A Mug's Game? Solving the Problem of Induction with Metaphysical Presuppositions

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Abstract

This paper argues that a view of science, expounded and defended elsewhere, solves the problem of induction. The view holds that we need to see science as accepting a hierarchy of metaphysical theses concerning the comprehensibility and knowability of the universe, these theses asserting less and less as we go up the hierarchy. It may seem that this view must suffer from vicious circularity, in so far as accepting physical theories is justified by an appeal to metaphysical theses in turn justified by the success of science. But this is rebutted. A thesis high up in the hierarchy asserts that the universe is such that the element of circularity, just indicated, is legitimate and justified, and not vicious. Acceptance of the thesis is in turn justified without appeal to the success of science. It may seem that the practical problem of induction can only be solved along these lines if there is a justification of the truth of the metaphysical theses in question. It is argued that this demand must be rejected as it stems from an irrational conception of science.

I

"I think that I have solved a major philosophical problem: the problem of induction...This solution has been extremely fruitful, and it has enabled me to solve a good number of other philosophical problems. However, few philosophers would support the thesis that I have solved the problem of induction. Few philosophers have taken the trouble to study - or even to criticize - my views on this problem, or have taken notice of the fact that I have done some work on it."

This is how Karl Popper opens his book *Objective Knowledge*.[1] There are at least two oddities about what Popper says here. First, Popper is wrong; he did not solve the problem of induction. Second, even by 1971, when this passage was first published, Popper's work on the problem of induction had received a great deal of attention.

Popper's words might, however, be uttered by me with far greater justice. For I really have solved the problem of induction. The solution has been extraordinarily fruitful, and has enabled me to solve a number of other philosophical problems.[2] But few philosophers - if any - would agree that I have solved the problem. Few, indeed, have taken the trouble to study, or criticize, my work, or are even aware that I have done some work on the problem.[3]

I think I know why this is the case. First, it is no doubt the fate of most of us seeking to contribute to philosophy: our work sinks without trace, without comment. Second, the problem of induction has been around for a very long time; anyone claiming to solve the problem is almost bound to be wrong. Third, there is a kind of "negative judgement through persistent neglect" effect. The first version of my proposed solution was published nearly thirty years ago: if there was anything in it, surely someone would have noticed and taken up the idea, by now. Fourth, as Popper points out elsewhere,[4] "analytic" philosophy has tended to be more interested in analysis of concepts than in proposed solutions to fundamental philosophical problems. Fifth, my solution amounts to a radical improvement of Popper's attempted solution. Popper was hostile to this, and Popperians today are hostile to it, precisely because I have the temerity to claim that I have radically improved Popper's ideas. Anti-Popperians are indifferent because they know Popper has failed to solve the problem, and they assume my approach inherits Popper's failure. Finally, and perhaps most damagingly, my proposed solution involves recognizing that science makes a persistent metaphysical assumption of "uniformity" or "unity". Philosophers at once know that any attempt to solve the problem of induction along these lines is hopeless. As Bas van Fraassen once put it "From Gravesande's axiom of the uniformity of nature in 1717 to Russell's postulates of human knowledge in 1948, this has been a mug's game" (van Fraassen, 1985, pp. 259-60). There is no need to study or criticize my proposed solution to the problem of induction: I am playing a well-known mug's game.

There is not much that I can do about the first five reasons for ignoring my work on the problem of induction: I can however at least set out to demolish the sixth reason. This is what I propose to do in what follows. I first give a brief sketch of my proposed solution to the problem of induction (spelled out in much greater detail elsewhere); I then demolish the thesis that it amounts to van Fraassen's "mug's game".[5] My hope is that this may provoke one or two readers to take note of what I have done (see Maxwell, 1998; see also Maxwell: (1968, 1972, 1974, 1976, 1977, 1979, 1980, 1984b, 1993, 1997b, 1999, 2000b, 2001, 2002b, 2002c, 2002d, 2004b, appendix, section 6).

II

My solution to the problem of induction is contained in a view about the aims and methods of science - a philosophy of science[6] - that I call "aim-oriented empiricism" (Maxwell, 1974, p. 140; 1998, pp. 6-13; 2004a; 2004b, ch. 1 and appendix). In what follows I outline aim-oriented empiricism, indicate how it solves the “methodological” and “theoretical” problems of induction, demolish the thesis that aim-oriented empiricism represents a mug's game, and conclude by showing how the view solves what may be called the “practical” problem of induction.

The fundamental line of thought behind aim-oriented empiricism (AOE) can be indicated like this. Theoretical physics, and therefore all of natural science (since theoretical physics is fundamental for natural science), persistently selects fundamental physical theories that help to unify the whole of theoretical physics. Thus Newtonian theory (NT) unifies Galileo's laws of terrestrial motion and Kepler's laws of planetary motion (and much else besides). Maxwellian classical electrodynamics, (CEM), unifies electricity, magnetism and light (plus radio, infra red, ultra violet, X and gamma rays). Special relativity (SR) brings greater unity to CEM (in revealing that the way one divides up the electromagnetic field into the electric and magnetic fields depends on one's reference frame). SR is also a step towards unifying NT and CEM in that it transforms space and time so as to make CEM satisfy a basic principle fundamental to NT, namely the (restricted) principle of relativity. SR also brings about a unification of matter and energy, via the most famous equation of modern physics, $E = mc^2$, and partially unifies space and time into Minkowskian space-time. General relativity (GR) unifies space-time and gravitation, in that, according to GR, gravitation is no more than an effect of the curvature of space-time. Quantum theory (QM) and atomic theory unify a mass of phenomena having to do with the structure and properties of matter, and the way matter interacts with light. Quantum electrodynamics unifies QM, CEM and SR. Quantum electroweak theory unifies (partially) electromagnetism and the weak force. Quantum chromodynamics brings unity to hadron physics (via quarks) and brings unity to the eight kinds of gluons of the strong force. The standard model unifies to a considerable extent all known phenomena associated with fundamental particles and the forces between them (apart from gravitation). The theory unifies to some extent its two component quantum field theories in that both are locally gauge invariant (the symmetry group being U(1)XSU(2)XSU(3)). String theory, or M-theory, holds out the hope of unifying all phenomena.[7]

It might be thought that, during the last 400 years or so, science has been pursued in a thoroughly open-minded, unbiased fashion, theories being selected impartially on the basis of empirical success alone, the emergence of increasing theoretical unity being a surprising and purely empirical discovery - unifying theories just being much more empirically successful than disunified rivals. Nothing could be further from the truth. In fact, in connection with every accepted unifying theory - NT, CEM, and the rest - there have always been endlessly many, easily formulated, disunified rival theories very much more empirically successful than the theories that have been accepted.[8]

Thus, given NT, for example, 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 three kilograms of gold dust and three kilograms of diamond dust are heated in a platinum flask to a temperature of 450ºC, in which case gravitation will instantly become a repulsive force everywhere. And so on. 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.[9] Such theories can even be concocted which are *more* empirically successful than NT, by arbitrarily modifying NT, in just this entirely *ad hoc* fashion, so that the theories yield correct predictions where NT does not, as in the case of the orbit of Mercury for example (which very slightly conflicts with NT).[10]

This last point may be made more generally, as follows. Most accepted physical theories, for most of the time that they exist, are confronted by various empirical difficulties. Let T be any one of the above unifying accepted theories - NT, CEM, or whatever. Typically, T is confronted by the following empirical conditions. There is a domain A of phenomena for which the predictions of T are wholly successful; there is a domain B for which T fails to predict the phenomena because the equations of the theory cannot be solved; there is a domain C where T is ostensibly refuted (because the predictions of T clash with the phenomena of C, but this may be due, not to T yielding false predictions, but to experimental error, or relevant physical conditions not being taken into account); and finally there is a domain D of phenomena which T fails to predict because they lie outside the scope of T. (Here the phenomena, in A to D, are to be understood as consisting of low-level empirical or experimental laws.) It is now easy to concoct rivals to T that are much more empirically successful than T, as follows. One such rival asserts: As far as phenomena in A are concerned, everything occurs as T asserts; as far as phenomena in B are concerned, the phenomena occur in accordance with established empirical laws; and the same for C, and for D. This rival to T, T* let us call it, reproduces all the empirical success of T (in A), successfully predicts phenomena T is not able to predict (in B), successfully predicts phenomena that refute T (in C), and successfully predicts new phenomena, that lie beyond the predictive scope of T (in D). It might be demanded that T* should predict new phenomena; but this demand can be met too, since "phenomena" here, are laws with content in excess of actual experiments that have been performed. T* satisfies every imaginable requirement for being an empirically more successful theory than T.[11]

And this has been the situation for all the accepted fundamental physical theories indicated above, for most of the time that they have been in existence: endlessly many rival, disunified theories have been available, far more successful empirically than the accepted, unifying theories, and these empirically more successful, grossly disunified or, as I have called them "aberrant" theories (see Maxwell, 1974, p. 128) are all ignored.

As most physicists and philosophers of physics would accept, *two* criteria are employed in physics in deciding what theories to accept and reject: (1) empirical criteria, and (2) criteria that have to do with the simplicity and unifying capacity of the theories in question. (2) is absolutely indispensable, to such an extent that there are endlessly many theories empirically more successful than accepted theories, that lack unity, and are not even considered as a result.

Now comes the crucial point. In persistently accepting unifying theories, and excluding infinitely many empirically more successful, disunified or aberrant rival theories, science in effect makes a big assumption about the nature of the universe, to the effect that it is such that no disunified theory is true, however empirically successful it may appear to be for a time. Furthermore, without some such big assumption as this, the empirical method of science collapses. Science would be drowned in an infinite ocean of empirically successful disunified theories.[12]

If scientists only accepted theories that postulate atoms, and persistently rejected theories that postulate different basic physical entities, such as fields - even though many field theories can easily be, and have been, formulated which are even more empirically successful than the atomic theories - the implications would surely be quite clear. Scientists would in effect be assuming that the world is made up of atoms, all other possibilities being ruled out. The atomic assumption would be built into the way the scientific community accepts and rejects theories - built into the implicit *methods* of the community, methods which include: reject all theories that postulate entities other than atoms, whatever their empirical success might be. The scientific community would accept the assumption: the universe is such that no non-atomic theory is true.

Just the same holds for a scientific community which rejects all disunified or aberrant rivals to accepted theories, even though these rivals would be even more empirically successful if they were considered. Such a community in effect makes the assumption: the universe is such that no disunified theory is true.

Thus 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 untenable. Science makes one big, persistent assumption about the universe, namely that it is such that no disunified or aberrant theory is true. It assumes that the universe is such that there are no pockets of peculiarity, at specific times and places, or when specific conditions arise (gold spheres, gold and diamond dust, etc.), that lead to an abrupt change in laws that prevail elsewhere. Science assumes, in other words, that there is a kind of uniformity of physical laws throughout all phenomena, actual and possible. Furthermore, science *must* make this assumption (or some analogous assumption) if the empirical method of science is not to break down completely. The empirical method of science of assessing theories in the light of evidence can only work if those infinitely many empirically successful but disunified theories are permanently excluded from science independently of, or rather in opposition to, empirical considerations; to do this is just to make the big, permanent assumption about the nature of the universe.[13]

Let us call this assumption of unity U; and let us call the view, just outlined, that in persistently only accepting unifying theories science presupposes U, "presuppositionism".

Most current views about science deny that science makes a substantial, persistent assumption about the universe. This is true, for example, of logical positivism, inductivism, logical empiricism, hypothetico-deductivism, conventionalism, constructive empiricism, pragmatism, realism, induction-to-the-best-explanationism, and the views of Popper Kuhn and Lakatos.[14] All these views, diverse as they are in other respects, accept a thesis which may be called standard empiricism (SE): In science, theories are accepted on the basis of empirical success and failure, and on the basis of simplicity, unity or explanatoriness, but *no substantial thesis about the world is accepted permanently by science, as a part of scientific knowledge, independently of empirical considerations*. It deserves to be noted that even Feyerabend, and even social constructivist and relativist sociologists and historians of science uphold SE as the best available ideal of scientific rationality. *If* science can be exhibited as rational, they hold (in effect), then this must be done in a way that is compatible with SE. The failure of science to live up to the rational ideal of SE is taken by them to demonstrate that science is not rational. That it is so taken demonstrates convincingly that they hold SE to be the only possible rational ideal for science (an ideal which cannot, it so happens, in their view, be met).

Presuppositionism is of course incompatible with SE, and thus incompatible with all the above doctrines. One crucial point needs to be noted about the argument so far: presuppositionism is more rigorous than all the above versions of SE *entirely independent of any justification of U, or justification for accepting U as a part of scientific knowledge* (that is in addition to the one given above). In saying this I am appealing to the following wholly uncontroversial requirement for rigour. (R) In order to be rigorous, it is necessary that assumptions that are substantial, influential and problematic be made explicit - so that they can be criticized, so that alternatives may be developed and assessed.[15]

All versions of SE fail to satisfy (R) in just the way in which presuppositionism does satisfy (R). Presuppositionism makes the assumption U explicit (and so criticizable and, we may hope, improvable), while all versions of SE deny that science does make any such assumption as U. Thus presuppositonism is more rigorous than all versions of SE *even in the absence of any kind of justification of U*. In short, quite independent of any claim to solve the problem of induction, presuppositionism is more rigorous, and thus more acceptable, than any of the above versions of SE. This has a major implication for all attempts at solving the problem of induction: no such attempt can succeed if any version of SE is presupposed, since these all lack rigour. Attempts at solving the problem of induction must at least begin with presuppositionism, unless a better view of science emerges. Far from presupposing the uniformity or unity of nature being a mug's game, it is all the other way round: attempting to construe science in such a way that science does *not* presuppose the uniformity or unity of nature is the mug's game, since all such views of science fail to satisfy elementary requirements for rigour, namely (R), and thus cannot provide a basis for solving the problem of induction that can hope to succeed. Presuppositionism is the only non-Mug's game in town unless, as I have said, something better turns up.

Presuppositionism is, however, as it stands, untenable. This is because it is not at all clear what the assumption U is, or ought to be. It is vital to appreciate that there are endlessly many different assumptions of unity which science may be construed to make, almost all of which are false (since they contradict each other). Even more urgent than any problem of justification, there is the following problem: How can the assumption of unity being made by science at present, implicit in current scientific views as to what counts as theoretical unity, and almost bound to be false, be *improved*? What is at issue is not the traditional philosophical problem of *justification* (which presupposes that U is true), but rather the scientific (and quasi-Popperian) one of *improving* what is almost bound to be false.[16]

In surveying the different possible ways in which the universe may be unified, one important point to appreciate is that there is no single, sharp distinction between unity and disunity or aberrance. By "unity" we could mean merely that physical laws are the same throughout space and time. Or we could mean, in addition, that physical laws remain the same as other variables change, such as velocity, temperature, or mass (so that, for example, Newton's inverse square law of gravitation does not abruptly become an inverse cube law as masses of 1,000 tons are reached). Or, more restrictively still, we could mean (in addition) that there is only *one* force in nature, and not three or four distinct forces (such as gravitation, the electromagnetic force, and the weak and strong forces of nuclear physics). More restrictively still, we could mean that there is just one kind of particle in existence, or just one physical entity, a self-interacting field spread throughout space and time. Finally, and even more restrictively, we could mean that space, time, matter and force are all unified into one, unified, self-interacting entity.

Even more restrictive assumptions can be made, which specify the kind of entity or entities out of which everything is composed. And at the other end of the spectrum, much looser, less restrictive assumptions could be made which, if true, would still make science possible. Thus science could assume: the universe is such that local observable phenomena occur, most of the time, to a high degree of approximation, in accordance with some yet-to-be-discovered physical theory that is not too seriously disunified.

It is always possible, of course, that the universe only appears to be physically unified (to some extent). Perhaps, as theoretical physics advances, everything will become increasingly complex (as even some physicists believe[17]). Perhaps a malicious God is in charge, who has been controlling the universe up to now in such a way that it is as if physics prevails everywhere, but who, shortly, will startle us all by causing a series of dramatic, large-scale miracles to occur which violate all known laws. Perhaps as we probe deeper into physical reality we will discover that the universe exemplifies, not physical laws, but something that is closer to a computer programme (as some people have suggested). The universe may be comprehensible, but not *physically* comprehensible. That is, it may be that *something* exists - God, a society of gods, an overall cosmic purpose, a cosmic "computer" programme - which controls or determines the way events occur, and in terms of which, in principle, everything can be explained and understood: but this *something* may not be a unified physical entity, a unified pattern of physical law, and thus the universe, though comprehensible, is not *physically* comprehensible. Finally, the universe may not be comprehensible at all, and yet it might still be possible for us to live, and to acquire some knowledge of our local circumstances.

How do we choose between these endless possibilities? Science must make some kind of choice. It is all-important that science makes a correct choice, or at least a good choice, since this choice will determine what (non-empirical) methods are employed by science to assess theories. If science chooses a cosmological thesis that is radically false, then science will only consider false theories, and will exclude from consideration all theories that might take one towards the truth. Science will come to a dead end. The more restrictive the chosen cosmological assumption is, so the more potentially helpful it will be in selecting theories, but also the more likely the assumption is to be radically false, thus imposing a block on scientific progress. On the other hand, the looser, the more unrestrictive the assumption is, so the more likely it is to be true, but the less helpful it will be in excluding empirically successful "disunified" theories. (Other things being equal, the less one says, the more likely it is that what one says is true. "The universe is not a chicken" is almost certainly true about ultimate reality, just because it says so little, there being an awful lot of ways in which the universe can not be a chicken.)

It is all-important that science makes the right assumption about the ultimate nature of reality; and yet it is just here, concerning the ultimate nature of reality, that we are most ignorant, and are almost bound to get things wrong. How on earth are we to proceed?

The solution to this dilemma - the fundamental epistemological and methodological dilemma of science - is to make, not *one* cosmological assumption, but a *hierarchy* of assumptions, the assumptions becoming less and less restrictive, asserting less and less, as one goes up the hierarchy: see diagram (please enlarge or print for clarity). At the top of the hierarchy, at level 7, there is a cosmological thesis asserting so little that it is very likely to be true, and such that its truth is necessary for the acquisition of knowledge to be possible at all. This is justifiably a permanent assumption of science. At the bottom of the hierarchy (at level 3) there is a thesis so restrictive, so substantial in what it asserts, that it is almost bound to be false. This assumption will almost certainly need revision as science proceeds.

According to this view, then, scientific knowledge can be represented (in a highly schematic and simplifying way) as being made up of the following seven levels: see diagram.[18] 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

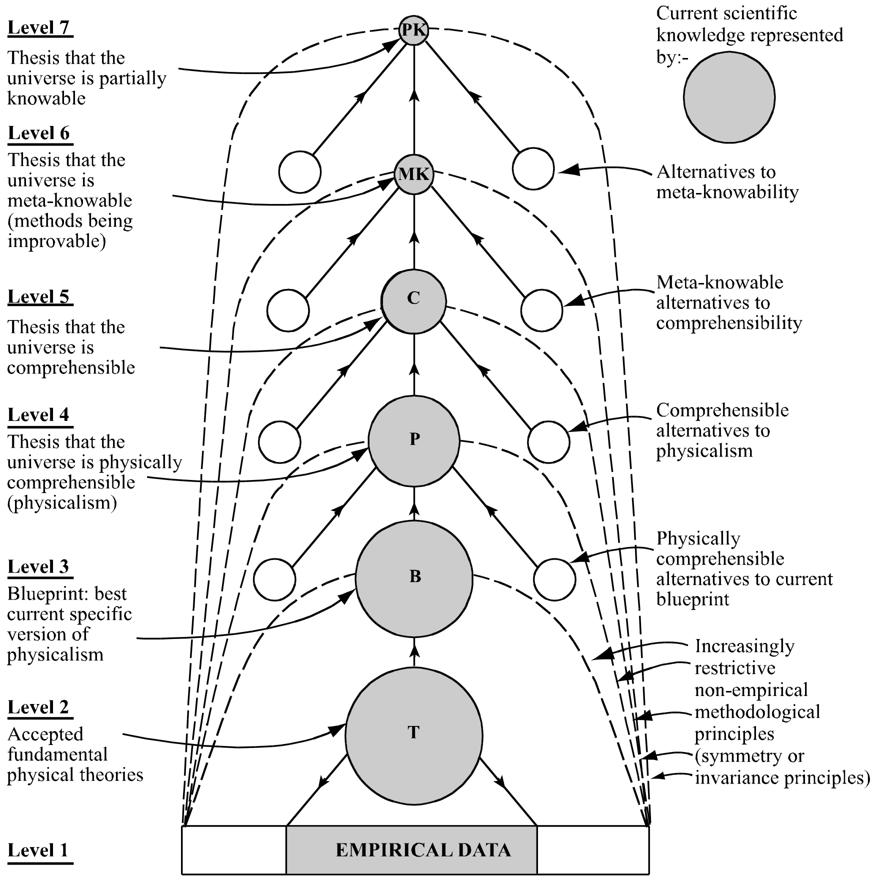

Diagram: Aim-Oriented Empiricism

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 *physically* comprehensible in some way or other (a thesis I shall call *physicalism*[19]). This asserts that the universe is made up one unified self-interacting physical entity (or one kind of entity), all change and diversity being in principle explicable in terms of this entity. What this amounts to is that the universe is such that some yet-to-be-discovered unified physical theory of everything is true. 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. This thesis asserts that the universe is such that there is *something* (God, tribe of gods, cosmic goal, physical entity, cosmic programme or whatever), which exists everywhere in an unchanging form and which, in some sense, determines or is responsible for everything that changes (all change and diversity in the world in principle being explicable and understandable in terms of the underlying unchanging *something*). A universe of this type deserves to be called "comprehensible" because it is such that everything that occurs, all change and diversity, can in principle be explained and understood as being the outcome of the operations of the one underlying *something*, present throughout all phenomena. Physicalism is a special case of this thesis. At level 6 there is the thesis that there is some rationally discoverable thesis about the nature of the universe which, if accepted, makes it possible to improve methods for the improvement of knowledge. "Rationally discoverable", here, means at least that the thesis is not an arbitrary choice from infinitely many analogous theses.[20] At level 7 there is the thesis that the universe is such that we can acquire some knowledge of our local environment sufficient to make life possible. (This, note, is a thesis about, or with implications for, the entire universe and not just about our local environment: if our local environment is to continue to be such that we can acquire some knowledge of it, then the rest of the universe must be such as to make this possible. There can be no explosions at the other end of the cosmos which spread with near infinite speed to engulf our environment.)

As we ascend the hierarchy, from level 3 to 7, 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 level 7 we arrive at a thesis so unspecific, so meagre in what it requires of the universe for it to be partially knowable, that it can only help and can never hinder the pursuit of knowledge, to accept this thesis as a part of knowledge whatever the universe may be like. This thesis is, as I have already said, justifiably a permanent part of scientific knowledge. At levels 3 to 6 we adopt that assumption which (a) is compatible with the assumption above it in the hierarchy (in so far as this is possible); (b) holds out the greatest hope for the growth of empirical knowledge (at levels 1 and 2); (c) seems best to support the growth of such knowledge.

Ideally, the thesis at level 2 implies the one at level 3, and so on up the hierarchy until one reaches level 7. This is true for levels 4 to 7. It breaks down dramatically, however, when we come to levels 2, 3 and 4. Fundamental theories currently accepted in physics, general relativity and the standard model, clash, and thus fail to exemplify physicalism. Furthermore, instead of postulating just one kind of self-interacting entity, the standard model postulates three kinds of forces, and many different kinds of particles with diverse properties, such as mass, that are not theoretically determined. All this is a sign of our ignorance (just as failure of theories to predict phenomena successfully is). What drives physics forward is the attempt to solve the problems that arise as a result of clashes between levels 1, 2, 3 and 4. According to this view, a basic task of theoretical physics will have been completed when a level 2 theory has been discovered which (a) in principle predicts all (physically) possible level 1 phenomena, and (b) implies a true level 3 thesis, which (c) exemplifies (and thus implies) the level 4 thesis of physical comprehensibility (physicalism).[21]

The diagram makes things look complicated, but the basic idea is extremely simple. By displaying assumptions and associated methods - aims and methods - in this hierarchical fashion, we create a framework of high level, relatively unspecific, unproblematic, fixed assumptions and methods within which low level, much more specific, problematic assumptions and methods may be revised as science proceeds, in the light of the relative empirical success and failure of rival scientific research programmes to which rival assumptions lead. 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.[22] Every effort is made, however, to confine such revisions to cosmological theses as low down in the hierarchy of theses as possible. Only persistent, long-term, dramatic failure (at levels 1 and 2) would lead us to revise ideas above level 3, let alone above level 4; only an earthquake in our understanding of the universe would lead us to revise ideas above level 5. 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, explosive growth of scientific knowledge.

It is this conception of science, postulating more or less specific, problematic, evolving aims and methods for science within a framework of more general, relatively unproblematic, more or less fixed aims and methods, that I call *aim-oriented empiricism* (AOE).[23] [For further details see Maxwell (1998), chapters 1 and 3-6; 2004a and 2004b] 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.[24]

This hierarchical view of AOE is put forward to solve the fundamental problem confronting presuppositionism, indicated above. It is put forward to solve the problem of *improving* the basic metaphysical assumption of science, implicit in persistent scientific preference for unifying theories even against the evidence, granted that some such assumption must be made, and it is almost bound to be false. The claim is that the hierarchical framework of AOE provides the best possible means for discovering metaphysical assumptions which best aid the task of improving knowledge of truth; AOE provides the best possible means for *improving* choice of metaphysical assumption. There is no attempt to justify the *truth* of metaphysical assumptions. At most, there is justification for choosing metaphysical thesis A over B granted that the aim is to make that choice which gives the best promise of aiding the search for knowledge of truth. Justification is involved only in the quasi-Popperian sense that the best possible justification of metaphysical assumptions that we can have is to expose these assumptions to the most searching criticism possible, to criticism best designed to promote progress in knowledge.

Something like AOE has always been implicit in scientific practice (otherwise science would have come to an end). AOE becomes all but scientifically explicit with the work of Einstein in discovering special and general relativity. Aspects of this work that are characteristic of AOE are the fundamental role played by the search for theoretical unity, and the vital role played by symmetry principles (such as Lorentz invariance and the principle of equivalence). These latter are fallible and revisable, and have the dual role of being both physical and methodological principles, all of which is integral to AOE. (They are represented in the diagram by the slanting dotted lines.) For further details see Maxwell (1993), pp. 275-305.

AOE, as indicated above, is intended to depict the metaphysical components of scientific knowledge given science as it exists today. AOE takes the specific form that it does in part because of what we have learned from Galileo onwards (or from the Presocratics onwards). History, in other words, is built into AOE. In the future, when we have learned more, AOE will be somewhat different. But however dramatic future revolutions in knowledge may be, we still ought to represent our knowledge in the same hierarchical form, with the same thesis, at level 7, at the top. Let us call this view "generalized AOE". When it comes to considering whether AOE succeeds in solving the problem of induction in a non-circular way, we need to consider various possible versions of generalized AOE which differ from AOE. The crucial question is: Can sufficiently good grounds be given for preferring AOE to all other rival versions of generalized AOE that one can think of? That is the proper way to formulate the problem of induction. (One striking feature of the problem of induction, as usually formulated, is its scientific sterility: work on the problem of induction has made no contribution to science, with the possible exception of Popper's work. But when the problem is formulated in the way just indicated, it is clear that it is potentially a highly fruitful problem for science: a version of generalized AOE that is genuinely an improvement over AOE is likely to be a major contribution to science itself.)

III

At this point, the basic objection to the whole approach being advocated here may be reiterated. Either AOE solves the problem of induction, or it does not. If it does not, no more needs to be said. If it does, then an element of justification must enter in. This in turn means that AOE must commit van Fraassen's mug's game. Choice of theory, at level 2, is justified in part by being compatible with choice of metaphysical thesis at level 3 or 4; this latter choice is justified in turn in terms of the success of science. We have here the vicious circularity of the mug's game. And it is inescapable. Interpreting AOE as a framework for detecting error, for criticism, does not help; even given this interpretation, there must be some justification for regarding metaphysical thesis U_2 as a better choice, an improvement over, more likely to be true than, thesis U_1: here, unavoidably, justification is present, which introduces the vicious circularity of the mug's game.

The first thing that needs to be said in response to this is that, as I have already emphasized, there is no question of justifying the *truth* (to some degree of certainty or probability) of any of the theses at levels 3 to 7. These theses remain, throughout, pure conjectures. I concur with Popper's thesis that all our knowledge is ultimately conjectural. (Whether such a view can claim to be the solution to the problem of induction is an issue I will take up below.)

At most, then, there is a justification for *accepting* such and such a thesis as a part of (conjectural) scientific knowledge, or *preferring* thesis A to thesis B.

Second, the top thesis is accepted on grounds which have nothing to do with the success of science at all. It is accepted because its truth is a necessary precondition for the acquisition of knowledge to be possible at all.

The thesis at level 7 asserts that the universe is such that it is possible for us to acquire some knowledge of our local circumstances (sufficient for it to be possible for us to continue to live). We are justified in accepting this thesis entirely in the absence of any justification for its truth (or probable truth), just because we have nothing to lose; accepting this thesis as a part of our knowledge can only help, and cannot obstruct, the task of acquiring knowledge whatever the universe is like (see Maxwell, 1998, pp. 186-7).

This elementary argument for permanently accepting this level 7 thesis can of course be challenged. What is beyond question, however, is that no circularity is involved here at all. The argument in support of accepting the level 7 thesis makes no appeal to the success of science whatsoever. Science is not even mentioned.

I might add that a part of the point of exhibiting the metaphysical assumptions of science in the form of a hierarchy, from level 3 to 7, is to overcome a fatal objection to one traditional approach to solving the problem of induction, versions of which have been argued for by, for example, Reichenbach (1938, sections 38-41), Braithwaite (1953, pp. 255-92) and Mellor (1988). This argues that we are rationally entitled to assume that there are sufficient regularities in nature for the inductive methods of science to meet with success because, if such regularities do not exist, no method will procure knowledge. But this argument tries to establish too much; it is not valid. Counterexamples can be imagined. The world might be such that "the inductive methods of science" meet with no success at all, and yet we can still acquire sufficient knowledge to live (see Maxwell, 1998, p. 185). The thesis of AOE, at level 7, might be called a "principle of uniformity", but it is very much weaker than the assertion that there are regularities such that "the inductive methods of science" meet with success. The fatal objection to the Reichenbach-Braithwaite-Mellor (RBM) approach is that (1) *either* it seeks to justify acceptance of a "principle of regularity" which, if accepted, suffices to justify science, but the argument is invalid; (2) *or* it is valid, but the "principle of regularity or uniformity" whose acceptance is justified is much too weak to justify science. AOE adopts (2), and recognizes that the acceptance of other, more restrictive "principles of uniformity" needs to be justified on other grounds; RBM, not acknowledging the hierarchy of principles, are doomed to opt for (1). There is another, related objection to RBM: "the inductive methods of science", at least as conceived of by RBM, are *not* the best available. They do not have the flexibility of the methods of AOE, which allow for the possibility of methods (associated with theses low down in the hierarchy) being *improved* in the light of improving knowledge, feedback being facilitated by the hierarchical structure of AOE between improving knowledge and improving knowledge-about-how-to-improve-knowledge (i.e. improving aims and methods). The traditional "inductive methods of science", as a result of their inflexibility, are both too restrictive, and not restrictive enough. Like most other traditional attempts at solving the problem of induction, RBM try to justify the unrigorous, and thus the unjustifiable. The status quo needs to be changed, improved, not justified.

What about the thesis of “meta-knowability” at level 6?

Here are two arguments for accepting meta-knowability which make no appeal whatsoever to the success of AOE science.

(i) Granted that there is *some* kind of general feature of the universe which makes it possible to acquire knowledge of our local environment (as guaranteed by the thesis at level 7), it is reasonable to suppose that we do not know all that there is to be known about what the *nature* of this general feature is. It is reasonable to suppose, in other words, that we can improve our knowledge about the nature of this general feature, thus improving methods for the improvement of knowledge. Not to suppose this is to assume, arrogantly, that we already know all that there is to be known about how to acquire new knowledge. Granted that learning is possible (as guaranteed by the level 7 thesis), it is reasonable to suppose that, as we learn more about the world, we will learn more about how to learn. Granted the level 7 thesis, in other words, meta-knowability is a reasonable conjecture.

(ii) Meta-knowability is too good a possibility, from the standpoint of the growth of knowledge, not to be accepted initially, the idea only being reluctantly abandoned if all attempts at improving methods for the improvement of knowledge fail.

(i) and (ii) are not, perhaps, very strong grounds for accepting meta-knowability; both are open to criticism. But the crucial point, for the present argument, is that these grounds for accepting meta-knowability, (i) and (ii), are independent of the success of science. This suffices to avoid circularity.[25]

But what about reasons for accepting theses at levels 5, 4 and 3? Are not these inevitably viciously circular? The thesis that the universe is comprehensible, at level 5, is accepted because no other idea, compatible with meta-knowability, has been so fruitful in generating empirically progressive research programmes; the thesis that the universe is physically comprehensible at level 4 is accepted because no other thesis, compatible with the level 5 thesis, has been so fruitful in generating empirically progressive research programmes;[26] and likewise for the thesis at level 3. In short, theories at level 2 are accepted because of empirical success and compatibility with level 3, 4 or 5 theses; and these theses are accepted because of their empirical fruitfulness. This would seem to be viciously circular in the most blatant fashion imaginable.

I have three arguments in refutation of this charge.

First, there is no question of the truth of theories being justified by an appeal to metaphysical theses, the truth of which is in turn justified by the success of science for the simple reason that AOE is thoroughly conjectural, and to that extent Popperian, in character, there being no attempt to justify the truth of either theories or metaphysical theses. Secondly, physicalism is *incompatible* with accepted fundamental physical theories, so there could be no question of the truth of one being justified by an appeal to the truth of the other. Physicalism is deployed to *criticize*, and to try to *improve*, accepted fundamental theories, not to *justify* their truth. Thirdly, and decisively, in so far as *acceptance* of physical theories is in part justified by an appeal to physicalism, whose acceptance is in turn justified by an appeal to the (apparent) success of science, which does involve a kind of circularity, this is licensed and legitimised by the level 6 thesis of meta-knowability. This asserts that the universe is such that there is some rationally discoverable thesis which, if accepted, makes possible the progressive improvement of more specific assumptions and methods in the light of the empirical success and failure of the research programmes to which they give rise. *If* meta-knowability is true, then progressively improving more specific metaphysical assumptions in the light of which seem to lead to the greatest empirical success, while at the same time choosing those empirically successful theories which best accord with these metaphysical assumptions, is just what needs to be done to make scientific progress. Meta-knowability, if true, justifies the element of circularity that is involved.

The gross invalidity of the genuinely viciously circular argument can be highlighted as follows. The argument seeks to justify acceptance of theory T by an appeal to metaphysical thesis M, and then justify acceptance of M by an appeal to the empirical success of T. But this argument works just as well (or ill) if we choose some empirically successful but horribly ad hoc rival to T, say T*, and a suitably ad hoc variant of M, say M*. We can now argue, with equal validity (i.e. none): we justify acceptance of T* by appealing to M*, and justify acceptance of M* by appealing to the empirical success of T*. We have here a way of testing whether or not a putative solution to the problem of induction is, or is not, viciously circular: it must provide some valid way of ruling out arguments that appeal to ad hoc theories and theses like T* and M*.

AOE, granted the level 6 thesis of meta-knowability, does provide this. Given that M accords with meta-knowability in being rationally discoverable, all ad hoc rivals of M (i.e. M*) are ruled out because these are not "rationally discoverable": M* is just one of infinitely many equally viable theses. Thus, *if* meta-knowability is accepted, AOE is not viciously circular - not circular in any invalid sense. Meta-knowability in effect asserts that the universe is such that no ad hoc or aberrant version of argumentation which appeals to T and M - a version which appeals to some T* and M* - can meet with success because all M*-type metaphysical theses are false.

It is of course absolutely vital that arguments for accepting meta-knowability do not themselves appeal to the success of science (for this would simply reintroduce vicious circularity at a higher level). The argument given above for accepting meta-knowability is weak, but it does not appeal, in any way whatsoever, to the success of science. Thus AOE is free of vicious circularity.[27]

IV

Even if AOE does not play van Fraassen's Mug's game, nevertheless how can it conceivably solve the *practical* problem of induction given its quasi-Popperian character?

Let me say at once that two versions of Popperianism deserve to be distinguished. On the one hand there is Popper's own view, which I shall call, with ironic intent, "dogmatic critical rationalism". This stresses merely the vital role that criticism has for rationality. Criticism is deployed, one might say, in an uncritical or almost dogmatic fashion. In contrast to this there is the version of critical rationalism which I wish to defend, which might be called "critical critical rationalism". This takes seriously the implications of a point emphasized, but not adequately followed up, by Popper, namely, that the whole point of *rational* criticism is to promote progress - and in connection with science, to promote progress in knowledge (and understanding). This means that theses which are demonstrably such that not accepting them can only harm and cannot help progress in knowledge whatever the universe is like, do not require (rational) criticism. They deserve to be permanently accepted. The cosmological thesis at level 7 is accepted on these grounds - in sharp contrast to anything found in Popper's work. Furthermore, it is all important, according to critical critical rationalism, to highlight that part of our knowledge which, we conjecture, it is most fruitful to criticize, from the standpoint of achieving progress in knowledge. Mere criticism is not good enough; we need to be critically critical,

critical of criticism itself, directing criticism to that which we conjecture it is most fruitful to criticize from the standpoint of achieving progress. A basic idea behind the hierarchy of AOE is just to display the metaphysical presuppositions of science in such a way that that which, we conjecture, it is most fruitful to criticize be brought to the fore, fruitful criticism being especially facilitated. Criticism needs to be directed, above all, at theses at levels 1, 2 and 3 - theses from 4 to 6 becoming increasingly unfruitful to criticize as we ascend the hierarchy, due to their increasing lack of factual content and increasingly indispensable role in the search for knowledge.

V

But how does any of this help - the reader may ask with rising impatience - with solving the practical problem of induction? I now address this question head on.

It is important to appreciate that there are three parts to the problem of induction. There is the *methodological* problem: to specify the precise methods involved in the choice of theory in science. There is the *theoretical* problem: to show that we are justified in accepting the scientific theories we do accept, granted the aim is to improve theoretical knowledge and understanding of the universe. And, perhaps hardest of all, there is the *practical* problem, the one just indicated above. The vast literature on the problem of induction is almost entirely devoted to the practical problem, but this is like trying to fly before you can crawl. The proper place to begin is with the methodological problem: if this has not been solved, to the extent of specifying *rigorous* methods for science, all efforts to solve the other two problems will be squandered on trying to justify the unjustifiable. This, in essence, is the reason for the long-standing failure of attempts to solve the problem(s) of induction.

Popper's falsificationism, like all other versions of SE, fails at the first hurdle (as I have, in effect, already pointed out above). Methods actually employed in physics involve persistently choosing unifying theories in preference to more empirically successful disunified rival theories. This in turn involves making a big, persistent metaphysical assumption, to the effect that all disunified theories are false. Rigour demands that this (implicit) metaphysical assumption be made explicit, within science, so that it can criticized and, we may hope, improved. But falsificationism cannot do this, because its criterion of demarcation declares metaphysics to be non-scientific. And no version of SE can do this either, because the metaphysical assumption, implicit in persistent scientific preference for unifying theories against the evidence, is repudiated, denied, by all versions of SE. All SE views about scientific method lack rigour.

And related to the lack of rigour, there is a lack of precise characterization. Presuppositionism, as we have seen, leads to AOE, and to the view that metaphysical assumptions and associated methods *evolve* with evolving knowledge. No version of SE (including falsificationism) can do adequate justice to this evolving, positive feedback aspect of scientific method.[28]

Finally, in order merely to specify precisely what the methods of physics are it is necessary to specify what unification in theoretical physics *is*. We have seen in outline, above, what this amounts to: the more the totality of fundamental theory in physics exemplifies physicalism, so the more unified it is. What matters here is the *content* of physical theory, not its *form*. This solution to the problem of saying what theoretical unity *is* (which defeated even Einstein[29]) falls naturally out of AOE: but it is unavailable to SE (because it involves acknowledging that physicalism is a part of scientific knowledge). Notoriously, attempts to say what theoretical unity is, within the confines of SE, have failed dismally (see Salmon, 1989, for a review). SE, constitutionally incapable of characterizing theoretical unity, is incapable of specifying precisely those methods of physics which appeal to the "unity" of theory. Falsificationism, like all other versions of SE, for these three inter-related reasons, fails to solve even the *methodological* problem of induction - whereas AOE solves the problem with ease.

Having failed to solve the methodological problem it follows at once, of course, that falsificationism and other versions of SE all fail to solve the theoretical and practical problems of induction as well.

This has a bearing on the question of whether the problem of induction is solvable at all. Most contemporary philosophers, historians and sociologists of science seem to have concluded that the practical or justificational problem is unsolvable, just because repeated efforts to solve the problem seem to have got nowhere. But what the above point shows is that there is an entirely straightforward reason for this failure. All these failed attempts failed even to formulate the problem correctly! To do that one needs first to have identified the correct methods of science.

There is another way in which the problem may be misformulated so as to render it insoluble. The formulation may make epistemological demands that are so high that they are quite impossible to fulfill. One such formulation would be: How can our confidence that empirically successful, accepted scientific theories are true be justified? This makes impossibly high epistemological demands. All physical theories so far put forward, whatever empirical success they may have achieved, are false![30] Formulated in this way, the problem is insoluble. A slightly less epistemologically ambitious formulation would be: How can our confidence that the empirical predictions of empirically successful, accepted theories are true be justified?" But this too asks for too much. All physical theories so far proposed, however empirically successful, yield false empirical predictions. A still less epistemologically ambitious formulation would be: How can our confidence that empirically successful, accepted theories yield true empirical predictions, within the standard range of phenomena (and accuracy) for which they have already been shown to be successful, be justified? But even this may ask for the impossible. Perhaps our customary confidence in science is misplaced. Perhaps just this is revealed by the correct solution to the practical or justificational problem.

In short, in order to avoid struggling to achieve the impossible, we need to formulate the problem in a somewhat more open-ended way than any of the above. The following stands a better chance of being solvable: How can our confidence that empirically successful, accepted theories yield true empirical predictions, within the standard range of phenomena (and accuracy) for which they have already been shown to be successful, be justified *in so far as such confidence is justified*?

We cannot just assume, from the outset, that the solution to the problem must justify our "pre-Humean" confidence in common sense and scientific knowledge - the confidence we had, that is, before learning of Hume's devastating arguments. For this may, again, be asking for the impossible. Perhaps Hume demonstrated, decisively, that such "pre-Humean" confidence is misplaced and unjustifiable. The correct solution to the justificational problem would, in this case, demonstrate just this to be the case. This, of course, is the view defended here.

We have seen that theoretical scientific knowledge makes assumptions about the cosmos. But there is more to it than that. Even very modest common sense knowledge, such as "I can walk across the room" or "This room will endure for the next ten seconds", makes assumptions about the cosmos, about the nature of ultimate reality. Such very modest common sense theses imply (a) that the entire cosmos is such that no cosmic explosion is occurring anywhere which will spread with nearly infinite speed to engulf and destroy the earth in the next few seconds. We only possess knowledge of such modest common sense items in so far as we possess knowledge of the cosmological thesis (a). Science and common sense both make cosmological assumptions.

If these cosmological assumptions can be established to be true in such a way that we are justified in being confident of their truth, then there is the hope that we can be justifiably confident of the truth of both some empirical predictions of empirically successful theories, and modest items of common sense. But if the relevant cosmological theses are such that they cannot conceivably be established to be true in such a way that we are justified in being confident of their truth, then it follows straight away, from elementary logic, that we cannot conceivably be confident of the truth of either the predictions of scientific theories or the items of common sense. Even our most modest, immediate, apparently secure items of common sense, having implications for the nature of the entire cosmos that are irredeemably speculative and conjectural, must themselves be irredeemably speculative and conjectural.

The relevant cosmological theses are indeed irredeemably speculative and conjectural, as everyone would surely admit. The conclusion is thus inescapable: scientific knowledge, and modest common sense knowledge, are both irredeemably speculative and conjectural as well. If we demand of the solution to the justificational problem of induction that it restores our pre-Humean confidence in scientific and common sense knowledge, then we demand the impossible. The attempt to restore such pre-Humean confidence can only undermine the rationality of science, in so far as it lulls us into a false sense of security, and leads us to believe that parts of our knowledge do not need critical scrutiny.

One demand that can be made of the correct solution to the justificational problem of induction is that it puts empirical data and scientific theories onto an equal epistemological footing. Ordinarily we assume we can be confident of the truth of factual statements about our immediate, observed surroundings: this is a table, that is a book, this is a Bunsen burner, and so on. Before encountering Hume (and his 20th century descendents, such as Einstein, Popper and Kuhn), we may feel equally confident of the truth (or approximate truth) of empirically successful, accepted scientific theories. But Hume's argument, reformulated a little, has the effect of opening up a gulf between evidence and theory. Any theory has infinitely many empirical consequences - and consequences for distant times and places. However many consequences we verify empirically, we will always be infinitely far away from verifying the theory. Even if particular items of evidence are known with absolute certainty, theories must, it seems, remain pure conjectures. They can be falsified, perhaps, but remain permanently unverifiable, to any degree whatsoever. The solution to the justificational problem must, it may be felt, remove this gulf in epistemological status between empirical data and theory.

AOE does just that, by making clear that empirical data, just like theories, contain cosmological implications. All knowledge, theoretical, empirical and common sense, is irredeemably conjectural because of these conjectural cosmological implications.

But if this is the case, what grounds are there for holding that AOE solves the justificational problem of induction? How does AOE do any better than Popper's conjecturalist position?

AOE, as have seen, solves the methodological problem, whereas Popper's falsificationism fails to do this. As a result, AOE is able to solve a very important part of the justificational problem which falsificationism, notoriously, fails to solve. This is the problem of discriminating decisively between those conjectures about whose truth we are so confident that we are prepared to entrust our lives to their truth, and conjectures about whose truth we have no confidence at all. Every time we fly in an aeroplane, cross a suspension bridge, or imbibe medicine, we entrust our lives to the correctness of relevant items of scientific knowledge, and confidently take for granted that rival conjectures that can be concocted (ostensibly even more empirically successful but horribly ad hoc) which predict we will die are all false. Accounting for this difference deserves to be regarded as the nub of the justificational problem. AOE accounts fully for this dramatic difference, whereas falsificationism entirely fails to do this.

John Worrall has dramatized the problem as follows. We are, let us suppose, standing on top of the Eiffel Tower, and we are confronted by two rival conjectures: one says if we jump we will float gently down to earth without harm; the other says we will fall in the usual way to our death (Worrall, 1989). Only lunatics think the first a viable possibility; the rest of us are absolutely confident in the truth of the second conjecture. How is this confidence to be justified? No version of SE comes up with an adequate answer, especially as aberrant versions of Newtonian theory or general relativity can be concocted which predict jumping on this occasion will lead to a soft, harmless landing, and which are empirically *more* successful than the non-aberrant versions of these theories. Can AOE justify our confidence that if we jump we will be killed?

If we grant the truth of the theses of AOE, from level 4 to 7, a straightforward answer can be given. Physicalism tells us that a unified pattern of physical law governs all phenomena. By far our best efforts at discovering invariant (or unified) laws governing such things as bodies in free fall near the earth's surface are Newton's theory of gravitation and, better still, Einstein's. No rival theory is even remotely as good at complying with the two requirements of (1) empirical success and (2) compatibility with physicalism. Theories that are empirically more successful and predict a gentle landing can be concocted, but these clash horribly with physicalism, and deserve to be rejected for that reason. But Newton's or Einstein's theory (plus additional information about such things as the mass of the earth) predict with stark clarity: jumping leads to rapid acceleration at roughly 32 ft per sec^2. Above all, a theory which accords with physicalism as well as Newton's or Einstein's theory, but predicts that jumping will lead to a gentle floating to the ground is nowhere on the horizon. Thus, given the truth of physicalism, there is absolutely no question, no grounds for serious doubt, whatsoever: jumping is for suicides only.

But we are not given the truth of physicalism. At most arguments deployed above give grounds for accepting physicalism granted our aim is to improve our conjectural knowledge of truth. There are arguments justifying *acceptance* of theses at levels 3 to 7, but no arguments justifying the *truth* of these theses. And it is the latter we require, it seems, to solve Worrall's problem, and the practical problem of induction more generally.

I have two replies to this objection.

First, even in the absence of any kind of justification of the truth of physicalism AOE succeeds, nevertheless, in distinguishing decisively between conjectures we are confident are true, to the extent even of entrusting our lives to their truth, and conjectures (even empirically more successful conjectures) about whose truth we have no such confidence.

Second, the demand that the truth of physicalism must receive some kind of justification before it becomes rationally acceptable for practical purposes is not just impossible to fulfil; it deserves to be rejected in that it stems from an unrigorous, untenable conception of science, and human knowledge more generally. If, and only if, some version of SE is correct, and science is based on evidence, and on metaphysical theses whose truth has been justified (if there are any) is the demand to justify the truth of physicalism itself justifiable. But all versions of SE are unrigorous and untenable. Hence, the SE demand to justify physicalism is itself unjustifiable, and must be rejected. What has been demonstrated above is that *all* significant factual knowledge, common sense and scientific, implies (and thus presupposes) cosmological theses: rigour requires that these unjustifiable cosmological theses are made explicit, so that they can be critically assessed and, we may hope, improved. To demand that such cosmological theses cannot be accepted unless their truth is justified condemns science to lack of rigour, because it ensures that unjustifiable cosmological theses will not be, and cannot be, accepted as a part of scientific knowledge. The demand deserves to be rejected.

Human knowledge has always had this inescapable cosmological dimension built into it. The illusion that science could dispense with such unjustifiable cosmological conjectures only crept in with the general acceptance of SE, some time after Newton and before the end of the 19th century. What needs to be done is not to justify the truth of physicalism, but rather to justify the claim that this cosmological conjecture has played a more fruitful role in the advance of science than any rival at that level. Just this was done above. *Science does not prove its cosmological conjectures; it sets out to improve those it has inherited from the past*. Physicalism is the best available, at that level of generality, and that suffices to solve the Worrall problem, and the justificational problem of induction. We are justified in entrusting our lives to the standard empirical predictions of those theories (a) which have met with sufficient empirical success, and (b) which, together with other such empirically successful theories, are more nearly compatible with our best metaphysical theses concerning the comprehensibility and knowability of the universe. Our best metaphysical theses, in turn, are those which have generated the most empirically progressive scientific research programmes. The circularity that seems to be involved here is legitimised by acceptance of meta-knowability.

VI

I now spell out in a little more detail the point just made, central to the solution to the practical problem of induction.

When viewed from the perspective of SE, it looks as if, in order to solve the practical problem, sufficiently good grounds must be given for the *truth* of physicalism, from scratch as it were. Acknowledging, initially, only knowledge of particular empirical facts, we must somehow provide an argument for the truth of physicalism which is so good that it justifies us in being entirely confident of the truth of physicalism even when our lives are at stake. Let us call this the "SE requirement" for solving the practical problem of induction. Given this requirement, the prospects for solving the practical problem of induction seem hopeless.

But this requirement itself deserves to be rejected. It is only acceptable if what I shall call "the SE prescription" is acceptable. But this latter is an intellectual disaster and deserves to be rejected. Hence the SE requirement must be rejected as well.

By the SE prescription I mean this: in order to develop science in a properly scientific, rigorous way, so that it is capable of delivering authentic, reliable knowledge, science must eschew all metaphysical presuppositions in the context of justification and base acceptance of scientific theories as far as possible solely on empirical considerations without reference to metaphysics. If this is correct, then it makes perfect sense to demand of any attempt to solve the practical problem of induction that it satisfies the SE requirement. But the SE prescription - eschewing metaphysical presuppositions in order to render science scientific and rigorous - is, we have seen, an intellectual disaster. It has entirely the opposite effect of the one intended. If taken seriously, instead of enhancing the rigour of science, it would destroy science and, indeed, all knowledge. The arguments of section II above demonstrate that science devoid of metaphysics is not possible. Selecting theories on the basis of empirical success and failure, no kind of assumption being made about what kind of world this is, cannot succeed if rigorously pursued because science would be overwhelmed by endlessly many empirically successful aberrant theories which would stultify science and render technological application impossible. (Or, if requirements of simplicity and unity are invoked, in addition to empirical requirements, then metaphysical assumptions of unity are being presupposed, but in a surreptitious fashion.) Science has survived and progressed despite, and not because of, acceptance of SE by the scientific community. Science has managed to do this by implementing SE in only a highly unrigorous and hypocritical fashion (implicit metaphysical presuppositions exercising a highly influential role over choice of theory).

The SE prescription is, then, an intellectual disaster. Unfounded metaphysical or cosmological assumptions about the comprehensibility and knowability of the universe are essential for science, and cannot be eliminated without disaster. What needs to be implemented, instead, is the "AOE prescription": roughly, endeavour to *improve* metaphysical assumptions explicit or implicit in current science and knowledge; do this by modifying existing assumptions in that direction which seems to be the most fruitful from the standpoint of acquiring empirical knowledge within a fixed framework of assumptions and methods which are such that these are required for the acquisition of knowledge to be possible at all. In other words, put generalized AOE into practice.

Before the advent of SE, the pursuit of science, or of knowledge more generally, invariably went on within a framework of religious and metaphysical assumptions. Christianity, the corpuscular hypothesis, and Galileo's thesis that the book of nature is written in the language of mathematics, played an especially important role in the sixteenth and seventeenth centuries as far as the birth and development of modern science was concerned. The orthodox SE prescription insists that science must be dissociated from such dubious, unfounded religious and metaphysical doctrines. But this cannot be done; at best, unrigorous and hypocritical science results. The impression that it has been done creates the insoluble problem of induction (as traditionally construed, from the SE perspective).

Instead, we need to see AOE science as explicitly *improving* on antecedently upheld religious and metaphysical theses. There is, in science, a substantial component of faith - but it is *rational* faith, openly acknowledged as conjectural in character, subjected to sustained criticism, and undergoing persistent modification in the direction of that which seems to lead to the most empirically progressive research, at levels 1 and 2. Science does not eliminate metaphysics; it implements a method which makes it possible for us to develop and choose those metaphysical ideas most fruitful for progress in empirical knowledge.

But once the SE prescription has been rejected as intellectually disastrous, and the AOE prescription accepted instead, it is clear that the SE requirement for solving the practical problem of induction must itself be rejected as intellectually unreasonable and unacceptable. The only rationale for adopting it arises from the idea that it is entirely proper to put the SE prescription into practice. But putting this into practice makes science impossible (for reasons wholly in addition to the resulting insolubility of the practical problem of induction). The SE requirement presumes a state of knowledge that has resulted from implementing the intellectually destructive SE prescription: this state of knowledge is an intellectual disaster, and must be rejected, and along with it the SE requirement. Taking the SE requirement seriously is rather like an athlete having both legs amputated and then expecting to win the 100 metres at the Olympics. Render science, and indeed all knowledge, impossible (by throwing away vital metaphysics) and it should occasion no surprise that a situation is created in which the practical problem of induction becomes impossible to solve as well.

For the last four centuries ever since Galileo, or perhaps for the last two and a half thousand years ever since the Presocratics, physicalism has been by far the most fruitful metaphysical thesis available from the standpoint of promoting progress in science, or in knowledge more generally. No rival metaphysical thesis has been remotely as fruitful. AOE correctly depicts our current scientific knowledge. Physicalism, in short, is justifiably a basic tenet of current (conjectural) scientific knowledge, our best attempt, at that vital level, of acquiring knowledge of truth, more secure, indeed, than any fundamental physical theory, such as quantum theory or general relativity. It makes very good sense not to jump off the Eiffel tower if you want to stay alive, for the reasons given above.

It may be asked: but if science must accept, and not eschew, religious or metaphysical ideas, what grounds are there for preferring physicalism to a thesis such as that God exists and benevolently arranges for the cosmos to be such as to make science possible? For my reply see Maxwell (1998, pp. 206-8; 2001, pp. 6-10; 2002a).

The argument given above is a special case of a more general argument concerning Cartesianism. The proper fundamental problem of epistemology is the problem of how we can best *improve* knowledge (improve what we have inherited from the past). The Cartesian prescription says: throw everything away we have inherited from the past except that which cannot be doubted, or is most secure: taking this as a secure base, build up rigorous, reliable knowledge. This Cartesian prescription has exercised a profound influence on philosophy, on both so-called rationalists and on empiricists. Influencing the former it leads to the search for secure principles founded on reason; influencing the latter it leads to the idea that observational knowledge alone constitutes the only acceptable Cartesian secure base, it being necessary to build the rest of knowledge up from this secure base. This, of course, is the SE prescription; it is a version of the Cartesian prescription. But the Cartesian prescription must be rejected. The proper way to set about *improving* knowledge is to acknowledge the conjectural character of what we have inherited from the past, subject it to critical scrutiny, and put generalized AOE into practice. In direct opposition to Cartesianism, this involves in part taking most seriously ideas that are most *vulnerable* to being found to be false, namely the falsifiable theories of science. Far from giving priority to ideas immune to doubt, we need to give priority to ideas most vulnerable to doubt. In this way, as Popper stresses, we make it possible for us to learn from our mistakes. But this Popperian prescription needs itself to be modified, as we have seen, so that some unfalsifiable, metaphysical ideas, inherited from the past, continue to be accepted and developed as a part of scientific knowledge, those in particular being accepted which either (a) must be true if knowledge is to be possible at all, or (b) are most fruitful in leading to progress in empirical knowledge. The Cartesian prescription deserves to be resoundingly rejected, and along with it the SE prescription and SE requirement.

The problem of induction is not just a philosophical puzzle à la Wittgenstein. Its long-standing insolubility is indicative of a fundamental defect in our understanding of science and its relationship with metaphysics and philosophy - a fundamental defect in our whole culture. Science does not stand opposed to metaphysics and philosophy; it is metaphysics and philosophy carried on employing the improved methods of investigation of empiricism: observation and, above all, experimentation (a point enshrined in the 17th century terms of "experimental" and "natural" philosophy). A basic task for philosophers today is to try to get across to the scientific community just how vital metaphysics and philosophy, properly conducted, are for science, so that scientists and philosophers can begin to collaborate on implementing AOE science, thus recreating natural philosophy. But this is unlikely to happen as long as AOE continues to be dismissed, so unjustly, as a mug's game.

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Notes

1. Popper (1972, p. 1).
2. These include: problems of unification and verisimilitude, the problem of rational scientific discovery, the problem of saying what it is that science has discovered about the ultimate nature of reality, problems concerning rationality, and the nature of social inquiry and, most important of all, the discovery that academic inquiry as it exists at present is profoundly defective when viewed from the standpoint of its capacity to help us learn how to become more civilized, there being an urgent need to bring about a revolution in the overall aims and methods of inquiry if we are to have what we so urgently need, a kind of inquiry rationally devoted to helping us acquire wisdom. See, for example, Maxwell: (1976; 1980; 1984a; 1984b; 1985; 1987; 1991; 1992; 1994; 1997a; 1998 ch. 3, 4 and 6; 2000a; 2002b; 2002d; 2003a; 2003b; 2003c; 2004a, 2004b).
3. Work of mine related to my proposed solution to the problem of induction has received some critical attention: see for example: Kneller (1978), pp. 80-91; Longuet-Higgins (1984); Collingridge (1985); Midgley (1986); Easlea (1986); Smart (2000); Hodgson (2002); Muller (2003).
4. Popper (1963, ch. 2).
5. My earlier defense against the charge of circularity has been brief and unsatisfactory: see Maxwell (1998, pp. 166-8).
6. One of the great mistakes made by endlessly many attempts at solving the problem of induction is to assume unthinkingly that science is wholly in order just as it is, the task being to find some way of justifying existing valid methods of science. What the long-standing insolubility of the problem of induction is trying to tell us, in my view, is that the orthodox conception of science, taken for granted by scientists and non-scientists alike, is untenable, and needs to be changed. Before the problem of induction can be solved we need to *change* the currently accepted conception of science; indeed, we need to change, not just our conception of science, but science itself. Properly conceived, the problem of induction involves formulating a new conception of the aims and methods of science, more rigorous than current conceptions, which is such that the problem of induction no longer arises. The task is not to justify the status quo, but to change the status quo so that the problem of justification no longer arises. Popper's failed attempt at solving the problem stands head and shoulders above the rest just because it fits this prescription: it consists of a new view about the aims and methods of science (at the time it was proposed), a new philosophy of science, namely falsificationism.
7. For the AOE account of what theoretical unification means see Maxwell (1998, ch. 3 and 4). See

also Maxwell (2004b, appendix, section 2).
8. In what follows "accept T" implies, not just "accept T as a working hypothesis for further research", but also "accept T for the purposes of technological and other practical applications, including contexts where human life may depend on the predictions of T being true".
9. All the possible phenomena, predicted by any dynamical physical theory, T, may be represented by an imaginary "space", S, each point in S corresponding to a particular phenomenon, a particular kind of physical system evolving in time in the way predicted by T. In order to specify severely disunified rivals to T that fit all available evidence just as well as T does, all we need do is specify a region in S that consists of phenomena that have not been observed, and then replace the phenomenon predicted by T with anything we care to think of. Given any T, there will always be infinitely many such disunified rivals to T.
10. For a more detailed discussion of empirically successful *ad hoc* rivals to accepted theories, see Maxwell: (1974; 1993; 1998, pp. 51-4).
11. The two prescriptions for formulating empirically more successful rivals to accepted unifying theories, indicated in this and the previous paragraph, can of course be combined to create further more empirically successful disunified rival theories.
12. It may be objected that physics might have developed in such a way that accepted physical theory reveals far less unity than the theories we in fact have today. In this case physics could not be construed as making a metaphysical assumption about underlying unity. But even in this counterfactual situation, endlessly many very much more disunified but more empirically successful rival theories could easily be formulated: these would have to be rejected on non-empirical grounds, or physics would drown in an ocean of rival theories. The persistent rejection of such much more disunified but empirically more successful rivals would involve the methods of physics making an implicit metaphysical assumption, to the effect that nature is unified to some extent at least (all grossly disunified theories being false). It is necessary to make some such assumption, however disunified the totality of accepted fundamental physical theory may be – even if the assumption made is rather weak in character, in that only gross disunity is denied.
13. For a much more detailed exposition of this refutation of standard empiricism see (Maxwell 1998, ch. 2).
14. For discussion of the claim that Kuhn and Lakatos defend versions of SE see Maxwell (1998), p. 40. Bayesianism might seem to reject SE, in acknowledging both prior and posteriori probabilities. But Bayesianism tries to conform to the spirit of SE as much as possible, by regarding prior probabilities as personal, subjective and non-rational, their role in theory choice being reduced as rapidly as possible by empirical testing: see Maxwell (1998), p. 44. For a critique of Bayesianism see Maxwell (2004b, pp. 188-191).
15. See Maxwell: (1984b), p. 224; (1998), p. 21.
16. What does it mean to say that U_2 is an "improvement" over U_1, U_2 being "closer" to the true characterization of the unity of nature, U_t, supposing there is such a thing, than U_1 is? One answer is that U_2 is an improvement over U_1 if U_2 is such as to lead to a more empirically successful research programme than U_1. But another answer can be given. Consider the steps that would need to be taken to move from U_t to U_2, and from U_t to U_1. Suppose there is some proposition, U_{t2}, which is such that (1) U_{t2} is a true specification of the unity of nature that is less precise than U_t, so that $U_t \rightarrow U_{t2}$; (2) there is a way of making U_{t2} more precise so that the resulting proposition is U_2, so that $U_2 \rightarrow U_{t2}$; (3) any proposition U^*_{t2} which satisfies (1) and (2) and is different from U_{t2} is less precise than U_{t2} (so that $U_{t2} \rightarrow U^*_{t2}$, but not vice versa). Suppose, too, that there is a proposition U_{t1} which satisfies (1), (2) and (3), but with U_2 replaced with U_1. Then we can say that:

(A) U_2 is closer to U_t than U_1 is (thus being an improvement over U_1) iff U_{t2} -> U_{t1} but not vice versa.

The intuitive idea, here, is that in order to get from U_t to U_2, we need to make less drastic, less fundamental, modifications to U_t than we do to get from U_t to U_1. U_2 is, as it were, more nearly a minor modification of U_t than U_1 is. U_t, U_1, U_2, U_{t2}, etc. all need to be understood to be more or less precise versions of a true version of physicalism.

None of this has any epistemological import. Like the problem of verisimilitude, it is purely a problem of what it means to say "U_2 is closer to the truth than U_1".

It would also be possible to declare U_2 to be closer to the truth than U_1 if there are physical theories, T_2 and T_1, precise versions of U_2 and U_1 respectively (so that T_2 -> U_2 and T_1 -> U_1, but not vice versa), such that T_2 is closer to the true theory of everything, T, than T_1 is (in the sense explicated in Maxwell, 1998, ch. 5), and there is no other such pair of theories, T^*_2 and T^*_1, such that T^*_1 is closer to T than T^*_2 is. This way of explicating "U_2 is closer to the truth than U_1" is, however, likely to be applicable in a much more restricted way.

17. For example: Gordon Fleming (personal communication).

18. This is an improved, if somewhat more complex, version of the view first put forward in Maxwell (1974), and a simplification of the even more complex version expounded in Maxwell (1998).

19. Smart (1963) has used the term 'physicalism' to stand for the view that the world is made up entirely of physical entities of the kind postulated by fundamental physical theories - electrons, quarks and so on. As I am using the term, 'physicalism' stands for the very much stronger doctrine that the universe is physically comprehensible, that it is such that some yet-to-be-discovered, unified "theory of everything" is true.

20. The universe being comprehensible is one way, but only one way, in which the level 6 thesis of meta-knowability may be true. Another way is for the universe to be such that an infinity of theoretical revolutions are required to capture the true theory of everything, the number of forces going up by one after each revolution. And there are other possibilities.

21. For further details see Maxwell (1998), ch. 1.

22. How can level 3 assumptions, or assumptions higher up in the hierarchy, both influence, and be influenced by, level 2 theories? What makes this possible is the feature of the hierarchy about to be indicated in the text, namely that, as one goes up the hierarchy, assumptions are more and more firmly upheld. Level 2 theories that accord with the best available level 3 assumption tend to be favoured over rivals that do not so accord. Nevertheless, a level 2 theory that clashes with the current level 3 assumption, but (a) accords with the level 4 assumption, and (b) is more empirically successful than theories that are in accord with the best level 3 assumption, will be accepted, and will lead to the rejection, or modification, of the level 3 assumption with which it clashes. Consider, however, a theory that clashes, not just with level 3, but level 4 as well, even though compatible with level 5, in such a way that no version of the idea that the universe is physically comprehensible, at level 4, can be rendered compatible with the theory. Such a theory would have to meet with far greater, sustained empirical success before it led to the overthrow of the current level 4 assumption. It would have to lead to empirical research programmes across a broad front of natural science even more successful than current science, based on the current level 4 assumption, before it would become acceptable. And this would be the case even more, given a theory that clashes with the current level 5 assumption. In short, an assumption at a given level may, for much of the time, determine choices lower down in the hierarchy; but every now and again, it may itself be revised, because the revised version accords better with the assumption above, or is more empirically fruitful or, more likely, both of these simultaneously.

23. Corresponding to each cosmological thesis, at level 3 to 7, 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". The aim corresponding to level 7 is relatively unproblematic: circumstances will never arise such that it would serve the interests of acquiring knowledge to revise this aim. 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.

24. Some features of AOE may seem reminiscent of Laudan's "normative naturalism" (see Laudan, 1984, 1987). There are however marked differences: "normative naturalism" is not committed to physicalism, and does not postulate the hierarchy of aims and methods of AOE, which makes the rational assessment of low-level aims and methods possible. I might add that Laudan's "normative naturalism" is derived from AOE, which was first expounded in a Colloquium I gave at Pittsburgh University in 1972, chaired by Laudan, the text of which became Maxwell (1974). Rescher has defended the view that science makes metaphysical presuppositions (see Rescher, 1973, 1977, 1987); his views also differ substantially from AOE. For an excellent survey of recent methodological views, including those of Laudan and Rescher (but excluding AOE) see Nola and Sankey (2000).

25. At this point I confess that in Maxwell (1998, p. 192-3) I give a third argument for accepting meta-knowability which does, perhaps, contain a whiff of circularity, in that it appeals to the *apparent* success of science. This suffices, I now think, to make this argument circular.

25. What justifies the claim that physicalism has been more fruitful for theoretical physics than any rival idea? This is justified by the point made in section II. All new, revolutionary, fundamental physical theories have been accepted because they (a) have brought greater unity to physics, and (b) have been more empirically successful, than any rivals - (a) being just as important as (b). In other words, *the* persisting non-empirical requirement for acceptance of revolutionary theory has been enhanced exemplification of physicalism (as far as theoretical physics as a whole is concerned) What irony that scientific revolutions - just that which convinced Kuhn (1962) that there are ruptures in science with nothing theoretical surviving each rupture - actually demonstrate just the opposite: the persistent and increasingly successful search for unity, the assumption of underlying unity being repeatedly reinforced by each successive revolution. It may be asked: But how can revolutionary theories reinforce physicalism when the totality of physical theory has always, up till now, clashed with physicalism? The answer: If physicalism is true, then all physical theories that only unify a restricted range of phenomena, must be false. Granted the truth of physicalism, and granted that theoretical physics advances by putting forward theories of limited but ever increasing empirical scope, then it follows that physics will advance from one false theory to another, all theories being false until a unified theory of everything is achieved (which just might be true). The successful pursuit of physicalism requires progressive *increase* in both empirical scope and unity of the totality of fundamental physical theory. It is just this which the history of physics, from Galileo to today, exemplifies - thus demonstrating the unique fruitfulness of physicalism.

27. For further details of the argument for AOE see Maxwell: (1998), (2001, ch. 3 and app. 3), (2002b), (2002d) and (2004).

28. Laudan (1977, 1984), inspired by Maxwell (1974) and earlier personal communication, does argue for changing methods of science within an SE view. But because of the anti-realist and SE character of his view, its lack of the hierarchical "meta-methodological" character of AOE, Laudan cannot do justice to the idea that new aims-and-methods need to be *appraised* so that those selected *improve* on

earlier aims-and-methods, there being positive feedback between improving knowledge and improving knowledge-about -how-to-improve-knowledge - a key feature of the rationality of science, according to AOE, and one which helps account for the explosive growth of modern science.

29. See Einstein (1949, pp. 20-25) and Maxwell (1998, pp. 105-6).

29. Galileo's laws of terrestrial motion and Kepler's laws of planetary motion are contradicted by Newtonian theory, in turn contradicted by special and general relativity. The whole of classical physics is contradicted by quantum theory, in turn contradicted by quantum field theory. Science advances from one false theory to another. Viewed from an SE perspective, this seems discouraging and is often called "the pessimistic induction". Viewed from an AOE perspective, as I have already mentioned, this mode of advance is wholly encouraging, since it is required by AOE. Granted physicalism, the only way a dynamical theory can be precisely true of any restricted range of phenomena is to be true of all phenomena. All physical theories must be false until we obtain a theory of everything!