Don't we all believe in scientific facts? Replies to my critics

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I am grateful to David Harker, Inmaculada de Melo-Martin, and Nicola Mößner, for insightful thoughts and criticisms of my book *Identifying Future-Proof Science* (IFPS).

I wish to start my response with an overarching comment that touches on all three reviews. Consider the question: Don't we all believe in scientific facts? As de Melo-Martin says, "I […] have little quandaries with Vickers' contention that some of our current scientific claims are future proof." My impression from the reviews is that all three reviewers do accept the modest idea that there are at least *some* scientific claims that deserve to be called 'established scientific facts'. An example from Ch.1 of IFPS is 'Smoking causes cancer'. Whilst there are breath-taking complexities if one enquires into the details of the causal link between smoking and cancer – and open questions remain concerning many of these details – the basic fact that smoking does cause cancer was concretely established during the 20th century.

Nothing in the three reviews contradicts this basic idea. And perhaps the reader will think it is an obvious and banal point. But it is worth stressing, not least because it contrasts sharply with the Popperian view that there is no such thing as confirmation, the Kuhnian view of paradigm shifts, and the 'pessimistic induction' idea of ruins piled upon ruins. Nor is it the case that my three reviewers are a biased sample. In my plenary address at the 2023 conference of the British Society for the Philosophy of Science (BSPS) I surveyed the audience regarding the statement "Science has put it beyond reasonable doubt that COVID-19 is caused by a virus", and over 95% agreed. Needless to say, philosophers are not well-known for reaching a consensus, on any matter. And this isn't because 'realism has won'; many of those in the audience that day do *not* regard themselves as 'scientific realists'. As argued in Ch.2 of IFPS, the topic of 'scientific facts' has surprisingly little to do with the scientific realism debate.

If I'm right that nearly all of us believe in (at least some) 'established scientific facts', it is remarkable that philosophers of science have neglected this topic for so long. It also means that the account put forward in IFPS has to be considered a 'first try'. And we do need an account. The vast majority of us believe that 'Smoking causes cancer' transitioned, during the 20th century, from a 'hypothesis' to an 'established scientific fact'. But when did that happen? When could it be said, uncontroversially, that 'Smoking causes cancer' is settled science? Are there necessary conditions for scientific facts? Sufficient conditions? When it comes to answering these sorts of questions, the major 'science and truth' debate in philosophy of science, the scientific realism debate, is surprisingly unhelpful.

These preliminary remarks set the stage for directly tackling three of the reviewer's concerns: (i) the claim that some of my best examples of future-proof science are problematic, (ii) the worry that my account leans heavily on the concept of 'approximate truth', but lacks a theory of this notion, and (iii) the contention that we don't need an account of future-proof science. I take these in turn, before turning to the remaining issues that were touched on by more than one reviewer.

First the concern that some of my examples are problematic, most forcefully put forward by de Melo-Martin. Which examples are singled out, to substantiate this worry? Certainly none of the 30 examples of 'singular facts' listed in Ch.1 of IFPS. Instead de Melo-Martin's examples are, "knowledge of the properties and behaviours of various different types of cancer" and "knowledge of numerous illnesses and diseases, including Parkinson's, diabetes, epilepsy, HIV/AIDS, Huntington's, spina bifida, etc". But

these are intended as topic spaces within which many 'singular facts' can be found. Given that they are topic spaces, they are not even candidates for meeting my criteria. The singular facts IFPS picks out from these two topic spaces are (a) 'Smoking causes cancer', and (b) 'Human immunodeficiency viruses (HIV) kill immune system cells (T helper cells)'.

There is a wider point to make here. De Melo-Martin is absolutely right to stress that, "[t]here is a significant amount of debate in the relevant scientific community about what exactly cancer is and about how to classify different types of cancer", and also, "it is hard to see how much current knowledge about many of these diseases [Parkinson's, etc] could meet Vickers' criteria." But actually, there is no disagreement here. The many open questions about cancer should not make us doubt whether smoking causes cancer, and the many open questions about HIV should not make us doubt that we know *some things* about these viruses and their interactions with immune system cells.

Extreme care is needed in these kinds of exchanges. Previously I reviewed *Resisting Scientific Realism* (Wray 2018) stressing that we do know some things about the inner and outer core of the Earth (Vickers 2020). The response (Wray 2020) stressed some recent developments in the science of these two 'cores', emphasising the extent of our uncertainty. But my claim that we know *some* things is compatible with new developments, and even big surprises. So too, there could be big surprises in store for our understanding of HIV, but this is compatible with the singular fact I carefully pick out. We should not think we are disagreeing when one scholar wishes to emphasise what we know, and another scholar prefers to emphasise what we don't know.

This point is more serious than it might first appear. As argued in Ch.2 of IFPS, even renowned 'non-realists' such as Stanford and Van Fraassen believe in established scientific facts such as 'Smoking causes cancer'. Van Fraassen spent his career emphasising what we do *not* know, or can't reasonably be said to know – this hardly rules out his believing that we do know lots of things, especially things concerning 'observables' broadly construed. Looking at the two of us, the difference in emphasis is dramatic; the degree of disagreement needn't be.

Harker picks on "evolutionary biology" and "anthropogenic climate change" as two of my examples where we should probably avoid the term 'established fact'. But if we turn to the 30 examples of *singular facts* stated in Ch.1 of IFPS, we find neither of these examples. Instead we find the following: (a) 'Human beings evolved from apes that lived on Earth several million years ago', and (b) 'The concentration of carbon dioxide in the Earth's atmosphere in the year 2020 was the highest it has been in three million years'. It says something about my 30 examples of 'singular facts' that no reviewer picks on *these* as problematic.

This brings us to (ii), Harker's concerns about the role of *approximate truth* in IFPS. He writes, "My biggest worry, however, concerns the possibility of future changes. […] Vickers does not offer a theory of approximate truth." The suggestion seems to be that we can't know what we mean when we say "Smoking causes cancer is an established scientific fact", unless we have a theory of approximate truth to substantiate just which revisions to 'smoking causes cancer' would be allowable without destroying the claim. But whilst the concept *approximate truth* is put front-and-centre in Harker's review, there is a reason it takes a back-seat in IFPS. In short, we can't possibly let a theory of scientific facts *depend on* a theory of approximate truth. Philosophers have long debated the concept *approximate truth*, without agreement. The literature is a mess; it appears to be one of those philosopher's dead-ends, where we can't expect a consensus to form any time soon (Oddie and Cevolani 2022). Think now of the audience at my BSPS lecture, where I surveyed regarding whether science has put it beyond reasonable doubt that COVID-19 is caused by a virus. The vast majority felt able to answer 'strongly agree', apparently without needing recourse to any theory of approximate truth.

Harker doesn't apply his concerns about approximate truth to any of the 30 singular facts listed in Ch.1 of IFPS. See what happens when we attempt to do so. Instead of saying "It's an established scientific fact that smoking causes cancer", we say, " 'Smoking causes cancer' is approximately true". Whilst I feel I know what the former means, I'm not sure I know what the latter means. If we want to bring truth into it at all, it seems reasonable to insist that 'smoking causes cancer' will remain *plain true*, regardless of any (feasible) scientific developments that may be waiting around the corner. The same goes for 'Human beings evolved from apes that lived on Earth several million years ago'. The key is this: one can carefully craft statements that are flexible enough to withstand any (feasible) future adjustments. Precisely how smoking causes cancer may change, without affecting 'smoking causes cancer'. And in the statement 'Human beings evolved from apes that lived on Earth several million years ago', we remain neutral on all kinds of underlying details. Thus, in the end, I want to say that the 30 'singular' examples in Ch.1 of IFPS are 'established scientific facts', avoiding *approximate truth*. But this is certainly an area where I welcome further dialogue, and revisions to the letter of IFPS may be called for. (I hope not the spirit.)

Turning to (iii), the suggestion that we don't need an account of 'facts' or 'future-proof science', de Melo-Martin asks, "What is it that certainty provides in practical terms that being *pretty sure* does not? […] I just wonder whether certainty is a bit overrated.". Harker seems to agree, stating, "[r]ather than regard these as established facts, we should regard our understanding of these issues as involving a high degree of confidence". I agree that in many contexts it is enough that we are 'very sure', or even 'quite sure'; if a government needs to act urgently, a policy-decision may depend on what our current best bet is. But are there really *no* contexts where an account of 'established scientific facts' would be useful? This seems implausible, especially supposing that many of us actually believe in them.

For example, it is clear that many *scientists* believe in established scientific facts. Occasionally, a scientist is shocked to learn that philosophers of science are yet to produce any account of them. As Hoyningen-Huene writes of the renowned evolutionary biologist Ernst Mayr, "Mayr often deplored that he was not aware that philosophers of science have investigated this transition from theory to fact." (Hoyningen-Huene 2022, p. 64, fn.23). Consider Mayr lecturing students on evolution and zoology at Harvard in the 1960s and 70s. From his perspective – as numerous other scientists, then and now – most of the 'textbook' information his students were asked to digest was what you might call *established knowledge*, often very hard-won over decades of scientific labour. For many scientists, it would seem bizarre to entirely restrict themselves to terms such as 'high confidence' for that body of textbook knowledge.

To provide a recent example, one author of an IPCC Special Report recently asked, 'Where is the boundary between "established fact" and "very high confidence"?' (Janzwood 2020, p. 1668). This affects IPCC authors, since they are expected to indicate their degree of confidence for statements in their reports, as in, "The ocean has absorbed about 30% of the anthropogenic carbon dioxide, resulting in ocean acidification and changes to carbonate chemistry that are unprecedented for at least the last 65 million years (high confidence)." But if the statement expresses an *established fact*, it can be stated without such a qualifier. Some statements in IPCC reports do *not* include such a qualifier, because the authors agree that it is an established fact. But they come to this agreement without any account to guide them; it is more of a shared intuition.

Not everyone working in science and philosophy will agree that 'established scientific facts' exist, or that we need an account of them. But some will. So too, not everyone believes in *scientific progress*, but a quite astonishing number of books and articles have been written on the concept (Losee 2004;

Niiniluoto 2019). If one were to go by the literature alone, one would think that the concept *scientific progress* is supremely important, and the concept *scientific fact* is entirely unimportant.

Of the remaining concerns, I take it that my diversity condition is the most important to tackle: *The relevant scientific community must incorporate a substantial diversity of perspectives*. All three reviewers worry about this. Mößner asks, "When exactly is this condition fulfilled?". Harker worries that my account requires "precise measures" of consensus and diversity.

But actually the diversity condition is doing less work in the account than the reviewers tend to suppose. At the heart of IFPS is this: the criteria are meant to be *sufficient* (not necessary). Thus the question of when, exactly, a community is diverse enough simply doesn't arise. At the same time, we can use 'common sense' to identify communities where the diversity of perspectives is *insufficient* for us to have confidence that a consensus within that group carries epistemic significance. A good example is the flat earth community, where a consensus exists that the earth is flat, but a basic 'diversity of perspectives' requirement is obviously not met. De Melo-Martin raises concerns about my use of 'common sense' when it comes to the diversity requirement. Whilst I agree that 'common sense' is a notion that cannot withstand much dialectical weight, I would insist that my account puts very little weight on it.

More contentiously, I claim in the book that 19th century classical physics doesn't count as a counterexample to my account because the community at the time didn't meet my diversity condition. I claim this for various reasons. As is well known, the community was at least 99% male. But more than this, it lacked diversity in every respect: it was comprised of a small number of wealthy white men from a small cluster of Western countries. This couldn't reasonably be described as a 'substantial diversity of perspectives', or at least, I am free to stipulate that the degree of diversity in the 19th century physics community doesn't meet my requirements. I can choose to raise the bar high: I don't need every candidate scientific fact in science to meet my requirements. My claim is merely that the ones that *do* meet the criteria are future-proof.

What of counterfactual claims, such as Harker's thought that scientific community diversity relevant to continental drift *could* have been different – an apparently nearby possible world where my criteria *were* met regarding the false claim 'Continents do not drift'? Similarly, one may worry that if the 19th century physics community *had* been large and diverse, we would still have had a solid 95% consensus, in which case my criteria would have been met for false 'classical' claims.

Whilst these thought experiments are interesting to explore, it remains the case that there has never been an example in the entire history of (actual) science where my criteria were met and then the claim in question ended up being false, despite enormous opportunity for that to happen (were it ever going to happen). In the case of 19th century classical physics, I don't consider it obvious that a large, diverse international scientific community would have reached a 95% consensus regarding relevant claims that might be picked out. Importantly, for a specific claim, I require that, "If prompted, they [scientists] would be willing to call it an 'established scientific fact'." This feature of the criteria sets the bar extra-high, and on this specific point I submit that a more diverse 19th century physics community would have been more cautious. Empirical studies indicate that men tend to be problematically incautious in certain ways, to a greater extent than women (Barber and Odean 2001; Hardies et al. 2013). If there is some truth in this, then a counterfactual 19th century physics community with good gender diversity might have been considerably more cautious than the actual 19th century physics community.

Another possible worry about the diversity condition is nicely expressed by de Melo-Martin. She writes, “Determining whether a particular scientific theory or claim is future proof requires a 95% scientific consensus by a scientific community that is *sufficiently diverse*. How do we determine what constitutes a sufficiently diverse scientific community? We look at scientific claims that are future proof.” But we can break this circle: it’s not the case that our *only* access to future-proof science is via a ‘sufficient diversity’ criterion, and our *only* access to ‘sufficient diversity’ is via scientific claims we know to be future proof. In particular, there are many claims, such as ‘smoking causes cancer’ and ‘continents drift’ that are now far beyond reasonable doubt. For ‘continents drift’ in particular, we don’t need to enquire about the measure of diversity of the relevant scientific community – we can watch them drift using GPS satellites. We can use examples such as this to get some handle on how much diversity is needed in a scientific community *for the specific purpose of being able to trust a solid scientific consensus that arises in that community*.

But the core claims of IFPS only depend on simpler ideas, such as that the measure of diversity in the 19th century physics community is *not* enough for a consensus to be reliable, and the measure of diversity in modern scientific communities *is* sufficient for a (very strong) consensus to be reliable.

Finally – since all three reviewers raise this issue – I must tackle the question of how we know when the criteria for future-proof science have been met, especially how we judge whether or not the scientific consensus has gone past 95%. I am sympathetic with these worries. This is why I include a section in Ch.9 ‘Implications for School Education’, where I argue that skills needed for ascertaining whether or not scientists have reached a (strong) consensus are woefully lacking, and should be taught to children of all ages. In addition, I think we need a dedicated ‘Institute for Ascertaining Scientific Consensus’, whose job it would be to survey scientists on particular statements of interest, providing the world with rich data on scientific community opinion. I am currently trying to set up such an institute (Adam 2023).

But in the meantime, IFPS suggests some basic ‘rules of thumb’ that anyone can use to judge whether there is a (strong) consensus. One of these is to check whether there is any debate about the issue at the relevant academic conferences, where the experts meet; if the matter is considered beyond reasonable doubt, there won’t be any debate about it. If experts are split, there will be at least some debate. Mößner writes, “the author does not clarify how a layperson could identify what the “relevant” conferences within a certain academic field might be”. But actually it is now extremely easy to find out. For example, I just now asked GPT4, "What are the key academic conferences where one might expect to find discussion and debate concerning the extinction of the non-avian dinosaurs?" This wasn’t my approach when I wrote on this topic in Ch.7 of the book (GPT didn’t exist then!), but the results from GPT4 tally perfectly with the conferences I did in fact look at during my own research. So this is a reliable approach, and it also works for the question “What are the relevant journals?” Moreover, GPT4 is excellent at telling you when a journal is a respected peer-reviewed scientific journal, and when it is a problematically biased journal (or even when a journal is somehow in-between these two extremes). In sum, laypersons have a good way of finding such information in a few seconds.

Whilst careful use of GPT does help, often it remains challenging to ascertain whether some statement of interest enjoys a consensus amongst relevant experts above 95%. You can’t ask GPT4 about the extent of scientific community discussion on the topic at relevant conferences, nor what the result of a mass survey of the relevant experts would reveal. But we can imagine a future world where people can make very informed judgements. This is a world where (i) a significant part of the school curriculum is devoted to educating the next generation about ‘how science works’, so that they leave school with much richer conceptions of expertise, consensus, and scientific community dynamics, and

(ii) we have a dedicated 'Institute for Ascertaining Scientific Consensus' providing rich data relevant to judging the extent of scientific agreement on various important topics.

Unfortunately, with apologies, I have not found space to attend to all of the three reviewers' many excellent comments. I hope the opportunity will arise for further dialogue, beyond this symposium.

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Citation on deposit: Vickers, P. (2023). Don't we all believe in scientific facts? Replies to my critics. Metascience, https://doi.org/10.1007/s11016-023-00926-w

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