nature portfolio

Peer Review File

Steady global surface warming from 1973 to 2022 but increased warming rate after 1990

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality.

31st Jul 23

Dear Dr Samset,

Your manuscript titled "Steady global surface warming through 2022, after a recent step up in warming rate" has now been seen by 2 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.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. In particular, we encourage you to add an analysis of a second climate model to address reviewer 1's concerns regarding potential model bias. This is not a condition for publication; however, if it is not possible to add a second model analysis, we will need you to add appropriate caveats to the main text.

Please highlight all changes in the manuscript text file.

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:
[link redacted]

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

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,

Heike Langenberg, PhD
Chief Editor
Communications Earth & Environment

On Twitter: @CommsEarth

EDITORIAL POLICIES AND FORMATTING

We ask that you ensure your manuscript complies with our editorial policies. Please ensure that the following formatting requirements are met, and any checklist relevant to your research is completed and uploaded as a Related Manuscript file type with the revised article.

Editorial Policy: Policy requirements (Download the link to your computer as a PDF.)

Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist:
Communications Earth & Environment formatting checklist

and also in our style and formatting guide Communications Earth & Environment formatting guide
.

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All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf.

In particular, the Data availability statement should include:
- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)
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- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

DATA SOURCES: All new data associated with the paper should be placed in a persistent repository where they can be freely and enduringly accessed. We recommend submitting the data to discipline-specific, community-recognized repositories, where possible and a list of recommended repositories is provided at http://www.nature.com/sdata/policies/repositories.

If a community resource is unavailable, data can be submitted to generalist repositories such as figshare or Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

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

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Summary of paper:
In this new study, the authors build off their recent methodology, published in Samset et al. (2022), to disentangle the role of sea surface temperature anomaly spatial patterns (internal variability) from the long-term global mean surface temperature (GMST) trend. This work extends the observed data through 2022, which accounts for the recent triple dip La Nina event and subsequent brief slowdown in the recent GMST trend since around 2016. They compare the raw and filtered rates of GMST warming with data from a collection of Coupled Model Intercomparison Project (CMIP) Phase 6 (CMIP6) models and one large ensemble simulation (CanESM5; only 20 ensemble members). The authors find that nearly all CMIP6 models have a higher 50-year mean warming rate than in observations, which is consistent with previous work. However, after filtering out the influence of internal variability, they importantly reveal a disconnect between how well CMIP6 models capture the recent higher rates of warming relative to those found in observations. Finally, there is a discussion about the need to better understand the influence of near-term warming rates, such as those that could be observed in response to changes in anthropogenic aerosol forcing. This is suggested by a call for the climate community to regularly track the 'filtered' GMST.

General Comments:
Overall, this is a very interesting letter-style study that builds off nicely from the author's previous work. I can see this paper being of high interest to the climate community, especially with the recent discussions of expected temperature records in the coming year or two - associated with the arrival of El Nino and possible effects of sulfate aerosol reduction. Many of the methodological choices have already been documented within their previous study, and I don't have too much more to comment on that. Before this paper should be considered for publication, I have two main concerns: 1) the use of only one large climate model large ensemble, which is known to exhibit a particularly high ECS (CanESM5), and 2) the conclusions inferred from the text/discussions about the different the rates of warming between the raw/filtered data can be quite confusing at times. For example, I had to read the study several times to confirm whether the short-term warming rate was consistent with the previous 50-years or whether it was increasing.

Recommendation:
I am suggesting major revisions due to the possible effort/time of including more analysis from another single model initial-condition large ensemble. The rest of my comments below are indeed very minor.

Disclaimer – This reviewer has expertise on climate modeling and large-scale climate variability. However, I have not directly worked with Green's Function, such as for the methodology used here to filter out the internal SST anomaly pattern on the long-term forced warming rate.

Specific/Technical Comments:
1. L10-11; I understand why this sentence was written, but global ocean heat content (OHC) is probably a more (consistently) reliable metric of anthropogenic climate change. Maybe the phrasing could be changed here slightly?
2. L13-14; Perhaps state the actual last influenced year, just so this line is clearer for historical/archival perspectives for readers several years/decades from now into the future.
3. L14-15; This is an example of where I think the text can be more refined. It reads at first that the rate of warming is steady and consistent, but then says suddenly that it has increased/warmed?
4. L18-19; New reference needed here – perhaps one of the annual recent state of the climate reports? References #1 and #2 are published prior to 2022.
5. L30; Warming continues steadily for OHC. Thus, I recommend changing to "… and an apparent short-term slowdown in the rate of GSTA warming."
6. L42; Typo in the reference list for "in revision" of #12
7. L65; Does this range leverage the ensemble members designed to sample uncertainty for

HadCRUT5 (https://www.metoffice.gov.uk/hadobs/hadcrut5/)?
8. L67; Are there any notable changes if this is updated using NOAAGlobalTemp Version 5.1, which better samples warming over the poles? (https://www.ncei.noaa.gov/products/land-based-station/noaa-global-temp; Vose et al. 2021)
9. L75; What do you mean for - "known from news reports of extreme conditions"
10. L83-84; It might be useful to annotate the actual GSTA next to each seasonal map in Supplemental Figure 1, which will better support the results description in the text about the relative warmer or colder seasons per 2021 and 2021.
11. L91-92; I think it might be helpful to improve clarity to make sure that filtered data or raw data is specifically restated in each subplot panel description for the figure 2's caption.
12. L112; Typo for "period"
13. L113-116; This actually seems like quite a large range to me in the rate of warming between these station-based observational products. Any thoughts?
14. L119-121; This sentence was a bit confusing to me at first read through.
15. 131-132; Why was CanESM5 chosen here (and not using the full 50 members), given that it has a particularly large/outlier ECS/warming (Po-Chedley et al. 2022)? As recent studies have shown (e.g., Deser et al. 2020), internal variability can be better quantified by using collections of climate model large ensembles. Since there are now numerous large ensembles available for both CMIP5 and CMIP6 class GCMs, I think it is important to consider how robust these results to a comparison with another large ensemble with observations.
16. L137-140; Which GCM(s) are close to the observations in Figure 2d? (the dark red dots)
17. L168; Up to this point in the text, it is not stated about the filtered data being derived from CESM1 fields, so this may be confusing to some readers (unless they read further down).
18. L222; State how many individual GCMs.
19. L220-221; Are the results sensitive to using 2-m temperature? In other words, what about using a blended atmosphere/ocean mean (Cowtan et al. 2015)?

Data availability statement:
1. L230-231; Please put a general link to the data on ESGF.

Figures/Tables:
1. General comment; Perhaps it is my PDF viewer, but the supplemental figures were a bit blurry for me.

References:
Cowtan, Kevin, et al. "Robust comparison of climate models with observations using blended land air and ocean sea surface temperatures." Geophysical Research Letters 42.15 (2015): 6526-6534.

Deser, C., Lehner, F., Rodgers, K. B., Ault, T., Delworth, T. L., DiNezio, P. N., ... & Ting, M. (2020). Insights from Earth system model initial-condition large ensembles and future prospects. Nature Climate Change, 10(4), 277-286.

Po-Chedley, Stephen, et al. "Internal variability and forcing influence model–satellite differences in the rate of tropical tropospheric warming." Proceedings of the National Academy of Sciences 119.47 (2022): e2209431119.

Vose, R. S., et al. "Implementing full spatial coverage in NOAA's global temperature analysis." Geophysical Research Letters 48.4 (2021): e2020GL090873.

Reviewer #2 (Remarks to the Author):

Overall an interesting, well written paper and an advancement of the previously published methods paper (Samset et al 2022, Nat Comms). While not entirely novel given it continues where the former

paper left it off, it does provide a relevant perspective on the current warming conversation in that it puts the most recent temperature observations (or trends for that matter) into perspective. In that sense, it is a welcomed complementary effort to a very recent review paper estimating the attributable warming level, including trends (which are consistent: Forster et al 2023, ESSD). I therefore consider the manuscript exciting and methodologically sound enough to be published in 'communications earth & environment'.

Below some comments which I'd ask the authors to address before publishing.

Lines 32-36:
"The increase in anthropogenic CO2 emissions, the primary driver of warming, has slowed to ~1%/year in the recent decade, relative to ~3%/year in the period 1991-2010. Emissions of SO2, precursor of cooling sulfate aerosols, have also decreased strongly over the last decade, largely due to strong Chinese efforts to combat air pollution."

—> On the other hand, CH4 emissions have continued to increase somewhat. 'also' (wrt sulfate) strikes me as odd given that only delta CO2 emissions have decreased, while actual CO2 emissions have continued to go up. In contrast SO2 emissions went down … hence 'also' sounds confusing to me.

Lines 51/52:
"Applying the method to the HadCRUT5 data series, we found that for the (then) last 50 years (1971-2020), global warming had progressed at a steady rate of 0.19 °C/decade."

—> How does that square with the results in Samset et al 2022, quote: "The filtered GSTA yields a 2011–2020 warming rate of 0.24 °C per decade, and a 30-year trend that is indistinguishable from the unfiltered results (0.21 °C). Similarly, for the decade 2001–2010, which is in the center of the so-called global warming hiatus period, unfiltered HadCRUT5 observations yield a warming rate of 0.08 °C per decade, while the filtered results show 0.21 °C per decade, again similar to the most recent 30-year trend (1991–2020)." ? Seems somewhat inconsistent to me, despite it being two different time intervals. Perhaps you could elaborate.

Lines 75-78:
"2021 began with a cold spell over northern Eurasia, driven by a collapse of the polar vortex, while SSTs are dominated by a very cold Central and Eastern Pacific. This had a strong cooling influence on global surface temperatures in Spring of […]"

—> Not sure if i got that completely right: Does the correction pick up the winter cooling over Eurasia (in that it projects onto the amplitude of the associated SST correction pattern)?

Samset et al 2022:
"The observed GSTA (from HadCRUT5) has first been detrended, via a 10-year moving boxcar average applied at each grid point (see Methods), to isolate (as far as possible) the influence of annual variability from decadal patterns and the effects of global warming since 1850."

—> In how far would the applied moving average pick up any accelerated warming during the last decade (in case there is one)? Guess it is related to the comment in lines 108-110, quote "We also note that the raw data consistently shows a stronger rate increase than the filtered data, indicating differences in the amount of warming filtered through the time period.", isn't it?

Lines 117-121:
"We note, however, that, similarly to what is found for OHC, the strongest increase in rates seems to have occurred around the middle of the 50-year period, with a levelling off in later years. This means that while we do find a consistent increase in warming rate over the last 50 years, we see no recent "acceleration" of global surface warming in the sense of a continuous rate increase."

—> Which seems to be overall consistent with Forster et al 2023 (https://essd.copernicus.org/articles/15/2295/2023/#&gid=1&pid=1) … except for the influence of reduced sulfate emission from ships due to regulation changes in 2020. They admit that their estimate is rather crude and it stands to reason that the impact might be more non-linear than currently accounted for in their extrapolated aerosol forcing estimate. Might be worth adding a short note of caution in your manuscript in that regard as well (perhaps right at the beginning, e.g. lines 32-36)

Little glitch in line 112: 'period'

Lines 134-136:
"This result has been discussed in previous literature and is likely due to a combination of high climate sensitivities in some CMIP6 models, and that the realized SST patterns are not well captured by the model ensemble, notably in the Pacific."

—> Any chance to present the CMIP6 results also as function of TCR rather than ECS? And picking nits here, the colours for the different ECS values in Fig 2d are difficult to distinguish. Perhaps there's room to make that easier to read …

Steady global surface warming through 2022, after a recent step up in warming rate

Samset et al. 2022

Response to reviewers

We thank both reviewers for their time and very valuable comments. The manuscript has for the most part been modified as suggested. The main changes are:

- The addition of two more large ensembles to Figure 2, with associated discussion and documentation.
- Clarifications regarding the various rate calculations and comparisons to previous work. This includes the addition of a table, for ease of reference.
- Update of the NOAA dataset to version 5.1, released July 2023, as suggested by Reviewer 1. This does not alter any conclusions of the paper.

Our full responses can be found inline below.

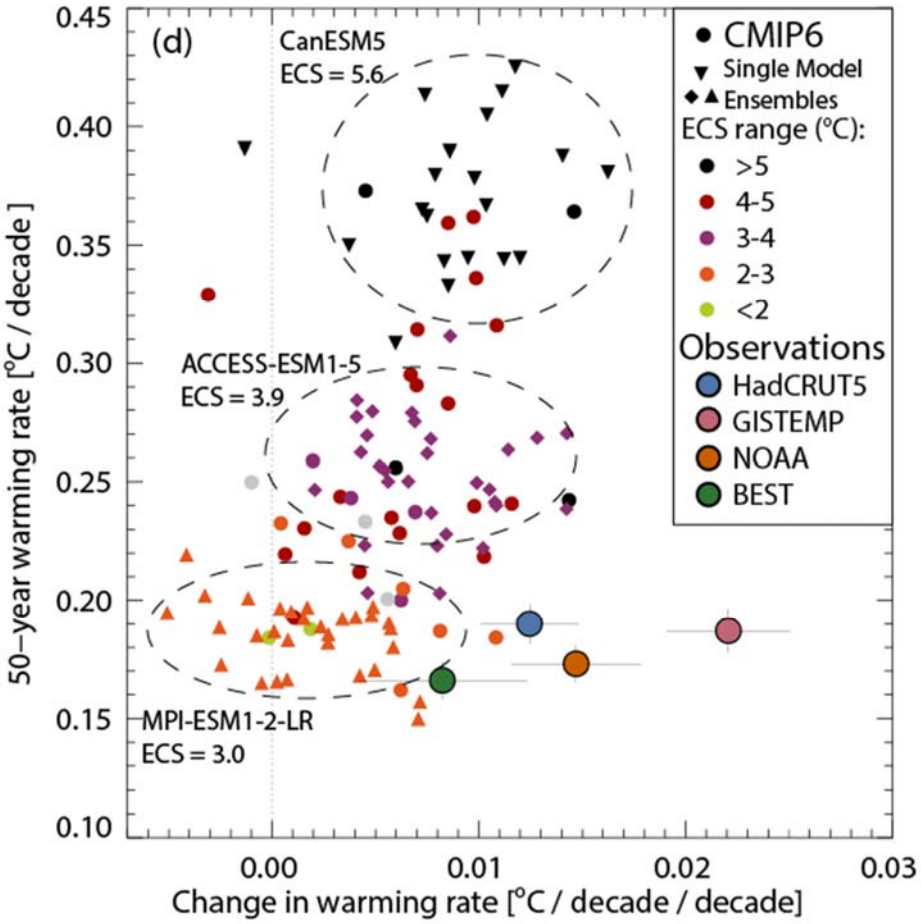
Reviewer #1 (Remarks to the Author):

General Comments:
Overall, this is a very interesting letter-style study that builds off nicely from the author's previous work. I can see this paper being of high interest to the climate community, especially with the recent discussions of expected temperature records in the coming year or two - associated with the arrival of El Nino and possible effects of sulfate aerosol reduction. Many of the methodological choices have already been documented within their previous study, and I don't have too much more to comment on that. Before this paper should be considered for publication, I have two main concerns:

1) the use of only one large climate model large ensemble, which is known to exhibit a particularly high ECS (CanESM5)

This is true. Originally, we added the ensemble to give an overall indication of the impacts of internal variability, which has been shown elsewhere to be similar between current models regardless of their ECS. However, we see that the use of this particular model can raise questions, so we added two more; one with ECS around 3 °C (MPI-ESM1-2-LR) and one around 4 °C (ACCESS).

As can be seen in the revised Figure 2d, the three models cluster around the centre and endpoints of the correlation:



The spread in each is indicative of the level of internal variability for these particular observables, and is broadly comparable between the three models. They don't overlap, however, and are all generally inconsistent with the observations, which would seem to strengthen the argument that there is some process or change here that the current models miss.

We updated the figure, methods and discussion to reflect the addition of these two additional model ensembles.

2) the conclusions inferred from the text/discussions about the different the rates of warming between the raw/filtered data can be quite confusing at times. For example, I had to read the study several times to confirm whether the short-term warming rate was consistent with the previous 50-years or whether it was increasing.

Thanks for flagging this. We've tried to clarify it in the text. In addition, we've added a table with all the rate and rate change values, filtered and unfiltered, for the four data series. Hopefully this makes it easier to compare numbers across the parts of the discussion. As an example, the revised abstract reads

"The change in global mean surface temperature is a crucial and broadly used indicator of the evolution of climate change. Any decadal scale changes in warming rate are however obfuscated by internal variability. Here, by filtering out modulations to the warming rate by sea surface temperature

(SST) patterns, we show that surface temperatures through the recent La Nina influenced years (2022) are consistent with the 50-year trend of 0.18 °C/decade. However, we also find clear indications of a step-up in warming rate in recent decades. CMIP6 models generally do not capture this observed combination of long-term warming rate and recent increase."

Specific/Technical Comments:

1. L10-11; I understand why this sentence was written, but global ocean heat content (OHC) is probably a more (consistently) reliable metric of anthropogenic climate change. Maybe the phrasing could be changed here slightly?

OHC is clearly more reliable as a quantitative metric, which is why we termed it an "indicator". But we agree this is not necessarily clear, so we have rephrased as follows:

"The change in global mean surface temperature is a crucial and broadly used indicator of the evolution of climate change, but…"

2. L13-14; Perhaps state the actual last influenced year, just so this line is clearer for historical/archival perspectives for readers several years/decades from now into the future.

Done.

3. L14-15; This is an example of where I think the text can be more refined. It reads at first that the rate of warming is steady and consistent, but then says suddenly that it has increased/warmed?

This point is well taken. We've revised the abstract and introduction, and a number of other passages, to clarify.

4. L18-19; New reference needed here – perhaps one of the annual recent state of the climate reports? References #1 and #2 are published prior to 2022.

Done.

5. L30; Warming continues steadily for OHC. Thus, I recommend changing to "… and an apparent short-term slowdown in the rate of GSTA warming."

Done. (We opted to say "rate of surface warming", but the meaning is the same as suggested by the reviewer.)

6. L42; Typo in the reference list for "in revision" of #12

Fixed.

7. L65; Does this range leverage the ensemble members designed to sample uncertainty for HadCRUT5 (https://www.metoffice.gov.uk/hadobs/hadcrut5/)?

No. We have opted to treat the data series as given in this analysis, and provide ranges based on the fits to the best estimate values. This allows us to treat all series consistently. A better-founded uncertainty range here would not affect the overall conclusions of the paper.

We added clarification of this to the sentence.

8. L67; Are there any notable changes if this is updated using NOAAGlobalTemp Version 5.1, which better samples warming over the poles? (https://www.ncei.noaa.gov/products/land-based-station/noaa-global-temp; Vose et al. 2021)

Thanks for making us aware of the updated dataset. We've switched to V5.1 throughout the paper. Overall it makes very little difference, except for the warming rate increase where NOAA is now closer to GISS than to HadCRUT5.

9. L75; What do you mean for - "known from news reports of extreme conditions"

This refers to weather events that had societal impacts and were broadly reported in the news media, with examples on the next lines. We have clarified this in the text.

10. L83-84; It might be useful to annotate the actual GSTA next to each seasonal map in Supplemental Figure 1, which will better support the results description in the text about the relative warmer or colder seasons per 2021 and 2021.

Thanks. We considered this, but found that it would require extensive explanation for the numbers to be readily interpretable by the reader. The main point of the plots is to show the overall pattern after detrending, on a season-by-season basis.

11. L91-92; I think it might be helpful to improve clarity to make sure that filtered data or raw data is specifically restated in each subplot panel description for the figure 2's caption.

Done.

12. L112; Typo for "period"

Fixed.

13. L113-116; This actually seems like quite a large range to me in the rate of warming between these station-based observational products. Any thoughts?

Indeed it is large, and, as shown in the new Table 1, they are actually inconsistent after filtering. Likely this is due to the difference in treatment of low measurement density areas like the Arctic, and the blending of land and sea surface data, but we do not really have grounds to speculate here beyond this. We do note the inconsistency in the revision, though.

14. L119-121; This sentence was a bit confusing to me at first read through.

We have tried to clarify.

15. 131-132; Why was CanESM5 chosen here (and not using the full 50 members), given that it has a particularly large/outlier ECS/warming (Po-Chedley et al. 2022)? As recent studies have shown (e.g., Deser et al. 2020), internal variability can be better quantified by using collections of climate model large ensembles. Since there are now numerous large ensembles available for both CMIP5 and CMIP6 class GCMs, I think it is important to consider how robust these results to a comparison with another large ensemble with observations.

Done. See the response to the first major comment above.

16. L137-140; Which GCM(s) are close to the observations in Figure 2d? (the dark red dots)

The two closest dots are the two MIROC versions. The newly added MPI-ESM1-2-LR ensemble also has a few members that come close, but that are still markedly lower than the observations in rate increase.

17. L168; Up to this point in the text, it is not stated about the filtered data being derived from CESM1 fields, so this may be confusing to some readers (unless they read further down).

Thanks. We changed this to point to Methods, which gives the full explanation.

18. L222; State how many individual GCMs.

Done. (33 models, 119 ensemble members.)

19. L220-221; Are the results sensitive to using 2-m temperature? In other words, what about using a blended atmosphere/ocean mean (Cowtan et al. 2015)?

This was tested in Samset 2022, and the answer is that there is no sensitivity to this choice for the analysis that we present here.

Data availability statement:
1. L230-231; Please put a general link to the data on ESGF.

Done.

Figures/Tables:
1. General comment; Perhaps it is my PDF viewer, but the supplemental figures were a bit blurry for me.

The resolution of the figures has been improved for the revised submission.

Reviewer #2 (Remarks to the Author):

Overall an interesting, well written paper and an advancement of the previously published methods paper (Samset et al 2022, Nat Comms). While not entirely novel given it continues where the former

paper left it off, it does provide a relevant perspective on the current warming conversation in that it puts the most recent temperature observations (or trends for that matter) into perspective. In that sense, it is a welcomed complementary effort to a very recent review paper estimating the attributable warming level, including trends (which are consistent: Forster et al 2023, ESSD). I therefore consider the manuscript exciting and methodologically sound enough to be published in 'communications earth & environment'.

Below some comments which I'd ask the authors to address before publishing.

Lines 32-36:
"The increase in anthropogenic CO2 emissions, the primary driver of warming, has slowed to ~1%/year in the recent decade, relative to ~3%/year in the period 1991-2010. Emissions of SO2, precursor of cooling sulfate aerosols, have also decreased strongly over the last decade, largely due to strong Chinese efforts to combat air pollution."

—> On the other hand, CH4 emissions have continued to increase somewhat. 'also' (wrt sulfate) strikes me as odd given that only delta CO2 emissions have decreased, while actual CO2 emissions have continued to go up. In contrast SO2 emissions went down … hence 'also' sounds confusing to me.

Good point. We have removed the 'also', and noted the CH4 increase.

Lines 51/52:
"Applying the method to the HadCRUT5 data series, we found that for the (then) last 50 years (1971-2020), global warming had progressed at a steady rate of 0.19 °C/decade."

—> How does that square with the results in Samset et al 2022, quote: "The filtered GSTA yields a 2011–2020 warming rate of 0.24 °C per decade, and a 30-year trend that is indistinguishable from the unfiltered results (0.21 °C). Similarly, for the decade 2001–2010, which is in the center of the so-called global warming hiatus period, unfiltered HadCRUT5 observations yield a warming rate of 0.08 °C per decade, while the filtered results show 0.21 °C per decade, again similar to the most recent 30-year trend (1991–2020)." ? Seems somewhat inconsistent to me, despite it being two different time intervals. Perhaps you could elaborate.

Thanks for noticing this. The issue here is probably with the word "steady". We've tried to clarify, both here and elsewhere in the manuscript, that while we find that recent years are consistent with the 50-year *mean* rate of warming, recent decades have a consistently higher rate. We saw this when working on the analysis for Samset 2022 as well, but did not explicitly call it out. Hopefully our rewording (also in response to similar comments from Reviewer 1), and the addition of Table 1 which lists the warming rates explicitly, should help clarify the issue.

Lines 75-78:
"2021 began with a cold spell over northern Eurasia, driven by a collapse of the polar vortex, while SSTs are dominated by a very cold Central and Eastern Pacific. This had a strong cooling influence on global surface temperatures in Spring of […]"

—> Not sure if i got that completely right: Does the correction pick up the winter cooling over Eurasia (in that it projects onto the amplitude of the associated SST correction pattern)?

The sentence refers to the fact that the Eurasia cold spell will affect the overall GSTA. It will, as noted, project onto the SSTs and therefore be picked up by the filtering routine, but we cannot with the present toolkit tell how much is removed. Any residual will be part of the remaining interannual variability.

Samset et al 2022:
"The observed GSTA (from HadCRUT5) has first been detrended, via a 10-year moving boxcar average applied at each grid point (see Methods), to isolate (as far as possible) the influence of annual variability from decadal patterns and the effects of global warming since 1850."

—> In how far would the applied moving average pick up any accelerated warming during the last decade (in case there is one)? Guess it is related to the comment in lines 108-110, quote "We also note that the raw data consistently shows a stronger rate increase than the filtered data, indicating differences in the amount of warming filtered through the time period.", isn't it?

Yes, that is what is referred to. We did extensive sensitivity testing of the method for Samset 2022, and the boxcar averaging broadly does not remove rate changes near the endpoints. However we have not performed a rigorous analysis of how strong a rate change would need to be to be visible. This is planned for a future update where we utilize a broader sett of Green's functions, in collaboration with the GFMIP community.

Lines 117-121:
"We note, however, that, similarly to what is found for OHC, the strongest increase in rates seems to have occurred around the middle of the 50-year period, with a levelling off in later years. This means that while we do find a consistent increase in warming rate over the last 50 years, we see no recent "acceleration" of global surface warming in the sense of a continuous rate increase."

—> Which seems to be overall consistent with Forster et al 2023 (https://essd.copernicus.org/articles/15/2295/2023/#&gid=1&pid=1) … except for the influence of reduced sulfate emission from ships due to regulation changes in 2020. They admit that their estimate is rather crude and it stands to reason that the impact might be more non-linear than currently accounted for in their extrapolated aerosol forcing estimate. Might be worth adding a short note of caution in your manuscript in that regard as well (perhaps right at the beginning, e.g. lines 32-36)

Good point. While we do not want to dwell too much on the shipping emission discussion here, we have now acknowledged the potential non-linearity in aerosol forcing:

"Because of the rapid and potentially non-linear influence of aerosol emission changes on surface temperatures8, and the remaining uncertainty on the total anthropogenic aerosol forcing of the climate12, the near-term GSTA evolution can…"

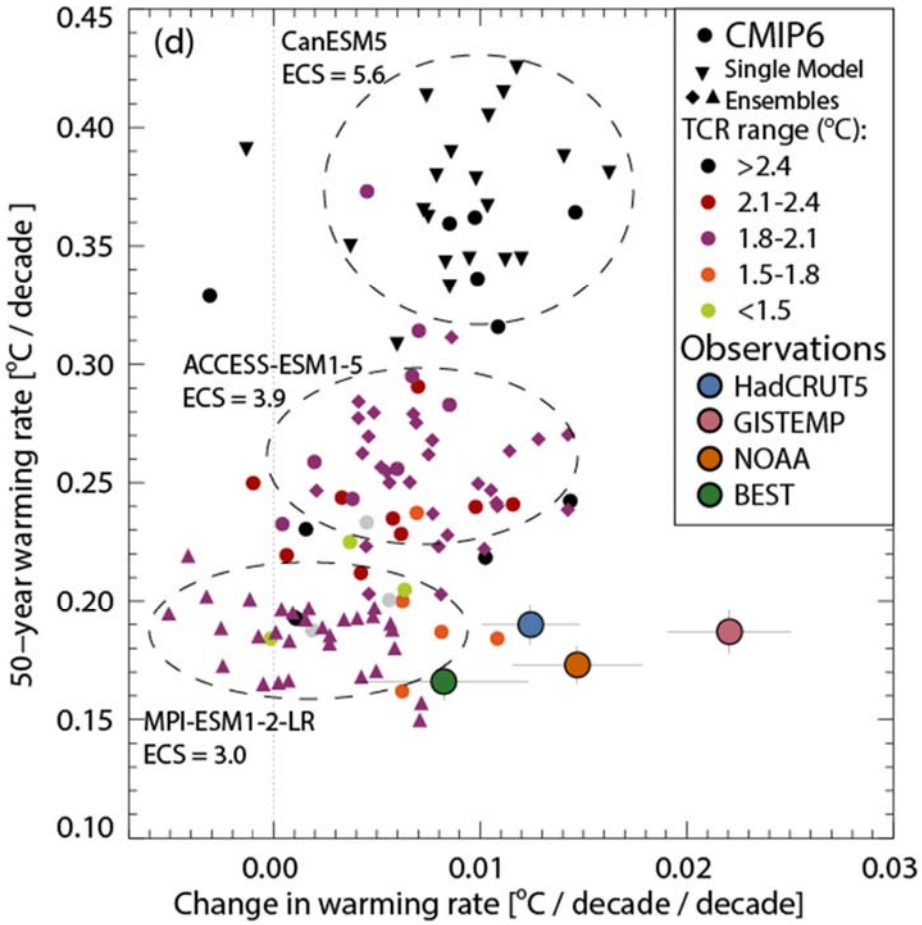
Little glitch in line 112: 'period'

Fixed

Lines 134-136:
“This result has been discussed in previous literature and is likely due to a combination of high climate sensitivities in some CMIP6 models, and that the realized SST patterns are not well captured by the model ensemble, notably in the Pacific.”

—> Any chance to present the CMIP6 results also as function of TCR rather than ECS? And picking nits here, the colours for the different ECS values in Fig 2d are difficult to distinguish. Perhaps there’s room to make that easier to read …

The figure has been revised, including new and hopefully clearer colors. We have TCR results too, and they are broadly consistent with what we see for ECS (see below). The newly added MPI-ESM1-5 and ACCESS ensembles are however quite close in TCR, while they differ by 1C in ECS, so this makes the current version of the TCR figure somewhat less informative. We have, however, added it to the supplementary materials and added a comment in the manuscript.



Decision letter and referee reports: second round

2nd Oct 23

Dear Dr Samset,

Your manuscript titled "Steady global surface warming through 2022, after a recent step-up in warming rate" has now been seen by our reviewer 1, 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 under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time, to edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

Please note that it may still be possible for your paper to be published before the end of 2023, but in order to do this we will need you to address these points as quickly as possible so that we can move forward with your paper.

EDITORIAL REQUESTS:

Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table".

*****Please take care to match our formatting and policy requirements. We will check revised manuscript and return manuscripts that do not comply. Such requests will lead to delays. *****

Please outline your response to each request in the right hand column. Please upload the completed table with your manuscript files as a Related Manuscript file.

If you have any questions or concerns about any of our requests, please do not hesitate to contact me.

SUBMISSION INFORMATION:

In order to accept your paper, we require the files listed at the end of the Editorial Requests Table; the list of required files is also available at https://www.nature.com/documents/commsj-file-checklist.pdf .

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

Best regards,

Heike Langenberg, PhD
Chief Editor
Communications Earth & Environment

On Twitter: @CommsEarth

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Reviewer Comments:
I thank the authors for addressing all of my comments and questions and especially for including additional climate model large ensembles in their analysis. In my view, this study is now acceptable for publication in Communications Earth & Environment.

COMMSENV-23-0856A

Response to reviewers

Dear editors,

There were no further comments from the reviewers, so for this final round we have only dealt with the points in the checklist. They include.

- Revised title and abstract, to comply with editorial policies
- Restructured (but otherwise unchanged) Supplementary Information
- Minor changes in wording to be consistent with the above changes

Otherwise, the paper is as before.

We thank you for the good and efficient handling of this manuscript.

Best regards, on behalf of the authors,

Bjørn H. Samset