Response to Reviewers – IGCC 2025 Indicators Paper

We thank all reviewers for their careful, constructive, and highly positive reviews of our manuscript. We greatly appreciate the time and expertise invested in providing detailed feedback. The comments have helped us to improve clarity, strengthen the presentation, and refine the framing of several aspects of the work.

Below, we provide a point-by-point response to all reviewer comments.

Reply to John Dunne

1. *My only substantive comment is with respect to questioning the appropriateness of Table 14 and suggesting a more detailed integration into the main text if it is to be included. Otherwise, my comments are minor:*

Response:

Thank you. We have discussed this as a team and even though aspects of the table are not well integrated, we still think Table 14 is worthwhile to present. We have added more discussion around the table to improve the context and pointed forward to it in key sections, such as the EEI.

2. *Is the intention to reflect on the next paragraph speaking of threats to funding? If so, it should be moved or recast into the value of this update in providing a reference point for the ongoing AR7? In that line, are there any recommendations for changes/updates in methods for AR7?*

Response:

We have recast as useful for IPCC AR7 as suggested and not made so much of the threat to observations in the abstract.

3. *period missing*

Typo corrected.

4. *add “ongoing” before “provision”*

Typo corrected.

5. *It is not clear to me why "CO2 Emissions" is separated from "Emissions of Well-Mixed GHGs" as it seems to imply that CO2 is not "well-mixed" when I presume some other aspect of the difference was intended.*

Response:

The nesting of one box inside another is used to denote that the inner box is a subset of the outer box, not separate. For example, CO2 emissions are a subset of well-mixed greenhouse gases, and the rate of warming assessment is a subset of the broader warming attribution assessment. We need to separate out the CO2 emissions in this way to make clear that the RCB depends on on CO2 emissions, not all well-mixed greenhouse gas emissions. We have updated the figure caption to make this subsetting clearer.

6. *BLUE, OSCAR, and LUCE are not defined.*

Response:

We clarify in the main paper that these are bookkeeping models, and now provide an additional sentence of explanation on them in the supplement.

7. *I suggest adding something "that remain only X% of the GHG contribution even after these increases" to contextualize the implication of their having started from "low levels"*

Response:

We have followed your suggestion and used this wording

8. *What is the difference between "UNFCCC F-gases" which have increased and "ODS F-gases" which have declined? A list of UNFCCC F-gases is given in 2.1, but I couldn't find a similar list for ODS F-gases.*

Response:

We now also list the main species of the ODS F-gases (e.g. CFCs, HCFCs, Halons and other non-“UNFCCC” species) in section 2.1.

9. *Is there a difference between "ozone depleting gases" and ODS above? I thought they were the same thing.*

Response:

In this context they are the same thing - wording updated.

10. *Suggest changing “In the case of models, production of near-real time information relies…” to “Production of near-real time information in the model-based estimates relies”*

Response:

Your wording suggestion has been employed.

11. *The statement “though this assumption may need to be revisited in the future.” doesn't seem relevant in the results section but would be interesting fleshed out in the discussion*

Response:

This phrase has been deleted: "though this assumption may need to be revised in future", and the reference to this section moved further down. We didn't feel that an extended discussion of this issue would fit with the flow of the current discussion section, but this issue should certainly be revisited!

12. *“and again in the most recent years.” – Why?*

Response:

Explanation added: "because of the difficulty to capture in near real time the regional emission dynamics due in particular to the rapidly evolving local air pollution policies (Szopa et al. 2021)"

13. *“Future updates of CEDS are expected to include uncertainties (Hoesly et al., 2018).” Seems out of scope for the present work.*

Response:

Sentence deleted, though point remains - we have only one best estimate SLCF emissions time series, and the uncertainty in emissions is likely to be large.

14. *I think more explanation for the use of "considerable" is necessary here. A 3% annual increase from 2024-2025 may not seem like much on its own, but you should point out that it is much higher than the 1.8% annual increase one derives from 2019 to 2024.*

Response:

We have moved the sentence order to highlight the influence of biomass burning as the explainer of this single year helps to explain the changes reported better.

15. *missing period*

Response:

Typo corrected.

16. *extraneous period*

Response:

Typo corrected.

17. *I think “but following IPCC convention” should be preceded and followed by commas.*

Response:

Typo corrected.

18. *While I see why the means are not constrained outside the training period, I don't think this explanation works for why the year to year anomalies suddenly look so different between ERA and Berkeley after 2012 or so when they were previously almost always tracking.... is there an decadal latency in getting some of the data or perhaps less data?*

Response:

19. *remove "and" and "it"*

Response:

Deleted as suggested.

20. *I'm not sure what to think of this table. There are many points of emphasis I agree are important, but the presentation is oversimplified and lacks context on the degree to which the current situation is better or worse than the past (there seems a general implicit statement that things are getting worse) and if there are recommended paths forward. For example, the line on "biogeochemistry" in the ocean section seems out of scope since no biogeochemistry has been discussed in the manuscript thus far outside of residual CO2 concentrations in TCRE. Further, the note suggests that BGC Argo is making things worse (except for oxygen) and perhaps intimating that ending BGC Argo is a good thing... Is this intended as a comparison between BGC Argo and the high quality ship-based measurements used as calibration, noting the decrease in ship measurements over the last decade? The intent is not clear. I note that the risks to some observational constraints are noted in the abstract, but I didn't notice a dedicated reference to the risks throughout the manuscript. Alternatives should be explicit. I feel like these risks should be first highlighted explicitly and consistently throughout the main manuscript, perhaps as a final sentence of each relevant section with the title "Risks" for each data type. Alternatively, I wonder if this discussion would be more appropriate for an opinion piece on climate research in the context of WCRP than as a research paper as much or most of it is not discussed in the paper before this last paragraph when many of these points are objectively crises of observation deserving intense community discussion rather than anecdotal mention.*

Response:

Thanks for these comments. Note that there were similar comments from Matthew Jones. On reflection we agree with his suggestion to limit this table to just those ECVs which directly inform the indicators in preceding sections. We have also tried to improve the context and direct relevance, add a small number of pertinent references and improve the narrative basis for the inclusion of the table. The edits have indirectly addressed specific points raised here about ocean biogeochemistry as this ECV no longer appears. The tables

will form an integral part of the GCOS Status Report and inform directly a high priority action plan. The link to these activities has been made clearer and more explicit. We feel given the urgency of the moment that the reduced table more directly tied to the preceding analysis is justified to be retained.

Reply to Matthew Jones

1. *Congratulations to the authors on their latest updates to the Global Climate Change Indicators report, which I have reviewed twice in the past. I am again struck by the authoritative and comprehensive nature of the report, with this group providing a service to the climate sciences by reminding us each year of the vast and accumulating evidence for anthropogenic climate change. Yet the article also neatly summarises this vast information at various levels of complexity, all the way down to a useful table and figure in the discussion section. This is a highly commendable achievement.*

Response:

Thank you for taking the time to review it (again), it is no small ask.

2. *The latest update includes some new additional analyses and has generated new datasets, notably the marine heatwave days indicator. The value of updating existing indicators with new data is also clear to me; the community requires dataset such as these to be continuously updated, interpreted and validated.*

Response:

Thank you.

3. *The future integration of IGCC with the C3S / CDS is a great outcome for the climate science community.*

Response:

We agree.

4. *I did not find any major concerns with the methodologies being presented, especially as many of these are either updates with input data extensions or rather incremental changes. I cannot claim to be an expert across all areas and especially across sections 5, 6, however I found those sections to be well-written and accessible to an "informed non-expert" like myself.*

Response:

Thank you.

5. *One top-level comment: Perhaps methods could be cut down further across reporting cycles by citing the report(s) in which a methodology was introduced/updated, and highlighting only any changes to the methodology since those earlier reports. This is done to good effect in some sections, but I think not always. (Something for the future).*

Response:

We agree, are trying to do this more and more to make the paper more accessible and will take your comments on board for future editions.

6. *The EEI doubling point did not quite land with me – some added precision about what EEI quantifies would possibly make this a harder-hitting statement.*

Response:

Slightly reworded for clarity.

7. *"updated interpretation of the exceptional warmth observed during 2023-2025" feels like a broad description: what is the new indicator underlying this statement?*

Response:

This clause was out of place, thank you for pointing this out, it is now deleted.

8. *Throughout this section, "equivalence" units like CF4-eq are used. Seems intuitive to me, but perhaps for absolute clarity a statement could be made that it is standard protocol to group related families of gases into the most abundant one in the "family" and express all "siblings" in equivalent terms?*

Response:

Added: "and the equivalent "-eq" concentrations are presented in terms of the most abundant species in the HFC, PFC and CFC categorizations".

9. *Fire emissions are being taken from GFED4.1s. This makes total sense given timelines for preparing the report, but a clearer acknowledgement of the significant increase in expected emissions fluxes, perhaps on the order of factor 2 for certain gases, might be useful. Presumably next year the authors will move to the GFED5 database (recently released), so the authors might consider adding a brief note on the expected (approx.) magnitude of change in emissions estimates and likewise change in any dependent indicator values that are sensitive to the fire emissions data. Doing so may help to prepare the reader for changes to be expected in next year's iteration of the report.*

Response:

This is a valid point and highlights some of the dataset continuation issues that we highlight towards the end of the paper. To be specific, GFED5 re-evaluations will lead to systematically higher emissions estimates for most species (table 7 in van der Werf et al., 2025,

https://www.nature.com/articles/s41597-025-06127-w/tables/7). Fire carbon emissions in GFED5 are 66% higher than those from GFED4s for the overlapping period, but due to a revision of emission factors (Binte Shahid et al., 2024 [https://gmd.copernicus.org/articles/17/7679/2024/]) changes can be larger for individual species, towards the factor of two as you remark.

We've added: Estimates from GFED for 2017 to 2025 are provisional. GFED5 re-evaluations will lead to systematically higher emissions estimates for most species (van der Werf et al., 2025), of the order of a factor of two for some species, and will affect the ratio of non-biomass and biomass-burning aerosol for those species significantly affected, potentially impacting ERF estimates

10. *The authors assume some reader knowledge of the instruments used to measure concentrations of these NO2. To guide less informed readers like myself, I'd suggest summarising the broad characteristics of these monitoring systems and any structural differences in the concentrations retrieved by different instruments (or clearly referring the reader to an appropriate source of such information).*

Response:

We already explicitly reference the satellite systems and provide references where the reader can find more information. We feel adding detail on the instruments involved is out of scope.

11. *Suggest to use notation "GFED4.1s" throughout the manuscript for clarity, given recent release of GFED5.*

Response:

We think we have flagged differences where important and explicitly point to the new GFED5 to come.

12. [*NB: I am not working directly on the approaches used in sections 5-7 of this article and so I trust defer to other reviewers and community commentors who will be better placed to review the content of those section; I feel that these sections are well-written and broadly intuitive for a "informed non-expert" reader and understand the advances to be broadly incremental versus IPCC AR6 and previous iterations of this annual report].*

Response:

Thank you.

13. *[NB: Section 8 does not appear to include any new methodologies and hence I treat this as a straightforward update of previously published analyses with limited need to review the approach or findings].*

Response:

This is the case, it exactly updates previous methods.

14. *"anthropogenic warming and the RCBs changed by less than expected from linear extrapolation in the last year". Could you possibly expand on the reasons that you think this might be the case? Are there any implications for the methodologies you use to extrapolate the indicators into year+1?*

Response:

We already say in lines 768-771 of the original paper "Note that the interannual increase in assessed human-induced warming since last year's assessment is smaller than the assessed rate of human-induced warming, due in part to the change from HadCRUT 5.0.2 to 5.1.0, which contributed to a small downward revision of historical warming compared to Forster et al. (2025)." Also, in general we do expect the inclusion of new observations to influence our calculated level of anthropogenic warming (i.e. we don't always get an answer identical to extrapolating forward from the previous year's results). Therefore, we don't think further explanation is needed.

15. *I'm not sure that the TCRE acronym was already defined in the text. More broadly, there are of course a vast number of acronyms used in the text. You might consider adding a supplementary glossary of acronyms for this or future reports.*

Response:

TCRE is expanded on first use, line 862 of original paper. It's a good idea on a glossary, we will consider it in future years.

16. *As written, I understand the TXx metric for the land to be a metric of extreme values of the global mean surface temperature, which makes it heavily weighted towards the Northern Hemisphere (where most of the land is). Some discussion about the value and limitations of this approach would be useful. I wonder if it would be more interesting to provide a count of days on which *any* individual AR6 land region experienced extreme temperatures.*

Response:

This is a standard metric used in AR6 which we update. We think this discussion is best left to IPCC AR7 authors.

17. *The spatial aspect of MHW definition is not currently clear to me as written (though I appreciate that it must be provided in the IPCC WGIII definitions). My key query is: Are*

*MHWs detected regionally, e.g. within specific ocean basins, or are they defined only when the entire 60N-60S ocean zone exceeds the 99th percentile threshold? I wonder if it would be more interesting to provide a count of days on which *any* individual AR6 ocean region experienced a MHW.*

Response:

Thank you for this comment. We agree that the spatial aspect of the MHW definition was not sufficiently clear as written. We have now revised the text, both in the main manuscript and the methods section in the supplementary material, to explicitly state that MHWs are identified at each grid cell and then spatially averaged to produce the global mean annual MHW day metric. We also do not use a count of days in which *any* region experiences a MHW because it is likely that at least some area of the ocean is experiencing MHW conditions on almost every day. Such a metric would therefore provide limited information about interannual variability or the magnitude of widespread MHW activity. In contrast, the spatially averaged annual MHW day metric captures both the frequency and spatial extent of MHW conditions, since years with widespread and/or more persistent MHWs produce higher global mean values.

18. *Section 10 – general comment (related to two above): I think this section would be neater if the ocean and land temperature extremes indicators were better aligned conceptually and in their unit of measure (currently, the *C anomaly for land temperature contrasts with the count of MHW days). I appreciate that this carries over from AR6, but perhaps this article provides an opportunity to bring land and ocean extreme temperature tracking into closer alignment. [Perhaps one to consider in future].*

Response:

We thank the reviewer for this thoughtful suggestion. We agree that a closer conceptual alignment between land and ocean temperature extreme indicators could be valuable. However, in the present manuscript we chose to remain consistent with the definitions and indicators used in the IPCC AR6 framework, which facilitates comparability with previous assessments. In addition, MHWs are introduced in this study for the first time, and we therefore preferred to retain the established metric at this stage. We appreciate the suggestion and agree that exploring a more harmonized treatment of land and ocean temperature extremes could be an interesting direction for future work.

19. *I read the central message here as: The threat to delivery of the ECV layers has knock-on consequences for future iterations of this publication (and presumably also AR7). To keep this message tight, I'd suggest to focus the content of Table 14 only on the ECVs that are directly used or extremely relevant to the indicators presented in the current report. I think it's noble to stick up for the GCOS mission in its entirety, but I wonder if presenting the full range of ECVs unintentionally undermines the central message – put bluntly, and very much playing devil's advocate here: if they are all*

equally essential, then why aren't you using them all as indicators in your report? Are some more essential than others, and are the most essential ones actually at least risk? Perhaps a workaround is to add a column that explains for each ECV either (1) that the ECV was essential to the current report because {x, y, z}, or (2) that the ECV would be essential to activities of this GCCI group/report by underpinning scheduled improvements in future iterations of the GCCI report. Or perhaps this comment can trigger some other ideas as to how to more directly connect the threat to the GCOS ECV programme with a threat to the delivery of this report series.

Response:

Thank you for this comment. We now restrict this table to only the subset of ECVs that are of direct relevance to earlier sections. We also take advantage of considerable work by GCOS that has improved the consistency and utility of these tables. We have also worked to improve the preceding text, providing additional citations to relevant literature alongside details which hopefully better ground the role of observations in supporting our analysis and other critical applications. The revised text also better calls out the role of coordination bodies such as WMO and GCOS to underpinning monitoring activities.

Reply to Chris Jones

Thank you for your helpful review and thoughts on the tables. This text has now been changed in line with other reviewer comments. The abstract has been shortened and other text expanded. We have kept the main point you would like though, that observations are under threat.

1. *General point – is there a plan as we head towards IPCC AR7 to update the methodology in this annual update? For now you rightly stick to the existing AR6 methodology with revised data. But over the coming year or two I assume this will transition towards new AR7 methodology if that evolves from AR6. Other assessments, such as the WCRP TCRE assessment can also be used to update your remaining carbon budget.*

Response:

Yes, this is the plan, we will add this material next year.

2. *Related to this – there are changes to the methodology that create some of the datasets you use. In particular the volcanic forcing provided to CMIP7 is rather different from that used in CMIP6. It's not clear in figure 5 which you use, but it would be valuable to know which, and the importance of the changes. My understanding is that many more smaller eruptions in the historical record are now included. An assessment of the climatic/ERF impact of this would be valuable here.*

Response:

Reflecting updates in available data, we switch to the CMIP7 volcanic forcing time series here, which is more up-to-date than the CMIP6 time series (ending 2014) and integrates well with continually updated data from GloSSAC and OMPS-LP. The change in dataset is potentially important, and Thomas Aubry is leading a paper evaluating the differences between the datasets.

3. *Ditto anthropogenic aerosol. CMIP7 vs CMIP6 aerosol forcings differ. Table 5 suggests that the only change in your assessment since last year is the reduction in emissions in 2025. But a revised historical dataset would also have an impact on the assessed ERF from aerosols. Can you explain which datasets you have used, and how these evolve from year to year?*

Response:

The change to CMIP6 data to CMIP7 occurred last year and was documented in the Supplementary Material.

4. *The section on extremes is interesting (and important). It is often stated that extremes might change more rapidly than the mean. It would be valuable if you could comment on this and if this is observed in your datasets. Table 9 suggests a very small amplification with extreme temperature of 1.92, compared to increase in mean temperature of 1.8 in table 5. Is this a significant amplification?*

Response:

Regarding MHWs: this is an excellent question and the subject of an ongoing study by several of the co-authors. If feasible, these new results could be included in next year's edition.

5. *Typesetting point on the Figures: - I found some of the text in pale colours hard to read. I hope the final accepted version will have stronger and more accessible text in the figures.*

Response:

We have checked the final figure text and the text colours. I think this is issue was mainly Figure 2, so we have adjusted this.