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ARTICLE

Justifying Scientific Progress

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Abstract

I defend a novel account of scientific progress centered around justification. Science progresses, on this account, where there is a change in justification. I consider three options for explicating this notion of change-in-justification. This account of scientific progress dispels with a condition for scientific progress that requires accumulation of truth or truth-likeness, and it emphasizes the social nature of scientific justification.

1. Introduction

I was surprised to learn that the philosophical literature on scientific progress has neglected a compelling contender. The contender that I consider here holds that science progresses when there is a change in scientific justification. Justification is central to scientific practice and a pillar of knowledge—hence my surprise.

Understanding scientific progress became important after Kuhn. Kuhn's work was seen as a threat to the rationality of science. If science undergoes revolutions, and if a scientific paradigm after a revolution cannot be compared on its epistemic merits to the scientific paradigm prior to the revolution, then, some followers of Kuhn thought, it is hard to see how science makes progress across scientific revolutions. Scientific revolutions as depicted by Kuhn motivated relativism, skepticism, antirealism, and the 1990s science wars.

Though most academics have worked off the hangover from post-Kuhnian extravagance, today we observe widespread public distrust of science. A compelling account of scientific progress could help constrain further deterioration of trust in science, at least when such trust is warranted. An account of scientific progress that is too demanding entails that science makes little progress and thus plausibly should receive little trust. An account of scientific progress that is not demanding enough entails that too many unreliable practices count as scientifically progressive, and thus we would place our trust in unreliable practices.

Existing accounts of scientific progress are too demanding or not demanding enough. The reason why many accounts of scientific progress are too demanding, as I argue in section 4, is that they have a truth requirement or something similar: they hold that for science to progress, it must accumulate more truths or more truth-like conclusions. Bird's (2022) "epistemic account" of scientific progress, for example, holds that science progresses when it accumulates knowledge, and knowledge requires truth. Dellsén's (2016) "noetic account" holds that science progresses when scientific understanding increases; by scientific understanding, Dellsén means the ability to make accurate explanations and predictions, and because accuracy is a factive notion, this account has a truth requirement. What Bird dubs "semantic accounts" are truth-centered: Rowbottom's (2008) account holds that science progresses when it accumulates true scientific beliefs, and Niiniluoto's (2014) "verisimilitude account" holds that science progresses when it accumulates truths or its theories become more truth-like. The account of scientific progress that I explore here is less demanding, though as I will explain, it is just as demanding as reliable scientific work itself, which is demanding enough.

The reason why other accounts of scientific progress are not demanding enough is that they do not require justification and make scientists seem like tinkerers. Most philosophical discussions of scientific progress seem to assume that if an account of scientific progress dispels with truth, then it must be something like a problem-solving account. Kuhn (1963) and Laudan (1977), and more recently, Shan (2019), hold that science makes progress as its ability to solve problems increases, and success at solving problems is judged by standards internal to a scientific discipline (or paradigm, or disciplinary matrix, or research tradition, etc.), rather than by a truth standard (for discussion of the main existing views of scientific progress, see Rowbottom 2023). Yet all sorts of problems can be solved without the progress of science. Defenders of such accounts could say that the problems that are relevant when thinking about scientific progress are necessarily empirical or theoretical in nature, and thus their solution amounts to scientific progress, though if that were so, progress would occur *because* such solutions would be justificatory.

There is space for an account of scientific progress that sits between the overly demanding truth-centered accounts and the underdemanding problem-solving accounts. My aim in section 2 is to articulate and defend such an account of scientific progress. I work out some nuances of this account in section 3. To address what is probably the most obvious question one might ask about this account, I argue in section 4 that scientific progress does not require the accumulation of truths or approximation to truths. In short, this is a novel and compelling account of scientific progress with justification at the center.

Scientific justification is special: it is communal and intersubjective. A complete theory of scientific progress requires that scientific findings have community uptake. Some existing accounts of scientific progress appear to neglect this, though these accounts may implicitly accept that scientific progress is a property of communities, and Bird (2022) explicitly (and convincingly) defends a social account of group belief in science, which could be applied to the notion of scientific progress to uphold a requirement of community uptake. I close the article in section 5 by defending a requirement of community uptake. The justification account of scientific progress is better than existing accounts, as it is consonant with science itself—this is an account of scientific progress faithful to the spirit of the scientific attitude and to the real achievements of science. This is scientific scientific progress.

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2. Scientific progress as change-in-justification

Here is the justification account of scientific progress: *science makes progress if and only if there is a change in justification.* For now the formulation is incomplete because it is silent on what receives justification and what constitutes a change in justification—I address this in section 3. Here I defend the general plausibility of a justification account of scientific progress. Can an account of scientific progress have justification as its centerpiece?

Pretty much every philosopher writing about scientific progress seems to think that the answer is no. Rowbottom (2008), for example, suggests that justification is only instrumental for scientific progress. Dellsén (2023) agrees. Though Bird (2008, 280) requires justification, he is explicit in his claim that nothing short of knowledge constitutes progress, and he specifically claims that justification, although necessary for knowledge and thus progress, is, without truth, insufficient for progress (see also Bird 2007). I address this question more thoroughly in section 4, arguing that truth is not required for scientific progress. I note now only that much of science is epistemic in the truest sense of the word, namely, not about truth at all but about evidence and the evidence–hypothesis relation, trying to determine what hypotheses or theories are justified based on available evidence and to improve those justifications. Scientific practice justly just is about justification.

I address the sufficiency of justification (absent truth or knowledge) as constitutive of scientific progress in section 4. Dellsén (2016) argues against the necessity of justification for progress. His example is Einstein's 1905 explanation of Brownian motion. Because on Dellsén's noetic account of scientific progress, the ability of a theory to explain phenomena is a marker of progress, Einstein's explanation of Brownian motion counted as progress. Yet, this explanation was based on the kinetic theory of heat, which at the time was speculative and so, claims Dellsén, unjustified. And thus, concludes Dellsén, justification is not necessary for scientific progress. However, precisely because the kinetic theory of heat was able to explain Brownian motion, the kinetic theory of heat received some degree of justification, because in general, the ability of a theory to explain a phenomenon provides some justification to that theory. So this is an example of progress, but contrary to Dellsén's claim, this example involved justification.

Another argument Dellsén (2023) gives that strikes me as compelling involves imagining a scientific discipline with a track record of consistently generating false theories. That dismal track record gives scientists in that discipline reasons to think that any current theories are probably false, akin to the pessimistic meta-induction. Now suppose that the discipline develops strong evidence for a theory. This seems like progress, but, claims Dellsén, those scientists would be unjustified in believing that the theory is true (because of the dismal track record in that discipline), and thus, concludes Dellsén, the justification requirement for progress is too demanding. However, note that it is the requirement that beliefs in the *truth* of the theory be justified that is too demanding. This case involves a change in justification for the theory precisely because the case involves the acquisition of confirmatory evidence for the theory. There can be an increase in justification for some hypothesis without there being sufficient grounds to believe that hypothesis. So if one has the intuition that Dellsén does about this case, namely, that it involves progress, the change-in-justification account of progress accommodates that.

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Refereeing the debate between the view that scientific progress is the accumulation of knowledge and the view that scientific progress is the accumulation of true scientific beliefs, Mizrahi and Buckwalter (2014) tested the intuitions of a large number of subjects and found that justification is important for intuitive judgments about what constitutes scientific progress. This provides some support for holding justification as a necessary component in an account of scientific progress.

An accumulation of new evidence can increase justification, and that would amount to scientific progress. The justification account of scientific progress is more general than the noetic, epistemic, and semantic accounts because it allows for nontheoretical progress. Dellsén (2018, 2), for example, claims that scientific progress is strictly about "improvement in our theories, hypotheses, or other representations of the world, rather than other improvements of or within science." (So on Dellsén's account, accumulation of more evidence would not count as progress, and nor would an increase in the confirmation of a hypothesis necessarily count as progress, because the mere increase in confirmation of a hypothesis does not need to involve an improvement of that hypothesis.) The noetic, epistemic, and semantic accounts of scientific progress are too theory-centric (for similar points, see Douglas 2014; Shan 2019; Massimi 2022).

Shan (2019) recently updated the problem-solving account of scientific progress. In this insightful update, the articulation of scientific problems is deemed just as progressive as the proposal of solutions to those problems. I agree that articulation of problems is important. However, without a change in justification, neither the articulation of a scientific problem nor a proposed solution to a scientific problem should be seen as constituting scientific progress. The mere articulation of a scientific problem is like posing a rhetorical question without answering it. Having an articulated scientific problem can be important for the development of some research programs, though it is not necessary. Lucky discoveries can occur without articulated problems, as, for example, occurred with Fleming's discovery of penicillin. That said, it is plausible to think that having articulated problems can contribute to scientific progress. (Bird and others rightly argue that contributing to progress does not necessarily constitute progress—just as a large grant for research may contribute to scientific progress but not constitute it.)

Justification according to what standard? One can perhaps simply adopt any favored account of justification on offer from epistemology. Yet I believe that two options are unattractive. One standard of justification could be strictly internalist by holding that beliefs are justified by the evidence immediately available to an individual scientist. This would be unsatisfying as an account of scientific progress, as it would render determinations of progress highly individualistic and idiosyncratic. Another standard could be that of an ideal epistemic community at the end of inquiry. That option would render justification epistemically inaccessible to virtually all practicing scientists, thereby sapping it of any practical, methodological bite for practicing scientists and of one of its advantages relative to a truth requirement for progress (section 4). An account of justification that sits somewhere between these two options is better. This could appeal to whatever principles and practices a scientific community establishes that serve to minimize epistemic risk, thereby enhancing the objectivity and reliability of its findings (Koskinen 2020).

3. Change in what?

What changes in a change of justification? I describe three options. The first, based on the number of justified beliefs, is my least favorite. The second, based on a notion of graded justification, and the third, based on a notion of change in confirmation, are, to my mind, both plausible, and they are perhaps interchangeable. Because the formal apparatus of the third option is well developed, it allows me to explore a range of nuances, and thus my treatment of the third option is more extensive than the first two.

3.1 Number of justified beliefs

One option to explicate change-in-justification, taking its cue from other recent accounts of scientific progress, would be to understand a change in justification in terms of a change in the number of justified beliefs that science accumulates. Just as Bird (2022) argues that scientific progress is the accumulation of knowledge (which entails the accumulation of justified true beliefs), and just as Rowbottom (2008) argues that scientific progress is the accumulation of true scientific beliefs, one could hold that a justification-centered account of scientific progress would be based on a change in the number of justified scientific beliefs.

Yet, I believe any account of scientific progress based on counting beliefs is implausible. Here is a problem for explicating scientific progress based on the number of beliefs (whether justified or true or both): in any scenario in which there is a justified true belief in *x*, and then more scientific work is performed that further establishes the plausibility of *x*, a belief-counting approach entails that no progress has been made, because there was no increase (or decrease) in the number of beliefs that are justified or true or both (because *x* was already justified prior to the additional scientific work and *x* is, by assumption, here true). A plausible example of this is the detection of the Higgs boson in 2012. Prior to the Large Hadron Collider experiments, the Standard Model of particle physics had a huge amount of empirical and theoretical support. On both a coarse-grained hypothesis like "the Standard Model is true" and a fine-grained hypothesis like "the Higgs boson exists," belief in these hypotheses before 2012 was justified. Yet the Large Hadron Collider experiments that detected the Higgs boson surely must count as scientific progress.

Like the other recent accounts of scientific progress mentioned earlier, a counting-beliefs approach would adopt an ungraded view of beliefs, about which we should be suspicious. In general, there are good reasons not to hold an ungraded account of belief (such as the lottery paradox). A graded view of doxastic states is also more consistent with scientific practice, insofar as science cultivates a fallible attitude toward its products. A long list of luminaries, including Merton (1942), Popper (1963), and Longino (1990), have emphasized the importance of organized skepticism about and criticism of scientific work and its results. One need not adopt Popper's aversion to confirmation to accept the importance of this critical attitude for science.

Finally, as Dang and Bright (2021) have recently argued, scientists need not believe claims that they assert as scientifically justified, particularly when many scientists work collaboratively on a project. (Lackey [2007] has made a more general argument that belief is not a norm of assertion.) Science is not an institution that simply gathers a set of claims that scientists sign up for either believing or disbelieving. In general,

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I believe it is a mistake for philosophers of science to follow epistemologists by developing a belief-centric epistemology of science; rather, scientific epistemology should be centered around confirmation of scientific hypotheses or theories, which can be based on credences of individuals but can involve much more.

3.2 Change in degree of justification

Another option to explicate change-in-justification, taking its cue from recent work in epistemology, would be to understand a change in justification as a change in the *degree* of justification for a belief or scientific claim. Some thinkers have argued that justification is a gradable property (see, e.g., Gerken 2022), which seems plausible.

A change in degree of justification can occur when a scientist generates new evidence, or when a new hypothesis is introduced, or when scientists improve the reliability of methods. A clear instance of a change in degree of justification occurs when newly acquired evidence increases the confirmation of an existing hypothesis (I discuss this change-in-confirmation approach to justification in the following section). When this happens, science makes progress. Such progress might be modest, or it might be dramatic, as occurred with Eddington's observation of the bending of light.

The degree of justification of a hypothesis or theory can be influenced by the so-called theoretical virtues, such as simplicity, scope, or accuracy, or other nonempirical features. For example, Dawid, Hartmann, and Sprenger (2015) argue that the "no-alternatives argument" can provide some justification for theories (however, for a discussion of a problem with appealing to theoretical virtues as a basis for theory choice, see Okasha 2011; Stegenga 2015).

The example of the detection of the Higgs boson, which was a problem for the belief-counting approach to change-in-justification, is a little less of a problem for this account. The 2012 detection of the Higgs boson was, of course, evidence for the existence of the Higgs boson, and evidence for the Standard Model. Yet before 2012, the existence of the Higgs boson and belief in the Standard Model were well justified. And because those claims were already well justified, the work at the Large Hadron Collider that led to the detection could add little justification, which might strike some as counterintuitive.

One potential concern for this approach is that we do not have a very well developed account of graded epistemic justification (for a recent argument that the landscape of graded justification is muddy, see Hawthorne and Logins 2021). Nevertheless, it is intuitive that epistemic justification is indeed graded, and one plausible way to articulate such a notion is by using the tools of confirmation theory.

3.3 Change in confirmation

One way to explicate change-in-justification, taking its cue from recent work in formal epistemology, would be to understand a change in justification in terms of a change in confirmation.

The best-developed account of scientific confirmation is based on the tools of probability. Sprenger and Hartmann (2019) lay out the basics of Bayesian confirmation theory and then apply it to several topics in philosophy of science, including the tacking paradox, the grue paradox, and the paradox of the ravens.

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I extend this approach to give an account of scientific progress in which confirmation is central.

An obvious case of progress is when new evidence is generated that adds confirmation to a hypothesis. Yet both increases and decreases in confirmation can constitute scientific progress. A decrease in confirmation can occur in an instance of failed replication: scientists might have a relatively high degree of confirmation for some hypothesis because an initial experiment provided evidence supporting the hypothesis, yet if a subsequent experiment attempting to replicate that initial experiment provides evidence disconfirming that hypothesis, this can count as progress. Indeed, replication failures have become especially important recently due to the so-called replication crisis in psychology. For example, Baumeister et al. (1998) published evidence supporting the existence of "ego depletion," the putative phenomenon in which subjects' self-control is a limited resource that can be used up; later, larger experiments did not observe ego depletion (e.g., Vohs et al. 2021), and such a replication failure should count as scientific progress.

When a scientist introduces a new hypothesis that can explain existing evidence better than already available hypotheses, that new hypothesis can undergo a huge increase in confirmation (from zero or undefined to substantial) while the already existing hypotheses undergo a decrease in confirmation (Lipton 2004). That is progress. A nice example of this was provided by Einstein, whose 1915 general theory of relativity was able to explain the precession of the perihelion of Mercury's orbit, a by-then well-established empirical phenomenon that could not be explained by existing physical theory.

A distinction that goes back to Carnap is between absolute confirmation, the degree to which some hypothesis H is supported by evidence E, and incremental confirmation, the extent to which the support of H changes upon getting E. Absolute confirmation is represented by Bayesians as the posterior probability, or the probability of H given E, $P(H|E)$. There are various measures of incremental confirmation, each with distinct formal representations. Two prominent measures include the difference measure, which is the difference between the posterior probability and the prior probability, $P(H|E) - P(H)$, and the likelihood ratio measure, which is the ratio between the likelihood of E given H divided by the likelihood of E given a contrast hypothesis H', $P(E|H)/P(E|H')$ (see Fitelson 1999). Because progress implies change, one might think that the approach here is based on incremental confirmation, though a justified change in confirmation implies a change in absolute confirmation, so one can make sense of this account of scientific progress according to either absolute or incremental confirmation. As is standard, $C(H,E)$ represents the incremental confirmation that E provides to H without specifying a particular confirmation measure, and $C_i(H,E)$ represents the incremental confirmation that E provides to H by confirmation measure *i*.

For Bayesian accounts of confirmation like that of Sprenger and Hartmann (2019), probabilities are representations of an agent's credence. To address the worry that such a subjective foundation cannot be the basis for characterizing central features of science, Sprenger and Hartmann respond by claiming that the agents they are modeling are ideal, rational, and responsive to evidence. Change in confirmation is represented using the formal measures noted earlier, and the probabilities represent

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credences of a rational scientist who responds appropriately to evidence such that their resulting credences are justified by their evidence.

Whether there is a *unique* way to appropriately respond to evidence is a controversial question in epistemology—for what it is worth, I do not believe that there is, yet arguing that point would take me astray (for differing views on the so-called uniqueness thesis, see White 2005; Kelly 2014). If there is a unique way to appropriately respond to evidence, then the formal measures of incremental confirmation are simply representations of that uniquely justified way to respond to evidence. If there is not a uniquely justified way to respond to evidence, then anyway, there are plausible constraints on justified responses to evidence.

Consider this example. Maria and Sasha want to evaluate hypothesis H, which says that "this drug does blah blah." Both Maria and Sasha have the same prior, P(H). A randomized trial is performed that gives evidence (E) suggesting that the drug does blah blah. If uniqueness is true—if there is a unique way to appropriately respond to evidence—then when given E, both Maria and Sasha will assign the same degree of confirmation to H. If uniqueness is false, then their assessment of the confirmation provided to H could differ. Perhaps Maria thinks that randomized trials are not as epistemically important as they are often made out to be (having read Worrall 2002), whereas Sasha thinks that randomized trials are more reliable than the alternative (having read Larroulet Philippi 2022). Yet for both Maria and Sasha, their posterior, P(H|E), must be greater than their prior, P(H), because E offers at least some confirmation of H (assuming the plausible "positive relevance" definition of evidence). Thus both Maria and Sasha conclude that H receives some incremental confirmation: for both Maria and Sasha, $C(H,E) > 0$. Thus, for both Maria and Sasha, according to the confirmation account of scientific progress, this episode involves scientific progress. (When given some other evidence E′, Maria and Sasha might disagree about which of E or E′ provides more confirmation to H and thus about which evidence contributes more scientific progress, but such disagreements are faithful to real scientific disputes.)

One challenge to this approach is that if both increases and decreases in confirmation can count as progress, then there can be a hypothesis that receives first an increase in confirmation and then a decrease of the same amount, and then an increase, and then a decrease, and so on, and that does not really look like progress. Consider confirmation of H by a sequence of experiments that generates $E_1 - E_N$ accordingly, and we measure confirmation with the difference measure C_d. We can have

$$
\begin{aligned}
C_d(H, E_1) &= x \\
C_d(H, E_2) &= -x \\
C_d(H, E_3) &= x \\
C_d(H, E_4) &= -x
\end{aligned}
$$

and so on to *N*. At the end of this sequence, the posterior probability of the hypothesis would be the same as its prior was before the sequence of experiments began. It might seem unintuitive to count this as scientific progress. Yet it is consistent with the confirmation account of scientific progress.

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If an episode in science went through a small number of such iterations, then I would have no problem calling that scientific progress. There are real cases that involve the plausibility of a hypothesis waxing and waning and then waxing again. For example, Margaret Mead (1928) shocked the world with her description of sexually permissive teenagers in Samoa; Derek Freeman (1983) then argued that Mead's evidence was unreliable, and thus the teenage sexual permissiveness hypothesis was disconfirmed; subsequently, Paul Shankman (2009) argued that Freeman had exaggerated his criticisms of Mead, and thus the teenage sexual permissiveness hypothesis was plausible, which is roughly where things now stand.

However, I doubt that many real cases in science involve more than a handful of such iterations—I cannot think of any, though if that is just a result of my ignorance, then I await edification.

Another possible challenge to this approach would be any instance in which there is scientific progress with no change in confirmation. Yet I cannot think of any examples, and I am tempted to think that this is because of the analytic relationship between scientific progress and change in confirmation. Consider first an example in which one has a prior of zero for some hypothesis and acquires evidence about that hypothesis, none of which is confirmatory; there has been an accumulation of evidence but no change in confirmation. Is it progress? I do not think so. That is because hypotheses for which we have zero priors are like "Santa Claus exists" or "my body is composed of fewer than seven atoms." Gathering evidence that provides no confirmation for such hypotheses is not scientific progress, and mutatis mutandis for priors of one (acquiring evidence that confirms the hypothesis "Santa Claus does not exist" is not scientifically progressive). If a posterior differs from the prior, and so there is a change in confirmation, there must be some newly developed confirmatory or disconfirmatory element, such as acquisition of evidence for the hypothesis or a refinement of the hypothesis, and such developments are progressive for science.

Evidence can, obviously, provide confirmation or disconfirmation to a hypothesis. One might be tempted to ask what the nature of evidence itself is. Addressing this question in any detail here would take this article astray. It is enough simply to say that evidence is that which provides confirmation or disconfirmation to hypotheses. The austere positive relevance definition of evidence holds that some evidence E is confirming evidence for a hypothesis H if and only if $P(H|E) > P(H)$. For the present purpose of explicating scientific progress, that should be enough to say about evidence.

Nevertheless, consider Williamson's (2000) $E = K$ thesis, which holds that one's evidence is constituted by what one knows. On that account, gathering new evidence amounts to an accumulation of new knowledge, and if this is so, then one might think that the account of scientific progress based on a change in confirmation or justification by new evidence is, after all, a knowledge-based account; Bird (2022), for example, pursues such an approach to scientific progress. Yet we have already seen ways in which a change in justification can occur without gathering new evidence, such as by introducing new explanatory theories or refining existing theories, solidifying background assumptions, and appealing to theoretical features like simplicity or other nonevidential considerations, such as the no-alternatives argument. So even on such an account of evidence, a justification account of scientific progress does not reduce to a knowledge account.

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I have defined scientific progress as a *change* in justification or confirmation. One might think that progress should be defined in terms of *increase* rather than mere change. That, however, would face the challenge mentioned earlier of explaining how a failed replication could count as progressive. More substantively, I believe that an account of scientific progress in terms of change in confirmation and an account in terms of increase in confirmation are equivalent. Suppose we are considering some hypothesis X and we get evidence E that provides some incremental *dis*confirmation to X; we can conceive an alternative hypothesis, Y, that says "not X," and Y, then, gets an increase in confirmation due to E. There is a change in confirmation (specifically, a decrease in confirmation) of X but an increase in confirmation of Y. So *increase-in-confirmation* and *change-in-confirmation* are formally interchangeable as an account of scientific progress.

Some increments in confirmation may be minuscule. And sequences of increments in confirmation can involve diminishing returns. Suppose I want to know if a coin is biased to heads. I toss the coin. Heads. I toss again. Heads. I toss again. Tails. Ten tosses, eight heads. One hundred tosses, seventy-seven heads. One thousand tosses, 789 heads. So I am now thinking that this coin is biased roughly to 0.8 heads. The hypothesis "this coin is biased to 0.8 heads" received a lot of confirmation in the first ten tosses, but the amount of incremental confirmation received by the ten tosses between the 700th toss and the 710th toss is much, much less. I raise this point here because it will make sense of an important example in section 4.

3.4 Summary

I have considered three options for explicating the notion of a change in justification. There may be other viable options, though these three seem like plausible contenders.

Because the justification account of scientific progress dispels with the truth requirement, it might be seen as a close cousin of the problem-solving account of scientific progress most thoroughly developed by Laudan (1977), as that account also dispels with the truth requirement. However, Laudan was positively allergic to thinking about scientific progress in terms of justification or confirmation. Whether a theory is "well or poorly confirmed," claimed Laudan, is irrelevant to assessing progress (22–23). All that matters on his account is if a theory can solve a problem. Problems, according to Laudan, can be empirical phenomena, and a solution can involve a theory providing an explanation of those phenomena, regardless of its confirmation (see Laudan 1977, 25). However, precisely because a theory receives some confirmation when it can explain an empirical phenomenon, Laudan perhaps should not have had such an allergy to a confirmation account of scientific progress. Yet, many instances of changes in confirmation are important and constitute progress but do not contribute to the solution of a problem. The problem-solving account is incomplete, and that is vivid when compared to the justification account.

So there is a lot to like about the justification account of scientific progress. It makes sense of so much scientific work, routine scientific work, such as generating new evidence—science progresses with the accumulation of new evidence, not just with the refinement of existing theory or the introduction of new theory, and so the justification account is more general and, I think, more intuitive than theory-centered accounts of scientific progress. It makes sense of the great value of introducing a new

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hypothesis that explains existing evidence. It makes sense of the importance of experiments aimed at replicating existing findings and the interest generated when such attempts fail. It emphasizes the importance of justification. It is given foundational legs by our best general philosophical theory of scientific confirmation and the epistemology of reasoning. It entails that scientific progress is epistemically accessible to scientists. What might be seen as its main shortcoming, namely, its lack of reference to truth, is in fact one of its merits, as I now argue.

4. Truth as convenient benediction

The justification account of scientific progress dispels with the necessity of accumulation of truths or related factive notions for scientific progress. Yet, a widespread belief is that the aim of science is truth or a related notion, such as knowledge. If the aim of science is truth or knowledge, then it is a natural thought that science makes progress as it accumulates truths or knowledge. We saw earlier that several prominent accounts of scientific progress have a truth requirement. My aim in this section is to offer three arguments against a truth requirement for scientific progress.

Ascriptions of scientific hypotheses as *true* are not typically part of routine scientific practice; rather, ascriptions of truth are typically retrospective benedictions. Such benedictions are convenient, as they provide a simple summary of the messy details of scientific work, for allocating credit, teaching students, distributing research funds, and communicating to the public. Truth is convenient benediction. This is not to say that truth is not important, or is not disquotational, or does not correspond; I believe that truth in general has those properties. My point is more modest—to call truth in science a benediction is to emphasize that ascertainment of truth can take a long time and, obviously and typically, occurs in retrospect.

In real time, scientists are able to ascertain justified changes in confirmation. In real time, scientists are not able to ascertain the achievement of truth. Benedictions of truth take time (Massimi 2016). When Watson and Crick finished building their model of the double helix structure of DNA, they were confident enough of their achievement to walk across the street to the Eagle pub in Cambridge to celebrate. They had a clear-eyed assessment of how well confirmed their model was. Yet their one-page 1953 paper in *Nature* was shot through with caution; they claimed that their model was a postulate, based on numerous assumptions, and that alternatives to their model, though unlikely, were possible. They were not giving their own finding a benediction. That benediction came nine years later, when they were awarded the Nobel Prize. So in some episodes of scientific progress, benedictions can be made soon after the scientific work itself. However, in other episodes of scientific progress, benedictions can take a very long time. For example, it took generations of scientists to properly establish Copernican theory (Westman 2011).

Laudan (1977) argued that real-time epistemic accessibility of scientific progress is a desideratum for an account of scientific progress—a scientist or a scientific community should be able to ascertain that by doing *x*, they are making progress. Just as a mountaineer should be able to determine if they are getting nearer to the summit, and just as a baker should be able to determine if the bread is rising, I find this epistemic accessibility requirement for scientific progress plausible. Laudan

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famously argued for antirealism (based on the pessimistic meta-induction); if antirealism is true, and if one held a truth requirement for scientific progress, then it would follow that science cannot make progress—Laudan took that as an argument against accounts of progress that have a truth requirement. Bird (2022) and others push back against Laudan by directly targeting the argument for antirealism. Yet one can adopt the epistemic accessibility desideratum without adopting antirealism. Here is the general point: the fact that it can take a long time after scientific work occurs for the truth of the findings of that work to receive benediction means that any account of scientific progress that maintains a truth requirement must violate the epistemic accessibility desideratum. (It also follows that truth cannot be a "norm of assertion" for science, contrary to Price's [2003] claim that truth is a norm for all assertoric discourse and to Bird's [2022] claim that knowledge is a norm of correctness for science.)

Similarly, it can take a very long time to learn that one's theories are false and not even approaching the truth. This raises the next problem for maintaining a truth requirement for scientific progress, what I call the *Ptolemaic challenge*. Ptolemaic astronomers toiled for centuries to tally the planets and stars and their positions over time. They developed an Earth-centered model of the solar system based on the geometry of epicycles (a smaller circle placed on the circumference of a larger circle). Their epicyclic models were very successful at explaining their observations, and when they observed anomalous celestial phenomena, they refined their models by adding more epicycles (For a detailed discussion of Ptolemaic astronomy, I recommend Kuhn's 1957 book *The Copernican Revolution*). It was a research program that lasted for many centuries, based on rigorous observations that were accounted for by mathematical theorizing that became more and more sophisticated. Yet all those models were false, and they were not, over all those centuries, getting any closer to the truth, as they were all models of the solar system placing Earth at the center. To maintain a truth requirement for scientific progress requires one to hold that Ptolemaic astronomy made no progress. *Not a drop.*

I find it odd to think that Ptolemaic astronomy made no progress. More than odd. Such a view is offensive to those ancient late-night observers of the starry sky, those scientists of the oldest science, those curious heirs to Babylon and those diligent students of Aristotle, those scientists who spent centuries in the cold, dark nights of northern Africa to record the movements of stars and planets on clay tablets and who devised intricate theories based on models of epicycles on epicycles, those scientists whose forebears designed the pyramids of Egypt to align with the stars and who calculated Earth's circumference to nearly its true value, those scientists who could at least offer a putative explanation of the westward motion of the sky and the eastward motion of the moon relative to the stars and the retrograde motion of planets by layering epicycles on epicycles, and who could predict astronomical observations to within the limits of what could be observed with the naked eye *one thousand years into the future* using epicycles on epicycles—epistemic feats surely more impressive than that which could be achieved today by most lovers of science.

Here we have the Ptolemaic challenge. If an account of scientific progress maintains a truth requirement, it must say that Ptolemaic astronomy made no progress. But Ptolemaic astronomy did make progress. The semantic, epistemic, and

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noetic accounts of scientific progress face the Ptolemaic challenge. For that reason, they are too demanding.

The Ptolemaic challenge also applies to the verisimilitude version of an epistemic account of scientific progress, as there is no sense in which subsequent iterations of Earth-centered models of the solar system were more truth-like. Niiniluoto (2014) distinguishes between real progress and estimated progress, where real progress is based on increasing verisimilitude and estimated progress is based on merely an apparent increase in verisimilitude. He would say that Ptolemaic astronomers merely seemed like they were making progress but that they were not in fact making progress. This, too, faces the Ptolemaic challenge.

One might think that after centuries of adding epicycles on epicycles, Ptolemaic astronomy was no longer making progress. Indeed, Lakatos's (1978) attempt to articulate a demarcation criterion for scientific research programs had precisely this sort of concern in mind. Lakatos held that a research program, by which he meant the development and testing of a sequence of theories, is progressive if the sequence of theories makes more and more predictions and if more and more of those predictions turn out to be true; and if a research program is not progressive, then, Lakatos claimed, it is "degenerating." On this account, later Ptolemaic astronomy was not progressive; it was degenerating. Yet, Lakatos's demarcation criterion placed too much emphasis on novel predictions; philosophers of science have pretty much reached a consensus that while predicted evidence can be more confirming than merely accommodated evidence, that is not always the case, and accommodated evidence can provide some confirmation to a theory (though arguing this point would take me astray; see, e.g., Barnes 2008; Frisch 2015). Moreover, a research program can make little or no progress but need not be "degenerating." Ptolemaic astronomy in the late Middle Ages was plausibly in a phase of "diminishing justificatory returns," as mentioned in section 3—though some incremental confirmation could be gained by adding a 37th epicycle, it was very little.

Laudan (1984) describes truth as a utopian aim for science, because, impressed by the pessimistic meta-induction, he claims that we can never achieve the aim, and even if we could, we could not know if the aim had been achieved. Bird (2022) rightly complains that this is an excessively skeptical position. With Bird, I agree that we can often come to know that science has achieved truth (though, as earlier, in science, that can take a long time). Yet sometimes we cannot know that we have achieved truth, or are approaching truth, and importantly, sometimes we cannot know that what we now take to be true is in fact false. That was the plight of the Ptolemaic astronomers for many centuries. Here my position is somewhere between Bird and Laudan. Truth is not a utopian norm, rather, it is a *nirvana norm*. With great diligence, some people may be able to achieve nirvana, just as with great diligence, science can achieve truth. Yet one might be approaching nirvana and not know it, and conversely, one might not be approaching nirvana but think otherwise.

Moreover, nirvana norms are not action guiding and can be used for assessment only retrospectively. Traffic laws guide action—they influence behavior in real time—and of course a police officer can appeal to a traffic law as the basis for pulling you over for speeding. A norm that says "seek truth" tells scientists little about how to behave, just as a norm that says "seek nirvana" tells me little about how to behave. Such abstract norms need supplementary, concrete, action-guiding norms.

In Buddhism, action-guiding norms are articulated in the Noble Eightfold Path. Each path is constituted by concrete, action-guiding norms; the "right speech" path, for example, says: no lying, no rude speech, no idle chitchat; the "right livelihood" path says: do not earn money by selling weapons, living beings, meat, or alcohol. Telling a person to seek nirvana is to tell them nothing—it is a nirvana norm. Telling a person to follow the Noble Eightfold Path is to give them very concrete guidance on action. The equivalent concrete norms for science would be whatever principles and practices one has good reason to think minimize epistemic risks and thus are reliabilityenhancing and ground claims to justification (Koskinen 2020).

I have given three arguments against maintaining a truth requirement for an account of scientific progress: the epistemic accessibility argument (truth as benediction), the Ptolemaic challenge, and the truth-is-a-nirvana-norm argument. To repeat, I am not suggesting that truth is unimportant or that science cannot attain truth. I am arguing only that scientific progress is to be judged by reference to changes in justification rather than achievement of truths or approximations to truth (which is, thus, a version of pragmatism, insofar as some pragmatists dispense with a truth norm but emphasize justification; see Rorty 1998). Science can come to discover truths about the world precisely by engaging in its justificatory practices, perhaps by adopting what Nagel (1986) called a "view from nowhere." However, science cannot adopt a view from no-who.

5. No view from no-who

Suppose Sasha is searching for the holy grail of science, the ultimate theory of everything, and after years of work, she makes a breakthrough discovery, a theory that unifies all physical laws and explains all existing anomalies. She writes up her finding. But she worries about her discovery being used to develop terrible weapons. She burns her manuscript, moves to Nepal and joins a Buddhist monastery, never speaks with anyone about her discovery, and lives out her final years in quiet solitude.

Sasha accumulated knowledge, a true finding that could solve many problems and that was, on traditional personalist grounds, justified. Some existing accounts of scientific progress would appear to maintain that Sasha made scientific progress. Yet she did not. I noted previously that scientific justification is communal and intersubjective. For a scientific achievement to contribute to scientific progress, there must be not only an in-principle possibility of community uptake but also some actual community uptake. Such uptake can take time, as occurred with the Copernican model of the solar system, but eventually, such uptake must occur. A finding that is observed by no one other than the scientist responsible for the finding can hardly be deemed scientific, let alone a contribution to scientific progress. Science cannot make progress with a view from no-who.

The cognitive achievements central to each of the accounts of scientific progress are nothing without community uptake. The existing literature on scientific progress has focused on these cognitive achievements, asking which kind of cognitive achievement is the fundamental kind for scientific progress. Yet Sasha's story shows that this is incomplete.

As Merton (1942), Longino (1990), Massimi (2022), and many others have emphasized, science is a social institution. Many scientists and philosophers of

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science have held that science is fundamentally public and that its methods and evidence must be intersubjectively accessible (e.g., Shapin and Schaffer 1985; Popper 1959; though for pushback against this publicity requirement, see Goldman 1997). Moreover, some philosophers argue that scientific communities are themselves epistemic agents (e.g., Bird 2022). A scientific finding must be made public in one way or another, at some time or other, for that finding to contribute to scientific progress, and the relevant scientific community must engage with that finding, and hold that finding to its standards, to determine if a change in confirmation pertaining to that finding is justified; if it is justified, the community can do further work on the hypothesis, refining it or relying on it to discover new findings; if it is not justified, further work can be done on it, or the finding can be discarded. (Ultimately, the community can decide whether the finding receives the benediction of truth, but as I argued in section 4, progress itself occurs at the moment of justified change in confirmation, not at the moment of benediction.)

Let us call this the community uptake requirement for scientific progress. One consideration in favor of the community uptake requirement is the simple fact that for future scientific work to develop based on an earlier finding, that finding must be available to other scientists. Another consideration in favor of the community uptake requirement is based on the lesson Longino (1990) taught us about the importance of criticism in science—if scientific findings are not shared in one way or another, they cannot be criticized, and criticism is a hallmark of objectivity. No one was able to critically evaluate Sasha's discovery. Still another consideration in favor of the community uptake requirement is that science education requires the content of science to be available. Still another consideration in favor of the community uptake requirement is Bird's (2022) argument that scientific communities themselves can be the bearers of scientific knowledge. Finally, benediction can occur only if the community uptake requirement is satisfied (For an extended and compelling argument that scientific justification is fundamentally communal and intersubjective, I recommend chapter 3 of Gerken [2022] and chapter 4 of Bird [2022], the latter of which develops an account of "social knowing.").

One might respond by holding that the work that goes into satisfying the community uptake requirement is not itself epistemic. This response could say that it is the cognitive achievement alone that matters for scientific progress. What is subsequently done with that cognitive achievement, goes this response, such as publication or discussion in the public sphere or education, does not add anything to the cognitive achievement itself. Many contingent, noncognitive (sociological) reasons could limit the uptake of scientific findings, but these should not speak against a scientific achievement counting as a contribution to progress. Yet this response is too insensitive to the social structure and function of science. (Making a related point, Harris [2021] argues to the effect that it is the doxastic states of communities of scientists rather than of individual scientists that matter for scientific progress.)

An interesting example of uptake not occurring can be seen in mathematics today in a dispute over whether the so-called abc conjecture has been proven. The *abc conjecture* is a fundamental conjecture in number theory, and were it true, many other famous theorems in number theory would follow, such as Fermat's last theorem. In 2012 the mathematician Shinichi Mochizuki posted on his own website a

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putative proof of the abc conjecture that ran for six hundred pages and was based on novel mathematical theory that he alone had developed over years. Mochizuki told no colleagues about his proof, but he had little need to, as rumors were already circulating. Yet when fellow mathematicians began to discuss the preprint, they noted that "it involves ideas which are completely outside the mainstream" and that it was like a "paper from the future, or from outer space" (Ellenberg 2012). Most mathematicians today consider the conjecture still unproven, and some have noted specific flaws in the putative proof. Mochizuki claims that the failure is with other mathematicians and not the proof. I find it compelling to think that at this point, the Mochizuki proof has offered little progress; if, in the future, mathematicians come to accept the validity of the proof, then progress will be made, but importantly, the work that went into the proof itself will constitute only part of that progress.

6. Conclusion

I have offered a new account of scientific progress that is superior to existing accounts. Existing accounts of scientific progress are either too demanding, as with the accounts that have a truth requirement, or not demanding enough, as with the problem-solving accounts of scientific progress. Science makes progress, on my account, when there is a change in justification. This account of scientific progress is more in line with scientific practice than are competing accounts, as scientific practice is fundamentally centered around practices of justification, and the fallibilism and organized skepticism of science are better suited to a justification-centered account of scientific progress. Finally, an account of scientific progress can be complete only by taking into account the social structure of science.

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