Peer Review File

2023 temperatures reflect steady global warming and internal sea surface temperature variability

Corresponding Author: Dr Bjørn Samset

Version 0:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Dr Samset,

Your manuscript titled "2023 record temperatures consistent with steady global warming and sea surface temperature variability" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication. In particular, we point out the need to comment on the use of a model-specific Green's function and mixed use of multiple and single realizations for various CMIP models. Some clarification on the Green's function method and of the second figure were also requested.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred.

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We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:
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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Sylvia Sullivan, PhD
Editorial Board Member
Communications Earth & Environment

Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment

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Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Using a Green's function, the authors separate the contributions from long-term warming and SST patterns to global mean temperature, to investigate if 2023 was exceptionally warm, or within the range of internal variability. They conclude that 2023 was not exceptional, but rather comparable to other record-breaking years, mainly due to ENSO resulting in a higher-than-average global mean surface temperature.

The manuscript is well-written and will be a valuable addition to the literature. In particular, it will contribute to the ongoing discussion about 2023 and whether or not the Earth system started warming more rapidly than expected.

My main concern relates to the use of the Green's function for this particular application. The Green's function provides an estimate of the gradient dGSTA/dSST, but calculates it by running a climate model to equilibrium. Could the authors comment on the following two points?
1) How dependent are the results on the use of the Green's function of CESM compared to other climate models, knowing that Green's functions differ between climate models?
2) The current Earth system is not in equilibrium, while the Green's function assumes it. That is, the Green's function perturbs the SST at the start of the simulation, and after 40 years the GSTA is averaged, when the system is in equilibrium. Is it really appropriate to use the Green's function for year-to-year (or month-to-month) comparisons? In other words, how important are the different timescales at play? For yearly averages, this is probably not an issue, but I am not sure about monthly timescales.

Finally, it was not immediately clear to me what the maps in Fig.2 are showing. Is this the product of the Green's function with the observed SST? So the global sum of this is the pattern correction of GSTA? A few words on what exactly is shown, would help readers understand the figure and results better.

** Technical corrections **
L34 -- reword sentence; double use of "combination/combined"
Ref. 11 -- update reference

Reviewer #2 (Remarks to the Author):

The authors claim that although 2023 was the warmest year on record it is not unexpected when accounting for steady global warming and SST pattern changes. I found it very difficult to understand the figures and the analysis method. It is still unclear if I thoroughly understood the analysis methods. The Green's function data is used to argue for the contribution of SST pattern effect is not unprecedented. It is explicitly stated that the origin of SST pattern change cannot be assessed from their study. However, the SST pattern change itself could be the effect of aerosol forcing or unexpected greenhouse effect. The 2015/2016 also shows similar accelerated warming from SST pattern effect associated with the ENSO, but this similarity is not sufficient to preclude the growing effect of climate change. I wasn't convinced with the paper.

Reviewer #3 (Remarks to the Author):

Review of “2023 record temperatures consistent with steady global warming and sea surface temperature variability” by Samset et al.

General comments:

Samset et al. show that the global mean record temperature observed in 2023 is in line with steady global warming when correcting for the impact of the sea surface temperature pattern based on a Green’s function. They show that the year 2023 was similarly anomalous than previous El-Nino years such as 2016, 1998, 1969 and 1952. To test this, the authors isolate the global mean surface temperature anomalies from the SST patterns following Samset et al., 2023. While the method is not new, its application to the last-years temperature record with substantial media coverage is novel and of wide interest for the community and also the public. I consider the statistical analyses as appropriate and valid. I congratulate the authors on this concise and well-written study that is well suited for the journal. I do have some mostly minor comments that I would like to see addressed before publication.

Specific comments:

1) L15ff: There is some inconsistency here in first stating “a steady rate [of global warming] of around 0.02°C/decade since at least 1970” and then mentioning an “apparent increase in warming rate”. This contradiction needs clarification to avoid confusion by the reader.

2) Introduction: Please reference and discuss the recent literature relevant for this study (Min 2024, https://doi.org/10.1038/s43247-024-01391-x; Jiang et al. 2024, https://doi.org/10.1038/s41598-024-52846-2; Cheng et al. 2024 https://doi.org/10.1007/s00376-024-3378-5).

3) L65: I assume this should be “annual mean corrections” in contrast to the monthly corrections.

4) L78ff: That year 1952 also shows an anomalously warm North Atlantic is not obvious from the correction map in Fig. 2c. Please clarify.

5) L81: I suggest to move “(Figure 2b)” to the end of the sentence.

6) The major shortcoming of the study, as also acknowledged by the authors, is the lack of understanding of the mechanisms of the strong surface warming in 2023. For instance, Min 2024 finds only a small contribution from El Nino, i.e. the SST pattern, for the global mean record temperatures but rather attribute the anomalous warming to anthropogenic factors. Please expand your discussion in L97-104 to better address the potential causes of the 2023 record temperatures in general and in the context of the most recent literature.

7) It is confusing why the unit of the maps in Fig.2 is [milli°C] whereas it is [°C] in Fig. 1e-f and 2b. Please add a sentence to the figure caption for clarification.

8) I am not fully convinced by how the CMIP models are used. The authors use 33 CMIP models for comparison to the observational record(s), but for some models they use multiple realizations (i.e. the large ensemble) and for others they do not. This mixes model differences and internal variability. I understand that the distinction is not too important for the comparison to observations here, but using large ensembles or multiple realizations of ACCESS-ESM1-5, CanESM5, EC-Earth3-Veg, EC-Earth3, IPSL-CM6A-LR and MPI-ESM1-2-LR gives much more weight to these models than to the individual realizations. I encourage the authors to check whether the observation-model comparison is sensitive to this unintended model weighting by redoing the analyses with just the first ensemble member each. Further, I suggest to move the model specification to the Supplementary Methods to the Methods of the main manuscript and to acknowledge/reference the modeling groups similar to has been done for the observational datasets used.

Author Rebuttal letter: The author's response to these comments can be found at the end of this file.

Version 1:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Dr Samset,

Your manuscript titled "2023 record temperatures consistent with steady global warming and sea surface temperature variability" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment

On behalf of

Sylvia Sullivan, PhD
Editorial Board Member
Communications Earth & Environment

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have sufficiently answered the questions by all referees, in my opinion. The manuscript is acceptable for publication in its current form.

Reviewer #2 (Remarks to the Author):

I don't have further comments.

Reviewer #3 (Remarks to the Author):

I thank the authors for the revision of their manuscript. I am happy with the revised version. I have only a technical minor comment that can be easily fixed without another round of revisions (see below). I now recommend publication of the paper.

I spotted some inaccuracies in Table 2 with respect to the paper references on the climate model simulations used:
1) For CanESM5 (currently N/A) the following paper should be cited:
Swart, N. C., Cole, J. N. S., Kharin, V. V., Lazare, M., Scinocca, J. F., Gillett, N. P., et al. (2019). The Canadian Earth System Model version 5 (CanESM5.0.3). Geoscientific Model Development, 12(11), 4823–4873. https://doi.org/10.5194/gmd-12-4823-2019

2) For MPI-ESM1.2-HR, reference 51 is not the best. It should be replaced by the following:
Müller, W. A., Jungclaus, J. H., Mauritsen, T., Baehr, J., Bittner, M., Budich, R., et al. (2018). A Higher-resolution Version of

the Max Planck Institute Earth System Model (MPI-ESM1.2-HR). Journal of Advances in Modeling Earth Systems, 10(7), 1383–1413. https://doi.org/10.1029/2017MS001217

3) Similarly, for MPI-ESM1.2-LR, reference 51 is not the best. It should be replaced by the following:
Olonscheck, D., Suarez-Gutierrez, L., Milinski, S., Beobide-Arsuaga, G., Baehr, J., Fröb, F., et al. (2023). The New Max Planck Institute Grand Ensemble with CMIP6 forcing and high-frequency model output. Journal of Advances in Modeling Earth Systems, 15, e2023MS003790. https://doi.org/10.1029/2023MS003790

Author Rebuttal letter: The author's response to these comments can be found at the end of this file.

2023 record temperatures consistent with steady global warming and sea surface temperature variability, Samset et al., 2024

Response to reviewers

We thank the reviewers for their time and their very helpful input on this manuscript. Details of the revisions made to the main text and the supplement can be found below, but broadly they consist of:

- Expanded discussion on the usage and validity of Green's functions for this particular analysis
- Improved discussion and citation of recent papers, including a number which were published spring 2024
- Addition of a full CMIP6 model table in the main manuscript, including model description paper references

On behalf of the authors, I again wish to thank the reviewers for their time.

Regards,

Bjørn H. Samset, 2024-07-08

Reviewer #1 (Remarks to the Author):

Using a Green's function, the authors separate the contributions from long-term warming and SST patterns to global mean temperature, to investigate if 2023 was exceptionally warm, or within the range of internal variability. They conclude that 2023 was not exceptional, but rather comparable to other record-breaking years, mainly due to ENSO resulting in a higher-than-average global mean surface temperature.

The manuscript is well-written and will be a valuable addition to the literature. In particular, it will contribute to the ongoing discussion about 2023 and whether or not the Earth system started warming more rapidly than expected.

Thank you, we greatly appreciate this assessment.

My main concern relates to the use of the Green's function for this particular application. The Green's function provides an estimate of the gradient dGSTA/dSST, but calculates it by running a climate model to equilibrium. Could the authors comment on the following two points?
1) How dependent are the results on the use of the Green's function of CESM compared to other climate models, knowing that Green's functions differ between climate models?

There is clearly a model dependence in our method. We are working implementing a multi-Green's-Function version of our analysis, however for our purposes we do not expect this to have a major influence. Other studies have shown that the overall response patterns, and also the TOA fluxes and general atmospheric responses, to localized SST perturbations are broadly similar between models. This will be further investigated by the Green's Function MIP project (https://doi.org/10.1029/2023MS003700), but initially, they find that the differences between current commonly used GFs may come both from model differences and from differences in simulation setup. A subset of the same authors however also conclude in a recent paper that "While we only use one GF developed from one AGCM in this study, the general pattern of λ is similar across models (Dong et al., 2019; Zhang et al., 2023; Zhou et al., 2017)." (Alessi and Rugenstein; https://doi.org/10.1029/2023GL105795). λ is here the spatially resolved radiative feedback parameter, which correlates closely with the surface temperature pattern response.

In the revised manuscript, we have acknowledged more clearly that our results are dependent on the features of NCAR CESM1, and also the literature cited above:

Note that the results will depend on the detailed response of the CESM1 model to localized SST perturbations, however other studies have shown that the general spatial pattern and atmospheric dynamical features of such Green's functions are broadly similar between models[1-4].

2) The current Earth system is not in equilibrium, while the Green's function assumes it. That is, the Green's function perturbs the SST at the start of the simulation, and after 40 years the GSTA is averaged, when the system is in equilibrium. Is it really appropriate to use the Green's function for year-to-year (or month-to-month) comparisons? In other words, how important are the different timescales at play? For yearly averages, this is probably not an issue, but I am not sure about monthly timescales.

This is an excellent point, and one we did spend a good deal of time considering when first developing this methodology (Samset et al. 2022 [5]). In brief, yes, we do find that this usage of GFs remains appropriate at monthly timescales, however we agree that it is not immediately obvious.

A "top level" argument is that the method does identify known features from previous years, such as El Nino/La Nina variations, including monthly variations through years. This is apparent e.g. from Figure 1 in the present manuscript.

It could be, however, that the GFs, being equilibrium runs, project too strong responses, or that the responses are stronger in a later month (a lagged response). We've investigated both of these questions for previous publications[5,6], using as a metric the amount of interannual or inter-month variability filtered out by the method. The filtering was most efficient using an unmodified strength, and for a lag of 0, indicating that the maximum potential of the current method is indeed the "immediate response" assumption.

On a deeper level, what initially determines the rapid atmospheric response to an SST variation will be the time scale of Rossby wave propagation. See e.g. Sardeshmukh and Hoskins 1988[7], which shows how atmospheric dynamical responses broadly develop on a sub-monthly scale. More recently, a publication based on the PDRMIP dataset (Stjern et al. 2023[8]) investigates the timescales of responses to CO_2 and aerosols perturbations in models of the same generation as our Green's function (including CESM1, using both CAM4 and CAM5), for fixed-SST (and coupled) setup. Even for a perturbation originating in the atmosphere, they find a near-surface heating response that is significantly evolved after a month, and representative of the final, equilibrium pattern, while of course not yet being of full strength.

This, in sum, convinces us that the overall patterns of SST variability on global mean temperatures should be captured by the GF filtering method.

We summarize this discussion in the revised manuscript as follows:

We note that the CESM1 derived GF used here has been documented to have very similar response patterns to GFs produced with other models[1-4]. The applicability of equilibrium simulations to capture monthly and interannual variability has been investigated for previous publications[5,6]. While the full atmospheric response will not be realized for monthly varying SST patterns, the rapid timescales of Rossby wave train propagation and other dynamical features means that the key features of a response do have time to develop [7,8].

Finally, it was not immediately clear to me what the maps in Fig.2 are showing. Is this the product of the Green's function with the observed SST? So the global sum of this is the pattern correction of GSTA? A few words on what exactly is shown, would help readers understand the figure and results better.

Thanks, this was indeed not well described. We have added the following explanation to the caption and the main text:

Maps show the product of the Green's function and the observed SST pattern, such that the total correction (globally or regionally) is the sum of all relevant grid boxes.

** Technical corrections **
L34 -- reword sentence; double use of "combination/combined"

Fixed. (Combined -> together)

Ref. 11 -- update reference

Fixed.

Reviewer #2 (Remarks to the Author):

The authors claim that although 2023 was the warmest year on record it is not unexpected when accounting for steady global warming and SST pattern changes. I found it very difficult to understand the figures and the analysis method. It is still unclear if I thoroughly understood the analysis methods. The Green's function data is used to argue for the contribution of SST pattern effect is not unprecedented. It is explicitly stated that the origin of SST pattern change cannot be assessed from their study. However, the SST pattern change itself could be the effect of aerosol forcing or unexpected greenhouse effect. The 2015/2016 also shows similar accelerated warming from SST pattern effect associated with the ENSO, but this similarity is not sufficient to preclude the growing effect of climate change. I wasn't convinced with the paper.

We are aware that the present paper is quite dense, but note that it does build on two longer, much more detailed publications that document the underlying method[5,6]. The revision, hopefully, also fills some gaps; see our responses to reviewers 1 and 3.

Also, crucially, we note that our aim, as clearly stated, is not to prove that there is no influence on 2023 temperatures from altered aerosol or greenhouse gas forcing. Quite the opposite; by considering the influence of internal variability, we aim to help clarify what role the two main anthropogenic factors have (as, indeed, we are doing in other publications and projects such as the Regional Aerosol Model Intercomparison Project).

It is clear that any full diagnostic of the reasons for the 2023 warming must include both natural and anthropogenic factors. Other publications have discussed aerosols, notably, while ours targets the role of the SST pattern, which has been missing from the debate so far. We are of course sorry that the reviewer is not convinced, but hope that on a second reading the method can still be found to be reasonable. We look forward to continuing the discussion of the full set of causes behind the 2023 record setting surface temperatures.

Reviewer #3 (Remarks to the Author):

Review of “2023 record temperatures consistent with steady global warming and sea surface temperature variability” by Samset et al.

General comments:

Samset et al. show that the global mean record temperature observed in 2023 is in line with steady global warming when correcting for the impact of the sea surface temperature pattern based on a Green’s function. They show that the year 2023 was similarly anomalous than previous El-Nino years such as 2016, 1998, 1969 and 1952. To test this, the authors isolate the global mean surface temperature anomalies from the SST patterns following Samset et al., 2023. While the method is not new, its application to the last-years temperature record with substantial media coverage is novel and of wide interest for the community and also the public. I consider the statistical analyses as appropriate and valid. I congratulate the authors on this concise and well-written study that is well suited for the journal. I do have some mostly minor comments that I would like to see addressed before publication.

We thank the reviewer for this assessment of our manuscript.

Specific comments:

1) L15ff: There is some inconsistency here in first stating “a steady rate [of global warming] of around 0.02°C/decade since at least 1970” and then mentioning an “apparent increase in warming rate”. This contradiction needs clarification to avoid confusion by the reader.

Thanks, very good point. We have revised this by making the two sentences more clearly linked:

Anthropogenic global warming, driven primarily by emissions of greenhouse gases, has progressed at a broadly steady rate of around 0.2 °C/decade since at least 1970[9]. Recently, however, several studies have also documented a minor step-up in the rate of global mean surface temperature anomaly (GSTA) increase around 1990[6,10,11], a continued rise in the global energy imbalance[12], and an acceleration in accumulation of ocean heat content[13].

2) Introduction: Please reference and discuss the recent literature relevant for this study (Min 2024, https://doi.org/10.1038/s43247-024-01391-x; Jiang et al. 2024, https://doi.org/10.1038/s41598-024-52846-2; Cheng et al. 2024 https://doi.org/10.1007/s00376-024-3378-5).

Thanks, these are all highly relevant. We have added them to the discussion in the introduction in the pertinent places (and further down, as suggested below by the reviewer).

3) L65: I assume this should be “annual mean corrections” in contrast to the monthly corrections.

Partly; we were referring to the spread of monthly corrections, and also, in effect, the annual means. The sentence has been clarified as follows:

In fact, when we select the four other years with strongest SST induced corrections (1952, 1969, 1998, 2016), their distributions of monthly corrections, as well as their overall annual corrections, are very similar to 2023.

4) L78ff: That year 1952 also shows an anomalously warm North Atlantic is not obvious from the correction map in Fig. 2c. Please clarify.

Thanks. The crucial missing word was “Tropical”. The subtropical North Atlantic indeed varies more between the years. We have clarified that this referred to the Tropical North Atlantic, where most of the early anomalous warming of 2023 occurred.

5) L81: I suggest to move “(Figure 2b)” to the end of the sentence.

Fixed.

6) The major shortcoming of the study, as also acknowledged by the authors, is the lack of understanding of the mechanisms of the strong surface warming in 2023. For instance, Min 2024 finds only a small contribution from El Nino, i.e. the SST pattern, for the global mean record temperatures but rather attribute the anomalous warming to anthropogenic factors. Please expand your discussion in L97-104 to better address the potential causes of the 2023 record temperatures in general and in the context of the most recent literature.

Thanks. The following was added to the discussion:
Other studies have recently investigated the role of pacific temperatures for the 2023 GSTA, reaching seemingly differing conclusions [14,15]. Our study indicates a marked role of El Niño through the latter part of 2023, consistent with Forster et al.[15].

7) It is confusing why the unit of the maps in Fig.2 is [milli°C] whereas it is [°C] in Fig. 1e-f and 2b. Please add a sentence to the figure caption for clarification.

Done.

8) I am not fully convinced by how the CMIP models are used. The authors use 33 CMIP models for comparison to the observational record(s), but for some models they use multiple realizations (i.e. the large ensemble) and for others they do not. This mixes model differences and internal variability. I understand that the distinction is not too important for the comparison to observations here, but using large ensembles or multiple realizations of ACCESS-ESM1-5, CanESM5, EC-Earth3-Veg, EC-Earth3, IPSL-CM6A-LR and MPI-ESM1-2-LR gives much more weight to these models than to the individual realizations. I encourage the authors to check whether the observation-model comparison is sensitive to this unintended model weighting by redoing the analyses with just the first ensemble member each.

This is a good point. We have included the ensembles for full consistency with a previous publication, where the effects of ensemble sizes and internal variability in CMIP6 models are explicitly discussed[6]. For the present manuscript, we have confirmed that the added ensemble members do not influence our analysis beyond adding additional statistics. As our aim here is to identify potential outlier years, we still wish to include as many members as possible in the main analysis. However, we have acknowledged the point raised by the reviewer in the main text:

Note that for some models, we use multiple ensemble members to further sample the effects of internal variability. The overall conclusions are not influenced by this.

Further, I suggest to move the model specification to the Supplementary Methods to the Methods of the main manuscript and to acknowledge/reference the modelling groups similar to has been done for the observational datasets used.

Thanks; we agree that this is crucial. The requested table has been added, together with references to model description papers and datasets.

References

1 Bloch-Johnson, J. *et al.* The Green's Function Model Intercomparison Project (GFMIP) Protocol. *Journal of Advances in Modeling Earth Systems* **16**, doi:10.1029/2023ms003700 (2024).

2 Dong, Y., Proistosescu, C., Armour, K. C. & Battisti, D. S. Attributing Historical and Future Evolution of Radiative Feedbacks to Regional Warming Patterns using a Green's Function Approach: The Preeminence of the Western Pacific. *Journal of Climate* **32**, 5471-5491, doi:10.1175/jcli-d-18-0843.1 (2019).

3 Zhang, B., Zhao, M. & Tan, Z. Using a Green's Function Approach to Diagnose the Pattern Effect in GFDL AM4 and CM4. *Journal of Climate* **36**, 1105-1124, doi:10.1175/jcli-d-22-0024.1 (2023).

4 Zhou, C., Zelinka, M. D. & Klein, S. A. Analyzing the dependence of global cloud feedback on the spatial pattern of sea surface temperature change with a Green's function approach. *Journal of Advances in Modeling Earth Systems* **9**, 2174-2189, doi:10.1002/2017ms001096 (2017).

5 Samset, B. H. *et al.* Earlier emergence of a temperature response to mitigation by filtering annual variability. *Nat Commun* **13**, 1578, doi:10.1038/s41467-022-29247-y (2022).

6 Samset, B. H. *et al.* Steady global surface warming from 1973 to 2022 but increased warming rate after 1990. *Communications Earth & Environment* **4**, doi:10.1038/s43247-023-01061-4 (2023).

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Response to reviewers and editorial requests

We thank the reviewers and the editor again for their time and very helpful feedback. In this revision, we have:

- Updated CMIP6 model references suggested by reviewer 3
- Changed the title and abstract as suggested by the editorial team
- Moved Supplementary Figures 1 and 2 up into the main manuscript, as also suggested by the editorial team

The last change required some minor alterations to the text to keep the flow of the document. See the tracked changes version for details. The figures have also been edited for visual clarity. However, no scientific content or concrete results have been changed, or added, in this revision. The discussion of the new figures was taken from the previous Supplementary Materials.

On behalf of the authors,

Bjørn H. Samset

Oslo, August 5th, 2024