Peer Review File

East Asian aerosol cleanup has likely contributed to the recent acceleration in global warming

Corresponding Author: Dr Bjørn Samset

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

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Dear Dr Samset,

Please accept our apologies for the delay in sending a decision on your manuscript. Your manuscript titled "East Asian aerosol cleanup has likely contributed to the recent acceleration in global warming" 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.

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. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

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,

Alice Drinkwater, PhD
Associate Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

EDITORIAL POLICIES AND FORMATTING

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Review of “East Asian aerosol cleanup has likely contributed to the recent acceleration in global warming” by Samset et al.

I have reviewed the authors' responses to comments on the initial manuscript and the revised manuscript. I feel my relatively minor concerns have been well addressed. I recommend accepting this timely manuscript with minor revisions. I like the added discussion on internal variability. I expect the research by Samset et al. to be of high interest to the wider climate research community and beyond, as the acceleration of global warming and its causes are of great importance to climate science and climate policy.

Minor points:

1) It is helpful that Figure 2 shows the surface temperature response for summer and winter. It would be helpful to add seasonal surface temperature responses also in Supplementary Figure 2 for individual models.

2) l351 ”reduction in aerosol-cloud interactions” - please rephrase
E.g. reduction in climate cooling through aerosol-cloud interactions

3) l45-46 the albedo of clouds -> the albedo and coverage of clouds

4) You might wish to fix the page numbering of the Supplementary Materials.

5) Supplementary Figure 6: Not sure if the chosen red colours bring out the differences between mean cloud fractions over the North Pacific in the best possible way.

6) Supplementary Figure 7:
The last column shows the difference between CERES and ERA5. ->
The last row shows the difference between CERES and ERA5.

7) Supplementary Table 3: Please note that the second table is empty!

Reviewer #3 (Remarks to the Author):

Samset et al. (2025)

I joined the review process at the current revision stage and have read both the revised manuscript as well as the authors' responses to the previous reviewers' comments.

In my view, this study could make a valuable contribution to advancing the understanding of aerosol climate impacts and informing future climate simulation designs. The analyses are generally well-constructed and clearly presented, although many of the conclusions remain indicative. That said, I do have a few additional questions regarding specific aspects of the analyses and discussions in the revised manuscript. Please see my detailed comments below (all line and figure numbers refer to the revised manuscript).

Major comments:

1.
I am curious about the rationale behind the selected time periods for both the observations and the RAMIP simulations. Reviewer #2 raised a similar point, and the authors responded that they chose 2035–2049 for RAMIP "as a compromise between wanting to see the full response to the emissions change, and reducing the influence of internal variability." However, this explanation remains unclear to me. According to Fig. 1, both the observations and RAMIP simulations exhibit similar emission reduction trajectories, despite the much slower decline in RAMIP. Given that 2035–2049 is already being used as a proxy for the observed changes, why don't you just use the full period of 2014–2049 for RAMIP? It seems to me that using the entire period better captures the emission evolution and further filters out the internal variability. I might be wrong, but I would appreciate a clearer justification.

Additionally, are the analysis results sensitive to the selected time periods in both the observations and the model simulations, particularly considering the relatively short observational period?

One more related question concerns the selection of different observational periods across the various figures. (i.e., 2014-2023 vs. 2005-2014 in Fig.1a; 2010–2023 vs. 1980-2010 in Fig. 3b; 2001-2010 vs. 2014-2023 in Fig. 4). Is there any specific reason for choosing different time windows in each case? Intuitively, one would expect the analyses to use consistent period selection.

2.
I am concerned about the convincingness of the authors' discussion between lines 256 and 267 regarding a similar warming pattern over the North Pacific in both observations and simulations. While the authors responded to Reviewer #3 for the discussions here, stating that they "did not indicate that the correlation implies causality", I am still concerned that the discussions are misleading and influenced by notable bias.

The pattern shown in Fig. 3c may be subject to significant bias due to the choice of time periods (i.e., comparing a 13-year trend from 2010–2023 with a 29-year trend from 1980–2009). Specifically, the observed PDO index (https://ds.data.jma.go.jp/tcc/tcc/products/elnino/decadal/pdo.html) shows a very weak linear trend during the 2010–2023 period because of the curvature of the time series during this short period, whereas there is a long-term negative trend in the PDO index during 1980–2009. Thus, the difference between these two periods does show a warming trend difference over the eastern North Pacific driven by PDO, as is shown in Fig. 3c. Therefore, I doubt how much EA aerosol contributes to the North Pacific maximum. I recommend that the authors interpret the similarities between Figs. 3c and 3d more cautiously, and explicitly acknowledge this caveat.

Fig.1: Time series of annual-mean PDO index, obtained from the JMA website(https://ds.data.jma.go.jp/tcc/tcc/products/elnino/decadal/pdo.html). Please check the attached file.

3.
The discussions on the difference between CERES and ERA5 in lines 311-323 are less misleading. The authors emphasize two “striking” features—a positive anomaly in the low cloud region of the eastern North Pacific and a negative anomaly over Eastern China—and suggest that these “may be indicative of regions where the recent aerosol changes are important for the reconstruction”. However, this interpretation appears to be selective. These two regions do not strikingly stand out as particularly exceptional in Fig. 4f, as several other regions show comparable strong differences. For example, eastern Australia and the eastern tropical Pacific show strong positive anomalies, yet are not discussed, likely because their signals do not align with the proposed emission trajectory discrepancies. This selective emphasis raises concerns about the potential bias and may mislead readers. I recommend reconsidering the discussion of the highly chaotic Fig. 4f and substantially rewriting the related discussions.

Specific comments:

Line 163: The phrase “above internal variability” is unclear to me. It seems to imply signal-to-noise comparison, but I did not see any discussions on it. Do the authors mean that the influence of internal variability has been largely removed by the multi-model-mean in Fig. 2a? Please clarify.

Line 168: It is a bit misleading to say the mean temperature evolves “smoothly”, as Fig. 2a shows large interannual variability, and the selected period is relatively short.

Line 170: The statement that the rate of change “is remarkably lower in the later decades” does not appear to be supported by Fig. 2a. There seems to be a roughly linear increase in temperature over 2015-2050. Please clarify/update the discussion here. By the way, the period should be changed to 2015-2049 to be consistent with other places.

Line 302: Replace “Eastern China” with “East Asian” for consistency.

Figure 1d: The caption does not include information about the standard deviation shown in the panel.

Figure 2b: Does panel b share the y-axis with panel a?

Figure 4c: Do panels c and d also show results for the period of 2035-2049? Please clarify in the caption.

Reviewer #4 (Remarks to the Author):

Review
This manuscript addresses a relevant question concerning the climatic implications of East Asian aerosol emission reductions. The use of multi-model Earth System simulations is appropriate, and the hypothesis—that regional aerosol reductions may have contributed to the recent acceleration in global warming—is both intriguing and important. However, the manuscript in its current form does not provide sufficient physical or quantitative support for its central conclusion. I therefore do not recommend it for publication at this stage. My main concerns are as follows:

The central claim that the reduction of East Asian sulfate aerosols has become a main driver of accelerated global warming since 2010 lacks sufficient physical and quantitative justification. Sulfate aerosols are known to exert a net cooling effect, and while their reduction may locally enhance warming by partially unmasking greenhouse gas (GHG) effects, the manuscript appears to overstate global-scale changes to this regional perturbation. This interpretation diverges from the broader literature, which consistently identifies the sustained rise in long-lived GHGs, particularly those emitted historically by developed countries, as the principal driver of global temperature increases.

The experimental design does not sufficiently distinguish between regional influences and global-scale climate responses. The perturbation applied to East Asian emissions is highly idealized, and there is little discussion of how internal variability and externally forced responses are disentangled. As a result, the strength of the proposed causal link between East Asian aerosol changes and the recent global warming trend appears overstated.

The analysis is based on a simplified setup and lacks adequate explanation of how the specific contribution of East Asian aerosol reductions is separated from other forcings. The relatively large uncertainty in the estimated global warming response (±0.05 °C) also raises concerns about the robustness of the findings. In the absence of a more rigorous and transparent attribution strategy, the conclusions remain speculative.

The climate effects of aerosols involve complex feedbacks through atmospheric circulation, cloud–radiative interactions, and air–sea interactions. The manuscript does not provide sufficient diagnostic analyses to clarify how a regional aerosol perturbation produces the proposed global-scale warming response. Without such mechanistic insight, the attribution lacks credibility and risks overstating the implications of the findings.

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Version 1:

Decision Letter:

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Dear Dr Samset,

Your manuscript titled "East Asian aerosol cleanup has likely contributed to the recent acceleration in global warming" has now been assessed by the editorial team. 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 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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Best regards,

Alice Drinkwater, PhD
Associate Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

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COMMSENV-25-1597-T: "East Asian aerosol cleanup has likely contributed to the recent acceleration in global warming", Samset et al.

Response to reviewers

Reviewer #1 (Remarks to the Author):

Review of “East Asian aerosol cleanup has likely contributed to the recent acceleration in global warming” by Samset et al.

I have reviewed the authors' responses to comments on the initial manuscript and the revised manuscript. I feel my relatively minor concerns have been well addressed. I recommend accepting this timely manuscript with minor revisions. I like the added discussion on internal variability. I expect the research by Samset et al. to be of high interest to the wider climate research community and beyond, as the acceleration of global warming and its causes are of great importance to climate science and climate policy.

We thank the reviewer again for their time and very helpful comments.

Minor points:

1) It is helpful that Figure 2 shows the surface temperature response for summer and winter. It would be helpful to add seasonal surface temperature responses also in Supplementary Figure 2 for individual models.

This has been added for JJA and DJF, consistent with Figure 2. See new Supplementary Figures 3-4.

2) l351 ”reduction in aerosol-cloud interactions” - please rephrase
E.g. reduction in climate cooling through aerosol-cloud interactions

Fixed.

3) l45-46 the albedo of clouds -> the albedo and coverage of clouds

Fixed.

4) You might wish to fix the page numbering of the Supplementary Materials.

Thanks. There was a conversion error in the submitted Supplementary Materials that led to some issues with page numbering and an empty table. These have been fixed.

5) Supplementary Figure 6: Not sure if the chosen red colours bring out the differences between mean cloud fractions over the North Pacific in the best possible way.

We tried a number of color scales here, balancing the wish to bring out the North Pacific differences as well as the overall cloud climatology of the models. We've reverted to the more traditional blue/white color scale in the revised version, where – in particular – the lack of stratocumulus clouds in GISS-E2-1-G, and to a lesser extent in CNRM-ESM2-1, is apparent as a continuous blue band of low cloud fraction.

6) Supplementary Figure 7:
The last column shows the difference between CERES and ERA5. ->
The last row shows the difference between CERES and ERA5.

Fixed.

7) Supplementary Table 3: Please note that the second table is empty!

Thanks. There was a conversion error in the submitted Supplementary Materials that led to some issues with page numbering and an empty table. These have been fixed.

Reviewer #3 (Remarks to the Author):

I joined the review process at the current revision stage and have read both the revised manuscript as well as the authors' responses to the previous reviewers' comments.

In my view, this study could make a valuable contribution to advancing the understanding of aerosol climate impacts and informing future climate simulation designs. The analyses are generally well-constructed and clearly presented, although many of the conclusions remain indicative. That said, I do have a few additional questions regarding specific aspects of the analyses and discussions in the revised manuscript. Please see my detailed comments below (all line and figure numbers refer to the revised manuscript).

Major comments:

1.
I am curious about the rationale behind the selected time periods for both the observations and the RAMIP simulations. Reviewer #2 raised a similar point, and the authors responded that they chose 2035–2049 for RAMIP "as a compromise between wanting to see the full response to the emissions change, and reducing the influence of internal variability." However, this explanation remains unclear to me. According to Fig. 1, both the observations and RAMIP simulations exhibit similar emission reduction trajectories, despite the much slower decline in RAMIP. Given that 2035–2049 is already being used as a proxy for the observed changes, why don't you just use the full period of 2014–2049 for RAMIP? It seems to me that using the entire period better captures the emission evolution and further filters out the internal variability. I might be wrong, but I would appreciate a clearer justification.

All time periods are chosen for physically based reasons. We have attempted to clarify this where the various periods are used in the document. Essentially, we're trying to answer two questions:

1: What is the overall climate response to a sustained reduction in East Asian aerosol emissions?
2: How can the answer to question 1 be related to recent global warming?

The RAMIP time period if 2035-2049 is chosen to answer question 1. Firstly, the endpoint (2049) is set by data availability. This is the last year where we have 10 simulations from each of the 8 models, and can still do a DJF season that extends into the subsequent year (2050). Next, our signal is a transient emissions change, starting at zero in 2015 and progressing through the entire simulation period – however with the majority (~75%) of the change being done by 2035 (Figure 1c). If we look at the entire 2015-2049 period, we would include in the average the first 20 years where the signal is still evolving, which is different to the question of the overall sustained response we want to find. However, given that the climate response to aerosol changes is generally thought to evolve rapidly (on a scale of years; see e.g. Stjern et al. 2023; doi:10.1175/jcli-d-22-0513.1; the15-year 2035-2049 period should serve as a proxy for the full, sustained response. An ocean related response will come in addition, but only on decadal to centennial timescales, which is beyond what we have simulated in RAMIP – and also beyond what has had time to occur in real life in response to recent East Asian emission changes.

For question 2, similarly, most of the emissions reduction in East Asia was (according to CEDS and other inventories) accomplished by around 2017, with only minor reductions thereafter (Figure 1c). By the same logic of a relatively rapid response, any climate response to this sustained emissions change should have contributed to global mean surface temperature in the period leading up to the present. Hence, we make the comparison of the 2035-2049 RAMIP period with the current warming by converting it to a warming rate between 2010 and 2023.

An alternative could be to use warming rate calculations directly. We have done this, and it makes no appreciable difference to the overall conclusions other than to heighten the uncertainty since this analysis is more sensitive to the dominating internal variability of the first, low-signal years of RAMIP simulations.

Additionally, are the analysis results sensitive to the selected time periods in both the observations and the model simulations, particularly considering the relatively short observational period?

Neither the overall conclusions nor the absolute numbers cited are particularly sensitive to the choice of time periods. The most crucial number is the starting date of the observational time series, which we chose to be 2010 to stay consistent with most Chinese literature on East Asian aerosol emissions reductions. However even here, setting the start date for East Asian aerosol emission reductions to e.g. 2013 (the most marked downturn in Figure 1c) does not change the main conclusion that the RAMIP temperature change is consistent with the extra warming from the recent "uptick" in global mean surface temperatures.

One more related question concerns the selection of different observational periods across the various figures. (i.e., 2014-2023 vs. 2005-2014 in Fig.1a; 2010–2023 vs. 1980-

2010 in Fig. 3b; 2001-2010 vs. 2014-2023 in Fig. 4). Is there any specific reason for choosing different time windows in each case? Intuitively, one would expect the analyses to use consistent period selection.

This, for the most part, is due to a combination of figure readability and data availability.

For Figure 1a, we are constrained by the length of the MODIS data series, and opted to split it approximately in half to show differences rather than use 2005-2009 as a short baseline. The main aim of the figure is to show a geographical pattern, rather than an absolute change, so we consider this a worthwhile tradeoff in order to reduce the influence of interannual variability.

For Figure 3b, we are discussing the long-term vs recent rates of warming. Here, given the longer availability of temperature data, 1980 was chosen partly to use the typical 30-year average for long-term climate quantities, and partly to stay well within the previously well documented period since 1970 of near-constant global mean surface warming rates. The 2010-2023 period was, as documented above, chosen to be consistent with literature on when the East Asian emissions reductions started.

For Figure 4e, the reasoning is similar to MODIS; we wanted to maximise the usage of available data, while providing as clear a picture of the change as possible (i.e the longest possible consistent periods before and after the start of the emissions change). Again, the conclusions are not sensitive to this choice of start and end years, within reasonable perturbations.

2.
I am concerned about the convincingness of the authors' discussion between lines 256 and 267 regarding a similar warming pattern over the North Pacific in both observations and simulations. While the authors responded to Reviewer #3 for the discussions here, stating that they "did not indicate that the correlation implies causality", I am still concerned that the discussions are misleading and influenced by notable bias.

The pattern shown in Fig. 3c may be subject to significant bias due to the choice of time periods (i.e., comparing a 13-year trend from 2010–2023 with a 29-year trend from 1980–2009). Specifically, the observed PDO index (https://ds.data.jma.go.jp/tcc/tcc/products/elnino/decadal/pdo.html) shows a very weak linear trend during the 2010–2023 period because of the curvature of the time series during this short period, whereas there is a long-term negative trend in the PDO index during

1980–2009. Thus, the difference between these two periods does show a warming trend difference over the eastern North Pacific driven by PDO, as is shown in Fig. 3c. Therefore, I doubt how much EA aerosol contributes to the North Pacific maximum. I recommend that the authors interpret the similarities between Figs. 3c and 3d more cautiously, and explicitly acknowledge this caveat.

Fig.1: Time series of annual-mean PDO index, obtained from the JMA website(https://ds.data.jma.go.jp/tcc/tcc/products/elnino/decadal/pdo.html). Please check the attached file.

We thank the reviewer for pointing this out. PDO is the main decadal mode of variability we referred to in our existing caveat, but we agree that this can and should be made clearer. In the revised version, we have acknowledged this explicitly, and also rearranged the discussion of Figure 3:

Since aerosol changes have regionally heterogeneous climate influences, as shown above, we next investigate the correspondence of simulated changes to observed regional warming rate. In Figure 3c, we show the observed difference between the 2010-2023 and 1980-2009 warming rates, in the four reconstructions. See Supplementary Figure 4 for individual time series and the two trend periods in isolation. In Figure 3d, we show the annual mean surface warming pattern from East Asian aerosol emission reductions in RAMIP. In both cases, we find a pattern of observed warming in the North Pacific, with two distinct maxima: one along the East Asian coastline, and the other following the west coast of North America, extending west to the center of the Pacific. This shows that the simulated increased global mean warming rate comes from a geographical region where observations also find an elevated warming rate since 2010, relative to previous decades. See Supplementary Figure 8 for the transient evolution of regional means (Western and Eastern North Pacific) for individual models. An important caveat, however, is that that for the observations, a 13-year trend will be strongly influenced by decadal scale variability, notably the Pacific Decadal Oscillation (PDO) which has changed markedly over the time period studied here[34].

3.

The discussions on the difference between CERES and ERA5 in lines 311-323 are less misleading. The authors emphasize two “striking” features—a positive anomaly in the low cloud region of the eastern North Pacific and a negative anomaly over Eastern China—and suggest that these “may be indicative of regions where the recent aerosol changes are important for the reconstruction”. However, this interpretation appears to be selective. These two regions do not strikingly stand out as particularly exceptional in Fig. 4f, as several other regions show comparable strong differences. For example, eastern Australia and the eastern tropical Pacific show strong positive anomalies, yet are not discussed, likely because their signals do not align with the proposed emission trajectory discrepancies. This selective emphasis raises concerns about the potential bias and may mislead readers. I recommend reconsidering the discussion of the highly chaotic Fig. 4f and substantially rewriting the related discussions.

It is true that there are many features in this plot. We only aimed to point out, for further study, the ones where we have an expectation that East Asian aerosols have a direct influence. The discussion has been restructured and modified to hopefully make this more clear, and now reads as follows:

ERA5 does include a treatment of aerosols, but it uses a CMIP5 era combination of historical emissions up to 2009 and subsequently the RCP emissions 35. These pathways do not include the recent reductions in East Asian emissions. Hence, the difference between CERES and ERA5 may be indicative of regions where the recent aerosol changes are important for the reconstruction, and have an influence on observed rates of change of TOA radiative fluxes. To see whether we can see any indication of this, we show the difference between CERES and ERA5 in Figure 4f. Among the many features that stand out here, we find a positive anomaly in the low cloud region of the eastern North Pacific, as well as a negative anomaly over Eastern China. We note that these are regions where downwind effects of East Asian aerosol emissions would be expected to contribute, and that this is indeed where RAMIP simulations indicate an influence from aerosol-cloud interactions. Recall, however, the previous caveat that PDO also changes in the time periods studied here, and that trends in sea surface temperatures will also influence ERA5 reconstructions. This means that we cannot make a clear attribution of the observed changes to aerosol emissions changes with this method, but rather point this out as an area for further, more in-depth studies.

Specific comments:

Line 163: The phrase “above internal variability” is unclear to me. It seems to imply signal-to-noise comparison, but I did not see any discussions on it. Do the authors mean that the influence of internal variability has been largely removed by the multi-model-mean in Fig.

2a? Please clarify.

Thanks. This has been clarified to "*with a mean change that extends beyond the interannual variability in the multi-model, multi-ensemble-member mean*".

Line 168: It is a bit misleading to say the mean temperature evolves "smoothly", as Fig. 2a shows large interannual variability, and the selected period is relatively short.

Changed to *"in overall correspondence"*.

Line 170: The statement that the rate of change "is remarkably lower in the later decades" does not appear to be supported by Fig. 2a. There seems to be a roughly linear increase in temperature over 2015-2050. Please clarify/update the discussion here. By the way, the period should be changed to 2015-2049 to be consistent with other places.

The comment about the later decades has been removed. The time period has been corrected.

Line 302: Replace "Eastern China" with "East Asian" for consistency.

In this case, we mean Eastern China in particular, rather than the full region.

Figure 1d: The caption does not include information about the standard deviation shown in the panel.

Fixed.

Figure 2b: Does panel b share the y-axis with panel a?

Yes. Added to the caption.

Figure 4c: Do panels c and d also show results for the period of 2035-2049? Please clarify in the caption.

Yes. Added to the caption.

Reviewer #4 (Remarks to the Author):

This manuscript addresses a relevant question concerning the climatic implications of East Asian aerosol emission reductions. The use of multi-model Earth System simulations is appropriate, and the hypothesis—that regional aerosol reductions may have contributed to the recent acceleration in global warming—is both intriguing and important. However, the manuscript in its current form does not provide sufficient physical or quantitative support for its central conclusion. I therefore do not recommend it for publication at this stage. My main concerns are as follows:

We thank the reviewer for their time and considerations. However, we are somewhat surprised by the comments, as they do not seem to reflect the full contents of our manuscript.

We provide extensive quantitative support for the climate response to a reduction of East Asian aerosol emissions through the use of 8 Earth System Models and 80 total realizations. Outside of a handful of large ensemble efforts (e.g., CESM2 Large Ensemble), we are unaware of a project with such a large number of realizations. Furthermore, unlike these large ensemble efforts, which focus on a single model, we have 8 different models which allows us to not only provide a robust estimate of internal climate variability (via the 80 realizations), but also intermodel spread. Each of our models essentially represents their CMIP6-era version, developed by modeling centers around the world, and used extensively by IPCC AR6 as well as thousands of peer-reviewed scientific papers over the last ~5-6 years to detect, understand and quantify historical and 21st century climate change. All models include a representation of interactive aerosol, including emission, transport, removal (and SO_2 chemistry) and direct and indirect aerosol radiative effects. All models have been rigorously developed and evaluated during the model development stage and subsequently during the last half decade. It is true that these models are a simplification of the real world, and that they possess shortcomings and uncertainties. But they remain the golden standard for detecting and attributing climate change. Including the impact of East Asian aerosol emission reductions on near-surface air temperature.

The central claim that the reduction of East Asian sulfate aerosols has become a main driver of accelerated global warming since 2010 lacks sufficient physical and quantitative justification. Sulfate aerosols are known to exert a net cooling effect, and while their reduction may locally enhance warming by partially unmasking greenhouse gas (GHG) effects, the manuscript appears to overstate global-scale changes to this regional perturbation. This interpretation diverges from the broader literature, which consistently identifies the sustained rise in long-lived GHGs, particularly those emitted historically by developed countries, as the principal driver of global temperature increases.

We are nowhere disputing the fact that the sustained rise in long-lived greenhouse gases is the principal driver of the historical increase in global mean near-surface air temperatures. Nor that the main emitters through history are from developed countries, though this is not the focus of the present paper. We are showing, based on our simulations, that the recent acceleration in the rate of global warming is consistent with the contemporaneous reductions of East Asian aerosol emissions. We also note (as included near the end of the paper), that this "effect" is relatively short-lived, i.e. we do not expect this reduction in East Asian aerosol emissions to continue amplifying the rate of warming thru the entire 21st century.

It is true, aerosol reductions will generally lead to larger warming near their source (e.g., Fig. 2). But aerosols are also transported away from their source regions, where they can significantly impact remote temperatures. In some regions, this remote effect may be comparable to the local effect. In the paper, we highlight the North Pacific as one of these regions, and relate this to aerosol-cloud effects. Also clear in our analysis is the importance of climate feedbacks for amplifying remote responses. In the Arctic, for example, East Asian aerosol emission reductions drive large warming, particularly during DJF (Fig. 2d).

Further, the mechanism behind a global-scale surface temperature response to regional aerosol changes, including those from East and South Asia, has been previously investigated in the literature. For example, Merikanto et al. 2021 (https://doi.org/10.5194/acp-21-5865-2021) performed idealized aerosol removal experiments using two independent Earth system models (ECHAM6.1 and NorESM1) and found a global mean surface warming of 0.26 ± 0.04 °C in response to the removal of East and South Asian anthropogenic aerosols. This warming was not confined to the source regions, but extended across the entire Northern Hemisphere and was amplified over the Arctic. Their analysis showed that atmospheric energy transport and radiative feedbacks redistribute the temperature response well beyond the emission regions. These findings are consistent with our conclusion that reductions in East Asian aerosols can contribute significantly to recent global warming acceleration, especially when considering the role of fast climate feedbacks. Similar studies were done by Westervelt et al.

(https://doi.org/10.5194/acp-18-12461-2018), Persad et al. (https://doi.org/10.1038/s41467-018-05838-6), and the PDRMIP collaboration (https://doi.org/10.1175/JCLI-D-17-0439.1).

It is unclear how we overstate the amount of global mean warming in response to this regional perturbation. It is true that the bulk of the warming to this regional perturbation is confined to the Northern Hemisphere (consistent with the hemisphere of the perturbation) (Fig. 2c-d).

The experimental design does not sufficiently distinguish between regional influences and global-scale climate responses. The perturbation applied to East Asian emissions is highly idealized, and there is little discussion of how internal variability and externally forced responses are disentangled. As a result, the strength of the proposed causal link between East Asian aerosol changes and the recent global warming trend appears overstated.

We clearly acknowledge that the East Asian perturbation represents an upper-bound based on SSP3-7.0 versus SSP1-2.6. And we acknowledge that this is idealized, as we implicitly assume that SSP1's air pollutant legislation and technological progress can be achieved in the SSP3 world. But as noted in the paper, the magnitude of this perturbation (e.g., 20Tg SO_2/year), or a 75% reduction, is essentially what occurred in the real world from ~2010 to near present day. We are using the 8 models and 80 realizations to quantify the climate impacts of this real-world aerosol emission reduction.

Internal variability is quantified from the spread of individual ensemble members. As noted above, we have 10 realizations for each of the 8 models, which represents a relatively unprecedented number of 80 realizations in total. The externally forced response for each model is quantified as the model mean signal. The externally forced response for all of the models is quantified as the multimodel mean. By averaging over the realizations, the effects of internal climate variability are reduced and the externally forced signal is more easily identified. We acknowledge that 10 ensemble members is likely the minimum number. Ideally, more realizations would be performed, but this is not very practical in reality due to computational restrictions.

The experimental design does distinguish between regional influences (the forcing) and global-scale climate responses. This is what the experiments are designed to do. We perform regional aerosol simulations (the regional forcing aspect) in a global climate model (the global-scale climate response aspect).

The analysis is based on a simplified setup and lacks adequate explanation of how the specific contribution of East Asian aerosol reductions is separated from other forcings. The relatively large uncertainty in the estimated global warming response (±0.05 °C) also raises concerns about the robustness of the findings. In the absence of a more rigorous and transparent attribution strategy, the conclusions remain speculative.

We have added more information on how the East Asian aerosol reductions are separated from other forcings. This is accomplished by differencing two simulations (ssp370-EAS126aer and ssp370) that are identically forced in all ways, except ssp370-EAS126aer has aerosol emissions that follow the SSP1-2.6 pathway (with aggressive air quality policies and strong aerosol emissions reductions due to swift ramping up of end-of-pipe measures for air pollution control) as opposed to the SSP3-7.0 pathway. We already acknowledge above that this is simplified in that we assume SSP1's air pollutant legislation and technological progress can be achieved in the SSP3 world. But again, this assumption is not particularly relevant, since East Asian aerosol emissions decreased in the real-world very similarly to our setup.

We are unsure how the reviewer describes the uncertainty in the estimated global warming response as "large". Certainly, at face value, 0.05°C is not large. Nor is it large compared to uncertainty estimates of the historical global warming trend. If the reviewer is suggesting 0.05°C is large relative to the signal of 0.07°C, we agree that they are comparable numbers. However, this represents a significant signal. Stated through other means, we note that 7 of the 8 models show significant global mean warming in response to East Asian aerosol emission reductions. Furthermore, 70 of the 80 realizations likewise support this result. Thus, it is true that we do not have 100% model or realization agreement. Nonetheless, we clearly have a robust result with 88% model agreement on the warming, as well as 88% realization agreement on the warming.

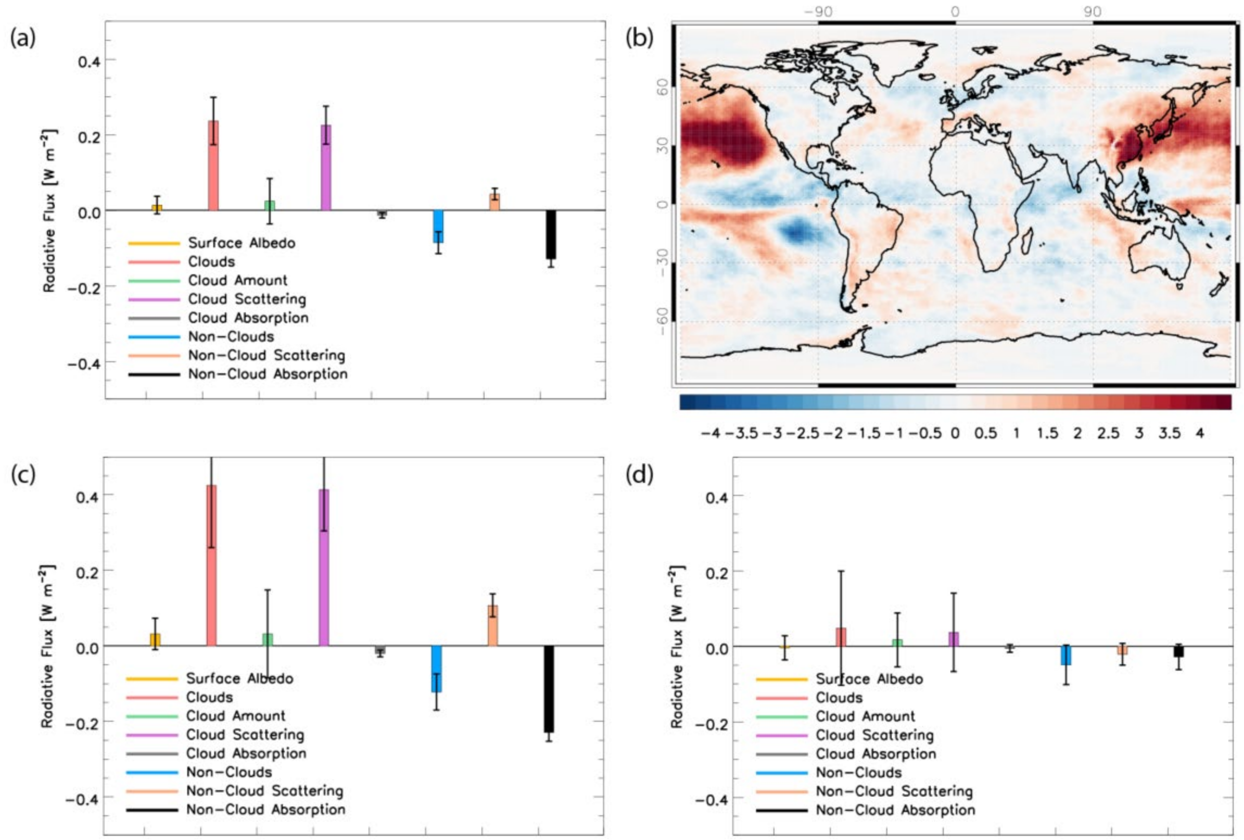
Again, to our knowledge, this is by far the largest dataset that has been applied to such a question, and hence will have much broader information – and diversity – than the simplified emulator based studies that have been adopted in the past. The uncertainty estimate is a strength of the analysis, and a key part of the advance presented in this paper, not a lack of robustness.

The climate effects of aerosols involve complex feedbacks through atmospheric circulation, cloud–radiative interactions, and air–sea interactions. The manuscript does not provide sufficient diagnostic analyses to clarify how a regional aerosol perturbation produces the proposed global-scale warming response. Without such mechanistic insight, the attribution lacks credibility and risks overstating the implications of the findings.

As noted above, the main mechanistic reasoning is the clarity of the experimental setup where the only change between the signal and the background simulations are the East Asian aerosol emissions. Further, the overall mechanistic responses to aerosol emissions in CMIP6 generation models (including the eight ones used here) have already been studied by the aerosol-climate community through efforts such as AeroChemMIP, RFMIP and Chapter 6 in the Working group I contribution to the 6th IPCC Assessment Report.

For RAMIP, a full, model-by-model mechanistic breakdown for each region is upcoming. The present manuscript is only the first of a range of studies, but where we highlight a conclusion of broad societal relevance. The full breakdown is beyond the scope of the present paper, and also not necessary for the overall conclusion. However as an example of the ongoing analyses, that fully supports the conclusions presented in the present work, we show below a figure from an Approximate Partial Radiative Perturbation (APRP) analysis for the CESM2 model. APRP breaks down the shortwave response to a model perturbation into a range of components, including surface albedo and a range of cloud effects. See e.g. Taylor et al. 2007, Journal of Climate, 10.1175/JCLI4143.1.

To first order, the climate responds to aerosol perturbations through changes in shortwave radiation. Response Figure 1 shows the APRP breakdown into SW components for CESM2, globally, and for the northern and southern hemispheres separately. The main takeaway is that clouds are the key modulator of SW changes, and especially clouds in the local and downwind regions of our emissions perturbation box (East Asia). Further, it is predominantly cloud scattering that contributes, albeit with the expected offsetting factor from absorption given that our perturbations include both sulfate and black carbon emissions.



Response Figure 1: APRP analysis of the RAMIP East Asia perturbation, for the CESM2 model (10 ensemble members). (a) Short wave radiative flux perturbation in the East Asia box for a range of factors. (b) Map (2035-2049) of total cloud fraction, the strongest radiative flux contributor from panel (a). (c) As (a), for the Northern Hemisphere. (d) As (a), for the Southern Hemisphere.

Again, going through an in-depth analysis of atmospheric circulation, clouds and air-sea interactions is important, but given the extensive nature of such analyses, they will be addressed in subsequent RAMIP papers.

Response to reviewers

There were no further reviewer comments for this iteration.

The changes to this version consist solely of editorial changes to comply with the Editorial Requests Checklist.