Studies in History and Philosophy of Science 46 (2014) 24–30

Contents lists available at ScienceDirect

Studies in History and Philosophy of Science

journal homepage: www.elsevier.com/locate/shpsa

Values and uncertainties in climate prediction, revisited

Wendy Parker

Department of Philosophy, Durham University, 50 Old Elvet, Durham, DH1 3HN, United Kingdom

ARTICLE INFO

Article history:
Available online 21 December 2013

ABSTRACT

Philosophers continue to debate both the actual and the ideal roles of values in science. Recently, Eric Winsberg has offered a novel, model-based challenge to those who argue that the internal workings of science can and should be kept free from the influence of social values. He contends that model-based assignments of probability to hypotheses about future climate change are unavoidably influenced by social values. I raise two objections to Winsberg's argument, neither of which can wholly undermine its conclusion but each of which suggests that his argument exaggerates the influence of social values on estimates of uncertainty in climate prediction. I then show how a more traditional challenge to the value-free ideal seems tailor-made for the climate context.

When citing this paper, please use the full journal title *Studies in History and Philosophy of Science*

1. Introduction

In a classic paper, Richard Rudner (1953) argued that ethical values are a required part of the internal workings of science. Since evidence never establishes a hypothesis *H* with certainty, scientists must decide whether the evidence is sufficiently strong to merit acceptance of *H*, and this decision reflects judgments about how bad it would be (in a typically ethical sense) to be mistaken in accepting or rejecting *H*. Thus, for example, we would demand stronger evidence before accepting the hypothesis that a new medical procedure for children is safe than before accepting the hypothesis that a manufactured lot of shampoo bottles meets desired quality specifications.

In a classic reply, Richard Jeffrey (1956) challenged Rudner's conclusion by denying that scientists are in the business of accepting or rejecting hypotheses in the first place. According to Jeffrey, scientists' job is to assign probabilities (degrees of belief) to hypotheses; it is then up to decision makers—with their social, political and ethical value commitments—to take action or not in light of those probabilities, that is, to determine whether the probabilities provided by scientists are high enough to warrant various courses of action. On what we might call a Jeffreyan view, the internal aspects of scientific research—including the assignment of probabilities to hypotheses—can and should remain free from the influence of social values.[1]

Today, philosophers of science remain divided on the appropriate roles of ethical and social values in science. Some, like Heather Douglas (2000, 2009), argue that such values are a required part of the internal workings of science in much the way that Rudner suggested. Others, like Sandra Mitchell (2004), seem to embrace something much closer to the Jeffreyan view. Recently, Eric Winsberg (2010, 2012; see also Biddle and Winsberg, 2009) has offered a novel challenge to the Jeffreyan view. He argues that, when it comes to hypotheses about future climate change, it is infeasible for climate scientists to exclude the influence of social values when assigning probabilities. As he puts it: "Scientists cannot assign probabilities to hypotheses about future climate change—or, more specifically, estimate the uncertainties of climate predictions—in a manner that is free from non-epistemic considerations…" (2010, p. 119).[2]

To begin to see what Winsberg has in mind, consider the following statement: The probability is 0.90 that global temperature will rise by between 2 and 3 °C during the 21st century if greenhouse gas emissions continue at current rates. This statement assigns a probability to a hypothesis about future warming. It is also a way

E-mail address: wendy.parker@durham.ac.uk

[1] In this discussion, I will use "social values" as Winsberg (2012, p. 113) does—as shorthand for things that are valued for paradigmatically social or ethical reasons.

[2] Winsberg here has in mind "one class of non-epistemic values in particular—those reflected in deciding that certain types of prediction tasks are more important than others" (Winsberg, 2010, p. 100). These values are among the ones he later refers to as "social values". The switch to "social values" seems to reflect a desire to avoid begging the question as to whether these values play important epistemic roles (see Winsberg, 2012, p. 113).

0039-3681/$ - see front matter © 2013 Elsevier Ltd. All rights reserved.
http://dx.doi.org/10.1016/j.shpsa.2013.11.003

of expressing uncertainty about how much warming would occur; instead of reporting that the temperature would definitely rise by, say, 2.1 °C, an assignment of probability is made to an interval of change. Winsberg argues that social values unavoidably influence probabilistic uncertainty estimates like these. What is especially novel about his argument is its grounding in the practice of complex simulation modeling: assignments of probability to hypotheses about future climate change are influenced by social values, he argues, because of the way these values come into play in the building and evaluating of climate models, on whose results the assignments of probability depend.

The present paper has two main aims: first, to critically evaluate Winsberg's model-based challenge to the Jeffreyan view and, second, to show how a more traditional set of replies to Jeffrey seems tailor-made for the climate context. Section 2 provides background on climate modelling and on uncertainty in climate prediction. Section 3 presents a distilled version of Winsberg's argument. Section 4 raises two objections to the argument; while neither can wholly undermine his conclusion that social values have some unavoidable influence on uncertainty estimates in climate prediction, each suggests that social values exert less of an influence than he implies. Section 5 revisits a more traditional set of replies to Jeffrey. Without necessarily endorsing these replies, it is shown that they dovetail nicely with recent discussions—internal to climate science—about the estimation of uncertainty in climate prediction.

2. Models, ensembles and probabilities

To understand Winsberg's argument, we first need to appreciate that most quantitative predictions of future climate change are made with the help of computer simulation models. These models are needed in part because the causal processes that shape the evolution of climate are numerous, nonlinear, and interactive. They include the transfer of radiation through the atmosphere, the movement of large-scale weather systems, the formation of local clouds and precipitation, ocean currents, the forming and melting of polar ice sheets, and many more. Not even the most insightful scientist can expect to foresee, in detail, the spatial and temporal patterns of climate change that these interacting processes will produce in response to rising greenhouse gas concentrations. So scientists attempt to represent these processes and interactions mathematically and then estimate solutions to these equations (taking small time steps) with the help of computers; the goal is to simulate how Earth's climate would change if greenhouse gas emissions were to rise at particular rates in the future.

But there is uncertainty about how to build these climate models in such a way that predictions of future climate change will have desired accuracy. The uncertainty stems in part from a limited theoretical understanding of the climate system, but also from the constraints placed by available computing power—the computer simulation needs to finish running before climate actually changes, and this means that even physical processes that are well-understood may need to be represented in a simplified way. In many cases, it is far from obvious how to make these simplifications such that predictions with desired accuracy will be produced.

Uncertainty about how to model the climate system is usually partitioned conceptually into three components. *Structural uncertainty* is uncertainty about the form that modeling equations should take. We can think of this as uncertainty about which causal processes should be represented in the model and with what sorts of equations.[3] *Parametric uncertainty* is uncertainty about the values of parameters within a given model structure, for example, about the numerical value that should be assigned to a parameter representing the entrainment of air into clouds from the surrounding environment. *Initial condition uncertainty* is uncertainty about which numerical values to assign to model variables at the start of the simulation. When it comes to future climate change, the question is: how much does this representational uncertainty matter when it comes to predictions of future climate change? That is, how do structural, parametric and initial condition uncertainty translate into predictive uncertainty?

One way to try to get a handle on this predictive uncertainty is to build and run climate models with different structures, parameter values and initial conditions, seeing how their projections of future climate differ. This is the basic idea behind *ensemble* climate prediction studies.[4] So far, these ensemble studies have tended to come in two varieties. In *perturbed-physics* studies, multiple projections of future climate are produced using a single set of modeling equations, but with a different combination of values assigned to uncertain parameters within the equations on each run (see e.g. Frame et al., 2009; Murphy et al., 2009). In *multi-model* ensemble studies, by contrast, projections are produced using climate models that differ in the form of some of their equations and often in various other ways as well (e.g. in their resolution, their numerical solution methods, their parameter values, etc.); typically, these are models developed at different modeling centers around the world (see e.g. Meehl et al., 2007b; Taylor, Stouffer, & Meehl, 2012). In both types of ensemble study, each model or model version may be run with a few different sets of initial conditions as well.

How to interpret the sets of projections produced in ensemble studies is a topic of some debate (see e.g. Parker, 2010; Smith, 2002; Stainforth et al., 2005, 2007). Nevertheless, increasingly methods are being developed and deployed that attempt to extract probabilistic information from such sets of projections: they assign (what appear to be) precise probabilities to future changes in climate as a function of the distribution of projections from today's climate models.[5] Such methods, which come in several varieties, are prime examples of those Winsberg has in mind when making his argument about the influence of social values.

3. A reconstruction of Winsberg's argument

With this background, we are ready to consider Winsberg's (2012) argument. Here, I present a distilled reconstruction, aiming to capture the key moves but omitting some of the details[6]:

> **P1:** State-of-the-art methods for estimating uncertainty about future climate change assign probabilities to changes in climate as a function of the distribution of projections from today's climate models (pp. 116–118).
> **P2:** The distribution of projections from today's climate models has been influenced by past choices made in the course of model development, such as which physical processes to incorporate next and how (pp. 127–128).
> **P3:** Model-development choices like these often are epistemically unforced, and social values fill the gap, making some options preferable to others (pp. 124, 130–131).
> **P4:** It is infeasible for scientists to correct for the effects that these value-influenced choices have on estimates of probability produced in ensemble studies; there is no Bayesian fix here (pp. 129–130).

[3] On some definitions, it also includes uncertainty about how these equations should be implemented and solved computationally.
[4] For further, non-technical discussion of ensembles see also Parker (2006, 2010, 2013).
[5] For discussions of methods and examples of results see e.g. Tebaldi & Knutti, 2007, and Murphy et al., 2009.
[6] Winsberg (2010) argued that the influence of social values here tended to result in uncertainty estimates that are biased in particular ways, but that idea was not emphasized in the more recent discussion (2012). Here I consider just his argument for unavoidable influence.

C: State-of-the-art estimates of uncertainty about future climate change are unavoidably influenced by social values (pp. 131–132).

P1 requires little explication, given the discussion of ensembles in the preceding section. **P2** is also straightforward: if different choices had been made in the course of building today's climate models, then the projections of future climate change that they produce would be somewhat different. For instance, choosing to represent a process in one way rather than another will make some difference to the results. Biddle and Winsberg (2009; see also Winsberg, 2010, chap. 6) have argued that even the order in which processes are added to a model makes a difference, as past modeling choices to some extent constrain future modeling choices; it simply may not work to represent a process in our model in a particular way, given that we have previously chosen to represent other processes in particular ways.

P3 and **P4** are more complex. **P3** relies on a distinction between epistemically forced and unforced choices in model building. This distinction is not entirely clear in Winsberg's discussion, but it seems to work as follows: An epistemically forced choice is one in which there are decisive, purely epistemic grounds for considering one model-building option to be better than all other available options, otherwise the choice is epistemically unforced. **P3** asserts that many choices in the building of climate models are epistemically unforced, and that social values are what very often (if not always) lead scientists to prefer some options to others. Winsberg argues that there are at least two ways that social values exert an influence here (see e.g. 2012, p. 124).

First, climate scientists may be aware that the available model-building options strike a different balance of inductive risks[7] with respect to a problem or task (e.g. the task of predicting whether changes in precipitation in a region will exceed some threshold) and may choose one option in light of its inductive risk profile. The choice reflects the value judgment that it would be worse (ethically or socially) to err in one way than in another. Second, modeling choices may be influenced by decisions to prioritize some predictive tasks over others—decisions which are themselves often shaped by social values. We prioritize the accurate simulation of changes in surface temperature, droughts and other weather extremes, for instance, because they have greater social and economic importance than many other quantities, e.g. the average height of the 200mb pressure surface. These prioritization decisions can influence subsequent choices in model building: given particular predictive priorities, we might focus on improving our representation of one causal process rather than another, or we might decide that the next process to be added to the model will be one rather than another.

P4 relates to what Winsberg calls the "Bayesian response to the Douglas challenge" (BRDC). The Douglas challenge—so named because it is attributed to Heather Douglas (2000, 2009)—is in effect a more general version of Winsberg's **P3**: doing science often involves epistemically unforced methodological choices whose options have different associated inductive risk profiles; such choices must be made on some grounds, and they often are (and should be) made in light of the judgment that it would be worse, ethically or socially, to err in one way than in another. According to the BRDC, this sort of influence on the part of social values can be screened out or compensated for via expert judgment—we can and should (on epistemic grounds) factor in the inductive risk profile of the method used. If a chosen method for detecting the presence of a disease has a tendency for false positives, for instance, we can and should take that into account when assigning probabilities to hypotheses in light of the results we obtain via that method.

P4 asserts that the BRDC is in practice unavailable in the case of ensemble climate prediction. According to Winsberg, applying the BRDC would require that an epistemic agent "be capable of making an informed judgment about how every single methodological choice on which a climate model is built ought to influence his or her degree of belief in a hypothesis that he or she is evaluating with the use of that model" (2012, p. 130), presumably for each model involved in the ensemble study. Several factors combine to make such judgments out of reach: many of the methodological choices will have been made by other scientists, over the (perhaps decades-long) history of the model's development; the impact of a given choice on the model's results often will be sensitive to earlier and later choices; the interactions among choices will be complex and numerous and thus difficult to untangle, especially for the typical scientist whose expertise is concentrated in one or two areas, such as radiation physics (see also ibid, pp. 125–129).

How exactly does Winsberg's argument present a novel challenge to the Jeffreyan view? The latter is sometimes characterized as the view that probabilities can and should be assigned to scientific hypotheses in a way that is free from the influence of social values. When expressed in this way, it is tempting to think that the Jeffreyan view requires that probability assignments should not reflect the influence of social values in any way. But in that case, the view could be easily dismissed via an argument that has nothing to do with complex simulation modeling. In particular, insofar as any assignment of probability to a hypothesis will depend on the state of current knowledge, which in turn will depend upon what we have found important enough to invest resources investigating, assignments of probability to scientific hypotheses will always be influenced by social values. On this interpretation, we would declare the Jeffreyan view a non-starter, and there would be no need for Winsberg's argument.

But this is not the sort of value influence that advocates of the Jeffreyan view want to deny, nor the sort of influence that Winsberg wants to demonstrate. Advocates of the Jeffreyan view no doubt agree that the state of knowledge at any given time depends on what we've considered important enough to investigate. What they insist is that social values need not and should not influence the probabilities that are assigned to hypotheses *in light of* (or *conditional on*) what has been revealed by investigations up to now. In other words, the Jeffreyan view asserts not that social values have no influence whatsoever in science but rather that the process of determining or estimating probabilities *relative to a body of evidence* can and should be insensitive to the social values the inquirers might happen to hold. In more general terms, the Jeffreyan view asserts that social values shouldn't influence our assessment of how evidence bears on hypotheses, even if these values do influence what evidence is available for us to consider in the first place.

Winsberg presents a novel challenge to the Jeffreyan view by arguing that, in some cases, state-of-the-art methods for estimating the probabilities that should be assigned to hypotheses of interest in light of current understanding are model-based methods which are unavoidably sensitive to social values. Perhaps in an ideal Bayesian world our cognitive and computational powers would be great enough that we would not need to rely on such methods; we would be able to propagate probabilistic representations of structural, parametric and initial condition uncertainty to arrive at assignments of probability to future changes in climate, without the help of computers and without having to rely on

[7] The balance of inductive risks associated with a choice or method usually refers to the extent to which that choice or method tends to produce type I vs. type II errors in a particular sort of inference task.

simplified and somewhat distorted distillations of current knowledge (i.e. climate models). But in the real world—given the limits of our actual cognitive and computational capacities, as well as the limits of current understanding of the climate system—this is not possible.

4. Evaluating Winsberg's argument

Has Winsberg offered a compelling challenge to the Jeffreyan view? Two objections to his argument will be discussed here. Neither will wholly undermine his conclusion that social values have some unavoidable, model-mediated influence on uncertainty estimates in climate prediction. But each will suggest that his argument exaggerates the influence of social values.

The first objection concerns **P3**. Winsberg claims that choices in the construction of climate models often are epistemically unforced. This seems right, especially when it comes to choices related to the parameterization of sub-grid processes.[8] But he also suggests that very often (if not always) what fills the gap, making some options in model building preferable to others, are social values. He supports this claim not with an empirical demonstration that lots of particular choices were in fact shaped by social values—indeed he emphasizes that the motivations for specific choices in the sometimes decades-long development of climate models are mostly hidden from our current vantage point—but rather with the deeper and more sweeping assertion that "no unforced methodological choice can be made in a value vacuum" (2012, p. 130). He suggests that the only plausible gap-filling candidates are social values; otherwise the choices would seem arbitrary (ibid, p. 131). But this is not the case. It seems clear that such choices can also be influenced or even determined by *pragmatic factors*.

Suppose a group of climate scientists is further developing their climate model now that more computing power is available. Which physical process should they "add" to their model next? Suppose their choice is epistemically unforced, i.e. they cannot argue on purely epistemic grounds that one process in particular should be added next. Must their choice then either be arbitrary or determined by social values? No. Pragmatic factors can also fill the gap. For instance, the scientists might already have in hand some computer code for process P but not for processes Q, R, or S. Or they might judge that it will be much easier to incorporate P than to incorporate Q or R or S, given past choices in model building. Or they might be experts on P but have much less understanding of Q and R and S. Or it might be that a leading modeling group incorporated P for reasons like those just identified, and now it is seen as *de rigueur* for state-of-the-art climate models to include P. And so on. Indeed, it is plausible that pragmatic factors like these often influence or even determine model development choices. So Winsberg needs to work harder to make the case that the influence of social values here is as pervasive and common as he suggests.

Winsberg may be tempted to reply that the fact that processes P, Q, R and S are the leading candidates is itself almost surely a function of social values: if social values hadn't led scientists to prioritize the accurate prediction of some quantities (e.g. changes in global mean temperature) rather than others, then they wouldn't have narrowed their focus to these processes to begin with. But even this is not so clear. The narrowing of focus might instead reflect *non-decisive epistemic considerations*. For instance, some climate system processes, such as the formation of clouds and precipitation, are thought to influence a broad range of climate variables in significant ways even on relatively short time scales, whereas other processes have smaller and/or more localized impact on those time scales; scientists might narrow their focus to a set of processes of the former type because not including them (or representing them poorly) can be expected to result in simulations that have significant errors in a wide range of climate variables.[9] No single, top-priority process might be identifiable on epistemic grounds alone, leaving the choice of which process to add next unforced, but the narrowing of focus might nevertheless have been driven by epistemic considerations.

The second objection relates to Winsberg's focus on methods that assign precise probabilities to hypotheses about future climate change. The objection is that this focus is somewhat misplaced: an argument that scientists cannot estimate "the uncertainties of climate predictions" in a manner that is free from the influence of social values ultimately should focus not on methods for producing precise probabilistic estimates—which are known to be artificially precise—but rather on methods for producing coarser depictions of uncertainty, since these coarser depictions can more accurately reflect the limits of current understanding. It will be suggested below, however, that any influence that social values exert in the production of these coarser estimates will be reduced compared to their influence on precise probabilistic estimates.[10]

First, though, it is worth explaining why precise probabilistic estimates of uncertainty about future climate change are artificially precise. The short explanation is that scientists do not yet understand the climate system well enough; one must know a lot to be a position to say with justification that the probability (degree of belief) that should be assigned to a hypothesis is 0.38 rather than 0.37 or 0.39. Current scientific understanding is insufficient to constrain expectations to that extent.[11] A longer version of the same explanation can be given in terms of structural uncertainty, i.e. uncertainty about the form that climate model equations should take. Scientists are not yet in a position to confidently identify and represent in their modeling equations all of the physical, chemical and biological processes that will significantly shape the extent of climate change in response to increased greenhouse gas emissions. The importance of some processes and feedbacks is unclear, as is how to represent others that are known to be important, such as cloud formation. Moreover, there remains a non-negligible but difficult-to-quantify risk of "unknown unknowns"—causal factors of which scientists remain unaware but that nevertheless will influence the extent of climate change significantly. So structural uncertainty itself cannot be accurately represented with a single probability distribution (whether this is a distribution over model structures or over projected changes) and thus neither can uncertainty about future climate change, insofar as structural uncertainty is one component of it.[12]

That precise probabilities are overly precise is recognized in practice as well. Expert groups like the Intergovernmental Panel

[8] Due to limited computing power, climate models must have relatively coarse spatial resolution. As a consequence, small-scale processes like the formation of individual clouds must be represented in a simplified way in terms of larger-scale variables (i.e. parameterized), and it is rarely clear how this can best be done.

[9] Of course, on longer time scales even processes of the latter type (e.g. various ocean processes) might be extremely important, insofar as they are nonlinearly coupled to other processes.

[10] Just to be clear: I mean any influence of social values via the model-development process, in the ways suggested by Winsberg.

[11] Note that this means that there are no "true" single-valued probabilities to be estimated here, nor to be distorted by social values. Current knowledge simply doesn't entail such single-valued probabilities for hypotheses about future climate change; at best it entails something less precise. Some Bayesians epistemologists might reply that the assignment of precise probabilities is almost always artificially precise—Bayesian models that use precise probabilities are known to be idealized models. Fair enough. The point here can then be expressed as follows: these Bayesian models are too idealized for the present context.

[12] There are also reasons to think that neither parametric nor initial condition uncertainty can be accurately depicted with a single probability distribution, but exploring this is not important for the present discussion.

on Climate Change (IPCC), for instance, do not report uncertainties about future climate change—or even about most other climate-related hypotheses—in the language of single-valued probabilities, but rather in less precise terms (see IPCC, 2007). The IPCC's recent "Guidance Note . . . on the Consistent Treatment of Uncertainties" (Mastrandrea et al., 2010) emphasizes that IPCC authors should "evaluate and communicate uncertainty at the appropriate level of precision" (p. 2) and that the appropriate level will vary with the finding or hypothesis under consideration, depending on the evidence available.[13] According to the guidance note, where evidence is very limited, uncertainty may be more appropriately expressed in qualitative terms and, moreover, even when quantitative expressions of uncertainty are justified, an appropriate expression may take the form of an imprecise (or interval) probability specification (ibid, p. 3). In some cases, there may be "sufficient information" to justify an interval that is relatively narrow (e.g. 90–95% probability), but in many cases the interval probability assignment may need to be rather broad, with bounds that are themselves somewhat fuzzy/uncertain (ibid). The guidance also provides some "calibrated language"—it relates a set of terms to particular (fuzzy) probability intervals—to promote consistency throughout the report (see Table 1). Accordingly, if a finding or hypothesis is characterized as *likely* by the IPCC experts, this indicates an interval probability assignment of (approximately) 0.66–1.0.[14]

Fig. 1 demonstrates an application of such fuzzy, interval probabilities from the previous IPCC assessment report (IPCC, 2007). For each scenario shown, the IPCC experts deemed it *likely* that the actual temperature change would fall within a range extending from 40% below the mean of projections from state-of-the-art models to 60% above that mean. That is, they concluded that the hypothesis that the temperature change would be in that range could be assigned an imprecise probability of *at least 0.66*.[15] The discussion associated with the figure suggests that this uncertainty estimate was based on a number of relevant considerations, including the several ensemble studies whose results are also depicted in the figure, the experts' beliefs about the limitations of those studies (e.g. limitations in their treatment of carbon cycle uncertainties), as well as the experts' broader physical understanding of the climate system (see Meehl et al., 2007a, p. 810). The details of the process by which the specified ranges were determined to be *likely*, however, were not given.[16]

Unfortunately, such details often are omitted, making it difficult to evaluate the procedures used. In fact, at present, it is not entirely clear what the best-available methods for arriving at coarser estimates of uncertainty regarding future climate change are or could be. Nevertheless, whatever their details turn out to be, we can expect that they will prompt scientists to take into account all relevant data/results as well as any recognized limitations (i.e. biases, shortcomings, etc.) of the studies in which those data/results were produced. In effect, they will include an analogue of the move called for by the BRDC, but with more flexibility—they will require scientists to characterize the limitations of their evidence-gathering methods, but they will not require scientists to do so in the language of single-valued probabilities. This analogue of the BRDC may be within reach, even when the traditional BRDC is not. For instance, scientists might be able to articulate good reasons for considering it *unlikely* (i.e. probability ⩽ approximately 0.33) that the change in global mean surface temperature during the 21st century would be more than 2 °C outside the range projected by a particular ensemble of today's models, even if they are not in a position to say exactly how the distribution of results from those models would have been different if alternative (reasonable) choices had been made in the course of model development. Their reasons might appeal to basic physical understanding of the climate system, the performance of the models in simulating past climate changes, etc. Justification for this level of discrimination/precision in characterizing uncertainty might well be possible, even when it is not possible for much more refined discriminations.

Table 1
IPCC AR5 Likelihood Scale.

Term	Probability
Virtually certain	99–100%
Very likely	90–100%
Likely	66–100%
About as likely as not	33–66%
Unlikely	0–33%
Very unlikely	0–10%
Exceptionally unlikely	0–1%

Table 1. Adapted from Mastrandrea et al. (2010, Table 1).

It is now perhaps easier to see why, even if social values sometimes do come into play in the model development process in the ways suggested by Winsberg, the influence of those values on estimates of uncertainty will be reduced when coarser estimates are given. The influence will be reduced insofar as choices in model development will less often *make a difference* to the uncertainty estimates produced. Whereas any change to the distribution of modeling results in an ensemble study will change the single-valued probabilities estimated as a function of that distribution (assuming no fortuitous cancelation in the effects of the changes to the distribution), coarser estimates will be less sensitive to such changes, often much less so. Consider, for instance, the IPCC categories in Table 1. Even if projections from today's models for a particular quantity X were somewhat different, this would not necessarily move a hypothesis about X from one category to another, e.g., from *likely* to *very likely*. Indeed, given the recognized limitations of both today's climate models and the design of today's ensemble studies (see e.g. Parker, 2010; Tebaldi & Knutti, 2007), small or even moderate changes in the distribution of such projections *should not* have much impact in uncertainty analyses, at least not for many quantities; granting such impact would indicate a failure to appreciate those limitations.

5. A more traditional reply?

Having examined Winsberg's model-based challenge to the Jeffreyan view, it is worth noting that a more traditional reply to Jeffrey seems almost tailor-made for the discussion surrounding uncertainty estimates in climate prediction. This more traditional reply—outlined by Rudner (1953) and Douglas (2009) and others—focuses on second-order uncertainty and is untouched by the move from precise probabilistic to coarser uncertainty estimates.

[13] This guidance note is for the Fifth Assessment Report (AR5), which is still being drafted at the time of this paper's writing. A guidance note was also provided for the Fourth Assessment Report (AR4).

[14] Note that this does not mean that the experts assign a particular probability somewhere in the range 0.66–1.0 but rather that current knowledge does not allow them to favor any one of the probability values over the others in this range.

[15] It is noteworthy that, not infrequently, the "likely" range of temperature change for a scenario was broader than the ranges to which individual ensemble studies assigned a precise probability of 0.90. This is readily seen in Fig. 1: the 5–95% probability bounds from a number of ensemble studies are indicated by vertical lines, and many of these lines are contained entirely within the associated wide grey bar.

[16] The guidance provided for the AR5 emphasizes the importance of giving a traceable account of the evaluation of evidence that led to a particular uncertainty estimate (see Mastrandrea et al., 2010, p. 2).

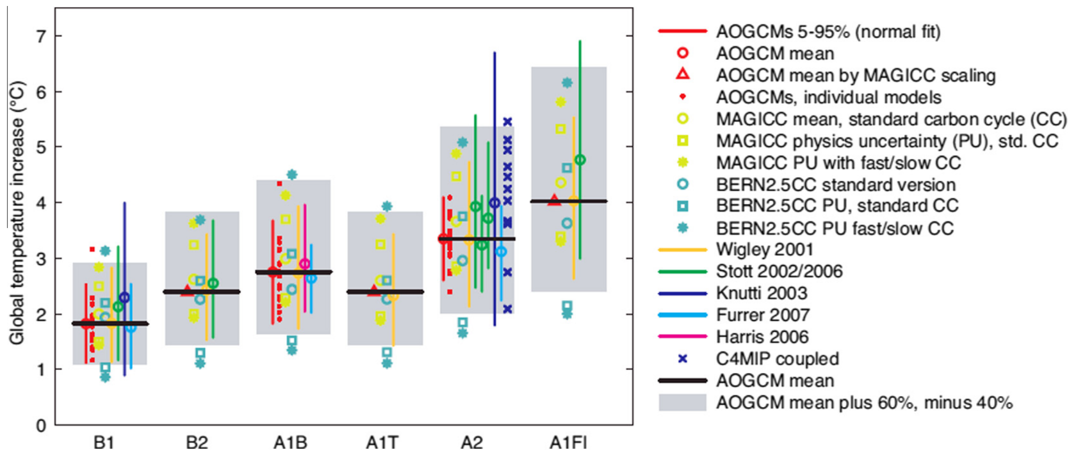

Fig. 1. Projections and uncertainties for global mean temperature change in 2090–2099 relative to the 1980–1999 average, under several emission scenarios and from numerous studies. The 5–95% ranges from probabilistic studies are indicated by solid vertical lines. The IPCC "likely" ranges are indicated by the wide grey bars. (Reprinted from *Climate Change 2007: The Physical Science Basis. Working Group I Contribution to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change*, Figure 10.29. Cambridge University Press.)

The reply goes roughly as follows: Estimates of uncertainty are themselves always somewhat uncertain; any decision to offer a particular estimate of uncertainty implies a judgment that this second-order uncertainty is insignificant/unimportant; but such a judgment is a value judgment, as it is concerned with (among other things) how bad the consequences of error (inaccuracy) would be; hence even decisions to offer coarser uncertainty estimates at least implicitly reflect value judgments (see e.g. Douglas, 2009, p. 85). A variation on this reply argues not just that uncertainty estimates always implicitly reflect value judgments but also that scientists ought to explicitly consider how bad the consequences of offering an inaccurate depiction of uncertainty would be and are remiss if they fail to do so (see ibid, chap. 4). Douglas (ibid.), for instance, argues for the latter by appeal to agents' general moral responsibility to consider the consequences of their actions.

These arguments are provocative in their own right and merit further examination. We might wonder, for example, whether it is possible to give an account of "doing the best one can" in gauging and representing one's uncertainty (including perhaps one's second-order or even higher-order uncertainty) such that, if one succeeds in "doing the best one can", then one does not have any further moral obligation to consider the consequences of error/inaccuracy in one's uncertainty estimates. However, this and other ways of trying to resist the traditional reply will not be explored in any detail here.[17] Instead, the aims of this section will be more modest: first, to show briefly how the traditional reply seems to dovetail with recent discussions—internal to climate science—concerning the offering of precise probabilistic estimates of uncertainty and, second, to draw attention to some more general issues connected with these discussions and debates in the climate context.

As noted above, the use of ensemble methods that deliver (what appear to be) precise probabilistic uncertainty estimates is increasingly common. In some cases, these methods are applied with the goal of informing real-world decision makers who are trying to plan for future climate change (see e.g. Murphy et al., 2009). Some climate researchers, however, have suggested that providing these precise probabilistic uncertainty estimates is inappropriate, first and foremost because current understanding is insufficient to warrant such precision, but in addition because of the adverse consequences that might result from offering overly precise estimates.[18] The possible adverse consequences that have been identified include both a loss of credibility for climate science and unnecessary harm to human populations, the latter as a result of decision makers taking at face value these probabilities and making poorer decisions than they would have made with coarser estimates that more accurately reflect the limits of current understanding. This at least suggests that some parties to the discussion not only share Douglas's view that, before offering uncertainty estimates, scientists ought to consider the consequences of inaccuracy but, in addition, believe that when it comes to expressing uncertainty about future climate change there are epistemically (and perhaps ethically) better options than precise probabilities.

A closer look reveals that at least some studies producing (what appear to be) precise probabilistic uncertainty estimates do explicitly acknowledge the existence of second-order uncertainty, but they nevertheless give confusing advice regarding the proper interpretation of study results. For instance, the extensive report accompanying one prominent ensemble study includes the following guidance:

> "Probabilistic projections, although they are designed to quantify uncertainty, require us to make a number of assumptions in their development, and hence they are themselves uncertain.... what may be an unacceptable uncertainty for one user may be quite acceptable for another application. However, as a general guideline we suggest that users should be able to use the distribution from the 10% to the 90% probability levels, but not outside this range, although data covering the full range is available. For some variables the limits may be more stringent than this" (Murphy et al., 2009, p. 92).

This gives the impression that users can take roughly at face value much of the probabilistic information produced in the study, which does not sit well with acknowledgements elsewhere in the report (e.g. p. 34) that some significant sources of uncertainty have not been taken into account.

In more general terms, the source of the difficulty here seems to be that sometimes experts are willing to *offer* uncertainty

[17] At least two philosophers have expressed doubt that the traditional reply is even a legitimate reply to Jeffrey. Mitchell (2004) sees a problematic conflation of belief and action. Winsberg (personal communication, 4 May 2013) argues that worries about second-order uncertainty would be moot for Jeffrey, insofar as Jeffrey insists that individuals always do have precise degrees of belief, which can be revealed by their betting behavior. Winsberg's model-based argument is intended to create trouble for Jeffrey even granting his assumption that individuals always have precise degrees of belief.

[18] The latter concerns have been expressed more often in talks and in informal discussion than in print, but see e.g. Stainforth et al. 2007 and Smith 2009.

estimates that they nevertheless are not willing to fully *own*, i.e. that they would agree are (in one or more ways) inaccurate depictions of the extent to which current understanding can constrain their expectations about future climate change (see also Parker, 2010; Rougier & Crucifix, 2012; Winsberg, 2012). In the climate context, this is likely occurring in part because, while methods for producing (overly) precise probabilistic uncertainty estimates are available, implementable, and deliver quantitative results in a familiar form, methods for producing coarser estimates with the help of expert judgment remain in their infancy, are unfamiliar, and may be perceived as lacking in rigor. Thus, even if it is recognized that uncertainty about future climate change would be more accurately characterized with some or other coarser estimate, it may seem easier to apply existing methods that deliver precise probabilities. Further work is needed on how to handle situations in which the ostensible results of an uncertainty estimation study are not ones that the researchers conducting the study can fully own and, in the bigger picture, how to more easily conduct studies that do allow for ownership of results.[19]

6. Conclusions

Winsberg has opened an important discussion regarding the extent to which social values are operating in the ‘‘nooks and crannies’’ (Winsberg, 2012, p. 130) of complex simulation modeling. This paper raised two objections to his argument that social values unavoidably influence estimates of uncertainty about future climate change. First, the argument overlooks the possibility (indeed the plausibility) that pragmatic factors often determine epistemically unforced choices in model development; it need not be social values that ‘‘fill the gap’’ when making such choices. Second, the argument’s focus on precise probabilistic uncertainty estimates is somewhat misplaced since, given the limitations of current understanding, uncertainty about future climate change is more appropriately depicted with coarser estimates. Moreover, we can expect that coarser estimates will be less sensitive to any influence that social values have via the model-development process. Neither of these objections shows Winsberg’s conclusion to be mistaken, but together they suggest that, if there is some unavoidable influence of social values (via the model development process) here, it is not as pervasive as Winsberg’s argument implies.[20]

Winsberg’s model-based challenge to the Jeffreyan view is a novel one. The discussion revealed, however, that debate surrounding the estimation of uncertainty in climate prediction—with its highlighting of second-order uncertainty and its concern about the negative consequences of offering erroneous/inaccurate uncertainty estimates—also hearkens back to earlier replies to Jeffrey given by Rudner, Douglas and others. In doing so, it calls attention to the fundamental issue of ownership in uncertainty estimation: we want methods for estimating and representing uncertainty that accurately communicate the limits of current knowledge. But whether such methods are within reach in the case of climate prediction, what they might be like in their details, and even what would count as accurate enough, remains to be seen.

Acknowledgements

Thanks to Leonard Smith, Eric Winsberg and two anonymous referees for very helpful feedback on earlier drafts. Thanks also to participants of meetings and colloquia where earlier versions of this material were presented, including those at the University of Cincinnati, the 2011 APA Eastern Division meeting and the 2012 Values and Norms in Modeling conference.

References

Biddle, J., & Winsberg, E. (2009). Value judgments and the estimation of uncertainty in climate modeling. In P. D. Magnus & J. Busch (Eds.), *New waves in the philosophy of science* (pp. 172–197). Palgrave Macmillan.
Douglas, H. (2000). Inductive risk and values in science. *Philosophy of Science, 67*, 559–579.
Douglas, H. (2009). *Science, policy, and the value-free ideal*. Pittsburgh University Press.
Frame, D. J., Aina, T., Christensen, C. M., Faull, N. E., Knight, S. H. E., Piani, C., et al. (2009). The climateprediction.net BBC climate change experiment: Design of the coupled model ensemble. *Philosophical Transactions of the Royal Society A, 367*, 855–870.
IPCC (2007). Climate change 2007: The physical science basis. In S. Solomon, D. Qin, M. Manning, Z. Chen, M. Marquis, & K. B. Averyt, et al. (Eds.), *Contribution of working group I to the fourth assessment report of the intergovernmental panel on climate change*. Cambridge University Press.
Jeffrey, R. (1956). Valuation and acceptance of scientific hypotheses. *Philosophy of Science, 22*, 237–246.
Mastrandrea, M. D., Field, C. B., Stocker, T. F., Edenhofer, O., Ebi, K. L., Frame, D. J. et al. (2010) *Guidance note for lead authors of the IPCC fifth assessment report on consistent treatment of uncertainties*. Intergovernmental Panel on Climate Change (IPCC). Available at: <http://www.ipcc.ch>.
Meehl, G. A., Stocker, T. F., Collins, W. D., Friedlingstein, P., Gaye, A. T., Gregory, J. M., et al. (2007a). Global climate projections. In S. Solomon et al. (Eds.), *Climate change 2007: The physical science basis. Contribution of working group I to the fourth assessment report of the Intergovernmental Panel on Climate Change* (pp. 747–845). Cambridge University Press.
Meehl, G. A., Covey, C., Taylor, K. E., Delworth, T., Stouffer, R. J., Latif, M., et al. (2007b). The WCRP CMIP3 multimodel dataset: A new era in climate change research. *Bulletin of the American Meteorological Society, 88*, 1383–1394.
Mitchell, S. D. (2004). The prescribed and proscribed values in science policy. In P. Machamer & G. Wolters (Eds.), *Science, values, and objectivity* (pp. 245–255). Pittsburgh University Press.
Murphy, J., Sexton, D., Jenkins, G, Boorman, P., Booth, B., Brown, K. et al. (2009) *UK climate projections science report: Climate change projections*. MetOffice Hadley Centre, Exeter. Available online at: <http://ukclimateprojections.defra.gov.uk/22565> Accessed September 30, 2012.
Parker, W. S. (2006). Understanding pluralism in climate modeling. *Foundations of Science, 11*, 349–368.
Parker, W. S. (2010). Whose probabilities? Predicting climate change with ensembles of models. *Philosophy of Science, 77*(5), 985–997.
Parker, W. S. (2013). Ensemble modeling, uncertainty and robust predictions. *WIREs Climate Change*. http://dx.doi.org/10.1002/wcc.220.
Rougier, J. C. & Crucifix M. (2012), Uncertainty in climate science and climate policy. Submitted for publication In E. Lloyd & E. Winsberg (Eds.), *Conceptual issues in climate modeling*, University of Chicago Press. Article available online at: <http://www.maths.bris.ac.uk/~MAZJCR/climPolUnc.pdf> Accessed September 30, 2012.
Rudner, R. (1953). The scientist qua scientist makes value judgments. *Philosophy of Science, 20*, 1–6.
Smith, L. A. (2002). What might we learn from climate forecasts? *Proceedings of the National Academy of Sciences, 99*, 2487–2492.
Smith, L. A. (2009) ‘‘*Toward decision-relevant probability distributions: Communicating ignorance, uncertainty and model-noise*’’. Power Point slides available online at: <http://www.rmets.org/sites/default/files/pdf/presentation/20091015-smith.pdf> Accessed September 30, 2012.
Stainforth, D. A., Aina, T., Christensen, C., Collins, M., Faull, N., Frame, D. J., et al. (2005). Uncertainty in predictions of the climate response to rising levels of greenhouse gases. *Nature, 433*, 403–406.
Stainforth, D. A., Allen, M. R., Tredger, E. R., & Smith, L. A. (2007). Confidence, uncertainty and decision-support relevance in climate predictions. *Philosophical Transactions of the Royal Society A, 365*, 2145–2161.
Taylor, K. E., Stouffer, R. J., & Meehl, G. A. (2012). An overview of CMIP5 and the experiment design. *Bulletin of the American Meteorological Society, 93*, 485–498.
Tebaldi, C., & Knutti, R. (2007). The Use of the multi-model ensemble in probabilistic climate projections. *Philosophical Transactions of the Royal Society A, 365*, 2053–2075.
Winsberg, E. (2010). *Science in the age of computer simulation*. Chicago University Press.
Winsberg, E. (2012). Values and uncertainties in the predictions of global climate models. *Kennedy Institute of Ethics Journal, 22*(2), 111–137.

[19] Winsberg (2012, p. 129 and Fn. 17) seems to call into question the very idea of ownership when dealing with groups rather than individuals. Perhaps a somewhat different notion will be needed.

[20] It is interesting to consider how the extent of unavoidable influence might change if understanding of the climate system were to advance to the point that (rather) precise probability estimates could be justified in principle. But it is difficult to draw any firm conclusions without also knowing something about what computer power and modeling capabilities would be like at that time. Thanks to Isabelle Peschard for prompting me to think about this.