IAJ The Integrated Assessment Journal
Bridging Sciences & Policy
Vol. 6, Iss. 3 (2006), Pp. 61–63

Response to the Comment by Hans-Martin Füssel

G. Yohe
Department of Economics, Wesleyan University, Middletown, CT 06459, USA

M. E. Schlesinger
The Climate Research Group, Department of Admospheric Sciences, University of Illinois at Urbana-Champaign, Urbana, IL 61801, USA

N. G. Andronova
Department of Atmospheric, Oceanic and Space Sciences, University of Michigan, Ann Arbor, MI 48105, USA

We are pleased that Füssel (2006) appreciates the reduced-form approach that we employed in our examination of the vulnerability of the Atlantic thermohaline circulation (the THC) to greenhouse-gas-induced increases in global-mean temperature and its sensitivity to a wide range of policy interventions in Yohe et al. (2006), and we commend his careful and informed reading of our work. We hereby confirm his assumption that the underlying structure of the economic forcing and the characterization of the link between increased greenhouse-gas concentrations and changes in temperature is the same as the structure we employed in Yohe et al. (2004). More specifically, the distributions of climate sensitivities coupled with associated lag parameters are exactly the same. We also applaud his concurrence with our qualitative conclusions: that the likelihood of a collapse of the THC is perhaps much higher than previously thought and that even strenuous mitigation policy could be uncomfortably ineffective in lowering this likelihood.

We are, however, a little perplexed that he quibbles so vehemently about the quantitative results. The numbers that emerge from climate models, be they simple reduced-form representations of specific processes like the one we used or complicated general circulation models of the sort Füssel cites as evidence, are never really to be taken as true representations of reality. They are model results. It follows that it is the qualitative implications of numerical analysis, if they can be shown to be robust, that are the important contributions of any work in this area. In that regard, his arguments would be reassuring if they were correct, since they would only underscore our conclusions. Unfortunately, we are not convinced that his analysis is appropriate.

Füssel is correct in noting that we link every climate sensitivity value with a specific lag parameter. This is because the associated lag parameters are, fundamentally, maximum-likelihood estimates derived from the historical record *contingent on the various climate sensitivities*. The association, perfect but not linear by any stretch of the imagination, is displayed in Figure 1 for the cli-

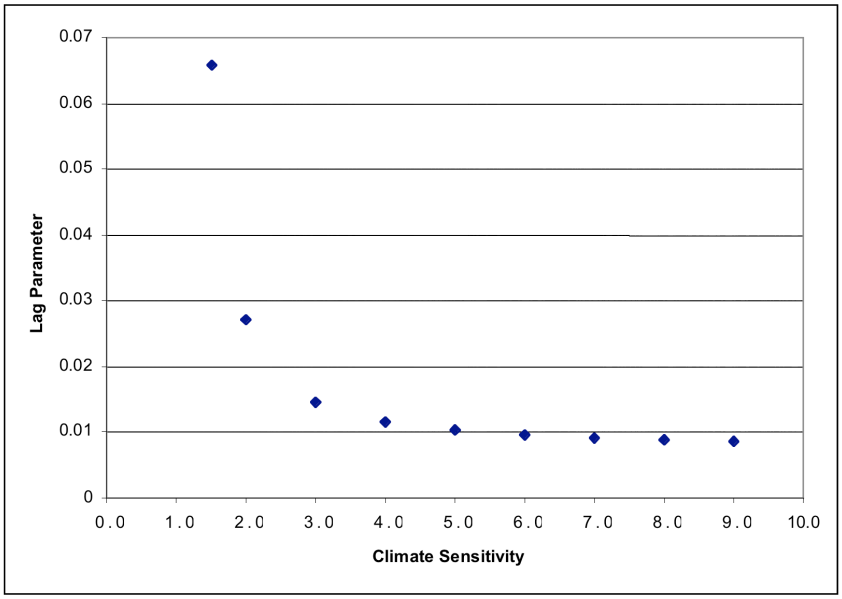

Figure 1: Maximum likelihood estimates of the lag parameter for specified climate sensitivities

mate sensitivities that we employed to produce a discrete representation of the continuous distribution reported in Andronova & Schlesinger (2001). Given the procedure by which they were estimated, it would have been entirely inappropriate to detach the lag parameters from their contingent sensitivities.

Did we, in adopting this estimation procedure, underestimate uncertainty? Perhaps, but we cannot tell by comparing the resulting distribution of temperature change through 2100 with the results of other models, even if they are more detailed in their representation of the climate system. They are, despite the care with which they have been constructed, only models. Put another way, we note Füssel's observation that the "transient climate change determined in the detailed studies ... is much better reproduced when only T_{2X} is varied and θ_1 is held fixed at its default value ... than when T_{2X} and θ_1 are varied assuming a deterministic relationship". Our contingent estimation procedure could have produced stable estimates for θ_1 if stability were consistent with the historical record, but it did not. Had we made the two parameters independent and not calibrated the model's effective heat capacity to reproduce the instrumental temperature record for each climate sensitivity, we would have spuriously inflated the uncertainty. This is clearly an undesirable thing to do. Indeed, we suspect that the null hypothesis of a stable lag parameter across the range of

climate sensitivities displayed in the underlying distribution would be rejected soundly by standard statistical tests.

That said, we close by confirming that we have little doubt that the uncertainty would expand, and so the strength of our qualitative conclusions would have increased, if we had incorporated other sources of uncertainty like, for example, a range of emissions scenarios from which various mitigation policies might be implemented. We think, therefore, that Füssel makes a valid point even if we suspect that it is for the wrong reason.

1 Bibliography

Andronova, N. G. & Schlesinger, M. E. (2001), 'Objective estimation of the probability density function for climate sensitivity', *Journal of Geophysical Research* **106**(22), 605–622.

Füssel, H.-M. (2006), 'Reducing the risk of a collapse of the Atlantic thermohaline circulation: A comment', *Integrated Assessment Journal*.

Yohe, G., Schlesinger, M. E. & Andronova, N. G. (2004), 'To hedge or not against an uncertain climate future', *Science* **306**, 416–417.

Yohe, G., Schlesinger, M. E. & Andronova, N. G. (2006), 'Reducing the risk of a collapse of the Atlantic thermohaline circulation', *Integrated Assessment Journal* **6**(1), 57–73.