more important, unity arises from the fact that the way the electromagnetic field divides up into the electric and magnetic fields differs for different reference frames travelling at uniform velocity with respect to each other. (Ignore the awkward point that this universe does not contain reference frames.) But, according to Einstein's special theory of relativity, nothing of absolute, or theoretically fundamental, significance can depend on choice of reference frame. We cannot regard the electromagnetic field as being made up of two distinct fields, the electric and magnetic fields, because any specific choice of electric and magnetic field would be arbitrary, in that it would amount to an arbitrary choice of reference frame. In short, the electromagnetic field is unified because it exhibits the *symmetry*, postulated by special relativity, of Lorentz invariance.

This last point can be stated more generally. If a number of apparently distinct fundamental physical entities are related to one another by means of a symmetry principle, analogously to the way the electric and magnetic fields of classical electromagnetism are related to each other by means of the symmetry principle of Lorentz invariance, then these entities can legitimately be regarded as diverse aspects of

one *unified* entity. Symmetry is thus an important feature of unity, and thus of physical comprehensibility.⁹

Given perfect comprehensibility, we can depart from it (in thought) in a number of related ways, and to different degrees. Given a perfectly comprehensible universe with N entities unified by a symmetry, we can alter the properties of some of these entities so that they are no longer unified. We can introduce physical entities with quite different dynamical properties, as when electrically charged particles are introduced into the universe that consists only of the electromagnetic field. We can, in short, increase the number of different sorts of fundamental entities - fields, particles or forces. More drastically, we can arrange that dynamical laws change as values of variables change, such as mass, relative velocity, or even spatial position or time: in this way we concoct grotesquely *ad hoc* universes of the kinds considered above. We can introduce spatially and temporally restricted objects with unique dynamical properties, the effect of which will be to mimic universes grotesquely *ad hoc* in space and time.

At the other end of the spectrum, even more precise and contentful assumptions than perfect comprehensibility are available. We can make diverse assumptions about the specific way the universe is comprehensible; we can assume that the one, unified entity is a classical or quantum field, or a quantum superstring field perhaps.¹⁰

Not only do we have the problem of choosing between this vast range of possible assumptions; even more serious, we have the problem of justifying acceptance of our choice as a secure part of scientific knowledge.

Confronted by these two problems of choice and justification, it may seem that the task of solving these problems is hopeless, and we would be better off returning to the orthodox view that the basic intellectual aim of physics makes no kind of metaphysical assumption about the nature of the universe at all. This would seem to be the attitude of the scientific community. But it is just here that the scientific community makes its big mistake. Whereas the traditional (neurotic) problem of induction (arising from misconstruing the aim of science) is insoluble, the new problems, that arises from acknowledging the real (repressed) aim of science, can be solved! This is the great advantage of freeing oneself from (rationalistic) neurosis. As long as one's real, problematic aim is repressed, one cannot tackle the problems associated with the aim: acknowledge the aim, and one can begin to tackle the problems associated with it.

The solution to the problems, as expounded in some detail in (Maxwell, 1998, chs. 1 and 3-6) can be put like this. Cosmological speculation about the ultimate nature of the universe, being necessary for science to be possible at all, must be regarded as a part of scientific knowledge itself, however epistemologically unsound it may be in other respects. The best such speculation available is that the universe is comprehensible in some way or other and, more specifically, in the light of the immense apparent success of modern natural science, that it is (perfectly) physically comprehensible. But both these speculations may be false; in order to take this possibility into account, we need to construe science as adopting, as a part of scientific knowledge, a hierarchy of cosmological assumptions about the comprehensibility and knowability of the universe, these assumptions asserting less and less about the universe as one ascends the hierarchy, thus being more and more likely to be true: see diagram 2. Corresponding to these cosmological assumptions there are methodological rules (not represented in the diagram) which govern acceptance of assumptions lower down in the hierarchy, and which, together with empirical considerations, govern acceptance and rejection of scientific theories. The top two assumptions, at levels 10 and 9, are such that accepting these assumptions as a part of scientific knowledge can only aid, and can never damage science (or the task of acquiring knowledge more generally) whatever the universe may be like. These are justifiably permanent items of scientific knowledge. Thus at level 10 we have the thesis that the universe is such that we can acquire some knowledge of our local

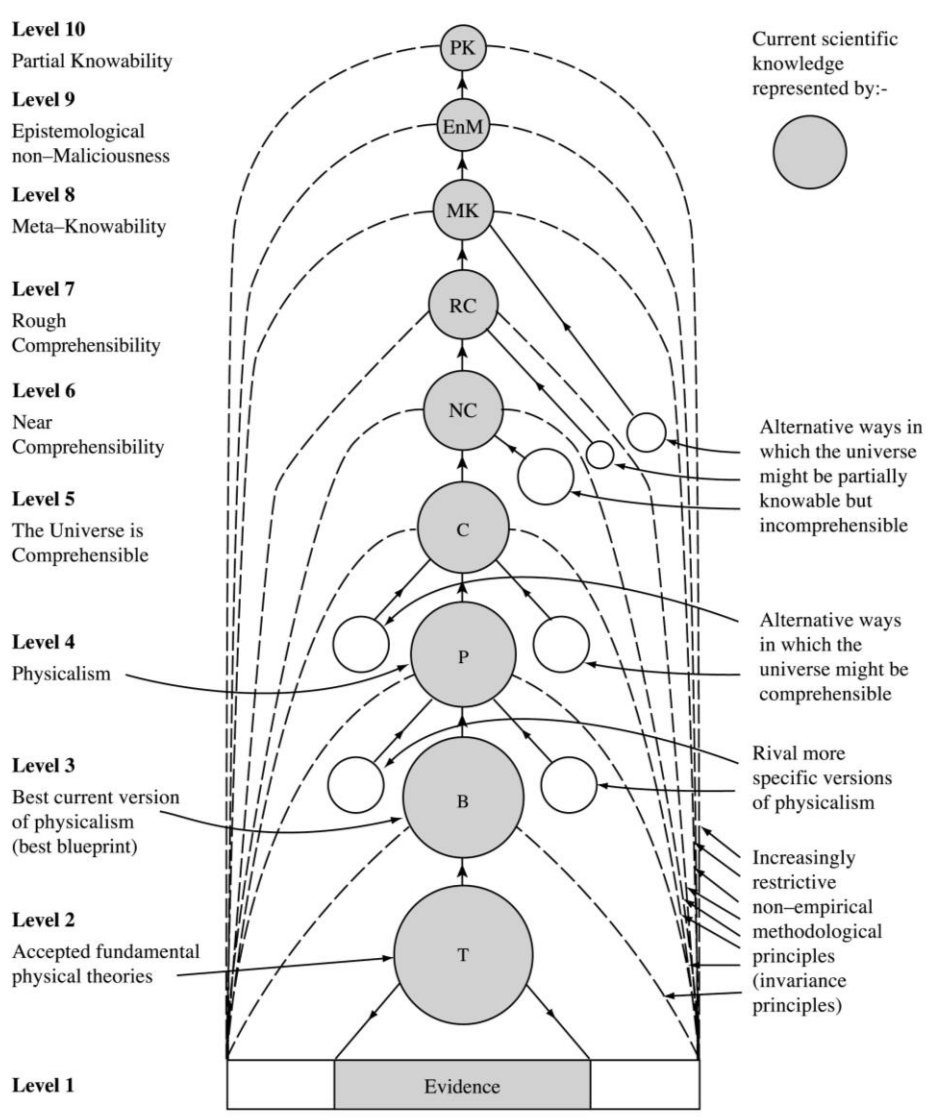


Diagram 2: Aim-Oriented Empiricism
(Enlarge to View)

circumstances: we are justified in accepting this as a permanent part of scientific knowledge. As we descend, from level 8 to level 3, the corresponding theses make increasingly substantial assertions about the nature of the universe: it becomes increasingly likely that these theses are false. At each level, from 8 to 3, we adopt that assumption which (a) is compatible with the assumption above it in the hierarchy (in so far as this is possible), and (b) holds out the greatest hope for the growth of empirical knowledge, and seems best to support the growth of such knowledge (at levels 1 and 2). If currently adopted cosmological assumptions, and associated methods, fail to support the growth of empirical knowledge, or fail to do so as apparently successfully as rival assumptions and methods, then assumptions and associated methods are changed, at whatever level appears to be required.¹¹ In this way we give ourselves the best hope of making progress, of acquiring authentic knowledge, while at the same time minimizing the chances of being taken up the garden path, or being stuck in a cul de sac. The hope is that as we increase our knowledge about the world we improve the cosmological assumptions implicit in our methods, and thus in turn improve our methods. As a result of improving our knowledge we improve our knowledge about how to improve knowledge. Science adapts its own nature to what it learns about the nature of the universe, thus increasing its capacity to make progress in knowledge about the world - the methodological key to the astonishing, accelerating progress of modern science.

This conception of science, postulating more or less specific evolving aims and methods for science within a framework of more general fixed aims and methods, I call *aim-oriented empiricism* (AOE).¹² The basic idea, let me re-emphasize, is that the fundamental aim of science of discovering how, and to what extent, the universe is comprehensible is deeply problematic; it is essential that we try to improve the aim, and associated methods, as we proceed, in the light of apparent success and failure. In order to do this in the best possible way we need to represent our aim at a number of levels, from the specific and problematic to the highly unspecific and unproblematic, thus creating a framework of fixed aims and meta-methods within which the (more or less specific, problematic) aims and methods of science may be progressively improved in the light of apparent empirical success and failure.

All this is a special case of a more general idea of *aim-oriented rationality* (to be discussed in Part III of the present paper), according to which, whenever basic aims are problematic (as they usually are in science and in life) we need to display aims at distinct levels of specificity and generality, thus creating a framework within which we have the best chance of improving more or less specific, problematic aims-and-methods as we proceed, in the light of success and failure.

According to AOE, then, scientific knowledge can be represented (in a highly schematic and simplifying way) as being made up of the following ten levels: see diagram 2. At level 1, we have empirical data (low level experimental laws). At level 2, we have our best fundamental physical theories, currently general relativity and the so-called standard model. At level 3, we have the best, currently available specific idea as to how the universe is physically comprehensible. This asserts that everything is made of some specific kind of physical entity: corpuscle, point-particle, classical field, quantum field, convoluted space-time, string, or whatever. Because the thesis at this level is so specific, it is almost bound to be false (even if the universe is physically comprehensible in some way or other). Here, ideas evolve with evolving knowledge. At level 4 we have the much less specific thesis that the universe is (perfectly) physically comprehensible in some way or other; and at level 5 we have the even less specific thesis that the universe is comprehensible in some way or other, whether physically or in some other way. And as we ascend the hierarchy further, from level 6 to 8, the theses become increasingly unspecific, demanding in turn less and less comprehensibility or knowability of the universe, so that it becomes increasingly likely that these theses are true. Until, at levels 9 and 10 we arrive at theses so unspecific, so meagre, in what they require of the universe for it to be partially knowable, that it can only help and can never hinder the pursuit of knowledge, to accept these theses as a part of knowledge whatever the universe may be like. These theses are justifiably a permanent part of scientific knowledge.¹³

One objection that may be raised to the above is that it would be more rational for science to make the least substantial cosmological assumption that it can that just suffices to exclude *ad hoc* theories from science, as opposed to the very much more substantial assumption of (perfect) physical comprehensibility, at level 4, or the even more substantial assumption at level 3. Science could, after all, make headway with such a minimal assumption; the much more precise and substantial assumptions, at levels 3 and 4, are not necessary for science to be possible.¹⁴

But this "non-*ad hoc*" view is *not* more rational than AOE. The assumption that the universe is physically comprehensible to the extent, at least, that it is not *ad hoc*, is both too restrictive, and not restrictive enough. Just conceivably, the universe might turn out to be *ad hoc*; or it might turn out to be comprehensible in some non-physical way: in both cases science (or the pursuit of knowledge more generally), might be possible. This can be accommodated by AOE, but not by the non-*ad hoc* view. Again, it is important that we take seriously metaphysical conjectures concerning comprehensibility much more specific than the non-*ad hoc* conjecture. We need to do this in order to give ourselves the best possible help with discovering new fundamental physical theories (see below). By

adopting increasingly specific conjectures, at levels 4 and 3, and revising these in the light of the empirical success and failure of the research programmes to which they give rise, we give ourselves the best hope of developing methodologically and heuristically fruitful conjectures. As I put it some years ago "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 to the progress of physics, and because it is *more vulnerable to criticism*, more open to being found wrong ... should the universe turn out to be comprehensible in some other way" (Maxwell, 1984, p. 224).

And there are other considerations in favour of AOE. Theoretical physics in practice requires of a new theory that it satisfies non-empirical requirements of simplicity and unity, much more restrictive and demanding than that the theory should be merely non-*ad hoc* (in the above sense). Most physicists today hold that the so-called standard model, the current quantum theory of fundamental particles and forces, cannot be correct, not because it is *ad hoc*, but because it lacks unity. Persistently to require of fundamental physical theory that it exhibits unity, in this way, is to assume, implicitly, that the universe itself exhibits dynamic unity (or physical comprehensibility). But rationality requires that this implicit assumption, substantial, influential and problematic, be made explicit so that it can be critically assessed and, we may hope, improved. This AOE does, but the non-*ad hoc* view fails to do. Furthermore, all the great theoretical revolutions in physics, from Galileo, Newton, Faraday and Maxwell to Einstein, Bohr, Schrödinger and beyond, constitute astonishing feats of theoretical *unification* of ever wider ranges of phenomena. There is no other idea, of comparable generality, that has been so fruitful for theoretical physics as the level 4 thesis of physical comprehensibility.¹⁵ This provides the justification for adopting it at level 4 within the framework of AOE. Finally, as I have shown elsewhere (Maxwell, 1998, chs. 2-4), if the level 4 thesis of physical comprehensibility is acknowledged to be part of scientific knowledge, one can make sense of what it *means* to assert that fundamental physical theory exhibits such and such a degree of *unity*. It means, roughly, that the content of fundamental theory exemplifies physical comprehensibility to extent N, where N is the number of kinds of physical entity postulated by the theory, and is some integer ≥ 1 . The non-*ad hoc* view cannot even make sense of what theoretical unification *means*.

I conclude that AOE is the more rational option.

As long as the scientific community continues to uphold SE, science will continue to suffer from the rationalistic neurosis diagnosed above.¹⁶ Only when scientists reject SE, and put AOE explicitly into scientific practice instead, will science free itself of its neurosis.

It is interesting to note, however, that the neurosis of science, being built into the intellectual-institutional structure of science, possesses a built in method for protecting itself against its destruction. According to SE, an idea, in order to be a potential contribution to science, must be empirically testable.¹⁷ But a diagnosis of the methodological or rationalistic neurosis of science is not itself a straightforward testable contribution to factual knowledge. Hence, it will be excluded from science. SE, once accepted by the scientific community and built into the institutional constitution of science, excludes such criticism of itself, on the grounds that such criticism is "philosophy of science", not science. This article, for example, is not the sort of article that a respectable science journal would consider publishing for a moment. The neurosis of science has its own mechanisms of defence, in other words, a feature of the situation

that mirrors methodologically points noted by Freud, Jung and other analysts of the human psyche.¹⁸

3 Does the Neurosis of Science Matter?

Science is one of the most astonishingly successful human endeavours ever. It has improved knowledge and understanding in leaps and bounds - almost, one is inclined to think, at an ever accelerating rate. Without science, the modern world is inconceivable. Given this amazing success, does the neurosis of science really matter? If such success can be achieved by a neurotic enterprise, is not neurosis a boon rather than a hinderance?

How damaging a neurosis is depends on how seriously the false, avowed aim and associated methods are taken. If they are taken very seriously indeed, much energy and activity being devoted to the attempt to pursue the avowed aim, and to rationalize actions as being designed to realize this aim, then the neurosis will be very damaging. But if only lip service is paid to the false, avowed aim, and the real, problematic aim is, in practice, pursued resourcefully and intelligently, then the neurosis will not matter too much. This, fortunately, is how it is with science, up to a point at least. The neurosis does not bite too deep. Scientists pay only lip service to the false, avowed aim of science; in their research they take it for granted that explanations for phenomena exist to be found, the universe being (more or less) comprehensible, non-explanatory hypotheses and theories being ignored. Science has made such extraordinary progress despite, and not because of, the officially accepted philosophy of science of SE.

This said, it must also be emphasized that the neurosis of science does have a number of damaging consequences. Or, equivalently, freeing science of its neurosis would have a number of good consequences. In Parts II and III of this essay I indicate what some of these consequences are.

Part II: Implications for Natural Science

1 Rational Scientific Discovery

How, then, might science become even more successful if it repudiated SE and put AOE into scientific practice instead?

Granted SE, it is a mystery as to how new fundamental physical theories are discovered. If such discovery involved extending existing theories, the thing might not be such a mystery. But new theories almost always contradict earlier theories. Newton's theory of gravitation contradicts Kepler's laws of planetary motion and Galileo's laws of terrestrial motion; Einstein's theory of gravitation contradicts Newton's. Quantum theory contradicts the whole of classical physics; and relativistic quantum theory contradicts non-relativistic quantum theory: see (Maxwell 1998, 124-125, 211-217) for details. How, then, are these new theories discovered?

SE cannot answer this question; and those who defend versions of SE, such as Popper, tend to hold that there is no rational method for the discovery of new theories in physics: see (Popper 1959, 31-32).

But granted AOE, the situation is very different. Whereas, given SE, scientific knowledge consists of just empirical data and testable laws and theories (levels 1 and 2 of diagram 2), given AOE, scientific knowledge consists of items at *ten* distinct levels. In particular, at level 3 there is the best current untestable metaphysical conjecture as to how the universe is physically comprehensible; at level 4 there is the somewhat vaguer metaphysical thesis that the universe is physically comprehensible in some way or other; and at level 5 there is the even vaguer thesis that the universe is comprehensible in some way or other.

An important point is that the level 4 thesis clashes with theoretical knowledge at level 2. The level 4 thesis asserts that the universe has a *unified* dynamic structure (some yet-to-be-discovered *unified* theory of everything being true) whereas, because of our

ignorance, theories at level 2 clash with one another, and fail to form a unified whole. The conjecture at level 3 is the best available compromise between the vaguely asserted *unity* at level 4 and the precisely asserted *disunity* at level 2. This compromise will clash with the disunity of level 2, and will no doubt clash with the unity of level 4.

In seeking to discover better level 2 theories, physicists now have quite definite tasks to perform. Taking clues from clashing existing fundamental theories at level 2, and the unity at level 4, the basic task is to discover how to modify the thesis at level 3 so that it is a better compromise between levels 2 and 4; any modified thesis that emerges then needs to be made more and more precise until it becomes a new testable theory. In putting forward modified versions of the level 3 conjecture, physicists will, in effect be developing new physical principles, new symmetry principles, which may act as guides to the construction of new theories.

It was in this way that Einstein discovered special and general relativity (Maxwell 1993). And, following Einstein's lead, it was in this way that more recent fundamental physical theories have been discovered, in particular the locally gauge invariant theories of quantum electroweak dynamics (which partially unifies electromagnetism and the weak force) and quantum chromodynamics (the theory of the strong force): see (Maxwell 1998, 135-139, and further references cited there).

This rational, but non-mechanical and fallible method of discovery is impossible granted SE. It requires that untestable, metaphysical ideas are rationally assessed, in terms of the justice they do to (1) unity and (2) accepted physical theories. But according to SE the only rational way of assessing ideas in science is in terms of empirical success and failure. One may, perhaps, by extension, assess untestable, metaphysical ideas in terms of their compatibility with existing accepted theories: but this is precisely the *wrong* way to assess ideas, granted that we seek to discover new theories. We require ideas that *clash* with existing theories. (In so far as SE gives any guidelines for the constructing new theories, they point in the wrong direction.)

One adverse consequence of the acceptance of SE by the scientific community is the phenomenon of scientific revolutions, brilliantly depicted in (Kuhn, 1962). By contrast, AOE science, insisting on sustained exploration of alternative blueprints as an integral part of normal research, would not experience Kuhnian revolutions.

In some respects, current theoretical physics proceeds in a way which is much closer to AOE than to SE. Theoretical physicists have invested a massive amount of work in developing superstring theory, or M-theory as it is now known. And yet, so far, no successful predictions have been forthcoming.¹⁹ Given SE, this is wildly unscientific behaviour. Given AOE, string theory, or M-theory is an attempt to develop new level 3 ideas, and is thus entirely scientifically respectable. The only caveat that one might add to that is that, because SE is still the official philosophy of science among physicists, there is a persistent lack of understanding as to how untestable metaphysical theories, such as string theory, are to be rationally developed and assessed. There is a certain failure among string theorists, for example, to appreciate the importance of trying to develop a number of rival level 3 ideas; and there is a widespread failure to appreciate that such work can be *rationally* (if fallibly) assessed even before it issues in empirical predictions. Because of the failure to appreciate that work of this type can be assessed rationally, in practice what tends to influence this work is mere *fashion*. The vast majority of theoretical physicists working in this field work on string theory (or M-theory), the fashionable thing to do; relatively few physicists explore other lines of inquiry. Few indeed are the physicists attempting to assess rationally the relative merits of rival research programmes.²⁰

2 The Philosophy of Science

Freeing science of its neurosis would bring about a major and much needed revolution in the philosophy of science. At present most work in the philosophy of science, in the

academic discipline that is, proceeds within the framework of, and seeks to justify SE (of one version or another): see (Maxwell 1998, ch. 2). But this, as we have seen, is exactly the wrong thing to do. Current philosophy of science is a deeply neurotic activity. Not only is the philosophy of science beset by long-standing problems about the nature of science, which resist all attempts at solution - most notably problems of induction and simplicity. In addition, work done in the philosophy of science seems to have no impact on science itself whatsoever. All this is symptomatic of the philosophy of science being the neurotic face of science.

As far as the scientific sterility of the discipline is concerned, this is something that scientists themselves occasionally comment on, as I have already mentioned. Thus Seven Weinberg recently declared: "From time to time ... I have tried to read 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 am not alone in this; 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, 133-134). And John Ziman, another theoretical physicist, some years ago commented: "the Philosophy of Science ...[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" (Ziman 1968, 31).

Philosophers of science themselves tend to be quite unashamed about the scientific sterility of their subject. They hold that their discipline is a "meta-discipline". Its task is to describe and justify scientific practice, but not to contribute to science itself. This view of their subject is forced upon them by their adherence to SE.

Cease to be the neurotic face of science, repudiate SE and adopt AOE instead, and all this changes. We may take a large part of the philosophy of science to be engaged in the tasks of specifying and justifying the aims and methods of science, and spelling out metaphysical assumptions implicit in scientific knowledge. Whereas SE decrees that this is the work of a "meta-discipline", AOE, on the contrary, demands that this must be pursued as a vital, integral part of science itself. Untestable metaphysical assertions, at levels 3 to 10 in the hierarchy of assumptions (see diagram 2) are not distinct from science; they are basic items of scientific knowledge. Methodological principles such as symmetry principles, associated with these assumptions (represented by dotted lines in the diagram), governing the choice of theories in physics, are a vital part of science itself. According to AOE, the activity of trying to improve the aim and methods of science (in the light of improving scientific knowledge and understanding) is a vital part of scientific work itself: the ability to improve its aim and methods in this way is a basic feature of scientific rationality, a vital part of the reason for the amazing success of science.

The transition from standard to AOE, in short, transforms the philosophy of science. Its character, its relationship to science itself, its ability to contribute fruitfully to science, are all transformed. And as a bonus, the long-standing unsolved problems of the philosophy of science, most notably problems of induction and simplicity, which cannot be solved granted SE, can be solved within the framework of AOE: see (Maxwell 1998, chs. 3-6).

Instead of being the discipline which actively helps sustain the neurosis of science, the philosophy of science has the chance to take a leading role in liberating science from its neurosis.

3 Science and Values

So far we have considered two rival possible aims for science: the (neurotic) aim of improving knowledge about the world, nothing being permanently presupposed about the world independent of evidence; and the (real) aim of improving knowledge about the

world which is presupposed to be comprehensible (or, more accurately, about which a hierarchy of increasingly insubstantial assumptions are made, including the assumption that the universe is comprehensible).

But this latter aim of seeking *explanatory truth* (as we may call it) is a special case of the more general aim of seeking *valuable truth*. Knowledge of truth that enables us to explain and understand is of great value; but so too is knowledge of truth that enables us to *do* things of value, to realize human goals of value, most notably via technological applications. There are the multitude of applications of scientific knowledge throughout almost all aspects of modern life: health, agriculture, industry, transport, communications.

SE, the neurotic conception of science, excludes metaphysics from science, as we have seen. Even more firmly, it excludes *values* from science. Viewed from the perspective of SE, it seems that values (apart from narrowly intellectual, scientific values) could only exert a corrupting influence on science. They could only lead scientists to accept some result as true because it is deemed desirable, or reject some result as false because it is deemed undesirable. Thus the thesis that there are no statistical differences in intelligence between men and women, let us say, might be deemed to be true, independent of evidence, on the grounds that it is desirable that it should be true.

If such considerations of desirability are allowed to influence scientific decisions about truth and falsity, then science is indeed subverted. But the moment the neurotic, SE picture of science is rejected, and AOE is adopted instead, it becomes obvious that there is another, entirely legitimate, indeed indispensable way in which human values influence science. Values, quite properly and inevitably, influence the *aims* that scientists pursue.²¹

The neurotic, SE idea that science should seek to improve knowledge of factual truth without judgements about what is desirable or of value influencing what truth is sought is impossible to fulfil. The number of facts out there awaiting potential investigation is infinite. The entire scientific community could devote itself to acquiring knowledge about a single matchbox, if it so chose: its composition, history, manufacture, exact history of each constituent atom, etc., etc. Inevitably, scientists must choose to investigate certain facts and phenomena, and ignore others.

This is not just inevitable; it is desirable. We want science to acquire useful or valuable knowledge. It is built into the very notion of scientific knowledge, that it is knowledge that has reached a certain threshold level of significance. In order to be published, it is not enough that a scientific paper establishes a new result; in addition the result must be deemed to be sufficiently important for the paper to be judged worthy of publication. A science which amassed knowledge of irredeemable trivia, would not be judged to be making splendid progress; it would be judged to be stagnating. Values are thus built into the very notions of scientific knowledge and scientific progress.

A proper, basic aim of science, then, is to improve knowledge of *valuable truth* - the aim of improving knowledge of *explanatory truth* being a special case of this.

The orthodox, neurotic, SE perspective may encourage the view that human values legitimately influence technological or applied science, but exercise no legitimate influence over pure science. Again, this is nonsense. Knowledge sought for its own sake is sought because of its human interest or significance. This is true of explanatory knowledge, of great theories of science that help us to explain and understand broad features of our world; but it is also true of, for example, knowledge sought because of its particular relevance to human life, such as knowledge about human origins and development, or the origins of life. Science is not interested, uniformly, in the contents of every chunk of space-time throughout the history of the universe: it is especially interested in highly significant or unusual chunks, much less interested in other chunks. Counting grains of gravel on paths, or leaves on trees, is of no interest in itself whatsoever, even though this might add to the store of human knowledge.

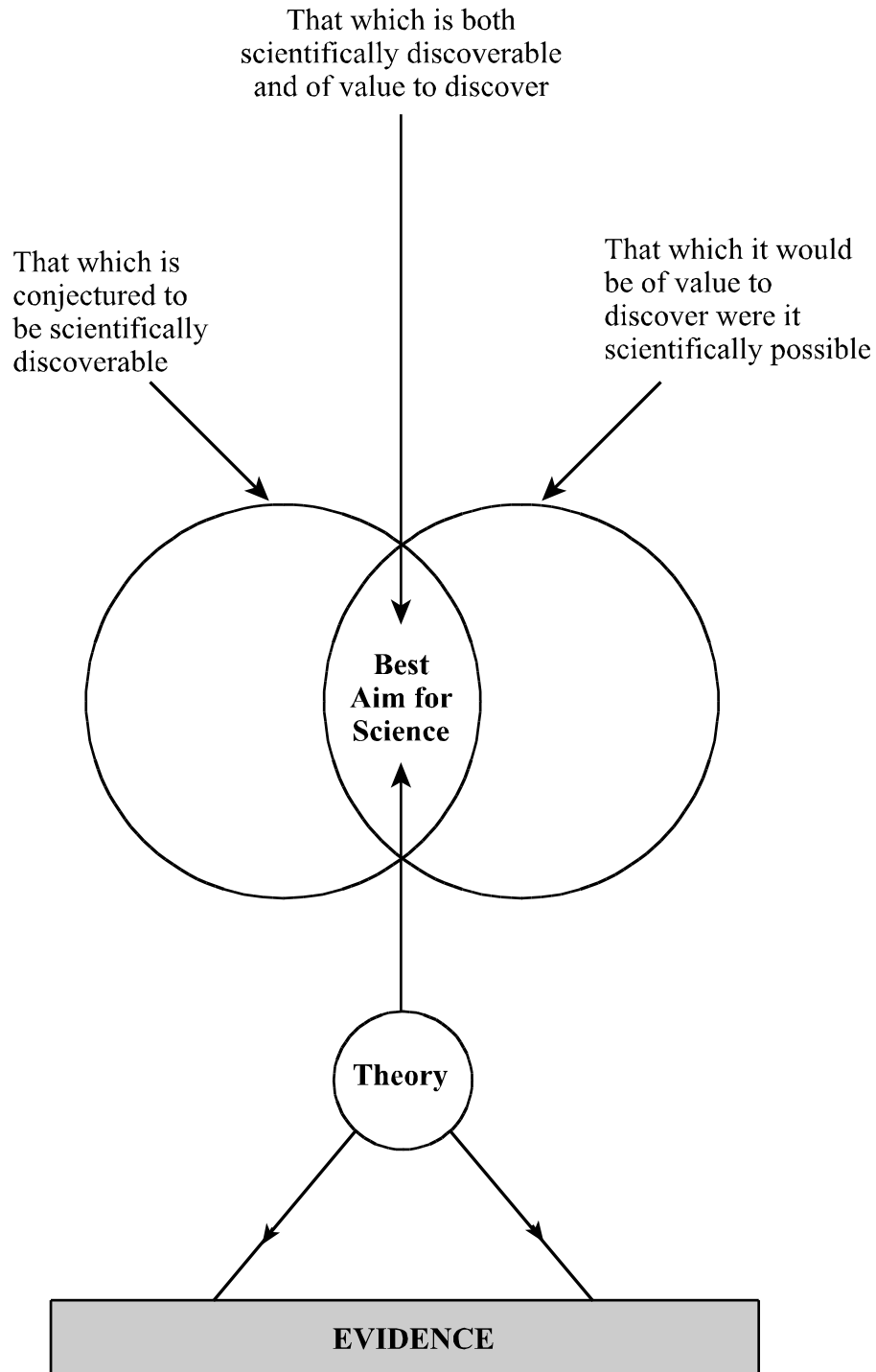


Diagram 3

Here, then, is a second, and in some respects much more serious neurosis of science: repression of the real aim of seeking *valuable truth* and its replacement by the officially recognized, neurotic, false aim of seeking *truth as such*, devoid of considerations of human value.

The aim of seeking valuable truth is, if anything, even more problematic than the aim of seeking explanatory truth. What is of value? Of value to whom? Who is to decide? How can conflicting values, conflicting needs, be resolved? How can we know what

there is to be discovered, that science is capable of discovering, that is of value? What will it be important for us to know in 50 years time, or 150 years time?

In order to free science of its neurotic repression of values, the first step that needs to be taken is to make explicit, within the intellectual domain of science (that is, within scientific journals, texts, conferences, undergraduate and graduate courses, lectures and seminars), *both* what it is conjectured is scientifically discoverable *and* what it is conjectured would be of human value to discover: see diagram 3. The idea, here, is that, as a result of making explicit conjectures concerning these two highly problematic domains, it will become easier to make a good choice of that even more problematic region of overlap between these two domains: that which is *both* scientifically discoverable *and* of value to discover: see diagram 3. Precisely because this region of overlap is so highly problematic to discover (in that it involves making guesses both about what is scientifically discoverable and of value), we need to create, as an important part of scientific research, a tradition of proposing and critically assessing ideas for future research aims, plus the critical assessment of existing research aims. There need to be scientific journals devoted to the attempt to improve the research aims of science.

Scientists may be in the best position to make good judgements about what there is that is scientifically discoverable; they are not, however, necessarily in the best position to decide, for the rest of humanity, what is of value. It is above all here, in connection with values influencing aims of research, that non-scientists must contribute to science itself. Science ceases to be objective if this does not happen.

To say this is not to say that questions of what is of value can be decided democratically, by a vote, a poll, or market research. Here, as in other parts of science, ideas, proposals, arguments, criticisms must be subjected to a good process of filtering, of critical assessment, so that it is the *best* ideas that come to be adopted by the scientific community.

But how can the "best" ideas be decided upon in the realm of value, that does not just prejudice the issue - the "best" ideas about what is of value corresponding, simply, to the values of those who are in charge of deciding what is and is not published, what is and what is not considered and adopted? How, to put the question slightly differently, can value-questions be decided objectively and rationally in a society which fails dismally to do any such thing - it being even uncertain as to what such decision-making about questions concerning what is of value would *mean*?

The answer is to do for questions of *value* what I have already argued needs to be done for questions of *metaphysics*. We need to create a hierarchy of conjectures as to what is of value, these value-conjectures becoming increasingly insubstantial, increasingly unproblematic and uncontroversial as we ascend the hierarchy. In this way, we can create a framework within which the rationally cooperative discovery of what is of value becomes possible. It becomes possible for humanity to *learn* what is of value - as we shall see below.

Those inclined to see science in terms of SE will, of course, deplore the suggestion that values should be incorporated into science, on the grounds that this can only undermine the objectivity and rationality of science.²² The true state of affairs is actually all the other way round. Those who seek to exclude value-questions from science undermine the objectivity and rationality of science. As I have emphasized above, values are inevitably a part of science, simply in influencing what scientists seek to try to develop knowledge about. What is at issue is: Are values that influence research aims repressed, so that they cannot be explicitly examined and, we may hope, improved (which subverts objectivity and rationality)? Or are such values acknowledged, there being an attempt to put forward and critically assess ideas about what values should influence research aims, in an attempt to improve values and aims? The choice is between repression, neurosis, dogmatism, and the prohibition of rational discussion on the one hand, and open acknowledgement of values influencing research aims, and the

sustained attempt to improve such values by means of reason, that is by means of conjecture and criticism, on the other hand. The latter option enhances the objectivity and rationality of science - the capacity of science to develop knowledge that really is of human value.

4 *Science and Politics*

Why does science seek to improve knowledge of valuable truth? Science does this in the hope that this knowledge will be *used* by people, in their lives, to enrich the quality of their lives. There is little point in a scientist discovering something of great potential value if no one makes use of it. Science comes to life, as it were, when it is used by people, in one way or another, as a part of life. Locked away in journals, or in the notebooks, computers or heads of scientists, scientific discoveries have only potential value, as far as the body of humanity is concerned.

It needs to be appreciated that this applies just as much to "pure" science as it does to "applied". Improving our knowledge and understanding of aspects of the universe and ourselves for their own sake is of value in so far as it is the knowledge and understanding of *people*, whether scientists or non-scientists, that is improved. If science one day becomes fully automated, so that only robots (without consciousness, let us assume) can do and "understand" science then, in one sense, science might be making splendid progress. But in another, much more important sense, science would have come to an end precisely because *human beings* no longer had any knowledge or understanding of the science that was being done. Instead of enhancing people's knowledge and understanding, science would fail to contribute anything intellectual or cultural to humanity. And all this is even more obvious when it comes to the *practical* or *technological* value of science. In *both* cases, what matters is the capacity of science to contribute to the enrichment of human life, either directly by means of provoking enhanced knowledge and understanding of, or curiosity about aspects of, the universe and ourselves, or indirectly by means of technological or other applications which enable people to achieve goals of value in life (such as health, travel, communication, etc.).

This means that the impersonal intellectual aim of science of improving knowledge of explanatory truth or, more generally, of valuable truth, is pursued as a *means* to the end of pursuing the *human, social, or humanitarian* aim of contributing to the enrichment of human life. The purely *intellectual* aims of science are pursued, we may say, in order to contribute to more fundamental *political* aims, "political" here being interpreted, not narrowly as "party politics", but broadly as the activity of seeking to improve the human condition. Science is fundamentally a part of a *political programme* to improve the quality of human life, and the intellectual aims of science are means to that more fundamental end.

But if those who view science from the orthodox, standard empiricist standpoint are inclined to throw their hands up in horror at the idea that science should include metaphysics and values, they will undoubtedly be apoplectic at the suggestion that the ultimate purpose of science is *political*! For them, the idea that science is a part of some *political programme* will be the ultimate obscenity. If taken seriously, such an idea, so they will passionately maintain and believe, will spell the end of science.

The reply to this vehement denial (the vehemence typical, of course, of neurosis) is essentially the same as before. Whether scientists like it or not, science is carried on within the human world, funded primarily by industry and government, its products used by people, industry, government and other institutions in a variety of ways in order to realize a variety of human ends, from the good to the bad. Not only is this *inevitable*; it is *desirable* that science should be used by people in life. It is both inevitable and (apart from dreadful misuses of science) *desirable* that science should be a part of a political process (in the broad sense of "political").

Repression of the human, humanitarian, social or political goals and dimensions of science amounts to no more than the denial of

an undoubted reality, even if a highly *problematic* reality.

All scientists recognize, of course, that the products of scientific research are used and misused by people, by industry, by governments in a variety of ways for a variety of social, economic or political ends. Those who see science in terms of SE, however, sharply dissociate the intellectual, the purely scientific aims and aspects of science from its human uses and misuses. They see science as having the intellectual aim to improve knowledge about aspects of the world around us; they do not see *science itself* as having any humanitarian, social or political aim or function. It is here that the act of repression, the neurosis comes in. Inevitably, and quite properly, in engaging in scientific research, teaching and publishing the results of that research, in a particular socio-economic-political context, there is a social, economic and political dimension to what the scientist does, however much this may be neurotically suppressed. And furthermore, it is quite proper that science, even at its most "pure" and esoteric, its most theoretical and far removed from the practical, is construed as making a contribution to the welfare of humanity, to the enrichment of human life. Enhancing our scientific understanding is worthwhile in so far as it leads to, is associated with, enhancing the understanding that *people* have of the world in which they live.

The argument now proceeds in exactly the same way as before, in connection with the suppression of metaphysics and values. Denying the social, humanitarian or political dimension of science, repressing these social aims of science, means that the severe *problems* associated with these dimensions, these aims, cannot be explicitly discussed within the intellectual domain of science. The result is that scientists are more or less helpless when it comes to having their work misused by industry, governments and others. Having declared the *use* and *misuse* of science to lie outside the intellectual domain of science, the scientific community is ill-equipped to combat misuses of scientific knowledge, and to develop the social or institutional means for the humanly valuable use of science. Repressing the political dimension of science undermines the capacity of science to be of real value to humanity.

At this point, however, the line of argument we have been pursuing may seem to become somewhat implausible. Is it really to be expected that scientists, as professional scientists, will enter the political domain, do battle with the might of industry, government and public opinion? Would not such a task leave no time for the scientific research itself? Science might come to an end through the sheer exhaustion of its practitioners, through compassion fatigue! Or, put another way, is it not the job of *social* scientists, rather than natural scientists (and so far we have been concerned exclusively with natural science) to come to grips with the social world?

This brings us to an even more serious dimension of neurosis associated with science, this time bringing in the social sciences. It will turn out that this neurosis affects not only the academically respectable social scientists, such as economics, political science, psychology and anthropology, but also the less academically secure social science with which we began, namely psychoanalytic theory! The argument concludes by coming full circle: psychoanalytic theory itself is deeply neurotic.

Part III: Implications for Social Inquiry

1 The Enlightenment Programme

In assessing the success of science, and whether its neurosis has adversely affected this success, much may depend on what we take the aim of science to be. If we take the aim to be to acquire knowledge and understanding of the universe, or to acquire humanly valuable knowledge and technological know-how, science must surely be deemed to be astonishingly successful (despite the disvalue of the technology of war).

But if the aim of science is taken to be to help promote human welfare or, more radically, to help humanity learn how to build a better world or become civilized, the answer must be, surely, that science, given this humanitarian, political aim, has not been especially successful. In countless ways, because of modern science and technology, life for most people in the industrially advanced world is, today, vastly better than life in medieval Europe, or in hunting and gathering tribes ten thousand years ago. But against this one has to put the grim aspects of our technologically advanced age: rapid population growth, millions killed in war, the threat posed by modern armaments, extreme inequality of wealth between the first and third world, destruction of traditional ways of life, languages and cultures, destruction of natural habitats such as tropical rain forests and extinction of species, rapid depletion of finite natural resources, pollution of land, sea and air, the latter causing thinning of the ozone layer and global warming.

All these grim aspects of our world have been made possible by modern science and technology. They have all arisen because we have solved the big problem of learning how to make progress in knowledge about the world (by creating modern science), but have not *also* solved the big problem of learning how to make social progress towards a wise, civilized world. Solving the first big problem of learning leads to rapidly increasing scientific knowledge and technological know-how, which in turn brings with it an immense increase in the power to act. But in the absence of global wisdom, in the absence of a solution to the second great problem of learning, the increase in the power to act may have good consequences, but will as often as not have all sorts of harmful consequences, whether intended or not, such as those just indicated.

Humanity is, in other words, at present, in a situation of great peril - unique peril, when judged from a historical perspective. Without modern science, lack of global wisdom did not matter too much; we lacked the power to wreak too much havoc on ourselves and our surroundings. With modern science, our power is terrifying, and global wisdom and civilization has become, not a luxury but a necessity.

There are those, of course, who blame science for our troubles. But this is to miss the point. It is not science that is to blame, but ourselves for failing to learn wisdom. Instead of blaming science, we should seek to *learn* from science - learn, in particular, from the extraordinary success of science in solving the first big problem of learning, its success, that is, in improving our knowledge.

Can we, in other words, learn from *scientific progress* towards greater knowledge how to achieve *social progress* towards a better, wiser, more civilized world? Can we generalize the progress-achieving methods of science, and then apply these generalized methods to the immense task of creating global civilization?

This is an old idea. It goes back to the Enlightenment of the 18th century. Indeed, this was the basic idea of the *philosophes* of the Enlightenment - Voltaire, Diderot, Condorcet et al.: to learn from scientific progress how to achieve social progress towards world enlightenment.

The best of the *philosophes* did what they could to put this immensely important 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 rationality in personal and social life: see (Gay, 1973).

Unfortunately, in developing the Enlightenment idea intellectually, the *philosophes* blundered. They developed the Enlightenment programme in a seriously defective form, in a *neurotic* form, and it is this immensely influential, defective, neurotic version of the programme, inherited from the 18th century, which may be called the "traditional" Enlightenment, that is built into late 20th century institutions of inquiry. Our current traditions and institutions of learning, when judged from the standpoint of helping us

learn how to become more enlightened, are defective, neurotic and irrational in a wholesale and structural way, and it is this which, in the long term, sabotages our efforts to create a more civilized world, and prevents us from avoiding the kind of horrors we have been exposed to during this century - wars, third-world poverty, environmental degradation. Rationalistic neurosis has become profoundly damaging!

The *philosophes* of the 18th century assumed, understandably enough perhaps, that the proper way to implement the Enlightenment programme was to develop social science alongside natural science. Francis Bacon had already stressed the importance of improving knowledge of the natural world in order to achieve social progress. The *philosophes* generalized this, holding that it is just as important to improve knowledge of the social world. Thus the *philosophes* set about creating the social sciences: history, anthropology, political economy, psychology, sociology.

This had an immense impact. Throughout the 19th century the diverse social sciences were developed, often by non-academics, in accordance with the Enlightenment idea. Saint-Simon, Comte, Mill, Marx, Durkheim, Weber all contributed to this development. Gradually, universities took notice of these developments until, by the mid 20th century, all the diverse branches of the social sciences, as conceived of by the Enlightenment, were built into the institutional structure of universities as recognized academic disciplines.²³

But, from the standpoint of creating a kind of inquiry designed to help humanity learn how to become civilized, all this amounts to a series of monumental blunders.

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 human endeavour, whatever the aims may be, and not just applicable to the endeavour of improving 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, civilized world.

Unfortunately, the *philosophes* of the Enlightenment got all three points disastrously wrong. They failed to capture correctly the progress-achieving methods of natural science; they failed to generalize these methods properly; and, most disastrously of all, they failed to apply them properly so that humanity might learn how to become civilized by rational means. That the *philosophes* made these blunders in the 18th century is forgivable; what is unforgivable is that these blunders still remain unrecognized and uncorrected today, over two centuries later. Instead of correcting the blunders, we have allowed our institutions of learning to be shaped by them as they have developed throughout the 19th and 20th centuries, so that now the blunders are an all-pervasive feature of our world.

So what exactly are the three blunders of the traditional Enlightenment, as embodied in academic inquiry today, and what needs to be done to put them right? Let us take them in turn.

The first blunder has already been discussed at some length. It involves accepting SE instead of AOE.

But what of the *second* blunder? The task, here, is to generalize the progress-achieving methods of science appropriately so that they become progress-achieving methods that are, potentially, fruitfully applicable to *any* problematic human endeavour. The task is so to generalize scientific rationality that it becomes rationality *per se*, helping us to achieve what is of value whatever we may be doing.

Needless to say, scientists and philosophers, having failed to specify the methods of science properly, have also failed to arrive at the proper generalization of these methods.

The best attempt known to me is that made by Karl Popper. According to Popper, science makes progress because it puts into practice the method of proposing theories as conjectures, which are then subjected to sustained attempted empirical refutation. Popper argues that this can be generalized to form a conception of rationality, according to which one seeks to solve problems quite generally by putting forward conjectures as to how a given problem is to be solved, these conjectures then being subjected to sustained *criticism* (criticism being a generalization of attempted empirical refutation in science).²⁴

Popper's ideas about scientific method and how it is to be generalized are an improvement over 18th century notions, but they are still defective. Popper's conception of scientific method is defective because it is a version of SE, which we have already seen is untenable. It fails to identify the problematic aim of science properly, and thus fails to specify the need for science to improve its aims and methods as it proceeds. Popper's notion of critical rationalism is defective in an analogous way. It does not make improving aims and methods, when aims are problematic, an essential aspect of rationality.

If, however, we take the above aim-oriented empiricist conception of scientific method as our starting point, and generalize that, the outcome is different. It is not just in science that aims are problematic; this is the case in life too, either because different aims conflict, or because what we believe to be desirable and realizable lacks one or other of these features, or both. Above all, the aim of creating global civilization is inherently and profoundly problematic. Furthermore, it is not just science that suffers from rationalistic neurosis; many other institutional and traditional endeavours repress problematic aims and acknowledge instead ostensibly unproblematic, token, "neurotic" aims instead. Quite generally, then, and not just in science, whenever we pursue a problematic aim we need, first, to acknowledge the aim; then we need to represent it as a hierarchy of aims, from the specific and problematic at the bottom of the hierarchy, to the general and unproblematic at the top. In this way we provide ourselves with a framework within which we may improve more or less specific and problematic aims and methods as we proceed, learning from success and failure in practice what it is that is both of most value and realizable. Such an "aim-oriented" conception of rationality is the proper generalization of the aim-oriented, progress-achieving methods of science.

So much for the second blunder, and how it is to be put right. We come now to the *third* blunder.

This is by far the most serious of the three blunders made by the traditional Enlightenment. The basic Enlightenment idea, after all, is to learn from our solution to the first great problem of learning how to solve the second problem - to learn, that is, from scientific progress how to make social progress towards an enlightened world. Putting this idea into practice involves getting appropriately generalized progress-achieving methods of science *into social life itself!* It involves getting progress-achieving methods into our institutions and ways of life, into government, industry, agriculture, commerce, international relations, the media, the arts, education. But in sharp contrast to all this, the traditional Enlightenment has sought to apply generalized scientific method, not to social *life*, but merely to social *science!* Instead of helping humanity learn how to become more civilized by rational means, the traditional Enlightenment has sought merely to help social scientists improve knowledge of social phenomena. The outcome is that today academic inquiry devotes itself to acquiring knowledge of natural and social phenomena, but does not attempt to help humanity learn how to become more civilized. This is the blunder that is at the root of our current failure to have solved the second great problem of learning.

In order to correct this third, monumental and disastrous blunder, we need, as a first step, to bring about a revolution in the nature of academic inquiry, beginning with social

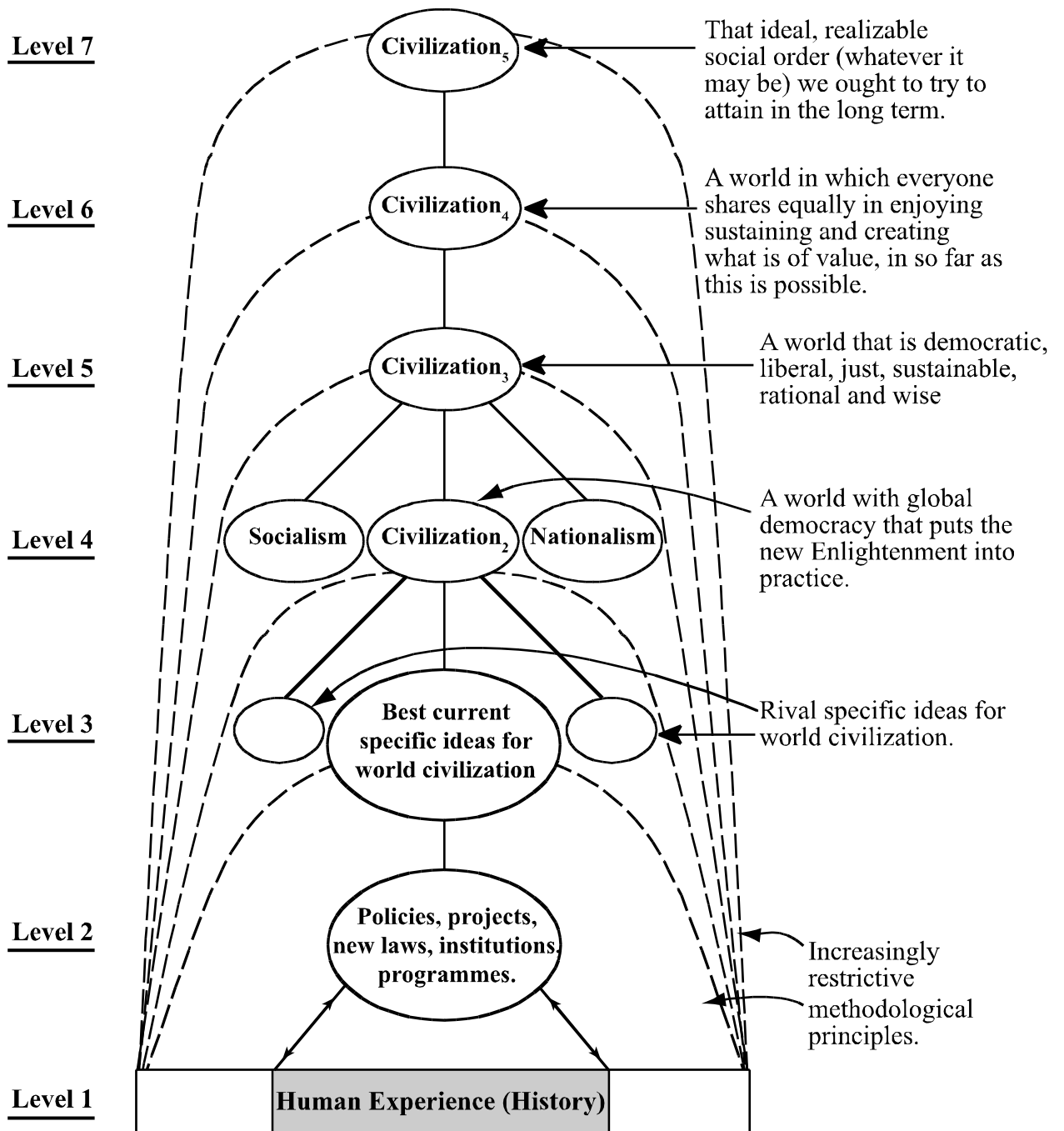


Diagram 4: Aim-Oriented Rationality
(Enlarge to read)

inquiry and the humanities, as I have argued at length elsewhere: see (Maxwell, 1984, 1992. 2000). Social inquiry is not primarily social *science*. Its proper basic task is to help humanity build into institutions and social life quite generally the progress-achieving methods of aim-oriented rationality (arrived at by generalizing the progress-achieving methods of science as indicated above). Social inquiry (sociology, economics, anthropology and the rest) is thus social *methodology* or social *philosophy*. Its task is to help diverse valuable human endeavours and institutions gradually improve aims and methods so that the world may make social progress towards global enlightenment. And the primary task of academic inquiry, more generally, becomes to help humanity solve its

problems of living in increasingly rational, cooperative, enlightened ways, thus helping humanity become more civilized. The basic aim of academic inquiry becomes to promote the growth of *wisdom* - wisdom being defined as the capacity to realize what is of value in life (and thus including knowledge and technological know-how). Those parts of academic inquiry devoted to improving knowledge, understanding and technological know-how contribute to the growth of wisdom.

As I have already remarked, the aim of achieving global civilization is inherently problematic. This means, according to aim-oriented rationality, that we need to represent the aim at a number of levels, from the specific and highly problematic to the unspecific and unproblematic. Thus, at a fairly specific level, we might, for example, specify civilization to be a state of affairs in which there is an end to war, dictatorships, population growth, extreme inequalities of wealth, and the establishment of democratic, liberal world government and a sustainable world industry and agriculture. At a rather more general level we might specify civilization to be a state of affairs in which everyone shares equally in enjoying, sustaining and creating what is of value in life *in so far as this is possible*. Diagram 4 depicts a cartoon version of what is required, arrived at by generalizing and then reinterpreting diagram 2.

As a result of building into our institutions and social life such a hierarchical structure of aims and associated methods, we create a framework within which it becomes possible for us progressively to improve our real-life aims and methods in increasingly cooperative ways as we live. Diverse philosophies of life - diverse religious, political, economic and moral views - may be cooperatively developed, assessed and tested against the experience of personal and social life. It becomes possible progressively to improve diverse *philosophies of life* (diverse views about what is of value in life and how it is to be realized) much as *theories* are progressively and cooperatively improved in science. In doing this, humanity would at last have learned from the solution to the first great problem of learning how to go about solving the second problem.

2 The Damaging Neurosis of Social Science

The rationalistic neurosis of natural science has not inhibited scientific progress too much, not, at least, if the aim of science is taken to be to improve expert knowledge. This is because (apart from discovery of new theories) dressed SE in practice does not differ too much from AOE. It is the philosophy of science, the neurotic face of science, engaged in the fruitless task of rationalization, of trying to justify the unjustifiable neurotic view of SE, that really suffers.

Fortunately, science ignores the philosophy of science.

When we come to social inquiry, however, all this changes dramatically. Here, rationalistic neurosis really does matter, and has far-reaching, long-term damaging consequences, both intellectual and humanitarian.

The rationalistic neurosis of social inquiry amounts to this. The social sciences arose and were developed in response to the Enlightenment idea of learning from scientific progress how to achieve social progress towards an enlightened world. The proper, fundamental aim of social inquiry is to help humanity learn how to become enlightened by cooperatively rational means. The *philosophes*, and especially those who came after them, thought that this meant developing social inquiry as social *science*: first, knowledge of society is to be developed; then, it can be applied to help solve social problems. But this, as we have seen, is to commit a disastrous blunder: in order to implement the basic Enlightenment idea properly, social inquiry needs to be social *methodology*, or social *philosophy*, not primarily social *science*. The proper, basic task of social inquiry is to get into our diverse institutions, traditions and ways of life, into the fabric of society, general progress-achieving methods arrived at by generalizing the progress-achieving methods of science. The *neurotic* aim is to restrict the task of social inquiry to acquiring knowledge of social phenomena; the real, unneurotic aim is to help humanity tackle its problems of

living by increasingly cooperatively rational means. The difference between the current neurotic character of social inquiry as social *science*, and the proper, unneurotic character of social inquiry as social *methodology* is dramatic and profound.

Furthermore, the rationalistic neurosis of social inquiry has far-reaching damaging consequences. It is this which ensures that humanity has so far *failed* to learn from its solution to the first great problem of learning how to solve the second great problem of learning. It is this which has prevented us from developing a kind of inquiry that is well-designed from the standpoint of helping us gradually learn how to become civilized.

The neurosis of natural science may not matter too much, for natural science itself, at least. But the (associated) neurosis of social inquiry is a disaster, both for social inquiry itself, and for humanity.

We need, then, a revolution throughout the diverse branches of social inquiry - economics, sociology, anthropology, psychology, history, political science. These disciplines are not primarily *sciences*; they are not, primarily, concerned to improve knowledge of social phenomena: their primary task is to help us tackle our problems of living by increasingly cooperatively rational means so that we may gradually make progress towards a civilized world. Each discipline, of course, seeks to acquire knowledge about people, cultures, institutions and social structures where this helps us discover what our problems of living are, and what we might be able to do about them. The primary intellectual task, however, is to promote increasingly cooperatively rational tackling of problems of living; it is not to acquire *knowledge* of social phenomena. Economics is concerned with the economic aspect of our problems, psychology with the personal, the psychological aspect; history and anthropology seek to inform us about our past successes and failures in tackling problems of living - a record of past successes and failures being essential to rationality. Sociology tries, amongst other things, to help us build aim-oriented rationality into our various institutions and social endeavours: politics, industry, agriculture, the law, the media, international relations, education, the arts. The *sociology* of theatre, for example, is the *philosophy* of theatre, the exploration and critical assessment of rival views as to what the aims and methods of theatre ought to be. For further details see (Maxwell 1984).

3 Philosophy and Sociology of Science

This revolution in social inquiry, necessary if social inquiry is to free itself of its neurosis and become fully beneficial to humanity, has one amusing consequence. It means that the *philosophy of science* becomes one and the same thing as the *sociology of science*.

At present, these two disciplines are so different, so much at odds with one another, that they scarcely speak to each other. Each is obliged to exist within the current framework, the current "paradigm" according to which the overall intellectual aim of academic inquiry is to acquire *knowledge*. The philosophy of science struggles with trying to solve the misconceived, neurotic problems thrown up by SE. It is normative in character; it seeks to formulate the rules, the methods, that science *ought* to employ in order to meet with success: but unfortunately, nothing that it comes up with seems to be of much use to science itself (as is, of course, to be expected, given the neurotic character of the discipline). The sociology of science, on the other hand, seeks to acquire *sociological* knowledge about science: it sees itself as a part of sociology, in turn a part of social science. The sociology of science is thus factual, not normative: it scorns the normative task of explicating methods that science *ought* to implement. Each discipline is more or less contemptuous of the work of the other.²⁵

Free the natural and social sciences of their neuroses, however, in the ways already indicated, and it is at once clear that the philosophy and sociology of science are one and the same discipline. The philosophy of science, in freeing itself from the hopeless, neurotic task of trying to prop up SE, needs to consider aims of science that are not just

narrowly conceived intellectual aims; it needs to consider broader, and more fundamental human, humanitarian or social aims. In doing so, philosophy of science needs to come to grips with the institutional and social structure and character of science. The sociology of science on the other hand, in freeing itself of the general neurosis of social science, becomes social methodology, or social philosophy, concerned to explore and critically assess possible and actual aims and methods for science: this is, of course, the philosophy of science.

In general, we may say that what the philosophy/sociology of science is to science, so the sociology of politics (let us say) is to politics itself. Social inquiry, quite generally, is to society what unneurotic philosophy/sociology of science is to science.

4 Academic Neurosis

Not only are natural science and social inquiry neurotic; academic inquiry taken as a whole suffers from rationalistic neurosis. As I have argued at some length elsewhere,²⁶ the fundamental intellectual and social aim of academic inquiry (science, social inquiry, the humanities, technological research, the formal sciences, education) ought to be to promote *wisdom*, where wisdom is defined as the capacity to realize what is of value in life, for oneself and others. Wisdom, so defined, includes knowledge, understanding and technological know-how, but much else besides. Like knowledge, wisdom can be conceived of as something that individual persons possess, and can also be conceived of in more impersonal terms as something possessed by institutions.

Inquiry rationally devoted to promoting wisdom - to helping humanity learn how to make progress towards a wise world - is what would have emerged if the 18th century Enlightenment programme had been developed and implemented free of blunders, if, in particular, social inquiry had been developed as social *methodology* rather than social *science*. If this had happened, we would have solved the second great problem of learning - the problem of learning how to become civilized or wise. But this did not happen. Instead, as I have already emphasized, social inquiry was developed as social science, and as a result what we have at present is a kind of academic inquiry which, though ultimately devoted to promoting human welfare (at least in principle) takes as its primary intellectual goal the pursuit of knowledge and technological know-how. This is the neurotic aim of academic inquiry. And this neurosis is, again, profoundly damaging in that it is this which has sabotaged the efforts of humanity to solve the second great problem of learning, to such an extent that most people probably believe that the problem is permanently insoluble.

5 Philosophical Neurosis

It may seem almost beyond belief that intellectual inquiry is riddled with rationalistic neurosis in the way that I have indicated. One can perhaps understand the pressures which have led to such a state of affairs arising in the first place. In Europe in the 15th and 16th centuries speculating about the nature of the universe could result in one being burnt at the stake. In the 18th century one risked imprisonment and worse if one speculated about such things as freedom, justice and democracy, and criticized existing religious and secular authorities. This continued to be the case in many parts of the world in the 20th century. It is entirely understandable that there are immense pressures on academics to refrain from tackling awkward problems experienced by people in their lives, having to do with such things as poverty, injustice, tyranny, bigotry, enslavement. But what is astonishing is that a certain wholesale avoidance of tackling such difficult problems of living in favour of tackling much less explosive problems of knowledge should persist even though this flies in the face of reason in the ways indicated. The neurotic blunders of natural science, social inquiry, and academic inquiry as a whole are *philosophical* blunders, in that they are blunders about fundamental *aims and methods*.

Where have the philosophers been all this time? Why have they failed to point these blunders out?

The answer is that philosophy is, perhaps, the most neurotic discipline of all. Far from struggling to free other disciplines from their neurotic straightjackets, philosophy has been most tightly bound in its own neurotic constraints.

The proper, unneurotic aim of philosophy is to help solve the most general, the most fundamental problems that there are. One absolutely fundamental, general problem is what may be termed "the human world/physical universe problem". This is the problem of understanding how the world as we experience it, imbued with sensory qualities, consciousness, free will, meaning and value, can be imbedded in the physical universe as conceived of by modern physical science.²⁷ The first serious task for philosophy is to keep alive an awareness of this problem, and keep alive imaginative and critical attempts to solve the problem. This philosophy (until recently, perhaps) has singularly failed to do. A second, related task for philosophy is to help humanity improve the aims and methods of various worthwhile endeavours in the light of problems (thus putting aim-oriented rationality into practice at a fundamental level). This includes the task of freeing inquiry from its neuroses. Again, philosophy has singularly failed to engage in this vital, fundamental task.

Instead, noble exceptions aside, philosophy in the 20th century has been split between arrogant, unintelligible bombast on the one hand, and sterile conceptual analysis on the other. On the one hand, philosophers like Hegel, McTaggart and Heidigger have overreached themselves, and have claimed to be able to arrive at secure knowledge about ultimate realities independently of science. On the other hand, other philosophers, most notably G.E. Moore, Bertrand Russell (in some of his phases) and the logical positivists, reacting against such bombast, have insisted on only an extremely modest role for philosophy, to the point almost of intellectual self-annihilation.

Thus, the logical positivists divided up all meaningful propositions into two categories, the empirical and the analytic (the empirical being verified by an appeal to experience, the analytic by an appeal to meaning). Science deals with the empirical; philosophy, not being an empirical science, must be devoted to establishing analytic propositions by means of analysis of meaning.

This positivist view was long ago rejected by almost everyone for all sorts of reasons. Mysteriously, however, the impoverishing implications for philosophy were not. For decades after logical positivism was first put forward in the 1930's, philosophy "in the analytic tradition" restricted itself to the analysis of concepts: mind, matter, truth, knowledge, justice, reality, and so on. And this "analytic" way of doing philosophy still lingers on.²⁸ Philosophy "in the analytic tradition" has only very slowly recovered from this travesty of what philosophy ought to be - this extreme philosophical neurosis.²⁹

Suffering from this self-imposed neurosis, "analytic" academic philosophy has been quite unable to perform its proper, serious, non-neurotic task: to tackle rationally (i.e. imaginatively and critically) our most general and fundamental problems, including problems concerning fundamental aims and methods of science, and of academic inquiry more generally.

A part of the problem is that, ever since logical positivism, "analytic" academic philosophers have been anxious about where and how philosophy fits in, as a respectable academic speciality alongside other specialities. (It was one of Wittgenstein's aims to turn philosophy into a respectable speciality, with an established method for analyzing meanings.) In the old days, philosophy, next to theology, was the supreme discipline. Then branches of philosophy split off and became established as independent sciences: first, natural philosophy became science; then such social sciences as economics and psychology split off from philosophy and became social sciences; and more recently, cosmology, logic and linguistics ceased to be a part of philosophy, and became

independently established sciences. It began to seem that nothing could be left after this process of attrition. Philosophy had become vacuous.

But this traditional view of philosophy spawning scientific disciplines until nothing remains itself rests on an untenable conception of science, and its relationship with philosophy, as we have seen. Granted SE, philosophy has no role within science. But granted AOE, philosophical, or metaphysical assumptions constitute a vital, central part of scientific knowledge. Furthermore, the philosophical task of developing and critically assessing the aims and methods of science, and of inquiry more generally, is vital for science, and for inquiry as a whole. Failure to engage in these tasks, due to neurotic obsession with "analysis", has made possible the neurosis of science, and of academic inquiry to perpetuate themselves unnoticed.

It is vital to appreciate, more generally, that not all academic specialities fit together in the same way. Philosophy is not just a speciality alongside other specialities. It is concerned with our most fundamental, general problems; the chief task of professional philosophers is to provoke others into thinking seriously about fundamental problems. It is not to do philosophy for others but, on the contrary, to keep alive a general discussion of philosophical problems above all amongst non-philosophers. A philosopher must be a kind of professional intellectual dilettante, interested in everything *except* academic philosophy, as it exists at present.³⁰

6 Self-Preservation of Institutional Neurosis

The persistence of scientific and academic neurosis cannot be blamed entirely on the neurosis of philosophy. Institutionalized neurosis has its own built-in mechanisms for self-preservation. As we saw above, SE, once accepted by scientists, protects itself from criticism by interpreting such criticism as philosophy of science, and therefore not a part of science. The failure of philosophers of science to justify SE is then interpreted by natural scientists as further justification for adopting the view that philosophy of science is best banished from science, an attitude that receives further support from the scientific sterility of neurotic philosophy of science. In this way neurosis tends to bring rationality into disrepute, thus discrediting the very intellectual tools needed to dismantle the neurosis.³¹

And there are other, related, factors that tend to preserve neurotic institutional structures, once established. Reputations and careers of senior natural and social scientists have been built up on the basis of, and within, the neurotic structure of science: such powerful insiders will not take kindly to the suggestion that their reputations and careers have been based on mistakes. Even those scientists who see through the neurotic structure of science will feel obliged to write, publish and teach in accordance with official, neurotic standards, simply in order to meet with acceptance and success. Thus do public myths perpetuate themselves, even when individuals, responsible for perpetuating them, no longer believe in them.

7 The Neurosis of Psychoanalytic Theory and Practice

I began by reinterpreting neurosis as a *methodological* notion, a notion in the theory of rational aim-pursuing. The outcome was that it became not just meaningful, but true, to assert that science suffers from rationalistic neurosis.

This casts an interesting light on the intellectual standing of Freudian theory, and psychoanalytic theory more generally. A number of philosophers and others have cast doubt on the intellectual standing of Freudian theory. Popper has criticized it for not being falsifiable, and hence not being scientific; Grünbaum has criticized it for having been either falsified, or not verified.³² And others have made other accusations.³³ But what the argument developed in this essay has shown is that it is not Freud who fails to match up to the exacting standards of science; on the contrary, it is *science* that fails to match up to the exacting intellectual standards of Freudianism reinterpreted

methodologically. Science suffers from rationalistic neurosis, and needs methodological therapy.

This indicates the tremendous increase in scope and power that accrues from reinterpreting Freudianism (and psychoanalytic theory more generally) *methodologically*. Thus reinterpreted, Freudianism becomes applicable to institutions, to anything that can be construed to pursue aims and to represent (and so misrepresent) aims being pursued.

One consequence of this methodological interpretation of Freud may not, however, be so welcome to Freudians. This is that Freudianism itself is rationalistically neurotic! Freud saw himself as a scientist, contributing to the science of the psyche. In this respect, Freud was just another product of the bungled Enlightenment, which involves interpreting social inquiry as social *science*. But, in general, social inquiry ought to be developed as social methodology; and in particular, Freudianism ought to be developed as methodology. In so far as Freudians in particular, and psychoanalysts more generally, conceive of their discipline as science, or as seeking knowledge of the human psyche, it suffers from rationalistic neurosis, and needs methodological therapy. Such a methodological reinterpretation of psychoanalysis - such a freeing of psychoanalysis from its rationalistic neurosis - would have profound implications for psychoanalytic theory and practice.

8 Conclusion

The natural and social sciences, and academic inquiry as a whole, suffer from rationalistic neurosis. This has far-flung damaging repercussions, both intellectual and social. At its most extreme, it means that humanity has not yet managed to develop traditions and institutions of learning well-designed from the standpoint of helping humanity learn how to create global civilization. This extraordinary state of affairs has persisted unrecognized in part because the intellectual failings are *philosophical* in character, and academic philosophy, which should have been actively correcting these persisting, institutionalized, philosophical blunders, has been obsessed with its own neurotic problems. We need an intellectual revolution to free the natural and social sciences, philosophy, and academic inquiry as a whole, from their damaging neuroses.

Notes

1. The notion of rationalistic neurosis was first introduced by me in print in (Maxwell 1984, 110-7).
2. To begin with, "science" means "physics", even "theoretical physics". This may seem a rather narrow interpretation of science. But first, in discussing the neurosis of theoretical physics I am, in effect, discussing the neurosis of the whole of natural science, in that theoretical physics is, from an explanatory standpoint, the fundamental science, and all other branches of natural science presuppose physics. And second, in Part II of this essay, I go on to consider broader neuroses of science that affect the whole of natural science, and technological research as well. And finally, in Part III, I will consider even more serious neuroses of social science, and of academic inquiry when considered as a whole. These are all repercussions of the neurosis of theoretical physics with which I begin.
3. As I use the term here, to say that the universe is comprehensible is to say that it is such that there is *something* (God, society of gods, cosmic purpose, unified pattern of physical law), which exists everywhere, throughout all phenomena, in an unchanging form, and which, in some sense, determines or is responsible for all change and diversity, and in terms of which all change and diversity can, in principle, be explained and understood. If this *something* is a unified pattern of physical law, then the universe is *physically* comprehensible. If there is not just one *something* responsible for all change, but a number of distinct *somethings*, then the universe is only comprehensible *to some extent*. The fewer the number, N, of distinct *somethings* that there are (other things being

equal), so the more nearly perfectly comprehensible the universe is, perfect comprehensibility obtaining if $N = 1$. For further details see text, and (Maxwell 1998, chs. 1, 3 and 4).

4. One of the referees of this paper has challenged this claim concerning the official aim of science. And it must be admitted that there are individual scientists who hold that an assumption about the simplicity or comprehensibility of nature is permanently implicit in the aim of science: Alan Sokal is one (personal communication). The mature Einstein might be cited as another (but even Einstein equivocates on the point: see (Maxwell, 1993) for a discussion). Despite this, we are justified, I claim, in holding that the scientific community as a whole holds, officially, that the aim of science (in the context of justification) is truth as such, no untestable (metaphysical) assumption being built permanently into the aim. As evidence for this sociological thesis, I would cite the following. First, it is not hard to find scientists asserting that evidence alone, in the end, decides what theories are to be accepted and rejected. Here are two examples. Max Planck: "Experiments are the *only* means of knowledge at our disposal. The rest is poetry, imagination", quoted in (Atkins 1983: xiv). Poincaré: "Experiment is the sole source of truth. It alone can teach us something new; it alone can give us certainty" (Poincaré 1952, 140). It is hard to find scientists asserting that some permanent metaphysical assumption *is* built into the aim of science. Second, one may note that most scientists endorse some version of Popper's demarcation criterion between science and non-science, encapsulated in the remark that "...in science, *only* observation and experiment may decide upon the *acceptance and rejection* of scientific statements, including laws and theories" (Popper 1963, 54). This, again, is not compatible with the idea that untestable assumptions are built into the aim of science. Third, and most important, if it was officially acknowledged that untestable metaphysical assumptions are built into the aim of science, there would surely be official discussion in physics journals and textbooks as to what exactly these assumptions are, and on what grounds they are made. Metaphysics and epistemology would play some role in undergraduate physics courses. But this does not happen. This alone establishes decisively, in my view, that the official view of the scientific community is that while, in the context of discovery, one may hope that the universe is simple, or even comprehensible, in the context of justification no such assumption can be made. For further grounds for holding this sociological thesis, see: (Maxwell 1984, chs. 2 and 6); (Maxwell 1998, ch. 2).

5. Two versions of SE need to be distinguished. On the one hand there is "bare" SE which asserts that empirical considerations *alone* determine choice of theory in science (including also, possibly, considerations having to do with empirical content). This is defended in (Popper 1959) and (van Fraassen 1980). On the other hand there is "dressed" SE which asserts that two considerations govern choice of theory in science: those that have to do with empirical success, and those that have to do with the simplicity, unity or explanatoriness of theories. The crucial point is that favouring simple theories must not commit science to assuming, permanently, that the universe itself is simple. Most scientists and philosophers of science uphold versions of "dressed" SE, even later Popper - see (Popper 1963, 241). Both versions are, I argue, untenable.

6. For a much more detailed exposition of this refutation of SE see (Maxwell 1998, ch. 2).

7. We require, in addition, perhaps, that the universe is presupposed to be such that the number of distinct fundamental physical entities, N , postulated by the true theory-of-everything, is not too large. If N is some enormous number, 10^{10} say, the universe can hardly be said to be physically comprehensible to any significant extent.

8. The distinction is conventional. We can always regard a universe made up of many particles all of one kind as being made up of one entity, a sort of discontinuous field, with non-zero values only where there are particles.

9. For a more detailed discussion of theoretical unity in physics, and its connection with symmetry, see (Maxwell 1998, chs. 3, 4 and appendix).
10. The range of possible assumptions considered here is only the tip of the iceberg. For further possibilities see (Maxwell 1998, chs. 1 and 3, and especially pp. 168-172, where a list is given of 20 different kinds of ways in which the universe might depart from perfect physical comprehensibility).
11. It may be asked: But how can acceptance of a level 3 assumption both influence, and be influenced by, acceptance of level 2 theories? The answer is that, at any stage in the development of science, rival level 3 ideas can contend; these lead to rival research programmes (Lakatos 1970), which can be assessed with respect to their relative empirical growth. Within a research programme, theories are rejected that clash with the basic level 3 idea; this idea is rejected if a rival research programme meets with greater empirical success over a period of time. Level 3 ideas are also assessed in terms of how well they exemplify the accepted level 4 thesis. (But this too is open to revision, if such a revision leads to a more empirically progressive research programme.) For further details of how metaphysical theses are to be selected, at various levels, partly on the basis of the empirical success and failure of rival research programmes, within the framework of AOE, see (Maxwell 1998, chs. 4 and 5).
12. Corresponding to each cosmological thesis, at level 3 to 10, there is a more or less problematic *aim* for theoretical physics: to specify that cosmological thesis as a true, precise, testable, experimentally confirmed "theory of everything". Aims corresponding to levels 9 and 10 are relatively unproblematic: circumstances will never arise such that it would serve the interests of acquiring knowledge to revise these aims. As one descends the hierarchy of cosmological assumptions, the corresponding aims become increasingly problematic, increasingly likely to be unrealizable, just because the corresponding assumption becomes increasingly likely to be false. Whereas upper level aims and methods will not need revision, lower level aims and methods, especially those corresponding to level 3, will need to be revised as science advances. Thus lower level aims and methods evolve within the fixed framework of upper aims and methods.
13. For further details see (Maxwell 1998, ch. 1).
14. This objection was made by the referee of this paper already referred to in note 5.
15. Kuhn (1970) has made familiar the idea that revolutions in science constitute theoretical ruptures or discontinuities. If one sticks to the two-tier view of science of SE, one may be obliged to adopt this view. But from the ten-tier perspective of AOE, it is obvious that revolutions in physics at least, constitute discontinuity at most at levels 2 and perhaps 3, there being continuity at level 4 and above (since Galileo at least, if not since the Presocratics). "*Far from obliterating the idea that there is a persisting theoretical idea in physics, revolutions do just the opposite in that they all themselves actually exemplify the persisting idea of underlying unity*" (Maxwell 1998, 181).
16. Recently, scientists and philosophers of science have leapt to the defence of the rationality of science in the light of postmodernist and sociological attacks: see (Sokal and Bricmont 1998); (Koertge 1998). What both sides of this battle overlook is that science as currently pursued and understood is not rational enough.
17. This is Popper's falsifiability criterion of demarcation: see (Popper 1959, 40-42).
18. I am personally well aware of the resistance of neurotic science to analysis, having been engaged in trying to get the argument of this paper, in one form or another, across to the scientific community for at least a quarter of a century: see (Maxwell 1974) for an early attempt.
19. For fascinating informal exposition and criticism of string theory see (Davies and Brown 1988); see also (Greene 1999).
20. One such physicist is Chris Isham: see (Isham 1997).
21. Given the SE picture of science, accepted laws and theories, and empirical data, are a part of knowledge, but ideas about *aims* are not; it is thus not at all obvious how values

are to influence the *content* of science, apart from illegitimately influencing acceptance and rejection of laws, theories, or empirical data. But given the AOE conception of science, ideas about *aims* are a part of the content of science. It becomes obvious that values may influence aims, but not acceptance or rejection of laws, theories or evidence. It must be admitted, however, that even in the context of aims, values legitimately influence research aims, but not judgements about what is true and false.

22. For a typical SE critique of the idea that values should influence science see (O'Hear 1989, 223-232). O'Hear takes it for granted that, in influencing science, values can only influence judgements concerning truth and falsity, which is clearly illegitimate. He fails to appreciate that it is inevitable and desirable that values should influence research aims, what scientists decide to try to develop knowledge about.

23. See (Hayek 1979) and (Fargaus 1993, Introduction).

24. "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" (Popper 1959, 44, n *1). See also (Popper 1963, 193-200); (Popper 1976, 115-6); (Popper 1972, 119 & 243).

25. For sociologists' perspectives see (Bloor 1976); (Barnes) 1974). For philosophers' perspectives see (Laudan 1977, ch. 7) and (Newton-Smith 1981, ch. X).

26. See Maxwell 1984, 1992, 2000).

27. Cartesian dualism is, of course, an early and immensely influential attempt at a solution to this problem. Much subsequent philosophy has struggled with problems engendered by Cartesian dualism: the mind-body problem, the problem of our knowledge of the external world, the problem of free will. But ironically, even though Cartesian dualism is nowadays rejected by most philosophers, this has not resulted in philosophy recognizing as fundamental to the discipline the problem which Cartesian dualism attempts to solve (or should be interpreted as attempting to solve). It is rare to find a philosopher asserting that the human world/physical universe problem is the fundamental problem of the discipline. Introductory courses in philosophy do not take this to be the basic problem of philosophy. In so far as such a tendency does exist, it began, perhaps, with (Smart 1963). The thesis that the human world/physical universe problem is fundamental to philosophy is defended in some detail in (Maxwell 2001).

28. Three influential "classics" of neurotic conceptual analysis are: (Wittgenstein 1953), (Ryle 1949), (Austin 1962).

29. In the last decade or so, in my view, there has been a tremendous upsurge in the intellectual vitality and seriousness of academic philosophy that is nominally "in the analytic tradition", due precisely to the repudiation of the idea that philosophy should restrict itself to analysis of concepts, and a return to the idea that philosophy should tackle real, fundamental problems about the real world. Two examples: (Kane 1996), (Chalmers 1996). Despite this, philosophy does not yet have the confidence to explore and critically assess actual and possible aims-and-methods of diverse human endeavours - science, industry, government, the law, education, the arts - with the aim of contributing to the *improvement* of these endeavours, in particular by detecting and "curing" rationalistic neurosis.

30. For a development of this theme see (Maxwell 1980).

31. More generally, within the neurotic aim for academic inquiry of acquiring *knowledge* one cannot easily raise questions about the desirability and rationality of this philosophy of inquiry, since to do so is to discuss rival views about what the aims and methods of inquiry ought to be, views that intermingle questions of value, fact reason and possibility, and which therefore do not fit into inquiry devoted to the pursuit of knowledge.

32. See (Popper 1963, 37-38); Grünbaum 1984).

33. See Grünbaum's book for references.

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