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What Is Scientific Progress?

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1 Introduction

What is scientific progress? The answer is simple. Science (or some particular scientific field or theory) makes progress precisely when it shows the accumulation of scientific knowledge; an episode in science is progressive when at the end of the episode there is more knowledge than at the beginning. This simple, cumulative conception of scientific progress is not original; indeed it has a venerable history.[1] Yet philosophers of science have almost entirely ignored this conception, at least since it was condemned by Kuhn and others in the 1960s. Even in the realist reaction against positivism and relativism the cumulative conception has not been rehabilitated. Realists have typically sought an account of progress in terms of increasing verisimilitude (truth-likeness, approximate truth) rather than increasing knowledge.

In this paper I will be comparing three approaches to characterising scientific progress: (i) the epistemic approach, (ii) the semantic approach, and (iii) the functional-internalist approach. The epistemic approach takes *knowledge* to be the concept we need in order to understand what progress is. (The only version of the epistemic approach I shall consider is the cumulative knowledge account I have already advertised.) The semantic approach takes *truth* (or verisimilitude) to be the central concept in defining progress. And the functional-internalist holds that progress is made when a scientific development succeeds in fulfilling a certain function (such as solving a scientific problem), where that function is understood in such a way that the scientific practitioners are themselves in a position to judge whether the function has been fulfilled.

I shall argue for the epistemic approach, the simple-minded cumulative knowledge account of progress, as follows. In §§ 2.1–2.2 I shall consider cases in which there are (i) differences between the accumulation of truth (or increasing verisimilitude) and the accumulation of knowledge and (ii) differences between the production of problem-solutions and the accumulation of knowledge. In such cases our intuitions about whether there is progress show that progress matches changes in knowledge, but not changes in truth or in problem-solving. In §§ 3.1–3.4 I shall look at the motivations for the semantic approach, in particular in its verisimilitude form, and show that the semantic approach has no theoretical advantages over the epistemic approach. In §§ 4.1–4.2 I likewise show that the alleged reasons for taking the functional-internalist approach do not stand up to scrutiny. Before concluding I briefly examine, in § 5, the link between the epistemic approach and the aim of science.

2.1 The epistemic conception versus the semantic conception of progress

One version of the semantic view of progress would be the claim that scientific progress is the accumulation of true scientific beliefs. This version has a structural similarity with my version of the epistemic approach, with truth instead of knowledge. But the difference between a semantic concept and an epistemic one is of course a deep difference indeed (even if it is not always respected by philosophers of science).[2] The cumulative truth view has had few explicit supporters[3]; the more popular version of the semantic approach takes scientific progress to be a matter of increasing verisimilitude. Verisimilitude or nearness to the truth has been the realists' weapon of choice in their battles against anti-realists. Popper, somewhat forlornly, hoped that his adherence to verisimilitude would differentiate him from his anti-realist contemporaries. More recently, Ilkka Niiniluoto (1987; 1999) has sought to build a whole philosophy of critical scientific realism on the foundations of his own account of verisimilitude. And a central component of Niiniluoto's critical scientific realism is the idea that progress is a matter of increasing verisimilitude (1980, 428; 1984, 76; 1999, 201).

In this section I shall show that the semantic conception yields a verdict about progress in certain kinds of case that is at odds with our intuitions. Given that science is an epistemic activity it seems almost tautologous to suggest that its success and so progress should be measured by epistemic standards. I shall argue that our intuitions concerning progress in possible episodes of change do imply that epistemic characteristics are essential to progress.

The semantic and epistemic accounts diverge when it comes to considering beliefs with insufficient epistemic support to count as knowledge. Imagine a sequence of beliefs that show the accumulation of truth. (Or increasing nearness to the truth—whether one prefers to couch the argument in terms

of accumulating truth or increasing verisimilitude is immaterial.) Let the truth (or the increasing verisimilitude) of these beliefs be entirely accidental—this is a sequence of lucky guesses or lucky irrational beliefs. Such a sequence will be progressive on the semantic approach but not on the epistemic approach. The latter gives the correct verdict here. Progress and rationality cannot diverge that easily.

Imagine a scientific community that has formed its beliefs using some very weak or even irrational method M, such as astrology. But by fluke this sequence of beliefs is a sequence of true beliefs. These true beliefs are believed solely because they are generated by M and they do not have independent confirmation. Now imagine that at time *t* an Archimedes-like scientist in this society realises and comes to know that M is weak. This scientist persuades (using different, reliable methods) her colleagues that M is unreliable. This may be that society's first piece of scientific knowledge. The scientific community now rejects its earlier beliefs as unsound, realising that they were formed solely on the basis of a poor method.

On the semantic view this community was making progress until time *t* (it was accumulating true beliefs) and then regressed (it gave up those beliefs). This, it seems, contradicts the verdict of our intuitions about this episode. The acquisition of beliefs by an unreliable method cannot be genuine scientific progress, even if the beliefs so acquired are, by accident, true. Far from being a regressive move, giving up those unreliably produced beliefs, because of a now well-founded belief that they were unreliably produced, is a positive, progressive step. So the semantic view yields a description in terms of progress and regress that conflicts with what we are intuitively inclined to say.

On the epistemic view that community made no progress at all until *t* at which time it did start to make progress. This, by contrast with the semantic view, does accord with the verdict of intuition. Figures 2.1 (a) and 2.1 (b) below show the change in true scientific belief and scientific knowledge respectively. Intuition tells us that fig. 2.1 (c) charts scientific progress. And so I conclude that progress does not match change in true belief but does match change in knowledge. Clearly the same argument may be run where the society forms a sequence of beliefs with increasing but accidental verisimilitude. Hence the semantic conception does not capture out intuition about scientific progress, whereas, in this case at least, the epistemic conception does.

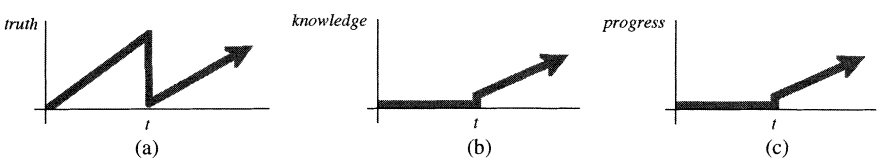

Figure 2.1. Progress matches changes in knowledge, not in truth alone, nor verisimilitude.

In the history of science there are, for good reasons, no episodes that illustrate precisely such a divergence between truth and rationality. Most scientists have been pretty rational and science as a whole is moderately if imperfectly rational. There are some exceptions to the latter. As is well known Réné Blondlot believed in the existence of what he called N-rays for what it is clear were entirely spurious and irrational reasons. Imagine for sake of argument that we were to discover that there are in fact hitherto unobserved entities answering to Blondlot's description of N-rays. So Blondlot's belief in N-rays would have been true but unjustified and not knowledge. The semantic approach would have to regard Blondlot's belief (which was widely shared in France) as constituting progress. That is clearly wrong.

Cases such as Blondlot's are atypical. There are actual (rather than imaginary) cases of true scientific beliefs that were not knowledge. But because of the general rationality of science, these beliefs will typically have had at least some confirmation. Hence we are entitled to show some admiration for scientists who got it right but lacked the evidence to prove their hypotheses. Aristarchus of Samos proposed a heliocentric system 2,000 years before Copernicus, with the geocentric views of Aristotle and Ptolemy filling the intervening gap. Alfred Wegener famously proposed the theory of continental drift over half a century before it became accepted. We might be tempted to think that science missed a trick or two here. Would it not have been more progressive to have accepted these theories at the time rather than reject them? The semantic approach unhesitatingly says *yes*. The epistemic approach is more circumspect. Neither Aristarchus nor Wegener had sufficient evidence for his hypothesis. Furthermore each was faced by strong counter-evidence. Had their theories been adopted, the result would not have been scientific knowledge. So it is not the case, on the epistemic view, that the history of science missed opportunities to make immediate progress.

2.2 The Epistemic Conception Versus the Functional-Internalist Conception of Progress

In this section I will look at what I call the functional-internalist approach to progress. The principle representatives of this approach are the puzzle- and problem-solving views of Kuhn and Laudan. (Much of what I have to say will apply to Lakatos' methodology of scientific research programmes also.) The view is functional because they take progress to be a matter of the success a scientific field has in fulfilling a function—that of solving problems. Why it is internalist I shall come to shortly.

When compared with the verisimilitude view, there is a superficial resemblance between the problem-solving conception and my cumulative knowledge view. While I see scientific progress as the accumulation of scientific knowledge, that view sees progress as the accumulation of solutions to scientific puzzles. The two views would coincide if:

(i) solving a puzzle is a matter of acquiring a certain sort of knowledge; and
(ii) all scientific knowledge is knowing some puzzle-solution.

However, the views do not in fact coincide since neither (i) nor (ii) is true. Furthermore, where the views differ, the cumulative knowledge account has the advantage.

Considering (ii), it is clear that not all scientific knowledge is a matter of knowing the solution to some puzzle. Astronomers and naturalists of the eighteenth and nineteenth century spent lives collecting data on stars and comets, or on new species and habitats. These were contributions, albeit not dramatic, to scientific progress. At the same time there are serendipitous discoveries that progress science whose importance is clear without their being solutions to any puzzles.

More significant is (i). Kuhn and Laudan do not think of solving a puzzle as involving knowledge, when knowledge is understood in the classical way as requiring truth. For Kuhn a puzzle is solved when a proposed solution is sufficiently similar to a relevant paradigmatic puzzle-solution. Laudan's account is superficially similar to Hempel's D-N model of explanation—a problem phenomenon P is solved by T when one can deduce P from T. But importantly Laudan does not require that T is true. He does not even require that P really exist: "A problem need not accurately describe a real state of affairs to be a problem: all that is required is that it be *thought to be* an actual states of affairs" (Laudan 1977, 16; Laudan's emphasis). Nicole d'Oresme and his contemporaries believed that hot goat's blood would split diamonds (Oresme 1968; example cited in Laudan 1977, 16). A theory from which the splitting of diamonds by hot goat's blood is deducible would, according to Laudan, provide a solution to Oresme's problem. It is clear that both Kuhn and Laudan countenance contributions to scientific progress that do not involve any knowledge. In Laudan's case this is because he thinks that we never have scientific knowledge—he accepts the pessimistic induction as sound. In Kuhn's case this was initially because he wanted to give an account of scientific change that is neutral over questions of truth and knowledge. In the postscript to the second edition of *The Structure of Scientific Revolutions* Kuhn adds to this an attack on the concepts of truth and verisimilitude.

Because they believe that as matters are normally understood knowledge and truth are problematic, Kuhn and Laudan adopt the classic strategy of the anti-realist. The anti-realist is impressed by sceptical arguments that on the basis of evidence E one cannot get to know facts of kind K, where 'K' in conceived of in a standard, natural way. But rather than be an out-and-out sceptic the anti-realist reconceives 'K' in such a way that 'K' *can* be inferred from E. And the easiest way of doing this is to reconceive 'K' in such a way that it is more or less identical to E (just as Berkeley reconceives 'corporeal substances' as identical to sensations). Similarly, it is natural to think that

success in problem-solving is *evidence for* the progress of science, when the latter is understood as the accumulation of knowledge. If one is sceptical about such an inference, one might conclude that we do not have scientific progress, or at least that we do not know whether we do. But one may prefer instead to reconceive of scientific progress so that it is identical to success in problem-solving. In which case the sceptical worry just evaporates and one is able to share the talk of the vulgar who do think that there is scientific progress. This move is clear in Laudan, who asserts, "... what I am suggesting is that we apparently do not have any way of knowing for sure (or even with some confidence) that science is true, or probable, or that it is getting closer to the truth. Such aims are *utopian*, in the literal sense that we can never know whether they are being achieved." (Laudan 1977, 126–7) Laudan uses this sceptical admission to justify his account of progress:

> "The workability of the problem-solving model is its greatest virtue. In principle, we can determine whether a given theory does or does not solve a particular problem.... If we have had to weaken our notions of rationality and progress in order to achieve this end, we are at least now in a position to *decide* whether science is rational and progressive—a crucial necessity denied to us if we retain the classical connections between progress, rationality, and truth." (Laudan 1977, 127)

It is this that makes Laudan's account (and similarly Kuhn's account) *internalist*. Whether or not a scientific community has made progress can be judged and known by that community, and does not depend on features that may be unknown to them (such as whether their problem is genuine and whether their solution is true or known).

But matters are not straightforward for the anti-realist conception of progress. If Oresme can solve his problem by coming up with a theory from which the splitting of diamonds by hot goat's blood is deducible, then Oresme has thereby contributed to scientific progress, according to Laudan. In fig. 2.2 (a) below the upward diagonal line represents this increase in problem-solving. If Oresme's solution is sufficiently similar to an appropriate paradigm then it will be a contribution to progress according to Kuhn's standards also. While such a solution might reasonably have seemed to Oresme and his contemporaries to be a contribution to progress it is surely mistaken to think that this *is* therefore a contribution to progress. Indeed, given that the solution would involve a falsehood as much as the problem itself Oresme's alleged progress adds falsity to falsity. Furthermore, imagine that some second scholar later comes along and proves at time t by impeccable means that Oresme's solution cannot work. Whereas we had a solution before, we now have no solution. Hence in fig. 2.2 (a) at time t the line dips downward. By Laudan and Kuhn's standards that would mark a *regress*. But the correct thing to say is that the later scholar did indeed contribute to progress in a

small way, by giving us knowledge that something previously thought to be true is in fact false. Fig. 2.2 (b) charts the change in knowledge. Fig. 2.2 (c) depicts our intuitions concerning the change in progress, viz. that Oresme's false solution to a false problem was not scientific progress, but the knowledge that his false solution is indeed false is some small contribution to progress. The fact that the graph for progress, (c), matches the graph for knowledge, (b), but not for problem-solving, (a), indicates that the problem-solving account of progress conflicts with intuition.

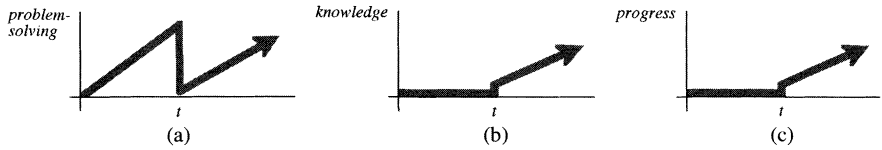

Figure 2.2. Progress matches changes in knowledge, not in problem-solving, internally construed.

This case is analogous to that considered when examining the semantic conception of progress. In that case we saw that an accumulation of truth (or verisimilitude) is insufficient for progress. In this case we see that an increase in problem-solving power is insufficient for progress.

An upshot of Laudan's and Kuhn's view of problem-solving and progress is that revisionary scientific changes (such as scientific revolutions) are not progressive in a straightforward way. Kuhn says that there is progress through revolutions, because the new paradigm solves more problems than its predecessor. But while the later paradigm may solve more problems it need not solve (or dissolve) all the problems that were previously solved. For example, Descartes' vortex account of planetary gravitation accounted for the fact that the orbits of the known planets are co-planar and have the same sense of rotation. Yet this account of gravitation was superseded by Newton's theory which gave no explanation of the phenomenon. Similarly the geocentric view of the planets made it quite natural that the Moon should present the same face towards the Earth while the heliocentric view does not. According to Laudan and Kuhn the move to the Newtonian and geocentric theories was accompanies by a loss of problem solutions in this respect. This phenomenon—often called *Kuhn-loss*—certainly raises interesting questions of why the transitions were made. That is not our current concern. Our current concern is whether we can describe the changes as progressive. For Laudan and Kuhn to be able to describe the changes as progressive we have to take progress to occur despite loss of problem-solving power in some areas. Thus we have to take an increase in problem-solving power in other areas to outweigh the losses described. That in turn requires that we are able to quantify problem-solving power which means that we need an idea of how to individuate and count problem solutions and how to weigh their relative importance. This is a

difficult matter, and Collingwood (1956, 329, 332) for one thought the problem is insoluble. Laudan (1997, 150; 1977, 31–40, 64–6) nonetheless thinks that we can weigh the importance and number of problems so as to measure whether gains in problem-solving outweigh losses.

The problem may not be impossible, and indeed even the epistemic, cumulative knowledge account of progress may invite supplementation with an account of which additions to knowledge are more significant and progressive than others. But for present purposes what deserves notice is the fact that the cumulative knowledge account does not *need* such supplementation in order to describe the Kuhn-loss transitions as progressive. For the problem 'solutions' that are lost are not knowledge. Since gravity is not explained by vortices, Descartes did not know what explains the co-planar orbits of the planets. He believed that the co-planar orbits are explained by vortices, but this belief is not knowledge since it is false. Since Kuhn-loss is not loss of knowledge it presents no difficulty, not even a prima facie one, for the cumulative knowledge account. The theoretical changes that involved Kuhn-loss brought with them gains in knowledge but no losses of knowledge (only losses of false belief). So the simple cumulative knowledge account can deliver the verdict that these changes are progressive without any need to weigh gains against losses.[4]

Although Kuhn and Laudan avoid scepticism about progress, their ability to talk with the vulgar is very far from perfect, since it is part of the vulgar concept that there is a gap between problem-solving (in their sense) and progress, the gap between evidence and what it is evidence for. Either their accounts of progress are attempts to capture the everyday concept of scientific progress. In which case their attempts are unsuccessful. Or they are seeking to reform the concept (the passage quoted from Laudan suggests that he might take such a view). In which case that reformation needs strong motivation. Later I shall review (and reject) the motivations provided by Laudan and Kuhn.

2.3 Conclusion

In sections 2.1 and 2.2 I have compared the epistemic conception with the semantic and functional-internalist conceptions respectively. By looking at cases where there is an increase in truth (but without the justification provided by a good method) and where there is an increase in problem-solving power (but without an increase in truth), we saw that neither an increase in truth nor an increase in problem-solving power are sufficient for scientific progress. In the first case the diagnosis is that an appropriate grounding in evidence or the use of a reliable method (i.e., justification of some sort) is a necessary condition of progress. In the second case, truth is a necessary condition, even if it is not a sufficient condition. The fact that truth and justification (where the latter might be provided by problem-solving power) are both necessary conditions tells us that we are in epistemic territory.

Matters would be simplest if we could conclude that truth and justification are individually necessary and jointly sufficient for progress. And if knowledge were justified true belief, my equation of progress with the accumulation of knowledge would fall out as a consequence. But we know that knowledge is not justified true belief, thanks to Gettier's counter-examples. Are then truth and justification jointly sufficient for a new scientific belief adding to progress? No, for precisely the same reasons that they do not add to knowledge. We may construct a Gettier style case of a scientific belief that is accidentally true and also justified (for reasons quite unconnected to the belief's truth). Such a case will not be a contribution to progress. As illustrated in figs. 2.1 and 2.2 when we draw a graph of scientific progress and a graph of change in knowledge, they coincide, whereas the graphs of progress and other conditions (such as truth or problem-solving) that fall short of knowledge do not coincide. I conclude that our ordinary conception of scientific progress is co-extensional with the concept of the growth of scientific knowledge.

In the next part and its successor I will argue that the disadvantages suffered by the semantic view and by the functional-internalist view in that they conflict with intuitions about progress, are not offset by any redeeming advantages. Clearly, the estimation of the overall costs and benefits of the semantic and functional-internalist approaches is a very large project and in consequence some parts of what follows are programmatic. Nonetheless, it is my intention that the detailed arguments presented as well as the programmatic elements should provide more than a prima facie case for the conclusion that the non-epistemic approaches have no compensating advantages when compared to the epistemic approach.

3.1 The Semantic View of Progress and the Concept of Verisimilitude

In this and the following sections I shall show that the semantic approach has no advantages that cannot also be exploited by the epistemic approach. There are two natural ways of cashing out the semantic approach. The first says that scientific progress is the accumulation of true scientific belief. The second says that scientific progress is the increasing nearness of theories to the truth.

It is perhaps surprising that the first variation has not, as far as I am aware, ever been supported by anyone. In addition to the strong scepticism of Laudan and Kuhn, which rejects knowledge and truth, there is also a weaker scepticism that concerns our ability to obtain precise truth. According to Niiniluoto, part of the popularity of the concept of verisimilitude is to be explained, by the fact that certain philosophers felt that while truth is itself unattainable, nearness to the truth is a useful and attainable alternative. He ascribes this motivation to Nicholas of Cusa and to Peirce

(Niiniluoto 1987). The point seems to be endorsed by Psillos (1999, 276): "In our interactions with the world, the exact truth cannot generally be had, especially concerning the unobservable, and spatio-temporally remote aspects of the world. A perfect match between theories and the world is impossible." This lends support to Laudan's claim (1981) that "realists . . . are (rightly) reluctant to believe that we can presume of any given scientific theory that it is true."

This milder, realist scepticism that although truth is unobtainable, truthlikeness is achievable, would, if correct rule out both the cumulative knowledge view of progress and the cumulative truth version of the semantic view. Clearly, if truth is unobtainable, then the accumulation of truth is unobtainable. And if the relevant propositions cannot be true, they cannot be known either, and so the accumulation of knowledge could not occur.

Why should we think that our theories are not strictly correct? The reason is that even many realists accept a weak form of the pessimistic induction, inferring from the premise that all past theories have been falsified to the conclusion that all current and future theories will be falsified also.[5] The inference is attractive to those who reflect on the fact that the great success of Newton's mechanics did not prevent its ultimate replacement by the theories of Einstein who himself predicted the eventual superseding of those theories also. This pessimism is of course consistent with supposing the later theories to be in an important sense better than the earlier ones, by being, for example, closer to the truth.

We shall be returning later to the pessimistic induction in a form that has the stronger conclusion that theories do not even get closer to the truth. For the time being it is worth challenging the premise. There are many venerable scientific propositions that have never been falsified and which we have no reason to suppose ever will be, and which do not state approximations. Here is a sample: blood circulates pumped by the heart, chemical substances are constituted by atoms, water is a compound of hydrogen and oxygen, light is electromagnetic radiation, electrons are negatively charged, the speed of light is constant for inertial observers, smoking causes cancer, the tides are caused by the gravitational influence of the moon, the continents have moved over time, mankind has evolved from ape-like ancestors, DNA has a double-helical structure. Philosophers of science, especially those who take the universal, precise, and quantitative theories of physics as their paradigm of scientific belief have been all too ready to accept the premises and indeed conclusion of the pessimistic induction. They have no reason to do so.

However, the supporter of verisimilitude might reply that even if we should not in general think our theories to be false, particular episodes such as the transitions from Galileo to Newton to Einstein or from Ptolemy to Copernicus to Kepler, will be transitions whose progressive nature is best accounted for in terms of verisimilitude since the relevant theories (with

the possible exception of Einstein's) are false. In section 3.3 I shall argue that insofar as verisimilitude can be used to describe such developments, the cumulative truth view and (with appropriate epistemic conditions being met) the cumulative knowledge view can also account for them.

3.2 Problems with Verisimilitude

In this section I shall argue that verisimilitude is not an appropriate concept for characterising scientific progress. I shall not be arguing that there is no coherent concept of verisimilitude or nearness to the truth. Rather I shall argue that there is no such concept that can be usefully applied to all kinds of proposition or theory or to groups of propositions or theories (such as, in the extreme, 'science' as a whole.).

I have already mentioned Popper and Niiniluoto as philosophers who have put forward precise accounts of verisimilitude. Oddie, Aronson, Harré, Way, and Giere are among the others who have attempted to give accounts of what verisimilitude or approximate truth are. Stathis Psillos (1999, 261–275) has argued convincingly that all these accounts fail. One reason why each fails is that none has sufficient generality. Each has a prima facie plausibility with respect to certain kinds of proposition or theory, but not others. Nonetheless Psillos holds that verisimilitude is the cornerstone of the realist's view of progress. According to Psillos we should make do with an informal concept of verisimilitude, the essence of which is the following: "A description *D is approximately true of S* if there is another state S^* such that S and S^* are linked by specific conditions of approximation and D is true of S^*" (1999, 277). So, according to Psillos, the gas law PV=RT is approximately true of real gases since it is true in a world of ideal gases and that world approximates to ours. Such an account depends upon its being the case that the discoveries of science all concern contingent propositions. But that is not true. Water is necessarily H_2O. So Dalton's hypothesis that water is H_2O is necessarily false and therefore describes no world that approximates to ours. Hence Dalton's hypothesis cannot be counted as approximately true of the actual world, on Psillos' account (and so no improvement on Aristotle, Priestley etc.).

No doubt Psillos' view may be re-cast in terms, for example, of models, where 'model' does not imply possibility. Even so, the sorry history of attempts to characterize approximate truth do show how difficult it is to give even an informal and general account of verisimilitude, let alone a formal and general account. It would be unwise to damn the verisimilitude view of progress merely on the ground that the concepts of verisimilitude or approximate truth lack formal or even informal definitions. The same charge may be levelled at the concept of knowledge and many other perfectly useful concepts besides. Nonetheless, the concept of knowledge does seem to be in a better shape to do the job of forming the basis of an account of scientific progress in

several respects. Even those who think that the concept of knowledge cannot be given an informative non-circular analytic definition tend to think that it may be usefully characterised and that it has an explanatory role. It is also a concept in ubiquitous use. The notion of verisimilitude lacks a worthwhile characterisation in place of a definition. It is in less general use than the concept of knowledge. It is not obviously explanatorily significant. And, most importantly, it is difficult to see how its use can be helpfully extended beyond the simple cases we do apply it to. An account of progress should in principle be applicable not simply to sequences of individual propositions, but also to sequences of theories, or the history of whole fields at once or indeed all of science considered as a single enterprise. It makes sense, for example, to say that quantum field theory has made progress whereas psychoanalysis has not. As I shall explain, it is highly doubtful that the notion of verisimilitude can be extended beyond simple cases, as it ought if it is to capture such uses of the concept of progress.

It must be accepted that we do often think of individual beliefs or claims as being close to the truth or far from it, and of one belief being closer to the truth than another. The problem is a matter of extending a notion whose clear application is limited to sets of individual propositions of the same logically simple (typically atomic) kind. Hence, if the time is 12.05, someone who says that the time is 12.00 is quite close to the truth but not as close as someone who says that the time is 12.04. But even a slight extension of this to compound propositions leads us into an area where judging relative nearness to the truth is impossible. Let it be that the temperature is 17°C. Which then of these is closer to the truth: 'the time is 12.00 & the temperature is 18°C' and 'the time is 12.04 & the temperature is 20°C'? If we are to understand progress in terms of verisimilitude, there need to be answers to questions of this kind. For we want to apply the notion of progress not simply to sequences of atomic propositions but also to sequences of complex propositions, hypotheses, and theories, to successive phases in the development of a scientific field, and even to all of science.

Here is a more significant problem. An undoubted fact is that the content of scientific belief grows—we now have beliefs about subjects matters that our predecessors never conceived of. Let it be that a science adds to the set of its generally accepted beliefs just one new belief that is, by intuitive standards, close to the truth. There is not even an intuitive sense in which the science as a whole is now closer to the truth than it was—unless that sense is identical to the thought that this science includes *more* (approximate) truth. There is clearly something better about believing 'the Earth's orbit is elliptical and Earth's orbit sweeps out equal areas in equal times' than just believing 'the Earth's orbit is elliptical'; but that improvement is not that the conjunction is overall closer to the truth than the single disjunct. It is that the conjunction has more worthwhile content than the single conjunct.

3.3 Progress with Approximate Truth

I have argued that the concept of verisimilitude suffers from various defects that make it a poor candidate for an understanding of scientific progress. But that criticism is consistent with Psillos' thought that there can be a useful informal, intuitive idea of nearness to the truth, one which might be given a more formal treatment in specific kinds of cases. In this section I shall grant—indeed require—that a general, informal notion of nearness to the truth has a use, and that there are more specific and formal ways of characterising nearness to the truth in particular fields. The purpose of this section is to defend the cumulative knowledge account of progress against the following objection:

> (O) Theories are very often at best only approximately true; they rarely attain full truth. Since knowledge entails full truth, theories cannot be the objects of knowledge. Consequently we cannot be accumulating knowledge in such cases. Hence the cumulative knowledge account cannot explain the sense in which out theories are getting better. The verisimilitude account can explain this and so is a better account.

The strategy is to argue that once we have granted a notion of approximate truth, then certain relevant propositions will have full truth (e.g. those of the form 'approximately p'). Since such propositions are fully true, they are potentially knowable should the right epistemic conditions be met, and the objection in (O) does not apply.

We can accept with Psillos and Niiniluoto that in many cases we need a notion of approximate truth without agreeing that there are no relevant propositions that can be characterized as fully true, as (O) implies. If p is approximately true, then the proposition q, that p is approximately true, is itself true, not merely close to the truth. This is legitimate, since if 'planets travel in ellipses' is a scientific proposition, then so is 'approximately, planets travel in ellipses'. Even if p is not true and so not knowable, q (q = approximately p) might well be knowable.[6]

One might have a conception of scientific *theories* whereby 'approximately p' is not a theory even if 'p' is. After all it is 'p' that will be used in explaining and predicting, not 'approximately p'. The first point in response is that this is irrelevant to the defence of the cumulative knowledge account. The latter says that scientific progress is the growth of scientific knowledge. Scientific knowledge will (locally) grow when any scientific proposition becomes known, even if that proposition is strictly speaking not a theory. Since not all scientific propositions are theories, our conception of theories is irrelevant. Furthermore, it is not clear that 'approximately p' is not a theory and cannot be used in explanation and prediction. I have taken 'approximately p' as equivalent to 'p is approximately true'. An objector may suggest that

whereas 'p' is about the world, 'p is approximately true' is a higher-order proposition about p, and for this reason is not a theory. Space does not permit a full discussion of this point which will draw upon issues in the theory of truth. Suffice to say that minimalists about truth will happily regard 'p is approximately true' as a proposition about the world. Such propositions can be used in explanations and predictions. For example, the fact that it is approximately true that the only force acting on the Earth is a central force directed towards the Sun explains why the Earth's orbit is approximately an ellipse. I think this is a better explanation than one involving the 'non-fact' that (precisely) the only force acting on the Earth is a central force... (Of course, explanations involving 'approximately p' are often parasitic on what p would explain if p were true. But perhaps not always. That the motion of a pendulum is approximately simple harmonic is explained by the fact that for small θ, $\sin\theta$ is approximately equal to θ. This is not best understood in terms of what would be explained if $\sin\theta$ were exactly equal to θ. For if $\sin\theta = \theta$ (exactly) then $\theta = 0$, and we do not have any pendulum motion at all.)

Having granted that the concept of approximate truth has a use, we can see that cases that show increasing nearness to the truth will also show the accumulation of (full) truth. And so those cases can in principle show the accumulation of knowledge also, should the required epistemic conditions be met. Such cases therefore present no special reason for preferring an account of progress in terms of verisimilitude. Let $<p_1,\ldots,p_k>$ be a series of hypotheses, accepted in that order over time, that monotonically get closer to the truth. This sequence shows progress according to the verisimilitude account. We can see that there is also a distinct but related sequence of propositions, entailed by the first, that exhibits the accumulation of truth and (potentially) the accumulation of knowledge. Let A(. . .) be a propositional operator whose meaning is given thus: A(p) iff approximately p.[7] Assuming first, for simplicity, that all the p_i are approximately true, the sequence of propositions $<A(p_1), \ldots, A(p_k)>$ will be a sequence of propositions each of which is fully true and adds to the truth provided by its predecessors. If these propositions are believed, then we have an accumulation of fully true propositions; and if they are sufficiently well supported by the evidence, the propositions will be known and we will have progress according the cumulative knowledge account. The improving precision of our approximations can be an object of knowledge.[8]

The latter is one specific kind of case. Sometimes the approach to the truth may not be monotonic. Let c represent the velocity of light, and Val(x) represent (*de re*) the numerical value of x (with appropriate units).[9] Researchers might over time attribute best values to c, numerically equivalent to $<\text{Val}(c+\varepsilon_0), \text{Val}(c-\varepsilon_1), \text{Val}(c+\varepsilon_2), \text{Val}(c-\varepsilon_3), \ldots>$, where all the $\varepsilon_i > 0$. The improving approximations fall on either side of c, approaching from both

sides. If both the first and second approximations are both approximately right, then so are all the others in the sequence. This means that (*de re*) beliefs whose contents are the members of the sequence <A(c=Val($c+\varepsilon_0$)), A(c=Val($c-\varepsilon_1$)), A(c=Val($c+\varepsilon_2$)), A(c=Val($c-\varepsilon_3$)), ...>, other than the first two, will not add to progress on the cumulative approaches. So the argument employed above that where there is verisimilitude there will be accumulation of truth (and so potentially knowledge) will not apply here.

Nonetheless we can see that there typically will be an accumulation of truth and knowledge available. Since the later approximations are within the narrowest bounds created by the earlier approximations, the approximations are getting better. I.e. if research has produced the values <... Val($c+\varepsilon_i$), Val($c-\varepsilon_{i+1}$), Val($c+\varepsilon_{i+2}$), Val($c-\varepsilon_{i+3}$),...> where Val($c+\varepsilon_i$) > Val($c+\varepsilon_{i+2}$) and Val($c-\varepsilon_{i+1}$) < Val($c-\varepsilon_{i+3}$) for all i, then Val($c+\varepsilon_{i+2}$) is closer to the truth than Val($c+\varepsilon_i$) and that Val($c-\varepsilon_{i+3}$) is closer than Val($c-\varepsilon_{i+1}$). We now consider a series of versions or precisifications of the approximation operator A: A_0, A_1, A_2, ..., where each later approximation in the series is more precise than its predecessors. For example, A_0 = very roughly, A_1 = roughly, A_2 = to a fair approximation, A_3= very nearly. Or the A_i could be more formal and precise approximation operators suitable for the appropriate field (e.g., margins for error: ±15%, ±10%, ±5%, ±2% etc.). For some such sequence of approximation operators, <A_0(c=Val($c+\varepsilon_0$)), A_1(c=Val($c-\varepsilon_1$)), A_2(c=Val($c+\varepsilon_2$)), A_3(c=Val($c-\varepsilon_3$)), ...> will be a sequence of true propositions whose later members do add to the truths entailed by the earlier members. Since researchers can see that the later values are within the bounds of earlier ones, they can have reason to believe that the later approximations are better than the earlier ones (in addition to whatever reason they may have for thinking that their techniques are more accurate). Hence they are likely to have beliefs where the later ones involve attributions of better approximation (even if the belief is merely of the form '(c=Val($c+\varepsilon_2$)) is a better approximation than (c=Val($c+\varepsilon_0$))'). Given the assumption of appropriate confirmation, the relevant sequence of beliefs will also represent the accumulation of knowledge. Although though there is still a margin for error, we *know* the value of the velocity of light with greater accuracy than previously.

To be clear, I have *not* argued that where there is increasing verisimilitude there is also the accumulation of knowledge. Obviously there might not be, as would be the case if the increasing verisimilitude were accidental. What I have argued in this section is defensive—it argued against the claim in (O) that we need the verisimilitude account *rather than* the cumulative truth or cumulative knowledge accounts, *because only the verisimilitude account can cope with progress in a sequence of theories none of which is fully true* (such as Ptolemy-Copernicus-Kepler). That claim is mistaken; we saw that where there is increasing verisimilitude there is also the accumulation of truth and so at least the *possibility* of the accumulation of knowledge.

3.4 Realism and Progress

Psillos (1999, 261) wants to salvage the notion of verisimilitude since he regards it as being a key implement in the realist's toolbox. There have been interminable debates surrounding the theses that realists ought to subscribe to. One problem is that it is too easy for anti-realists to find counterexamples to the positive claims proposed, which suggests that a realist ought to limit herself to the negative aim of defeating the sceptical proposals of the anti-realist.

However, if there is an appropriate positive realist claim, I propose that it is the thesis that science has always progressed: the history of science is marked by the accumulation of knowledge. Such a claim seems less vulnerable to the challenge of anti-realist counterexamples, since such a counterexample would have to be an episode of scientific change that reduced scientific knowledge or an extended period without any addition to scientific knowledge. Scientific regress is not impossible—reductions in knowledge can happen. Knowledge can be forgotten and it can be undermined by unlucky misleading counter-evidence (but much less easily than true belief). But such occurrences are rare for knowledge in general and even rarer for scientific knowledge.[10] Stagnation might be thought to be a greater threat to the claim that science has always progressed. But even then it is implausible that in modern times there was a period in which no science was contributing to the growth of knowledge. Science as a whole has progressed so long as some field somewhere has contributed to knowledge.

The claim that there has always been scientific progress is a minimal realist claim that may be added to in various ways. For example, one could make it apply to specific fields. As it stands the claim is even consistent with some forms of anti-realism, such as van Fraassen's constructive empiricism. Van Fraassen can agree that there has been growth in the knowledge of the empirical adequacy of theories, or growth in the knowledge of purely observational propositions. And so another way of making a stronger realist claim of this kind would be to claim that there has been progress in regard of our knowledge of theoretical propositions concerning the unobservable.[11]

4.1 The Functional-Internalist Conception of Progress and the Pessimistic Induction

Functional-internalists such as Laudan and Kuhn regard themselves as having grounds for rejecting not only the view that science shows accumulation of knowledge but also the view that science exhibits increasing verisimilitude. Both philosophers cite two grounds: (i) the pessimistic induction, and (ii) the transcendence of truth. In this section I shall consider the former and in the next I shall examine the latter.

In promoting the pessimistic induction Laudan is not merely rejecting an argument for realism but is also mounting what would be a strong argument

in favour of anti-realism were it acceptable (Laudan 1984). Yet there is no reason to think that it is. The argument is an inference from the alleged past failures of well-supported and favoured theories to the falsity of current and future theories. We have already encountered reasons for thinking that the premises of the argument are weak—a raft of important and long-standing discoveries that have never been falsified.

Furthermore, the argument suffers from the weakness of its induction. As we all know thanks to Goodman's new riddle and Hempel's paradox of confirmation, not all Fs that are G provide equal confirmation of the hypothesis that all Fs are Gs. Indeed, an F that although G is very close to not being G can in fact be *counter-evidence* to the hypothesis that all Fs are Gs (by suggesting that there is an as yet unobserved very similar F that is a marginal non-G). In the case of theory-development we have later Fs (theories) that are derived from earlier Fs, in particular in relation to the earlier ones being G (false). The falsity of earlier theories is the very reason for developing the new ones—with a view to avoiding that falsity.[12] It would be folly to argue that because no man has run 100 m in under 9.5 seconds no man ever will. On the contrary, improvements in times spur on other competitors, encourage improvements in training techniques and so forth, that make a sub 9.5 second 100 m quite a high probability in the near future.[13] The analogy is imperfect, but sufficiently close to cast doubt on Laudan's pessimistic inference. Later scientific theories are not invented independently of the successes and failures of their predecessors. New theories avoid the pitfalls of their falsified predecessors and seek to incorporate their successes. Even if the successor theory is false also, we cannot apply a simple enumerative induction. It might be at least as rational to think that the successive improvements suggest that the true theory is not far off. Of course, successive improvements would be consistent with an asymptotic approach to the truth. The point of this part of the argument is that we cannot make any good inference from the premise 'the succession of theories $T_1, \ldots T_n$ are all false' to the conclusion 'later theories in this sequence will also be false' without additional information. To make an inference of this kind requires knowledge also of the content of those theories and of the details of their relationship to the evidence. Certainly there is no *global* pessimistic induction.

The realist of my stripe can happily accept *local* instances of the pessimistic induction for two reasons. The first reason is that growth in knowledge elsewhere will permit a general thesis that science shows progress. The second reason is that even a succession of false theories can permit some knowledge. For a start there is the negative knowledge that some earlier theory is false. The knowledge may be less dramatic than knowledge that some theory is true, but it is scientific knowledge nonetheless. Popper was right to think that falsification even on its own can contribute to scientific progress. Furthermore, some suitable approximate and restricted version of a false theory might well be true and knowable. We might be impressed by the falsification

of Newton's theories concerning gravity by the precession of the perihelion of Mercury. We might pessimistically fear a similar fate for their Einsteinian successors. Yet that is compatible with knowledge that Newton's laws provide a good approximation for middle-sized dry goods travelling at moderate speeds within the Solar System and not too near the Sun, and also compatible with knowledge that general relativity is an even better approximation for these things (and a good approximation for some other things and circumstances besides). When faced with a successful but false theory an obvious research project is to find an improved theory. Another reasonable research project is to find the limits and margins for error within which the old theory holds true.

In conclusion, the pessimistic induction seems a poor inference; there is no reason to suppose that it rules out the accumulation of scientific knowledge.

4.2 The Functional-Internalist Conception of Progress and Transcendent Truth

Kuhn also accepts the pessimistic induction, but makes more of the alleged transcendence of truth.[14] In the first edition of *The Structure of Scientific Revolutions* Kuhn's stance is a neutralist one concerning the truth of theories and our knowledge of them. One of his targets in that book is a Whiggish approach to the history of science, whose explanation of the success of past theories is coloured by our current belief in the truth of those theories. Kuhn's aim is to build a theoretical framework that will permit the explanation of developments in scientific beliefs solely in terms of information available to the agents concerned (primarily information concerning the success of theories in providing puzzle-solutions). In so doing Kuhn seeks to avoid Whiggery by eschewing use of the notion of truth altogether.

This may be a methodologically wise move as regards the history of science. Kuhn does avoid the charge that this makes science an intellectual game whose relationship to reality is tenuous. His account of progress is central to an intended minimal concession to realism. Kuhn asserts that science progresses in a non-trivial way, even through revolutions. The key factor in choosing a new paradigm will be its ability to preserve as far as possible the puzzle-solving power of its predecessor while permitting the solution or dissolution of as many outstanding anomalies as possible (Kuhn 1970, 169).

We have already seen that such a conception clashes with the intuitive notion of progress, since a badly false theory might permit the apparent solution of merely apparent puzzles, a development that would be progressive in Kuhn's sense but not in the intuitive sense. That just serves to remind us that the intuitive sense does appeal at least to the notion of truth (and I think to knowledge also). Non-Whiggish methodology might reasonably eschew the use of the intuitive notion of progress. But that is no reason to substitute an alternative. What Kuhn should have said is the following: historians of science

should do without the notion of truth (as applied to theories, hypotheses, etc.) in explaining how science develops; historians should also therefore avoid the notion of progress; theirs is the history of the development of science, not of the progress of science. That *methodological* advice to historians does not prevent our being able so say truly (in some other capacity) "Priestley was wrong to think that combustion is to be explained by the release of phlogiston and Lavoisier was correct in believing oxygen to be the component of air responsible for burning." Such statements made with the benefit of hindsight may be of no use to the methodologically careful historian but they are true nonetheless. If the historian's explanations are to be neutral between realism and anti-realism, they should be neutral about progress as well as truth.

By invoking a notion of progress, Kuhn breaks this strict neutralism. In the Postscript to the second edition of *The Structure of Scientific Revolutions* he goes further by attacking the concept of truth in a version of an argument familiar from Kant and others. The argument is that a 'correspondence' conception of truth entails scepticism (Kuhn 1970, 206).[15] If the truth of statement S is a matter of its matching the fact F, then knowing that S is true is a matter of knowing that there is this match. That in turn requires access to the fact F independently of the statement S—knowledge requires considering S and F separately and seeing that they match. But where S is a scientific theory any access we could have to the facts such as F is only via theories such as S. So knowledge is not possible. (Kuhn takes this to show that the correspondence theory is incoherent. But that only follows if scepticism is incoherent.)

This argument is confused in a number of ways. For the present the following should be sufficient commentary.[16] Kuhn does not explain, nor does anyone else who employs the argument, why access to F needs to be independent of S, let alone prior to knowledge that S is true. The truth of S may consist in its matching F. But it does not follow from this, that knowledge of the truth of S requires *unmediated* knowledge of that matching. Why cannot knowledge of the matching be mediated (for example, by inference)? We might think that the matching between S and F is the best explanation of the truth of propositions deduced from S. Or we might think that the fact that F is the best explanation of certain other facts. From which we infer that F exists, from which we in turn infer the truth of S.

Note that Kuhn's argument, were it sound, would be an argument not solely against the correspondence theory of truth, in the sense of 'correspondence theory' that we associate with Russell, Wittgenstein, and Austin. It would undermine other views of truth that take truth to depend on the way the world is. What it assumes about truth is precisely this—knowledge of the truth of a theory requires independent access to the theory and to that part of the world it describes. But nothing in realism or in any conception of truth requires this. More generally there is nothing in Kuhn to persuade us that

a realist conception of progress is illegitimate or needs to be replaced by an anti-realist conception.

The weakness of Kuhn's argument notwithstanding, Kuhn's work left philosophy of science with a general sense that the cumulative conception of progress is somehow naïve. It may have been thought that arguments concerning the theory-ladenness of observation had enabled realist ontology and semantics to be resuscitated yet left realist epistemology in a precarious position. If so, a semantic account of progress might seem more attractive than an epistemic one. That way one can be a realist without having to address issues of scepticism. Indeed, the very conception of progress as increasing *nearness* to the truth seems to implicitly acknowledge the sceptical view that we have not yet actually got any truth. In any case, if knowledge entails truth, then true belief is easier to achieve than knowledge, and a fortiori *almost*-true belief is easier to achieve than knowledge. From which it seems to follow that progress-as-increasing-verisimilitude is easier to achieve than progress-as-the accumulation-of-knowledge. As we have seen, this is a misleading way of looking at things. Precisely because knowledge is harder to achieve, it is more stable than true belief. On the semantic approach progress is *too* easy—it can be accidental, in which case regress can occur just as easily. Hence it is easier to show that there is continuous progress without regress when those changes are understood in terms of gain or loss of knowledge.

I have argued that the epistemic approach to progress better meets our intuitions than either the semantic conception or the internalist conception. Furthermore, the epistemic approach allows us to be better realists than the semantic conception, while the arguments that motivate the internalist against the epistemic conception (and the semantic conception) are weak.

5 Progress and the Aim of Science

Our conception of scientific progress is linked to what we take the aim of science to be. In general, something like the following principle holds:

> (A) if the aim of X is Y, then X makes progress when X achieves Y or promotes the achievement of Y.

That scientific progress is the accumulation of knowledge is what one would expect if one takes the aim of science to be the production of knowledge.[17]

(A) includes a clause that says that progress is made when one promotes the achievement of one's goal. While I do not think that just *anything* that makes success more likely is progress, it is nonetheless true that we think that progress is made when certain means to an end are achieved: making necessary preparations, clearing obstacles, getting half-way there. If so, one should regard science as progressing when a development promotes the

growth of knowledge. I won't pursue this aspect of progress in great detail, since I think the relevant developments that promote knowledge will themselves be knowledge. For example, one may progress towards knowledge of whether some theory is correct by accumulating relevant evidence. If one accepts Timothy Williamson's equation of evidence and knowledge, then that evidence-gathering process will itself be the accumulation of knowledge (Williamson 2000, 184–208). Perhaps progress is made when there is an advance in scientific method, which thus promotes the goal of attaining knowledge. According to a naturalistic, pluralistic view of scientific method, new methods and techniques are themselves the products of science, and so progress in scientific method will not typically be distinguishable from progress in scientific knowledge. Not just any knowledge promoting development will be a part of scientific progress. Social developments may promote scientific knowledge and may be a spur to progress but will not be themselves scientifically progressive.[18]

The view that science aims at knowledge is a natural one, but not one that is universally accepted. Some might argue that knowledge is not enough—science aims at *understanding*. For example, imagine a team of researchers engaged in the process of counting, measuring, and classifying geologically the billions of grains of sand on a beach between two points. Grant that this may add to scientific knowledge. But it does not add much to understanding. Correspondingly it adds little to scientific progress. It seems therefore that we should conceive of the aim of science in terms of understanding as well as knowledge—and our characterization of progress should reflect this also. However, while the importance of understanding is clear, that does not contrast with the aim of knowledge, because all (genuine as opposed to apparent) understanding is also knowledge. To *understand* why something occurred is to *know* what causes, processes, or laws brought it about. Nonetheless, the case of the pointless investigation of the grains of sand does invite a supplementation of my account. The case can be accommodated by holding that the increase in knowledge is a contribution to progress, but a very slight and insignificant contribution. In which case we will want to know which additions to knowledge are significant and which not. So far my account says nothing about the rate of progress. It is plausible to hold that those additions to knowledge that are also instances of understanding are, other things being equal, more significant than those that are not. I will however leave a detailed discussion of the important question of what contributions to knowledge contribute most to progress (and in particular the role of understanding) for another occasion—not least because it is a much more difficult question.

In another direction, it may be claimed that science aims merely at true theories rather than at knowledge of the truth of theories. This is because it is widely assumed that truth is the aim of belief in general, with scientific belief being just one kind of belief. This general presumption about the aim

of belief would explain why realists have looked to an account of progress in terms of truth or verisimilitude. In the light of (A), the semantic approach to progress lines up with the view that belief aims at truth, while the epistemic approach lines up with the view that the aim of belief is knowledge rather than truth. Thus if the arguments presented for the epistemic view are persuasive then they also lend support to the view that belief aims at knowledge, since given (A) that view of the aim of belief best explains why we think that progress consists in the accumulation of knowledge. Conversely, independent argument for the knowledge view of the aim belief will support the central thesis of this paper.

This is not the place to pursue a general epistemic enquiry concerning the aim of belief. The purpose of this section has been to point to the link between that project and the debate over progress. However, given the presumption in favour of the view that belief aims at truth, I shall conclude with a few remarks intended to point the reader in the other direction. Timothy Williamson's recent attempt to redraw the geography of epistemology places knowledge at the centre of the map (Williamson 2000). A major feature of this conception is the view that belief aims at knowledge. It is as if we only have the concept of belief in order to describe the mental state we are in when we have attempted to know but have failed. While Williamson does not argue for this claim directly, the motivating role it plays in his overall conception means that the success of his arguments would strongly support that view. From this perspective, the arguments for the knowledge view of scientific progress can be seen as a contribution to the same project. It might seem that it is difficult to argue directly for the knowledge view of the aim of belief over the truth view by considering the actual processes of belief formation. On the one hand knowledge entails truth, so aiming at knowledge will ipso facto require aiming at truth. On the other hand, someone who aims at truth will seek to form her beliefs in ways that yield truths reliably. If she is successful in choosing the appropriate ways of forming a belief then the outcome will be knowledge. Even if not, her behaviour will resemble someone who seeks knowledge. So a truth-seeker may not be readily distinguishable from a knowledge-seeker.

Even so, I suspect that on closer inspection we can see that there are indeed differences between truth-seekers and knowledge-seekers. If true beliefs are desired, then that desire will be satisfied by a lucky guess. So a truth-seeker who has nothing else to go on ought to believe at random since that will maximise true belief. The important point here is that desiring truth does not entail that one desires to avoid falsehood. Hence, since believers do not and should not believe at random, the truth view of the aim of belief is typically modified, so that the aim of belief is characterised as the complex aim of achieving truth subject to the proviso that falsity is always avoided.[19] But, I suggest, our choice of belief forming behaviour is much better explained by regarding the aim of belief as the simple one of knowledge. As already remarked, since knowledge entails truth, the knowledge

seeker will ipso facto be seeking truth. The aversion to falsity—the requirement of reliability that truth-seeking cannot explain alone—is explained by knowledge-seeking precisely because a mechanism that might easily have given a false belief cannot generate knowledge even when it in fact generates true belief. Randomness in belief-formation is inconsistent with belief aiming at knowledge.[20] It is not appropriate to pursue this argument further here—I hope simply to have indicated the direction in which the argument should be taken.

This line of argument for the epistemic view of progress will depend on its being the case that the attitude that scientists hold towards the propositions of science is one of belief. The argument would be undermined if the attitude were something less than belief. Sometimes indeed scientists do not believe their favoured hypotheses, nor even approximations to them. That will be the case, for example, when they have insufficient evidence to rule out all the competing hypotheses. We might be told that such-and-such is the currently favoured model. In such a case reluctance to believe will be explained precisely because were the scientists to believe their belief would not amount to knowledge because of the lack of sufficient evidence. Such hypotheses will be those subject to current investigation and presumably the aim of such investigations is to produce further evidence that in due course may permit belief. Van Fraassen holds that science aims at acceptance of theories, not belief. But even acceptance is not independent of belief, since it involves belief that the theory in question is empirically adequate. Hence the current accounts of belief and progress would still be applicable to the belief component of acceptance. Even if the positivist conception of theories as inference-rules (and not as propositions at all) were true, the product of satisfactory research will still be a belief, a belief that one theory is a more reliable truth-preserving inference rule than another. So long as belief plays a central role in the description of the nature of science, there will be room for the epistemic account of progress.

6 Conclusion

Given that the aim of belief is knowledge, it follows that changes in belief are progressive when those changes increase or promote knowledge. In particular, there is progress in scientific belief when scientific knowledge increases. This epistemic approach has not always been clearly distinguished from the semantic counterpart, that progress is the augmentation of or improving approximation to true belief. Yet it is clear not only that these are importantly different accounts, but that they differ in their verdicts over actual and possible episodes. In a community which encourages belief on flimsy evidence, scientific beliefs will come and go. On the semantic view of progress an episode in which a truth is believed by accident and then abandoned will count as progress followed by regress, whereas on the epistemic view, there

will have been neither. This means that the view of science as having shown for the bulk of its history a continuous and monotonic progress is easier to maintain on the epistemic than on the semantic view. This supports the contention that the thesis that science progresses (as conceived on the epistemic view) is the appropriate slogan for scientific realists. Such an approach also avoids the problem of saying what exactly increasing verisimilitude amounts to for a large, diverse, and growing body of beliefs, taken all together.

If one is a sceptic, as Laudan is and Kuhn became, then the epistemic conception of progress will lead one to the conclusion that there has been no progress. Instead of accepting that conclusion Laudan and Kuhn developed internalist views of progress related to problem- or puzzle-solving. Apart from being motivated by poor sceptical arguments, the internalist conception of progress fails to match our ordinary concept of progress, for the latter clearly is concerned with external properties such as truth or knowledge. We do not regard a pseudo-problem resolved by further false beliefs as progress, however much it may appear to be progressive from the internal perspective.

Laudan thinks that it is an advantage of his account that progress can be assessed internally. But making progress that easy to achieve is to make progress not worth having. The semantic account makes progress more difficult and more worthwhile, by relating it to the clear benefits of truth. On that view, for us to know that progress was made when scientist S came to believe theory T will require us to know that T is true or that it is closer to the truth than its predecessors. S herself will know she has made progress if and only if she knows T to be true or approximately true. On the epistemic account, progress is even harder to achieve. For S to have made progress, it is not enough that T be true, S must know that T is true. Correspondingly, for S to know that she has made progress, S must know that she knows that T is true. S may well be in such a position, but since one does not necessarily know that one knows, it will also be possible to be in the position of having made progress but not knowing that one has done so. This is plausibly the case when T is as the cutting edge of a field and when new methods and techniques are used in confirming T. Far from being internally accessible, like many of the best things in life, the most exciting contributions to progress are often recognizable as such only with the benefit of hindsight.[21]

Notes

[1] C.f. the motto to Francis Bacon's *New Organon* "multi pertransibunt et augebitur scientia" ("many shall go about and knowledge shall increase") and Sir William Bragg: "If we give to the term Progress in Science the meaning which is most simple and direct, we shall suppose it to refer to the growth of our knowledge of the world in which we live." (Bragg 1936, 41)

[2] I do not wish to rule out the possibility that epistemic concepts might ultimately be defined in terms of semantic ones. Nonetheless, I intend my argument to support Williamson's view (Williamson 2000) that knowledge is the central epistemic concept and does not have an analysis.

[3] Niiniluoto briefly discusses the cumulative truth view (Niiniluoto 1984,76–7). He says the view is "nowadays widely regarded as giving a naïve and oversimplified picture of the development of science." He rejects the view because, among other reasons, it cannot, he says, accommodate strictly false theories that are nonetheless idealizations that are close to the truth. I reject this reason below.

[4] Laudan (1977, 147) notes that several philosophers have promoted cumulative accounts of progress, including Whewell, Peirce, Duhem, Collingwood, Popper, Reichenbach, Lakatos, and Stegmüller. That an account of progress is cumulative is consistent with its conforming to any of the epistemic, semantic, or functional-internal. conceptions. What will vary among the cumulative versions of such views will be what is accumulated (knowledge, truth, or problem-solutions). But since knowledge is harder to come by and more difficult to shift once acquired, the problem of a corresponding loss will be rarer for the epistemic conception than for the other conceptions.

[5] Others have chosen verisimilitude for different sceptical reasons. Popper of course did not accept the pessimistic induction. But his inductive scepticism and his conviction that theories have a zero probability of truth mean that he cannot think of progress either as accumulating knowledge or as accumulating truth.

[6] 'Approximately p' may be read as 'it is approximately true that p'.

[7] C.f. footnote 6 above.

[8] As Bragg (1936, 41) states "The first purpose of scientific enquiry is to add to the extent and *accuracy* of our knowledge." (My emphasis; c.f. footnote 1)

[9] So the proposition represented '$c = \mathrm{Val}(c + \delta)$' where $c = 2.99793\text{x}10^8\mathrm{ms}^{-1}$ and $\delta = 0.00002\text{x}10^8\mathrm{ms}^{-1}$ is to be read as '$c = 2.99795\text{x}10^8\mathrm{ms}^{-1}$'.

[10] The realist might in any case be able to accommodate a small number of peripheral and unusual counter-examples. Realism can survive a weakening of the claim to the assertion that almost always science has progressed. The claim that science has always progressed might be restricted to a given period.

[11] The existence of scientific progress is consistent with the loss of non-scientific knowledge such as (valuable but unscientific) knowledge of herbal remedies.

[12] Peter Lipton makes the point that the 'Darwinian' evolution of theories rules out an application of enumerative induction on the falsity of past theories. See (Lipton 2000, 197ff).

[13] The current record is 9.77 seconds (Asafa Powell, 14 June 2005).

[14] On the pessimistic induction, Kuhn says, "All past beliefs about nature have sooner or later turned out to be false." (Kuhn 1992, 14)

[15] The argument is elaborated in (Hoyningen-Huene 1993, 263–4).

[16] For a more extended discussion, see (Bird 2000).

[17] C.f. footnote 10.

[18] In pursuing science one may have goals other than the gaining of knowledge. But the goal of knowledge is the central and constitutive goal, in that if in general X is pursued without aiming at knowledge then X is not science. For example, science and technology are today much intertwined, so a research project might have the technological aim of producing a more malleable kind of steel and the scientific one of knowing things about the malleability of different kinds of steel alloy. However, should a process be pursued without any view to generating knowledge, such as making unsystematic adjustments to an existing technique in order to make it work better, then that process would not be scientific, even though it might be technological.

[19] Something like this seems to be David Papineau's view: he thinks of knowledge as the 'state a concerned enquirer needs to get into as a means to achieving [the] desire [to avoid false beliefs].' (Papineau 1993, §5.5). Not believing at all is the best way of avoiding false belief, but presumably the concerned enquirer wants to have beliefs (indeed true ones). So Papineau's individual is someone who seeks (true) beliefs at the same time as aiming to avoid false ones. Knowledge, on his view, is what one ends up with when appropriately pursuing this complex aim.

[20] This is the principle of safety. (See Williamson 2000, c.7.) Safety is linked to the thought that to give us knowledge our belief-forming processes should be reliable.

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References

Bird, A. (2000) *Thomas Kuhn* Chesham: Acumen.

Bragg, W. (1936) "The Progress of Physical Science" in J. Jeans *et al.* 1936 *Scientific Progress* London: George Allen and Unwin.

Collingwood, R. (1956) *The Idea of History* New York: Oxford University Press.

Hoyningen-Huene, P. (1993) *Reconstructing Scientific Revolutions* Chicago: University of Chicago Press.

Kuhn, T. S. (1970) *The Structure of Scientific Revolutions* (2nd edition) Chicago: University of Chicago Press.

Kuhn, T. S. (1991) "The Road Since Structure" in Forbes, Fine, and Wessels (eds) 1991 *PSA 1990 Proceedings of the 1990 Biennial Meeting of the Philosophy of Science Association vol 2* East Lansing: Philosophy of Science Association, 3–13.

Kuhn, T.S. (1992) "The Trouble with the Historical Philosophy of Science" *Robert and Maurine Rothschild Distinguished Lecture 19 November 1991 An Occasional Publication of the Department of the History of Science* Cambridge, MA: Harvard University.

Laudan, L. (1977) *Progress and its Problems* London: Routledge and Kegan Paul.

Laudan, L. (1984) *Science and Values* Berkeley: University of California Press.

Laudan, L. (1981) "A Confutation of Convergent Realism" *Philosophy of Science* 48: 19–49.

Lipton, P. (2000) "Tracking Track Records" *Proceedings of the Aristotelian Society (Supplementary Volume)* 74: 179–205.

Niiniluoto, I. (1980) "Scientific Progress" *Synthese* 45: 427–462. (Reprinted as c.5 of Niiniluoto 1984.)

Niiniluoto, I. (1984) *Is Science Progressive?* Dordrecht: Reidel.

Niiniluoto, I. (1987) *Truthlikeness* Dordrecht: Reidel.

Niiniluoto, I. (1999) *Critical Scientific Realism* Oxford: Oxford University Press.

Oresme, N. (1968) *A Treatise on the Uniformity and Difformity of Intensities* (M. Claggett ed.) Madison, Wisconsin.

Papineau, D. (1993) *Philosophical Naturalism* Oxford: Blackwell.

Psillos, S. (1999) *Scientific Realism: How Science Tracks Truth* London: Routledge.

Williamson, T. (2000) *Knowledge and its Limits* Oxford: Oxford University Press.