Reviewer 2

On the whole this is a useful paper that recognizes that climate change mitigation has costs and the optimal temperature target for how much we allow the world to warm depends on not just the damages of climate change but also the costs of mitigation. This is not a new insight but the paper represents a substantial advance over many of the IAMs that have been used in the past by incorporating a much more up-to-date approach to estimating the damage associated with a given temperature rise. On the mitigation side the paper includes a more robust estimation of how the costs of dealing with climate change will change with temperature and, it appears, a great deal more detail on how that will evolve with changes in technology.

Thank you for your review and helpful comments! We carefully revised the manuscript and hope we could answer all your questions and concerns. With these responses we submit a manuscript that contains all tracked changes in response to the reviewer comments.

I think that this paper usefully fills a gap in the existing literature by updating our understanding of the trade-off between mitigation costs and damages based on new science. However, there are several details of the implementation that concern me.

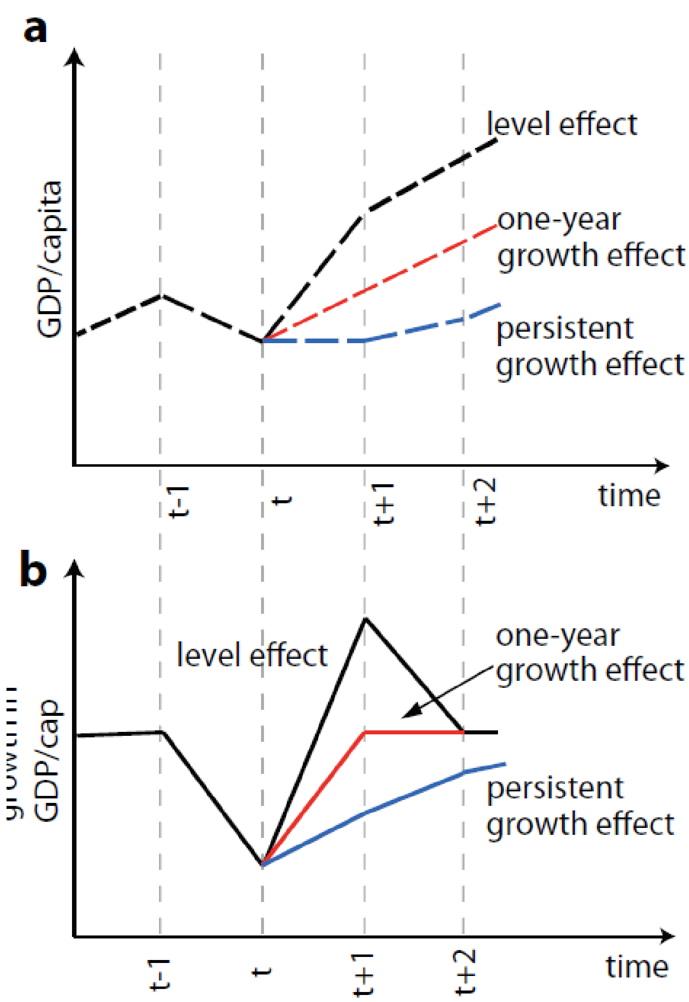
First, I share the concern pointed out by the first reviewer that the estimates from Burke et al. may not represent permanent declines in growth rates but rather represent a levels effect. Further, assuming these effects remain constant to the end of century, without accounting for adaptation, is problematic. Burke et al. suggest they see no evidence of changes in the relationship since the 1960s and take that as evidence of there being little adaptation but knowledge of climate change has changed a great deal since the last 40 years of the 20th century and it seems unlikely there will not be adaptation going forward. That also suggests a specification that does not treat damages as permanent declines in growth rates.

Thank you for emphasizing this important assumption. We carefully thought through our assumptions and added significant discussion on the choice and implication for the results. We adjusted all crucial parts in the main text of the paper (abstract, introduction, method, discussion and conclusion) and added a section in the appendix A6.

Assumptions about the persistence of damages clearly influence the total damage costs and thus the result of our study. Burke et al. (see Extended data figure 2a below) differentiate three cases with increasing damage: i) level effects, ii) one-year-growth effects (leading to persistent level effects.), and iii) persistent growth effects. The latter case (as defined) includes additional future growth effects in response to an initial climate-change-related event. Hsiang and Jina, 2014, show such additional annual growth rate reductions until about 15-20 years after a disaster based on cyclone data. Assuming one-year-growth effects not only neglects a potential recovery (towards level effects) but also excludes potential additional growth decreases in following years (towards persistent growth effects).

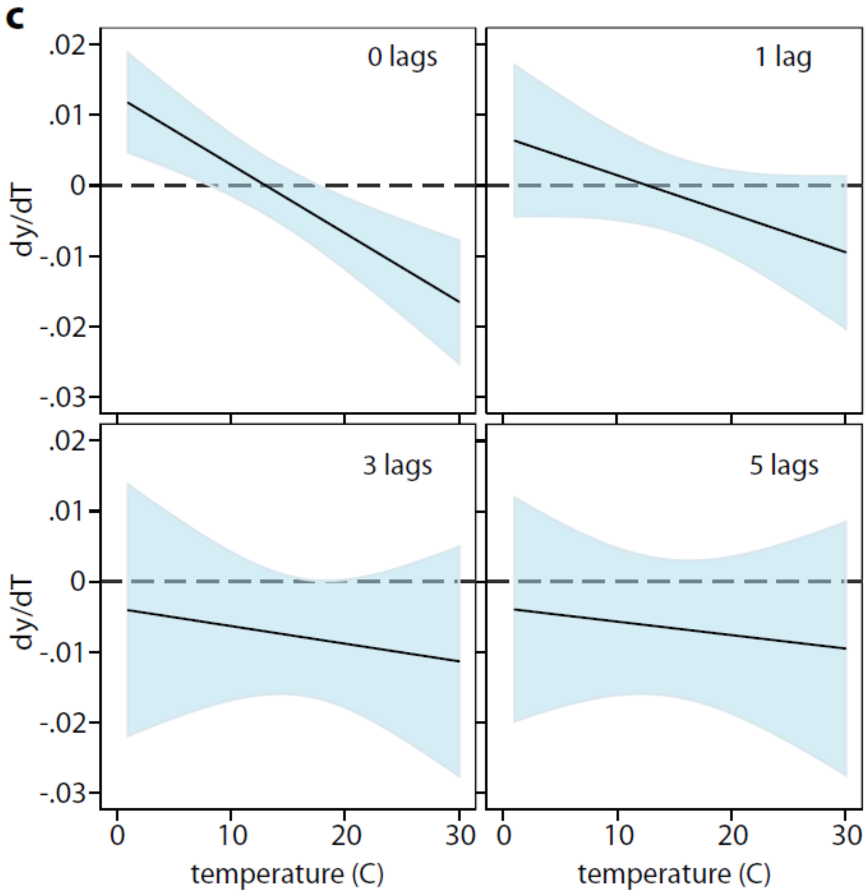
In addition, a more-complex combined case is possible and seems plausible at least for e.g. severe extreme weather events: additional growth decreases could first increase the cumulated damage compared to one-year-growth effects, before recovery allows returning towards an original growth trajectory, which could be regarded as a larger multi-year level effect.

The assumption of one-year-growth effects leads to higher damages than pure level effects and to lower damages compared to persistent-growth effects. The relation to a larger multi-year level effect depends on its duration and amplitude (and also on the pure rate of time preference).



Trying to consider the full range of possible assumptions of persistence is both valuable and challenging, as we argue below. In the manuscript, we now point to this as one possible next step for further research (which is of course already going on). This would ideally be based on a regression analysis that consistently derives damage estimates for different assumptions of persistence. One way is to increasingly include time lags into the regression analysis (next paragraph). As an alternative, one could assume a functional form of damage decay (e.g. exponential) and scan through different parameter values. Cumulated damages for a range of persistence values could then be combined with mitigation costs.

The magnitude and temporal structure of damages is uncertain, specifically when increasing the time horizon to derive insights on the persistence of damages. Burke et al. tried quantifying the latter by including lags in the regression. The resulting regression parameters get increasingly uncertain with more lags and the resulting damage impact is unclear. While for zero lags (=fully persistent damages) Burke's functional assumption of a parabolic (non-linear) response function can be confirmed very well within the uncertainty ranges (95% confidence interval) (see Burke's extended data figure below 2c, top right panel), this changes with the introduction of lags. Specifically for three- and five-year lags (2c, bottom), the median regression results (black) are embedded in broad confidence intervals that allow for all sorts of functional response shapes and magnitudes. The median realization is below the x axis such that if a linear decline of dY/dT is assumed, the corresponding quadratic response function does not have a maximum anymore and also very cold countries would lose from any warming. The limited size of the *data set (<10000) lets the signal get very weak when introducing additional variables (such as lags). While the cumulated damages (median values) reduce with introducing one lag pointing towards level effects, the impacts significantly increase with more lags. Given this and the increased uncertainty, we regard the lag-analysis (and question of persistence) as inconclusive.*



Newell et al. 2018[2] *evaluate the performance of growth and level effect models with respect to the statistical significance of their results. They conclude that while the best-performing models are those that relate temperature to GDP levels, it cannot be precluded that growth-effect models are superior. Burke and Tanutama*[3] *(working paper) very recently provide additional evidence for growth effects in a sub-national impact study.*

To sum up, the question of level vs. growth effects is relevant and open. We argue that a consideration of a range of persistence assumptions would ideally be based on a consistent empirical analysis that varies a persistence parameter. This is beyond the scope of our analysis. Here we assume one-year-growth effects and honestly communicate this important assumption. The effect of this assumption, in terms of how results are impacted cannot yet be answered in terms of magnitude and sign, as there is literature arguing towards both either level effects (which would reduce cumulated damages) or more persistent growth effects (increasing cumulated damages). We regard our focus and contribution as the combination of Burke's default parameterization of damage response (with high regression parameter robustness based on the assumption of one-year-growth effects) with global climate mitigation costs. We emphasize this now in the abstract, the introduction and the discussion and conclusion. Thanks again for inspiring us to deeper thinking and clarification on this issue. We also added this detailed discussion to the appendix.

Second, I also would like more information about the REMIND model. It is a model of mitigation costs that is unfamiliar to me and the information included in the paper makes it difficult to assess whether it is appropriate.

Thank you! We added more paragraphs on the REMIND model in the main text and added two sections in the appendix: one on the REMIND model and one on mitigation cost uncertainties. Moreover we cite two main additional references for more detail:
[1] https://www.iamcdocumentation.eu/index.php/IAMC_wiki
[2] https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2697070

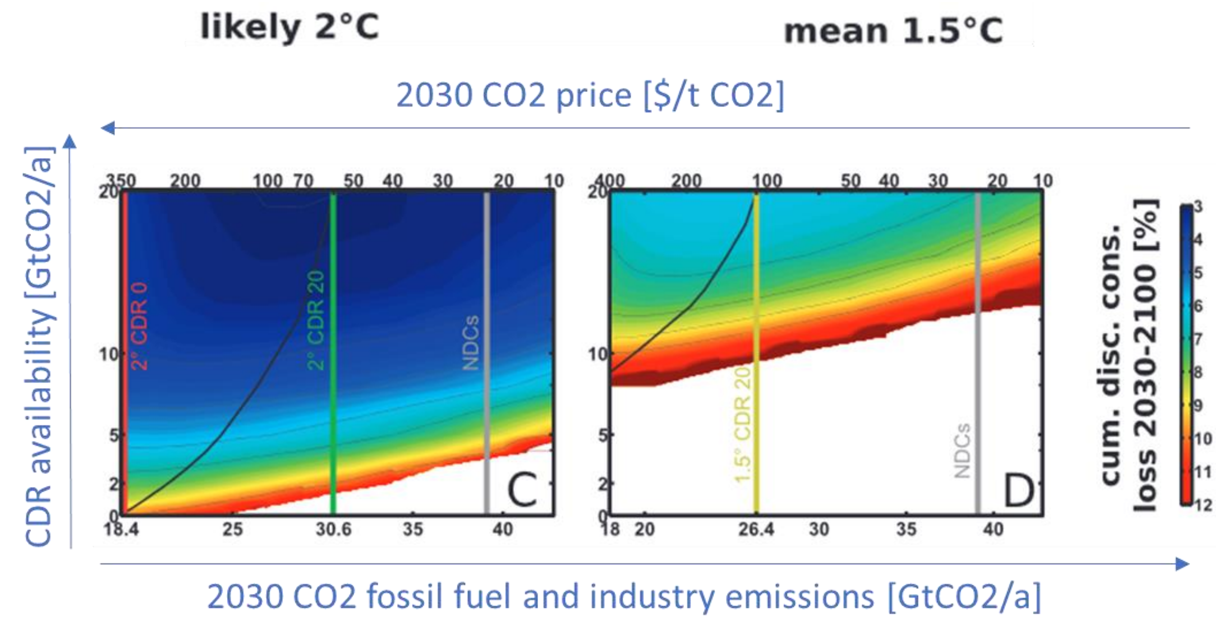
In addition, we responded to each of the REMIND model questions that reviewer 1 was posing.

Third, in the mitigation scenarios the authors assume "harmonized greenhouse gas emissions pricing as of 2020." That seems extremely unlikely to happen at this point. Similarly, while it is not clear when CCS is assumed to be widespread, any assumption that CCS is widespread in the very near future seems equally unrealistic. At the very least I'd like to see how the estimates change if these assumptions are relaxed and the time at which harmonized emissions pricing moves out into the future. I would expect that this increases the mitigation costs and so results in a higher temperature target but there may be offsetting effects (another reason to clarify the details of the REMIND model).

A "harmonized greenhouse gas emissions pricing as of 2020" is the standard IAM modeling way to incentivize the global transition towards ambitious climate targets (in addition to other policy instruments such as subsidies). This does not imply that the modeling results are not valid if a global scheme does not exist in 2020 or later. Bertram et al., 2015, Nature Climate Change show that also an imperfect policy mix with delayed (until 2030) and fragmented carbon pricing can initiate a similar transformation at comparable mitigation costs.

At the same time, we frame our analysis by commenting on this also in the conclusion part: „a lack of political or societal will, partial interest groups and lobbying power, weak institutions, or insufficient international cooperation could hamper or delay a transition such that mitigation costs increase. Our analysis is meant to inform the ongoing international climate negotiations under the assumption that these barriers can be overcome."

For all technological aspects of the system transformation in the REMIND model and similar IAMs we refer to two main references that focus on very ambitious 1.5-2°C warming scenarios[9,16]*. The key dependency of mitigation cost (and achievability of warming limits) are assumptions on the availability of CDR (carbon dioxide removal) including BECCS (biomass with CCS). The below figure derived with the REMIND model*[17] *shows the dependence of long term mitigation costs (=cumulated discounted consumption losses 2030-2100) on i) CDR availability (y-axis) and ii) short-term costs (upper x-axis: 2030 carbon pricing). This shows how reaching two temperature targets (left: likely 2°C, right: mean 1.5°C) depends on the availability of CDR and its tradeoff with short-term costs. The feasibility frontier is indicated by the red areas (very high mitigation cost). White spaces show areas beyond achievability. Short-term action (incentivized by high CO2 prices until 2030) can keep targets in reach even if less CDR is achievable in the long term.*



Fourth, I found the aggregation of costs and damages confusing. It seems that it would be better to estimate both costs and damages in the same framework rather than estimating them separately and then trying to aggregate. As the other reviewer points out, using the REMIND temperature pathways in the damage estimation seems the obvious way forward.

We understand your concern. The reason why we cannot use the same temperature trajectories is that the REMIND model is not coupled with a GCM, but with MAGICC ('Model for the Assessment of Greenhouse Gas Induced Climate Change'). This is a reduced-complexity model that is often used by the IPCC, for key scientific publications, and by a number of Integrated Assessment Models. It calculates temperatures only with annual temporal resolution and for four earth sub-systems: ocean, land, northern and southern hemisphere.

The resulting challenge is linking mitigation costs from the REMIND model and damage estimates based on a suite of GCM simulations. We have to find some kind of linking procedure because we rely on using GCMs for two reasons:

1. *Using GCMs we can reflect climate modelling uncertainties via the variety of different approaches used and assumptions made by the different climate modelling teams.*
2. *Using GCMs allows applying a high spatial resolution to then derive population-weighted temperatures based on spatially highly-resolved (0.5° x 0.5°) dynamic population projections and to then derive country-specific temperature data for each country to then derive country-specific GDP losses.*

We agree with the reviewer that costs on both sides, mitigation and damages, depend on the temperature trajectories (i.e. on the timing of mitigation and damages). Ideally, we would have an identical set of temperature trajectories for a sufficient number of scenarios (~10) that span the full global warming range. However, running new GCM scenarios for several models is a tremendous effort that does not sufficiently strengthen the key point of this analysis: This study is showing that i) empirical estimates from a new research strand can be and need to be combined with detailed mitigation cost estimates (ideally from detailed integrated assessment models like REMIND), and that ii) a partial *analysis combining empirical damages with one of the best state-of-the-art energy-economy-climate models shows that under assumptions on adaptation and persistence the "optimal" temperature increase is in the range of the Paris agreement for a wide range on normative assumptions on equality and time preference. Given that we could not build our analysis on an identical set of temperature trajectories for both mitigation and damage scenarios, we had to match both sides as described and made sure that their qualitative shape is similar. We had been excluding RCP60 from the analysis before as its emission trajectory is qualitatively different from the other RCPs and the REMIND scenarios.*

Fifth and finally, using a hard threshold to delineate between rich and poor seems fine but this should both be subject to robustness checks – what happens when the threshold is more or less strict? And countries should be allowed to move across the threshold overtime. As the author's point out, accounting for India's growth in the last few decades substantially increases the damage estimates in Burke et al. Despite climate change countries will continue to grow and as they grow richer they may become more adept at dealing with climate change. The estimates should allow for that.

We agree and have been applying the suggested procedure. We had been stating that there is a fixed separating GDP value y *. Now we describe in more detail that the countries are newly evaluated against this separating value depending on each year's GDP level, i.e. also countries in transition are moving into a regime, which uses the rich-country specification of the empirical response function.*

"The separating value y *is the median per-capita GDP in 2010, i.e. at the end of Burke's historical period. We do not assume that countries remain in the rich or poor category, but their response function is evaluated on the basis of each year's per capita GDP. Poor countries increasingly transition across the per-capita GDP threshold into the regime where the 'rich' specification for the response function is applied." (see supplement A2)*

Minor points:
- There were a number of typos throughout the manuscript. A quick read through should clean most of those up.

Thank you! All authors had a fresh read of the paper when revising it and we hope we covered this.

- I thought the figures 1 and 2 were clear and helpful but figure 3 may not have been necessary.

We agree and moved it to the appendix. Thank you!

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