SCIENTIFIC PROGRESS

Alexander Bird, University of Bristol

Abstract

What constitutes scientific progress? This article considers and evaluates three competing answers to this question. These seek to understand scientific progress in terms of problem-solving, of truthlikeness, and of knowledge respectively. How does each fare, taking into consideration the fact that the history of science involves disruptive change, not merely the addition of new beliefs to old beliefs, and the fact that sometimes the history of such changes involve a sequence of theories all of which are believed to be false, even by scientific realists? The three answers are also evaluated with regard to how they assess certain real and hypothetical scientific changes. Also considered are the three views of the goal of science implicit in the three answers. The view that the goal of science is knowledge and that progress is constituted by the accumulation of knowledge is argued to be preferable to its competitors.

Keywords scientific progress, problem-solving, puzzle-solving, truthlikeness, verisimilitude, knowledge, approximate truth, goal of science, aim of belief.

1 Introduction

“What Des-Cartes did was a good step. You have added much several ways, & especially in taking the colours of thin plates into philosophical consideration. If I have seen further it is by standing on the shoulders of Giants.” Newton’s famous lines come in a letter to Hooke concerning research into the nature of light, carried out by Newton, Hooke, and Descartes. Those words have come to represent the idea of scientific progress, the idea that science is a collective enterprise in which scientists add to the edifice upon which their colleagues and predecessors have been labouring. While the metaphor is an old one, it is only in the early modern period that thinkers came to view history and culture—science in particular—in terms of progress. Descartes (1637: 85) himself, in the *Discourse on Method*, having remarked on the state of knowledge in medicine—that almost nothing is known compared to what remains to be known—invites “men of good will and wisdom to try to go further by communicating to the public all they learn. Thus, with the last ones beginning where their predecessors had stopped, and a chain being formed of many individual lives and efforts, we should go forward all together much further than each one would be able to do by himself.” Descartes’s desire found its expression in the scientific societies of the time, such as the Académie des Sciences (1666) and the Royal Society (1660), and in learned journals, such as the *Journal des Sçavans* and the *Philosophical Transactions of the Royal Society*. Zilsel (1945) finds late renaissance artisans publishing knowledge of their crafts in the spirit of contributing incrementally to the public good and understanding. But it is in the work of Francis Bacon that progress as an ideal for science is first promoted as such. In *The Advancement of Learning* and *The New Organon*, Bacon lays down the growth of knowledge as a collective goal for scientists, knowledge which would lead to social improvement also. And in his *New Atlantis* Bacon articulates a vision of a society centred upon Salomon's House, a state-supported college of scientists working on cooperative projects and the model for the learned academies founded in the seventeenth century.

With the notion of scientific progress in hand, several questions naturally arise: What *is* scientific progress? How does one promote scientific progress? How can we detect or measure scientific progress? Is there in fact progress in science?[1] This essay concentrates principally on the first question, but the various questions are related. For example, Descartes and Bacon accepted that science is the sort of activity where progress can be made by collective activity and in particular by adding to the achievements of others. This contrasts with the idea of progress in the arts. Although controversial, a case can be made that progress in the arts is made when the horizons of an art form are expanded through the exploration of new expressive possibilities. If so, that progress is typically individual and not straightforwardly cumulative—new artworks do not pick up where their predecessors left off. Furthermore, it seems plausible that the very point of scientific activity is to make progress whereas it is rather less plausible that the raison d'etre of the arts is related to progress of any kind. If this is right, the scientific progress is the sort of thing that is best promoted collectively, can be added to incrementally, and which is linked intimately to the aim of science.

2 Progress and the aim of science

How does a conception of progress relate to the goals of an activity? The simple view of progress here is that if an activity A aims at goal X, then A make progress insofar as it gets closer to achieving X or does more of X or does X better (depending on what X is and how it is specified). However, it is worth noting that an activity can be said to show progress along dimension D, even if that activity does not have any goal, let alone a consciously entertained or publicly agreed goal, to which D is related. Progress in the arts, as mentioned, is like this. We might judge that political institutions have made progress if they are more responsive to the desires and needs of citizens even if we don't suppose that this is the aim of those institutions. In such cases we judge progress by some appropriate standard, typically concerning a good that relates to the nature of the activity in question. So an artist might aim at producing a certain kind of aesthetic experience in an audience. An appropriate standard for judging progress in the arts is therefore the expansion of the possibilities for producing aesthetic experiences. Note also that a collective enterprise may make progress and may have a goal even is it is not the intention of any individual to contribute to that progress or to promote that goal. The artist may be focused solely on the experience of her audience and have no specific intention of widening the expressive possibilities of her art-form. Nonetheless, if her work in fact does broaden the range of expressive possibilities, then she may thereby be contributing to artistic progress. A business may have the aim of making a profit, though most of its employees (perhaps even all of them) do not have this as their individual goal.

So we can talk of progress in science even if we do not attribute an aim to science and do not attribute the same aim to scientists. That said, most commentators do

[1] Niiniluoto (1980: 428) raises three questions corresponding to first, third, and fourth of these

attribute a goal to science and tend to assume that scientists have that goal also. So, the three principal approaches to scientific progress relate to three views of the aim of science, in accordance with the simple view of progress, as follows:

(a) Science aims at solving scientific problems. Science makes progress when it solves such problems.

(b) Science aims at truth. Science makes progress when it gets closer to the truth.

(c) Science aims at knowledge. Science makes progress when it adds to the stock of knowledge.

Some well-known views of the aim of science might appear not to fit any of the above. Bas van Fraassen (1980: 12) holds empirical adequacy to be the aim of science. But this can be accommodated within (b) insofar as the aim of empirical adequacy is the aim of achieving a certain kind of truth, the truth of a theory's observational consequences.

All three approaches accept that the goals of science can be achieved collectively and in particular in an incremental and (normally) cumulative way, just as described by Descartes. Problem-solving takes place within a tradition; problems solved by a scientist will add to the problems solved by the tradition. A theory developed by one scientist may be improved by a successor bringing it closer to the truth. Scientists will add to the stock of knowledge generated by predecessors (including by improving their theories).

3 Progress as solving scientific problems

Scientists engage with scientific problems. Such problems arise from a tradition of solving problems. Given that we can account for the motions of the known planets within the framework of Newtonian mechanics and theory of gravitation, how should we account for the motion of the moon? This problem troubled astronomers and mathematicians for several decades in the eighteenth century. It spurred on research: Alexis Clairaut eventually showed that the difficulty in reconciling observations with Newtonian theory was due to the use of unsuitable approximations. In solving this problem, Clairaut thereby made progress in astronomy.

The principal proponents of the problem-solving approach to progress are Thomas Kuhn (1970) and Larry Laudan (1977). Kuhn uses the terminology of 'puzzle-solving' but this has the same intent as Laudan's 'problem-solving'. For both, scientific activity takes place within a tradition of research. Research traditions are characterized by shared commitments that form a background which gives significance to research problems and which guides the testing and evaluation of theories (i.e. proposed solutions to problems). For example, those in a common research tradition will share basic ontological commitments (beliefs about what sorts of entities there are—e.g. in medicine whether there are humours or germs), background theories (e.g. Newtonian mechanics), mathematical techniques (the calculus), and methods of theory assessment (e.g. significance testing in classical statistics). Kuhn used the terms 'paradigm' and 'disciplinary matrix' to refer to research traditions as just described.

Kuhn emphasized a particularly important component of a research tradition, shared exemplars of scientific problems and their solutions that act as a model for future problem-solving within the tradition. (Kuhn also used the term 'paradigm' to refer to such exemplars). These paradigms-as-exemplars are Kuhn's answer to the question about how science is able to make progress. For individuals they explain the cognitive psychology of problem-solving. Training with exemplars allows scientists to see new problems as similar to old ones and so as requiring a similar approach in solving them. For research communities, shared exemplars and disciplinary matrices permit the individuals and groups within the community to agree on fundamentals and so to agree on the problems needing to be solved, the means of solving them, and for the most part, whether a proposed solution is satisfactory. Research carried out in this way, governed by a shared disciplinary matrix, Kuhn calls 'normal science'. Kuhn contrasts normal science with 'pre-paradigm' (or 'pre-normal') science, a period of science characterised by a multiplicity of schools that differ over fundamentals. Pre-paradigm science fails to make progress because intellectual energy is put into arguing over those fundamental disagreements rather than into solving agreed puzzles according to agreed standards. For mature sciences, a more important contrast is 'extraordinary' (or 'revolutionary') science, which occurs when the research tradition finds itself unable to solve particularly significant problems. Under such circumstances a change of paradigm is needed. New kinds of problem-solution are required, ones that differ in significant ways from the exemplars that had previously dominated the field.

According to Laudan (1981: 145), '*science progresses just in case successive theories solve more problems than their predecessors.*' Kuhn and Laudan regard progress during normal science as unproblematic—during normal science solutions to problems are generally accepted and add to the sum of problems solved. Extraordinary science is less straightforward. That is because the rejection of a paradigm (i.e. a rupture within the research tradition) will often mean that some of the problems previously regarded as solved (by that paradigm, within that tradition) are no longer regarded as solved. For example, Descartes's vortex theory of planetary motion explained why the planets moved in the same plane and in the same sense (rotational direction). The successor theory, Newton's inverse square law of gravitational force, solved various problems (e.g. why the planets obeyed Kepler's laws), but lacked an explanation for the aforementioned problems to which Descartes's theory offered a solution. Hence the adoption of Newton's theory required relinquishing certain apparent problem-solutions. Such reductions in problem-solving ability are known as 'Kuhn-losses'. The existence of Kuhn-losses makes assessment of progress more complicated. For according to Laudan's account, the change in question will be progressive only if the Kuhn-losses are compensated for by a greater number of successful problem-solutions provided by the new paradigm. That requires being able to individuate and count problems. Furthermore, presumably some problems are more significant than others, and we would therefore want to weight their solutions accordingly.

Laudan (1981: 149) admits that it is not entirely clear how to carry out this individuation of problems but argues that all theories of progress will come up against that or an analogous difficulty—individuating confirming and disconfirming instances. We shall see that this is a rather less pressing matter for the other views of progress. Kuhn on the other hand accepts that there is no definitive way of making such assessments. The significance of a problem is determined by the tradition or paradigm which gives rise to it. There is no uncontentious way of making such assessments across paradigms. This is the problem of incommensurability. Thus there can be rational disagreement between adherents of two different paradigms. But that does not mean that Kuhn denies that there is progress through revolutions.

On the contrary, Kuhn (1970: 160–73) is clear that there is progress. Scientists operating within the new paradigm must be able to say, from their perspective at least, that the new paradigm retains much of the problem-solving power of its predecessor and that the reduction in the number of problems solved (the Kuhn-losses) are outweighed by the ability of the new paradigm to solve the most pressing anomalies of the predecessor while offering the promise of new problems and solutions to come. Incommensurability means that during a revolution this assessment—the assessment that moving to the new paradigm is progressive relative to retaining the old paradigm—is not rationally mandated and can be rationally disputed.

What counts a solving a problem? For that matter, what counts as a problem or puzzle in the first place? A crucial feature of the problem-solving approach is that it is the scientific tradition or paradigm that determines what a problem is and what counts as a solution. For Kuhn puzzles may be generated by a paradigm in a number of ways. For example, if a theory at the heart of a paradigm involves an equation with an unknown constant, then one puzzle will be to determine the value of that constant; subsequently, determining its value with greater precision or by a different means will also be puzzles. Most puzzles acquire their status by similarity to exemplar puzzles and solutions are accepted as such on the basis of similarity to exemplary solutions. For Laudan also, problems are determined by the tradition and its current leading theories—for example there exists a (conceptual) problem if the theory makes assumptions about the world that run counter to prevailing metaphysical assumptions. In the simplest cases, an empirical problem is solved by a theory if the theory entails (along with boundary conditions) a statement of the problem—there is no requirement that the theory should be true. I have called this feature of the problem-solving approach 'internalist' in the epistemological sense (Bird 2007: 69). Internalist epistemologies are ones that maintain that the epistemological status, e.g. as justified, of a belief should be accessible to the subject. The problem-solving approach is internalist in that it provides, as Laudan (1981: 145) says, an account of the goal of science (and so of progress) such that scientists can determine whether that goal is being achieved or approached—the community is always in a position to tell whether whether a scientific development has the status of being progressive.

A consequence of this way of thinking about problems and solutions is that entirely false theories may generate problems and solutions to those problems. It implies a field might be making progress (not merely appearing to make progress) by the accumulation of falsehoods, so long as those falsehoods are deducible from the principal theories in the field. Since, according to the dominant theory among alchemists, all matter is made up of some combination of earth, air, fire, and water, it should be possible to transmute one substance into another by appropriate changes to the proportions of these elements. Consequently, a leading problem for alchemists, was to discover a mechanism by which one metal could be transmuted into another; alchemists provided solutions in terms of the dominant theory, i.e. solutions referred to the four elements and the four basic qualities (moist, cold, dry, and warm). Neither the problem nor its solutions are genuine, but that does not count against there being progress in alchemy according to the problem-solving approach. Early writers in the Hippocratic tradition disagreed about which are the correct humours. One, the author of *On Diseases, IV*, proposes blood, phlegm, bile, and water. This sets up an asymmetrical relationship with the elements—the humours and elements overlap in one, water, but not the others. By replacing water by black bile, a symmetrical relationship between the humours, elements, and qualities could be restored. Furthermore, according to the author of *One the Nature of Man*, a relationship between the humours and the seasons could be established, explaining the prevalence of phlegm in winter for example (Nutton 1993: 285). The problem-solving approach takes such developments as progressive, despite their being devoid of any truth.

It is also an implication of the problem-solving approach that the rejection of any theory without a replacement counts as regressive, so long as the theory did raise and solve some problems. Laudan, Kuhn, and others point out that theories do not get rejected outright with nothing to replace them, in which case this scenario does not normally arise. However, even if rare, it does occur. In 1903 René Blondlot believed that he had discovered a new form of radiation, which he named 'N-rays' ('N' for Nancy, where he was born and worked). N-rays attracted much interest until the American physicist R. W. Wood visited Blondlot's laboratory and surreptitiously removed a crucial prism from Blondlot's experiment. Blondlot nonetheless continued to see the rays. After Wood's intervention it became accepted that N-rays simply did not exist; scientists gave up research in N-rays and the community repudiated its earlier 'discoveries'. During the intervening period, 1903–5, over three hundred articles on the topic were published (Lagemann 1977), discussing the nature of the rays and their properties (e.g which materials could generate or transmit the rays). N-ray research was carried out as normal science—research problems were posed and solved. So according to the problem-solving approach N-ray research was progressive until 1905. And because its repudiation after Wood involved a loss of problems and solutions without any compensating gain, that repudiation must count as regressive. That conclusion will strike many as just wrong—N-ray research did not add anything to progress, but its rejection did perhaps add something.

Laudan regards the disconnection between progress and truth as an advantage of his account. Truth, says Laudan (1981: 145), is transcendent and 'closed to epistemic access'. (Kuhn (1970: 206) also argues that truth is transcendent.) Note that to assert this is to assume anti-realism. Scientists gather evidence and provide arguments as to why that evidence gives us reason to believe, to some degree or other, the relevant hypotheses. Scientific realists claim that, in many mature sciences at least, this process does in fact lead to beliefs that are true or close to the truth. Anti-realists deny this. Laudan's claim that truth is transcendent, if correct, implies that the scientists are on a hiding to nothing. Their evidence and arguments cannot be leading us towards the truth, for they did, then truth would not be transcendent—it would be epistemically accessible through the process of scientific research.

So scientific realists have two reasons to reject the problem-solving approach. First, the motivation ('truth is transcendent') that Laudan gives for the internalism of that approach assumes anti-realism. Secondly, the approach gives perverse verdicts on particular scientific episodes: if scientists in the grip of a bad theory add further falsehoods (false problem-solutions) that counts as progressive; if on the other hand they realise their error and repudiate those falsehoods, that counts as regressive.

4 Progress as increasing nearness to the truth

A natural response to the realist's objections to the problem-solving approach is to seek an account of progress in terms of truth—what I have called the *semantic* approach to progress (Bird 2007: 72). If the problem-solving approach takes progress to consist in later theories solving more problems than their predecessors, then a truth-approach might take progress to consist in later theories containing more truth than their predecessors. This would occur, for example, when later theories add content to earlier theories and that new content is true.

However, the view of progress as accumulating true content has not found favour even among realists. The principal reason is the recognition that theories are often not in fact true. A good theory may in fact be only a good approximation to the truth, and a better successor theory may cover the same subject matter but just be a better approximation to the truth without in fact being strictly true itself. So realists have tended to favour accounts of progress in terms of increasing *truthlikeness* (nearness to the truth, verisimilitude), not (directly) in terms of truth itself. Indeed, what David Miller (1974: 166) regards as the 'problem of verisimilitude' is the problem of saying how it is that there is progress, for example in the sequence of theories of motion from Aristotle to Oresme to Galileo to Newton, even though all the theories are false. It would seem that we do have an intuitive grasp of a nearness to the truth relation. In the following cases, T_2 is intuitively nearer to the truth than T_1 and T_4 is nearer to the truth than T_3: Although it is not clear whether T_4 is nearer to the truth than

The value of Avogadro's constant, N_A

truth	$N_A = 6.022 \times 10^{23}$ particles per mole
$\mathbf{T_1}$ (Perrin 1908)[2]	$N_A = 6.7 \times 10^{23}$ particles per mole
$\mathbf{T_2}$ (Rutherford 1909)	$N_A = 6.16 \times 10^{23}$ particles per mole

Nature (e.g. chemical formula) of water

truth	water is H_2O
$\mathbf{T_3}$ (Aristotle)	water is an element
$\mathbf{T_4}$ (Dalton)	water is a compound with the formula HO

T_2, there is no reason why we should expect any verdict on such a question; the predicate '. . . nearer to the truth than . . . ' delivers only a partial order.

It would seem to be an obvious task for one who prefers the increasing nearness to the truth approach to provide an account of what nearness to the truth is and when one theory is nearer to the truth than another. However, it is unclear that this is an essential task. It might be thought that if none of the various available accounts of truthlikeness is satisfactory, then that severely undermines the truthlikeness account of progress. On the other hand, consider the simple accumulation of truth view with which I started this section. Would it be a fair criticism of that view that philosophers do not have an agreed, unproblematic account of what *truth* is? Is the latter a problem for scientific realism, insofar as the latter also appeals to a notion of truth? Presumably not—we can and do use the concept of truth unproblematically. So the issue may come down to this. Is the concept of truthlikeness a theoretical or technical one introduced for the philosophical purposes discussed above? Or is it a concept with a pre-philosophical use, one that has clear application in a sufficient range of important cases? If the former, then the lack of a clear philosophical account of truthlikeness does cast doubt on the coherence of the concept and its use to account for progress. If the latter, then (like the concepts of truth, cause, knowl-

[2] See Becker (2001) for details.

edge etc.) the truthlikeness concepts has a legitimate philosophical application even in the absence of a satisfactory analysis.[3]

Either way, it will be instructive to look at one of the key debates in the early development of theories of truthlikeness. The basic idea behind Popper's (1972: 52) account of verisimilitude is that theory A is closer to the truth than B when A gets everything right that B gets right and some other things right besides, without getting anything wrong that B doesn't also get wrong.[4] Now this turns out to fail even in standard cases where A is false but is clearly closer to the truth than B (e.g. provides a closer numerical approximation of some constant) (Tichý 1974). It can be shown that A will have some false consequences that are not consequences of B. So one might be tempted to drop the second condition: so long as the truths are accumulating, we can ignore the fact that there might be some accumulation of false consequences also. But a (*prima facie*) problem with ignoring the falsehoods is that a change to a theory might add to the true consequences in a small way but add to the false consequences in a major way. One might believe that such as change should be regarded as regressive. Focussing just on true consequences would also imply that a maximally progressive strategy would be to adopt an inconsistent theory, because it has *every* truth among its consequences.

A different approach initiated by Tichý and others (Hilpinen 1976; Niiniluoto 1977) considers how much the possible worlds described by theories are like the actual world. Tichý imagines a language with three atomic propositions to describe the weather: 'it is hot', 'it is rainy', and 'it is windy'. As it happens the weather is such that all three propositions are true. Smith believes: 'it is not hot and it is rainy and it is windy' whereas Jones believes 'it is not hot and it is not rainy and it is not windy'. We can say that Jones is further from the truth than Smith since there are three respects in which a world may be like or unlike the actual world and the possible world described by Jones's theory differs from the actual world in all three respects (so the distance from the actual world is 3) while the possible world described by Smith's theory differs from the actual world in one respect (so distance = 1). The world of Jones's theory is thus more distant from the actual world than the world of Smith's theory, and so the latter theory is close to the truth than the former. Miller (1974)

The weather according to Smith and Jones (original)

truth	hot	rainy	windy
Jones	not hot	not rainy	not windy
Smith	not hot	rainy	windy

The weather according to Smith and Jones (redescribed)

truth	hot	Minnesotan	Arizonan
Jones	not hot	Minnesotan	Arizonan
Smith	not hot	not Minnesotan	not Arizonan

[3] I am now inclined to think that our ability to give a clear and unambiguous judgment in the cases just mentioned is evidence in favour of the second view whereas I previously (Bird 2007) took the first.

[4] More formally: A is closer to the truth than B iff B's true consequences are a subset of the true consequences of A and A's false consequences are a subset of the false consequences of B and one or both of these two subset relations is a proper subset relation.

however considers an alternative language with the atomic propositions ‘it is hot’, ‘it is Minnesotan’, and ‘it is Arizonan’. ‘It is Minnesotan’ is true precisely when ‘it is hot ↔ it is rainy’ is true, and ‘it is Arizonan’ is true precisely when ‘it is hot ↔ it is windy’ is true. In this language the truth is expressed by ‘it is hot and it is Minnesotan and it is Arizonan’, while Smith’s beliefs are expressed by ‘it is not hot and it is not Minnesotan and it is not Arizonan’ and Jones’s beliefs are expressed by ‘it is not hot and it is Minnesotan and it is Arizonan’. Given this language for the description of possible worlds, the world described by Smith’s theory differs from the actual world in three respects (distance = 3) and the world of Jones’s theory differs in just one respect (distance =1). So Jones’s theory is closer to the truth than Smith’s. It would thus appear that this approach makes verisimilitude relative to a language. And that is a problematic conclusion for the scientific realist, who wants progress and so (on this approach) verisimilitude to be objective. There are various ways in which one might tackle this problem. For example, one might say that some languages are objectively better at capturing the natural structure of the world. (While I think that is correct, it seems insufficient to solve the problem entirely.) Or one might try quite different approaches to truthlikeness. (See Oddie (2014) for a clear and detailed survey of the options.) Eric Barnes (1991) proposes a more radical response that notes an epistemic asymmetry between the beliefs expressed in the two sets of vocabularies; this, I propose in the next section, is instructive for the purposes of understanding progress.

What is more obviously problematic for the truthlikeness approach are cases where our judgments of progress and of truthlikeness are clear but divergent. I have suggested that we can imagine an example of this by considering again the case of Blondlot and N-rays.[5] Observation of new effects, with new apparatus, particularly at the limits of perceptual capacities, is often difficult, so it is not entirely surprising that scientists could honestly think they were observing something when they were not. Almost all the work on the subject was carried out by French scientists—although some British scientists claimed to detect N-rays, most scientists outside France (and quite a few in France) could not detect them. It is plausible that the French N-ray scientists were motivated by national sentiment (Lagemann 1977), consciously or unconsciously proud that that there were now French rays to stand alongside the X-rays (German), cathode rays (British), and canal rays (German again) discovered and investigated in the preceding decades.[6] Blondlot had an excellent reputation—he was a member of the Académie des Sciences; others who worked on N-rays were well respected, including Augustin Charpentier and Jean Becquerel (son of Henri Becquerel) (Klotz 1980). No doubt these reputations also had a persuasive role—in that context the observation or otherwise of N-rays became a test of a scientist’s skills not of the theory. Whatever the explanation, it is clear that the scientific justification for belief in N-rays was at best limited. Now let

[5] Bird (2007, 2008). See Rowbottom (2008, 2010) for an opposing view and further discussion of this case.

[6] X-rays were first observed by Röntgen in 1895, while cathode rays were first observed by another German, Johann Hittorf, in 1869 and named as such by a third, Eugen Goldstein. Nonetheless, cathode rays were investigated primarily by British physicists—they were produced in the eponymous tube devised by William Crookes and their nature, streams of electrons, was identified by J. J. Thompson in 1897, confirming the British (Thompson and Schuster) hypothesis against the German (Goldstein, Hertz, and Wiedemann) one that they were a form of electromagnetic radiation. Canal rays, also known as anode rays, were also produced in a Crookes tube and were discovered by Goldstein in 1886 and investigated by Wien and Thompson. Until N-rays, the French has no horse in the physical ray race.

us imagine, counter-to-fact, that at some later date it is discovered that there really are rays answering to many of Blondlot's core beliefs about N-rays. Many of Blondlot's beliefs (and those of the scientific community in France) turn out to be correct or highly truthlike, although those scientists held those beliefs for entirely the wrong reasons (their techniques could not in fact detect the genuine rays). So according to the truthlikeness view of progress, Blondlot's work contributed to progress (by adding true and highly-truthlike propositions to the community's beliefs) whereas Wood's revelation was regressive (because it removed those beliefs). That seems entirely wrong—even if Blondlot got things right (by accident) the episode is clearly one of pathological science, not of progressive science—excepting Wood's intervention, which got things back on track.

The preceding example shows that the truthlikeness account of progress conflicts with our judgments concerning particular instances. The case is partly hypothetical—in science it is difficult to get things right (factually correct or highly truthlike) for the wrong reasons. There are nonetheless some real cases. For example, it was believed in the Renaissance period that infants and young children had more body water as a proportion of total body weight than older children and adults. The source of this belief was a physiological theory based on the doctrine of the humours. As mentioned above, the body was supposed to be governed by four humours (bile, black bile, blood, and phlegm); ill-health is a matter of an imbalance or corruption of the humours. The natural balance changes with time, and since the humours are characterised by their proportions of the four basic qualities (moist, dry, warm, and cold), the changing balance of the humours implies change in the natural proportion of these qualities with age. The prevailing view was that the predominant humour in childhood is blood, which is moist and warm (Newton 2012). Children are moist because they are formed from the mother's blood and the father's seed, both of which are moist and warm. Childhood is also influenced by the Moon, which was held to be warm and moist. In youth, however, the dominant humour is choler, which is warm and dry. And so as a child grows to maturity it loses moisture, and indeed this drying continues with age, explaining the wrinkled skin of the elderly in contrast to the soft skin of babies and infants. When humoural theory was dismissed in the late eighteenth and nineteenth centuries, these opinions fell into abeyance. It turns out that modern physiology has shown that body water is highest in neonates and then declines (Friis-Hansen et al. 1951; Schoeller 1989). So the Renaissance doctors, basing their opinion in the false humoural theory, fortuitously has correct opinions about the variation of moisture in the body with age. According to the increasing truthlikeness view, when true implications of humoural theory were drawn, that science thereby made progress, but regressed when these beliefs were dropped along with the humoural theory. On the contrary, even the fortuitously true consequences of humoural theory were no contribution to scientific progress insofar as they were based principally on that theory and not on appropriate evidence.

5 Progress as the accumulation of knowledge

The third approach to scientific progress is an *epistemic* one—scientific progress is the accumulation of scientific knowledge; as Sir William Bragg put it "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). As Mizrahi (2013) shows, Bragg was far from alone in his opinion—scientists in general take knowledge to be at the core of scientific progress. It is also my own preferred view.

Let us return to Miller's example of Smith and Jones who are making assertions about the weather. In Miller's version of the story, they inhabit a windowless, air-conditioned room. So they are making guesses, Jones guesses that it is not hot and it is not rainy and it is not windy while Smith guesses that it is not hot and it is rainy and it is windy. The truth is that it is hot and it is rainy and it is windy; so none of Jones's guesses is correct whereas two of Smith's are correct. If we further assume that Smith's guesses are made after Jones's, then that would look like progress according to the truthlikeness view. But as argued in the preceding section, lucky guesses are no contribution to progress. Now let us imagine, as does Barnes (1991: 315), a variation whereby Smith and Jones form their beliefs not by guessing but by investigating indicators of temperature, precipitation, and airspeed in their vicinity, using methods that are usually very reliable by normal scientific standards. However, things go awry in the case of all Jones's investigations, leading to his false beliefs. They go awry for Smith also in the case of temperature, but his methods for measuring precipitation and airspeed are reliable and working fine. Accordingly, Smith knows that it is rainy and that it is windy. Now consider the beliefs of Smith and Jones when expressed using the language of 'hot', 'Minnesotan', and 'Arizonan'. The oddity is that now Jones has two correct beliefs: that the weather is Minnesotan and that it is Arizonan. Let us have a closer look at how it is that Jones has these two true beliefs. The belief that is is Minnesotan is logically equivalent to the belief that it is hot $\leftrightarrow$ it is raining, which in turn is equivalent to the belief **DIS** that either it is both hot and rainy or it is neither hot nor rainy. Jones believes this proposition because he believes that it is not hot and not rainy. So he believes the second disjunct

Why Jones believes that the weather is Minnesotan

	Jones believes	
	is it Minnesotan	
	which is equivalent to	
	DIS:	
it is hot and rainy	OR	it is not hot and it is not rainy
true		*false*
not believed by Jones		*believed by Jones*

of **DIS**. Note that this disjunct of **DIS** is false. Since **DIS** is a simple consequence of that disjunct, Jones believes the whole of **DIS**. However, the whole of **DIS** is true, since the first disjunct is true—a disjunct that Jones believes to be false. So the position is that Jones believes a true proposition, **DIS**, because it is a disjunction and he believes one of the disjuncts—the one that happens to be false. As Barnes (1991: 317) points out, Jones's belief has the structure of a standard Gettier case: a case of a true and justified belief, but where the truth and justification come apart. The justification (in this case Jones's normally reliable investigation that has gone awry) is attached to the false disjunct of **DIS** whereas the truth comes from the other disjunct of **DIS** for which Jones has no justification (in fact he thinks it is false). As a standard Gettier case, we must deny that Jones has knowledge in this case, although he has fortuitously got a true belief.

The problem we faced was that it looked as if truthlikeness depends on choice of language. Combined with the progress-is-increasing-truthlikeness view, that implies that Smith makes progress relative to Jones when we express their beliefs using normal language and that Jones makes more progress when their beliefs are expressed using the terms 'Minnesotan' and 'Arizonan'. Barnes's important observation shows that the change of language makes no corresponding difference as regards *knowledge*: a shift of language reveals that Jones has some true beliefs, but it doesn't render those beliefs epistemically successful—they are not knowledge. So that in turn means that while the truthlikeness view faces problems, the epistemic view can simply bypass them.[7] Fortuitously true beliefs notwithstanding, Jones knows nothing substantive about the weather whereas Smith does, and therefore Smith contributes to progress and Jones does not.

A suitable response therefore proposes that progress should be seen in terms of increasing knowledge, not nearness to the truth. This, the epistemic view, gives the right results in the other cases considered also. If by fluke Blondlot had some true beliefs about N-rays, they would not have been knowledge and so would have made no contribution to knowledge. Renaissance doctors did not know that infants have high body water, although that is true, because they had no way of determining that—their beliefs were based on the radically false humoural theory. So there was no progress when they came to have those beliefs either. Likewise, normal science based on a radically false theory, such as the theory of four elements and four basic qualities, is not progressing, even if it appears, from the practitioners' point of view, to be providing successful solutions their problems. That is because in such cases there is no addition of knowledge.

The view that progress consists in the accumulation of knowledge faces an objection relating to that which stimulated the truthlikeness approach. A straightforward accumulation of truth view suffered from the fact that there could be progress in a sequence of theories all of which are false. The idea of truthlikeness, which could increase through that sequence, appeared to solve the problem. If we now switch to the epistemic approach we face a difficulty that doesn't appear to have such a straightforward solution. Knowledge entails truth, so the community cannot know any proposition in a series of false propositions: however close to the truth p may be, if p is strictly false, then it cannot be known that p. Nor, however, is there an obvious analogue to truthlikeness for knowledge: a state that is much like knowledge except that the content of that state is false. It might be that we could attempt a formal definition of such a state—call it approximate knowing. Approximate knowing would be a certain kind of belief. For example, it might be proposed that the state is a belief that p that is justified, and the justification is appropriately linked to the fact that p has high truthlikeness. Note that if an approach of this sort says anything detailed about what approximate knowing is (such as in this proposal) then it is likely to draw on a particular definition of (non-approximate) knowledge, substituting truthlikeness for truth. Yet accounts and definitions of knowledge are notoriously subject to counterexample. So it may be that the most we can confidently say about this state is that it is like knowing except for a false content. Those, like Williamson (1995), who take knowledge to be a factive mental state, different in kind from belief, will note that approximate knowing is not a state of the same kind as knowing.

[7] Barnes himself thinks that we can use the epistemic features of such cases to identify an appropriately privileged language—appropriate to that epistemic circumstance. The language issue strikes me as a red herring and that Barnes's contribution is more effective without the detour via language.

While I do not rule out the idea of approximate knowing, a better approach starts by noting that when a theory T is accepted, what scientists believe is true is not limited to a precise statement of T—indeed they might not believe that precise statement. Scientists will believe many other propositions that are related to T, most importantly its most obvious and salient logical consequences. For example, Dalton not only believed (a) that water is HO, he also believed (b) that water is a compound of hydrogen and oxygen and (c) that water is made up of molecules containing each containing a small number of atoms of hydrogen and oxygen, and so forth; Rutherford almost certainly did not believe (d) that Avogadro's constant has the precise value of 6.16 x 10^{-23}mol^{-1}, but he did likely believe (e) that the value of Avogadro's constant is approximately 6.16 x 10^{-23}mol^{-1}, or (f) that the value of Avogadro's constant lies between 6.0 x 10^{-23}mol^{-1} and 6.3 x 10^{-23}mol^{-1}. While (a) and (d) are false propositions, (b), (c), (e) and (f) are true propositions. Given the evidence and methods that Dalton and Rutherford employed, it is plausible that Dalton knew (b) and (c) and Rutherford knew (e) and (f). And insofar as their predecessors did not have this knowledge, then Dalton and Rutherford contributed to progress, according to the epistemic approach. The relevant consequences are logically weaker and so less informative that the propositions, (a) and (c), from which they are derived: in the case of (e) and (f), the consequences are inexact propositions while in the case of (b) and (c) the consequences are exact propositions but omit details contained in the original proposition (a).

This approach to defending the epistemic view of progress, in the case of false but progressive theories, requires that we are able to find (true) propositions that are consequences of such theories and which are now known by the community but which were not known previously. It is highly plausible that this condition can be met.[8] As the examples above suggest, if a proposition is strictly false but highly truthlike, then there will exist related non-trivial true propositions. At the very least 'T is highly truthlike' will be such a proposition (for false but highly truthlike theory T). But in many cases we will be able to find propositions more precise than this.[9]

The next question concerns belief—do the scientists in question believe the true proposition in question? In some cases this may be clear enough—Dalton clearly believed (a) and also (b) and (c). But in other cases it might be less clear, often because the scientist does not believe the principal truthlike but false proposition. The most difficult cases will be those where the theory in question is a model that scientists know from the outset to be strictly false. Nonetheless, in such a case, if the model does contribute to progress, then it will have some significant element of truth or truthlikeness (at least relative to its predecessors). And that will have been

---

[8]See Niiniluoto (2014) for criticisms of this claim—what follows, I hope, provides some answers to those criticisms.

[9]Is 'T is highly truthlike' a proposition of science? Your answer might depend on your view of the truth predicate and its relatives, such as 'truthlike'. For example, you might think that their use involves semantic ascent: 'T' is about the world whereas 'T is true' concerns words (and their relation to the world). On the other hand, if you are a redundancy theorist about truth, you will think that'T is true' says exactly what 'T' says and is equally about the world. Presumably analogous remarks could be make about 'truthlike'. In my view, even if the truth and truthlike predicates do involve semantic ascent, they can still be used in scientific propositions: they still make claims about the world, even if they say something else as well. For example, if an economist says 'the simple supply and demand model is a good approximation to the truth as regards many commodity markets, but it is not a good approximation for markets with asymmetric information, such as the markets for complex financial securities and healths insurance' then they are saying something *both* about the world and about a theory; the assertion does not seem any the less a scientific statement because of that.

achieved by suitable gathering of evidence and reasoning. If so, the scientists promoting the model ought to be able to say something about the respects in which the model matches reality, even if, at a minimum, that is just to say that the model delivers approximately accurate predictions within certain ranges of the relevant parameters. They will typically have some idea regarding some of the implications of theory, that these are supported by the evidence and reasoning whereas others are not. For example, the simple kinetic theory of gases is clearly false, since it assumes that gas molecules have no volume. So no-one ever believed the theory *in toto*. But scientists do believe non-trivial implications of the theory: that gases are constituted by particles; that the temperature of a gas is in large part a function of the kinetic energy of the particles; and that the ideal gas equation holds with a high degree of approximation for gases at moderate pressure and temperature.[10]

So false theories, even those known to be false, can contribute to progress on the epistemic view, because they will often have significant true content or implications that are believed by scientists on the basis of good evidence and reasoning, i.e. these implications are known propositions. Thus the epistemic approach looks to be able to accommodate progress made through false theories. At the same time, it delivers more accurate verdicts regarding cases of accidentally true unjustified (or partially) theories. Which gives it a distinct advantage, in my opinion. On the other hand it does need to confront the widely held view that truth is the aim of inquiry and belief, and that the justification element of knowledge is not constitutive of the aim of science (nor of scientific progress) but is rather merely instrumental in achieving that aim (Rowbottom 2010; Niiniluoto 2011).

6 Progress and the aim of science

As Niiniluoto (2011) puts it, 'Debates on the normative concept of progress are at the same time concerned with axiological questions about the aims and goals of science.' At the outset I linked approaches to progress with views regarding the aim of science. Laudan and Kuhn think that science aims at solving problems, and so success in so doing is their standard of progress. Oddie (2014) accepts that truth is the aim of inquiry, motivating accounts of truthlikeness.[11] Barnes (1991) and I take knowledge to be the goal of science, and accordingly hold scientific progress to be the addition of scientific knowledge.[12] Note that because a goal can have subsidiary goals as means, those who hold that science aims at truth or truthlikeness can agree that science aims at problem-solving, since solving a problem will be a route (albeit a fallible one) to the truth about the matter in question. Likewise the view that knowledge is the goal of science will imply that problem-solving and truth are legitimate goals for science, since pursuing these will further the pursuit of knowledge.

[10]Note that belief here does not need to be occurrent; it may be dispositional. This is not to say that scientists will believe all the significant true implications of truthlike model—many scientists did not believe the ontological implications of the kinetic theory, developed in the 1850s and 1860s, until the twentieth century. It will may a matter of further discovery that a certain element of a model is highly truthlike rather than merely instrumental; such discoveries will themselves be contributions to progress, since now those elements will themselves, by being believed, become knowledge.

[11]I note that in the earlier 2007 version of his (2014) Oddie begins '*Truth* is the aim of inquiry', whereas he now commences more circumspectly '*Truth* is widely held to be the constitutive aim of inquiry.'

[12]The debate about the aim of science naturally relates to debates concerning the nature and aim of belief; cf. Velleman (2000); Wedgwood (2002); Owens (2003).

So the difference between the views must turn on differences between these goals as ultimate or constitutive goals.

So although the problem-solving view of the constitutive aim of science is consistent with scientific realism, it is not a view that will appeal to the realist. For if that view were correct, then a piece of science would have achieved its goal if it solves some problem (in the internalist) sense of Kuhn and Laudan but is nonetheless false. The only way of making that view acceptable is to reject the notion of truth (or truthlikeness) or at least adopt a strong kind of scepticism that would make the goal of truth utopian. If one is a realist and thinks that truth is achievable, then one will be inclined to reject the idea that a problem-solution that *appears* to solve a problem but is false satisfies the aim of science.

It might appear difficult to draw a distinction between the goals of truth and of knowledge. After all if one uses rational means to pursue the first goal and one does achieve it, then one will typically have achieved the second also. There are differences, however. If one could believe a contradiction and all of its consequences, then one would thereby believe all truths (and all falsehoods). That achievement would be to achieve the aim of science to a maximal degree, if that aim is truth. Whereas it would not achieve the aim of science, if that aim is knowledge. If to avoid this it is added that one's beliefs must be logically consistent, one could adopt a policy of believing propositions at random, so long as they are consistent with one's existing beliefs. This would lead to a large quantity of true belief, but to no knowledge. Neither of these policies, though productive of truth if successfully adopted, would count as promoting the goal of science. If that verdict is correct, then the view that science aims at knowledge has an advantage. Alternatively the truth aim would have to be supplemented by an avoidance of error aim. In which case there is a question about how to balance these aims: to what extent does a possible gain in truth outweigh a risk of falsity? The epistemic account does not have to face such a question, since risky belief, even if true, fails to be knowledge.

7 Conclusion—realism, progress, and change

A simplistic picture of the history of science presents science as highly reliable source of truth—science progresses because it is able to add new truths to the stock of old truths. While it not clear that any scholar really believed this to be true, it may be the impression given by some whiggish, heroic histories of science. Furthermore, optimistic accounts of the scientific method or of inductive logic seem to suggest science ought to progress by accumulating truth. The history of science portrayed by Kuhn and others, with its periodic reversals of belief, rejects such a view. And if realist philosophies of science say that the history of science ought to be like this, then those philosophies may be rejected too.

If one draws anti-realist conclusions from this history the view of progress as increasing problem-solving power looks attractive. However, such a view does have to take a stand on what is happening during those moments of rupture when previous problem-solutions are rejected—episodes of Kuhn-loss. One may ague that there is an overall increase in problem-solving power, but to do so implies that there is a weighting of problems and their solutions, so that the new problem-solutions combinations are worth more than those that are lost. Furthermore, Kuhn is clear that old problem-solutions may be rejected on the promise of more and better to come. Problem-solving power is clearly a dispositional concept on this view. If so, it may not be as transparent as Laudan would like, whether an episode of scientific change is progressive.

Realists will hold that this approach to progress suffers from not distinguishing apparent from real progress. That there is little or no difference is a consequence of the internalism espoused by Laudan—it should be possible to tell directly whether a problem has been solved by a proposed solution and progress thereby made. But to those who avail themselves of the concept of truth, a sequence of false problem-solutions to a pseudo-problem (a problem founded on a false assumption) cannot be progress, however convincing such solutions are to those working within that paradigm.

The realist can maintain that the existence of falsehoods in the history of science is consistent with realism, so long as those falsehoods are in due course replaced by truths, without truths being replaced by falsehoods. Nonetheless, the favoured variant on this for realists has been to conceive of progress in terms of truthlikeness. A sequence of theories may be progressive even if false when each is close to the truth than its predecessor. This view can avoid much of the difficulty raised by Kuhn-loss, so long as the new problem-solutions are closer to the truth than those they replace. And that seems to be the case in the standard examples. Although there was Kuhn-loss in the rejection of Descartes's vortex theory, his problem-solutions were badly false (it is not because gravity operates via a vortex that the planets rotate in a plane and in the same direction). So the loss of those 'solutions' should be nothing for a truth/truthlikeness account of progress to worry about. That said there are some cases of truth-loss (e.g. that concerning the moisture content of infants' bodies, when the humoural theory was abandoned) that *per se* would be regarded as regressive by the truth/truthlikeness standard. One might think that this is wrong because it is progressive to cease believing in a proposition, true or false, if one discovers that one's previous reasons for believing it are themselves mistaken. The truth/truthlikeness view might attempt to retrieve the situation by arguing that in such cases the truth-loss is outweighed by truth-gain, which would then require weighting some truths as more significant than others. More problematic are hypothetical cases where there is no corresponding significant gain (such as the example in which N-rays turn out to exist and Blondlot was right but for the wrong reasons).

Neither Kuhn-loss nor truth-loss are, *per se*, problematic for the epistemic account. For the episodes discussed, actual and hypothetical, are not regressive since the beliefs rejected do not amount to knowledge. A regressive episode for the epistemic view is one where there is knowledge-loss—a scientific change that involves scientists knowing less at the end than at the beginning. Realists will expect such episodes to be few and far between. For if scientists have acquired enough evidence and good reasons so that they know that *p*, it will be fairly unlikely that evidence and reasons will come along that will persuade them to give up belief in *p*. That said, we can conceive of hypothetical cases: if a mass of misleading evidence is generated, then the community might lose its knowledge. But even such cases need not trouble the epistemic account of progress; for such episodes really would be regressive. So the epistemic account can claim both to match our pre-theoretic judgments about what kinds of (real or hypothetical) episode are progressive better than its rivals while also finding it more straightforward to deliver the verdict that science has indeed generally been progressive.

References

Barnes, E. 1991. Beyond verisimilitude: A linguistically invariant basis for scientific progress. *Synthese* **88**: 309–339.

Becker, P. 2001. History and progress in the accurate determination of the Avogadro constant. *Reports on Progress in Physics* **64**: 1945–2008.

Bird, A. 2007. What is scientific progress? *Noûs* **41**: 64–89.

Bird, A. 2008. Scientific progress as accumulation of knowledge: a reply to Rowbottom. *Studies in History and Philosophy of Science Part A* **39**: 279–281.

Bragg, W. 1936. The progress of physical science. In J. Jeans, W. Bragg, E. V. Appleton, E. Mellanby, J. B. S. Haldane, and J. S. Huxley (Eds.), *Scientific Progress*. London: George Allen and Unwin.

Descartes, R. 1637. *Discourse on Method* (transl. A. Wollaston, 1960, Penguin ed.). Harmondsworth: Penguin.

Friis-Hansen, B. J., M. Holiday, T. Stapleton, and W. M. Wallace 1951. Total body water in children. *Pediatrics* **7**: 321–327.

Hilpinen, R. 1976. Approximate truth and truthlikeness. In M. Przelecki, A. Szaniawski, and R. Wójcicki (Eds.), *Formal Methods in the Methodology of the Empirical Sciences*, pp. 19–42. Dordrecht: Reidel.

Klotz, I. M. 1980. The N-ray affair. *Scientific American* **242**: 168–175.

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

Lagemann, R. T. 1977. New light on old rays. *American Journal of Physics* **45**: 281–284.

Laudan, L. 1977. *Progress and its Problems: Toward a theory of scientific growth*. Berkeley: University of California Press.

Laudan, L. 1981. A problem-solving approach to scientific progress. In I. Hacking (Ed.), *Scientific Revolutions*, pp. 144–155. Oxford: Oxford University Press.

Miller, D. 1974. Popper's qualitative theory of verisimilitude. *British Journal for the Philosophy of Science* **25**: 166–177.

Mizrahi, M. 2013. What is scientific progress? lessons from scientific practice. *Journal for General Philosophy of Science* **44**: 375–390.

Newton, H. 2012. *The Sick Child in Early Modern England, 1580–1720*. Oxford: Oxford University Press.

Niiniluoto, I. 1977. On the truthlikeness of generalizations. In R. E. Butts and J. Hintikka (Eds.), *Basic Problems in Methodology and Linguistics*, pp. 121–147. Dordrecht: Reidel.

Niiniluoto, I. 1980. Scientific progress. *Synthese* **45**: 427–462.

Niiniluoto, I. 2011. Scientific progress. In E. N. Zalta (Ed.), *The Stanford Encyclopedia of Philosophy* (Summer 2011 ed.).

Niiniluoto, I. 2014. Scientific progress as increasing verisimilitude. *Studies in History and Philosophy of Science Part A* **46**: 73–77.

Nutton, V. 1993. Humoralism. In W. F. Bynum and R. Porter (Eds.), *Companion Encyclopedia of the History of Medicine*, Volume I, pp. 281–291. London: Routledge.

Oddie, G. 2014. Truthlikeness. In E. N. Zalta (Ed.), *The Stanford Encyclopedia of Philosophy* (Summer 2014 ed.).

Owens, D. J. 2003. Does belief have an aim? *Philosophical Studies* **115**: 283–305.

Popper, K. 1972. *Objective Knowledge*. Oxford: Clarendon Press.

Rowbottom, D. 2008. N-rays and the semantic view of scientific progress. *Studies in History and Philosophy of Science Part A* **39**: 277–278.

Rowbottom, D. P. 2010. What scientific progress is not: Against Bird's epistemic view. *International Studies in the Philosophy of Science* **24**: 241–255.

Schoeller, D. A. 1989. Changes in total body water with age. *The American Journal of Clinical Nutrition* **50**: 1176–1181.

Tichý, P. 1974. On Popper's definitions of verisimilitude. *British Journal for the Philosophy of Science* **25**: 155–160.

van Fraassen, B. 1980. *The Scientific Image*. Oxford: Oxford University Press.

Velleman, J. D. 2000. On the aim of belief. In *The Possibility of Practical Reason*, pp. 244–281. Oxford: Oxford University Press.

Wedgwood, R. 2002. The aim of belief. *Philosophical Perspectives* **16**: 267–297.

Williamson, T. 1995. Is knowing a state of mind? *Mind* **104**: 533–65.

Zilsel, E. 1945. The genesis of the concept of scientific progress. *Journal of the History of Ideas* **6**: 325–349.