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The example of the IPCC does not vindicate the Value Free Ideal: a reply to Gregor Betz

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Abstract In a recent paper, Gregor Betz has defended the value-free ideal: "the justification of scientific findings should not be based on non-epistemic (e.g. moral or political) values"against the methodological critique, by reference to the work of the International Panel on Climate Change (IPCC). This paper argues that Betz's defence is unsuccessful. First, Betz's argument is sketched, and it is shown that the IPCC does not avoid the need to "translate" claims. In Section 2, it is argued that Betz mischaracterises the force of the methodological critique. Section 3 shows why the methodological critique still applies to the work of the IPCC even on a refined version of Betz's argument. Section 4 then considers an alternative way of defending the work of the IPCC which is in-line with, but does not clearly vindicate, the value-free ideal.

Keywords Inductive risk · Value free ideal for science · International Panel on Climate Change · Climate science · Epistemic values · Knowledge

The value-free ideal (VFI) for science states that: "the justification of scientific findings should not be based on non-epistemic (e.g. moral or political) values" (Betz 2013, p.207).[1] In recent years, many philosophers have resuscitated a powerful argument originally made by Richard Rudner against the feasibility and/or desirability of excluding non-epistemic values from scientific justification (Douglas, 2009; Elliott, 2011; Steele, 2012). In this journal, Gregor Betz has responded to this "methodological critique" – also known as the "argument from inductive risk" and the "argument from transient under-determination" (Biddle, 2013) – both at a philosophical level and, more concretely, by suggesting that the work of the International Panel on Climate Change (IPCC) might exemplify value-free science. Assessing these claims is extremely important. As Betz argues, an attractive democratic principle holds that the people, rather than experts, should decide which non-epistemic values should guide policy. If

[1] All subsequent page number references are to this paper unless otherwise stated.

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scientists' findings must appeal to their non-epistemic values, this principle seems threatened (p.207). Furthermore, the specific question of whether the IPCC's work is value-free is of immediate political significance. A recurrent theme in political debate is that climate science is somehow value-laden, and that this is problematic (Coady and Corry 2013). Given the stakes involved in these arguments a key question for a socially-relevant philosophy of science is whether the work of the world's leading authority in climate science is or must be value-laden.

I agree with Betz that challenges to the epistemic purity of science conflict with important non-epistemic concerns, and think it is extremely important to understand whether the IPCC's work is (problematically) value-laden. However, after clarifying the background to debate in §1, in this paper I argue that Betz mischaracterises the force of the methodological critique (§2) and that the IPCC's work does not avoid this critique (§3). In §4, however, I suggest that we can learn important lessons from Betz to construct an alternative account of how the IPCC's work might be "value free", although it is unclear that this possibility fully blunts the methodological critique.

1 Betz on the methodological critique and the IPCC: lost in translation

There are many ways in which we might seek to undermine or to defend the VFI. However, perhaps the most potent attack in recent years focuses on the problem of inductive risk. According to Betz, this "methodological critique" of the VFI runs as follows.To arrive at policy-relevant results, scientists must accept or reject "plain hypotheses" (i.e. claims like "all A's are B's") (p.212). These decisions are typically made under situations of uncertainty (i.e. "when the empirical evidence or the theoretical understanding of a system is limited", p.211). Adoption of such claims therefore always involves a substantive risk of error. According to the methodological critique, scientists must appeal to non-epistemic value-considerations to decide when errors are tolerable; hence, scientific justification cannot be value-free.[2] One important contribution Betz makes is to distinguish two forms of this "must": logical or moral. However, whether logical or moral, the argument seems to depend on the premise that scientists must accept or reject plain hypotheses. Betz denies this. Instead, building on work by Richard Jeffrey (1956), he argues that scientists can adopt "hedged hypotheses", claims like "it is very probable that all A's are B's" (p.213). Such claims can be confirmed beyond reasonable doubt by the available evidence (p.214), i.e. adopting hedged hypotheses does not involve a significant risk of error. Therefore, the methodological critique do not show that scientists must (in any sense) appeal to non-epistemic values in adopting hypotheses. For Betz, "the methodological critique of the value free ideal is ill-founded" (p.218).

Betz seeks to undermine the conclusion that scientists *must* (in some sense) appeal to non-epistemic values by denying that they *must* adopt outright plain hypotheses. Why, though, think that scientists *can* adopt "hedged", rather than "plain", hypotheses? One line of argument appeals to common sense considerations. However, Betz also makes a

[2] An important aspect of Betz's article is that he very clearly distinguishes between logical and moral versions of the methodological critique, (pp.209–211): I agree completely with everything he says on this topic, so say nothing more on it in this paper.

more concrete suggestion. At (roughly) 5-yearly intervals, the IPCC produces Assessment Reports, which are intended to provide policy-makers with a summary and synthesis of "the state of scientific, technical and socio-economic knowledge on climate change, its causes, potential impacts and response strategies" (IPCC, 2013a). As Betz claims, these reports typically report "hedged hypotheses" about the likelihood of various climactic effects, the likely success of different interventions, and so on (pp216-218). Betz is clear that whether the IPCC does in fact succeed in avoiding the methodological critique is contestable, because both "the guiding framework and the actual practice might be improved upon" (p.218). However, he suggests that even if actual practice falls short, these problems are surmountable. As such, the example of the IPCC "shows forcefully how scientists can articulate results as a function of the current state of understanding and thereby avoid arbitrary (methodological) choices" (p.218). The example of the IPCC strengthens Betz's claim that it is false that scientists' *must* adopt plain hypotheses, and is all the more compelling given the political storms which rage around climate research. However, Betz's example is double-edged: if we can show that even scientists who followed an improved version of the IPCC's guidelines would be open to the methodological critique, then we have reason to doubt that this critique is avoidable. I shall now sketch one such concern, suggested by Katie Steele (2012). Although Steele's concerns are not insuperable, thinking through them helps clarify later discussion.

Steele concedes that if scientists can limit themselves to reporting probablistic claims, then they need not make non-epistemic value-judgments in justificatory contexts. However, she notes that even if climate scientists can assign epistemic probabilities to hypotheses, these are not what the IPCC demands they report. Rather, the IPCC's "Guidance Notes" require scientists to translate these probabilities into coarse-grained qualitative measures of certainty, for example, that claims enjoy "high confidence" or "medium confidence". In turn, the guidelines for reporting coarse-grained claims leave leeway for judgment: in Steele's example (Steele 2012, 898), whether being 0.6 confident counts as "high confidence" ("about 8 out of 10") or "medium confidence" ("about 5 out of 10").

Although he denies that scientists must assign precise probabilities to hypotheses (p.214), Betz understands the methodological critique as turning on a worry about translating from states of epistemic uncertainty to a binary choice to adopt-or-not.[3] However, following Steele, even if IPCC authors do not make such a stark choice as adopt-or-not, they must still make some translation. Such translation might itself involve (substantive, not trivial) risks of error, and seems subject to the methodological critique. (For a more generalised version of a similar concern, see Wilholt 2013, p.239). Of course, the IPCC might set out more complex guidelines, for example specifying that "medium confidence" includes claims between 0.4 and 0.65. If so, if it is beyond reasonable doubt that a claim enjoys 0.6 probability, then it will also be beyond reasonable doubt that it enjoys "medium confidence". However, as Betz concedes, scientists will rarely be able to assign precise degrees of probability. When they judge the probability of some hypothesis as within some range which crosses a "boundary"

[3] Suggesting, for example, in response to a case suggested by Heather Douglas that "rather than opting for a single interpretation of the ambiguous data, scientists can make the uncertainty explicit through working with ranges of observational values" (p.212).

they face a problem in translating their state of uncertainty into a standardised terminology, and no translation manual can cover all eventualities.

These concerns do not show that Betz is wrong that the IPCC specifically, and scientists more generally, *could* avoid the methodological critique. The IPCC could stop expecting scientists to translate probability assignments into a standardised language at all (and other scientists could follow suit). However, Steele's work complicates Betz's claims in two ways. First, Betz cannot point to the actual operations of the IPCC as proof that scientists do, and hence can, adopt "hedged hypotheses", thereby weakening his argument from an existential to a possibility proof of value-free science. Second, there might be good non-epistemic reasons for the IPCC to use standardised language, for example related to effective communication. Although such possibilities do not undermine Betz's claim that we have non-epistemic reasons to resist appeal to non-epistemic considerations in policy-making, they suggest a qualification: non-epistemic considerations do not necessarily favour value free science.

2 The value of certainty

I will return to the IPCC in §3 below, but first set out a more general problem for Betz. Betz thinks that the possibility that scientists might report "hedged hypotheses" is important because such claims are "beyond reasonable doubt", and, he claims, the methodological critique starts from a worry that scientists must make claims which are not "beyond reasonable doubt" (pp.214–216). This section shows that this way of framing the methodological critique is mistaken.

A policy of adopting hedged, rather than plain, hypotheses, is not the only way in which scientists might ensure their claims are beyond reasonable doubt. They could also do so if they adopted plain hypotheses, but only when, according to their evidence, it is extremely unlikely that those hypotheses are false. For example, a scientist who runs a statistical test has two ways of adopting only results beyond reasonable doubt: by adopting hedged claims ("given my tests, it is possible, but not very likely that p") or by adopting plain hypotheses only when they meet a high significance level.[4]

If, on Betz"s reading, the methodological critique can be responded to using either the hedging or the cautious strategies then he has misrepresented that critique. The most compelling way in which to represent the methodological critique is as follows: when we are deciding whether or not to adopt some plain hypothesis under circumstances of uncertainty we run a risk of a "false positive" (adopting a claim which is not true) and a risk of a "false negative" (failing to adopt a claim which is, in fact, true). Our trade-off between these two types of error should be guided, in part, by the expected practical costs and benefits of acting on each. For example, if we are testing whether some chemical, with known beneficial uses, is also fatally toxic to humans then, all else being equal, we should be more willing to tolerate false positives than if we are testing whether or not that chemical has minor side effects.[5] Imagine now a scientist who deals

[4] In further support of this proposal, and to show it does not rely specifically on the possibility of statistical testing, consider the case of the law (from where the concept of "beyond reasonable doubt" originates). If the only way of ensuring that our claims are "beyond reasonable doubt" is to adopt "hedged" rather than "plain" claims, then we could not make sense of how jurors could ever reasonably declare a defendant "guilty".
[5] For a more fleshed out version of such a case, see Douglas, 2000.

only in plain hypotheses and insists that she will adopt the plain hypothesis that the drug is fatally toxic if and only if, relative to her evidence, such a claim is very unlikely to be a false positive. Such a strategy might ensure that any claims she makes are "beyond reasonable doubt". However, a proponent of the methodological critique might object to this decision as reflecting an ethically unacceptable lack of concern for human life. Betz assumes that as long as scientists can limit themselves to making claims which are beyond reasonable doubt, then the methodological critique is empty. However, this is a mistake, because that critique suggests that a claim being beyond reasonable doubt may not be a necessary condition for its adoption, even if it is sufficient.

However, Betz might have seemed to identify a line of response to the methodological critique, even if he has mis-characterised its force. Although not his stated argument, we might read Betz as follows.[6] Scientists have a choice between adopting hedged hypotheses and adopting plain hypotheses. If they adopt the first route, they can communicate useful information to policy-makers without themselves making value judgments about the importance of different types of error. If they adopt the second route, by contrast, they must decide which types of error are most important to avoid. Such decisions will involve the kind of value judgment which undermines democratic norms. Clearly, the first route seems preferable in terms of democratic norms. Note that this reformulation relates to Betz's stated concerns in a tricky way: the strategy of adopting hedged rather than plain hypotheses allows scientists to ensure the claims they make are beyond reasonable doubt, but this is a side-effect of, not a reason for, choosing that strategy.[7] The real bite of Betz's response is to show as long as scientists simply report the logical relationships between data and hypotheses then they need not make any judgment about when claims are "certain enough" to warrant policy-makers' adoption.

3 How deep does the methodological critique go?

Even if Betz has misdiagnosed the force of the methodological critique, maybe he has found a way to avoid it. Unfortunately, in this section I argue that, quite apart from the concerns raised in §1, the IPCC's work is an excellent example of why reporting hedged hypotheses does not obviously guarantee value freedom. To motivate this discussion, consider how Betz tries to deal with a second version of the methodological critique.

There is an obvious rejoinder to Betz's claim that scientists who adopt hedged hypotheses thereby only adopt claims which are beyond reasonable doubt (familiar from Rudner's initial discussion of inductive risk): adopting evidence claims might involve adopting claims which are not beyond reasonable doubt (p.211). It seems, then, that the "methodological critique" can simply be re-run at a deeper level: choices about whether or not to adopt evidence claims must be guided by non-epistemic judgments.[8] In response, Betz makes a simple but important point that the methodological critique

[6] I am grateful to an anonymous referee for suggesting this line of argument

[7] Dropping the notion of "beyond reasonable doubt" from Betz's arguments might seem to collapse his claims into those suggested by Richard Jeffrey in earlier debates over inductive risk (Jeffrey, 1956), but this would overlook how Betz goes well beyond Jeffrey in his understanding of our options for assessing and communicating uncertainty.

[8] Wilholt, 2013, p.239 provides a nice summary of this problem, albeit in the context of providing an alternative response to the methodological critique.

should not end up as a kind of global scepticism (p.215). As long as the evidence claims we use to form “hedged hypotheses” are “beyond reasonable doubt” to raise the methodological critique with regard to these claims would be empty. In turn, Betz thinks that many evidence claims are beyond reasonable doubt. Raising the methodological critique to a strategy of reporting hedged hypotheses based on such reports would, then, be irrelevant to scientific practice in the same way that the possibility that an evil demon is tricking us into believing in an external world is irrelevant to organising a football match. This response may seem sensible, but it is premised on the understanding of the methodological critique criticised in §2. Is the reworked version of Betz’s argument – that scientists who report “hedged hypotheses” seem better placed to communicate useful information in a non-value-laden manner than scientists who adopt plain hypotheses – susceptible to the “deeper” methodological critique?

The methodological critique is most forceful when scientists must decide whether or not to adopt claims where there is good evidence both for and against those claims – i.e. where choices to adopt or not run significant chances of false positives and false negatives – and where there is a relationship between adopting those claims and practical action – i.e. the significant epistemic chances are relevant to significant real-world costs. In these circumstances, any decision as to whether or not to adopt claims can be interpreted as reflecting an ethical value judgment as to which of two types of practically costly errors it is most important to avoid. Therefore, for reporting hedged hypotheses to be “value free”, it must be the case that those hypotheses are relative to evidence claims the adoption of which did not involve trading-off practically significant errors. I now argue that Betz’s own example of potentially value-free science, the IPCC’s reports, shows just how difficult meeting this condition might be.

Hedged hypotheses report the likelihood of claims relative to some body of evidence. What is the body of evidence relative to which the IPCC reports its “hedged hypotheses”? Although not how they frame matters (for obvious reasons), the IPCC’s guidelines provide a general answer: they assess hypotheses relative to “all relevant scientific information”. In more concrete terms, in constructing this body-of-evidence, “priority is given to peer-reviewed scientific, technical and socio-economic literature”. Although it does include some “non peer-reviewed literature, such as reports from governments and industry”, the IPCC notes that “use of this literature brings with it an extra responsibility for the author teams to ensure the quality and validity of cited sources and information” (IPCC, 2013b). In short, the IPCC reports probabilities on the basis of evidence, where, typically, to count as evidence a claim must have appeared in a peer-reviewed form.

A proponent of the “deeper” methodological critique needs to show that choices as to what to include in a body of evidence do run significant, practically-relevant risks of false positives and false negatives. I suggest that the IPCC’s construction of its body of evidence does involve making a significant judgment about how to trade-off risks of adopting false evidence claims versus failing to adopt true evidence claims. Plausibly, claims which have appeared in peer-reviewed journals are unlikely to be false. However, the IPCC could be more liberal in constructing its body of evidence; for example, it could be less reluctant to include evidence from non peer-reviewed publications, or from "grey" unpublished literature. The IPCC's refusal to include such evidence might be justified on the grounds that (on average) such reports are less likely to be true than those in peer-reviewed publications.[9] However, failing to include such reports clearly runs a risk of excluding some true reports from the IPCC's evidence. In effect, in constructing a body of evidence the IPCC makes a judgment about the relative importance of false positives and false negatives of the sort which gives rise to the methodological critique.

Showing that the "methodological critique" applies to adoption of evidence claims requires showing not only that decisions about adopting such claims run significant chances of false positives and false negatives but also that these decisions have significant practical consequences. I shall now explore an actual case which shows how this second condition relates to the IPCC's work. Both the Third and Fourth Assessment Reports of the IPCC contained discussion of claims that the West Antarctic Ice Sheet (WAIS) might "collapse" (i.e. lose ice and hence contribute to overall sea level rises).[10] The Third Assessment Report claimed that there was no risk of collapse in the short term (i.e. up to 2,100), but that the WAIS might collapse subsequently, and, although noting "high uncertainty" about such estimates, provided long-term projections for the highest potential rate of ice loss. The Fourth Report, by contrast, reported that there is already relatively rapid loss of ice from the WAIS – i.e. suggesting that collapse might already be occurring – but that they could not provide any estimate of short- or long-term ice-loss. Therefore, tables showing possible sea level rises in the Twenty-First Century did not include possible contributions from Greenland or Antarctica.

In their rich and detailed analysis of this case, O'Reilly, Oreskes and Oppenheimer understand this "rapid disintegration of consensus" over the WAIS a result of several different factors. For current purposes, I shall pick out one aspect: in the period between the Third and Fourth Assessment Reports, new data emerged which seemed to contradict the dynamical models which had previously been used to predict the WAIS, thereby undercutting both the assumption that the Ice Sheet would be stable in the short term and predictions about its longer term behaviour. This data did not, however, show that the WAIS was *more* stable than previously assumed, but that it was already melting. However, articles which developed this data and generated new predictions about the WAIS were published too late to be included within the Fourth Assessment Report's remit. At the time of writing, these results were known, relevant papers were drafted, models constructed and so on, but these claims had not undergone peer review. In the words of one of O'Reilly *et al's* interviewees, "it seemed to us we just couldn't do it [provide a numerical estimate for the effect of WAIS collapse] because the IPCC depends on using peer-reviewed results" (O'Reilly, Oreskes and Oppenheimer, 2012, p723).

When IPCC authors adopt hedged hypotheses they need to decide on a body of evidence relative to which they adopt those hypotheses. In effect, they use "has been peer reviewed" as a principle for making such choices. Relative to this inclusion/exclusion criterion, IPCC authors made the right choice in writing the Fourth

[9] In this context, consider the controversy over the Fourth Assessment Report's mistaken projection for melting of Himalayan ice caps, which arose from reliance on non-peer-reviewed sources (Pearce, 2010).

[10] The following example relies heavily on O'Reilly et al. 2012.

Assessment Report. Given the uncertainty over data and models in the published literature, it was impossible even to form a hedged hypothesis. Furthermore, such a criterion might be justified as being highly “epistemically conservative”. However, IPCC authors could have used other, less rigorous criteria for including reports within their body-of-evidence; for example, that if new projections seemed plausible to experts, then those reports should form part of their body-of-evidence. This inclusion criterion is laxer than the “has been peer-reviewed” criterion, in the sense that it would increase the chance that false reports would be included. However, using such a criterion is hardly the same as saying “anything goes”. Furthermore, had the IPCC used it, then maybe they would have been able properly to form a hedged hypothesis about likely sea level changes. In turn, it seems plausible that the choice of criterion might have had a significant impact on practical action, in that policy-makers would be more likely to take steps against the disintegration of the WAIS if they have a numerical estimate of the loss rate.

In this case it seems that we can run a version of the methodological critique aimed not at scientists’ adoption of hypotheses, but at their adoption of evidence claims. Even when the IPCC offers hedged hypotheses, these statements turn heavily on what it includes in its body of evidence. Decisions about what evidence reports to adopt involved trading-off significant risks of false positives and false negative, where the relevant choices have a significant relationship to action. Therefore, the methodological critique runs, choices of which evidence claims to adopt can be understood as shaped by a (contestable) ethical judgment of the relative badness of needless action and misguided inaction. Indeed, not only could a proponent of the methodological critique run such an argument, but some of the fierce controversy over the (lack of) WAIS predictions in the Fourth Report can be understood in these terms. For example, when the climate scientist James Hansen describes debates over the WAIS as involving a “dangerous reticence”, with explicit reference to the IPCC, his complaint is precisely that epistemic conservatism can lead to disastrous ecological effects (Hansen, 2007).

Of course, these comments do not show that science cannot be value free. Perhaps there are areas of scientific research where it is clear-cut which evidence reports should be included in a body of evidence. Scientists who reported hedged hypotheses on the basis of these reports would not be open to the methodological critique. Neither do these comments show that in cases where adoption of evidence claims is not straightforward, such as that of the IPCC, reporting cannot be value free. For example, IPCC protocols could be rewritten to allow authors to report multiple “hedged hypotheses” each indexed to a different “body of evidence”. Such a “value-free” IPCC report would, however, be far distant from the actual report: Betz cannot point to actual practice as even an approximation of value-free science. Furthermore, such a Report would be an extremely complex document for policy-makers. For example, it might end up reporting a low confidence for some hypothesis relative to one body of evidence and a high confidence for the same hypothesis relative to a second body of evidence! The problems with using this document would be further compounded if we take Steele’s worries (outlined in §1) seriously: not only would there be multiple reports of the probability of the same hypothesis, but, to ensure value freedom, these reports would be framed in complex mathematical terms, rather than a more easily comprehensible standardised vocabulary. The next section outlines the relevance of these comments both to assessing the VFI generally and to understanding the IPCC’s work specifically.

4 Saving value-freedom?

Proponents of the methodological critique who claim that value-freedom is *impossible* are, as Betz suggests, mistaken. However, there is a difference between showing that some state-of-affairs is *possible* and showing that it is *ideal*. The discussion above shows that achieving value-freedom for science comes at a significant non-epistemic cost. Scientists might avoid an apparently illegitimate insertion of ethical values into policy-making if they follow Betz's advice, but only by adopting a reporting regime which renders them incapable of saying anything useful for policy-makers at all. Indeed, a truly value-free science would only push the problem of value-freedom to another level: an IPCC Report which was truly "value-free", in the way Betz proposes, would be so complicated that to render it useful to policy-makers would require "interpreters"; such "interpreters" would need to be scientifically trained; and these scientists would face problems of inductive risk. Maybe policy-impotence is a price worth paying to avoid democratic illegitimacy, but this is highly contestable, and there is a large difference between thinking that value-freedom is ideal and thinking it the lesser of two evils.

We seem, then, to face an impasse: Betz has provided us with an account of how science could be value-free, but this aim might be difficult to obtain; even when obtainable, of dubious non-epistemic value; and simply to create a new problem about the proper role of scientific interpreters. On the other hand, accepting that useful scientific work will be value-laden also seems unappealing, particularly given the value pluralism characteristic of modern liberal societies. The IPCC's reports sharply exemplify this problem. Given the discourse of politicised science which surrounds climate debates, publicly acknowledging that the IPCC's reports are not "value free" would seem (further) to undercut their epistemic authority. We seem to be in a lose/lose situation. However, in conclusion, I argue that Betz's concept of "being beyond reasonable doubt" suggests an alternative way in which the IPCC's work might, in fact, be "value-free". Although the relevant form of value freedom may not avoid the methodological critique, it does, at least, allow the IPCC both to provide some useful input into policy-making while respecting value pluralism.

In §2, I argued that Betz is wrong to assume that the methodological critique can be avoided if scientists restrict themselves to making claims which are "beyond reasonable doubt", because scientists might adopt plain hypotheses only when they are "beyond reasonable doubt" but still be subject to the methodological critique. A toxicologist who refuses to adopt the hypothesis that a drug has harmful side effects unless she has overwhelming evidence that this is the case would avoid making claims not "beyond reasonable doubt", but her strategy seems subject to the methodological critique. However, note a complication which §2 did not discuss. The toxicologist's commitment to avoiding "false positives", even at the cost of "false negatives", need not reflect an *ethical* value judgment, but might reflect an *epistemic* value judgment: that it is far worse, epistemically speaking, to adopt false claims than it is to believe true claims. To justify treating this preference as reflecting an epistemic value judgment, consider the concept of knowledge.

Knowledge is typically defined in terms of a safety condition, as in Pritchard's definition: "if an agent knows a contingent proposition, ϕ, then, in nearly all nearby (if not all) possible worlds in which she forms her belief about ϕ in the same way she forms her belief in the actual world, that agent only believes ϕ when ϕ is true" (Pritchard, 2005, 163). One way of ensuring that our beliefs are "safe" in this sense is to form our beliefs through methods which minimise our chance of false positives. When we form beliefs through methods which are not thus calibrated, even when the resulting belief is true, employing a similar method in nearby possible worlds may well have led us to the same belief when it was false. If so, a preference for avoiding "false positives" – a preference for limiting oneself to making claims which are "beyond reasonable doubt" – can be seen as reflecting a commitment to the value of knowledge. Clearly, if anything counts as epistemically valuable then knowledge does!

Remember, the "Value Free Ideal" holds that "the justification of scientific findings should not be based on *non-epistemic* (e.g. moral or political) values" (Betz 2013, p.207, emphasis added). If I am correct that a preference for avoiding false positives can be understood as reflecting a commitment to the epistemic value of knowledge, then our imaginary toxicologist can be said to respect this ideal: although she does make claims which go beyond the available evidence, in making such claims she is guided by an epistemic value judgment. It is important to note two features of this claim. First, one might deny the premise that there is a tight connection between the avoidance of false positives and the epistemic value of knowledge. Defending this connection is beyond the scope of this paper, but note that my claim here is intended as a highly-qualified conditional: given standard accounts of knowledge, we can identify one way of defending a preference for avoiding false positives which appeals solely to an epistemic value, that of knowledge. Whether or not knowledge should be construed in these terms, and, if so, whether or not it truly is valuable, are important questions, but beyond the scope of this paper.[11]

Second, and more importantly, it should be stressed that saying that the toxicologist's preference could be understood as guided by an epistemic value judgment does not in-and-of-itself undermine the concerns of proponents of the methodological critique. Rather, it simply moves them up a level, as it were. A proponent of that critique can concede that the toxicologist's trade-off is not based on a non-epistemic value judgment if it is based on the (real or perceived) value of knowledge, but object that she should not value the pursuit of knowledge over more immediate non-epistemic costs associated with "false negatives".

The arguments above are, then, compatible with thinking that the toxicologist fails to track what is truly of epistemic value, and that, even if her choices do track epistemic value, they are still morally problematic. Why, then, introduce the concept of "knowledge" at all? Framing debate in this way is useful because the (highly idealised) example of the toxicologist points towards a more general phenomenon: that the avoidance of "false positives" is often seen as an important characteristic of "good" scientific research methodology. Indeed, it is precisely the fact that such judgments are "black boxed" – say, by unthinking commitment to use of high p values in hypothesis testing – which occasions the ire of many commentators committed to (something like) the methodological critique.[12] Therefore, I propose that the considerations above help us to grasp the real challenge of the methodological critique, not so much as a concern

[11] I am very grateful to an anonymous referee for making me aware of the need to clarify my position on these issues.

[12] See, for example, Cranor (1993)

that science cannot be value-free, but as a concern that it is unclear why "knowledge", as normally construed, should be deemed so valuable.

Thinking through the relationship between epistemic conservatism and knowledge helps us, then, better to grasp the theoretical issues raised by the methodological critique. Do these comments also help us better understand the work of the IPCC? I will now argue that they do, and then show how these insights also provide materials for defending a preference for avoiding "false positives" against the charge that such a preference might be morally problematic. The comments above suggest that even if the IPCC's work involves making decisions about how to trade-off inductive risks, it does not thereby fail to meet the "value free ideal". In the previous section, I argued that in constructing the body of evidence on the basis of which it forms "hedged hypotheses" the IPCC makes choices about how to balance the risk of including false reports against the risk of failing to include true reports. I also suggested that its way of balancing these risks reflects a strong commitment to avoiding inclusion of false reports even at the cost of failing to include some true reports. The comments in this section suggest that such a practice can be seen as "value free", in the sense that it is guided by epistemic, rather than non-epistemic, values. Therefore, someone who objects to the IPCC's work on the grounds that it is based on contestable ethical and political judgments is confused. Even if the IPCC's cautious epistemic strategy has significant potential non-epistemic costs – as in the case of the WAIS – its inner workings are not themselves based on ethical or political values.

Even if, *contra* Betz, the IPCC's work does involve solving "inductive risk" problems, it still counts as "value free science". As such, it seems to avoid the challenge that its work involves appeal to the kinds of ethical and political values which might be contestable in a democratic society. Furthermore, the way in which the IPCC retains its "value freedom", does not, unlike Betz's suggested strategy, require the IPCC to produce multiple estimates of the likelihood of the same hypothesis relative to different bodies-of-evidence, but only to report the likelihood of hypotheses relative to the best available evidence. As such, it seems better suited to policy-makers' needs. However, these comments in favour of the IPCC's strategy still seem open to the worries raised above: why value "value freedom" given the potential for ethically problematic "under-reporting", as exemplified by the case of the WAIS?

There is no easy answer to this challenge. However, I do suggest that the proper response to these concerns might build on the following reflection. Let us assume that if its advice is to be useful, the IPCC must settle on a single body-of-evidence, and, as such, must trade-off inductive risks in some manner. Quite apart from the intrinsic value of knowledge, we can identify at least one advantage of doing so by relying only on evidence claims which are "beyond reasonable doubt": although such a policy may well under-report the risks of climate change, at least it will not over-report them. Therefore, even if policy-makers cannot assume that "the IPCC reports that there is a risk" to be a necessary and sufficient condition for assuming that claim to be true, they can, at least, treat it as a sufficient condition. As such, the IPCC can enjoy a particular kind of authority in climate change debates: it may not tell us all of the risks we should worry about, but it does tell us about the ones we must worry about. This may be rather a weak justification of the IPCC's practices, but in the messy world where it is neither clear which evidence claims are correct nor which non-epistemic values should guide policy, it may be the best we can hope for.

5 Conclusion

The previous section defended the claim that the IPCC's work is, in a sense, a form of value-free science, and suggested some ways in which to justify this form of value freedom on non-epistemic grounds. However, these justifications fail to vindicate the Value Free Ideal as an ideal, as opposed to as a second best. What, then, are the broader implications of these remarks for understanding the nature and scope of the methodological critique? In this paper, I have argued that Betz is wrong on at least two counts: first, he is wrong that scientists who commit themselves to adopting only those claims which are "beyond reasonable doubt" thereby avoid the methodological critique; second, this confusion aside, his proposals for a "frank" science might allow scientists a form of value-freedom, but only at the cost of impotence. As such, it is unclear that Betz-style "value freedom" is worth pursuing.

However, I have also argued that Betz's work contains two deeply important insights. First, he shows that it is a mistake to think that taking non-epistemic values seriously necessarily requires scientists to take such values into account in the process of scientific reasoning. Rather, as I argued towards the end of the previous section, there may well be good non-epistemic reasons why scientists should adopt kinds of inferential practices which also dovetail with the goal of producing knowledge.[13] Second, he is right that the "beyond reasonable doubt" standard is key to thinking through the methodological critique. In the final section, I have tried to explain how this standard is of great important both epistemically – as related to the difference between true belief and knowledge – and practically – that a claim is "beyond reasonable doubt" functions as a sufficient reason for adopting that claim in policy-making. As such, I have suggested that scientists' commitment to epistemic values may

[13] This may seem rather strange, but note an interesting parallel with legal contexts here: courts which convict only when it is "beyond reasonable doubt" that some individual committed a crime might be thought to act in ways which reflect a concern with knowledge. Clearly, however, even if this is true, we might still seek to justify the importance of knowledge in this context by appeal to (broadly) moral considerations. I suggest, tentatively, that a similar two-level structure may be in place in the case of thinking through the role of non-epistemic values in science. This structure may, however, seem to raise a new question (suggested by an anonymous referee): if non-epistemic values are what ultimately *justifies* a practice which can be described as aiming at an epistemic good, then hasn't one conceded that the relevant practice is, in some sense, value-laden (with non-epistemic values)? This is a tricky question which goes beyond the scope of this paper, but note that there seems to be a difference between saying that non-epistemic values should govern decisions within a practice and appealing to non-epistemic values to justify that practice as worthwhile. By analogy, note that one might hold an ultra-positivist account of science, which banishes any role for non-epistemic values from its proper practice, but still hold that such a practice should be pursued because ultimately it produces technological goods of significant practical utility. It would, I suggest, be strange to say a proponent of such a view denies the "Value Free Ideal".

also allow them to play a particularly important role in policy-making: the claims they are willing to adopt are the claims which all should be willing to act on. On the other hand, proponents of the methodological critique are surely correct that even if a claim being “beyond reasonable doubt” may be a necessary reason for scientists to adopt it, given their epistemic aims, it cannot be a necessary condition for policy-makers to adopt it. If so, the “Value Free Ideal” is likely to be problematic in a world where we expect scientists not only to tell us what we must care about, but also what we should care about. In this situation, I suggest that, all things considered, it is probably preferable to change our view of what science can do, rather than to use contestable value judgments to guide policy, but this is, at best, a defence, rather than a vindication, of the value free ideal.[14]

References

Betz, G. (2013). In defence of the value free ideal”. *European Journal for Philosophy of Science, 3*, 207–220.

Biddle, J. (2013). State of the Field: Transient Underdetermination and Values in Science. *Studies in History and Philosophy of Science, 44*, 124–133.

Coady, D and Corry (2013)R The climate change debate: an epistemic and ethical inquiry (London: Palgrave Macmillan)

Cranor, C. (1993). *Regulating Toxic Substances*. Oxford: Oxford University Press.

Douglas, H. (2000). Inductive risk and values in science”. *Philosophy of Science, 67*(4), 559–579.

Douglas, H. (2009). *Science, policy and the value free ideal*. Pittsburgh: University of Pittsburgh Press.

Elliott, K. (2011). *Is a little pollution good for you?* London: Oxford University Press.

Hansen, J (2007) “Scientific reticence and sea level rise” Environmental Research Letters 2 (2) (April-June 2007)

IPCC (2013a) “Activities” web-page at http://www.ipcc.ch/activities/activities.shtml#.UgpPoW1Jp-w (accessed 13 August, 2013)

IPCC (2013b) “Principles and procedures” web-page at http://www.ipcc.ch/organization/organization_procedures.shtml#.UgpQg21Jp-w (accessed 13 August, 2013)

Jeffrey, R. (1956). Valuation and acceptance of scientific hypotheses. *Philosophy of Science, 23*(3), 237–246.

O’Reilly, J., Oreskes, N., & Oppenheimer, M. (2012). “The rapid disintegration of consensus: the West Antarctic Ice Sheets and the International Panel on Climate Change”. *Social Studies of Science, 42*, 709–731.

Pearce, F (2010) “Debate heats up over IPCC melting glaciers claim” New Scientist 2743; 16th January 2010

Pritchard, D. (2005). *Epistemic Luck*. Oxford: Oxford University Press.

Steele, K. (2012). The scientist qua policy advisor makes value judgments”. *Philosophy of Science, 79*(05), 893–904.

Wilholt, T. (2013). Epistemic trust in science”. *British Journal for Philosophy of Science, 64*, 233–253.

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