Forthcoming in *Studies in History and Philosophy of Science*

Scientific Progress Without Increasing Verisimilitude: In Response to Niiniluoto

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First, I argue that scientific progress is possible in the absence of increasing verisimilitude in science's theories. Second, I argue that increasing theoretical verisimilitude is not *the* central, or primary, dimension of scientific progress. Third, I defend my previous argument that unjustified changes in scientific belief may be progressive. Fourth, I illustrate how false beliefs can promote scientific progress in ways that cannot be explicated by appeal to verisimilitude.

1. Scientific Progress Without Increasing Verisimilitude

Niiniluoto (2014, p. 77) argues that 'scientific progress can be defined by increasing verisimilitude', and against Bird's (2007, 2008) view that scientific progress should be understood in terms of knowledge.[1] In the course of his discussion, Niiniluoto criticizes my arguments against Bird's view, which appear in Rowbottom 2008 & 2010. The first goal of this paper is to respond to those criticisms. The second is to illustrate that Niiniluoto's view of progress is too narrow.

[1] I am broadly sympathetic to Niiniluoto's criticisms of Bird's view, several of which were anticipated by Cevolani and Tambolo (2013). So I will not discuss those.

I will jump straight to the second, bolder, task. (Let's start with a bang!) How can we show that scientific progress should not be 'defined by increasing verisimilitude'? We need only find a single case, hypothetical or actual, where the verisimilitude of scientific theories fails to increase, yet science nevertheless makes progress. Then we may conclude that increasing verisimilitude in the content of science is *not necessary for*, even if it turns out to be sufficient for, scientific progress.

I'll give such a hypothetical case in the next paragraph. But beforehand, I must prepare the ground by signaling my agreement with Niiniluoto (2011) on two matters. First, the notion of scientific progress is normative: 'the theory of scientific progress is not merely a descriptive account of the patterns of developments that science has in fact followed. Rather, it should give a specification of the values or aims that can be used as the constitutive criteria for "good science".' Hence, '[t]he task of finding and defending such standards is a genuinely philosophical one which can be enlightened by history and sociology but which cannot be reduced to empirical studies of science.' (ibid.) Second, progress may be multi-faceted: 'Progress is a goal-relative concept. But even when we consider science as a knowledge-seeking cognitive enterprise, there is no reason to assume that the goal of science is one-dimensional.' (ibid.) This is the case in Niiniluoto's own account, in so far as both truth *and* informativeness (or what Kuhn called 'scope') are significant. On a one-dimensional account focused on truth, by way of contrast, making modern science's claims closer to the truth by limiting its domain, e.g. by throwing out all scientific theories except those of biomechanics, could be progressive.

Brace yourself. It's thought experiment time.[2] Imagine that all the leading scientists working in a specific area of physics have gathered to discuss the state of their field. (This could be mechanics, for instance.) They all agree that they have identified the true general theory in the domain. Moreover, it is *true* that they have identified the true general theory in the domain, and their beliefs that they have done so are *justified*. (So estimated verisimilitude equals actual verisimilitude.) They discuss what they should do next, if anything. Is there any further scientific progress possible in their area? If not, then it would be reasonable for them to crack open a jeroboam of champagne, celebrate their great success, and move on to new things.

Now Niiniluoto's view entails that no more scientific progress is possible in the area, because a true *general* (i.e., maximally verisimilar) theory has been found. The corollary is that it would be acceptable for the scientists to cease working in that area.[3] But this is incorrect. Why? First, the true theory could be difficult, or even impossible, to use for predictive purposes. It could concern some initial conditions that are beyond our ability to determine the values of. Or it could be unusable in many situations in which predictions would be desirable, due to the need for arduous calculations. Second, even if it were of considerable predictive use, it might fail to

[2] I cannot make it my business to defend the use of thought experiments here, although I have reservations about their use in some circumstances. Suffice it to say two things. First, the other participants in this debate also use thought experiments (whether fully hypothetical or counterfactual in character) – Bird 2007, for example, is full of them – and I am fighting fire with fire. Second, I can see no easy way to avoid using fire, because even judgements about what might instead have happened in history are thought experimental (*qua* counterfactual) in character.

[3] Naturally, they could remain willing to re-examine the theory in the future, in the event that new evidence, questioning its truth, should come to light.

measure up to our explanatory expectations. Imagine that it involved considering numerous variables, such that it was hard to appreciate how changes in one would tend to affect changes in another, in many applications, without the use of extensive computer simulations. It would not serve to grant insight, or to build 'physical intuition'. Consider also the role of models, and approximations and idealizations, in bringing theories into contact with experience. Think of pendulum motion. Knowing the true theory thereof is simply not enough. We want to know the models, like that of the simple pendulum, too. To be more specific, you could know the theory of real pendulum motion, in principle, without spotting that when the angle of swing is small, the sine of the angle is approximately equal to the angle, and hence that the motion is approximately simple harmonic provided that (approximately) no damping occurs. In other words, the complete true theory is rather complex; it includes factors dealing with friction at the bearing, air friction, the mass of the rod, refers to the sin of the angle, and so on. Therefore it obscures the result that pendulum motion is approximately simple harmonic, in the aforementioned circumstances. It is true that this result *can* be derived from the true theory. But such derivations are sometimes extremely difficult, and making them requires a lot of effort.[4]

[4] Another brief example, of a different type, may be helpful. Consider Bohm's theory of quantum mechanics, as against an indeterministic alternative (such as Popper's, which involves single case probabilities). The former may be a neater and easier way to think of matters in many contexts – to build physical intuition about what will happen in various experimental scenarios, and to avoid worries about the effects of measurement – although it may be further away from the truth. The world might really be indeterministic, and particles might lack definite positions, for example. For more on Bohm's theory, see Cushing (1994).

How might Niiniluoto respond? One option is to say that any remaining work in the field has the character of engineering, rather than science. First, however, this seems to inappropriately marginalize applied science. Do we *really* want to say that any work to generate new predictions from a true theory, once a true theory is found, counts as non-scientific? (Kuhn, for one, would have disagreed; classification and prediction are both parts of his 'normal science', as detailed in chapter three of *The Structure of Scientific Revolutions*.[5]) Do we want to say that Lagrangian mechanics was a development in engineering, rather than physics, in so far as it did not question Newton's laws of motion? It is a curiosity, then, that Lagrangian mechanics is widely regarded as a theoretical development in physics. Second, more trenchantly, even if we accept that improving the predictive power of theories – e.g. by reformulating them or developing models to apply them – is not part of science, such developments promote understanding and insight too. So if we took the 'only engineering remains' line, we would be forced to conclude that improving understanding of how phenomena interrelate does not, by itself, count as scientifically progressive.[6]

Another response is to deny that the scenario in the thought experiment is possible. And the most natural way to do this, I think, is to suggest that any true general theory, in a given area, must also be maximally virtuous relative to possible alternatives, i.e. maximally simple, comprehensible, predictively useful, and so forth.[7] There is

[5] See also the more detailed discussion in Rowbottom 2011b & 2011c.

[6] I hold a non-factive account of understanding, on which the propositions expressing an understanding may not be true, in line with that presented by Elgin (2007); we will return to this issue subsequently.

[7] Note that the 'possible alternatives' need not be general, or also have maximal scope; for example, there could be multiple alternatives with individually less scope, but jointly as much scope. Hence, it is not necessary to assume that there is more than one theory that saves all the phenomena in the domain.

plausibly no empirical evidence to this effect, from history, in so far as we haven't acquired any true general theories. But even accepting that we *have* identified some such theories, the claim is *comparative*. And what empirical grounds do we have for making a conclusion about all *possible* alternatives, even restricting 'possible' to 'conceivable', when we do not know how many unconceived alternatives there are, let alone what they look like?

Moreover, there appears to be evidence from the history of science that T may be less virtuous than T^*, in key respects, even when T is more verisimilar than T^*. (That is, granting that the central theories in some domains *have* increased in verisimilitude.) For example, the two-spheres model of the universe – where Earth is stationary at the centre, and the sphere of stars rotates around it – is much simpler and easier to use, for navigation at night on the sea, than any truer model that I am aware of. Similarly, it is much easier to make everyday predictions in mechanics by disregarding relativistic considerations – and even by positing non-existent forces, such as the centrifugal, Euler, and Coriolis forces – than it is to take them into account. So why should we expect such results *not* to occur in the special case where T happens to be true, as well as more verisimilar than T^*? Why not say as follows? Truth is sometimes stranger – more complex, more unaccommodating, less elegant, less comprehensible – than fiction.

In fact, Niiniluoto (2002, pp. 183–184) seems to agree with this statement – and, moreover, that there is no a priori case to the contrary – in so far as *simplicity* is concerned (at least):

> [*S*]*implex sigillum veri*. This is a highly dubious metaphysical doctrine: why on earth should nature be simple? Why should there be any a priori reasons to regard simpler theories as more probable than their rivals?

We should conclude that acquiring theories with greater verisimilitude is not necessary for progress, even if it is sufficient for progress. (In fact, I doubt it is even sufficient for progress. But I will save that argument, which is more intricate, for a different occasion.[8]) Niiniluoto's account is too narrow. It disregards the significance of increasing predictive power and understanding (or inappropriately assumes that these only come on the coat tails of true, or approximately true, theories).

In passing, before I continue, I should also mention my sympathy with the recent argument of Mizrahi (2014), to the effect that increasing *know how* – which, like Ryle, I take to be distinct from propositional knowledge[9] – is an additional means by

[8] See Rowbottom (In Progress), where I develop my account of progress. For now, I would draw attention to the following passage, which has been missed by some commenting on my position (Rowbottom 2010, p. 245):

> I am not presupposing that science progresses by producing true or truer *theories*… the semantic view of scientific progress… only requires that science makes progress by discovering new truths… Such truths could take the form of 'T1 is approximately empirically adequate in the class of circumstances C', or even 'T2 is false', and so on. As such, I do not take the semantic view of progress [as Bird defines it] to presuppose scientific realism, or even structural or entity realism… *I take it to be neutral on the realism issue.*

[9] For recent criticism of this view, see Stanley and Williamson (2001) and Stanley (2011). See also Bengson and Moffett (2011).

which science can make progress.[10] I just think truth – especially with respect to claims about observables – is rather more central, although I won't be able to argue for that here.

2. Increasing Verisimilitude Vs. Increasing Predictive Power and Understanding

As Niiniluoto (2011) and I agree, recall, 'there is no reason to assume that the goal of science is one-dimensional'. Thus, one might hold that increasing verisimilitude is the *primary (or central)* dimension of scientific progress, while accepting that there are other unrelated dimensions in which it can progress (even when maximal verisimilitude is achieved). And one might respond to the scenario described in the previous section by judging that further progress could be made, while denying that such forms of progress are as important, or valuable, as increasing verisimilitude (when that's also possible).[11] So in summary, to grant that increasing verisimilitude is *unnecessary* for making scientific progress is not to deny that increasing verisimilitude is the primary means of making scientific progress.

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[10] I disagree with Mizrahi (2014), however, that we should relate accounts of scientific progress closely to the aims of scientists. As I argue in Rowbottom 2014, the aims of scientists bear no interesting relationship to what has been misleadingly called 'the aim of science'.

[11] I suspect that Niiniluoto would find this option attractive, on the basis of the following passage:

> In my view, the axiology of science should… be governed by a primary rule: try to find the complete true answer to your cognitive problem, i.e. try to reach or approach this goal. Truthlikeness measures how close we come to this goal. As secondary rules, we may then require that our answer is justified, simple, consilient, etc. (Niiniluoto 2002, p. 174)

My response to this subtle move is to proffer another thought experiment, and elicit a judgement on the proper conduct of the scientists therein. Brace yourself again.

Imagine that all the experts in a specific area of chemistry gather together to discuss how to direct future research. They unanimously agree – and are correct (in virtue of knowing, or believing justifiably, if you so wish) – that their two best strategic options will lead to two mutually exclusive outcomes. If they take the first, they will maximize their predictive power concerning, and understanding of, the phenomena with which their branch of chemistry is concerned. If they opt for the second, they will discover the true unified theory in that domain. Their dilemma arises because their resources are limited. Pursuing all the goals simultaneously would result in extremely limited progress in achieving each. Unfortunately, moreover, pursuing the truth will result in limited incidental progress toward maximising predictive power and understanding, and vice versa.

After a heated discussion, the scientists unanimously agree that they should forego the chance to find the true theory of everything in their domain. Instead, they opt to pursue greater predictive power and understanding. Have they made the wrong decision, in some significant sense? Have they failed to do what ideal scientists would do, in such circumstances?

I contend that it is reasonable to answer these questions in the negative, even if it is assumed that the scientists have no pressing practical need to achieve greater predictive power or understanding. And note that this is not to presume that the scientists take the *correct* course of action, in pursuing predictive power and understanding. It suffices for each option to be as good, as scientific, as the other.[12]

Why do I approve of their decision, and think that it is at least as good as the alternative? Grant, for the sake of argument, that finding the truth (about the unobservable, *inter alia*) has considerable intrinsic value.[13] First, why deny that achieving understanding of how phenomena interrelate has similar value? I expect that Niiniluoto would *not* deny this, but would instead insist that truth is a necessary condition for understanding. This is suggested by the following passage (if we assume that understanding and explanation are appropriately related):

> The realist of course appreciates empirical success like the empiricist… But for the realist, the truth of a theory is a precondition for the adequacy of scientific explanations. (Niiniluoto 2002, p. 167)

I take Elgin (2007) to have argued convincingly that understanding is non-factive, but will not rehearse her arguments here; that would be another paper. Instead, suffice it to say that differences of opinion on this matter are crucial. For if understanding requires true theories, then the thought experiment involves an impossible scenario. Understanding and truth would *never* come apart, as I contend that they can.

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[12] I think that the scientists *did* take the correct option. I shall not argue for this view here, though, since my primary target is Niiniluoto's account, and closely aligned possible accounts, of scientific progress. See Rowbottom (In Progress) for a defence of (axiological, as well as semantic) instrumentalism.

[13] I hold that the value is purely instrumental, but consider the assumption of intrinsic value to be a generous concession (to many realists) in this context. It also streamlines the discussion.

Second, even granting that understanding is factive, why deny that achieving great predictive power has value equal to that of finding true theories (with high scope)?[14] After all, this involves acquiring a means by which to *derive many truths* concerning observables. And as I have already argued in the previous section, acquiring the true theory in a domain does not entail acquiring the theory that will be the most predictively useful, or indeed a theory that will be of any predictive use whatsoever, in said domain. So I will rest my case on this issue.

I take the following to be a reasonable stance. Achieving maximal verisimilitude is *no more central* to scientific progress than achieving maximal predictive power and understanding. Or to put it differently, the *primary* aspect of scientific progress should not be 'defined [purely] by increasing verisimilitude' (Niiniluoto 2014, p. 77).

3. On Progressive Unjustified Scientific Beliefs

I will now move on to resisting the criticism that Niiniliuoto offers of my main argument – presented in Rowbottom 2010 – against Bird's (2007, 2008) knowledge-based view of scientific progress. This is as follows:

> Rowbottom… argues that… "unjustified scientific beliefs may be progressive" by defending the semantic view by another thought experiment. Imagine two equally advanced planets with similar theories and technologies,

[14] The thought experiment can be adjusted so that it concerns only predictive power versus truth, if desired.

> one with true beliefs without justification, the other with the same beliefs but with justification…

> [T]he primary application of the notion of scientific progress concerns successive theories which have been accepted by the scientific community. Some sort of tentative justification for such theories is presupposed (even by a radical fallibilist like Popper). Irrational beliefs and beliefs without any justification simply do not belong to the scope of *scientific* progress. (Niiniluoto 2014, p. 76)

However, it appears that Niiniluoto and I use 'justification' in different senses; I think my sense is stronger than his, and is more influenced by contemporary epistemology. And I say this on the basis of his comment about Popper. For Popper (1983, p. 259) writes, of science:

> As to its authority, or confirmation, or probability, I believe that it is nil; it is all guesswork, *doxa* rather than *epistēmē*.

Now whereas Niiniluoto takes Popper's position to be compatible with 'some sort of tentative justification for theories' existing, most contemporary epistemologists would take the previous quotation to entail that the content of science is *devoid* of justification, as do I.[15] And that's because it denies that the content of science is

[15] Strictly speaking, talk of 'justification of theories' should be distinguished from talk of 'justification of belief in theories'. It is important to distinguish between the content of a belief, and the believing itself, in order to understand critical rationalism. See Musgrave (1999, pp. 319–325).

probable (or confirmed). In support of this claim about epistemology, consider the words of Engel (2011):

> Fallibilists [about justification]… contend that the kind of justification requisite for knowledge need only *render probable*, but need not entail, that for which it is justification … [M]ost contemporary epistemologists have embraced fallibilism so that empirical knowledge remains at least in principle possible. Fallibilistic justification is thought to rule out epistemic luck by making one's belief *extremely probable*. [Emphasis mine]

Now consider Popper's view on scientific theories, by way of comparison. Popper (1959, Appendix *vii) argued that the (logical) probability of any universal hypothesis is zero relative to any finite set of observation statements. It follows – on the additional assumption that there is no way other than observation by which to raise the probability of such theories – that they never possess 'fallibilistic justification' in the above sense.[16]

However, this is *not* to deny that scientists operate with reasons. It is, as critical rationalists say, to deny that they operate with good reasons, rather than critical

[16] I could go into much more detail, and be much more precise, here – e.g., explain that the probabilities in question are not relevant if they are understood as subjective (or indeed aleatory) rather than logical in character, that statistical hypotheses will be in similar trouble when their domain is infinite, and so on – but I have already covered this elsewhere. See Rowbottom 2011a and Rowbottom 2013.

reasons (at the very least, when it comes to comparing theories).[17] It might be added that to deny the presence of justification in science is not to deny the presence of: (a) the *appearance* of justification (*qua* confirmation); or (b) community methods and even (Kuhnian) disciplinary matrices. And I did not want to suggest that we imagine a scenario, on either planet, where any of these aspects of science were not present.

My presentation of the 'two planets' thought experiment treatment was also highly sensitive to which account of justification one prefers, and may be construed as comparative in character.[18] Against non-reliabilists (e.g., internalists), for example, the thought experiment may be understood such that reliable methods are nevertheless employed on the planet where justification is absent. Reliable (and stable) movement towards the truth might be made on said planet, as a result. Unfortunately, the fine details of Niiniluoto's preferred account of justification – internalist or externalist, foundationalist or coherentist, and so on – are not clear to me. So I cannot presently devise a version of the thought experiment that targets this.

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[17] Several of Popper's ex-students compare 'good reasons' with 'critical reasons'. See, in particular, Bartley (1984) and Miller (1994). See also Popper (1983, pp. 19–20):

> [W]e cannot give any positive justification or any positive reason for our theories and our beliefs. That is to say, we cannot give any positive reason for holding our theories to be true… We can often give reasons for regarding one theory as preferable to another. They consist in pointing out that, and how, one theory has hitherto withstood criticism better than another. I will call such reasons *critical reasons*… Giving reasons for one's preference can of course be *called* a justification (in ordinary language). But it is not justification in the sense criticized here.

[18] Rowbottom (2010) covers several such accounts, including the one preferred by Bird (2007, 2008) and Williamson (2000).

At this juncture, I might have appealed to the experiments of Mizrahi and Buckwalter (2014), on ordinary conceptions of scientific progress. Many subjects therein judged that progress sometimes occurs in the absence of justification (although they also judged that the *addition* of justification would result in more progress). And some readers may take this as evidence that progress is possible in the absence of justification. However, I believe that the results of thought experiments are theory-laden, and that experts have many shared theories about science – based on awareness of its content, social structure, and history, *inter alia* – that non-experts lack. Like Sorensen (2014), I therefore hold that folk responses to philosophical thought experiments are of no more significance to philosophy than folk responses to physical thought experiments are to physics.

I will make one final point in this section. The thought experiment criticized by Niiniluoto can be modified to involve a planet with justified scientific beliefs falling short of knowledge (e.g., due to Gettier considerations) versus a planet with scientific knowledge. I noted this in the following passage, which Niiniluoto (2014) does not mention:

> I have made my task considerably more difficult than necessary, if the discussion is to be restricted to Williamson's view of knowledge (which Bird supports). For on this view, it is possible to have justified true beliefs without knowledge and therefore the comparison might be drawn between planets inhabited by people with justified true scientific beliefs and people with scientific knowledge… (Rowbottom 2010, p. 242)

This version of the thought experiment is potentially effective against any version of the epistemic view of scientific progress, provided that justified true belief does not entail knowledge. Niiniluoto (2014) presents no objection to it.

4. On False Beliefs Promoting Progress

Briefly, in a footnote, Niiniluoto (2014, p. 76, f.9) also claims that: 'Rowbottom's other thesis that "false beliefs may promote progress" can be explicated by the notion of approximate truth.' But I am dubious that any such explication is required.

Perhaps Niiniluoto has in mind that false beliefs which promote progress must be approximately true (or closer to the truth than their predecessors)? I grant that *some* (hypothetical or actual) situations where false beliefs promote progress can be explained in such a way. For example, Kepler's laws were plausibly more truth-like than any earlier theories concerning planetary trajectories, and belief in those laws was partly responsible for movement away from long-entrenched metaphysical views about (the unchanging, perfect, circular nature of) celestial motion that impeded progress in astronomy. However, as I will show below, flatly false beliefs can also promote progress.

But maybe Niiniluoto thinks instead that adding a (totally) false statement to a theory T may result in a new theory, T^*, which is more verisimilar than T? Again, I grant the point. But I do not see how this provides a *general explication* of why "false beliefs may promote progress". That's because there are many other ways in which false beliefs can promote progress.

First, consider how false community belief that a true theory is well-confirmed can promote progress. Imagine a leading scientist lies about performing an experiment to test the theory (because he lacks funds to do it), but if he *had* performed the experiment, he would have got (approximately) the same results as those that he fabricated. The lies, and the resultant false beliefs in the scientist's testimony, promote community belief in the theory. And then experiments to test the theory are performed, much sooner than they otherwise would have been. The theory is genuinely corroborated as a result! (And you are welcome to imagine this results in *knowledge* that the theory is genuinely corroborated, and thereby of the theory. Then the example tells against Bird's (2007, p. 84) extraordinary view that: 'developments that promote knowledge will themselves be knowledge'.)

Second, consider a false theory that is widely accepted to be true, because it has been employed repeatedly to make successful predictions. Eventually, it is used to make a false prediction. But that prediction is false *only* because of the assumption of, due to community belief in, initial conditions that do not obtain. The theory is then *correctly* classified as false, as a result. Isn't that progress promoted by a false belief?[19] If Neptune had not been located on the basis of Le Verrier's calculations, for instance, then it might have been realized earlier than it was that Newtonian mechanics is false. The point here rests on Duhem's thesis, that theories cannot be tested in isolation. The

[19] Note that the false belief, here, might involve another theory used to calculate the initial conditions. So here, a false belief in a theory may be directly responsible for progress.

prediction of the orbit of Uranus, which was found to be incorrect, employed Newton's laws of motion and gravitation, along with data on the masses and positions of the planets in the solar system. But the data on masses and positions of the planets in the solar system may not have been approximately true, in so far as Neptune was not included.

There are several other ways in which false (and not approximately true) beliefs can promote progress: belief that two theories are true or approximately so, when one is (or both are) not, may encourage scientists to conjoin the theories, and conjoining them may be useful for many predictive purposes; belief that an experiment was poorly performed, when it was not, may encourage a scientist to repeat it, and the circumstances (i.e., variables not controlled for) may change in such a way as to make the experiment give a different result, thereby promoting useful theory revisions; and so on. But I shall not overegg the pudding by providing a long list. I rest my case.

5. Conclusion

First, scientific progress is possible in the absence of increasing verisimilitude in science's theories. Second, central aspects of scientific progress do not involve science's theories increasing in verisimilitude. Third, unjustified changes in scientific belief may be progressive. And fourth, false beliefs can promote scientific progress in ways that cannot be explicated by appeal to verisimilitude.

Acknowledgements

Special thanks to two anonymous referees, who provided many useful criticisms and comments on an earlier version of this paper; it is considerably stronger as a result of their efforts. Thanks also to attendees of the CHESS and Philosophy department seminars at the University of Durham, and Nancy Cartwright, Robin Hendry, Rune Nyrup and William Peden in particular. This research was supported by: Hong Kong's University Grants Committee ("The Instrument of Science", Humanities and Social Sciences Prestigious Fellowship); and also Durham University's Institute of Advanced Study in association with the European Union (COFUND Senior Research Fellowship).

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