Ensembles as Evidence, Not Experts: On the Value and Interpretation of Climate Models

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[Draft; comments extremely welcome. Please contact at corey.dethier[at]gmail.com before citing or quoting.]

Abstract

Climate scientists frequently interpret climate models as providing probabilistic information, a practice that has come under substantial criticism from philosophers of science. In this paper, I argue that this practice has (previously unacknowledged) advantages. In particular, though the literature has focused on the use of probabilities in communicating results, climate scientists regularly treat probabilities generated by models not as the final products of research but instead as evidence or as an intermediate step in a longer reasoning process. In these cases, inter-model variation provides important information about the amount of uncertainty that is warranted by the evidence—information that can only be captured in some sort of probability distribution. Even if we accept extant arguments against the probabilistic interpretation of climate models in the context of communication, therefore, the advantages of the probabilistic interpretation of climate models in other areas makes it a substantive question whether those arguments can be extended to the more general case.

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0 Introduction

Climate scientists frequently employ groups or "ensembles" of climate models when evaluating hypotheses about the past, present, and future climate. In many cases, they interpret the results given by these ensembles as providing probabilistic information—that is, they treat the variation between the different members of the ensemble as providing evidence about the probability of various alternative scenarios. Philosophers have written extensively about both these "ensemble-based methods" and the probabilities that they generate, most of it quite critical: though the details of the arguments differ, the broad consensus within philosophy seems to be that extant ensembles are not properly "independent" in the way that they would need to be to (e.g.) apply statistics to them, and thus that the probabilistic results of such applications are not worthwhile.[1]

Most of the critical literature has focused exclusively on the use of probabilities in communicating the results of climate science to policy-makers or the public. Communication is not a particularly representative use of ensemble-based methods and the probabilities that they generate, however. On the contrary, climate scientists regularly employ the probabilities generated by models

[1] For examples, see Betz (2007, 2015), Carrier and Lenhard (2019), Jebeile and Barberousse (forthcoming), Katzav (2014), Katzav et al. (forthcoming), Parker (2010a,b, 2013), Parker and Risbey (2015), and Winsberg (2018). There is to my knowledge only one paper that explicitly defends the practice (Dethier forthcoming).

not as the final products of research but instead as evidence or as an intermediate step in a longer reasoning process. In these contexts, the probabilistic interpretation of ensembles is crucial, because the variation between models provides important evidence about how we should distribute our confidence over various possibilities. Not only is there good philosophical motivation for paying attention to this information, but methods that make use of it have been shown in at least some cases to be more accurate and reliable than methods that don't.

Interpreting climate models as providing probabilistic information thus plays an important role in climate science—there are distinct advantages to this approach and no good alternatives. The upshot: even if we accept extant arguments against the probabilistic interpretation of climate models in the context of communication, it is a substantive question whether those arguments can be extended to the probabilistic interpretation of climate models in general.[2] Whether or not climate scientists should adopt a probabilistic interpretation of climate models in a particular context depends on the costs and benefits of doing so relative to the available alternatives, and the literature simply has not demonstrated that the alleged costs outweigh the very real advantages.

In more detail, the plan is as follows. Section 1 discusses a general problem for simulations made using of climate models, namely that such simulations fail to uniquely support a single posterior probability function. Section 2 shows how moving to the probabilistic interpretation of an ensemble of models mitigates this problem. Finally, section 3 argues that there's no good alternative to the probabilistic approach in these cases.

Two quick notes. First, in what follows, I'll focus on a paradigm case of ensemble-based methods, namely the application of statistics to the set of results generated by simulations run on an ensemble of climate models. As I'll stress, however, what the arguments really motivate is not the use of this particular method but rather merely that we use some method that takes the variation between model results into account. There are worthwhile debates to be had about which ensemble-based methods (in this broad sense) climate scientists should use, and I want to leave the door open for other approaches so long as they take account of inter-model variation in some way.

[2] The extent to which the prior literature is interested in the more general case is an open question. Certainly, Betz (2007), Parker (2010b), and Parker and Risbey (2015) are explicitly narrowly constrained to the question of communication. By contrast, Katzav et al. argue that precise probability functions "should not be used in the climate context" (Katzav et al. forthcoming), and there is nothing in the text to indicate whether they in fact intend a narrower claim.

Second, as may already be clear, I don't intend the arguments given in this paper to settle the issue of whether climate scientists should employ a probabilistic interpretation of climate models (though, cards on the table, I think they should). As such, I won't be considering all of the arguments against the probablistic interpretation. This is partly simply a matter of space and partly because those philosophical arguments that might be thought to apply beyond the communicative context have been addressed by Dethier (forthcoming). Mostly, though, it's because I think the best versions of those arguments have components that I'm not in the best position to evaluate—in particular, the questions to ask are how severe the misrepresentations are in practice and how we should weigh the advantages of the probabilistic representation against the risks.

1 Climate modeling and imprecision

1.1 Using climate models to evaluate hypotheses

Here I briefly outline how climate models are used in evaluating hypotheses about the (future) climate. To make the discussion more concrete, consider equilibrium climate sensitivity (ECS), the °C change in temperature that will be observed given a doubling of the atmospheric CO_2 concentration.

Were we Bayesian rational agents in an ideal situation, our estimate for ECS would consist in a precise probability distribution over possible values of ECS, and this distribution would be generated by conditionalizing our prior expectations on the total evidence. In practice, of course, this isn't feasible.[3] We're rarely if ever in a position to directly employ our total evidence in evaluating hypotheses. It's not as though we have access to a complete description of all of the evidence collected up to this point, let alone an understanding of the probabilistic relationships between that description and various hypotheses. Instead of calculating the probability of a hypothesis like ECS = 2.5°C directly on the total evidence, therefore, scientists build theories and models that systematize the evidence as well as possible. These theories and models then tell us what we should believe about the future.

[3] It's common for (Bayesian) epistemologists to wave away concerns about feasibility by pointing out that the relevant standards are "evaluative" rather than "normative." Fair enough. Science isn't concerned with the reasons that an agent might have in an abstract evaluative sense, however, but instead with the reasons that can be made intersubjectively salient (Longino 1990): you have to be able to demonstrate to other people that a hypothesis is warranted. Feasibility questions—e.g., can your evidence be communicated?—are thus relevant to philosophy of science in a way that they (arguably) aren't to (ideal) epistemology.

In the context of climate science, the relevant models are usually global climate models. We can think of global climate models as consisting of a number of gridded shells, with each shell representing a layer of the atmosphere and each grid box a location in that layer. Each grid box is assigned a number of climate variables, representing (e.g.) the average temperature and precipitation in that region over the course of a time-step (say, a month). The relationships between the variables found in various grid boxes are given by a series of equations that determine how a change in the climate variables of one box affects other variables in that box as well as the variables in its neighbors. At the simplest level, quantities like heat will simply defuse through the system, but of course there are more complicated effects as well.[4]

To use a global climate model in estimating a quantity like ECS, climate scientists run computer simulations in which the model is "forced" to take on a new state by an exogenous change; in the case of ECS, for instance, one standard procedure is to rapidly double the amount of CO_2 in the (simulated) atmosphere.[5] Comparing the end-state of the simulation to the initial state yields a point-value quantity for the change in average temperature *in the model*. So suppose that the in-model change in average temperature, represented by $\Delta\bar{T}$, is 2.5°C. In what follows, I'll speak of a model "saying" or "reporting" that $\Delta\bar{T} = 2.5$°C. The idea here is that the "model report" is akin to an "instrumental reading": the quantity that our computer simulation spits out is *like* the quantity that we read off a thermometer. It's a data point to be recorded and interpreted and which our hypotheses about the climate will be expected to account for (compare Parker 2020).

In what follows, I'll often speak about the interpretation of model reports in Bayesian language (though nothing hangs on this particular choice of framework). In this framework, "interpreting" model reports means conditionalizing on them in accordance with Bayes' rule. In our ECS example, that means that the probability that we (should) assign to a hypothesis like ECS = 2.5°C on the basis of the model report that $\Delta\bar{T} = 2.5$°C is given by

$$\begin{aligned} Pr^*(\text{ECS} = 2.5) &= Pr(\text{ECS} = 2.5 \mid m : \Delta\bar{T} = 2.5) \\ &= \frac{Pr(\text{ECS} = 2.5)Pr(m : \Delta\bar{T} = 2.5 \mid \text{ECS} = 2.5)}{Pr(m : \Delta\bar{T} = 2.5)} \end{aligned}$$

[4] The picture I've presented here is simplified in a number of ways. For a deeper discussion, see a climate modeling primer such as Gettelman and Rood (2016) and McGuffie and Henderson-Sellers (2014). For a philosophical introduction, see Winsberg (2018, 27–54).

[5] These simulations are only one of the different ways that climate scientists estimate ECS. For a comparison, see IPCC (2013, 922–923, 1110).

where "$m : \Delta\bar{T} = 2.5$" indicates that the model m is reporting that $\Delta\bar{T} = 2.5$°C. The crucial point is that what we take from the model report depends on how well (we think) the model is tracking the truth, or, in the Bayesian framework, on the likelihood ratio.

1.2 The problem of imprecise evidence

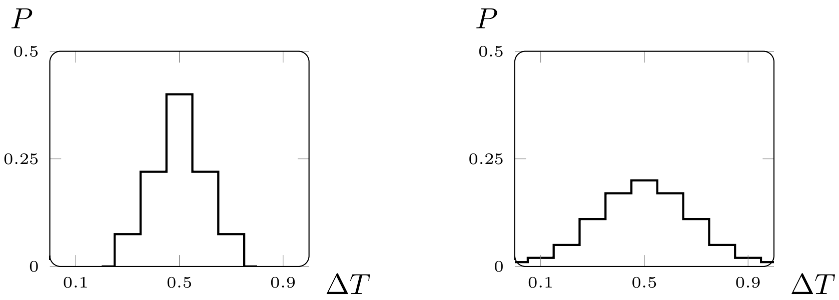
It is uncontroversial that climate models are not perfect: they misrepresent or idealize some real climate processes, omit or paramaterize others, and rely on assumptions that are risky or arbitrary in the sense that we don't know whether they're true.[6] It is also uncontroversial that climate models are (relatively) "opaque" in the sense that it is hard to tell how any one idealization affects the accuracy of the model with respect to a variable of interest.[7]

The upshot of these two facts is that climate scientists are rarely (if ever) in a position to know exactly how to interpret a given model report—in our Bayesian framework, they don't know the likelihood of a given model report on different hypotheses. It's helpful to be slightly more concrete. So suppose that our model generates a report of $\Delta\bar{T} = 2.5$°C. For the sake of simplicity, suppose further that we know that the likelihood of observing different values for $\Delta\bar{T}$ given some hypothesis h is given by a normal distribution centered on the truth. Essentially: we know that the hypothesis on which our model report has the highest likelihood is ECS $= 2.5$°C, and that the likelihood of the model report falls off as we move to hypotheses that assign more distant values to ECS. The *sole* question in this simplified example is how quickly the likelihood falls off. If the normal distribution has a standard deviation of .25, our confidence in different values of ECS will look like the graph pictured in figure 1a. And if the normal distribution has a standard deviation of .5, our confidence in different values of ECS will look like the graph pictured in figure 1b. Insofar as we're uncertain about which assumption about the model's accuracy we ought to adopt, we'll be equally uncertain about which distribution we should prefer.

Situations like this one are sometimes described by epistemologists as involving "imprecise evidence" (see, e.g., Carr 2019). That terminology can be confusing, however. It's not the case that the datum that we're conditioning on is itself imprecise; on the contrary, the model's reported value for $\Delta\bar{T}$ can be

[6] See IPCC (2013, chapter 9). The point is also widely acknowledged by philosophers: see Carrier and Lenhard (2019), Jebeile and Barberousse (forthcoming), and Parker (2010a).

[7] The terminology of "opacity" is owed to Humphreys (2004); for discussions of opacity in the context of climate models, see Carrier and Lenhard (2019), Lenhard and Winsberg (2010), and Parker and Winsberg (2018).



(a) Expected model accuracy is given by $P(m : \Delta T = x|\Delta T = .5) \sim \mathcal{N}(.5, .1)$.

(b) Expected model accuracy is given by $P(m : \Delta T = x|\Delta T = .5) \sim \mathcal{N}(.5, .2)$.

Figure 1: Posterior probability distributions for values of ECS at the $1/4^{\text{th}}$ of a °C level induced by different views about likelihoods. Priors assumed to be identical and uniform.

calculated with as much precision as we like. Instead, the problem is that we're not in a position to justify a precise interpretation of the model report—we can't pick out a single probability function as *the* assignment of probabilities that the report supports.

Here's another way at getting at this contrast. Evidence can warrant more or less precise conclusions in at least two different senses. On the one hand, the evidence can warrant more or less precise conclusions in the sense of ruling out possible values for a quantity. To illustrate, contrast learning the proposition that [[ECS falls between 0.5 and 4.5°C]] with learning the proposition that [[ECS falls between 1.5 and 3.5°C]]. The latter rules out more possible values for ECS and is thus more precise in what we might call a first-order sense.

On the other hand, the evidence can warrant more or less precise conclusions in the sense of ruling out possible *distributions* over values. The most familiar (but not the only) way to understand this higher-order sense of precision is in terms of what are called imprecise probability distributions.[8] So, in a standard Bayesian framework, updating your priors $Pr(\cdot)$ on a piece of evidence E yields a single preferred posterior probability function $Pr^*(\cdot) = Pr(\cdot|E)$. That is: the standard Bayesian framework treats all evidence as maximally precise in that it only allows for a single probability distribution. In the example we

[8] For an overview, see Bradley (2019) and Mahtani (2019). The alternative that I have in mind replaces imprecise probability's sets of functions with a modal frame and the accompanying access relations (see Dorst 2019; Dorst et al. forthcoming). The differences between these two approaches shouldn't matter for the present discussion.

saw above, however, our uncertainty about likelihood functions meant that we were uncertain about which of two posterior probability functions to adopt—rather than a single probability function Pr^*, we had a set of them $\{Pr_1^*, Pr_2^*\}$. In this example, E is less than maximally precise in a higher-order sense: it doesn't rule out all but one distribution over the possible values.

To summarize, climate modeling—or at least the project of using climate models to estimate quantities like ECS—faces a problem. Due to the heavily idealized and relatively opaque nature of climate models, climate scientists often don't know the likelihood of a given model report on different hypotheses about quantities of interest. Their uncertainty about likelihoods renders the evidence provided by the model report *imprecise* in the second-order sense just sketched: the evidence allows for a variety of possible distributions over different values for ECS.

Generally speaking, imprecision in our evidence is undesirable: we prefer to be in situations where the evidence warrants more precise hypotheses rather than those in which it only warrants less precise ones. This general preference holds regardless of what sense of precision is at issue and is particularly acute in climate science. As is widely discussed in the scientific literature, the available evidence places much tighter bounds on the low end for ECS than on the high end. Given that higher values for ECS represent relatively disastrous scenarios, however, practical questions concerning (e.g.) what CO_2 concentrations we should aim to stay beneath are highly sensitive to the probability distribution over various unlikely high-end options (Weitzman 2012). In climate science, therefore, imprecise evidence is not just undesirable in an abstract sense—it presents a genuine practical problem.

In the next section, I'll argue that ensembles of models *help*: they provide evidence that is *more* precise than the evidence provided by a single model.

2 From a model to an ensemble

2.1 Ensemble-based methods: a primer

As we saw above, one way that climate scientists estimate quantities like ECS is by running a simulation on a climate model to generate what I've called a "model report," which are like instrumental readings in the sense that they are evidence that needs be "interpreted." (We modeled this "interpretation" step with Bayesian updating, but we could represent in other ways.) "Ensemble-based methods" proceed along largely the same lines: the same simulation is run on each of the models in the ensemble, generating a set of model reports.

The crucial difference is that scientists do not reason from or interpret the individual model reports directly; instead, they reason using the features of the set of model reports as a whole.

The standard method for turning the set of reports generated by an ensemble into evidence involves employing statistics. In short, this means assuming that the set of reports behaves *as though* it were drawn from some sort of population according to a given sampling procedure. In the simplest case, for instance, climate scientists might assume that the reports behave as though they were randomly drawn from a population centered on the truth. Or (more realistically), they might assume that each of the members of the ensemble is an equally realistic representation of the true climate and thus that the ensemble behaves like a random sample from a population that contains the truth as one of its members.[9] These assumptions are essentially qualitative ways of fixing what's called a "statistical model," a set of assumptions about the probabilistic relationship between various hypotheses and the observed data (i.e. the model reports). Given a statistical model, the observed reports can be used to generate a probability distribution over various alternatives, and these probability distributions (again, as opposed to the individual model reports) are then what climate scientist employ in making judgments about how much confidence we should assign to various hypotheses.

It's worth being a little bit more concrete here. So consider the second of the two assumptions given above. Essentially, this assumption amounts to the stipulation that for any temperature x, the probability that ECS $= x$ is equivalent to the probability that an arbitrary model generates a report that $\Delta\bar{T} = x$. Given this stipulation, the likelihood of observing a given set of model reports on the assumption that ECS $= x$ is just the probability of drawing a report that $\Delta\bar{T} = x$ from the same distribution that characterizes the (imagined) population. So, for instance, if the underlying population is normally distributed, then the likelihood of observing a sample with mean $\Delta\bar{T}$ of 2.5 and standard deviation of .25 on the assumption that ECS falls between 2 and 3 is given by:

$$\begin{aligned} Pr(\mu = 2.5, \sigma = .25 \mid 2 < \text{ECS} < 3) &= Pr(2 < m_i < 3 \mid \mu = 2.5, \sigma = .25) \\ &= \int_2^3 \frac{1}{.25\sqrt{2\pi}} e^{-\frac{1}{2}\left(\frac{z-2.5}{.25}\right)^2} dz \end{aligned}$$

We can then use these likelihood assignments as part of either a Bayesian updating procedure or a classical hypothesis test—climate scientists use both

[9] The contrast between these two options is discussed at length in Annan and Hargreaves (2010, 2011) and Sedláček and Knutti (2013).

approaches, though the classical one is currently much more popular.

There are three points that I want to stress before moving on to a discussion of how the use of ensembles *helps* address the problem identified in the last section. First, what I'll be arguing below is that the proper parallel to draw here is not between the reports generated by the ensemble and the individual model report but rather between the probability distribution generated by the ensemble-based method and the individual model report. That is: the probability distributions need to be "interpreted" in the same way that an individual model reports does. In this respect, the ensemble-based method is no different from a method based on a single model. What differs between the two cases is that the probability distribution provides us with information that a single model report doesn't. In particular, there's no analogue of the variance (the second moment of the distribution of ensemble results) in the single-model case.

Second, to reiterate a point from the introduction, the ensemble-based method I've sketched here is simply a paradigm case of the most popular approach, and other approaches are possible. Some climate scientists have experimented with interpreting ensembles by weighting the different members and taking their weighted average (Knutti et al. 2017; Sanderson, Knutti, and Caldwell 2015); alternatively, some philosophers have suggested a pooling approach that employs imprecise probabilities and explicitly accounts for the stakes in interpreting the ensemble (Roussos, Bradley, and Frigg 2021). Which of these approaches is best is an interesting question that I don't want to address here; as we'll see, my contention is solely that there are good reasons for climate scientists to use some ensemble-based method that takes account of the variation between the different model reports. In other words, climate scientists should interpret ensembles in a "probabilistic" manner. My arguments leave room for disagreement about how to calculate (and use) the relevant probabilities, when and where to coarse-grain or invoke "imprecise" probabilities, but not as to whether the interpretation should be probabilistic.

Finally, as should already be clear, the move to an ensemble doesn't solve the problem of the last section. Recall: in a Bayesian framework, the problem is that we don't know the likelihood relationship between the observed data and various hypotheses. Exactly the same problem arises here, as illustrated above: the debates about which statistical model we should employ in interpreting ensembles are essentially debates about the proper assumptions to make about the likelihood of observing a particular distribution of model reports. The upshot is that the probability distribution generated by an ensemble-based method counts as "imprecise evidence" in the same sense that a single model report does: both allow for a variety of posterior probability distributions over different values for ECS.

2.2 Ensembles and precise evidence

Nevertheless, moving to an ensemble *helps*. The easy way to illustrate this point is by considering the simplified example of the last section. There, we stipulated that the likelihood function—that is, the probability of observing a model report of $\Delta\bar{T} = x$ given a hypothesis ECS $= y$—was given by a normal distribution centered on the truth. Unfortunately, even given this strong assumption, a single model report just doesn't provide us with any information to narrow down the class of possible distributions. In other words, even when we know that the distribution is normal, a single model doesn't tell us how wide or narrow we should expect the normal distribution to be.

An ensemble does. Given the assumption that the likelihood function is given by a normal distribution centered on the truth, an ensemble will allow us to pick out a preferred distribution over possible values for ECS. The crucial difference between the two cases is the inter-model variation, which provides information about the width of the likelihood function: the more variation there is in the sample, the wider we should expect the normal distribution that represents the likelihood function to be. So, just to be concrete, in this case, the likelihood function for an arbitrary model report m_i would be given by a probability density function, meaning that we can calculate the likelihood of any observed report as follows

$$Pr(x < m_i < y \mid \text{ECS} = \mu) = \int_x^y \frac{1}{\sigma\sqrt{2\pi}} e^{-\frac{1}{2}\left(\frac{z-\bar{m}}{\sigma}\right)^2} dz$$

where $\bar{m}$ is the ensemble mean and σ is given by:

$$\sigma = \sqrt{\frac{1}{n-1}\sum_{i=1}^{n}(m_i - \bar{m})^2}$$

with n being the number of models in the ensemble.

Of course, we know that extant ensembles aren't actually like random samples from a normal distribution centered on the truth—indeed, they don't approximate such random samples terribly well (Knutti et al. 2010)—and so the assumption just outlined is not in fact warranted. That's why, as stressed above, we cannot say that ensembles solve the problem of imprecise evidence. But they do help: the variation between ensembles allows us to narrow the range of plausible likelihood functions and thus to rule out as implausible some possible posterior probability distributions over values of ECS. Returning to the language of imprecise probabilities, we can think of single-model methods as delivering a large set of permissible probability functions and ensemble-based methods as delivering a (strictly) smaller set of such functions. Insofar as we desire less imprecision (all other things being equal), we should prefer the ensemble-based method.

What's going on here is basically that inter-model variation provides what epistemologists term *higher-order evidence*. Both a single model and an ensemble provide us with an estimate for the true value of ECS—the model report on the one hand and the mean of the distribution on the other. What the ensemble provides, in addition, is variation between ensemble members, which serves as higher-order evidence concerning how accurate we should expect this estimate to be; all other things being equal, the more variation, the less we should trust the mean as an estimator. The information provided by this variation is what's gained by shifting from the single point-value report to the distribution generated by the ensemble-based method. Of course, the relevant distribution is itself imperfect—it may be misleading or inaccurate in the same way that the first-order evidence may be—but the mere possibility of these sorts of problems doesn't mean that it's not useful.

Here's a slightly different way of making the point. Suppose there's a set of mutually exclusive propositions $\{P, Q, R, ...\}$ and that we don't know exactly what probability we should assign to each of them. One way to proceed here is to adopt the one that seems most likely to be true and to treat it as true—e.g., to work under the assumption that $Pr(P)$ (say) is equal to 1. Later on, we can qualify our results to address the fact that our work was carried out under this risky assumption. Obviously, this isn't an ideal strategy, but it's essentially the strategy that we employ when using a single model. We're taking our "best guess" at how to represent the world, proceeding as though it's entirely accurate, and then keeping our concerns about reliability in mind when updating on the resulting report.

In this analogy, using an ensemble-based method is akin to adopting a more equitable distribution of probabilities over the set. So, for example, if our models are equally divided between P, Q, and R and we weight each model equally, that's equivalent to assigning each proposition a probability of 1/3. Since we don't know what probability distribution our total evidence warrants in this case, the resulting probability distribution may be misleading—just as in the single model case, we shouldn't update on the results without taking our concerns about reliability into account. Importantly, however, the ensemble allows us to build *some* of our concerns about reliability into the method itself: the ensemble-based approach accounts for the possibility of error due to assuming P rather than Q or R. As a consequence, the ensemble and the probability functions that it generates are likely to be better at capturing what we should believe than the single-model and its point-prediction. Essentially, the latter requires us to assign 100% of our confidence to one option, while the former allows us to adopt confidence distributions that more closely track our true confidence. The end result is information that requires less qualification than that provided by a single model. Adopting a particular distribution over the set is risky, but the risk is less substantial than in the single-model case.

The important takeaway is that ensemble-based methods mitigate the problem of imprecise evidence outlined in the last section in virtue of the fact that there's variation between ensemble members that isn't present in a single model. Or, in plainer English, methods that make use of the differences between models provide us more guidance about how to distribute our confidence than methods that don't.

2.3 The concrete benefits of variation

The advantages of ensemble-based methods are not merely philosophical. Of course, it's widely recognized that ensemble means are generally more accurate than the report generated by any single model. The point is made explicitly by the empirical work on ensembles that is commonly cited in the philosophical literature (see, e.g., Knutti et al. 2010) and is in some sense unsurprising: basically any average of a set of estimates will have a higher expected accuracy than the individual estimates (Roussos 2020, 119–20).[10]

In saying that the advantages of ensembles are not merely philosophical, however, I don't have this kind of increase in accuracy in mind. After all, my claim in this section is that the variation between ensemble members provides valuable information—it's open for a critic to argue that ensemble means are valuable but that the variation between ensemble members isn't. I'm going to wrap up this section by arguing that that position is wrong: variation between ensemble members provides climate scientists with concrete advantages over and above the advantage of having a point-value estimate with higher

[10] That said, I think the increase in accuracy gained by averaging is underappreciated. As Annan and Hargreaves (2011) argue, the degree to which ensemble means outperform individual models is not fixed by abstract mathematical considerations and demands explanation. To me, this looks like a problem for the critic of ensemble-based methods: the *surprising* accuracy of ensemble averages looks like empirical disconfirmation of the view that ensembles are too "opportunistic" to be useful. My thanks to Joe Roussos for pointing out to me that the mathematical property here holds not just for means but for a wide variety of averages.

expected accuracy. Briefly, the reason why is that the estimates generated by climate models aren't just treated as ends in themselves but are frequently used as parts of longer and more complex strings of reasoning. In these contexts, the variation between ensemble members is crucial: even adopting the ensemble mean in these longer chains of reasoning introduces an additional source of error that climate scientists avoid through the use of ensemble-based methods—in short, it's like rounding in the middle of a calculation.

Our running example can be used to illustrate this point. As noted above, ECS is estimated in a wide variety of ways. So far, we've focused on direct estimates generated by running a simulation on a climate model or set of models that generates a value for the change in temperature. One of the other ways that climate scientists estimate ECS involves using temperature data to estimate the effect that past increases in CO_2 have had on temperature and then extrapolating from those results.[11]

Speaking roughly, this method of estimating ECS works in the following way. Climate scientists collect substantial data on past changes to temperature and then run complex regressions to determine how much of the past temperature change can be attributed to CO_2 and how much to other factors such as the interval variability of the climate system. To run these regressions, we need a quantified understanding of how different factors affect the climate. So, for example, we consistently observe that while the planet as a whole is warming, the upper atmosphere is actually cooling. To determine how much of the observed warming is caused by CO_2 and how much by other factors, we need to know how these different factors affect the distribution of heat throughout the atmosphere. This information—what's sometimes called the "signature" or "fingerprint" of a particular factor—is usually provided by climate models.

Simplifying and abstracting substantially, the resulting regression equation looks like this:

$$Y = \sum_{i}^{n} \beta_i X_i + \upsilon_Y$$

where Y is the observed data; β_i and X_i are the percentage of the increase due to the i^{th} factor and the signature of that factor, respectively; and υ_Y is the internal variability of the climate. Standard least squares algorithms are then used to estimate the β terms. The results indicate how much of observed

[11] Stott et al. (2006) is the earliest paper that I'm aware of to estimate quantities like ECS in this way; many, perhaps most, contemporary papers on the attribution of climate change to humans now include sections in which ECS and other variables are estimated using the methods described below.

warming a particular factor is responsible for; if the least squares analysis yields a result that $\beta_{GHG} = .95$, for example, that would indicate that greenhouse gases are responsible for 95% of observed warming.[12] Climate scientists can then use results that the regression spits out for CO_2 to estimate ECS.[13]

The point of this example is that the methodology relies on the accuracy of the "signatures" for the different factors—the X terms—and these are estimated using climate models. Standard regression techniques require the assumption that the signatures are given (that is, perfectly accurate). Since our climate models are not perfectly accurate and thus cannot be expected to deliver perfectly accurate estimates for the X terms, this presents a concrete problem for climate scientists: when using standard regression methods, errors in the estimation of the X terms will lead to errors in the estimation of the β terms and thus errors in the estimation of ECS (Carroll et al. 2006).

To address this problem, climate scientists employ ensemble-based methods. There are a couple of different approaches that they have adopted. The first, developed by Huntingford et al. (2006), replaces the X terms with a probability distribution over possible values for X estimated using an ensemble of climate models. The more recent approach, first outlined by Schurer et al. (2018), runs a standard regression for each estimate of X given by the different models to generate probability distributions for the relevant β terms and then uses a Bayesian updating procedure to generate a ensemble probability distribution for the β terms based on the set of distributions generated by each model. In both cases, the end result is a probability distribution over the β terms that can then be used to estimate ECS. Unsurprisingly, tests against data with known properties indicate that both methods generate results that both more accurate and more reliable than those generated by regressions that employ either a single model or just the ensemble mean (Hannart, Ribes, and Naveau 2014; Schurer et al. 2018).

The key takeaway is the following. To estimate ECS in the manner sketched above, we need *some* representation of the signature of factors like CO_2 and thus some estimate for the X terms. We can either (a) adopt a point-value estimate for each X term (generated either by a single model or, better, by taking the mean of an ensemble of estimates) or (b) adopt the probability distribution generated by an ensemble-based method. Both options are vulnerable to misrepresentation: the first might assign the wrong value to an X

[12] Papers on attribution from the late 90s—paradigmatically, Allen and Tett (1999)—fit the present description relatively well. Contemporary work is often much more complicated.

[13] For various reasons, this isn't quite as simple as taking the observed change in temperature, multiplying it by β_{CO_2} and dividing by the observed increase in CO_2, but we can forego the details of this last step for present purposes.

term; the second might assign the wrong probability to a possible value for an X term. In this sense, they're analogous. Nevertheless, as stressed above, there's an important sense in which the latter is less of a misrepresentation because even if it only loosely approximates the confidence that we should assign to each possible value for X, it approximates that distribution better than the first option. After all, the first option can be thought of as adopting a probability distribution that assigns probability 1 to a particular estimate for X. And in providing this more accurate representation of the actual state of our uncertainty, ensemble-based methods allow us to generate more accurate and reliable estimates of related quantities like ECS.

In short: when estimating some quantity of interest (ECS), climate scientists often find themselves needing to rely on model-generated estimates of some other quantity (the X terms). In these contexts, employing a probability distribution over the other quantity can improve the estimate of the quantity of interest. Since the variation found in ensembles provides higher-order evidence about what distribution to adopt, methods that take account of this variation in generating probability distributions allow us to more accurately and reliably estimate the quantity of interest. The upshot is that the higher-order evidence provided by ensembles is not just valuable in an abstract philosophical sense; its presence concretely improves the science.

3 Is there another way?

I've argued that there are real (and concrete) advantages to the probabilistic interpretation of climate models—that is, there are real advantages to taking the inter-model variation to provide evidence about the probability of various scenarios. Given that this approach has been so roundly criticized in the literature, however, we might wonder if there is some other way of capturing the same advantages. There isn't: while there are different probabilistic approaches that may be better or worse, non-probabilistic approaches cannot provide the same information. After all, inter-model variation is the thing, and non-probabilistic approaches are committed to ignoring the information that it provides.

That said, one might reasonably expect that there would be non-probabilistic approaches that would be nearly as good and that wouldn't share the (alleged) defects of the probabilistic approach. So, for instance, philosophers such as Carrier and Lenhard (2019) and Jebeile and Barberousse (forthcoming) have advocated for the use of "model spread" on the grounds that it captures some features of the distribution between models while better approximating the true level of precision that is warranted by the evidence. Similarly, Betz (2015), Katzav (2014), and Katzav et al. (forthcoming) have argued in favor of "possibilist" interpretations in which each model represents a "real possibility" but where the distribution of the models tell us nothing more than that.

Neither of these alternatives is capable of capturing the advantage of extant ensemble-based methods—worse, there are good reasons to think that they are liable to be even more misleading than the probabilistic approaches defended here.

To see why, it's helpful to consider the oldest and most frequently repeated objection to the use of ensemble-based methods, namely that extant ensembles don't accurately represent the full spread of possibilities, and (thus) that the probabilities that they generate don't accurately represent the uncertainty that we either do have or should in fact have. This complaint is driven largely by empirical work aimed at evaluating how accurately ensembles represent those targets that we can test them against. Though there's some debate about how exactly to interpret the empirical results—see Annan and Hargreaves (2011) and Sedláček and Knutti (2013)—the general picture that emerges is that, when compared to a random sample, ensembles under-represent the extremes.[14] And there's a clear explanation for why: extant ensembles are "opportunistic," meaning they're not designed with the goal of covering all of the possibilities. Instead, each model is individually designed to be as accurate as possible—they differ because different scientists make different choices about how best to represent the relevant targets. To build the most accurate model, scientists will follow the successful work of previous scientists, tune the variables in their model using empirical data, and reject assumptions or approaches that lead to results that are "too far" from the empirical data already collected. As a consequence, we should expect ensembles to cluster around the strategies and choices that have proved to be most successful so far.

Note that the mere fact that extant ensembles misrepresent provides us with a very poor argument for rejecting ensemble-generated probabilities. Indeed, philosophers of science have roundly rejected the inference in its general form: the received wisdom is that all scientific representations misrepresent their targets in some ways but that many (if not most) are nevertheless useful and informative (see Teller 2004). It's no good to object to the use of idealizations here when we happily accept them in other cases; you can't consistently argue against ensemble-based methods on the grounds that extant ensembles

[14] This has arguably changed over the last couple years (Tokarska et al. 2020). These changes only further undermine the motivation for rejecting the probabilist interpretation, however, so I'll forego discussing them here.

are idealized unless you're also willing to argue against the use of climate models—or indeed, all models—on the same grounds.

If there's a good objection to the use of ensemble-based methods here, it's a more specific one, namely that in actual practice the fact that climate models under-represent extreme scenarios is liable to lead to erroneous decisions about climate policy in a way that we cannot correct for using background knowledge.

To be clear, I think this is the most important objection to the use of extant ensembles and that whether it succeeds largely depends on an empirical question, namely: in practice, how predictable are the deficiencies in extant ensembles, and how well can climate scientists account for them via adjusting the assumptions embedded in ensemble-based methods? After all, as Horowitz (2019) stresses, evidence that is predictably misleading is not really misleading at all—you simply have to correct for known errors. To my knowledge, however, no one has yet even attempted a systematic demonstration that climate scientists can never account for the known deficiencies in ensembles, and the success of ensemble-based methods relative to those methods that don't employ ensembles (see §2.3 and note 10) provides at least face-value evidence that the practical problems here are not always insuperable.

More importantly, however, if the "opportunistic" character of extant ensembles is a problem for the probabilistic interpretation, it's *more* of a problem for non-probabilistic approaches. For purely mathematical reasons, we should expect that the model reports of an ensemble of any realistic size will fail to capture extreme scenarios: even if the models were genuinely randomly sampled from the full range of possibilities, the probability of getting a model that represents the extreme possibilities is relatively low in ensembles of only 10 or so models. There is, for example, only a roughly 40% chance of having a model that represents an extreme that has a probability of .05. Of course, we know that the models aren't randomly sampled—as just discussed, we have good reason to think that ensembles under-sample from the extremes—meaning that we should expect the chances of observing model reports at the extremes are even lower.

Since both the possbilist interpretation and model spread rely on model reports in the same way that the probabilist interpretation does, they can't fix *this* problem: they won't tell us about some of the relevant possibilities. But in the fact the situation is even worse for these alternative views. By making use of inter-model variation, ensemble-based methods allow climate scientists to estimate what the tails of the distribution look like even in cases where we don't have samples from them. (This is true, for what it's worth, regardless of whether we assume that the sample is normally distributed around the truth or shares some other relationship with it.) To be sure, the resulting distributions may misrepresent the probability of these extreme scenarios—they may underestimate how likely they are even after corrections—but by extrapolating from the actual model reports, climate scientists are able to gain some insight into extremes that the ensemble doesn't represent. The same isn't true of the non-probabilistic alternatives: neither model spread nor the possibilist reading allows for this kind extrapolation. Insofar as our worries about ensembles concern the misrepresentation of extreme possibilities, therefore, we're better off making use of the variation between models to generate probability distributions than we are using proxies like model spread or less informative possibilist approaches that offer us no tools for extrapolating the data we do have to the extreme cases.

To reiterate from above, none of this is to say that ensemble-based methods are perfect or that the probabilities that they generate should be taken as the final word on a subject. On the contrary, one of the main lessons of the foregoing is that ensemble-based methods are defensible precisely because climate scientists can often account for their (known) flaws using background knowledge or empirical corrections. Once we understand ensemble-based methods in this way, it becomes clear that they provide us with more tools for investigating unexplored and unconceived scenarios than we would otherwise have. Rejecting ensemble-based methods because extant ensembles under-represent certain scenarios is thus not just under-motivated, it's counter-productive.[15]

It's worth discussing one final alternative that has been raised by both Betz (2007) and Katzav et al. (forthcoming), namely the use of "imprecise" probability functions. Given the discussion of the prior sections, it's easy to motivate the use of imprecise probabilities rather than precise ones. As we've already seen, ensemble-based methods don't solve the problem of imprecise evidence. Ensembles allow us to make *more* precise judgments—in the setting of imprecise probabilities, they justify adopting a strictly smaller set of probability functions—but they don't warrant adopting the single precise probability function generated by the application of statistical tools. So even if ensembles help, we should still prefer imprecise probabilities to the precise ones generated by most ensemble-based methods.

I'm sympathetic to this argument, but not convinced. It seems to me to be an open question whether the imagined imprecise methods would in fact be preferable in practice, where this question is one about the costs and ben-

[15] Katzav et al. (forthcoming) allege that precise probability functions "lose" information about uncertainty. That's true insofar as the contrast class is an (idealized) more complex probabilistic representation (Bradley and Drechsler 2014). It's at least not clear that it's true for any alternative to precise probability distributions on the table, however, and the opposite is true for any non-probabilistic alternative.

efits (compare Bradley 2019, §3.5). Perhaps imprecise probabilities buy us an increase in accuracy, but only a marginal one and only at substantial costs in terms of complexity or the amount of processing power and/or data required. In such circumstances, it may not be worthwhile to use imprecise methods as opposed to precise ones. Regardless, I see this alternative as a friendly one—after all, "imprecise" probabilities are still probabilities. The question of which probabilistic approaches we should prefer in the practice of climate science remains a relatively unexplored area, and the aim of the present paper is not to stump for one probabilistic approach rather than another. For now, given the widespread criticism of probabilistic approaches in the literature, it's enough to show that they have distinct advantages over non-probabilistic approaches.

Allow me to step back. Climate scientists need to represent aspects of the climate that are not perfectly understood. There are many desiderata for such representations. One is that they should accurately capture our uncertainty with respect to the feature in question. But others include that they should be as constrained by the empirical evidence as possible; that they should be mathematically tractable; that they should be reliable, accurate, and trustworthy in realistic (as opposed to heavily idealized) conditions; and that they should be informative and easily understood. I think that it's likely that the precise probabilities generated by ensemble-based methods will, in many contexts, be the best representational tool according to this suite of desiderata. Strictly speaking, however, my position is a weaker one, namely that taking account of the variation between models should be treated as one desideratum—this variation is informative and ignoring it only worsens our epistemic situation.

4 Conclusion

This paper offers an argument in favor of the use of ensemble-based methods in climate science and the probabilities that they generate. There are three key takeaways. First, that climate modeling faces a problem due to what epistemologists call "imprecise evidence": we don't know (precisely) how to interpret the evidence produced by climate models. Second, that ensemble-based methods are able to mitigate this problem by making use of inter-model variation. Importantly, the value added by these methods is not merely philosophical; as we saw, there are least some cases where employing ensemble-based methods improves the accuracy and reliability of the results. Third, and finally, while there may well be *probabilistic* alternatives to the methods that are currently employed in climate science, non-probabilistic alternatives cannot capture the same advantages and we should even expect them to be worse—to misrepresent in a more serious and problematic fashion. It may still be the case that on the final accounting the ensemble-based methods employed by climate scientists are ultimately too flawed to be worthwhile, and in particular, there remain unaddressed arguments for why it's a bad idea to use these probabilities in communicating the results of climate science to the public. What we've seen, however, is that there are very real advantages the probabilsitic interpretation of climate models outside that context and it's a substantive question whether the arguments that hold in that restricted domain can be extended the general case.

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