AGU Advances

Authors' Response to Peer Review Comments on

New potential to reduce uncertainty in regional climate projections by combining physical and socio-economic constraints

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Author Response to Peer Review Comments on 2023AV000887

[Author Response begins on the next page.]

We thank the reviewers for their helpful feedback. Especially reviewer #1 makes excellent points. We respond to each point in blue below. In light of the many changes made to the manuscript, we do not list them all here, but they can be found in the track-changes manuscript.

Generally, we emphasize that this is a perspective paper intended to rally the community to accelerate integration of socio-economic and physical constraints on climate projections where possible. We cannot, in the scope of this paper, resolve all the reviewer concerns quantitatively, as some pertain to currently unquantifiable uncertainties – though substantial efforts were made in the revised version. We also emphasize that our estimate of the initial total uncertainty is based on what is contained in the CMIP6 archive, which forms the basis of most if not all current climate adaptation science. Additional uncertainties likely exist and should be sampled in a more structured way, though we argue that the current state of the science nonetheless allows the development of some potentially useful constraints.

We strongly revised the manuscript to provide a more balanced assessment of the current opportunities and limitations and hope that the reviewers see value rather than risk in communicating this perspective.

On behalf of all authors,
Flavio Lehner

Reviewer #1

This manuscript combines two types of constraints, physical climate and socio-economic, and estimates their effect on the uncertainty of regional climate projections. It provides an update and important extension of earlier work describing the relative contributions of scenario, response and internal variability uncertainty to projections of future climate change. Conceptually the study is interesting, but limitations in the input assumptions (and the socio-economic input assumptions in particular) means that the high-level findings and messaging of the paper are not as robust as the text is suggesting. This runs the risk of misleading readers by indicating an overconfidently narrow range of future climate impacts.

This risk should be fully mitigated by revising the manuscript to accurately reflect the limitations in socio-economic assumptions that are being used. Unfortunately, these limitations most likely mean that the overarching finding of reduced projection uncertainty at the regional scale cannot be supported anymore.

There are key limitations in the socio-economic assumptions from Moore et al (2022; M22 from here on) that bias the results towards over-confident uncertainty ranges:

1) The model applied by M22 does not allow for net-negative CO_2 emissions. This is an arbitrary modelling assumption when considering recent real-world and scenario evidence (https://www.stateofcdr.org/). Given that (mainly land-based) carbon-dioxide removal (CDR) measures are currently delivering removal at a rate of 2 $GtCO_2$/yr this assumption is overly conservative and clearly reduces present and future possibilities at the lower end of the scenario space.
This is a good point and we do not see an issue with leaving the door open for substantial CDR by 2050 and beyond. We therefore revise our weighting scheme to leave the lowest emissions

scenario (SSP1-1.9) untouched. The constraint on response uncertainty still means that the lower bound of temperature projections increases a bit, but by less than before.

2) The M22 model also does not consider the evolution or reform of political institutions, neither in a positive nor a negative direction. This assumption means that the findings of M22 are perfectly in line with what one would expect from the SSP-RCP framework, when excluding institutional change. Indeed, the M22 model is calibrated on historical data and performing well in hindcasts in developed countries. That means that it is fundamentally modelling a world that reflects and extension of historical societal dynamics, or in other words, SSP2. The resulting scenarios range from M22 is fully consistent with this assumption: the original SSP2 baseline ends up at around 7 W/m2 and at the lower end struggles to reach a 2.6 W/m2 level - which is to be expected when CDR is excluded. It is very doubtful that the future scenario space is indeed accurately covered and constrained by such assumptions on political institutions, as institutional capacity can both improve or strongly deteriorate (in case of an outright war) over the coming decades.
It is not our goal to advocate for or defend the M22 model in this perspective paper, though we note that the M22 model contains feedbacks that effectively create changes in political institutions, via the political economy feedback that changes the likelihood of climate policy moving in one direction or the other. In the end, our constrained range in 2100 is still wider (1.3°C to 4°C) than what is expected to result from *already implemented policies* (2.2°C to 3.5°C; IPCC 2022). Further, assigning lower probabilities to high emissions scenarios is supported by a growing body of literature and not contingent on M22.

However, we generally agree with the reviewer's assessment and admit that we are not in a position to *exclude* any scenario from being plausible. Yet, this does not mean that all scenarios are equally likely. The IPCC has historically simply not assigned probabilities to scenarios while the climate community took this to give equal weight to each scenario, from Hawkins&Sutton until today. This equal-weight interpretation was probably never well-justified and the last decade of socio-economic development has further affected the likelihood of certain scenarios. Ignoring that information seems odd. Here, we basically just echo other literature suggesting that high emissions scenarios can be downweighed *if* the risk tolerance of a practitioner allows it. This framing is not much different from what engineers do: infrastructure is built to withstand certain return levels, not all return levels. If a practitioner does not want to expose themselves to tail risk, they don't need to, something we acknowledged in the original submission, but try to stress more clearly in the revised version. We also substituted "down-weighting" for "excluding" to better reflect our interpretation of the current scenario literature.

The importance of the influence of two above-mentioned points on emissions projections is highlighted explicitly in M22 which says that "the future cost and effectiveness of mitigation technologies, and the responsiveness of political institutions emerge as important in explaining variation in emissions pathways and therefore the constraints on warming over the twenty-first century".
Again, we advocate not for excluding high emissions scenarios, but for downweighing them. As physical climate scientists, we support running high emissions scenarios for their signal-to-noise ratio. On the possibility of an outright war upending currently implemented policies: by the same token, we could also have a series of large explosive volcanic eruptions, changing the climate dramatically for decades. This is plausible but its probability is unknown – and it is not sampled in CMIP. However, the scientific community is ready to provide estimates of the impact of such an event if practitioners are concerned about it. The same applies to high emissions scenarios,

which we now clarify should continue to be included in the portfolio of future simulations.

There are two additional limitations in the M22 model that cause uncertainty in global and regional projections to be narrower than what is expected based on our understanding of climate physics and potential variations in societal dynamics.

3) M22 covers non-CO2 greenhouse gas emissions/mitigation/forcing (and hence their implied warming) in a very rudimentary way which doesn't capture the potential variation or uncertainty. In particular, M22 uses a simple linear relationship between CO2 mitigation and non-CO2 reductions. The evidence provided in M22 (Extended Data Fig. 6) already shows the large potential variation in this relationship, which remains uncaptured by the analysis. In addition, it is clear from the scenario literature (e.g., DOI: 10.5194/gmd-13-5259-2020) that this relationship includes many more degrees of freedom and the M22 outcomes are therefore unnecessary constrained in this dimension. The same is therefore true for the analysis presented in the paper that is reviewed here.
We obtained the M22 model and varied the parameter that describes the relationship between CO_2 and Non-CO_2 emissions to sample a range of functional forms from M22's Extended Data Fig. 6. While it affects the lower bound of the resulting global temperature projections (introducing ~0.3K of uncertainty by 2100) due to relatively larger influence of non-CO_2 forcing in low scenarios, it did not affect the upper bound significantly since high emissions scenarios forcing tends to be dominated by CO_2. Since we now refrain from constraining the lower bound of the scenario uncertainty anyways, this overall does not meaningfully affect the scenario constraint.

However, we take this opportunity, together with other reviewer feedback, to advocate for emulators to help better sample the scenario-climate response space in the future.

4) Finally, M22 does not provide spatial patterns of short-lived climate forcers and variations thereof (and neither do the SSP-RCP scenarios in a very structured way). Local abundances of short-lived climate forcers, such as black carbon, organic carbon, aerosol precursors, can markedly impact local to regional warming, and affect climate extreme events. In addition, because of teleconnections, regional climate might be further affected by changes in the short-lived climate forcer loading in other regions, adding to the overall uncertainty. At the same time, real-world spatial patterns of short-lived climate forcer abundances are most likely prone to the most extensive variation currently not captured by scenarios. The current scenario literature underexplores this dimension as in most scenarios the entire world either follows a cleaner or remains on a high-pollutant path. Given the differentiation in regional development paths in the real world and the variations in local decisions/effectiveness about air-pollution control, this is likely to be an overly constrained assumption that is currently reflected in the available literature. The presented analysis does not reflect on this dimension, and I appreciate that its relative importance is hard to estimate in absence of a dedicated structured analysis exploring it.
We are aware of the potential local influence of aerosols and the associated uncertainties. While we have cited several papers to emphasize this caveat, we happily expand on this in the revised paper. It is worth noting that we constrain global temperature by scaling *all* the model-scenario combinations in the CMIP archive. In other words, we do not discard any model-scenario combination. Therefore, we sample all the regional aerosol influences available in the CMIP archive, although some get scaled more than others. Fig. 1c thus samples all available regional aerosol influences and reflects them in the bootstrapped PDFs. It is also worth noting that the SSP scenarios are already not completely globally homogenous. SSP3-7.0 has Eastern

Hemisphere regions increase emissions and Western Hemisphere regions decrease, while SSP1-1.9 has everybody decrease emissions. As far as observations, global SO_2 emissions from 2000 to 2019 appear to be declining quickly, almost tracking SSP1-1.9 (CEDS data: https://pure.iiasa.ac.at/id/eprint/18395/; see also https://twitter.com/Peters_Glen/status/1494220069861314562/photo/1 for a quick illustration). In other words, the world is currently reducing aerosol emissions relatively rapidly.

Further, we understand the reviewer's concern that the SSP database undersamples possible aerosol trajectories, even as CMIP6 samples a wider range of aerosol trajectories than CMIP5 did. As the reviewer is aware, this is not something we can address quantitatively with the currently available modeling structure, though new efforts are underway that should aid this, namely the RAMIP (Wilcox et al. 2022).

Nevertheless, it is illustrative to conduct a back-of-the-envelope calculation to estimate the potential expansion of uncertainty from spatially more heterogenous aerosol trajectories. Westervelt et al. (2020) provide estimates from three models, with different aerosol forcing sensitivities, in which present-day aerosol emissions are reduced to zero in individual regions (US, EU, China, India). This is useful because most regions are projected to see either flat or declining aerosol emissions compared to present-day, so present-day emissions are a reasonable and relatively "high" baseline (recall also above comment on current trends in SO_2 observations). It is also useful because some of these regions are among the most potent in exporting radiative forcing globally; the same emission changes in tropical regions have a much smaller global effect (Persad and Caldeira 2018). The warming in Central Europe resulting from individual regional emissions reductions ranges from 0 to 0.3K (up to 0.5K if emissions are reduced to zero in Europe itself, but arguably this scenario is already covered in more moderate fashion under SSP1-1.9). The CMIP6 5-95% range for 2050 Central Europe temperature changes in our paper is 3K (0.4-3.4K relative to ~today). Thus, the most extreme aerosol reduction case (+0.3K) expands this uncertainty by 10%. To speculate about the lower end of the temperature range, one could assume most regions clean up their air (SSP1-1.9), but individual regions continue to emit (SSP3-7.0). Persad and Caldeira (2018) approximated this scenario with one model in which all countries reside at their 1850 aerosol emissions level but individual regions emit year-2000 China emissions. For Central Europe, temperature changes of -0.2 to +0.3K ensue. Thus, this has the chance to expand the lower bound of uncertainty by another 7-10%. Of course, both of these scenarios are highly idealized and miss a potentially important feedback: a change in aerosol emissions can result from a change in fossil fuel emissions and thus greenhouse gases (unless it is exclusively achieved through filters, but this is not consistent with observations). Therefore, in reality, the effect illustrated by these idealized simulations would likely be somewhat muted by counteracting radiative forcing from greenhouse gases. Based on this, in the revision, we discuss a possible expansion of the regional projection uncertainty by 10-20%.

In summary, while we agree that uncertainty from spatially more heterogenous aerosol forcing is somewhat undersampled in SSPs, we do not think it completed erodes our ability to constrain future projections currently. However, such heterogeneous emissions scenarios would certainly complicate the development of constraints for individual regions and further research is needed to quantify this uncertainty more systematically. In particular, the effects on the hydrologic cycle are likely to be more non-linear than for temperature and thus regional projections constraints on variables such as precipitation will be more challenging. We added discussion to that end.

All in all, these four points suggest that the impact of the socio-economic uncertainty is likely underestimated by the analysis in the current manuscript. Part of this underestimation is due to limitations in the underlying M22 study, and particularly point (4) is an aspect that is also poorly covered in the scenario literature as a whole. I appreciate that this makes addressing them challenging. However, it is important that the key findings and messages of the paper are not based on estimates of socio-economic uncertainty that are not structurally underestimated to begin with.
We want to avoid structural underestimation of the uncertainty. In our revisions, we made an effort to address the reviewer's concerns, specifically:

1) We now include negative emissions scenarios, leaving the lower bound unconstrained
2) We note that our constrained range is still wider than what is expected to occur based on already implemented mitigation policies and summarized in the IPCC report, so our premise to assign lower probability to high emissions scenarios is not critically dependent on assumptions in M22
3) We sample additional relationships between CO2 and non-CO2 emissions not covered in M22 and find very similar results for the upper bound
4) We estimate the effect of hypothetical and spatially more heterogenous aerosol forcing based on existing literature and conclude that it has a small effect relative to the total uncertainty, but that it needs more research.

These revisions reduce the strength of our constraint, though not fundamentally. Ultimately, we hold that there is potential to constrain regional climate change projections through a combination of socio-economic and physical constraints. This perspective is but the beginning of trying to quantify this potential.

References

IPCC, 2022: Summary for Policymakers. Climate Change 2022: Mitigation of Climate Change. Contribution of Working Group III to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change, Cambridge University Press, Cambridge, UK and New York, NY, USA, p. 50.

Persad, G. G., and K. Caldeira, 2018: Divergent global-scale temperature effects from identical aerosols emitted in different regions. Nat Commun, **9**, 3289, https://doi.org/10.1038/s41467-018-05838-6.

Westervelt, D. M., and Coauthors, 2020: Local and remote mean and extreme temperature response to regional aerosol emissions reductions. Atmos. Chem. Phys., **20**, 3009–3027, https://doi.org/10.5194/acp-20-3009-2020.

Wilcox, L. J., and Coauthors, 2022: The Regional Aerosol Model Intercomparison Project (RAMIP). Geoscientific Model Development Discussions, https://doi.org/10.5194/gmd-2022-249.

Reviewer #2

I would like to believe the contention of this paper, that we can use a constrained range for planning. However, a stronger case (or a more elaborate description of the key assumptions) needs to be made. Regarding the 3 central aspects that would constrain future projections:

1. Narrower global temperature change. Since this derives from the comparison of simulated historical temperatures with observations and extensively covered in IPCC AR6, this seems justifiable
We agree.

2. Narrower regional range because they assert that there is strong correlation between regional and global temperature change. This contention seems to be based on a single paper, which I have not read. But is this universally true? For example, what about the central U.S. summer "warming hole"? This is outside (on the low end) of the range of model simulations and does not follow global temperature changes.
We use the constrained projections from one specific paper but cite several others that have reached the same conclusions using other methods. Importantly, the constraints are developed locally. If a particular region does not have a strong historical relationship (like the example the reviewer speculates about), this will not lead to a strong constraint. So, the reviewer's concern is inherently accounted for in the method.

3. Socio-economic feedback and rapid adoption of renewables. I have some concerns about the reliability of these contentions. Is the trend toward rapid adoption of renewables sustainable? There appears to be increasing opposition (due in part to NIMBYism and in part to environmental concerns) to the large renewable projects that seem essential to maintain the trend. Has the rapid adoption thus far merely taken advantage of "low hanging fruit" in terms of installations? Furthermore, the real problem of scaling up of storage capacity seems to be underappreciated. Can we mine enough of the key minerals for batteries (lithium, cobalt, etc.). Environmental concerns about opening old mines and starting new mines appears to be increasing and slowing that effort. Can complete electrification of the transportation sector really happen in our democratic society where there is likely to be fierce opposition to legislation mandating this, given the widely publicized limitations of electric vehicles (perhaps that would change with a charging network equivalent to the current gas stations network, but it brings up the issue of where that power is coming from given the growing opposition to large projects noted above). Nuclear power expansion appears to be off the table in some parts of our society. Has the socio-economic modeling taken into account these formidable barriers?
These are good questions that are however not in the purview of our study. We rely on current literature that suggests that high fossil fuel futures are less likely, which is our main scenario constraint. Our constrained scenario range leaves open the question of whether the pace of renewable deployment can be accelerated or not. It mostly just asserts that it is less likely that coal consumption will expand massively by the end of the century.

Generally, in developed countries, mining of new fossil fuel sources is also met with opposition, so permitting is unlikely to be solely a critical minerals issue. Further, permitting, including for nuclear, is currently mostly an issue in the US and EU and does not necessarily translate to other countries where the bulk of future emissions growth might occur. This leaves the door open for a relatively wide range of future emissions scenarios – as reflected by our constraint.

My summary opinion of this manuscript is that it at a minimum requires a list of caveats and/or more convincing explanations of the contentions. Perhaps the few cited papers address these, but the reader should not have to read these to accept the conclusions here.

We have expanded the list of caveats. Our intension was to write a short perspective paper, largely using peer-reviewed and published methods. We attempted to summarize the relevant papers concisely, so the reader does not have to read them. We hope that with the added text in the revised version the reader receives the necessary information while keeping the paper reasonably short.

Reviewer #3

Review of: "Strong reduction of uncertainty in regional climate projections from combining physical and socio-economic constraints"

This paper argues that recent projections of regional climate conditions are more accurate than past measurements, such as those reported in AR6, when using more predictable future socio-economic activities and climate system's responses to emissions. As an Economist, I recognize that there are many elements beyond my background and current expertise in this paper. Yet, I believe that the following three points deserve to be added or clarified for the readers:

- In the paper, socio-economic activities are understood as the driver of emissions and hence of changes in climate conditions. However, one could also argue that the impact of climate change on socio-economic activities is now better understood than in the past so we are experiencing a reduction in uncertainty in the measurement of these impacts and in the identification of appropriate mitigation or adaptation strategies. A recent example is Dall'erba et al. (2021) who highlights how the impact of a drought event transmits across all US states through an adjustment in the interstate trade flows of agricultural commodities. Their approach and results are in stark contrast with past measurements (e.g. Deschênes and Greenstone, 2007) which consider the local impact of a drought event on yield only.
We welcome this comment and added a sentence discussing it. While the modeling framework underlying the scenario constraints does take into account feedbacks between climate impacts and socio-economic activity, it does not do so at the spatial and sectoral granularity of the here cited studies, leaving room for adding further constraints based on reduced uncertainty in those subfields.

- Table 1 indicates that there is a larger reduction in uncertainty coming from the response component in the mid-term while the reduction is larger for the scenario component in the long run. Why is this the case? Can you, in addition, clarify if table 1 is at the global level and thus if we shall expect the changes in the relative share of each component to vary geographically?
This is because scenario uncertainty dominates projection uncertainty in the second half of the century because scenarios diverge strongly post-2050 (see Hawkins&Sutton 2009 and many papers since), so constraints on scenario uncertainty will have a relatively larger impact later in the century.

As for Table 1, it is for global temperature, as described in the caption and text. We added clarification that, indeed, the relative importance of constraints on response vs scenario uncertainty vary with region.

- Minor point: I would suggest reducing the circumference of the pie charts in figure 1b when constraints are added so as to show that the overall uncertainty decreases.

Thanks for the suggestion.

Dall'erba S., Chen Z., Nava N. (2021) Interstate Trade Will Mitigate the Negative Impact of Climate Change on Crop Profit, American Journal of Agricultural Economics, 103, 1720-1741.
Deschênes O., Greenstone M. (2007) The Economic Impacts of Climate Change: Evidence from Agricultural Output and Random Fluctuations in Weather, American Economic Review, 97, 354-85.