This paper compares estimates of climate changes damages to GDP with costs of mitigation in order to identify the level of warming that minimizes the combined welfare losses. This is an important and innovative contribution. It is widely recognized that the cost-benefit IAMs used to make statements about optimal warming levels rely on outdated science. Much attention has been devoted recently to improving the representation of climate damages in these models. But the representation of mitigation costs is equally flawed. This paper makes a substantial advance in improving the representation of mitigation costs and combining with recent results of the magnitude of climate damages in order to assess optimal warming levels.

In addition to tackling an important question, the paper does a good job of examining sensitivity to preference parameters (specifically the pure rate of time preference and inequality aversion) and of communicating uncertainty from both climate models and damage function estimation. Nevertheless, I have five major concerns about the current manuscript – three related to the damage function and two to the mitigation cost estimates.

Damage Function

1. The paper uses results from the regression presented in Burke, Hsiang and Miguel (BHM). In the main specification, BHM assume that the growth rate effects estimated are permanent losses to growth. In fact, evidence for this is fairly weak – including one additional lag term substantially decreases the effect size and produces standard errors that overlap zero (BHM, Extended Data Figure 2c). More recent work has also shown suggested that the effects estimated by BHM are unlikely to be fully persistent (1). Given the indeterminacy in regression results regarding the question of whether these are growth or level effects, the authors should present using damage function specifications that include additional lag terms.

2. The authors are using a damage function specification that allows for different effects between rich and poor countries. As best I can determine, these classifications are fixed (i.e. a particular country remains on the poor damage function throughout the simulation, no matter how rich it gets). Instead the authors should account for the fact that poor countries are becoming richer over time, and therefore should eventually transition to the "rich" damage function after passing a certain income threshold.

3. I am unconvinced by the approach taken to try and "match" the climate damages based on RCP scenarios to the REMIND mitigation cost scenarios. The cumulative nature of the GDP impacts, combined with time discounting, means specific temperature trajectories could have a large effect on estimated damages. This means the interpolation approach taken here is almost certainly invalid since a least-cost approach to a peak warming of X degrees is different from any given RCP. Given the simplicity of damage estimates here (i.e a very simple function of population-weighted temperature change), I am not clear why the REMIND temperature trajectories themselves were not used to calculate damages, allowing for a direct mapping between damages and mitigation costs.

Mitigation Costs:

1. Given the main contribution of this paper is the comparison of mitigation costs with climate damages, far more information is needed on how mitigation costs are estimated in the REMIND model. At the moment there is only a very cursory paragraph describing this. Questions I would like answered include 1) how is energy demand estimated? 2) is the same SSP2 used to estimate energy demand? 3) how elastic is energy demand in the model and what is the empirical basis for this? 4) are these general equilibrium costs? 5) if not, to what extend are they likely to over- or under-estimate general equilibrium costs (particularly important for very ambitious mitigation targets)? 6) what are the uncertainties on the mitigation costs?

2. My main specific concern regarding mitigation costs surrounds the endogeneity of energy demand. A world where climate change damages have reduced GDP by up to 40% compared to baseline is a world with very different energy demand compared to a world without climate damages. The costs of reaching particular temperature targets would be correspondingly lower. A couple recent papers have examined this feedback between climate damages and the energy system (2, 3). My understanding is that this is not currently addressed. Ideally the authors would re-run REMIND, adjusting energy demand to account for the GDP damages.

Minor Comments

- The authors discuss extensively the limitations and uncertainties involved in the damage function calculation. I think it is important to have similar discussion about mitigation costs (e.g. why these might be either over or under estimates) and what effect that would have on the conclusions. Particularly given the fact that uncertainties in mitigation costs are not quantified.
- I don't think the term "non-economic" is appropriate for describing losses that are not captured by GDP. Economics captures any change that affects human welfare, including effects on ecosystems or health that would not be captured by GDP. The term "non-market" would be more appropriate.

References

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3. Bastien-Olvera BA (2019) Business-as-usual redefined: Energy systems under climate-damaged economies warrant review of nationally determined contributions. *Energy* 170:862–868.