nature portfolio

Peer Review File

Quantifying climate loss and damage consistent with a social cost of carbon

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Reviewer comments:

Referee #1

(Remarks to the Author)
This paper proposes a methodology for computing loss and damage (L&D) of CO2 emissions to be used for the attribution of climate change damages that materialize through time to specific emitters. The authors develop this methodology based on insights from the literature on the social marginal cost of carbon. Key elements are (i) the construction of counterfactual scenarios that characterize damages without the contribution of a particular emitter, which is based on a reduced-complexity climate model and pattern scaling, and (ii) the prediction of macroeconomic impacts, captured by an econometric model relating local average temperatures to local income (GDP). The authors show that cumulative future damages of CO2 emitted in 1990 (to materialize between 2020 and 2100) are three orders of magnitude higher than their historical counterpart, defined as the damages caused by those emissions between 1990 and 2020. Defining L&D as the sum of those two, the authors apply their method to compute L&D for countries, firms and even individuals. They also provide estimates of bilateral L&D between pairs of countries or regions, and show how the value of carbon dioxide removal can be assessed within their framework.

This paper strikes me as highly original in how it combines results in climate science, economics and accounting to advance research on a difficult and policy relevant measurement issue. The results are significant insofar as they provide very elaborate quantitative estimates of economic damages of emissions past. Those estimates provide guidance for carbon offset payments by individuals and firms that are conscious about their past CO2 footprint and the consequences for global climate change. The results may also be applied in an international policy context, as the authors suggest, though I believe this to be a second-order application given that the underlying assumption that all emissions are marginal is not appropriate in this context(more on this below). There is a lot of material in this paper, and the authors are not very explicit about what they themselves regard as the most significant contribution. While a lot of the motivation draws on suggest examples from the international policy context, the authors also caution against regarding the L&D estimates as legally binding. To sum up, the significance of the rich set of results summarized in Figures 2-4 lies in the measurement, not so much in any policy implications that might follow from them.

The paper is very well written. The exposition and figures are clear and highly didactic. While this made for truly enjoyable reading, the paper strikes me as somewhat long by journal standards.

The authors make appropriate use of statistics. They use best-practice methods to quantify multiple sources of scientific uncertainty. References are appropriate.

My main comments concern the validity of the approach.

1. I wondered how well the climate model and pattern scaling account for non-linear effects of CO2 on temperatures, in particular when computing counterfactual emission paths for large emitters. Figure ED1a suggests that for a large emitter like the US, L&D calculations will involve non-negligible changes to world emissions. If the true relationship between

emissions and GMST is non-linear (tipping points being an extreme case), a reduced-complexity climate model could potentially lead to large errors in such a case. On p. 20 of the methods section, the authors briefly address this issue, but I needed more convincing to believe that non-marginal changes hardly affect the results, as is claimed there. For example, the authors could show results comparing the forward run to perturbation experiments for the large magnitudes considered here, e.g. a fifth of world emissions. Given that FAIR is a “reduced-complexity model” (p.9, l.310), such comparison would be more credible if the reference is not FAIR itself but a different, full complexity model. I am concerned that the extensive use of linear approximation in reduced-complexity models might lead the authors to get a false sense of accuracy when comparing results across different pulse sizes, or even in a full run of the FAIR model. A test with an alternative model would seem to have much more power to reject linear effects. It seems plausible to me that such tests to evaluate FAIR may have been done in existing studies.

2. There is a deeper issue related to non-marginal effects. When considering bilateral net damages simultaneously for a large set of countries, the authors’ approach to evaluating counterfactual scenarios country by country, holding emissions in the remaining countries fixed, could lead to a vast overstatement of L&D because total emissions are multiplied with marginal damages when the true damage is given by integrating under a marginal cost curve. Since the bulk of emissions is inframarginal, this is only accurate if marginal damages are constant and equal average damages. This case seems unlikely on conceptual grounds, and is ruled out in practice by the assumption of non-linear growth effects. Given that only one country’s emissions cause marginal damages, the approach could vastly overestimate the total L&D. Setting t0 at 1990 might limit the extent of this overestimation in your numerical results, but the mathematical inconsistency remains. This is less of a problem for the contribution of individuals and firms (with the possible exemption of carbon majors) whose emissions can be meaningfully characterized as marginal. Notwithstanding this, once the L&D scheme you suggest is rolled out to everyone -not just a few individuals or firms- treating everyone as marginal does not lead to conservative estimates but quite likely to the opposite.

3. In the estimation of future damages, several factors compound to produce large damage estimates.

a. First and foremost, the assumption of direct temperature effects on the growth rather than the level of income likely drives most of the cumulative effects over the 80-year horizon. While the estimated relationships underlying this seem sound (and I appreciate the discussion in the methods and supplemental materials), they play such a crucial role that it seems worthwhile to explore the sensitivity of the FD-CO2 to this fundamental assumption. How much of the effects are driven by the downward sloping branches of the growth-temperature curve as opposed to mere level effects of T on y (i.e., “no growth effects at 2020” scenario)? Currently the compounding effects of exponential discounting and those of exponential growth are subsumed in one number for the main estimates. Figure ED8 shows that changing assumptions about growth and discounting can both have massive effects, especially in cases where one is not keeping the other one in check. Comparing estimates in rows 1 and 5 of that figure strongly suggests that seemingly small differences in the temperature-growth rate relationship can approximately quintuple the SC-CO2 estimates.

b. Another factor is that the FD period considers a longer time horizon than HD which mechanically leads to larger present values. I suppose this is obvious to the authors, but I didn’t see it mentioned in the manuscript.

c. A crucial assumption is the stability of the growth-temperature relationship estimated on data from 1960-2020. This assumption maintains that countries will not be able (or not willing) to dampen the negative income effects as mean temperatures rise. I find this very questionable in light of the authors’ estimates highlighted in the abstract: $4 for HD-CO2 and $327 for FD-CO2. Those numbers imply that virtually all the economic returns on adaptation will accrue after 2020 and provide economic justification for considerable adaptation investments in order to offset the negative growth impacts during the 2020-2100 period. The authors address this possibility to some extent by considering “no effects after …” scenarios, but I think this point merits a deeper discussion of why we should think that adaptation will play no role throughout the remainder of this century and beyond. While statistically sound, the finding that there was no adaptation between 1960-2020 period, when climate damages where minimal compared to the study period, has very limited predictive power for a world in which climate change lowers the levels and growth rates of national income in many countries and, importantly, mankind knows much more about the phenomenon of climate change, its vulnerability to it, and the value of adapting to it.

Assuming no adaptation leads to higher L&D estimates in this framework, which adds to the likely overestimation of total damages discussed in comment 2). The authors make it clear that they cannot econometrically estimate an adaptation term, but some additional scenario analysis, particularly for the 2020-2100 period, could shed light on the issue.

4. The possibility to adapt creates a moral hazard problem because L&D is defined as “the net present value of economic and non-economic impacts attributable to the emissions of greenhouse gases through their impact on the climate, net of any adaptation that was undertaken” (p.3). That is, countries could find it optimal not to invest in adaptation if doing so entitles them to higher L&D payments. I think this aspect deserves to be at least mentioned and possibly elaborated upon. If the authors’ proposed scheme were to be used in practice, then adaptation would almost certainly draw a lot of attention due to its strong impact on the scale of payments.

Minor points:

5. Equation 3 and following: The D function should reflect that damages also depend on the stock of emissions.

6. Figure 1d: To explain the effect of different discount rates, the figure should plot values for two alternative discount rates, not the same discount rate for different discounting horizons.

Typos
- L. 249: calculation “of” bilateral
- L. 276: Figure 1d
- L. 567: where there exist

Referee #2

(Remarks to the Author)
The article develops a framework for quantifying loss and damage from climate change, including the ability to identify monetary losses, and to link them to specific countries, companies, and people or behaviours (e.g. long haul flights). This is welcome at a time when countries have agreed under the United Nations Framework Convention on Climate Change (UNFCCC) to establish a Loss and Damage fund, but with limited specificity about how the fund might work. Some parts of the framework are thought provoking, for example the use of discount rates with historical damages. The article is generally clearly written.

In general I find that the article – for example in the title and abstract – overstates the comprehensiveness of its framework. The framework exclusively deals with losses (or gains) that can be quantified based on the relationship between annual mean temperature and GDP. As the authors note, it does not include losses from sea level rise, for example. It also does not deal with losses associated with those extreme weather events which are not related to annual mean temperature (which is most extreme weather events). I think that if the authors wish to propose something that will satisfy loss and damage negotiators and advocates, this is a big omission. The Loss and Damage negotiations originated in debates over how to address sea level rise. And recent debates have focuses on extreme weather events that do not relate to annual mean temperature. For example specific events that have featured prominently in Loss and Damage discussions are Typhoon Haiyan (prior to the establishment of the Warsaw International Mechanism), and the flooding in Pakistan last year (prior to the establishment of the Loss and Damage fund). It should also be noted that loss and damage frameworks (including the Warsaw International Mechanism) include a focus on the most vulnerable, and studying losses in terms of GDP arguably does not represent the diversity of impacts within a country.
Given the interdisciplinary nature of the topic I also think that the text as currently written has a little too much assumed knowledge about existing research (for example the social cost of carbon, and extreme event attribution).

I would therefore suggest the following major revisions:

1. Revise text to make clear from the start, and in the abstract, that the findings only relate to losses associated with mean temperature effects on GDP. In some cases this is a case of adding more text, and in others a case of removing it. For example line 75 refers to non-economic impacts. However, it seems to me that the framework deals with a very narrow set of economic impacts. I don’t see non-economic impacts reflected in the framework. Early on in the article I also got the impression that the framework would deal explicitly with adaptation and I didn’t really see this reflected clearly later on.

2. Extend and deepen discussion of whether / how the framework might be applied beyond annual mean temperature and GDP.

3. Revise text to make the concepts clearer for the reader. In particular – what is the social cost of carbon? How is it currently calculated and how would it be done differently using this framework?

4. Integrate uncertainty into the reported findings. The study deals with uncertainty, but it comes quite late on in the article. In many places including abstract and section 3.2 one number is quoted for each estimate of loss. This seems misleading given the huge uncertainty ranges associated with these estimates. I suggest the authors give a range, to make clear that the reader that we cannot know these numbers precisely. This is a key issue that anyone wishing to implement a compensation mechanism would need to deal with.

5. Add more references into the rationale to make clearer that there is already early work to try to quantify losses from climate change from the extreme event attribution perspective. Lines 35-36 imply that very little has been done in this space, which could be a bit misleading.

6. Make clear that the UNFCCC decision text from the Paris Agreement explicitly notes that L&D in the Paris Agreement does not refer to liability and compensation. This makes discussion of this article, in the terms it is currently written, quite difficult in a policy context.

Minor comments

27-30 – suggest to provide a citation, since this is just one view of loss and damage.

30-32 – Suggest to provide a citation for litigation focusing on loss and damage. My understanding is that most legal cases about climate change do not currently focus on loss and damage.

57-59 – what is meant by compensation and debt “in an accounting sense”? This should be explained further, as these terms are very loaded in the context of the negotiations. It seems a bit inconsistent to use wording such as "compensation owed by an emitter" and then to state the paper will not address "the ethical question of actual legal obligation". Suggest that the authors explain further their distinction, or consider using other terminology. For example some other literature has used the term "contribution" to refer to the inputs of specific emitters.

69 – "including that L&D is indistinguishable from adaptation efforts and costs," – does this refer to ref 4? If so, I don't think it is quite right, as the interviews with experts did not suggest adaptation was indistinguishable from L&D, but rather that some view the role of the UNFCCC as a whole as avoiding L&D, through mitigation and adaptation. Other viewpoints suggest overlap between actions that might be considered adaptation and those that are designed to deal with L&D. This doesn't mean that adaptation and L&D are indistinguishable, but rather that they might overlap.

77-78 – what does it mean to make individuals "whole"? This seems important to explain for an interdisciplinary audience.

78 – "benefits" – how would these be dealt with? Would the individuals need to pay emitters?

154 – I wouldn't necessarily agree that mean temperature is always well measured. There are still large gaps in gridded datasets over central Africa for example.

170 – CO2s are? – Suggest rewording, perhaps "CO2 is well mixed"?

213 – expected is repeated twice

317 – GDP acronym undefined

561-562 - "To the extent that these channels are quantitatively important, our damage estimates will understate the total damages associated with historical emissions." – as a researcher focusing on climate change impacts this feels like a rather large understatement, given that we are often hearing mean climate is not something people feel the impact of, and extreme weather events have much more impact.

Figure 1 – what is meant by the different orange colours?

Fig 2e – what are uncertainty ranges based on?

Figure ED2 – what is growth here? GDP? In general this figure could use more description.

Figure ED3 – needs more explanation. "Panels show estimated marginal effects from global pooled regression" – effects of what on what?

Figure ED4a – needs more explanation. What is GDP weighted temperature? What is the unit?

BHM – undefined acronym

DJO – undefined acronym

What is the difference between figure 4 and Figure ED12?

Referee #3

(Remarks to the Author)
This is a very interesting paper on an important topic. The authors formalize the clear link between established approaches to assess the Social Cost of Carbon and the topical question of loss and damage quantification (a somewhat similar approach has been suggested e.g. by (1)). They further discuss strengths of their approach as well as remaining challenges. To demonstrate the usefulness of their approach they quantify past and future damages caused by major emitting countries, companies and individuals. I especially like that they also quantify the benefits of more sustainable life choices in terms of damages avoided and that they explore the potential of carbon dioxide removal in limiting future damages.

While I really like the general approach, the specific numbers the authors present depend on a number of modelling choices and assumptions and not all of them are satisfactorily justified and discussed:
1) Assumption of growth effects. As the authors themselves note several times, the magnitude of their damages crucially hinges on their assumption that a permanent temperature increase leads to a permanently reduced growth rate. The question of growth-vs-level effects is a much-debated question in the literature as it has such a strong influence on the magnitude of impacts, especially when assessed over long time horizons, compare e.g. (2, 3). The authors test this assumption using distributed lag models with up to five lags (and it is interesting that the results are less noisy than in Burke et al., 2015!) but theoretically economies could still recover after these five years (4, 5). Given that the authors have such a long panel, is it possible to use even more lags to get more clarity here?

2) Further impact channels: the authors only account for market impacts of changes in annual mean temperature – a limitation they should acknowledge and discuss in more depth

a. Recent work shows that daily temperature variability, extremes, precipitation changes, tropical storms, fluvial floods etc. can bring further economic harms (and in some cases benefits for some regions), especially when impacts are assessed using subnational data, see e.g. (6–12).
b. The authors use national data arguing that studies based on subnational data find roughly consistent results. However, one important difference is that studies using subnational data typically find a lower optimum temperature (13, 14), implying that several countries e.g. in Europe are also harmed by temperature increases (whereas the current study based on national data finds that they benefit, compare Fig. 2 c+d). This could change e.g. Fig. 4. While I acknowledge that there are trade-offs as subnational data are typically not available for many countries in the Global South, the authors should expand their discussion accordingly.

3) Role of adaptation: The authors claim that their estimates are net of adaptation. In fact, their non-linear temperature specification allows them to account for some long-run historical adaptations (to prevailing climate conditions). However, the role of other potentially important moderators such as income is not explicitly assessed like that. If I understand it correctly, the authors' argument here is that they find similar temperature-economic growth response functions for different 20-y periods. As incomes have risen quite a bit since the 1960s this points to limited adaptation. It'll be helpful if the authors discuss this in a bit more detail, also acknowledging that - while historical adaptation may have been limited - unprecedented future warming may spur (or damages may impede) adaptation efforts

** Specific questions/ need for clarification ** – in general it's a bit difficult to follow the methods as they are described in three different sections (Intro, Methods, SI)

1) Eq. 2+ 15: Here, I am a bit confused but maybe this is just because of the use of the term counterfactual which does not seem appropriate with respect to past emissions and temperatures (given that it denotes something that has not happened but could have happened if ...): My understanding is (also following Extended Data Fig. 1 that is very helpful - but I may be wrong here) that what the authors call counterfactual are actually *observed* emissions E and temperatures and the perturbed emissions E' and temperatures are those that we would have had if an emissions pulse hadn't occurred. Correct? But then just after eq. 2 they state that "delta Y is almost always positive" (probably they mean negative?), indicating damages and that income is higher in the counterfactual (do they mean perturbed?) setting with lower emissions which then doesn't fit to the above. A clarification using unambiguous language would be helpful.

2) Eq 11+14: Probably a typo but here the authors should divide by the total population in that country instead of by g. Also: do \lambda and \gamma both denote the population in a given grid cell? The authors should use consistent notation and ideally avoid using the same (Greek) letters for different things (e.g. \delta is simultaneously for denoting time differences, year fixed effects and the pure rate of time preference, r
simultaneously for the discount rate and the ratio of grid-to-global average warming, etc.)

3) Eqs. 22+24: \alpha and y^hat are mixed up/both used to denote the country-specific mean growth rates.

4) Why did the authors pick SSP3-7.0 as main emission trajectory and how does that influence their results?

5) Figs. 3+4: I guess an argument could be made that the discount rates for the damages that have occurred prior to settlement and those in the future are not necessarily the same (e.g. because the future is uncertain whereas past interest rates can be assessed)

6) I understand why the authors assess damages till 2100 or 2300 (and I don't ask them to change that) but this is still an arbitrary cut-off implying that emissions emitted earlier generally cause larger damages simply because damages can accumulate over a longer time horizon. While the authors acknowledge and discuss this, I guess for the whole L&D debate it would make more sense to assess all damages caused within the subsequent x years

** Minor comments **
1. Data: the authors only state that they're using ERA5 data and the reference says it is actually ERA5-Land (but the reference is not displayed correctly), so I'm assuming that the data are on a 0.1°-grid and have already been aggregated to the monthly level? This information should be added to the Data & Methods section.

2. Fig 1a: are these cumulative losses?

3. ED Fig 2
a. Panel a: labels could be more specific/explained in caption (e.g. does 'linear TT' mean linear temperature term ?!)
b. Panel b: how big is the sample for the different periods?
c. It would be helpful if both panels had the same y-axis

4. There are some typos/missing words, abbreviations not spelt out when used for the first time, and especially in the Supplemental Materials the language is very colloquial (precip instead of precipitation, etc.)

** References **
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10. R. Damania, S. Desbureaux, E. Zaveri, Does rainfall matter for economic growth? Evidence from global sub-national data (1990--2014). J. Environ. Econ. Manage. 102, 102335 (2020).
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13. M. Burke, V. Tanutama, “Climatic constraints on aggregate economic output” (2019).
14. M. Kalkuhl, L. Wenz, The Impact of Climate Conditions on Economic Production. Evidence from a Global Panel of Regions. J. Environ. Econ. Manage. 102360 (2020).

Version 1:

Reviewer comments:

Referee #1

(Remarks to the Author)
I appreciate the changes the authors have made to the manuscript and extended materials in response to my comments and those of other referees. I think they have resulted in a more streamlined paper and more compelling presentation of the results.

Overall, I am satisfied with the authors’ responses to the substantial points raised in my last report. The additional robustness analysis and changes in the text go a long way to clarify the modeling choices made and spell out their limitations.

Regarding the estimated temperature-growth relationship, a central element of the analysis, I appreciate the additional work the authors have put into estimating distributed-lag models and local projections. As a result of the authors’ changes to the econometric specification, the estimates of per-ton L&D have changed by orders of magnitude. For example, HD-CO2 has increased from an intial $4 to $180 in the new version, similarly FD-CO2 from $327 to $2,000 per tonne. Hence these estimates seem to be highly sensitive to the estimated temperature-growth relationship. As the authors do not fail to mention, this relationship continues to be heavily debated in the literature. For example, the recent paper by Nath, Ramey and Klenow (2023) argues that estimates of warming on GDP based on the methodology in Burke, Hsiang and Miguel (2015) tend to be at the upper end of the range of estimates in the literature. Estimates by Nath et al. are one order of magnitude lower than BHM. While this particular paper has yet to undergo peer review, it highlights once again that future climate damages projected over long time horizons critically depend on whether or not temperature changes have permanent effects on economic growth.

Not surprisingly, the authors have taken a particular stand on this issue, arguing for the BHM approach. In light of the above-mentioned large parameter changes they have obtained in going from their previous implementation of BHM to the current one, I think it would be wise, in case of acceptance, to require them to publish a replication package that includes all data and code, allowing other researchers to replicate the L&D estimates in the paper as well as conduct their own sensitivity analyses.

Minor comments:
- I appreciate the way cumulative uncertainty has been integrated into the presentation of results.
- L. 132 better: in *the* last 60 years
- L. 152 “there” instead of “their”
- L.227-229: I did not understand the meaning of the two sentences until reading the explanations given by the authors regarding this in the rebuttal. Can this be rephrased?
- I find the description of scenarios in ED9 too cryptic- can this be expanded or better explained?
- In Figure 4, looking only at emissions from the most recent 30-40 years can be justified as in lines 599 and following, but the figure would likely look very different if historical emissions were considered all the way back to 1960 or earlier. I think this would be interesting. On the recipients’ side, the ranking is not very surprisingly given by GDP levels, given that all growth effects are relative to these levels.

(Remarks on code availability)
I have reviewed the code because downloading this package might allow authors to find out about my identify. Packages should be hosted elsewhere where authors have no way of monitoring from which IP address the data are downloaded.

Even in this case, I think that the code should best be reviewed by a professional data editor. It seems unlikely that referees can accomplish this in the short amount of time given.

Referee #2

(Remarks to the Author)
The authors have responded to all of the reviewer comments, including doing new analysis.
The new set of figures has some very large differences to the original manuscript. This has not been adequately highlighted in the response to reviewers, and I confess that I only noticed as I was about to submit my review. In Figure 2 and Figure 4, the largest estimated impacts (by quite a margin) are in the USA. I think the authors need to explain why this is, and what this means about their methods: are they very sensitive to methodological choices? Do the estimated damages by country differ dramatically based on different methodological choices? This has important implications if the framework were to be used in policy. Equally, they should comment on the implications of the results – if the largest losses and damages are in the USA, what does this mean for L&D as an approach to address climate change impacts in vulnerable countries? Does this imply that the USA is the most vulnerable to climate change? And what about the countries that have been campaigning for L&D since the 1990s? (i.e. small island states).
Clearly, a framework which estimates L&D based on GDP will always estimate larger damages in countries which have large GDP and relatively small damages in countries that have low GDP to begin with. So is this the best approach to support low emitting countries suffering impacts from extreme weather events and sea level rise which are exacerbated by emissions from countries with high GDP? This relates to the comments in my previous review - that the paper does not sufficiently emphasise that this is only a partial estimate of L&D.
The three main issues I had with the paper originally have also not been fully addressed:
1. The paper still minimizes the importance of (a) hazards which are not related to mean temperature and (b) impacts which are not related to GDP. (a) and (b) are arguably the majority climate change impacts which matter for people. These are also the impacts that are more often referred to in discussions around Loss and Damage.

2. In places, the wording and emphasis in the paper is still quite difficult to understand and put in context for a reader a different academic field.

3. The paper makes only passing references to extreme event attribution research, which is another approach that has been discussed in the context of loss and damage negotiations. In fact, there has been quite a lot of literature, over the past 10 years or more, which has discussed extreme event attribution and the challenges and opportunities for using it in the context of L&D policy. Some of the same issues are coming up here, for example: the challenge of developing a scientific approach when the policy is vague, the challenges of describing an approach to compensation, when compensation is a word that some negotiators are not even allowed to say out loud, and the challenges of developing scientific evidence for supporting those impacted by climate change, when those countries are also those for whom the strength of scientific evidence is likely to be weakest due to data limitations. I did not fully elaborate on this point in my previous review, as I thought it might be the case that the authors were aware of this work, but needed to add a bit more comment and citation. On reading the response to reviewers and looking again at the paper, in particular the new additions at the end of the discussion, I think maybe the authors are not aware of this literature, and therefore I have suggested some references that are important to help frame the paper (Hulme et al. 2011, Huggel et al. 2016, James et al. 2019, King et al. 2023).
These three issues make it difficult to connect the work with the existing wealth of literature on climate change impacts and adaptation, and growing literature on loss and damage. Given the broad readership of Nature I think further changes are needed to help the reader understand the implications and scope of the study. Furthermore, from a policy perspective it is important that the reader can understand that the framework proposed cannot address many (or even most?) climate change impacts. If the framework from the paper were picked up and used to design the distribution of L&D funding, it would overlook important impacts, including the impacts of sea level rise on small island states, impacts for the most vulnerable, and loss and damage following specific disasters.
I have made some specific suggestions to help address these issues.

Suggested corrections and comments

Title: "Quantifying climate change loss and damage consistent with a social cost of greenhouse gases" – I don't think that "the social cost of greenhouse gases" will be very meaningful to people outside of economics. I also think "social" is a strange word to use to describe impacts on GDP, since the paper does not deal with other impacts which will have much more "social" impact e.g. loss of housing from flooding, loss of homeland from sea level rise, loss of livelihood from drought, loss of cultural heritage, damage to mental health from extreme weather events… I suggest a modification to the title: "Quantifying climate change loss and damage associated with the impact of average temperature on economic growth"

Abstract – the authors have made several important changes to the abstract – noting that they are only referring to "one component of L&D" and also adding confidence intervals around the estimates. This is much appreciated. I think that, with the suggested change to the title, this is sufficient to make clear that the paper is focusing on average temperature and aggregated economic output. However, if the title is not changed, I would like to see additional wording added to the abstract, something like “It is important to note that many climate change hazards (e.g. sea level rise, flooding, drought) are not related to average temperature, and many losses and damages are not related to aggregated economic impacts. Therefore our estimates of L&D are likely to be a substantial underestimate of all L&D from climate change.”

Line 34 – 39 – the authors have added more recognition of the work in extreme event attribution, which is much appreciated. However, these sentences imply that extreme event attribution research only relates emissions and impacts. In fact there has been some work in extreme event attribution which links emissions to specific countries or companies. There is a summary of this in a book chapter James et al. (2019), section 5.3.3. I suggest citing a couple more papers, and rewording to acknowledge this (suggested change in italics).

“However, unlike the large body of empirical work which explores how global climate change will affect global and local economic outcomes, the question of whether and how emissions by specific entities (e.g. countries, companies, or individuals) can be linked to specific damages has received less formal and empirical attention in economics – although substantial headway has been made in understanding how the probability of specific extreme events and their impacts can be linked to a changing climate13–17.”

Reviewing this paper has made me reflect that it would be fantastic for there to be more collaboration between the econometrics community and the extreme event attribution community, and it would be fantastic to get an economics perspective on research in extreme event attribution that has estimated monetary losses from extreme weather events.

Line 46-48 – Suggest to edit (edit in italics):

“Drawing from multiple fields, and focusing on the impacts of average temperature on aggregate economic output, we show how…”

Line 51-56 – The authors have rephrased their comments about L&D with respect to the Paris Agreement. I appreciate the effort to navigate a difficult political topic. However, this reasoning is a little hard to follow – what is the distinction between stating that L&D should reimburse damages from those who have emitted, and stating that this is not necessarily what is “owed”? Is the idea to suggest a possible approach to L&D, but then to say that this approach is not necessarily morally correct? The next paragraph then goes on to talk about liability, which is explicitly excluded in the Paris Agreement. So I don’t think it is possible to say that the proposed approach is consistent with the Paris Agreement.

The “completely reimburse” and reference to “non-economic impacts” in this paragraph are also a bit confusing – since the paper does not propose a framework to deal with non-economic impacts, and – as the authors note – it is “partial” so it does not provide a framework for how to completely reimburse loss and damage.

One suggestion for how to address this: the authors could (a) acknowledge L&D is contentious, that the Paris Agreement explicitly states that L&D does not refer to liability or compensation, (b) note that there are some who nevertheless view L&D as a route towards compensation, and that there are important discussions about compensation and reparations outside of the UNFCCC framework, for example with reference to legal cases, (c) therefore note the potential value of academic analyses to conceptualise how L&D can be related to specific emitters, (d) highlight the authors own suggested approach to conceptualise L&D, (e) highlight that their framework shows how to compute part of this L&D.

Line 69 - could you explain a little more what is meant by marginal? I note there is lots of discussion with the other reviewers about marginal emissions/costs/effects, but I think for a wider readership it is not obvious what is being referred to. Please also check other references to marginal later in the text.

Line 79 – The authors have added more information about the SCC which is much appreciated. What is meant by “nests”? Please clarify.

Line 98-99 – suggested edit (edit in italics):

“Our implementation allows estimation of past and future economic damages associated with average temperature change from any observed or projected emissions perturbation”

Line 142-143 – “individual populations” – does this mean country populations? Suggest to rephrase as “individual countries” or “country populations”

“damages” – is this damages to GDP? Suggest to rephrase “cumulative damages to GDP”

Line 148 – what is meant by local warming? Is it country level? If so suggest to rephrase e.g. “country-level warming”

Is “damages” damages to GDP? Suggest to rephrase “damages to GDP”

Line 153-157 - this is clearer, thank you. Suggest to add “country-level” before warming, line 154. Suggest adding “(including from sea level rise and extreme weather events)” after “warming climate” (line 156). Suggest adding “(such as loss of life, damage to mental health, loss of livelihood, loss of homeland)” after “non-economic damages” (line 156).

Line 209-212 – It’s interesting that in this part of the text and in the response to the reviewers, the authors’ emphasis is on

rates of past adaptation as a predictor of rates of future adaptation. What about policy and public awareness? Although it arguably hasn't been that successful yet, recent decades have seen a huge growth in initiatives around climate change adaptation. Do the authors assume at all the effort, funding, research that is being invested in deliberate adaptation by countries, companies, farmers, households and beyond will not work? If so, suggest to briefly note why this is. If not, suggest to rephrase the sentence "it is of course possible that
future adaptation could exceed current adaptation" so that it is does not imply that future adaptation is unlikely.

It would also be useful to compare the findings of L&D with and without adaptation with other research to estimate the benefits of adaptation e.g. Faldauer et al. (2022).

Line 221 – Suggest to edit "L&D" to "L&D to GDP from average warming"

Line 222-224 – This part is a bit tricky to follow, could a little more explanation be added?

Line 230-291 - How uncertain are these estimates? Is it possible to add uncertainty ranges?

Line 300 – "simple scenario" – would "hypothetical scenario" be more appropriate?

Line 319 – suggested edit (edit in italics) – "develop a framework for estimation of L&D to GDP from average warming". It is misleading to say that the framework is developed for the estimation of L&D in general, since it does not cover all types of L&D.

Line 354-358 – one of the challenges in adapting to date, has been that parties to the UNFCCC have failed to deliver on the adaptation funding they've promised. For similar reasons, it might be a struggle to generate L&D funding. This might be a further challenge worth discussing.

Line 391-393 - suggest to add more examples to help the reader put the work in context. For poorly captured in GDP data examples could also include loss of income by farmers (where crop failure due to high temperatures might increase prices and therefore lead to increases in GDP or no change in GDP whilst farmers experience loss and damage). For non-economic impacts, examples could include damage to physical and mental health. For impacts not highly correlated to country-level temperatures, examples could include tropical cyclones, heavy rainfall events, ocean acidification, marine heat waves.

Line 396 – "To the extent that these channels are quantitatively important" – this seems to imply that the quantitative importance of climate change impacts beyond mean temperature is in question? This seems misleading. Frame et al. (2019) estimate that at least $30bn of damage from Hurricane Harvey can be attributed to climate change. Of course, this is just one extreme weather event. Otto et al. (2020) describe how estimates from many extreme weather events could be collated to develop an inventory of impacts from anthropogenic climate change. Frame et al. (2018) demonstrate a similar approach for New Zealand, estimating that climate change related floods and droughts cost New Zealand >$800m over a recent 10 year period. This is without mentioning the economic implications of sea level rise. Perhaps the sentence could be rephrased:

"Given the substantial economic damages associated with extreme weather events, some of which can be attributed to climate change (Frame et al. 2018, 2019), the impactful nature of sea level rise, and the importance of non-economic loss and damage (Tschakert et al. 2019), our damage estimates understate the total damages associated with historical emissions, likely substantially."

Line 400-403 – Could the authors expand a bit on which cases this is more likely to be applicable for? For example, many non-economic impacts (such as damage to mental health) are less well documented, more difficult to predict, and difficult to quantify.

Line 404-411 – It is important to note that these criteria are most often missing in places that are highly vulnerable to climate change, as has been discussed with reference to extreme attribution research, with many researchers expressing caution at the use of science for policy for this reason. See Hulme et al. 2011, Huggel et al. 2016, James et al. 2019, King et al. 2023).

Line 457 – "Our focus on average temperature helps overcome concerns regarding the use of extreme event attribution in L&D assessments" – perhaps, but it raises new concerns, because the hazards and impacts that many L&D campaigners care about are ignored. The focus on GDP will also lead to additional concerns because the largest damage estimates for the richest countries.

References

Frame, D., Rosier, S., Carey-Smith, T., Harrington, L., Dean, S., & Noy, I. (2018). Estimating financial costs of climate change in New Zealand. 1–18. https://treasury.govt.nz/publications/commissioned-report/estimating-financial-costs-climate-change-nz

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Huggel, C., Wallimann-Helmer, I., Stone, D., & Cramer, W. (2016). Reconciling justice and attribution research to advance climate policy. Nature Climate Change, 6(10), 901–908. https://doi.org/10.1038/nclimate3104

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Tschakert, P., Ellis, N. R., Anderson, C., Kelly, A., & Obeng, J. (2019). One thousand ways to experience loss: A systematic analysis of climate-related intangible harm from around the world. Global Environmental Change, 55(July 2018), 58–72. https://doi.org/10.1016/j.gloenvcha.2018.11.006

(Remarks on code availability)

Referee #3

(Remarks to the Author)
I thank the authors for the clarifications and additional analyses. I continue to consider their framework as really useful and the applications as quite illustrative. With regards to the specific numbers they present, their magnitudes have increased considerably compared to the initial version, partly by more than an order of magnitude (with Social Cost of Carbon estimates in the order of >$2000/ton). This is because the authors now do not only assume growth effects but also lagged growth effects, i.e. the growth effect of a temperature shock builds up over five years and then stabilizes at that level. To justify that, the authors present distributed lag models where they regress growth on temperature levels with up to 10 lags and a local projections approach.

Given that this assumption has such a big influence on the damage magnitudes, the authors should do more to justify + discuss this choice:
1) From the figures alone, it is hard to judge how noisy/significant the individual lagged terms really are and how well supported this assumption is by the data. The authors should add more details on both approaches; in particular:
a) What about concerns of serial correlations - how did they arrive at the conclusion that the inclusion of one lagged growth term in the local projections approach is sufficient and why did they include lagged growth terms but not lagged temperature terms?
b) how are standard errors treated in both approaches?
They should present regression coefficients + their significance levels, information criteria for models with different numbers of lags and plot the respective coefficients incl. their uncertainties such that one can better see how well the lagged growth effects assumption is supported by the data.

2) Given that the question of growth-vs-level effects is heavily discussed in the literature (1, 2) and that the authors here opted for something one could call "growth effects plus", they should add more discussion on that choice and its implications. For example, more conservative approaches have been proposed where growth is regressed on first-differenced temperature or temperature shocks, implying that the model prior is one of level effects and it is then tested whether the effects persist any further (3–5). While certainly arguments could be made supporting less conservative modelling choices with respect to climate risks, the authors should make more transparent + discuss in the context of the literature that (and why) they are here opting for a less conservative model prior of growth effects.

Smaller points:
Thanks a lot for streamlining the methods – it's now much easier to follow. I'm still a bit confused by the statement that the baseline is what has been observed in the past (eq. 2, l. 482)– shouldn't it be the other way around? What has been observed in the past is our world in which this emission pulse has actually occurred and that should be compared to a counterfactual world without that additional emission pulse? I guess in practice it doesn't really matter (also following the authors' argument on the not-so-marginal historical emissions of some countries) and like that it's more consistent with the SCC framework but a short clarification would be helpful + appreciated.

l. 199: the authors state that the SC-CO2 is US$ 2379/ton - where can I find that in Fig 2? I guess it is US $2120/ton? What number is correct?

The explanation that higher discount rates imply that the value of past damages is larger at the time of settlement compared to when they occurred is detailed at least 3x in the manuscript – consider shortening

l. 396-397: “to the extent that these channels are quantitatively important ...”: compare refs. (3, 6).

check eq. (16), in particular the product operator ∏_(t=k)^t and what is initial year k?

Fig ED3: add proper label to y-axis, recommended to use g (not y), as in eq. (19)

Additional SI section on robustness of local projections approach: some typos

References
1. B. A. Bastien-Olvera, F. Granella, F. C. Moore, Persistent effect of temperature on GDP identified from lower frequency temperature variability. Environ. Res. Lett. 17, 84038 (2022).
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4. I. B. Nath, V. A. Ramey, P. J. Klenow, “How Much Will Global Warming Cool Global Growth?” (2023).
5. A. Bilal, D. R. Känzig, “The Macroeconomic Impact of Climate Change: Global vs. Local Temperature” (2024).
6. P. Waidelich, F. Batibeniz, J. Rising, J. S. Kikstra, S. I. Seneviratne, Climate damage projections beyond annual temperature. Nat. Clim. Chang., 1–8 (2024).

(Remarks on code availability)

Version 2:

Reviewer comments:

Referee #1

(Remarks to the Author)
I am satisfied with the authors' replies to my comments and those of the other two reviewers. I found the revised manuscript much improved and have no further comments.

(Remarks on code availability)

Referee #2

(Remarks to the Author)
The authors have written a thorough response to all the review comments, as well as doing some new analysis (for example, calculating damages as a percentage of GDP in Figure 2). I am grateful to the authors for being so thorough, and I am satisfied with their responses to my comments regarding:

- Clarifying terminology to make it clearer to people from different fields
- Acknowledging related research in extreme event attribution
- Description of Loss and Damage policy and it’s connection (or not) with compensation

They have also edited the text to make it clearer that the paper refers to temperature impacts on GDP only, and to highlight that there are other climate change impacts that are also damaging and relevant to L&D policy.

However, I still have some concerns with the current manuscript.

1. I am still struck by how sensitive the results are to different methods, which has also been highlighted by the other reviewers. Whilst the authors can be commended on how many different methodological choices they have tested, I do not think there is adequate emphasis in the text to make it clear to the reader that the estimates have massive uncertainty ranges. Figure ED12 has confidence intervals which span an order of magnitude, but they are hidden away and would not be noticed by most readers.

I also think it is fundamental that this uncertainty is given adequate discussion (at a minimum, at least a large paragraph at the beginning of the discussion section, and ideally also highlighted in the abstract). The message the reader might get from main text is that the methods for quantifying L&D are almost there, and ready to be used for policy applications. But if the methods were to be applied, how should the user decide which methods and which part of the uncertainty range to use? And would the level of uncertainty lead to controversy in the application of the method? L&D is already controversial. If someone were to try to take Aramco to court for climate damages, would it be an issue that the estimated future damage has a range of $16T - $194T? Furthermore, I believe that this confidence interval is only based on the new method, and if the previous method were to be used there might be a different estimate again? Perhaps there are ways around applying the results given the methodological sensitivity, but it should as a minimum be discussed.

2. In the previous review I highlighted that the new analysis has shown that the USA was the country with the most L&D, which is interesting and something which warrants further discussion. The authors have responded to my comment in the response to reviewer document, and also edited Figure 2 so that the damages are presented as a proportion of GDP. However, they have not added more discussion. Figure 4 still shows the important finding that this method demonstrates the most damage in the USA, followed by EU, followed by China. This is noted in the text, but the implications of it are not discussed. Please add a paragraph to the discussion to highlight this and discuss the implications for applications. Does this imply that the USA should receive compensation for damages from climate change? For some people L&D is all about equity and justice – it feels like an oversight not to discuss this more.

3. The authors have diligently responded to my comments to make clearer that the paper is about the impact of mean temperature on GDP, and not all L&D from climate change. However, I still think that – reading this paper again, as a researcher who works on climate change impacts and L&D – it feels quite disconnected from the literature I am more familiar with. When I first came across the relationship between mean temperature and GDP (Figure ED2) I was shocked! I did not expect there to be a relationship between these two things! It's very interesting, but I think quite surprising and unintuitive for many people who work on impacts and adaptation. As a climate scientist in this space I am constantly being told by colleagues who are experts in vulnerability and adaptation "average temperature does not impact people, we need to look at extreme events, or what matters on the ground". I think these same people will see a paper on L&D in Nature and want to read it. To draw them in and help them connect with this work, I would suggest to add a sentence or two in the introduction which introduces and justifies the focus on mean temperature and GDP. Perhaps above the current line 143. I would suggest to highlight that there is a body of work that examines climate change impacts through the lens of mean temperature and GDP, that there is evidence for a strong interannual relationship there, and briefly outline some of the reasons why mean temperature impacts GDP.

Minor comments

Line 19 – please add uncertainty range
Line 20 – please add uncertainty range
Line 24 – missing apostrophe? Should it be "temperature's effect on the aggregate economy"?
Line 168 – suggest to remove "unmodeled" - not sure what "unmodeled climate extremes" are?
Line 250-255 – please add uncertainty ranges into text
Line 257, Line 258, Line 279 - please add uncertainty range into text
Line 374 – "limited evidence that adaptation has been happening in the aggregate in recent decades" - I assume this refers to limited evidence that there has been adaptation in terms of GDP growth to mean temperature? This may also be because in impacts and adaptation work mean temperature is often viewed as something that doesn't really have an impact on people, and therefore adaptation efforts focus more on other things, for example early warning systems to reduce impacts from extreme weather, and infrastructure to cope with sea level rise. For example, Indian cities have implemented heat action plans, which may not have benefits for GDP but – if effective – could have huge benefits for public health. I would suggest to nuance your statement – it is misleading to suggest that adaptation is unsuccessful by only looking at mean temperature and GDP, and undermines the efforts ongoing to try to adapt to other climate change impacts.

(Remarks on code availability)

Referee #3

(Remarks to the Author)
This is a great paper that formalizes and illustrates the straight-forward yet very helpful proposition to use the Social Cost of Carbon framework to help guide loss & damage funds and climate change litigation.
There is one major issue in the computation of the specific damages and hence numbers the authors present: They argue that a one-time temperature shock is not recovered but leads to permanently lower economic output. That is, the growth rate reduction in that year is not compensated by more growth in one of the following years. In Fig ED3 they show that for their historical data, this is the case for at least 10-15 years as the cumulative marginal effects are then still different from zero. They then assume (and yes, this is an assumption), that it will also not be recovered after 20, 50, 80 years and apply this to the future (up until 2300 for some projections).
What this implies is that a permanent temperature increase (as can be expected under future warming) will lead to permanently reduced growth rates. If the local temperature increase is big enough, growth rates can be permanently negative and economic output in those places will become zero at some point (a 100% reduction in GDP as can be seen in Fig 4 in Burke, Hsiang & Miguel (2015), abstracting from discounting here). Countries will not be able to recover from this anymore, unless temperatures go down again.
This is a fairly big assumption to be made for decades into the future - even if such effects can be identified in historical data for 10-15year periods. In the last two rounds of revisions, I have hence asked the authors to show more support for this assumption and make its implications more transparent. I appreciate that they now show results for 5 and 10lags, respectively, as well as for a local projections approach, incl. robustness checks. However, the same shortcoming applies here: since you can only include a certain number of years x, you cannot know what will happen after these x years.
Ideally one would thus include as many lags as possible (I completely agree with the authors that starting with a large

number of lags is important for model selection) but estimates become noisier, the more lags are included, rendering the discrimination between growth and level effects inherently challenging.
A more conservative approach (and by conservative, I mean less strong assumptions about the future, not lower damages per se) which is increasingly applied in the literature is thus to assume that future damages persist for as long as could be robustly identified in the historical data but no longer than that.
The authors should make the implications of their growth effects assumption transparent, ideally show a range of outcomes under different persistence assumptions.
Apart from this specific but crucial aspect, this is a great paper and I will be happy to see it published. As the authors mention themselves, the general framework does not depend on one specific damage function but can be used to compute ranges of possible outcomes under different damage functions and assumptions.

(Remarks on code availability)

Version 3:

Reviewer comments:

Referee #2

(Remarks to the Author)
The authors have responded to all my comments, and the revised manuscript reads well. I just have one final minor comment.

Line 222 – remove “somewhat” – as this is an understatement

(Remarks on code availability)

Referee #3

(Remarks to the Author)
Thank you for exploring how alternate persistence assumptions affect your estimates.

Since the damage function is so key to your analysis but not shown explicitly, it will be helpful if you add a respective figure (e.g., as in Fig 5a in BHM2015), for better comparability with other damage functions.

Other than that, I have no further comments.

(Remarks on code availability)

Below we respond to each referee comment. Original referee comments are in bold, our responses are in plain text, and changes we made to the manuscript are in italics.

Referee #1 (Remarks to the Author):

This paper proposes a methodology for computing loss and damage (L&D) of CO2 emissions to be used for the attribution of climate change damages that materialize through time to specific emitters. The authors develop this methodology based on insights from the literature on the social marginal cost of carbon. Key elements are (i) the construction of counterfactual scenarios that characterize damages without the contribution of a particular emitter, which is based on a reduced-complexity climate model and pattern scaling, and (ii) the prediction of macroeconomic impacts, captured by an econometric model relating local average temperatures to local income (GDP). The authors show that cumulative future damages of CO2 emitted in 1990 (to materialize between 2020 and 2100) are three orders of magnitude higher than their historical counterpart, defined as the damages caused by those emissions between 1990 and 2020. Defining L&D as the sum of those two, the authors apply their method to compute L&D for countries, firms and even individuals. They also provide estimates of bilateral L&D between pairs of countries or regions, and show how the value of carbon dioxide removal can be assessed within their framework.

This paper strikes me as highly original in how it combines results in climate science, economics and accounting to advance research on a difficult and policy relevant measurement issue. The results are significant insofar as they provide very elaborate quantitative estimates of economic damages of emissions past. Those estimates provide guidance for carbon offset payments by individuals and firms that are conscious about their past CO2 footprint and the consequences for global climate change. The results may also be applied in an international policy context, as the authors suggest, though I believe this to be a second-order application given that the underlying assumption that all emissions are marginal is not appropriate in this context(more on this below). There is a lot of material in this paper, and the authors are not very explicit about what they themselves regard as the most significant contribution. While a lot of the motivation draws on suggest examples from the international policy context, the authors also caution against regarding the L&D estimates as legally binding. To sum up, the significance of the rich set of results summarized in Figures 2-4 lies in the measurement, not so much in any policy implications that might follow from them.

The paper is very well written. The exposition and figures are clear and highly didactic. While this made for truly enjoyable reading, the paper strikes me as somewhat long by journal standards.

The authors make appropriate use of statistics. They use best-practice methods to quantify multiple sources of scientific uncertainty. References are appropriate.

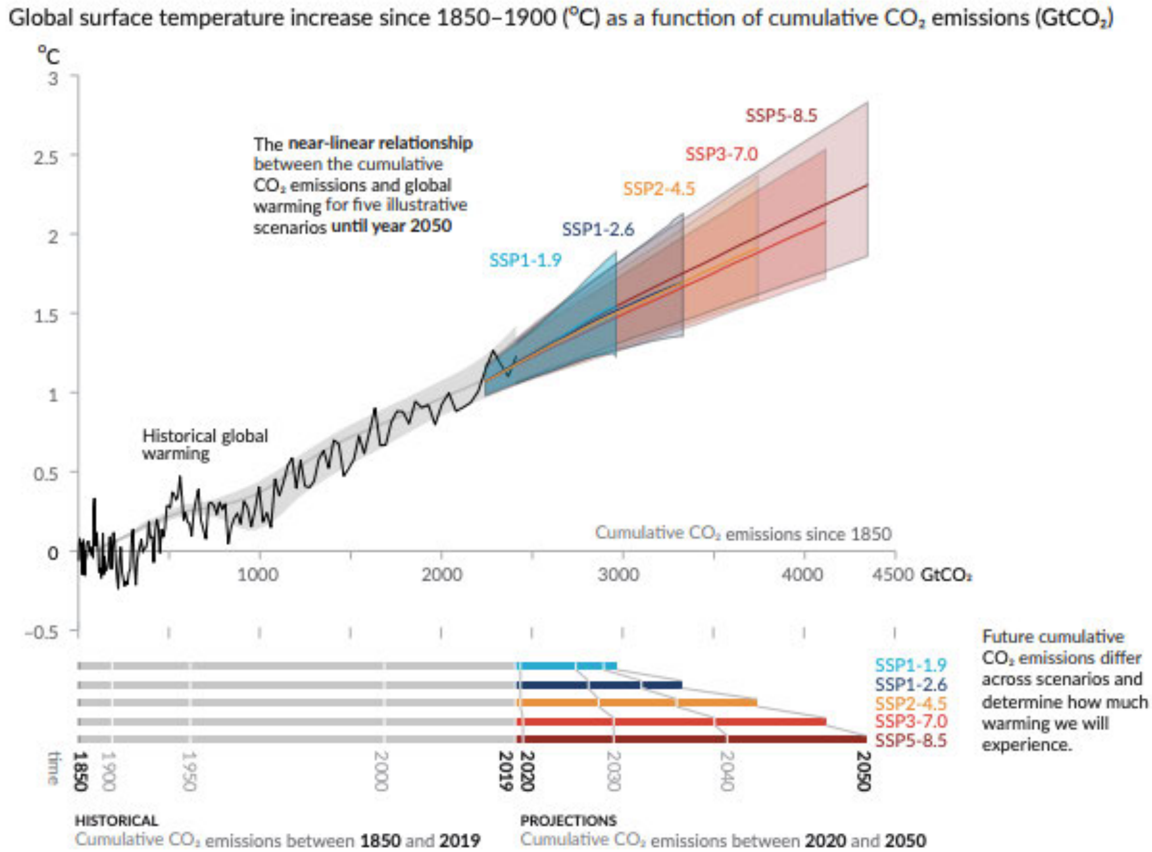
My main comments concern the validity of the approach.

1. I wondered how well the climate model and pattern scaling account for non-linear effects of CO2 on temperatures, in particular when computing counterfactual emission paths for large emitters. Figure ED1a suggests that for a large emitter like the US, L&D calculations will involve non-negligible changes to world emissions. If the true relationship between emissions and GMST is non-linear (tipping points being an extreme case), a reduced-complexity climate model could potentially lead to large errors in such a case. On p. 20 of the methods section, the authors briefly address this issue, but I needed more convincing to believe that non-marginal changes hardly affect the results, as is claimed there. For example, the authors could show results comparing the forward run to perturbation experiments for the large magnitudes considered here, e.g. a fifth of world emissions. Given that FAIR is a "reduced-complexity model" (p.9, l.310), such comparison would be more credible if the reference is not FAIR itself but a different, full complexity model. I am concerned that the extensive use of linear approximation in reduced-complexity models might lead the authors to get a false sense of accuracy when comparing results across different pulse sizes, or even in a full run of the FAIR model. A test with an alternative model would seem to have much more power to reject linear effects. It seems plausible to me that such tests to evaluate FAIR may have been done in existing studies.

We thank the referee for this comment, and the referee is absolutely correct that our approach relies on FaIR to be able to correctly calculate the impact of non-negligible changes in emissions on GMST.

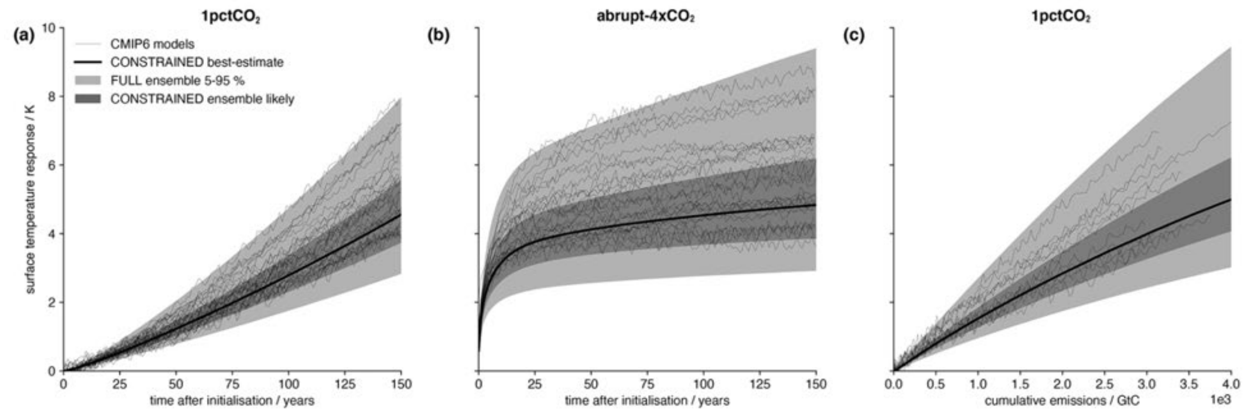
We first note that decades of climate science research (e.g. as summarized by IPCC AR6 WG1) has clearly established the proportional relationship between cumulative emissions and GMST. Below is the canonical cumulative emissions plot from IPCC AR6 WG1. As the plot shows, warming is linear in past emissions and expected to be linear in future emissions. Thus we do not agree that we should expect a reduced-complexity model to predict a non-linear relationship between emissions and warming: full-complexity models do not predict such a relationship.

Second, as we have now elaborated more fully in the text (see below), we note that FaIR was designed precisely to do what the referee wants: to correctly emulate the behavior of full-complexity models in the response of GMST to large emissions perturbations. The paper documenting FaIR v2 (Leach et al 2021) implements exactly the experiments the referee seems to have in mind, showing in detail that FaIR can reproduce the global climate system response to GHG and aerosol emissions simulated by the much more complex models in CMIP6. Not only does it match projected change in GMST under different emissions scenarios, but it also reproduces key climate processes (e.g. carbon cycle response) underlying these temperature responses. Below is Fig 8 reproduced from Leach et al 2021, which shows that the 90% credible interval of the FaIR ensemble runs (shown in light gray) spans the CMIP6 model ensemble range for both a gradual increase in CO2 concentrations of 1% year and an abrupt 4x increase in CO2 concentrations.



We have now further clarified these points in the Methods text. The relevant section now reads:

"*To translate any emissions history into a change in local climate, we first use a reduced-complexity climate model to translate emissions into a change in global mean surface temperature (dGMST), and then use the CMIP6 ensemble of fully-coupled global climate models (Eyring et al 2016) to translate global temperature changes into local changes. Specifically, for the first step, we use v2 of the Finite Amplitude Impulse Response model (FaIR) (Leach et al 2021), a reduced complexity climate model with a carbon cycle that reproduces the established linear relationship between cumulative emissions and warming (Masson et al 2021) and is able to simulate equilibrium and impulse-response behavior of more complex global climate models, including adequately capture the range of responses in the CMIP6 climate model ensemble to multiple large emissions perturbation experiments (Leach et al 2021).*"



2. There is a deeper issue related to non-marginal effects. When considering bilateral net damages simultaneously for a large set of countries, the authors' approach to evaluating counterfactual scenarios country by country, holding emissions in the remaining countries fixed, could lead to a vast overstatement of L&D because total emissions are multiplied with marginal damages when the true damage is given by integrating under a marginal cost curve. Since the bulk of emissions is inframarginal, this is only accurate if marginal damages are constant and equal average damages. This case seems unlikely on conceptual grounds, and is ruled out in practice by the assumption of non-linear growth effects. Given that only one country's emissions cause marginal damages, the approach could vastly overestimate the total L&D. Setting t0 at 1990 might limit the extent of this overestimation in your numerical results, but the mathematical inconsistency remains. This is less of a problem for the contribution of individuals and firms (with the possible exemption of carbon majors) whose emissions can be meaningfully characterized as marginal. Notwithstanding this, once the L&D scheme you suggest is rolled out to everyone -not just a few individuals or firms- treating everyone as marginal does not lead to conservative estimates but quite likely to the opposite.

If we understand the referee's point, the general worry is that the impact of emissions perturbations – both on temperature and on economic impacts – could depend non-linearly on the size of the perturbation and whether other emissions are changing at the same time. We agree that this is a potential concern. To address this, we now do three things in the revised manuscript to ensure that damages from non-marginal emissions changes are adequately measured.

First, and exactly to the referee's point, we do <u>not</u> use estimates of damages from a marginal emission to calculate total damages from non-marginal changes. This is admittedly confusing since we had written the equations in the main text, for simplicity and clarity, to show how to calculate damages using marginal emissions. Our actual calculation of damages for any given emissions perturbation uses the actual perturbation to calculate damages rather than using the estimates of damages from a marginal ton and multiplying by the number of tons in the perturbation.

Second, we conduct additional experiments to understand whether the per-ton impact of an emissions pulse depends on the size of the pulse. Using an emissions year of 1990, we vary the size of the pulse by 8 orders of magnitude (1000t pulse to 100Gt pulse) and calculate the per-ton damages for each. We find that the largest pulses have per-ton effects that are only a few percent different than the smallest pulses – again reflecting the fact noted above that warming is a function of long-term cumulative emissions. See new Fig ED8a.

Third, we conduct yet another set of additional experiments where we calculate what happens to our estimates of damages from US emissions if we assume that no other country changes their emissions at the same time (our baseline assumption) vs whether we assume that all countries simultaneously reduce their emissions by 10%, 30%, 50%, or 70%. As shown in new Fig ED8b, we find that impacts of US emissions are smaller when other countries also reduce their emissions, but that differences are again very modest - only a few percent relative to our baseline. Damages are smaller under joint emissions reductions relative to unilateral emissions reductions because US emissions are acting on a world that is slightly cooler than it otherwise would have been. However, this effect is again small for the same reason as before: warming is linear in cumulative emissions, and emissions perturbations are small relative to total cumulative emissions.

Our conclusion from these useful experiments is that, for nearly all perturbations of interest, computing damages by multiplying marginal damages by tons emitted is a very close approximation to the computation of damages arrived at by fully simulating the exact pulse of interest. As we now describe in the text, we retain the exact formulation for all estimates reported in the paper but indicate that for nearly all perturbations of interest, marginal damages can also reliably be used as a basis for computing damages. We now write at the beginning of the L&D section of the results on line 209:

"We use our framework to quantify components of L&D from three types of emitters: individuals, companies, and countries. While emissions from large emitters might not be considered marginal, we show that multiplying per-ton estimates of HDCO2, FDCO2, and SCCO2 from above by total emissions is a very close (i.e., within a few percent) approximation to L&D estimates computed by pulsing total emissions through our framework (Fig ED8). This means that estimates of marginal per-ton damages can be used to accurately estimate L&D from emissions perturbations we don't consider here. Nevertheless, for precision, here we calculate all specific attributed L&D using the exact pulse of interest."

We also describe these findings in a new section in the re-written Methods:

"*Finally, emissions perturbations from large emitters (e.g. the US) are non-marginal: the impact of emissions changes in one year could meaningfully depend on emissions and damages from previous years, for instance because the damage from warming in one year is enough to affect the size of the economy that the next year's warming acts upon. In this case, estimating total or bilateral L&D damages by summing up marginal damages could misstate total damages by not accounting for this dependence. However, we show that errors are likely small: our benchmark approach of estimating per-ton marginal damage using a 1Gt pulse is a close approximation of per-ton damages from much smaller or much larger emissions pulses (Fig8a). The main reason* *is that global warming is linear in cumulative emissions, and global damages are locally linear in warming. Thus even large emissions perturbations are small relative to total global cumulative emissions, and the resulting modest warming they create generates roughly linear impacts on damages. Nevertheless, to precisely compute the impact of large, multi-year emissions perturbations (e.g. removal of decades of country emissions or carbon major emissions), we calculate damages by feeding the full multi-year emissions perturbation to FaIR rather than estimating marginal damages from pulses in different years and multiplying by tons emitted.*

A related concern is that the impact of one country reducing its emissions could depend on the impacts of others reducing theirs, and this would not be captured by our one-at-a-time perturbation approach. To assess this, we calculate what happens to our estimates of damages from US emissions if we assume that no other country changes their emissions at the same time (our baseline assumption) vs whether we assume that all countries simultaneously reduce their emissions by 10%, 30%, 50%, or 70%. We find that impacts of US emissions are slightly smaller when other countries also reduce their emissions (Fig8b), but that differences are again very modest - only a few percent relative to our baseline. Damages are smaller under joint emissions reductions relative to unilateral emissions reductions because US emissions are acting on a world that is slightly cooler than it otherwise would have been. However, this effect is again small for the same reason as before: the rough linearity in the link between emissions and warming, the relatively small impact on global warming of even a large perturbation, and the locally linear relationship between modest warming and damages."

3. In the estimation of future damages, several factors compound to produce large damage estimates.

a. First and foremost, the assumption of direct temperature effects on the growth rather than the level of income likely drives most of the cumulative effects over the 80-year horizon. While the estimated relationships underlying this seem sound (and I appreciate the discussion in the methods and supplemental materials), they play such a crucial role that it seems worthwhile to explore the sensitivity of the FD-CO2 to this fundamental assumption. How much of the effects are driven by the downward sloping branches of the growth-temperature curve as opposed to mere level effects of T on y (i.e., "no growth effects at 2020" scenario)? Currently the compounding effects of exponential discounting and those of exponential growth are subsumed in one number for the main estimates. Figure ED8 shows that changing assumptions about growth and discounting can both have massive effects, especially in cases where one is not keeping the other one in check. Comparing estimates in rows 1 and 5 of that figure strongly suggests that seemingly small differences in the temperature-growth rate relationship can approximately quintuple the SC-CO2 estimates.

The referee is correct that the magnitude of our estimates is indeed a function of our finding that temperature affects the growth rate rather than the level of income (GDP). To further bolster this component of the analysis, we have now extensively revisited the question of whether temperature affects the growth rate of GDP, and revised our manuscript and main estimates substantially as a result.

We conduct two tests for growth effects. First, following the pioneering work of Dell, Jones and Olken (2012), we test for growth effects by estimating distributed lag models with increasing numbers of lags, and asking whether the sum of the marginal effects is different than zero. Relative to our previous draft, we now extend our distributed lag model out to 10 years of lags rather than 5 years, finding that estimates at 10 years look very much like estimates at 5 years, albeit with somewhat wider confidence intervals (perhaps expected since we are now summing twice as many coefficients). This is shown in a new ED Fig 3.

Second, following recent macro literature, we now estimate dynamic impulse-response functions using the local projections method of Jorda (2005) and a large macro literature that followed (e.g. Nath et al, 2023). This method estimates the growth in income between years t and t+j as a function of a one-year temperature shock in year t, re-estimating this effect for larger and larger values of j. For a one year shock to have growth effects, it must be that estimated values do not return to zero at larger values of j. We run these impulse-response functions out for 15 years after an initial shock. These are now shown in ED Fig 3e-f. As we describe in the Methods, results are very consistent with the distributed lag models in suggesting that temperature shocks have growth effects:

"Results are consistent with the distributed lag models. In cold countries, a hot year initially boosts output, but that impact turns negative (but not statistically significant) after a few years. In countries with temperatures above approximately 15˚C, a hot year reduces output immediately and that effect persists for at least the next 15 years, although confidence intervals are somewhat wider as time progresses. We obtain very similar point estimates if we first residualize temperature shocks in year t on temperature shocks in the three previous years and then re-estimate the impulse response functions."

We conduct additional robustness on these estimates in a new section in Supplemental Methods (see "Robustness of local projections estimates), finding our conclusions are robust to these alternate approaches for estimating growth impulse response functions. We conclude that the data provide strong evidence that temperature changes have a persistent effect on output, consistent with growth effects. As a result of these exercises, and because both this referee and another rightly pushed us to understand and incorporate growth effects if they existed, we now use these "longer run" models as our preferred models – a choice that the growing literature on this subject agrees with (e.g. Nath et al. 2023). In particular, for our main estimates we now use the 5-lag distributed lag model instead of the zero-lag model. Our motivation for this choice is provided in a new Fig ED4 and discussed in the Methods:

"Given strong evidence of growth effects from both of these approaches, we choose as our baseline temperature-output damage function the model with the number of lags at which damages appear to have stabilized - i.e. the point at which adding additional lags does not change the marginal effect of temperature on growth. To study this stabilization, we compute GDP-weighted marginal effects for each lagged model (i.e. for the distributed lag model with one lag, with two lags, etc., and for the local projections model after one year, two years, etc.).

GDP-weighted marginal effects are the average of the estimated marginal effects at each country's average temperature weighted by that country's GDP. After computing weighted marginal effects for each model, we then fit a breakpoint algorithm (Muggeo 2003) to formalize if and where estimates stabilize.

Estimates are shown in Fig ED4. We find that marginal effects stabilize after about three lags in the local projections models, and after about seven lags in the distributed lag models. The two approaches give similar estimates at 5 lags, and so we choose the 5-lag model as our baseline model and test robustness of our main estimates against other lag choices (e.g. zero lag). Including lags in the response function, as both the data and earlier work (Dell et al 2012) suggest is appropriate, has strong implications for our estimates: positive marginal effects in the zero-lag model at the cold end of the temperature distribution become less positive and are no longer statistically significant as more lags are added. This suggests that, with additional warming, we should be much more confident in negative growth effects in hotter regions than we should be in positive growth effects in cooler regions. For nearly all countries on the planet, point estimates on cumulative marginal effects are negative at lag lengths where marginal effects have stabilized."

The referee is also correct that the results are sensitive to various other analytic choices, including established sources of uncertainty like the discount rate as well as uncertainty in both regression estimates and climate model projections. Our findings are not unique in that regard. For instance, recent benchmark estimates of the temperature-mortality partial SCC (Carleton et al QJE 2022) differ by 2-3x depending on the choice of discount rate and differ by 7-10x depending on analytic choices about how to account for risk aversion, and either regression or climate uncertainty alone are enough to change the *sign* of the reported estimates.

Based on this comment and related comments by the other referees, we now account for both regression uncertainty and climate uncertainty in all of our main estimates and report this uncertainty in the abstract and main text. Unlike the Carleton et al paper, and unlike previous temperature-growth estimates reported in Burke et al 2015, our confidence intervals rarely contain zero – a result of substantially less econometric uncertainty than was present in earlier studies. Nevertheless, for many estimates of interest, we cannot rule out substantially larger or smaller estimates than our primary point estimates (i.e., uncertainty remains substantial), and we now report all of this uncertainty in the abstract as well as describe it in the main text and in new figures. Figure ED6 shows uncertainty in our main estimates for HD-CO2 and FD-CO2 from Figure 2, and Figure ED13 now shows uncertainty in our main bilateral loss and damage estimates reported in Fig 4.

b. Another factor is that the FD period considers a longer time horizon than HD which mechanically leads to larger present values. I suppose this is obvious to the authors, but I didn't see it mentioned in the manuscript.

Yes good point - on lines 170 when we discuss FD vs HD, we have now added the following statement to the text:

"*The differences in magnitude are a result of the longer time period over which future damages can occur and the fact that future warming will be acting on a warmer world*."

c. A crucial assumption is the stability of the growth-temperature relationship estimated on data from 1960-2020. This assumption maintains that countries will not be able (or not willing) to dampen the negative income effects as mean temperatures rise. I find this very questionable in light of the authors' estimates highlighted in the abstract: $4 for HD-CO2 and $327 for FD-CO2. Those numbers imply that virtually all the economic returns on adaptation will accrue after 2020 and provide economic justification for considerable adaptation investments in order to offset the negative growth impacts during the 2020-2100 period. The authors address this possibility to some extent by considering "no effects after …" scenarios, but I think this point merits a deeper discussion of why we should think that adaptation will play no role throughout the remainder of this century and beyond. While statistically sound, the finding that there was no adaptation between 1960-2020 period, when climate damages where minimal compared to the study period, has very limited predictive power for a world in which climate change lowers the levels and growth rates of national income in many countries and, importantly, mankind knows much more about the phenomenon of climate change, its vulnerability to it, and the value of adapting to it.

Assuming no adaptation leads to higher L&D estimates in this framework, which adds to the likely overestimation of total damages discussed in comment 2). The authors make it clear that they cannot econometrically estimate an adaptation term, but some additional scenario analysis, particularly for the 2020-2100 period, could shed light on the issue.

This is a reasonable point: just because economies do not appear to have adapted in the past, this doesn't mean they won't adapt in the future – although we note that the roughly 1.3C of warming we have experienced to date is nearly half of the expected warming by end of century under our benchmark SSP3-7.0 scenario (and more than half under more aggressive mitigation scenarios), and so the world has already had substantial experience with climate change and a corresponding opportunity to adapt.

Nevertheless, to explore this possibility of future adaptation and its impact on our estimates, we now implement two adaptation scenarios. In both, we successively flatten the temperature-GDP response curve between 2020 and 2100, such that the effect of warming is half as large by 2060 (relative to today) and eliminated by 2100.

We then re-calculate the SCC – the quantity that is perhaps easiest to compare across scenarios – assuming either that there are no impacts at all after 2100, or that there are no growth impacts after 2100 but that the residual wedge in income levels that accumulated between 2020 and 2100 remains after 2100 (as depicted in Fig ED9). Under these two scenarios, we estimate an SCC that is either about 1/3rd lower (for damages ending in 2100) or about 2/3rds lower (for damages ending in 2300) than corresponding estimates that do not assume a flattening response function in the 21st century. These estimates suggest that, as expected, unprecedented adaptation has the ability to substantially reduce future damages, although they of course cannot reduce past damages.

These new estimates appear in an updated Fig ED9, and we have added the following text to the main manuscript to describe the possibility of adaptation, the scenarios we run, and the impact of these scenarios on future impacts.

"Another key assumption, for the calculation of the SC-CO2 as well other future damages, is the assumed rate of future adaptation. Our historical damage function does not assume no historical adaptation but rather estimates damages net of all adaptations that have occurred, and our baseline estimates of future damages assume that future adaptation will resemble past adaptation; this assumption, at least in the near term, is consistent with historical evidence that the damage function has not flattened over time (Fig ED2). However, it is of course possible that future adaptation could exceed current adaptation. To model this possibility, we estimate additional scenarios in which the historical damage function is successively flattened each year until it is perfectly flat (i.e. marginal damages equal zero at all temperatures) in the year 2100. For scenarios where damages end in 2100, such adaptation reduces the SCC by one-third; for scenarios where damages cumulate through 2300, adaptation reduces the SCC by two-thirds (Fig ED9). These results indicate that unprecedented adaptation in the future could substantially reduce future damages, although a full accounting would also require estimates of adaptation costs, which are unlikely to be zero."

We have also added additional discussion text on this point:

"Investments in adaptation will also be critical for limiting future damages. We show that under one scenario in which future adaptation slowly eliminates marginal damages by end-of-century, cumulative damages could be reduced by more than half -- although this estimate does not account for the costs of achieving such adaptation. Furthermore, there exists scant quantitative evidence that identifies specific investments that are able to reduce risk and limit damages at scale (Berrang et al 2021), and limited evidence that adaptation has been happening in the aggregate in recent decades (Fig ED2, Fig ED3d), despite substantial global income growth and the world having already experienced, by 2023, nearly half of the total warming expected under some scenarios by 2100. Furthermore, in a system where loss and damage payments are calibrated to damages net of adaptation, as we propose, countries could perhaps find it optimal to under-invest in adaptation if doing so entitles them to higher L&D payments. Designing an incentive-compatible system of L&D payments will be a key practical challenge in addressing loss and damage that has yet to occur, and such design is another important area for further work."

4. The possibility to adapt creates a moral hazard problem because L&D is defined as "the net present value of economic and non-economic impacts attributable to the emissions of greenhouse gases through their impact on the climate, net of any adaptation that was undertaken" (p.3). That is, countries could find it optimal not to invest in adaptation if doing so entitles them to higher L&D payments. I think this aspect deserves to be at least mentioned and possibly elaborated upon. If the authors' proposed scheme were to be used in practice, then adaptation would almost certainly draw a lot of attention due to its strong impact on the scale of payments.

This is an interesting and important point that we hope will spur future research by both theorists and practitioners! We have now made the referee's point almost verbatim in the Discussion, and thank them very much for the suggestion:

"*Furthermore, in a system where loss and damage payments are calibrated to damages net of adaptation, as we propose, countries could perhaps find it optimal to under-invest in adaptation if doing so entitles them to higher L&D payments. Designing an incentive-compatible system of L&D payments will be a key practical challenge in addressing loss and damage that has yet to occur, and such design is another important area for further work*."

Minor points:

5. Equation 3 and following: The D function should reflect that damages also depend on the stock of emissions.

Thanks. Instead of adding notation we have explained this in words in the revised Methods section.

6. Figure 1d: To explain the effect of different discount rates, the figure should plot values for two alternative discount rates, not the same discount rate for different discounting horizons.

We have adjusted that panel to show the different discount rates on the same panel.

Typos
- L. 249: calculation "of" bilateral
- L. 276: Figure 1d
- L. 567: where there exist

Thanks! Fixed.

<u>Referee #2 (Remarks to the Author):</u>

The article develops a framework for quantifying loss and damage from climate change, including the ability to identify monetary losses, and to link them to specific countries, companies, and people or behaviours (e.g. long haul flights). This is welcome at a time when countries have agreed under the United Nations Framework Convention on Climate Change (UNFCCC) to establish a Loss and Damage fund, but with limited specificity about how the fund might work. Some parts of the framework are thought provoking, for example the use of discount rates with historical damages. The article is generally clearly written.

In general I find that the article – for example in the title and abstract – overstates the comprehensiveness of its framework. The framework exclusively deals with losses (or gains) that can be quantified based on the relationship between annual mean temperature

and GDP. As the authors note, it does not include losses from sea level rise, for example. It also does not deal with losses associated with those extreme weather events which are not related to annual mean temperature (which is most extreme weather events). I think that if the authors wish to propose something that will satisfy loss and damage negotiators and advocates, this is a big omission. The Loss and Damage negotiations originated in debates over how to address sea level rise. And recent debates have focuses on extreme weather events that do not relate to annual mean temperature. For example specific events that have featured prominently in Loss and Damage discussions are Typhoon Haiyan (prior to the establishment of the Warsaw International Mechanism), and the flooding in Pakistan last year (prior to the establishment of the Loss and Damage fund). It should also be noted that loss and damage frameworks (including the Warsaw International Mechanism) include a focus on the most vulnerable, and studying losses in terms of GDP arguably does not represent the diversity of impacts within a country.
Given the interdisciplinary nature of the topic I also think that the text as currently written has a little too much assumed knowledge about existing research (for example the social cost of carbon, and extreme event attribution).

I would therefore suggest the following major revisions:

1. Revise text to make clear from the start, and in the abstract, that the findings only relate to losses associated with mean temperature effects on GDP. In some cases this is a case of adding more text, and in others a case of removing it. For example line 75 refers to non-economic impacts. However, it seems to me that the framework deals with a very narrow set of economic impacts. I don't see non-economic impacts reflected in the framework. Early on in the article I also got the impression that the framework would deal explicitly with adaptation and I didn't really see this reflected clearly later on.

Thanks - this is a very reasonable comment and we fully agree that our empirical estimates represent only one component of overall damages and that this should be crystal clear to the reader. As detailed below, we have added and revised text in the abstract, main text, and discussion to reflect this important point. We also attempt to more clearly distinguish between our proposed definition and framework for estimating loss and damage (which, following IPCC discussions, we believe *should* incorporate non-economic impacts) and our empirical implementation of our framework – which, like all other comprehensive damage estimates we are aware of, are knowingly partial, in the sense that they do not capture components of damage that are expected to be important but are hard to currently quantify.

In the abstract, we now write:

"Here we develop such a framework, linking it explicitly to recent efforts to calculate the social cost of carbon dioxide (SC-CO2), and demonstrate its use in a variety of applications. Using empirical estimates of the relationship between temperature and aggregate economic output, one component of climate damages, we calculate that future damages from past emissions, one component of L&D, are at least an order of magnitude larger than historical damages from the same emissions, a more commonly discussed component of L&D…"

In the main text, just prior to the Results section, we now write:

"*We emphasize that our focus is also on damages to GDP from warming, which is a subset of known and anticipated damages from climate change, and will not account for a range of economic and non-economic damages that could result from a warming climate. Our estimates, much like existing benchmark estimates of SC-CO2, are thus ``partial''.*"

In the discussion, we now write:

"*Our quantitative estimates capture an important aggregate channel -- GDP -- through which climate damages have occurred, and likely subsume a large class of microeconomic channels through which warming can have negative economic impacts, including changes in agricultural productivity, labor supply and labor productivity, and energy use. Other channels that are poorly captured in GDP data (e.g. heat related mortality (Carleton et al 2022) or various non-economic impacts like ecosystem function (Bastien and Moore 2021)) or that are not highly correlated with interannual variation in country level temperatures (e.g. sea level rise (Depsky et al 2022)) will not be reflected in our current estimates. Similarly, our results do not fully capture the impact on GDP of variation in temperature and other climatic extremes, as these are imperfectly correlated with annual mean temperature and exert independent influence on damages (e.g. Callahan and Mankin 2021, Kotz et al 2022). To the extent that these channels are quantitatively important, our damage estimates will understate the total damages associated with historical emissions. Similarly, our approach does not account for the impact of pollutants that are co-emitted with CO2; these pollutants (e.g. particulates) tend to be less well-mixed than GHGs and thus their damages depend on the location of emission (Burney et al 2022). However, our basic approach could be amended to include these other channels, so long as there is a clear mapping between changes in global mean surface temperature and location-specific changes in the climate variable of interest, and a credible way of linking these changes to damages*."

2. Extend and deepen discussion of whether / how the framework might be applied beyond annual mean temperature and GDP.

We have added additional text to the Discussion making this point clear (we have italicized the additions relevant to this specific point):

"*Our quantitative estimates capture an important aggregate channel -- GDP -- through which climate damages have occurred, and likely subsume a large class of microeconomic channels through which warming can have negative economic impacts, including changes in agricultural productivity, labor supply and labor productivity, and energy use. Other channels that are poorly captured in GDP data (e.g. heat related mortality (Carleton et al 2022) or various non-economic impacts like ecosystem function (Bastien and Moore 2021)) or that are not highly correlated with interannual variation in country level temperatures (e.g. sea level rise (Depsky et al 2022)) will not be reflected in our current estimates. Similarly, our results do not fully capture the impact on GDP of variation in temperature and other climatic extremes, as these are imperfectly correlated with annual mean temperature and exert independent influence on damages (e.g. Callahan and Mankin 2021, Kotz et al 2022). To the extent that these channels are* *quantitatively important, our damage estimates will understate the total damages associated with historical emissions. Similarly, our approach does not account for the impact of pollutants that are co-emitted with CO2; these pollutants (e.g. particulates) tend to be less well-mixed than GHGs and thus their damages depend on the location of emission (Burney et al 2022). However, our basic approach could be amended to include these other channels, so long as there is a clear mapping between changes in global mean surface temperature and location-specific changes in the climate variable of interest, and a credible way of linking these changes to damages.*"

3. Revise text to make the concepts clearer for the reader. In particular – what is the social cost of carbon? How is it currently calculated and how would it be done differently using this framework?

We now define the social cost of carbon on line 64 using its widely accepted definition: "the net-present value of total additional net harm (or benefit) that accrues to society as a result of one additional unit of CO2 emissions", and cite up front a National Academy Report (NAS 2017) and an EPA report (EPA 2022) that describe at great length the SCC and its calculation. We then clarify that our goal is to design a framework for estimating the various components of L&D that mimic the now well-developed framework for estimating the SCC. Our framework nests the typical value of the SCC – the discounted cumulative future damage from a ton of CO2 emitted today – as a special case.

The amended paragraph in the Introduction now reads:

"This approach enables decomposition of L&D into past and future damages, and aligns the financial accounting framework of L&D with the existing framework for the ``Social Cost of Carbon Dioxide'' (SC-CO2) (NAS 2017, EPA 2022), which is the net-present value of total additional net harm (or benefit) that accrues to society as a result of one additional unit of CO2 emissions (equivalent concepts exist for other greenhouse gasses; our focus here will be on CO2). Our framework nests the typical value of the SC-CO2 -- the discounted cumulative future damage from a ton of CO2 emitted today -- as a special case."

We then also compute our own estimate of the SCC. By design it uses the same basic components that others use – a climate model, a set of socioeconomic scenarios, a damage function, a discounting module – but we use a different damage function than what is used in some other estimates. We now make this clear in the Results section on the social cost of carbon:

"*Given its policy importance, we estimate and report SC-CO2 values under a broad range of analytic choices, including different discounting schemes, assumed counterfactual future growth rates (the rate at which economies would grow absent warming), whether growth effects or damages occur past 2100, the rate of adaptation, and which statistical model is used for a damage function. See Figure 1 for a schematic depiction of how damages are computed under alternate time horizons. By design, our approach uses very similar climate, socioeconomic scenario, and discounting components as used in recent estimates (EPA 2022), but uses our ``top-down'' GDP-temperature damage function instead of additive ``bottom-up'' damage functions that combine sector-specific damages from mortality, agriculture, energy, and labor*."

4. Integrate uncertainty into the reported findings. The study deals with uncertainty, but it comes quite late on in the article. In many places including abstract and section 3.2 one number is quoted for each estimate of loss. This seems misleading given the huge uncertainty ranges associated with these estimates. I suggest the authors give a range, to make clear that the reader that we cannot know these numbers precisely. This is a key issue that anyone wishing to implement a compensation mechanism would need to deal with.

This is a good point, and related to points made by other referees. We have now computed and reported uncertainty on all of our main estimates, including in the abstract and in revised figures. New Fig 2c now reports whether country-level damages from a marginal emission are statistically significant, and Fig 2e reports uncertainty on SCC estimates, new Fig ED 6 reports uncertainty on estimates of HD-CO2 and FD-CO2 for different emissions years, and new Fig ED13 reports uncertainty on historical damages from country-level emissions for large emitters, and for damages to "receiving" countries from the US. Relevant confidence intervals around key estimates are then reported in the abstract and main text. If this paper is found suitable for publication, we also plan to release a database where all the bilateral country-level L&D estimates can be looked up along with their uncertainty.

5. Add more references into the rationale to make clearer that there is already early work to try to quantify losses from climate change from the extreme event attribution perspective. Lines 35-36 imply that very little has been done in this space, which could be a bit misleading.

Thanks. That sentence has now been rewritten with multiple relevant citations added:

"However, unlike the large body of empirical work which explores how global climate change will affect global and local economic outcomes, the question of whether and how emissions by specific entities (e.g. countries, companies, or individuals) can be linked to specific damages has received less formal and empirical attention -- although substantial headway has been made in understanding how the probability of specific extreme events can be linked to a changing climate (Diffenbaugh et al 2017, Bouwer 2019, Frame et al 2020, Seneviratne et al 2021 IPCC, Strauss et al 2021)."

6. Make clear that the UNFCCC decision text from the Paris Agreement explicitly notes that L&D in the Paris Agreement does not refer to liability and compensation. This makes discussion of this article, in the terms it is currently written, quite difficult in a policy context.

Thanks for this important point. We now emphasize in both the Introduction and the Discussion that we are estimating the size of damages caused in one location by emissions in another, which is not necessarily equivalent to the liability of the emitting entity or the compensation owed to the receiving country. For instance, in the introduction we now say right up front:

"*Stated another way, L&D is a payment schedule that would completely reimburse all individuals for the damages (or benefits) that they have experienced or will experience from climate change, paid for by the individuals that caused these impacts via emissions -- although we emphasize, consistent with Article 8 of the Paris Agreement, that these damage estimates do not necessarily equal what is ``owed" by one entity to another, as that is a moral and legal question beyond the scope of this analysis*."

In the first paragraph of Discussion we now say:

"*Our resulting estimates of L&D caused by emitting entities to receiving entities do not necessarily equal what is ``owed" by the former to the latter, as that is a moral and legal question.*"

And have a full paragraph a few lines below (lines 363 and following) that begins: "*Our framework cannot resolve difficult legal, ethical, and empirical questions related to the extent to which an emitting entity ``owes" another entity for damages caused by emissions related to its activities*."

Minor comments

27-30 – suggest to provide a citation, since this is just one view of loss and damage.

We have added 4 citations to IPCC documents and scientific literature.

30-32 – Suggest to provide a citation for litigation focusing on loss and damage. My understanding is that most legal cases about climate change do not currently focus on loss and damage.

Citation added, thanks.

57-59 – what is meant by compensation and debt "in an accounting sense"? This should be explained further, as these terms are very loaded in the context of the negotiations. It seems a bit inconsistent to use wording such as "compensation owed by an emitter" and then to state the paper will not address "the ethical question of actual legal obligation". Suggest that the authors explain further their distinction, or consider using other terminology. For example some other literature has used the term "contribution" to refer to the inputs of specific emitters.

We have re-worked the language throughout to separate our estimating of damages from specific emissions from discussion of compensation, as we agree that these are separate concepts and in this paper we measure the former.

69 – "including that L&D is indistinguishable from adaptation efforts and costs," – does this refer to ref 4? If so, I don't think it is quite right, as the interviews with experts did not suggest adaptation was indistinguishable from L&D, but rather that some view the role of

the UNFCCC as a whole as avoiding L&D, through mitigation and adaptation. Other viewpoints suggest overlap between actions that might be considered adaptation and those that are designed to deal with L&D. This doesn't mean that adaptation and L&D are indistinguishable, but rather that they might overlap.

Thanks. This was poor wording on our part. We now just say, citing the Boyd article, that there have been multiple interpretations of L&D in practice.

77-78 – what does it mean to make individuals "whole"? This seems important to explain for an interdisciplinary audience.

This was jargon that we have now eliminated. We have reworded that sentence to now read:

"Equivalently, L&D is the payment schedule that would be required to completely reimburse all individuals for the damages (or benefits) that they have experienced or will experience from climate change, paid for by the individuals that caused these impacts via emissions."

78 – "benefits" – how would these be dealt with? Would the individuals need to pay emitters?

In principle, yes. In practice, it seems unlikely. We don't have a good proposal here, so now just state the challenge explicitly in the revised Discussion:

"*Similarly, we do not propose how legal or political systems should address the fact that a given emission generates benefits for some parties and damages for others, and that the net effect of these impacts can be uncertain in sign and magnitude even when the location-specific impacts are not. This uncertainty is particularly salient for historical damages, in which a cooler world saw more benefits*. "

154 – I wouldn't necessarily agree that mean temperature is always well measured. There are still large gaps in gridded datasets over central Africa for example.

Fair enough - we now say "relatively well measured in most of the world" (note this section now moved to methods).

170 – CO2s are? – Suggest rewording, perhaps "CO2 is well mixed"?

Fixed, thanks

213 – expected is repeated twice

Fixed, thanks

317 – GDP acronym undefined

Now defined earlier in the Introduction - thanks.

561-562 - "To the extent that these channels are quantitatively important, our damage estimates will understate the total damages associated with historical emissions." – as a researcher focusing on climate change impacts this feels like a rather large understatement, given that we are often hearing mean climate is not something people feel the impact of, and extreme weather events have much more impact.

We have now added text at multiple points in the main manuscript to emphasize that our impact estimates are "partial" in the sense of only capturing one – albeit quite large – channel through which climate change could impact society, and how the channel we study might or might not capture other channels of interest. The revised Discussion paragraph now reads:

"Our quantitative estimates capture an important aggregate channel -- GDP -- through which climate damages have occurred, and likely subsume a large class of microeconomic channels through which warming can have negative economic impacts, including changes in agricultural productivity, labor supply and labor productivity, and energy use. Other channels that are poorly captured in GDP data (e.g. heat related mortality (Carleton et al 2022) or various non-economic impacts like ecosystem function (Bastien and Moore 2021)) or that are not highly correlated with interannual variation in country level temperatures (e.g. sea level rise (Depsky et al 2022)) will not be reflected in our current estimates. Similarly, our results do not fully capture the impact on GDP of variation in temperature and other climatic extremes, as these are imperfectly correlated with annual mean temperature and exert independent influence on damages (e.g. Callahan and Mankin 2021, Kotz et al 2022). To the extent that these channels are quantitatively important, our damage estimates will understate the total damages associated with historical emissions. Similarly, our approach does not account for the impact of pollutants that are co-emitted with CO2; these pollutants (e.g. particulates) tend to be less well-mixed than GHGs and thus their damages depend on the location of emission (Burney et al 2022). However, our basic approach could be amended to include these other channels, so long as there is a clear mapping between changes in global mean surface temperature and location-specific changes in the climate variable of interest, and a credible way of linking these changes to damages."

Figure 1 – what is meant by the different orange colours?

The different colors were meant to denote time marching forward, but are not critical. We have now fixed the orange to be constant in the figure.

Fig 2e – what are uncertainty ranges based on?

They are based on propagating both climate uncertainty (which includes both uncertainty from FaIR as well as pattern-scaling uncertainty from the GCMs) as well as "econometric uncertainty", or the statistical uncertainty in the estimated relationship between warming and growth in historical data. This is now noted in the caption, with reference to Fig ED10 which has more detail on the relative importance of these different sources of uncertainty.

This is also described in the Methods on lines 887 and following:

"*For a set of analytic choices regarding discounting, time horizon of impacts, counterfactual growth rates, and the statistical model used for the damage function, we can calculate uncertainty in the estimated SC-CO2 resulting from quantifiable uncertainties in our empirical pipeline. We find that the two largest sources of uncertainty are uncertainty in the historical relationship between temperature and growth (as estimated by our regression analysis), and uncertainty in the sensitivity of GMST to a marginal emission (as estimated by FaIR; Fig ED10). Less important is uncertainty across the climate model ensemble in the mapping of GMST to local warming. We do not consider uncertainty in emissions inventories*."

Figure ED2 – what is growth here? GDP? In general this figure could use more description.

Thanks. We have added additional detail to the caption, including defining growth.

Figure ED3 – needs more explanation. "Panels show estimated marginal effects from global pooled regression" – effects of what on what?

Thanks. We have added additional detail to the caption, including defining the regression and what we mean by marginal effects.

Figure ED4a – needs more explanation. What is GDP weighted temperature? What is the unit?

Thanks. We have now defined GDP-weighted temperature in the caption: "temperature averaged across all countries in a year, weighted by each country's GDP in that year"

BHM – undefined acronym

Now defined, thanks.

DJO – undefined acronym

Now defined, thanks.

What is the difference between figure 4 and Figure ED12?

As described in the caption, Fig 4 shows damages since 1990 and ED14 shows damages since 1980.

<u>Referee #3 (Remarks to the Author):</u>

This is a very interesting paper on an important topic. The authors formalize the clear link between established approaches to assess the Social Cost of Carbon and the topical question of loss and damage quantification (a somewhat similar approach has been suggested e.g. by (1)). They further discuss strengths of their approach as well as remaining

challenges. To demonstrate the usefulness of their approach they quantify past and future damages caused by major emitting countries, companies and individuals. I especially like that they also quantify the benefits of more sustainable life choices in terms of damages avoided and that they explore the potential of carbon dioxide removal in limiting future damages.

While I really like the general approach, the specific numbers the authors present depend on a number of modelling choices and assumptions and not all of them are satisfactorily justified and discussed:
1) Assumption of growth effects. As the authors themselves note several times, the magnitude of their damages crucially hinges on their assumption that a permanent temperature increase leads to a permanently reduced growth rate. The question of growth-vs-level effects is a much-debated question in the literature as it has such a strong influence on the magnitude of impacts, especially when assessed over long time horizons, compare e.g. (2, 3). The authors test this assumption using distributed lag models with up to five lags (and it is interesting that the results are less noisy than in Burke et al., 2015!) but theoretically economies could still recover after these five years (4, 5). Given that the authors have such a long panel, is it possible to use even more lags to get more clarity here?

This is an important point and was also raised by other referees. To further bolster this component of the analysis, we have now extensively revisited the question of whether temperature affects the growth rate of GDP, and revised our manuscript and main estimates substantially as a result.

We conduct two tests for growth effects. First, following the pioneering work of Dell, Jones and Olken (2012), we test for growth effects by estimating distributed lag models with increasing numbers of lags, and asking whether the sum of the marginal effects is different than zero. Relative to our previous draft, we now extend our distributed lag model out to 10 years of lags rather than 5 years. We find that estimates at 10 years look very much like estimates at 5 years, albeit with somewhat wider confidence intervals (perhaps expected since we are now summing twice as many coefficients). This is shown in a new ED Fig 3a-c.

Second, following recent macro literature, we now estimate dynamic impulse-response functions using the local projections method of Jorda (2005). This method estimates the growth in income between years t and t+j as a function of a one-year temperature shock in year t, re-estimating this effect for larger and larger values of j. For a one year shock to have growth effects, it must be that estimated values do not return to zero at larger values of j. We run these impulse-response functions out for 15 years after an initial shock. These are now shown in ED Fig 3e-h. As we describe in Methods, results are very consistent with the distributed lag models in suggesting that temperature shocks have growth effects:

"Results are consistent with the distributed lag models. In cold countries, a hot year initially boosts output, but that impact turns negative (but not statistically significant) after a few years. In countries with temperatures above about 15C, a hot year reduces output immediately and that effect persists for at least the next 15 years, although confidence intervals are somewhat wider as time progresses. We obtain very similar point estimates if we first residualize temperature shocks

in year t on temperature shocks in the three previous years and then re-estimate the impulse response functions."

We conclude that the data provide strong evidence that temperature changes have a persistent effect on output, consistent with growth effects. As a result of these exercises, and because both this referee and R1 rightly pushed us to understand and incorporate growth effects if they existed, we now use these "longer run" models as our preferred models. In particular, for our main estimates we now use the 5-lag distributed lag model instead of the zero-lag model. Our motivation for this choice is provided in a new Fig ED4 and discussed in the Methods:

"Given strong evidence of growth effects from both of these approaches, we choose as our baseline temperature-output damage function the model with the number of lags at which damages appear to have stabilized - i.e. the point at which adding additional lags does not change the marginal effect of temperature on growth. To study this stabilization, we compute GDP-weighted marginal effects for each lagged model (i.e. for the distributed lag model with one lag, with two lags, etc and for the local projections model after one year, two years etc). GDP-weighted marginal effects are the average of the estimated marginal effects at each country's average temperature weighted by that country's GDP. After computing weighted marginal effects for each model, we then fit a breakpoint algorithm (Muggeo 2003) to formalize if and where estimates stabilize.

Estimates are shown in Fig ED4. We find that marginal effects stabilize after about three lags in the local projections models, and after about seven lags in the distributed lag models. The two approaches give similar estimates at 5 lags, and so we choose the 5-lag model as our baseline model, testing robustness of our main estimates against other lag choices (e.g. zero lag). Including lags in the response function, as both the data and earlier work (Dell et al 2012) suggest is appropriate, has strong implications for our estimates: positive marginal effects in the zero-lag model at the cold end of the temperature distribution get less positive and are no longer statistically significant as more lags are added. This suggests that, with additional warming, we should be much more confident in negative growth effects in hotter regions than we should be in positive growth effects in cooler regions. For nearly all countries on the planet, point estimates on cumulative marginal effects are negative at lag lengths where marginal effects have stabilized."

2) Further impact channels: the authors only account for market impacts of changes in annual mean temperature – a limitation they should acknowledge and discuss in more depth

a. Recent work shows that daily temperature variability, extremes, precipitation changes, tropical storms, fluvial floods etc. can bring further economic harms (and in some cases benefits for some regions), especially when impacts are assessed using subnational data, see e.g. (6–12).

b. The authors use national data arguing that studies based on subnational data find roughly consistent results. However, one important difference is that studies using subnational data typically find a lower optimum temperature (13, 14), implying that several countries e.g. in Europe are also harmed by temperature increases (whereas the

current study based on national data finds that they benefit, compare Fig. 2 c+d). This could change e.g. Fig. 4. While I acknowledge that there are trade-offs as subnational data are typically not available for many countries in the Global South, the authors should expand their discussion accordingly.

This is also a fair point and again echoed comments by other referees. We have revised our manuscript in two ways. First, to the point about lower optimum temperatures in subnational data, we now use the 5-lag model as our main model, which has a much lower optimum temperature than the 0-lag model reported in our original manuscript but is more consistent with the subnational data as mentioned by the referee. This indeed has substantial influence on our results, as anticipated by the referee, but we feel is most consistent with both our data as well as the new set of subnational papers the referee references (as described in our new text above).

Second, we have added and revised text in the abstract, main text, and discussion to make very prominent the important point that our estimates only account for one component of climate impacts – albeit (in our view) an important one. In the abstract, we now write:

"Here we develop such a framework, linking it explicitly to recent efforts to calculate the social cost of carbon dioxide (SC-CO2), and demonstrate its use in a variety of applications. Using empirical estimates of the relationship between temperature and aggregate economic output, one component of climate damages, we calculate that future damages from past emissions, one component of L&D, are at least an order of magnitude larger than historical damages from the same emissions, a more commonly discussed component of L&D…"

In the main text, just prior to the Results section, we now write:

"*We emphasize that our focus is also on damages to GDP from warming, which is a subset of known and anticipated damages from climate change, and will not account for a range of economic and non-economic damages that could result from a warming climate. Our estimates, much like existing benchmark estimates of SC-CO2, are thus ``partial''.*"

In the discussion, we now write:

"*Our quantitative estimates capture an important aggregate channel -- GDP -- through which climate damages have occurred, and likely subsume a large class of microeconomic channels through which warming can have negative economic impacts, including changes in agricultural productivity, labor supply and labor productivity, and energy use. Other channels that are poorly captured in GDP data (e.g. heat related mortality (Carleton et al 2022) or various non-economic impacts like ecosystem function (Bastien and Moore 2021)) or that are not highly correlated with interannual variation in country level temperatures (e.g. sea level rise (Depsky et al 2022)) will not be reflected in our current estimates. Similarly, our results do not fully capture the impact on GDP of variation in temperature and other climatic extremes, as these are imperfectly correlated with annual mean temperature and exert independent influence on damages (e.g. Callahan and Mankin 2021, Kotz et al 2022). To the extent that these channels are quantitatively important, our damage estimates will understate the total damages associated with historical emissions. Similarly, our approach does not account for the impact of pollutants that* *are co-emitted with CO2; these pollutants (e.g. particulates) tend to be less well-mixed than GHGs and thus their damages depend on the location of emission (Burney et al 2022). However, our basic approach could be amended to include these other channels, so long as there is a clear mapping between changes in global mean surface temperature and location-specific changes in the climate variable of interest, and a credible way of linking these changes to damages*."

3) Role of adaptation: The authors claim that their estimates are net of adaptation. In fact, their non-linear temperature specification allows them to account for some long-run historical adaptations (to prevailing climate conditions). However, the role of other potentially important moderators such as income is not explicitly assessed like that. If I understand it correctly, the authors' argument here is that they find similar temperature-economic growth response functions for different 20-y periods. As incomes have risen quite a bit since the 1960s this points to limited adaptation. It'll be helpful if the authors discuss this in a bit more detail, also acknowledging that - while historical adaptation may have been limited - unprecedented future warming may spur (or damages may impede) adaptation efforts

Thanks, and R1 raised a similar point: just because economies do not appear to have adapted in the past, this doesn't mean they won't adapt in the future – although we note that the roughly 1.3C of warming we have experienced to date is nearly half of the expected warming by end of century under our benchmark SSP3-7.0 scenario, and so the world has already had ample experience with climate change.

We have now made the changing-income point more explicit. On line 120 we say:

"The temperature-GDP damage function … has not changed appreciably in the last 60 years (Fig ED2a), *a period during which global average per capita incomes increased more than three-fold*, indicating limited adaptation to date."

Second, to explore this possibility of future adaptation and its impact on our estimates, we now implement two adaptation scenarios. In both, we successively flatten the temperature-GDP response curve between 2020 and 2100, such that the effect of warming is half as large by 2060 (relative to today) and eliminated by 2100.

We then re-calculate the SCC – the quantity that is perhaps easiest to compare across scenarios – assuming either that there are no impacts at all after 2100, or that there are no growth impacts after 2100 but that the residual wedge in income levels that accumulated between 2020 and 2100 remains after 2100 (as depicted in Fig ED9). Under these two scenarios, we estimate an SCC that is either 1/3rd lower (for damages ending in 2100) or 2/3rds lower (for damages ending in 2300) than corresponding estimates that do not assume a flattening response function in the 21st century. These estimates suggest that, as expected, unprecedented adaptation has the ability to substantially reduce future damages, although they of course cannot reduce past damages.

These new estimates appear in an updated Fig ED9, and we have added the following text to the main text to describe the possibility of adaptation, the scenarios we run, and the impact of these scenarios on future impacts.

"Another key assumption, for the calculation of the SC-CO2 as well other future damages, is the assumed rate of future adaptation. Our historical damage function does not assume no historical adaptation but rather estimates damages net of all adaptations that have occurred, and our baseline estimates of future damages assume that future adaptation will resemble past adaptation; this assumption, at least in the near term, is consistent with historical evidence that the damage function has not flattened over time (Fig ED2). However, it is of course possible that future adaptation could exceed current adaptation. To model this possibility, we estimate additional scenarios in which the historical damage function is successively flattened each year until it is perfectly flat (i.e. marginal damages equal zero at all temperatures) in the year 2100. For scenarios where damages end in 2100, such adaptation reduces the SCC by one-third; for scenarios where damages cumulate through 2300, adaptation reduces the SCC by two-thirds (Fig ED9). These results indicate that unprecedented adaptation in the future could substantially reduce future damages, although a full accounting would also require estimates of adaptation costs, which are unlikely to be zero."

We have also added additional discussion text on this point:

"Investments in adaptation will also be critical for limiting future damages. We show that under one scenario in which future adaptation slowly eliminates marginal damages by end-of-century, cumulative damages could be reduced by more than half -- although this estimate does not account for the costs of achieving such adaptation. Furthermore, there exists scant quantitative evidence that identifies specific investments that are able to reduce risk and limit damages at scale (Berrang et al 2021), and limited evidence that adaptation has been happening in the aggregate in recent decades (Fig ED2), despite substantial global income growth and the world having already experienced, by 2023, nearly half of the total warming expected under some scenarios by 2100. Furthermore, in a system where loss and damage payments are calibrated to damages net of adaptation, as we propose, countries could perhaps find it optimal to under-invest in adaptation if doing so entitles them to higher L&D payments. Designing an incentive-compatible system of L&D payments will be a key practical challenge in addressing loss and damage that has yet to occur, and such design is another important area for further work."

**** Specific questions/ need for clarification ** – in general it's a bit difficult to follow the methods as they are described in three different sections (Intro, Methods, SI)**

We have now substantially re-written and streamlined the methods. The main text has a general overview of the approach, but all the notation is now developed in one place in the Methods section. Thanks for this useful suggestion.

1) Eq. 2+ 15: Here, I am a bit confused but maybe this is just because of the use of the term counterfactual which does not seem appropriate with respect to past emissions and temperatures (given that it denotes something that has not happened but could have happened if ...): My understanding is (also following Extended Data Fig. 1 that is very

helpful - but I may be wrong here) that what the authors call counterfactual are actually *observed* emissions E and temperatures and the perturbed emissions E' and temperatures are those that we would have had if an emissions pulse hadn't occurred. Correct? But then just after eq. 2 they state that "delta Y is almost always positive" (probably they mean negative?), indicating damages and that income is higher in the counterfactual (do they mean perturbed?) setting with lower emissions which then doesn't fit to the above. A clarification using unambiguous language would be helpful.

The referee's interpretation is exactly right, and s/he is also right that the language as written was confusing. Regarding "counterfactual", we have now instead used the word "baseline" :

"*Damage at a specific moment in time (a flow) from the marginal emissions perturbation is then the difference between the income expected under the perturbed emissions relative to a ``baseline" emissions history, which for past years is just the observed emissions and for future years is emissions as projected by the socioeconomic scenarios described below*."

We have also clarified the language around the sign of damages. We now say:

"*However, as we will show, if perturbed emissions are higher than baseline emissions, the change in income is typically positive -- that is, in the exposed population, income is higher in the baseline setting with lower emissions -- and so we use "damage" as shorthand for the change in income.* "

2) Eq 11+14: Probably a typo but here the authors should divide by the total population in that country instead of by g. Also: do \lambda and \gamma both denote the population in a given grid cell? The authors should use consistent notation and ideally avoid using the same (Greek) letters for different things (e.g. \delta is simultaneously for denoting time differences, year fixed effects and the pure rate of time preference, r, simultaneously for the discount rate and the ratio of grid-to-global average warming, etc.)

Good catch - the weights were incorrectly written. In both cases we take the weighted average where the weights sum to one. We have also expanded our use of greek letters (nearly exhausting our knowledge of greek) so as to avoid using the same letter for different things.

3) Eqs. 22+24: \alpha and y^hat are mixed up/both used to denote the country-specific mean growth rates.

Good catch - corrected.

4) Why did the authors pick SSP3-7.0 as main emission trajectory and how does that influence their results?

Our method uses the CMIP6 global climate model simulations to quantify the spatial pattern of the "forced response" in order to translate the global temperature response calculated by FAIR for a given unit of emissions into country-level temperature responses. We follow the IPCC AR6 in using SSP3-7.0 to quantify the spatial pattern of forced response per unit of global warming

(IPCC AR6, WGI Chapter 4, FAQ 4.3 Figure 1). Traditionally, there are a couple of reasons that high forcing scenarios are used to calculate the spatial pattern of the forced response. First, using a higher forcing scenario enables a wider range of global warming to be sampled (as shown in FAQ 4.3 Figure 1 of the IPCC WGI AR6 report). In addition, as the forcing increases, the signal-to-noise ratio of the forced response relative to internal climate variability increases (e.g., Hawkins and Sutton, BAMS, 2009), enabling more robust quantification of the spatial pattern scaling per unit of global forcing and of the uncertainty in the spatial pattern of the forced response (which we show in Figure ED10). However, as the caption of FAQ 4.3 Figure 1 states, "Regional changes in temperature (left) and precipitation (right) are proportional to the level of global warming, irrespective of the scenario through which the level of global warming is reached."

We now explicitly describe this choice in the Methods section on line XX:

"We choose the relatively high-emissions SSP3-7.0 scenario to conduct this pattern scaling because using a higher forcing scenario enables a wider range of global warming to be sampled (as shown in FAQ 4.3 in IPCC WGI AR6), and because as the forcing increases, the signal-to-noise ratio of the forced response relative to internal climate variability increases (e.g., Hawkins and Sutton 2009), enabling more robust quantification of the spatial pattern scaling per unit of global forcing and of the uncertainty in the spatial pattern of the forced response."

5) Figs. 3+4: I guess an argument could be made that the discount rates for the damages that have occurred prior to settlement and those in the future are not necessarily the same (e.g. because the future is uncertain whereas past interest rates can be assessed)

Yes, we agree that someone could reasonably make that argument. Our goal is not to take a stand on what past or future discount rates should be, but compute damages under a range of rates such that others can make their own choices – including choosing past and future rates that differ.

We have now explicitly stated this in the Methods on line 584:

"*Estimates in Fig 2 allow analysts to compute past and future damages from past emissions under discount rates of their choice, including a choice that uses different rates for past and future damages*."

6) I understand why the authors assess damages till 2100 or 2300 (and I don't ask them to change that) but this is still an arbitrary cut-off implying that emissions emitted earlier generally cause larger damages simply because damages can accumulate over a longer time horizon. While the authors acknowledge and discuss this, I guess for the whole L&D debate it would make more sense to assess all damages caused within the subsequent x years

Agreed, these are convenient but arbitrary cutoffs. We now just acknowledge this explicitly in the Discussion section on line 361:

"*Finally, whether future damages should be cumulated through 2100 or 2300 or some other year* *is a somewhat arbitrary choice with a first-order effect on estimated damages, and a consensus choice by the research or policy community will be important in clarifying the scale of L&D..*"

**** Minor comments ****

1. Data: the authors only state that they're using ERA5 data and the reference says it is actually ERA5-Land (but the reference is not displayed correctly), so I'm assuming that the data are on a 0.1°-grid and have already been aggregated to the monthly level? This information should be added to the Data & Methods section.

Yes, thanks - we fixed the reference and added detail on the native 0.1deg resolution and the population-weighted aggregation.

2. Fig 1a: are these cumulative losses?

No, these are annual flows in panel a, as described in the caption.

3. ED Fig 2
a. Panel a: labels could be more specific/explained in caption (e.g. does 'linear TT' mean linear temperature term ?!)
b. Panel b: how big is the sample for the different periods?
c. It would be helpful if both panels had the same y-axis

Thanks - we have made these edits to the figure, including adding the period-specific sample sizes.

4. There are some typos/missing words, abbreviations not spelt out when used for the first time, and especially in the Supplemental Materials the language is very colloquial (precip instead of precipitation, etc.)

Thanks - we have revised the Supplemental to make sure everything is defined and the style is consistent with the rest of the paper. We have also removed duplicative text that was in SI and Methods.

Below we respond to each referee comment. Original referee comments are in bold, our responses are in plain text, and changes we made to the manuscript are in italics.

Referee #1 (Remarks to the Author):

I appreciate the changes the authors have made to the manuscript and extended materials in response to my comments and those of other referees. I think they have resulted in a more streamlined paper and more compelling presentation of the results.

Overall, I am satisfied with the authors' responses to the substantial points raised in my last report. The additional robustness analysis and changes in the text go a long way to clarify the modeling choices made and spell out their limitations.

Thanks. We appreciate the referee's kind words.

Regarding the estimated temperature-growth relationship, a central element of the analysis, I appreciate the additional work the authors have put into estimating distributed-lag models and local projections. As a result of the authors' changes to the econometric specification, the estimates of per-ton L&D have changed by orders of magnitude. For example, HD-CO2 has increased from an intial $4 to $180 in the new version, similarly FD-CO2 from $327 to $2,000 per tonne. Hence these estimates seem to be highly sensitive to the estimated temperature-growth relationship. As the authors do not fail to mention, this relationship continues to be heavily debated in the literature. For example, the recent paper by Nath, Ramey and Klenow (2023) argues that estimates of warming on GDP based on the methodology in Burke, Hsiang and Miguel (2015) tend to be at the upper end of the range of estimates in the literature. Estimates by Nath et al. are one order of magnitude lower than BHM. While this particular paper has yet to undergo peer review, it highlights once again that future climate damages projected over long time horizons critically depend on whether or not temperature changes have permanent effects on economic growth.

Thanks for this comment. The broader insight that different analyst choices can have large effects on estimated damages correctly applies to most areas in the climate damages literature, not just the temperature-growth area. As an example, in the recent EPA-led exercise to update the SCC using bottom-up sector-specific damage estimates, estimates of health related damages differed between input studies by a factor of 2, and estimates of agricultural damages differed by a factor of 25. Thus our setting is not at all unique in this broader literature.

Specifically in our setting: estimates by Nath et al in their working paper are in fact about half as large as ours, not an order of magnitude smaller (theirs is roughly a -10% GDP impact by 2100, compared to our roughly -20% impact; the exact comparison depends on the years of aggregation and warming scenario used). In a newly revised version of their estimates, they have told us in personal communication that their numbers are now even closer to ours than their original working paper numbers. So, reported differences between their paper and ours will likely be smaller once their paper goes through peer review (though we will all of course need to wait for the published results). Similarly, we note that our estimates are extremely close (within 5%) of recent SCC estimates from Bilal and Kanzig (2024 working paper), another recent high-profile entrant in the temperature-aggregate damages literature. It is therefore the case that differences across studies in this temperature-output literature are actually beginning to converge, at least relative to other climate impact areas – although they each still provide much larger damage estimates than the bottom-up estimates suggest. So we politely disagree with the referee that these different papers are providing fundamentally different answers. In our revised discussion, we now make these comparisons with the bottom-up and top down estimates clearer. The added text now reads:

"Our estimates of the SC-CO2, the quantity most easily comparable to other studies, are at least 5x larger than recent estimates using ``bottom-up" cross-sectoral approaches to damage estimation (Rennert et al 2022, EPA 2022), although they are very similar to a recent global econometric estimate (Bilal and Kanzig 2024) as well as to bottom-up estimates that use equity weights to account for differential welfare effects in low income countries (Nath et al 2022)."

Note that the Nath citation is to a different paper where they estimate the SCC using a different approach. The Nath Klenow Ramey paper cited by the referee does not compute the SCC.

Not surprisingly, the authors have taken a particular stand on this issue, arguing for the BHM approach. In light of the above-mentioned large parameter changes they have obtained in going from their previous implementation of BHM to the current one, I think it would be wise, in case of acceptance, to require them to publish a replication package that includes all data and code, allowing other researchers to replicate the L&D estimates in the paper as well as conduct their own sensitivity analyses.

Yes absolutely - as with all our papers, we publish all of our data and code to allow full replication of every result in the main paper and the supplemental materials.

Minor comments:
- I appreciate the way cumulative uncertainty has been integrated into the presentation of results.
- L. 132 better: in *the* last 60 years
- L. 152 "there" instead of "their"

Thanks, fixed.

- L.227-229: I did not understand the meaning of the two sentences until reading the explanations given by the authors regarding this in the rebuttal. Can this be rephrased?

Thanks. We have attempted to re-phrase to clarify the meaning. The text in that section now reads as follows:

"*We use our framework to quantify components of L\&D from three types of emitters -- individuals, companies, and countries --- again focused on the impact of average warming on GDP. We show that the calculation of the impact of removing an emitter's entire emissions history, which in principle could be large enough to generate non-linear effects, can be closely approximated by simply multiplying their schedule of emissions in each year by estimates of HD-CO2, FD-CO2, and SC-CO2 from above (see Methods).*"
- I find the description of scenarios in ED9 too cryptic- can this be expanded or better explained?

Thanks. Because we were out of space in the caption, we added additional explanation in the Methods section "Sensitivity of damage estimates to analytic choices" and now point readers to this section at the end of the caption.

- In Figure 4, looking only at emissions from the most recent 30-40 years can be justified as in lines 599 and following, but the figure would likely look very different if historical emissions were considered all the way back to 1960 or earlier. I think this would be interesting. On the recipients' side, the ranking is not very surprisingly given by GDP levels, given that all growth effects are relative to these levels.

Thanks, this is a fair point. We have now re-run the estimates in Fig 4 using emissions data starting in 1960 – results are displayed in new Fig ED16. Global damages are more than 5x higher than starting in 1990, a result of the larger attributed overall warming and the much longer period over which growth impacts can aggregate. As anticipated by the referee, the share of attributed damages to specific emitters changes to some degree with the start year: the US share stays about the same (it has contributed a roughly constant 20% of global emissions for the last half century), but the Chinese share drops and the European share of attributed damages rises with the earlier start year.

We have amended the relevant text to read:

"*Estimates are roughly twice as large for emissions starting in 1980 rather than 1990 (Fig ED15) and more than five times larger for emissions starting in 1960 (Fig ED16), with China representing a smaller share (and EU a larger share) of damages the longer the time window; these results highlight the importance of clarifying t0 in historical L&D calculations*."

Referee #1 (Remarks on code availability):I have reviewed the code because downloading this package might allow authors to find out about my identify. Packages should be hosted elsewhere where authors have no way of monitoring from which IP address the data are downloaded. Even in this case, I think that the code should best be reviewed by a professional data editor. It seems unlikely that referees can accomplish this in the short amount of time given.

Thanks. The code and data for replicating all main results is now available at https://github.com/echolab-stanford/loss_damage where it can be accessed anonymously by reviewers.

Referee #2 (Remarks to the Author):

The authors have responded to all of the reviewer comments, including doing new analysis. The new set of figures has some very large differences to the original manuscript. This has not been adequately highlighted in the response to reviewers, and I confess that I only noticed as I was about to submit my review. In Figure 2 and Figure 4, the largest estimated impacts (by quite a margin) are in the USA. I think the authors need to explain why this is, and what this means about their methods: are they very sensitive to methodological choices? Do the estimated damages by country differ dramatically based on different methodological choices? This has important implications if the framework were to be used in policy. Equally, they should comment on the implications of the results – if the largest losses and damages are in the USA, what does this mean for L&D as an approach to address climate change impacts in vulnerable countries? Does this imply that the USA is the most vulnerable to climate change? And what about the countries that have been campaigning for L&D since the 1990s? (i.e. small island states). Clearly, a framework which estimates L&D based on GDP will always estimate larger damages in countries which have large GDP and relatively small damages in countries that have low GDP to begin with. So is this the best approach to support low emitting countries suffering impacts from extreme weather events and sea level rise which are exacerbated by emissions from countries with high GDP? This relates to the comments in my previous review - that the paper does not sufficiently emphasise that this is only a partial estimate of L&D.

Thanks for these insightful comments and for the careful reading of our revised manuscript. Below we address in turn the numerous points and questions that the referee raises here.

Are results sensitive to methodological choices? Yes, as is almost always the case in the climate damages literature, results can depend substantially on choices made by the analyst. This has always been the case with certain parameters (e.g. the discount rate) and choices about time horizon (the future period over which impacts should cumulate) or choices about how to study the relationship between climate and the outcome (e.g. older approaches using cross-sectional variation and more modern approaches using panel variation). In our setting, results do depend critically on the period over which a given temperature shock is allowed to affect growth. Models that only allow temperature to affect growth in the current period generate much smaller estimates than models that allow temperature to affect growth in current and subsequent years. Our updated paper has a now much expanded methods and supplemental section on our approach to model selection, which we believe follows best practices, given particulars of our setting. This is a choice that can be determined empirically and that's what we do in the paper. We then show extensive results under alternate models and parameter choices, transparently propose which choices we think are most appropriate, and provide all the analysis and underlying data a researcher or policymaker would need to generate results under a different set of choices.

Do the largest dollar losses in the US imply that the US is the most vulnerable to climate change? Absolutely not, and we appreciate the referee bringing this up. The US dollar losses are largest because the US is the largest economy, and we agree with the referee that (for example) our map in Fig 2 provides an incomplete picture of who is hit hardest in a welfare sense. In response, we have gone back and computed both per capita losses as well as losses as a percentage of GDP in each country. The latter in particular highlights the vulnerability issue the referee brings up, and we now use % impacts as the main result shown in the maps in Fig 2). A new Fig ED7 in the supplement now shows the original map in terms of total $ losses, as well as total $ losses per capita.

Is this the best approach to support low emitting countries suffering impacts from climate change? In this paper we develop a framework for linking specific emissions to specific damages, and to apply that framework to develop (partial!) estimates of past and future damages and to whom they can be attributed. Our approach and results could be useful to policymakers wishing to support low emitting countries suffering impacts from climate change – and indeed, the damages we estimate here in those countries, even though they are only partial, are substantially higher than many existing estimates, and are very large in per capita terms and as a proportion of overall income. This means that in a world in which emitting countries compensate countries suffering damages, our numbers imply that these transfers should be very large – again, substantially larger than what most existing estimates would suggest, including estimates focused on non-GDP impacts.

The three main issues I had with the paper originally have also not been fully addressed:
1. The paper still minimizes the importance of (a) hazards which are not related to mean temperature and (b) impacts which are not related to GDP. (a) and (b) are arguably the majority climate change impacts which matter for people. These are also the impacts that are more often referred to in discussions around Loss and Damage.
2. In places, the wording and emphasis in the paper is still quite difficult to understand and put in context for a reader a different academic field.
3. The paper makes only passing references to extreme event attribution research, which is another approach that has been discussed in the context of loss and damage negotiations. In fact, there has been quite a lot of literature, over the past 10 years or more, which has discussed extreme event attribution and the challenges and opportunities for using it in the context of L&D policy. Some of the same issues are coming up here, for example: the challenge of developing a scientific approach when the policy is vague, the challenges of describing an approach to compensation, when compensation is a word that some negotiators are not even allowed to say out loud, and the challenges of developing scientific evidence for supporting those impacted by climate change, when those countries are also those for whom the strength of scientific evidence is likely to be weakest due to data limitations. I did not fully elaborate on this

point in my previous review, as I thought it might be the case that the authors were aware of this work, but needed to add a bit more comment and citation. On reading the response to reviewers and looking again at the paper, in particular the new additions at the end of the discussion, I think maybe the authors are not aware of this literature, and therefore I have suggested some references that are important to help frame the paper (Hulme et al. 2011, Huggel et al. 2016, James et al. 2019, King et al. 2023).
These three issues make it difficult to connect the work with the existing wealth of literature on climate change impacts and adaptation, and growing literature on loss and damage. Given the broad readership of Nature I think further changes are needed to help the reader understand the implications and scope of the study. Furthermore, from a policy perspective it is important that the reader can understand that the framework proposed cannot address many (or even most?) climate change impacts. If the framework from the paper were picked up and used to design the distribution of L&D funding, it would overlook important impacts, including the impacts of sea level rise on small island states, impacts for the most vulnerable, and loss and damage following specific disasters.

Thanks for these important and very constructive comments. We respond to them in the context of the specific and helpful suggestions made below.

I have made some specific suggestions to help address these issues.
Title: "Quantifying climate change loss and damage consistent with a social cost of greenhouse gases" – I don't think that "the social cost of greenhouse gases" will be very meaningful to people outside of economics. I also think "social" is a strange word to use to describe impacts on GDP, since the paper does not deal with other impacts which will have much more "social" impact e.g. loss of housing from flooding, loss of homeland from sea level rise, loss of livelihood from drought, loss of cultural heritage, damage to mental health from extreme weather events… I suggest a modification to the title: "Quantifying climate change loss and damage associated with the impact of average temperature on economic growth"

Thanks for this suggestion. We are going to take the referee's advice in the next comment and alter the abstract rather than the title. The term "social cost of greenhouse gases" is a technical scientific term used to measure the impact of climate change. Because a main point of our paper is to show how the computation of loss and damage can be done in a way directly consistent with consensus methods laid out in US Government and National Academy reports for the calculation of the social cost of carbon and methane, our view is that this phrase belongs in the title. We note that the phrase has recently been used similarly in a Nature title to indicate this metric (Rennert et al 2022 *Nature*: Comprehensive evidence implies a higher social cost of CO2).

Abstract – the authors have made several important changes to the abstract – noting that they are only referring to "one component of L&D" and also adding confidence intervals around the estimates. This is much appreciated. I think that, with the suggested change

to the title, this is sufficient to make clear that the paper is focusing on average temperature and aggregated economic output. However, if the title is not changed, I would like to see additional wording added to the abstract, something like "It is important to note that many climate change hazards (e.g. sea level rise, flooding, drought) are not related to average temperature, and many losses and damages are not related to aggregated economic impacts. Therefore our estimates of L&D are likely to be a substantial underestimate of all L&D from climate change."

We have added a slightly shortened version of this as the second to last sentence in the abstract: *"We emphasize that these estimates do not capture important losses and damages that are not related to temperatures effect on the aggregate economy, and therefore our estimates of L&D are likely to be a substantial underestimate of all L&D from climate change."*

Line 34 – 39 – the authors have added more recognition of the work in extreme event attribution, which is much appreciated. However, these sentences imply that extreme event attribution research only relates emissions and impacts. In fact there has been some work in extreme event attribution which links emissions to specific countries or companies. There is a summary of this in a book chapter James et al. (2019), section 5.3.3. I suggest citing a couple more papers, and rewording to acknowledge this (suggested change in italics).

"However, unlike the large body of empirical work which explores how global climate change will affect global and local economic outcomes, the question of whether and how emissions by specific entities (e.g. countries, companies, or individuals) can be linked to specific damages has received less formal and empirical attention in economics – although substantial headway has been made in understanding how the probability of specific extreme events and their impacts can be linked to a changing climate13–17."

Thanks - these were important omissions on our part. Building on the referee's suggested text, we have now re-worked that paragraph to say:

"*However, unlike the large body of empirical work which explores how global climate change will affect global and local economic outcomes, the question of whether and how emissions by specific entities (e.g. countries, companies, or individuals) can be linked to specific damages has received less formal and empirical attention -- although substantial headway has been made in understanding how the probability of specific extreme events and their impacts can be linked to a changing climate [13-17], and how anthropogenic forcing and its impacts on climate extremes can be linked to specific national, regional, or corporate emitters (Otto et al 2017, James et al 2019), albeit with some disagreements on how such attribution can be applied in an L&D setting (King et al 2023)."*

Reviewing this paper has made me reflect that it would be fantastic for there to be more collaboration between the econometrics community and the extreme event attribution community, and it would be fantastic to get an economics perspective on research in

extreme event attribution that has estimated monetary losses from extreme weather events.

We agree 100%, and would welcome such collaboration! In fact, our author team begun exactly out of this idea and is literally a collaboration between climate climate science extreme event community and the econometrics community. More of this would be great.

Line 46-48 – Suggest to edit (edit in italics):

“Drawing from multiple fields, and focusing on the impacts of average temperature on aggregate economic output, we show how…”

See response to the next comment - we have combined this suggestion with the next suggestion in a re-worked paragraph.

Line 51-56 – The authors have rephrased their comments about L&D with respect to the Paris Agreement. I appreciate the effort to navigate a difficult political topic. However, this reasoning is a little hard to follow – what is the distinction between stating that L&D should reimburse damages from those who have emitted, and stating that this is not necessarily what is “owed”? Is the idea to suggest a possible approach to L&D, but then to say that this approach is not necessarily morally correct? The next paragraph then goes on to talk about liability, which is explicitly excluded in the Paris Agreement. So I don’t think it is possible to say that the proposed approach is consistent with the Paris Agreement. The “completely reimburse” and reference to “non-economic impacts” in this paragraph are also a bit confusing – since the paper does not propose a framework to deal with non-economic impacts, and – as the authors note – it is “partial” so it does not provide a framework for how to completely reimburse loss and damage.

One suggestion for how to address this: the authors could (a) acknowledge L&D is contentious, that the Paris Agreement explicitly states that L&D does not refer to liability or compensation, (b) note that there are some who nevertheless view L&D as a route towards compensation, and that there are important discussions about compensation and reparations outside of the UNFCCC framework, for example with reference to legal cases, (c) therefore note the potential value of academic analyses to conceptualise how L&D can be related to specific emitters, (d) highlight the authors own suggested approach to conceptualise L&D, (e) highlight that their framework shows how to compute part of this L&D.

This is extremely helpful. We have reworked both of these introductory paragraphs to better highlight how our definition of L&D relates to Article 8 in the Paris Agreement, and how our empirical implementation of our proposed definition focuses on annual temperature and GDP. We clarify that our central proposed L&D definition does not attribute damages to specific emitters, and thus is consistent with Paris. Using the referee’s proposed logic above, we then show that we can extend our framework to quantify emitter-specific attributable damages, which is central to multiple ongoing legal cases around the world.

That paragraph now reads:

"We show how existing approaches used to quantify future aggregate climate damages can be re-purposed to also quantify bilateral, attributable loss and damage from historical emissions. Building on IPCC documents[4,9] and a growing academic literature[10,11], we propose that L&D be computed as the net present value of economic and non-economic impacts attributable to the emissions of greenhouse gases through their impact on the climate, net of any adaptation that was undertaken. Consistent with Article 8 of the Paris Agreement, this definition of L&D does not imply liability on the part of the emitter(s) or owed compensation in impacted regions. However, important discussions about compensation and reparations are occurring outside of the UNFCCC framework (such as in numerous ongoing legal cases (Stuart et al 2021)), and thus there is scientific value in conceptualizing and quantifying how L&D can be attributed to specific emitters. We show how the above source-agnostic measure of L&D can be equivalently computed as the payment schedule that would completely reimburse all harmed parties for the damages (or benefits) that they have experienced or will experience from climate change, paid for by the parties that caused these impacts via emissions -- although we again emphasize that these damage estimates do not necessarily equal what is ``owed" by one entity to another, as that is a moral and legal question beyond the scope of this analysis."

Line 69 - could you explain a little more what is meant by marginal? I note there is lots of discussion with the other reviewers about marginal emissions/costs/effects, but I think for a wider readership it is not obvious what is being referred to. Please also check other references to marginal later in the text.

Yes sorry that was jargon. We now define "marginal" after its first mention as "additional unit of emissions above background".

Line 79 – The authors have added more information about the SCC which is much appreciated. What is meant by "nests"? Please clarify.

Also jargon. We have re-written to now read: "The SC-CO2 for emissions today -- a commonly referenced benchmark – is then a special case of our general framework."

Line 98-99 – suggested edit (edit in italics): "Our implementation allows estimation of past and future economic damages associated with average temperature change from any observed or projected emissions perturbation"

Great – added. Thanks.

Line 142-143 – “individual populations” – does this mean country populations? Suggest to rephrase as “individual countries” or “country populations”. “damages” – is this damages to GDP? Suggest to rephrase “cumulative damages to GDP”

Great suggestions – now reads: “Our approach is generalizable to the calculation of cumulative damages to GDP, for country populations or globally, through 2020 and after 2020…”

Line 148 – what is meant by local warming? Is it country level? If so suggest to rephrase e.g. “country-level warming” Is “damages” damages to GDP? Suggest to rephrase “damages to GDP”

Great – added both clarifications. Thanks.

Line 153-157 - this is clearer, thank you. Suggest to add “country-level” before warming, line 154. Suggest adding “(including from sea level rise and extreme weather events)” after “warming climate” (line 156). Suggest adding “(such as loss of life, damage to mental health, loss of livelihood, loss of homeland)” after “non-economic damages” (line 156).

Thanks for these suggestions. That sentence now reads: *“We emphasize that our focus in this example is also on damages to GDP from warming, which is a subset of known and anticipated damages from climate change, and will not account for damages from unmodeled climate extremes or sea-level rise, or from the broader range of economic and non-economic damages that could result from a warming climate (including loss of life, damage to mental health, and loss of livelihoods or homeland). Our estimates, much like existing benchmark estimates of SC-CO2 (EPA 2022), are thus partial estimates, yet our framework is applicable to these other contexts.”*

Line 209-212 – It’s interesting that in this part of the text and in the response to the reviewers, the authors’ emphasis is on rates of past adaptation as a predictor of rates of future adaptation. What about policy and public awareness? Although it arguably hasn’t been that successful yet, recent decades have seen a huge growth in initiatives around climate change adaptation. Do the authors assume at all the effort, funding, research that is being invested in deliberate adaptation by countries, companies, farmers, households and beyond will not work? If so, suggest to briefly note why this is. If not, suggest to rephrase the sentence “it is of course possible that future adaptation could exceed current adaptation” so that it is does not imply that future adaptation is unlikely.
It would also be useful to compare the findings of L&D with and without adaptation with other research to estimate the benefits of adaptation e.g. Faldauer et al. (2022).

We agree and very much hope that adaptation can and will occur! However, in the GDP data we focus on, we see basically zero evidence of adaptation since 1960, in that the response function has not changed at all. A subset of the author team has a separate paper in review (https://www.nber.org/papers/w32985) quantifying adaptation across a much broader range of outcomes that is unfortunately quite similar in that we see fairly limited evidence of past adaptation, despite lots of resources being spent and already 1.3C of warming. So, recent trends do not provide optimism, but again, this does not mean future adaptation can't vastly exceed past adaptation. We have amended that section to now read:

"Our historical damage function does not rule out historical adaptation but rather estimates damages net of all adaptations that have occurred, and our baseline estimates of future damages implicitly assume that future adaptation will resemble past adaptation. This assumption, at least in the near term, is consistent with historical evidence that the damage function has not flattened over time (Fig ED2b, Fig ED3d), and evidence on similarly limited adaptation across other sectors (Burke et al 2024). However, it is of course possible that, with increased recognition and funding, future adaptation could exceed current adaptation. To model this possibility,…."

Line 221 – Suggest to edit "L&D" to "L&D to GDP from average warming"

Thanks – that sentence now reads: *"We use our framework to quantify components of L&D from three types of emitters -- individuals, companies, and countries --- again focused on the impact of average warming on GDP."*

Line 222-224 – This part is a bit tricky to follow, could a little more explanation be added?

Yes this was indeed confusingly written. This issue is now rewritten more simply, and then discussed more extensively in a revised Methods section. The main text paragraph now reads:

"We use our framework to quantify components of L\&D from three types of emitters -- individuals, companies, and countries --- again focused on the impact of average warming on GDP. We show that the calculation of the impact of removing an emitter's entire emissions history, which in principle could be large enough to generate non-linear effects, can be closely approximated by simply multiplying their schedule of emissions in each year by estimates of HD-CO2, FD-CO2, and SC-CO2 from above (see Methods)."

And the relevant revised Methods section is now titled "Non-marginal and interdependent emissions reductions" (we don't reproduce it here for brevity).

Line 230-291 - How uncertain are these estimates? Is it possible to add uncertainty ranges?

Yes - good idea. Fig ED12 now reports point estimates and 95% confidence intervals for all values reported in Fig 3.

Line 300 – "simple scenario" – would "hypothetical scenario" be more appropriate?

Yes, that's probably more appropriate [redacted] Fixed.

Line 319 – suggested edit (edit in italics) – "develop a framework for estimation of L&D to GDP from average warming". It is misleading to say that the framework is developed for the estimation of L&D in general, since it does not cover all types of L&D.

We have reworded the sentence as follows:

"We propose a formal definition for quantifying L&D that is grounded in economic principles and in recent advances in the measurement of climate change damages, develop a framework for its estimation, and carry out an example empirical implementation of that framework focused on the impact of changing mean temperature on country-level GDP."

We note that our framework could be applied to any damage function, so long as there exists a mapping from emissions to a change in the relevant climate variable, and a mapping from the climate variable to an outcome of interest. We now say exactly this in the discussion as well:

"*More broadly, our framework for computing loss and damage should be applicable in any setting with the following ingredients: accurate baseline measurements of some outcome of interest (GDP, health, etc), a credible damage function linking that outcome to a measured climate variable, a modeling approach able to estimate local changes in that climate variable as a function of emissions perturbations, and accurate estimates of emissions from an emitting entity of interest.*"

Line 354-358 – one of the challenges in adapting to date, has been that parties to the UNFCCC have failed to deliver on the adaptation funding they've promised. For similar reasons, it might be a struggle to generate L&D funding. This might be a further challenge worth discussing.

Good suggestion. We have added that directly to paragraph. That sentence now reads:

"One clear challenge is that public funding for adaptation has lagged both promises and needs (UNEP 2024), suggesting loss and damage funding could be similarly difficult to realize."

Line 391-393 - suggest to add more examples to help the reader put the work in context. For poorly captured in GDP data examples could also include loss of income by farmers (where crop failure due to high temperatures might increase prices and therefore lead to increases in GDP or no change in GDP whilst farmers experience loss and damage). For non-economic impacts, examples could include damage to physical and mental health. For impacts not highly correlated to country-level temperatures, examples could include tropical cyclones, heavy rainfall events, ocean acidification, marine heat waves.

Thanks. We have added additional examples as suggested. That sentence now reads:

“Other channels that are poorly captured in GDP data (for instance, damages to physical and mental health or various non-economic impacts such as ecosystem function, human displacement, and loss of cultural heritage or homeland) or impacts that are not highly correlated with interannual variation in country level temperatures (e.g. sea level rise, tropical cyclones, or extreme rainfall events), will not be reflected in our current estimates. Similarly, our results do not fully capture the impact on GDP of variation in temperature and other climatic extremes, as these are imperfectly correlated with annual mean temperature and exert independent influence on damages.”

Line 396 – “To the extent that these channels are quantitatively important” – this seems to imply that the quantitative importance of climate change impacts beyond mean temperature is in question? This seems misleading. Frame et al. (2019) estimate that at least $30bn of damage from Hurricane Harvey can be attributed to climate change. Of course, this is just one extreme weather event. Otto et al. (2020) describe how estimates from many extreme weather events could be collated to develop an inventory of impacts from anthropogenic climate change. Frame et al. (2018) demonstrate a similar approach for New Zealand, estimating that climate change related floods and droughts cost New Zealand >$800m over a recent 10 year period. This is without mentioning the economic implications of sea level rise. Perhaps the sentence could be rephrased:

“Given the substantial economic damages associated with extreme weather events, some of which can be attributed to climate change (Frame et al. 2018, 2019), the impactful nature of sea level rise, and the importance of non-economic loss and damage (Tschakert et al. 2019), our damage estimates understate the total damages associated with historical emissions, likely substantially.”

We have added these citations to the previous sentence listing what we do not pick up when we measure GDP, and simply amended this sentence in question to read:

“As a result, our damage estimates will understate the total damages associated with historical and future emissions, perhaps substantially.”

Line 400-403 – Could the authors expand a bit on which cases this is more likely to be applicable for? For example, many non-economic impacts (such as damage to mental health) are less well documented, more difficult to predict, and difficult to quantify.

Thanks. We note that we have worked exactly on quantifying mental health impacts, e.g see here. We have edited that sentence to now read:

“However, our basic approach could be extended to include many of these other channels, so long as there is a clear mapping between changes in global mean surface temperature and location-specific changes in the climate variable of interest, and a credible way of linking these changes to damages. Such an extension will be more straightforward in settings where damage

valuation is common (e.g. mortality (Carleton et al 2022)) and substantially more difficult in settings where impact quantification is challenging (e.g. loss of place (Tschakert et al 2019)) and/or where attribution of observed climatic extremes is difficult or contested (King et al 2023).”

Line 404-411 – It is important to note that these criteria are most often missing in places that are highly vulnerable to climate change, as has been discussed with reference to extreme attribution research, with many researchers expressing caution at the use of science for policy for this reason. See Hulme et al. 2011, Huggel et al. 2016, James et al. 2019, King et al. 2023).

We have now included this important point in the above sentence, please see above.

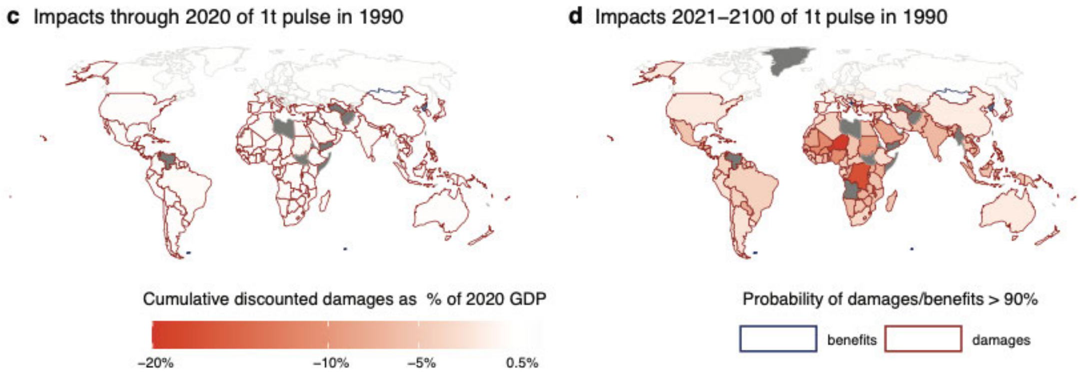
Line 457 – “Our focus on average temperature helps overcome concerns regarding the use of extreme event attribution in L&D assessments” – perhaps, but it raises new concerns, because the hazards and impacts that many L&D campaigners care about are ignored. The focus on GDP will also lead to additional concerns because the largest damage estimates for the richest countries.

Fair point. We have made two changes in response. In that paragraph, we now again emphasize that we will miss many important impact channels and that these missed channels are particularly important in LMIC settings:

“As a result, our damage estimates will understate the total damages associated with historical and future emissions, perhaps substantially, and not capture certain damages that are critically important for many low- and middle-income countries (e.g loss of homeland).”

Our second edit is to now focus in our main damage figure (Fig 2) on relative damages at the country level (i.e. damages as a percent of GDP) rather than total damages. This emphasizes that in percentage terms, damages are substantially larger in lower-income tropical countries. The new map is displayed below and these results are now highlighted in the main text as well, eg lines 193 and following in the revised manuscript:

“Aggregate past and future damages from historical emissions are a mix of modest benefits or limited damages in high-latitude countries, where we find that warming has a limited or uncertain impact on growth, and widespread damage in mid-latitude and tropical countries, where warming harms growth with high confidence and where damages as a percentage of current GDP are substantial (Fig 2c-d).”


c Impacts through 2020 of 1t pulse in 1990
d Impacts 2021–2100 of 1t pulse in 1990
Cumulative discounted damages as % of 2020 GDP
−20%
−10%
−5%
0.5%
Probability of damages/benefits > 90%
benefits
damages

Referee #3 (Remarks to the Author):

I thank the authors for the clarifications and additional analyses. I continue to consider their framework as really useful and the applications as quite illustrative. With regards to the specific numbers they present, their magnitudes have increased considerably compared to the initial version, partly by more than an order of magnitude (with Social Cost of Carbon estimates in the order of >$2000/ton). This is because the authors now do not only assume growth effects but also lagged growth effects, i.e. the growth effect of a temperature shock builds up over five years and then stabilizes at that level. To justify that, the authors present distributed lag models where they regress growth on temperature levels with up to 10 lags and a local projections approach.

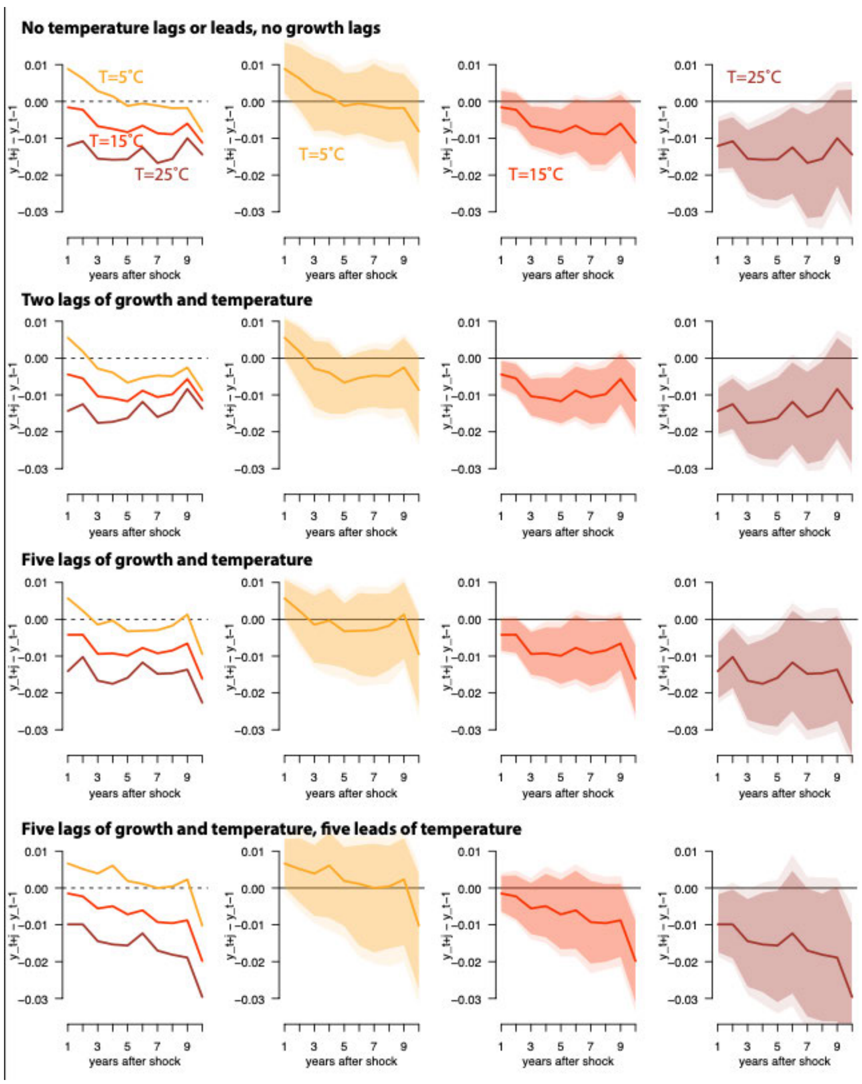
We thank the author for his or her kind words. An important point of disagreement: we do not "assume" growth effects nor lagged growth effects - we estimate the model and let the data determine whether there are persistent effects on output (i.e. the sum of contemporaneous and lagged effects is different than zero) and whether these effects are larger or smaller as more lags are included. The fact that the sum of contemporaneous and lagged effects is not zero at most points in the temperature distribution is what tells us there are persistent effects.

We now state this clearly in the Methods section describing the econometrics:

"To distinguish between ``level effects", in which output returns to its previous trajectory in the years following a temperature shock, or ``growth effects", in which output is permanently lower following a temperature shock, we again follow earlier work (Dell et al 2012) and estimate distributed lag models where growth is modeled as a function of contemporaneous and lagged values of temperature. The sum of contemporaneous and lagged effects offers an empirical test of whether the effects of a temperature shock on output are persistent or transitory."

Given that this assumption has such a big influence on the damage magnitudes, the authors should do more to justify + discuss this choice:
1) From the figures alone, it is hard to judge how noisy/significant the individual lagged terms really are and how well supported this assumption is by the data. The authors should add more details on both approaches; in particular:
a) What about concerns of serial correlations - how did they arrive at the conclusion that the inclusion of one lagged growth term in the local projections approach is sufficient and why did they include lagged growth terms but not lagged temperature terms?
b) how are standard errors treated in both approaches?
They should present regression coefficients + their significance levels, information criteria for models with different numbers of lags and plot the respective coefficients incl. their uncertainties such that one can better see how well the lagged growth effects assumption is supported by the data.

Thanks for these suggestions. We have made numerous additions and revisions to the paper in response. First, in new Supplemental Methods Fig S6, we show robustness of the local projections approach to different controls for lags of growth, lags of temperature, and/or leads of temperature. Specifically, we now run the model in four ways: with no lags of growth or temperature, with two lags of each, with five lags of each, or with five lags of each and five leads of temperature. The subplots in the panel show local projections estimates for countries with average temperatures of 5C, 15C, or 25C. We find that these alternate controls for temperature and growth lags all generate quantitatively and qualitatively similar estimates.



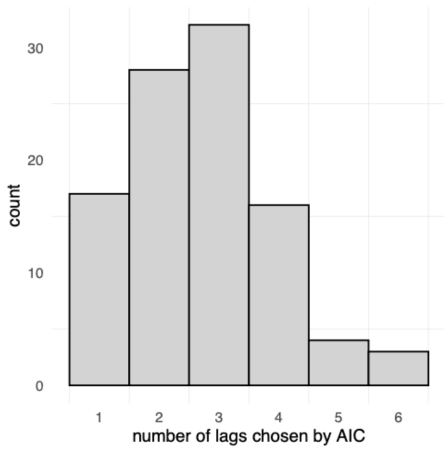
Regarding the estimates for individual coefficients, what matters for our analysis is not whether individual coefficients are significantly different than zero (or from each other), but whether cumulative effects are different from zero at different lag lengths. Given noise in the outcome data, it is possible (and probable) that individual coefficients can be noisy, especially as we are estimating quadratic functions.

Regarding standard errors, all are calculated using robust standard errors, clustered at the country level. This accounts for arbitrary forms of temporal autocorrelation within a country over time and spatial autocorrelation across contemporaneous locations within a country, as well as cross-correlations.

We respectfully disagree with the referee that his/her proposed tools for model selection – either dropping individual variables that are not significant, as in Kotz et al 2024, or selecting lag length based on AIC/BIC – are reliable methods for model selection in our context. This is for two reasons. First, as noted by the referee, temperature is autocorrelated, meaning that individual coefficients will pick up part of the effect of other variables if those variables are omitted, or that the model will split the variation between variables if they are highly correlated and both are included in the model. In this setting, individual coefficients can be noisy but the sum of the coefficients is precisely estimated. Second, and perhaps more importantly, using model selection tools that select on (penalized) likelihood or MSE is unreliable in low r2 settings - i.e where independent variables of interest explain a small amount of the overall variation in the outcome of interest. Third, as pointed out by Greene in his benchmark 2003 econometrics textbook (section 19.2.3), if too few lags are included, then estimates of the coefficient vector on the lags are biased and inconsistent, and the covariance matrix is biased as well, and so inference (e.g. selection based on coefficient significance) is unlikely to be successful.

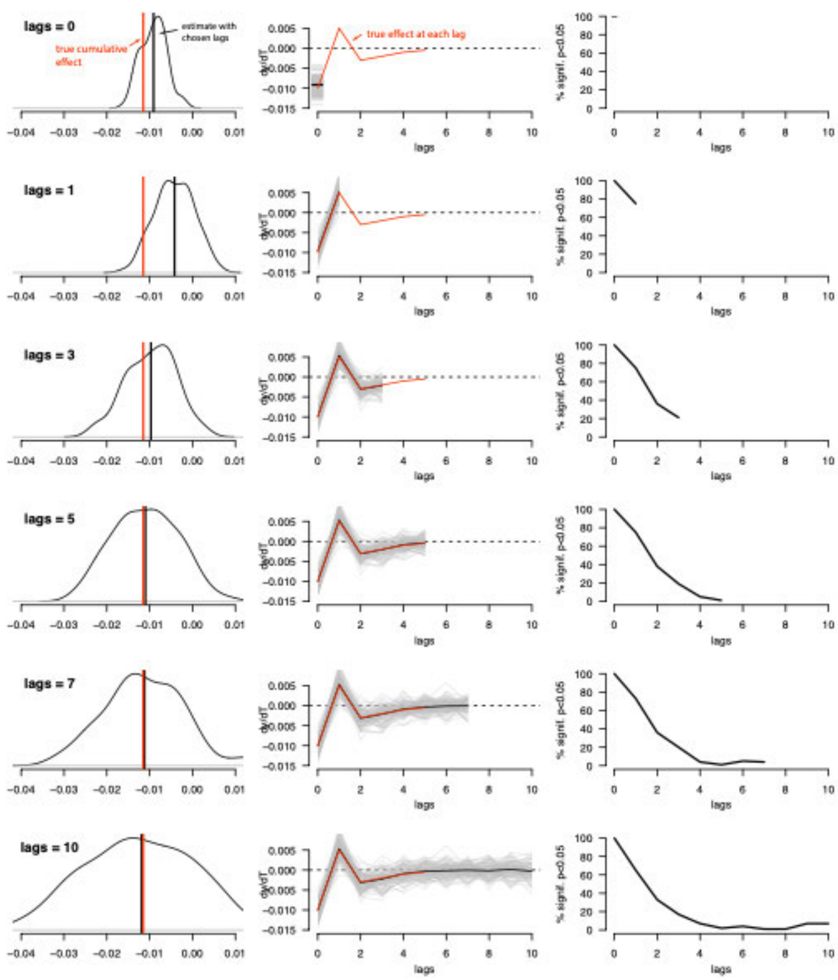
A simple simulation demonstrates these points. We simulate a panel setting where the “true” relationship between temperature and GDP growth is a five-lag model (i.e. where a given temperature increase in one year affects GDP growth up to five years in the future), where the autocorrelation structure in the temperature data is exactly how it is in our observed real-world data, and where noise is added to the outcome variable (GDP growth) such that the r2 of the relationship between GDP growth and temperature is what it is in the observed data (namely, fairly low, near 0.01). In essence, we are re-generating our panel data to have similar autocorrelation and noise properties as the true data, but where we know exactly what the temperature-GDP relationship is. We then re-estimate the GDP growth ~ temperature relationship using models that have zero to 10 lags, and use AIC to choose the preferred number of lags. We re-run this simulation 100 times, with new draws of the noise each time, and study two quantities: how often the AIC criteria chooses the “true” 5-lag model as the preferred model, and how often “significance filtering” (or choosing whether to include individual coefficients based on their individual statistical significance) leads to the selection of the correct number of lags.

Results in the first plot below (new Fig S2 in the paper) shows a histogram (across 100 simulations) of how many times the AIC criterion chose a model with a given number of lags. Given the noise in the outcome data (many things beyond temperature affect GDP growth) and the autocorrelation in the temperature data, an AIC criteria chooses the “true” 5-lag model only rarely, mostly choosing models with fewer lags given the penalty term.



The next plot (new Fig S3 in the paper) shows the results for lag selection and implications for model bias when using significance filtering to choose lags. The model is biased (by construction) until 5 lags are included, and inclusion of additional lags just adds noise but no additional bias. However, significance filtering again would have led to the selection of too few lags. Thus, as a rule, it is always considered safer (in the sense of minimizing bias) to have too many rather than too few lags; again, Greene (2003) makes this point explicitly.

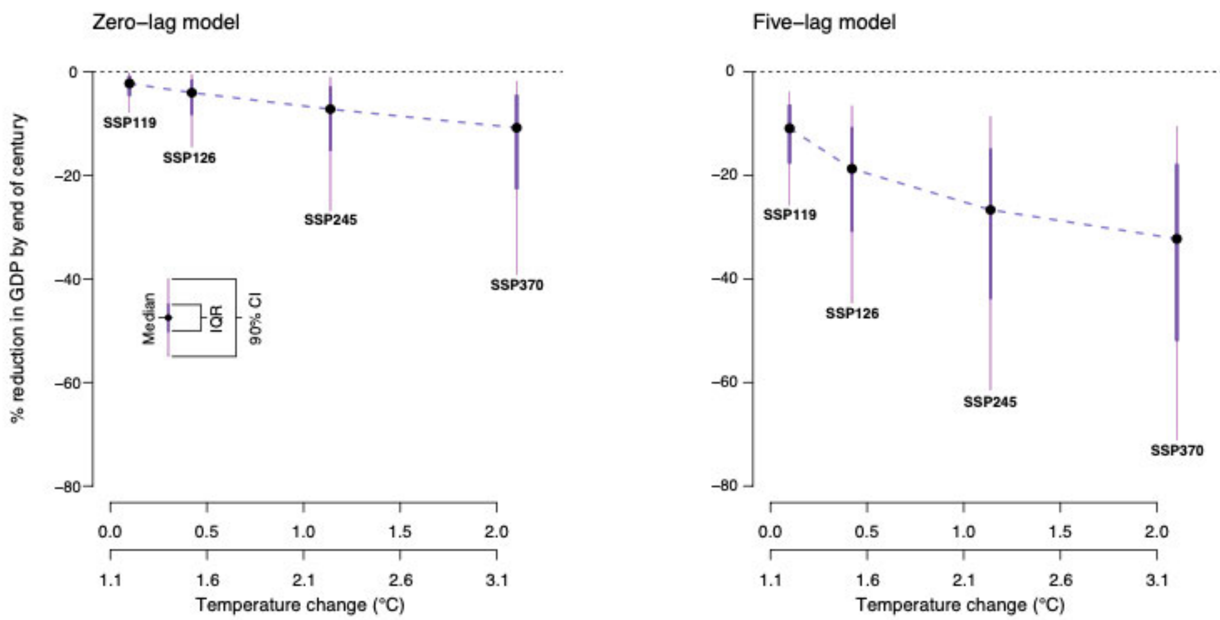
Figure S3: **Distributed lag estimates with too few lags are biased in simulation, and significance filtering does not select the correct number of lags**. Simulation is as in Fig S2; see text. **Left panels**: estimated cumulative effects under different lag lengths; true model has effects out to 5 lags. Red vertical line is the true cumulative effect after 5 lags, black line is the mean effect for a given lag length across simulations, and the density plot is the distribution across simulations. Models with too few lags are biased in their estimates of cumulative effects. Models with too many lags (i.e. more than five) are unbiased, but cumulative effects estimates are increasingly noisy as more lags are added. **Middle panels**: estimates on individual lag coefficients for the same models. Red is true coefficients in the 5-lag model, black is mean estimated coefficients across simulations, gray lines are estimates for each simulation. **Right panels**: for each model and coefficient, the proportion of simulation runs where a 5% significance filter (i.e. the decision to retain a given lag because the p-value on that lag was <0.05) would have selected that lag. The true model has effects out to five lags, but given noise in the outcome, later lags are unlikely to be selected using this significance filter, leading to bias in the estimated cumulative effects (i.e. choosing a model with too few lags).



Given failure of these model selection approaches to consistently select the right model in a simulated setting that matches key aspects of our real-world data, we instead follow the advice of Greene (2003) and the results in this simulation, and test models with large numbers of lags, and then look for lag lengths where cumulative effects no longer change upon inclusion of additional lags. As shown in Fig ED4, that is somewhere between 3-7 lags.

2) Given that the question of growth-vs-level effects is heavily discussed in the literature (1, 2) and that the authors here opted for something one could call "growth effects plus", they should add more discussion on that choice and its implications. For example, more conservative approaches have been proposed where growth is regressed on first-differenced temperature or temperature shocks, implying that the model prior is one of level effects and it is then tested whether the effects persist any further (3–5). While certainly arguments could be made supporting less conservative modelling choices with respect to climate risks, the authors should make more transparent + discuss in the context of the literature that (and why) they are here opting for a less conservative model prior of growth effects.

We disagree with the characterization that our approach is inherently less conservative than other approaches in the literature, including the referenced first differencing approach, or other approaches that estimate level effects. First, as noted above, there is no inherent reason that including more lags makes results more negative – it does so only if the lagged terms indeed suggest that temperature in one year can negatively impact GDP in future years. Second, our chosen approach does not generate more negative impacts than other recent work. Specifically, we calculate that the recent Kotz et al 2024 paper that uses a first differencing approach finds nearly identical impacts of global climate change by 2100 under RCP2.6 (-20%) than what we get using a five-lag growth model. See the below global damage estimates for warming scenarios up through RCP7.0 from our model; these results are not in the current manuscript (as global damages are not a focus of loss and damage discussions) and were calculated for a separate conference paper. While we have not run damages under RCP8.5, we believe that our model would generate substantially smaller damages than the Kotz et al result of -60% global damages at RCP8.5. Our SCC estimates are also nearly identical to a recent analysis by Bilal and Kanzig (2024 NBER working paper), who estimate a levels model and calculate a SCC of $1056 using a 2% discount rate; our comparable estimate under the same discounting assumptions is $1012 . Finally, our estimated SCC values are smaller than recent "bottom-up" estimates that use sector-specific econometric damage functions and then use equity weights to account for differential welfare effects in low income countries (Nath et al, "The Welfare Economics of a Data Driven Social Cost of Carbon", 2022, working paper). So there is nothing about our approach that is inherently more or less conservative than other approaches.



We have now added sentences to the Discussion to make these comparisons more explicit:

"Nevertheless, our estimates of the SC-CO$_2$, the quantity most easily comparable to other studies, are roughly 5x larger than recent estimates using ``bottom-up'' cross-sectoral approaches to damage estimation (Rennert et al 2022, EPA 2022), although they are very similar to a recent global econometric estimate (Bilal and Kanzig 2024) as well as to bottom-up estimates that use equity weights to account for differential welfare effects in low income countries (Nath et al 2022)."

Smaller points:
Thanks a lot for streamlining the methods – it's now much easier to follow. I'm still a bit confused by the statement that the baseline is what has been observed in the past (eq. 2, l. 482)– shouldn't it be the other way around? What has been observed in the past is our world in which this emission pulse has actually occurred and that should be compared to a counterfactual world without that additional emission pulse? I guess in practice it doesn't really matter (also following the authors' argument on the not-so-marginal historical emissions of some countries) and like that it's more consistent with the SCC framework but a short clarification would be helpful + appreciated.

Thanks – yes in practice since these perturbations are very small, the damages from adding a ton are equivalent to the avoided damages from reducing a ton. Because we have decided to represent damages using positive values, running the emissions perturbations by adding a marginal ton to us makes the most sense. As suggested by the reviewer, we now note this choice and equivalence in the Methods text:

"We assume here that an additional ton of emissions is being added to a baseline emissions trajectory, and the goal is to estimate the damages from this additional ton; an approach that instead subtracted a ton of emissions and calculated the avoided damages from this reduction would yield an equivalent estimate to our approach but of opposite sign."

l. 199: the authors state that the SC-CO2 is US$ 2379/ton - where can I find that in Fig 2? I guess it is US $2120/ton? What number is correct?

Yes, good catch! We had failed to update that number in the text - $2120 is the right value, which we have now updated in the revised manuscript.

The explanation that higher discount rates imply that the value of past damages is larger at the time of settlement compared to when they occurred is detailed at least 3x in the manuscript – consider shortening

l. 396-397: "to the extent that these channels are quantitatively important ...": compare refs. (3, 6).

Thanks. This section has been reworked using the proposed language of Referee 2. That entire section of the paragraph now reads:

"Our quantitative estimates capture an important aggregate channel -- GDP -- through which climate damages have occurred, and likely subsume a large class of microeconomic channels through which warming can have negative economic impacts, including changes in agricultural productivity, labor productivity, and energy use. Other channels that are poorly captured in GDP data (e.g. damages to physical and mental health\cite{carleton2022valuing} or various non-economic impacts such as ecosystem function \cite{bastien2021use}, human displacement, and loss of cultural heritage or homeland\cite{tschakert2019one}) or impacts that are not highly correlated with interannual variation in country level temperatures (e.g. sea level rise \cite{depsky2023dscim}, tropical cyclones\cite{frame2020economic}, or extreme rainfall events) will not be reflected in our current estimates. Similarly, our results do not fully capture the impact on GDP of variation in temperature and other climatic extremes, as these are imperfectly correlated with annual mean temperature and exert independent influence on damages (e.g. \cite{callahan2022globally,kotz2022effect}). As a result, our damage estimates will understate the total damages associated with historical and future emissions, perhaps substantially."

Check eq. (16), in particular the product operator ∏_(t=k)^t and what is initial year k?

Thanks -good catch again. "k" was our original notation for initial emissions year, which we have now replaced with t_0**.**

Fig ED3: add proper label to y-axis, recommended to use g (not y), as in eq. (19)

Thanks – fixed.

Additional SI section on robustness of local projections approach: some typos

Thanks – hopefully found all of them now.

Referee comments are in bold, and our responses our in plain text.

<u>Referee #1 (Remarks to the Author):</u>

I am satisfied with the authors' replies to my comments and those of the other two reviewers. I found the revised manuscript much improved and have no further comments.

Thank you!

<u>Referee #2 (Remarks to the Author):</u>

The authors have written a thorough response to all the review comments, as well as doing some new analysis (for example, calculating damages as a percentage of GDP in Figure 2). I am grateful to the authors for being so thorough, and I am satisfied with their responses to my comments regarding:

- Clarifying terminology to make it clearer to people from different fields
- Acknowledging related research in extreme event attribution
- Description of Loss and Damage policy and it's connection (or not) with compensation

They have also edited the text to make it clearer that the paper refers to temperature impacts on GDP only, and to highlight that there are other climate change impacts that are also damaging and relevant to L&D policy.

Excellent - we are glad that our revisions were helpful.

However, I still have some concerns with the current manuscript.

1. I am still struck by how sensitive the results are to different methods, which has also been highlighted by the other reviewers. Whilst the authors can be commended on how many different methodological choices they have tested, I do not think there is adequate emphasis in the text to make it clear to the reader that the estimates have massive uncertainty ranges. Figure ED12 has confidence

intervals which span an order of magnitude, but they are hidden away and would not be noticed by most readers.

I also think it is fundamental that this uncertainty is given adequate discussion (at a minimum, at least a large paragraph at the beginning of the discussion section, and ideally also highlighted in the abstract). The message the reader might get from main text is that the methods for quantifying L&D are almost there, and ready to be used for policy applications. But if the methods were to be applied, how should the user decide which methods and which part of the uncertainty range to use? And would the level of uncertainty lead to controversy in the application of the method? L&D is already controversial. If someone were to try to take Aramco to court for climate damages, would it be an issue that the estimated future damage has a range of $16T - $194T? Furthermore, I believe that this confidence interval is only based on the new method, and if the previous method were to be used there might be a different estimate again? Perhaps there are ways around applying the results given the methodological sensitivity, but it should as a minimum be discussed.

Thanks - this is a reasonable point. We have added a new paragraph (second paragraph in the revised Discussion) to address this important point. We distinguish three main sources of uncertainty; (1) "traditional" uncertainty arising from statistical uncertainty in the relationship between climate and economic outcomes combined with climate uncertainty in the climate response to GHG perturbations, (2) "model choice" uncertainty on how exactly to model the relationship between (e.g.) temperature and GDP or how to incorporate adaptation, and (3) "social choice" uncertainty which includes factors such as discounting, the horizon over which to estimate damages, and whether to attribute emissions using Scope 1 or Scope 3 emissions. All estimates of climate damages must grapple with these sources of uncertainty, and they are almost always large - causing results to differ in sign in some important cases (e.g. Carleton et al 2022, Rode et al 2021) although not in ours.

We again emphasize that a main contribution of our work is to provide a quantitative definition of loss and damage, and a framework for its estimation that is consistent with how the scientific community has decided to estimate related quantities, chiefly the social cost of carbon. We cannot resolve all of the remaining sources of uncertainty that accompany our empirical implementation of our framework, just as other published damage estimates cannot resolve them. Thus we opt to clearly identify where uncertainties are coming from, and which of these uncertainties are potentially resolvable by further scientific advances (the first two sources named above) and which are policy or societal choices not easily resolved by science (the last source). A legal or policy system quantifying L&D will have to grapple with these sources of uncertainty as best as we can currently constrain them scientifically.

The added Discussion paragraph reads:
"*As is common in most climate damage estimation, the precise magnitude of damages we estimate remains somewhat uncertain. Overall uncertainty stems from three main sources: what we might call "traditional" uncertainty, or the combined effect of econometric and climate model uncertainty, which is contained in our reported confidence intervals and quantitatively decomposed in Fig ED11; "model choice" uncertainty, including whether to model GDP impacts in growth or levels or how to incorporate potential adaptation; and "social choice" uncertainty, including choices over the discount rate, the period over which to aggregate damages, and which emissions to attribute to a given emitter. Accumulating data and further scientific advances hold promise for making progress on the first two sources. Our goal in this paper is not to resolve these scientific sources of uncertainty but instead to present a framework for L&D estimation and an empirical implementation of it that acknowledges currently unresolved uncertainties. It is possible that the scientific standards of evidence we adopt, including reported 95% confidence intervals, are more stringent than ``more likely than not" standards employed in many legal settings (Lloyd et al 2021). The final source of uncertainty, and the overall assignment of damages, will necessarily be guided by legal, ethical, and practical concerns that are not provided by our framework.*"

2. In the previous review I highlighted that the new analysis has shown that the USA was the country with the most L&D, which is interesting and something which warrants further discussion. The authors have responded to my comment in the response to reviewer document, and also edited Figure 2 so that the damages are presented as a proportion of GDP. However, they have not added more discussion. Figure 4 still shows the important finding that this method demonstrates the most damage in the USA, followed by EU, followed by China. This is noted in the text, but the implications of it are not discussed. Please add a paragraph to the discussion to highlight this and discuss the implications for applications. Does this imply that the USA should receive compensation for damages from climate change? For some people L&D is all about equity and justice – it feels like an oversight not to discuss this more.

Thanks, important point. Given the editor’s request that we reduce text size by half, we have added the following brief text to the discussion, lines 241-245:

“*Our results also highlight that while relative damages tend to be largest in low-income settings (Fig 2c,d), absolute damages are largest in the world's largest economies (Fig 4, Fig ED7), a result of smaller percentage damages acting on much larger economies. Calculated absolute damages again do not necessarily indicate what is ``owed" to these economies by other emitters, as this must be informed by ethical and legal frameworks beyond the empirical framework we use here*.”

3. The authors have diligently responded to my comments to make clearer that the paper is about the impact of mean temperature on GDP, and not all L&D from climate change. However, I still think that – reading this paper again, as a researcher who works on climate change impacts and L&D – it feels quite disconnected from the literature I am more familiar with. When I first came across the relationship between mean temperature and GDP (Figure ED2) I was shocked! I did not expect there to be a relationship between these two things! It’s very interesting, but I think quite surprising and unintuitive for many people who work on impacts and adaptation. As a climate scientist in this space I am constantly being told by colleagues who are experts in vulnerability and adaptation “average temperature does not impact people, we need to look at extreme events, or what matters on the ground”. I think these same people will see a paper on L&D in Nature and want to read it. To draw them in and help them connect with this work, I would suggest to add a sentence or two in the introduction which introduces and justifies the focus on mean temperature and GDP. Perhaps above the current line 143. I would suggest to highlight that there is a body of work that examines climate change impacts through the lens of mean temperature and GDP, that there is evidence for a strong interannual relationship there, and briefly outline some of the reasons why mean temperature impacts GDP.

Thanks, we have added the following language on line 90 (note shortened intro to meet editor’s word length requirements), emphasizing that annual temperature-GDP relationships are well studied, a large source of damages, but that they will not capture all sources of damage:

“Relationships between mean annual temperature and GDP have been well explored in the literature (Dell et al 2012, Burke et al 2015, Ricket et al 2018, Diffenbaugh et al 2019, Nath et al 2023, Bilal et al 2024), likely capture many channels through which a warming climate affects economic outcomes, and represent the largest source of monetized damage in many climate impact analyses (Ricke et al 2018, Bilal et al 2024, EPA 2022). The reduced-form temperature-GDP damage function we use is robust across statistical models, has not changed appreciably in the last 60 years (Fig ED2), and provides strong evidence that temperature is affecting the growth rate of GDP, not just the level (Fig ED3; Methods, SI Methods). Such damages are a subset of known and anticipated damages from climate change, and will not account for damages from sea level rise, from climate extremes uncorrelated with annual mean temperature, or from outcomes not captured in GDP.”

Minor comments

Line 19 – please add uncertainty range
Line 20 – please add uncertainty range

Line 24 – missing apostrophe? Should it be “temperature’s effect on the aggregate economy”?

Fixed, thank you. Note some of these sentences have been reduced in length to meet the editor’s word requirements.

Line 168 – suggest to remove “unmodeled” - not sure what “unmodeled climate extremes” are?

Thanks - we have changed this to “....and will not account for damages from sea level rise or from climate extremes uncorrelated with annual mean temperature”.

Line 250-255 – please add uncertainty ranges into text
Line 257, Line 258, Line 279 - please add uncertainty range into text

Done, thanks.

Line 374 – “limited evidence that adaptation has been happening in the aggregate in recent decades” - I assume this refers to limited evidence that there has been adaptation in terms of GDP growth to mean temperature? This may also be because in impacts and adaptation work mean temperature is often viewed as

something that doesn't really have an impact on people, and therefore adaptation efforts focus more on other things, for example early warning systems to reduce impacts from extreme weather, and infrastructure to cope with sea level rise. For example, Indian cities have implemented heat action plans, which may not have benefits for GDP but – if effective – could have huge benefits for public health. I would suggest to nuance your statement – it is misleading to suggest that adaptation is unsuccessful by only looking at mean temperature and GDP, and undermines the efforts ongoing to try to adapt to other climate change impacts.

Thanks. Our read of the quantitative literature is that many adaptation actions are being tried (Berrang-Ford et al 2021), as the referee notes, but few are being systematically evaluated in terms of their proven ability to reduce damages (again, Berrang-Ford et al 2021), and there is limited systematic evidence that we have thus far seen large scale adaptation in a way that has broadly lessened societal damages (Burke et al 2024). As a result, L&D – which is damages net of adaptation -- remains important.

Given word constraints, we have shortened that section to now read:

"Investments in adaptation could also limit future damages, yet quantitative evidence identifying specific investments that reduce risk and limit damages at scale is limited (Carleton et al 2016, Berrang-Ford et al 2021), as is evidence that society has been broadly successful in reducing damages in recent decades (Burke et al 2024) (Fig ED2b, Fig ED3d). Adaptation funding has lagged both promises and needs (UNEP 2024), and if L&D payments are tied to damages net of adaptation, as we propose, countries may under-invest in adaptation to maximize compensation --- highlighting the need for incentive-compatible system design."

Referee #3 (Remarks to the Author):

This is a great paper that formalizes and illustrates the straight-forward yet very helpful proposition to use the Social Cost of Carbon framework to help guide loss & damage funds and climate change litigation.

Thanks for the kind words.

There is one major issue in the computation of the specific damages and hence

numbers the authors present: They argue that a one-time temperature shock is not recovered but leads to permanently lower economic output. That is, the growth rate reduction in that year is not compensated by more growth in one of the following years. In Fig ED3 they show that for their historical data, this is the case for at least 10-15 years as the cumulative marginal effects are then still different from zero. They then assume (and yes, this is an assumption), that it will also not be recovered after 20, 50, 80 years and apply this to the future (up until 2300 for some projections). What this implies is that a permanent temperature increase (as can be expected under future warming) will lead to permanently reduced growth rates. If the local temperature increase is big enough, growth rates can be permanently negative and economic output in those places will become zero at some point (a 100% reduction in GDP as can be seen in Fig 4 in Burke, Hsiang & Miguel (2015), abstracting from discounting here). Countries will not be able to recover from this anymore, unless temperatures go down again. This is a fairly big assumption to be made for decades into the future - even if such effects can be identified in historical data for 10-15year periods. In the last two rounds of revisions, I have hence asked the authors to show more support for this assumption and make its implications more transparent. I appreciate that they now show results for 5 and 10lags, respectively, as well as for a local projections approach, incl. robustness checks. However, the same shortcoming applies here: since you can only include a certain number of years x, you cannot know what will happen after these x years.
Ideally one would thus include as many lags as possible (I completely agree with the authors that starting with a large number of lags is important for model selection) but estimates become noisier, the more lags are included, rendering the discrimination between growth and level effects inherently challenging.
A more conservative approach (and by conservative, I mean less strong assumptions about the future, not lower damages per se) which is increasingly applied in the literature is thus to assume that future damages persist for as long as could be robustly identified in the historical data but no longer than that.
The authors should make the implications of their growth effects assumption transparent, ideally show a range of outcomes under different persistence assumptions.

Thanks - this is a very reasonable concern. We have now run additional simulations where we estimate results under alternate persistence assumptions. Specifically, given that we find that growth effects appear to stabilize after 5 years and remain constant through at least year 10 (Figs ED3, ED4), we estimate models that subsequently allow recovery to trend over the next 5, 10, or 20 years. Thus, the effect of a 1C increase has a negative cumulative growth effect through year 10 that then returns to zero after 15, 20, or 30 years. We note that these are *much* faster return intervals than what is implied by Nath et al 2023, who implicitly estimate that it takes growth about 50 years to return to pre-shock “frontier” levels following a shock, and who also cannot rule out permanent growth effects. As with the earlier adaptation experiments suggested by the referee, we focus on measuring effects on the SCC, an easily comparable quantity. Experiments are described in more detail in Methods on line 792 and following. This section now reads:

“*Our second scenario relates to the debate over whether temperature increases have permanent effects on economic growth (``growth effects"), or whether initial negative effects on growth are compensated for by accelerated growth in later periods such that there is no cumulative effect on growth (``level effects"). Our historical data indicate that no such acceleration is apparent for 10-15 years after a temperature increase (Fig ED3), but estimates become increasingly noisy as additional lags are added, and it remains possible that recovery could occur in subsequent years. To understand the impact of such recovery on our estimated damages, we estimate models that allow cumulative growth impacts of a given year of warming to return to zero after either 5, 10, or 20 years. Specifically, we assume that negative impacts on growth stabilize after 5 years and are constant through year 10 (consistent with our data), and then recover linearly over the following 5, 10, or 20 years. In the simplest linear model of growth on temperature $y_t = \sum_{t-5}^t \beta_t T_it$, we would estimate cumulative marginal effects $\hat{\theta} = \sum_{t-5}^t \hat{\beta_t}$, and then starting 10 years after a shock, increase growth by $-\hat{\theta}/n$ for n recovery years; cumulative growth effects equal zero 10+n years after the temperature increase. Our actual implementation uses the quadratic growth response function, such that marginal effects are calculated separately for each country depending on their average temperature*.”

Results are shown in an updated Fig ED10, where we are using the scenario where impacts are only estimated through 2100. At a 2% discount rate, we find that the 5-, 10-, and 20-year rebound SCCs are respectively 70%, 65%, and 55% smaller than the SCC that assumes “full” (no-rebound) growth effects. These results are comparable to the Nath et al finding that allowing for “medium run” but not “permanent” growth effects reduces global impacts by about half.

These results are now described on line 135 in the main text:

“Similarly, allowing for growth rebounds following initial temperature-related declines, such that cumulative growth effects are eliminated, reduces SCC estimates by half; however, such a scenario finds mixed support in existing literature (Nath et al 2023), and rebounds are not apparent at least 15 years after an initial shock in our data (Fig ED3).”

Apart from this specific but crucial aspect, this is a great paper and I will be happy to see it published. As the authors mention themselves, the general framework does not depend on one specific damage function but can be used to compute ranges of possible outcomes under different damage functions and assumptions.

Thanks for the kind words and for all the helpful comments and feedback.

Comments from the Editor:

Regarding the content, after consulting with our legal department, although we do not see specific risk upon publication under the US law, we require you to acknowledge (if relevant) in the text that some of these parties (named companies or individuals) may have paid carbon offsets.

Thanks. We have added the following sentence to the first paragraph of the discussion:

Our estimates do not account for the existence or efficacy of any emissions-offsetting behavior on the part of emitting entities.

***Regarding the format, please pay particular attention to the following points.**
1. They need to shorten the main text to 3,500 words and Methods to 3,000 words. Remaining content needs to move to SI.

The main text is now 3395 words by our count. For the Methods, we have opted to move the entire methods section to Supplemental Methods. We found it exceedingly awkward to have what is essentially a short methods summary in the main text, a medium methods description in Methods, and then a detailed description in Supplemental Methods. So, we took the less common but hopefully allowable step to move all methods to Supplemental, cited appropriately in the main text.

2. Remove second layer of subheadings and avoid using generic headings such as Discussion.

We have stripped most sub-headers. We note however that many Nature papers retain a "Discussion" header, and others retain many sub-headers. We have thus retained the "Discussion" header, avoiding an awkward narrative transition, but have omitted the rest.

3. Keep the number of ED figures up to 10 and move the remaining 9 to SI.

Done.

4. Break the reference into main text references (up to 50) and methods-related references.

Done.

5. They need to provide an Acknowledgement, ACS, COI, DAS and CAS.

Done.

6. Reorder the section. The order in which your text file (without embedded figures) should be presented is as follows: title, authors and affiliations, main text, references from main text, main figure legends, online Methods section, Data and Code Availability statements, references only cited in the online Methods section, acknowledgments and author contribution statements, main text Table, and then the Extended Data Figure legends.

Done.

7. Remove the main figures from the article file and re-supply them individually in an acceptable format such as EPS, AI, PS, PDF, PPT, CDR, PSD or XLS (for graphs). There should be only one file per figure containing all the panels. Main-text figure legends should be placed after main-text references and before Methods section.

Done.

8. Avoid using rainbow color in figures.

Thanks. We assume this was in reference to Figure 2a,b, which we have now given a continuous, non-rainbow color scale.

9. Provide map permissions wherever map is used.

Done. This was added to acknowledgments, following what Hultgren et al did (we used same source)

10. move the underlying data and code to DOI-minting public repository (such as Zenodo or similar, please see https://www.nature.com/sdata/policies/repositories#envgeo for a list of some recommended repositories) and provide the access link in Data availability statement.

We have opted to use Github as we have with previous Nature papers, but can move to Zenodo if there is a strong preference.

11. Rename ED items as Extended Data Figure 1,2,3.. and SI Figures/Tables as Supplementary Figure/Table 1,2,3..Make sure they are cited in the main text. Extended Data figures, tables (separately) should be re-supplied in either JPEG, EPS or TIF format.

Done.

12. Provide SI Guide.

Done.

<u>Referee #2 (Remarks to the Author):</u>

The authors have responded to all my comments, and the revised manuscript reads well. I just have one final minor comment.

Line 222 – remove “somewhat” – as this is an understatement

Removed - thanks.

<u>Referee #3 (Remarks to the Author):</u>

Thank you for exploring how alternate persistence assumptions affect your estimates.

Since the damage function is so key to your analysis but not shown explicitly, it will be helpful if you add a respective figure (e.g., as in Fig 5a in BHM2015), for better comparability with other damage functions.

Other than that, I have no further comments.

Thanks - this is now added as Figure S11 in the supplemental, and discussed briefly in the text.

The authors have responded to all of the reviewer comments, including doing new analysis.

The new set of figures has some very large differences to the original manuscript. This has not been adequately highlighted in the response to reviewers, and I confess that I only noticed as I was about to submit my review. In Figure 2 and Figure 4, the largest estimated impacts (by quite a margin) are in the USA. I think the authors need to explain why this is, and what this means about their methods: are they very sensitive to methodological choices? Do the estimated damages by country differ dramatically based on different methodological choices? This has important implications if the framework were to be used in policy. Equally, they should comment on the implications of the results – if the largest losses and damages are in the USA, what does this mean for L&D as an approach to address climate change impacts in vulnerable countries? Does this imply that the USA is the most vulnerable to climate change? And what about the countries that have been campaigning for L&D since the 1990s? (i.e. small island states).

Clearly, a framework which estimates L&D based on GDP will always estimate larger damages in countries which have large GDP and relatively small damages in countries that have low GDP to begin with. So is this the best approach to support low emitting countries suffering impacts from extreme weather events and sea level rise which are exacerbated by emissions from countries with high GDP? This relates to the comments in my previous review - that the paper does not sufficiently emphasise that this is only a partial estimate of L&D.

The three main issues I had with the paper originally have also not been fully addressed:

1. The paper still minimizes the importance of (a) hazards which are not related to mean temperature and (b) impacts which are not related to GDP. (a) and (b) are arguably the majority climate change impacts which matter for people. These are also the impacts that are more often referred to in discussions around Loss and Damage.

2. In places, the wording and emphasis in the paper is still quite difficult to understand and put in context for a reader a different academic field.

3. The paper makes only passing references to extreme event attribution research, which is another approach that has been discussed in the context of loss and damage negotiations. In fact, there has been quite a lot of literature, over the past 10 years or more, which has discussed extreme event attribution and the challenges and opportunities for using it in the context of L&D policy. Some of the same issues are coming up here, for example: the challenge of developing a scientific approach when the policy is vague, the challenges of describing an approach to compensation, when compensation is a word that some negotiators are not even allowed to say out loud, and the challenges of developing scientific evidence for supporting those impacted by climate change, when those countries are also those for whom the strength of scientific evidence is likely to be weakest due to data limitations. I did not fully elaborate on this point in my previous review, as I thought it might be the case that the authors were aware of this work, but needed to add a bit more comment and citation. On reading the response to reviewers and looking again at the paper, in particular the new additions at the end of the discussion, I think maybe the authors are not aware of this literature, and therefore I have suggested some references that are important to help frame the paper (Hulme et al. 2011, Huggel et al. 2016, James et al. 2019, King et al. 2023).

These three issues make it difficult to connect the work with the existing wealth of literature on climate change impacts and adaptation, and growing literature on loss and damage. Given the broad readership of *Nature* I think further changes are needed to help the reader understand the implications and scope of the study. Furthermore, from a policy perspective it is important that the reader can understand that the framework proposed cannot address many (or even most?) climate change impacts. If the framework from the paper were picked up and used to design the distribution of L&D funding, it would overlook important impacts, including the impacts of sea level rise on small island states, impacts for the most vulnerable, and loss and damage following specific disasters.

I have made some specific suggestions to help address these issues.

Suggested corrections and comments

Title: "Quantifying climate change loss and damage consistent with a social cost of greenhouse gases" – I don't think that "the social cost of greenhouse gases" will be very meaningful to people outside of economics. I also think "social" is a strange word to use to describe impacts on GDP, since the paper does not deal with other impacts which will have much more "social" impact e.g. loss of housing from flooding, loss of homeland from sea level rise, loss of livelihood from drought, loss of cultural heritage, damage to mental health from extreme weather events… I suggest a modification to the title: "Quantifying climate change loss and damage associated with the impact of average temperature on economic growth"

Abstract – the authors have made several important changes to the abstract – noting that they are only referring to "one component of L&D" and also adding confidence intervals around the estimates. This is much appreciated. I think that, with the suggested change to the title, this is sufficient to make clear that the paper is focusing on average temperature and aggregated economic output. However, if the title is not changed, I would like to see additional wording added to the abstract, something like "It is important to note that many climate change hazards (e.g. sea level rise, flooding, drought) are not related to average temperature, and many losses and damages are not related to aggregated economic impacts. Therefore our estimates of L&D are likely to be a substantial underestimate of all L&D from climate change."

Line 34 – 39 – the authors have added more recognition of the work in extreme event attribution, which is much appreciated. However, these sentences imply that extreme event attribution research only relates emissions and impacts. In fact there has been some work in extreme event attribution which links emissions to specific countries or companies. There is a summary of this in a book chapter James et al. (2019), section 5.3.3. I suggest citing a couple more papers, and rewording to acknowledge this (suggested change in italics).

"However, unlike the large body of empirical work which explores how global climate change will affect global and local economic outcomes, the question of whether and how emissions by specific entities (e.g. countries, companies, or individuals) can be linked to specific damages has received less formal and empirical attention *in economics* - although substantial headway has been made in understanding how the probability of specific extreme events and their impacts can be linked to a changing climate[13-17]."

Reviewing this paper has made me reflect that it would be fantastic for there to be more collaboration between the econometrics community and the extreme event attribution community, and it would be fantastic to get an economics perspective on research in extreme event attribution that has estimated monetary losses from extreme weather events.

Line 46-48 – Suggest to edit (edit in italics):

"Drawing from multiple fields, *and focusing on the impacts of average temperature on aggregate economic output,* we show how…"

Line 51-56 – The authors have rephrased their comments about L&D with respect to the Paris Agreement. I appreciate the effort to navigate a difficult political topic. However, this reasoning is a little hard to follow – what is the distinction between stating that L&D should reimburse damages from those who have emitted, and stating that this is not necessarily what is "owed"? Is the idea to suggest a possible approach to L&D, but then to say that this approach is not necessarily morally correct? The next paragraph then goes on to talk about liability, which is explicitly excluded in the Paris Agreement. So I don't think it is possible to say that the proposed approach is consistent with the Paris Agreement.

The "completely reimburse" and reference to "non-economic impacts" in this paragraph are also a bit confusing – since the paper does not propose a framework to deal with non-economic impacts, and – as the authors note – it is "partial" so it does not provide a framework for how to completely reimburse loss and damage.

One suggestion for how to address this: the authors could (a) acknowledge L&D is contentious, that the Paris Agreement explicitly states that L&D does not refer to liability or compensation, (b) note that there are some who nevertheless view L&D as a route towards compensation, and that there are important discussions about compensation and reparations outside of the UNFCCC framework, for example with reference to legal cases, (c) therefore note the potential value of academic analyses to conceptualise how L&D can be related to specific emitters, (d) highlight the authors own suggested approach to conceptualise L&D, (e) highlight that their framework shows how to compute *part of* this L&D.

Line 69 - could you explain a little more what is meant by marginal? I note there is lots of discussion with the other reviewers about marginal emissions/costs/effects, but I think for a wider readership it is not obvious what is being referred to. Please also check other references to marginal later in the text.

Line 79 – The authors have added more information about the SCC which is much appreciated. What is meant by "nests"? Please clarify.

Line 98-99 – suggested edit (edit in italics):

"Our implementation allows estimation of past and future economic damages *associated with average temperature change* from any observed or projected emissions perturbation"

Line 142-143 – "individual populations" – does this mean country populations? Suggest to rephrase as "individual countries" or "country populations"

"damages" – is this damages to GDP? Suggest to rephrase "cumulative damages to GDP"

Line 148 – what is meant by local warming? Is it country level? If so suggest to rephrase e.g. "country-level warming"

Is "damages" damages to GDP? Suggest to rephrase "damages to GDP"

Line 153-157 - this is clearer, thank you. Suggest to add “country-level” before warming, line 154. Suggest adding “(including from sea level rise and extreme weather events)” after “warming climate” (line 156). Suggest adding “(such as loss of life, damage to mental health, loss of livelihood, loss of homeland)” after “non-economic damages” (line 156).

Line 209-212 – It’s interesting that in this part of the text and in the response to the reviewers, the authors’ emphasis is on rates of past adaptation as a predictor of rates of future adaptation. What about policy and public awareness? Although it arguably hasn’t been that successful yet, recent decades have seen a huge growth in initiatives around climate change adaptation. Do the authors assume at all the effort, funding, research that is being invested in deliberate adaptation by countries, companies, farmers, households and beyond will not work? If so, suggest to briefly note why this is. If not, suggest to rephrase the sentence “it is of course possible that future adaptation could exceed current adaptation” so that it is does not imply that future adaptation is unlikely.

It would also be useful to compare the findings of L&D with and without adaptation with other research to estimate the benefits of adaptation e.g. Faldauer et al. (2022).

Line 221 – Suggest to edit “L&D” to “L&D to GDP from average warming”

Line 222-224 – This part is a bit tricky to follow, could a little more explanation be added?

Line 230-291 - How uncertain are these estimates? Is it possible to add uncertainty ranges?

Line 300 – “simple scenario” – would “hypothetical scenario” be more appropriate?

Line 319 – suggested edit (edit in italics) – “develop a framework *for estimation of L&D to GDP from average warming*”. It is misleading to say that the framework is developed for the estimation of L&D in general, since it does not cover all types of L&D.

Line 354-358 – one of the challenges in adapting to date, has been that parties to the UNFCCC have failed to deliver on the adaptation funding they’ve promised. For similar reasons, it might be a struggle to generate L&D funding. This might be a further challenge worth discussing.

Line 391-393 - suggest to add more examples to help the reader put the work in context. For poorly captured in GDP data examples could also include loss of income by farmers (where crop failure due to high temperatures might increase prices and therefore lead to increases in GDP or no change in GDP whilst farmers experience loss and damage). For non-economic impacts, examples could include damage to physical and mental health. For impacts not highly correlated to country-level temperatures, examples could include tropical cyclones, heavy rainfall events, ocean acidification, marine heat waves.

Line 396 – “To the extent that these channels are quantitatively important” – this seems to imply that the quantitative importance of climate change impacts beyond mean temperature is in question? This seems misleading. Frame et al. (2019) estimate that at least $30bn of damage from Hurricane Harvey can be attributed to climate change. Of course, this is just one extreme weather event. Otto et al. (2020) describe how estimates from many extreme weather events could be collated to develop an inventory of impacts from anthropogenic climate change. Frame et al. (2018) demonstrate a similar approach for New Zealand, estimating that climate change related floods and droughts cost New Zealand >$800m over a recent 10 year period. This is without mentioning the economic implications of sea level rise. Perhaps the sentence could be rephrased:

"Given the substantial economic damages associated with extreme weather events, some of which can be attributed to climate change (Frame et al. 2018, 2019), the impactful nature of sea level rise, and the importance of non-economic loss and damage (Tschakert et al. 2019), our damage estimates understate the total damages associated with historical emissions, *likely* substantially."

Line 400-403 – Could the authors expand a bit on which cases this is more likely to be applicable for? For example, many non-economic impacts (such as damage to mental health) are less well documented, more difficult to predict, and difficult to quantify.

Line 404-411 – It is important to note that these criteria are most often missing in places that are highly vulnerable to climate change, as has been discussed with reference to extreme attribution research, with many researchers expressing caution at the use of science for policy for this reason. See Hulme et al. 2011, Huggel et al. 2016, James et al. 2019, King et al. 2023).

Line 457 – "Our focus on average temperature helps overcome concerns regarding the use of extreme event attribution in L&D assessments" – perhaps, but it raises new concerns, because the hazards and impacts that many L&D campaigners care about are ignored. The focus on GDP will also lead to additional concerns because the largest damage estimates for the richest countries.

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