The comments of referee#1 are reproduced below in black. Our responses are in blue.

This is a peculiar paper.

The authors take two functions for the economic impact of weather and pretend that these represent the economic impact of climate change. One of these functions is unpublished, the other is known to be wrong.

- We believe such functions based on economic impact of weather cannot represent the economic impact of climate change. However, the authors of these functions (Burke et al. and Newell et al.) actually do. All the idea in our work is to illustrate to what absurd results using recent past weather to estimate large climate change impact can lead.
- The function of Burke et al. (2015) has been published in Nature, has been cited in the literature several hundred times and has been used to compute the social cost of carbon (e.g. Ricke et al., 2018). The authors also published several other papers based on similar methodologies (e.g. Hsiang (2016), Burke et al. (2018), Diffenbaugh & Burke (2019)). In our opinion, if the function is known to be wrong this point has not received enough attention so far, at least among economists.
- The function of Newell et al. (2018) has not been published in a peer-reviewed journal, but it was interesting to consider it anyway because 1) it belongs to the family of damage functions assuming an impact of climate on GDP level rather than growth, leading to very small damages; 2) it is based on the same data and methodology than Burke et al. (2015).

Burke, M., Davis, W. M., & Diffenbaugh, N. S. (2018). Large potential reduction in economic damages under UN mitigation targets. *Nature*, *557*(7706), 549-553.

Diffenbaugh, N. S., & Burke, M. (2019). Global warming has increased global economic inequality. *Proceedings of the National Academy of Sciences*, *116*(20), 9808-9813.

Hsiang, S. (2016). Climate econometrics. *Annual Review of Resource Economics*, *8*, 43-75.

Ricke, K., Drouet, L., Caldeira, K., & Tavoni, M. (2018). Country-level social cost of carbon. *Nature Climate Change*, *8*(10), 895-900.

The authors estimate these functions for a period of modest warming and extrapolate to a scenario with large cooling. They pay little attention to specification and confidence intervals.

- We perform an "ad absurdum" demonstration. Therefore, we consider that using only the main specification of Burke et al. (2015), which is the most analyzed in their work, is sufficient for the sake of the demonstration.

The authors refer to but do not use the functions of the economic impact of climate change. They ignore the one study of the economic impact of cooling by Ralph d'Arge.

- I do not fully understand your remark. We refer to other functions in the introduction, as a general context, but our work did not aim at applying all published damage functions to our hypothetical cooling.
- I assume you refer to the paper of Ralph d'Arge from 1979, "Climate and economic activity". We did not cite this paper because it is cited in the review of Tol (2018), which is mentioned in the introduction. Indeed, d'Arge (1979) estimated the impact of a 1°C cooling, and found a very small impact of -0.6% in the world GDP. So, as it

seems to be the only previous paper investigating the impact of a cooling we could add it in the references, but the magnitude of cooling is anyway much smaller than the one we assume in our work. Also, d'Arge's work does not seem to be available on the internet, therefore I do not know what was his methodology, but considering the large development in climate sciences over the past 40 years, this work might be out of date.

They do not compare their results to previous estimate of the impact of a shutdown of the thermohaline circulation, the scenario that comes closest to the one considered here.

- Concerning the impact of a shutdown of the THC, there is indeed for instance the work by Link & Tol (2011), using the FUND model. But their work uses an integrated assessment model, when we restricted ourselves to econometrics methods. Also, in their work, the cooling induced by the collapse of the THC is much smaller than in our work (only -1,7°C in average on the north hemisphere) and occurs within the context of global warming. The scenario is therefore utterly different from ours.
- There are also the works of Kuhlbrodt et al. (2009) or Anthoff et al. (2016), but again it uses integrated assessment models, and the THC collapse occurs within the context of global warming, so that the global temperature change is much smaller than in our scenario. Moreover, in the case of THC collapse the cooling is more or less limited to the north hemisphere.
- These works rely on climate projections from numerical modelling only, with all the associated uncertainties, whereas we wanted to refer to a known past period for which there are also data, as explained in our responses to M. Verbitsky.

Anthoff, D., Estrada, F., & Tol, R. S. (2016). Shutting down the thermohaline circulation. *American Economic Review*, *106*(5), 602-06.

Kuhlbrodt, T., Rahmstorf, S., Zickfeld, K., Vikebø, F. B., Sundby, S., Hofmann, M., ... & Jaeger, C. (2009). An integrated assessment of changes in the thermohaline circulation. *Climatic Change*, *96*(4), 489-537.

Link, P. M., & Tol, R. S. (2011). Estimation of the economic impact of temperature changes induced by a shutdown of the thermohaline circulation: an application of FUND. *Climatic Change*, *104*(2), 287-304.

I recommend a major revision because the idea is nice, but major revision here means replace the paper.

Equation (1) is wrong. The left-hand side is stationary. Temperature on the right-hand side is non-stationary. This cannot be, but fortunately there are year dummies. You thus regress economic growth on the cointegrating vector of temperature and trend. The statistical properties of this are unknown, but results are biased because you measure the cointegrating vector with error. The procedure may work in sample, but you cannot extrapolate without estimating the year dummies for future years. Burke set future dummies to zero, which is really quite a stupid thing to do.

- We strictly followed Burke et al. (2015) equations. The criticism you put forward is interesting and to be honest we never thought about it. We would hence appreciate if you could provide some references where this has been published. Discussing the mathematical issues of Burke et al. approach would be very useful and certainly strengthen our argument. That being said, our point was not to engage into that line of criticism but rather to merely do an "ad-absurdum" demonstration. We indeed believe, as stressed in the response to your first point, that, given the wide coverage and impact of the Burke et al. (2015) and ensuing papers, a more explicit demonstration of the irrelevance of the approach is a useful complementary contribution to a more mathematical/statistical critique.