

Thank you for responding to my comments and questions regarding your manuscript. After reading the updated manuscript and response to reviewers, I am convinced that the article does not need any meaningful additional analyses. The calculations conducted by the authors represent an important and comprehensive assessment of sector-specific climate damages in the United States and will undoubtedly have profound policy impact.

However, I am not fully satisfied with the authors' replies to my concerns. I will note that I found the response very difficult to digest because no precise textual changes were listed and no text from the manuscript was reproduced, so I cannot directly trace what has been changed in response to each of my concerns. I aim to provide an accurate reflection of manuscript updates below, but it is possible I have missed something because no direct links to the updated text were provided.

Second, the authors largely responded directly to me regarding each of the limitations of the analysis that I had raised. But in most cases, the text of the manuscript did not change in a meaningful way. I am convinced that the authors have an important contribution without needing to address each of these limitations head on. However, I do think it is necessary for key assumptions and limitations to be transparently communicated to the readers. I list the specific cases where I think this is critical below.

1. Adaptation. The authors insufficiently communicate that most sectors in their analysis assume no adaptation takes place (see Table A2-1). Text on lines 198-204 mentions that different adaptation scenarios are modeled to the extent possible given available scientific evidence, but this text should also explicitly note that only two of 20 sectors include proactive adaptation, making it likely that estimated impacts are overly severe. Given that temperature-related mortality dominates their overall damage estimates, the fact that these results ignore adaptation, despite strong evidence of historical adaptation (Heutel et al., 2021; Barreca et al., 2016), should be openly discussed and justified.
2. Income effects on the damage function. The authors state that "most of the sectoral damages are proportional to GDP per capita" (line ~275). In our exchange, we agree that in some cases damages may be higher in wealthier populations, but in other cases the reverse may be true. Instead of omitting this important point, the authors should mention that they make this structural assumption despite evidence that income could push damages in either direction.
3. Spatial heterogeneity of warming. The authors should clearly state that they ignore uncertainty in the spatial distribution of warming across the United States. It makes sense to cite Sarofim et al. (2021) to justify this choice, but it shouldn't be left to the reader to figure out what forms of climate uncertainty are or are not included.
4. Damage function uncertainty. The authors show some uncertainty in damage functions for mortality by comparing *across* impact models. However, many impact models, in particular econometric ones, also have damage function uncertainty *within* each model. The authors should clearly state that such damage function uncertainty is omitted. Showing that the range across these three modeling approaches lies within the

distribution of climate uncertainty results does not address my concern – if you combined damage function uncertainty with climate model uncertainty, as is recommended (Burke et al., 2015), results would undoubtedly convey far larger uncertainty ranges.

5. Electricity. The authors reply to me, oddly, by stating that although they include only electricity, the study they draw on also has natural gas. Why not include natural gas, then? At a minimum, the authors should clearly convey that natural gas expenditures are not included in their analysis (noting that their inclusion would likely lower damages).
6. Overlapping sectors and sector interlinkages. We can agree to disagree on the importance of these issues in the current FrEDI results, but the authors should at least mention the limitations of FrEDI in these areas. Its omission from the manuscript, given the many-sector bottom-up approach, is troubling.
7. I remain confused by lines 85-90, which imply that a user could pair *any* climate scenario with *any* socioeconomic scenario, which is not what the authors do in this paper (and which is concerning, given logical inconsistencies that could arise).
8. The authors stated that they included a footnote clarifying that their calculations are equivalent to a domestic SCC, but I cannot find that footnote anywhere.