The authors develop a climate-economy framework whose main purpose is to illustrate the importance of taking “the unknown” seriously in this area. More precisely, the paper considers risk, model ambiguity, and model misspecification. The focus is on the decision problem of a planner whose preferences embody uncertainty aversion. That is, a market outcome is not considered. The key contribution of the paper is to propose a framework—i.e., methods useful for addressing this kind of issue—and then to apply it to three examples. None of the examples can be viewed as full-fledged, quantitative settings, so it is difficult to draw concrete policy conclusions from them. At the same time, they illustrate possible magnitudes that could arise. Overall it is an impressive paper. My comments will be organized into three sections: I will discuss the broad motivation for focusing on uncertainty, I will comment on the specific modeling approach, and I will make some final concluding remarks.

Uncertainty? The authors emphasize the (perhaps striking) lack of knowledge about the carbon dynamics, the temperature dynamics, and the economic effects of climate change. But is the climate-economy area really so full of uncertainties? Don’t we know already that the burning of fossil fuel significantly affects the global climate in a negative direction? On this point, I am in full agreement with the authors: there is far from a scientific consensus on the quantitative importance of emissions for atmospheric carbon concentration, for the climate, and for human welfare. To be clear, the authors do not dispute the logic behind the argument that human activity affects the climate: this logic, which is present in all the IPCC reports and which is well known much beyond the research sphere, is really a consensus when it comes to the main channels and their qualitative features. The disagreements—between climate researchers, to the extent there is disagreement—is instead about the relative importance of different mechanisms and about the overall magnitude of the effects. It is, for example, not wholly implausible that the effects of burning even rather large amounts of fossil fuel are negligible. Here, the argument would be that there are feedback effects working in the direction of cooling, such as increased reflection of sunlight from aerosols and clouds, whose prevalence can be increased as a result of a higher atmospheric carbon concentration. Of course, there are also well-known feedback effects working to increase warming. So the point here, rather, is that the net effect contains much uncertainty, as reflected in the figure in the paper showing how climate sensitivity varies greatly across the 144 models considered. What is of course key is that there is a sizable right tail in these experiments: severe warming is possible too.

Turning to the damages to humans, my own summary is that there is even greater uncertainty in this domain than for the natural-science variables. The literature on damage measurements, though, growing rapidly at the moment, is nascent and is typically based on short enough time series that it is difficult to draw reliable inference, especially about the effects of significant warming over the longer run. The longer run is key here since climate change is mostly about how future generations are affected by warming; most models really predict a slow warming process. One line of defense against “radical” climate policy, such as carbon taxes at the level proposed in the Stern Review and by

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1Indeed, some climate models predict a higher global temperature increase due to historic emissions than that observed today, and one hypothesis is that those models overpredict warming because they do not incorporate aerosol formation.
many others, is that even large drops in GDP will be more than canceled by economic growth (assuming that growth continues at its historic rates); another line of defense is that adaptation—say, in the form of slow migration away from areas hit hard by warming—ought to be feasible. Of course, there are many reasonable counterarguments too.

Finally, isn’t a separate, powerful argument in favor of immediate, radical action that of tipping points? More precisely, the idea here would be that beyond a certain temperature level, or atmospheric carbon concentration, we enter into different dynamics that, moreover, are irreversible—at least for hundreds if not thousands of years. My own perception, when entering into this research area, was that my skills in solving for and understanding non-linear dynamics in macroeconomic models would be key precisely because of tipping points. It did not take long to realize, however, that there was no consensus at all in this area either, making it difficult to formulating quantitative models of climate change embodying global tipping points. First, there is agreement on a long list of different kinds of local tipping points (or tipping points specific to limited phenomena) and each of these can perhaps be argued to be well understood. However, what the implied global system would look like need not involve severe non-linearities but could perhaps equally well be described as smooth and linear. Second, to be sure, there are some global tipping points, such as the melting of the ice caps and the release of methane from the tundra, but the speed of these changes may not be so high or, in the case of methane, so long-lasting.\footnote{Methane is a very potent greenhouse gas but does not stay very long at all in the atmosphere, unlike carbon dioxide, which depreciates extremely slowly.} Then again, we really do not know, and recent measurements of the thickness of ice caps are alarming, so it may be that this non-linear than previously thought.

In conclusion, the best way to summarize this is, again, that there is large uncertainty. We just do not know, and the range of possible outcomes is uncomfortably large. So at the very least, with this understanding it is easy to counter the climate skeptics: they are simply hoping that, when the uncertainty has been revealed, we are in the left tail of warming and climate change, and the mere reliance on hope does not seem responsible. A more sophisticated argument would be that radical climate action is very costly for our economies, indeed so costly that it is not worth it. However, then explicit cost-benefit analyses are needed, and the point of the present paper is precisely to move the debate in this direction.

**The approach** The authors present two related climate-economy models. In the first one, underlying the first two examples, emissions have direct utility benefits; in the second, emissions are useful in production.\footnote{Though not described this way exactly, the setup is similar to assuming a production function that is Leontief in capital and emissions.} On the cost side in both models, consumption is “damaged”, along the lines of how damages are described in the literature, but allowing for (random) non-linearities. Both models are global—no regional heterogeneity is considered—and, in terms of the economics, quite stripped down. A feature they have in common is that fossil fuel is not modeled as a resource in finite supply; nor is it costly to produce. Thus, market economies not subject to regulation of taxation would gener-
ate emissions without bound in every period.\(^4\) The setting may still be appropriate for studying illustrative planning problems but is probably well less suited for analyses of how to achieve good outcomes in market economies. Relatedly, in a market economy one would need to be specific on whether or not different agents/generations have the same kind of preferences, knowledge, and concern for model mis-specification as the planner here.

On a pure methodological level, the model is set up in continuous time and the authors argue that this formulation facilitates some of the analysis. I have a slight concern here, as I worry that most economists, including those otherwise well versed in dynamic macroeconomic theory, are not sufficiently skilled on a technical level to fully grasp all the subtleties; this is certainly true for myself. The topic is a very important one and I worry that too much of the potential audience is lost by carrying out the analysis at this level of technicality. Perhaps it helps that a large part of the paper is devoted to discussing how to model and evaluate uncertainty and mis-specification, but it could also be a hurdle. Could perhaps a simple static model with an ex-ante/ex-post distinction have been used, at least as an introduction?

On a more substantive level, is it necessary to also consider recurrent shocks and risk? As I argue above, I’m fully convinced that uncertainty is key in the area, but I wonder if risk couldn’t be dispensed with, thereby simplifying matters significantly. I do not think that fluctuations per se, and smoothing/worrying about them, is the main issue in this area. In my view, rather, it is the significant uncertainty about outcomes that ought to drive modeling. In Hassler, Krusell, and Olovsson (2018), we examine this issue informally. We define two kinds of major errors—that of permanently selecting the wrong climate policy. One type of policy error is that which would result if we follow the advice of the climate skeptics (essentially setting the carbon tax to zero) and it turns out that they are wrong: that both climate change and the damages from it are at the extremes of the intervals indicated by researchers. The other major policy error is that we are wrong in the opposite direction: we drastically reduce carbon use when it turns out ex post not to have been needed. Our contention implicit in that work is that we should mostly worry about the extremes—and also not about the fluctuations per se—and that simple calculations of this sort can also be helpful. We find, in particular, that the second type of error—that of being over-zealous about fighting climate change—is far smaller than the first one.\(^5\) Relative to our analysis (aside from differing in various details), the current paper allows for aspects of slow learning, which could certainly be important, and it formalizes preferences allowing us to choose one policy.

The paper is also related to the discussion over the size of the optimal carbon tax. Obtaining a large value for this tax appears to have been an aim for some researchers, and a number of candidates have been developed. The list includes: (i) a low discount rate, (ii) strong convexity in the mapping between global temperature and damages, (iii) fat tails in climate/damage outcomes and utility curvature, (iv) long-run growth effects, and (v) distributional aspects (lack of inter-regional transfers and a stronger concern for the developing world than expressed by available measures of foreign aid). The present

\(^4\)This would not need to occur in the second model the authors present, since the benefits of emissions are zero here beyond a certain point.

\(^5\)In recent work, we are also studying the roots of these findings in more detail: Hassler, Krusell, and Olovsson (2021).
research could be seen as providing another candidate, which is ambiguity aversion, as formalized in the paper. I would like to have seen more of a discussion of how we can obtain guidance in selecting numerical values for the relevant preference parameters, since these are key; the paper, as I understand it, mostly gives us illustrations. There is also a parallel here to the discussion of discount rates, where the following issue arises: if one selects a very low discount rate—one that is much lower than rates typically used in, say, the cost-benefit analyses of potential long-term infrastructure investments—then it would be important to square this value with the much higher market interest rates. In the case of ambiguity, is the degree of such aversion mainly to be present among climate-policy planners, or should it be present in other realms of government policy as well? Do we expect private agents to embody it too? If so, one should guide calibration/estimation of these important parameters accordingly.

**Concluding remarks**  The paper outlines a thorough and ambitious agenda toward formalizing and analyzing uncertainty in the climate-economy context. It is laudable and I very much hope it will have major impact. I have not commented on the specific findings in the paper, in part because they are reasonable given the assumption, but most importantly because they are illustrative examples and thus not yet full-fledged analyses of policy. Aside from the comments and recommendations I have made, let me add that I think the authors’ contributions could be boosted significantly by considering richer economic settings. One of the main ways in which economists can contribute in this area is to offer our specialized expertise in, precisely, economics: how markets work, or do not work, and how people can be expected to respond to various forms of policy. The present paper does not leverage our knowledge and key insights as economists as much as it could. Rather, it focuses on how to conceptualize uncertainty and formalize preferences with aversion to the unknown, and I worry that actual policymakers do not perceive a need for such insights. Thus I would highly recommend a combination of the approach in this paper with a more full-fledged description and analysis of market economies and available policy instruments.

**References:**
