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Peer Review File

Accelerated North Atlantic surface warming reshapes the Atlantic Multidecadal Variability

Corresponding Author: Professor Davide Zanchettin

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

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Version 0:

Decision Letter:

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Dear Professor Zanchettin,

First of all, please allow me to apologise sincerely for the long delay in sending a decision on your manuscript titled "Accelerated North Atlantic surface warming redefines the Atlantic Multidecadal Variability", which is due to unusual difficulties in finding reviewers. It 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. Please ensure you address all the points raised, and 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:
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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,

Heike Langenberg, PhD
Chief Editor
Communications Earth & Environment

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Comments to authors:

In their paper titled «Accelerated North Atlantic surface warming redefines the Atlantic Multidecadal Variability», the authors examine the Atlantic Multidecadal Variability (AMV) by comparing observed sea surface temperature observations over the 20th century with multimodel 20th century and future simulations from CMIP6 climate models. They suggest that natural fluctuations of AMV is currently damped meaning that natural variations in AMV will be overwhelmed by long-term emission-driven warming. The paper address the nature of the AMV, including the relative role of external forcings or intrinsic factors, which is an important topic that feeds directly into the current climate change debate. As such the paper is certainly fitting for this journal. The paper is well written in it current form, and the statistical methodology is valid. However, I have some critical comments that lead me to suggest major revision.

1. Is the Atlantic SST accelerating? A key premise for this paper is the claim that observations show an ongoing acceleration of the warming. While this certainly may be true, the observational basis for this is still very short (a few years) as seen in Fig. 1a. You mention potential short term events (like Honga Tonga eruption) in the Introduction. Couldn't this «acceleration» be due to short-term events like this? Or what about ENSO? Some more ellaboration around this would be nice.
2. Detrending of Atlantic SST. This is a long standing issue regarding how to examine AMV. As you mention in the paper the way you detrend can impact how much the recent NA SST trend can be attributed to AMV. In the paper you only look at linear and quadratic fit of the Atlantic SST. Given the importance of this, I would really like to see this explored a bit further. For instance, Sutton et al. 2018 use a different definition of AMV where they take Atlantic mean temperaure minus the global mean excluding the North Atlantic. This definition has been used extensively in the literature. This definition should therefore also be included and tested.
3. Climate models and skill. I find the large diversity of skills in how models represent the AMV quite interesting. Are there characteristics about the «skillful» sub-ensemble models that stands out? Higher resolution? Better resolved stratosphere? Others? Or is this just «luck of the draw»? Kravtsov et al. 2018 stated the following about CMIP5 models: «While climate models exhibit various levels of decadal variability and some similarities to observations, none of the simulations considered match the observed signal in terms of its magnitude, spatial patterns and their sequential time development». Can you comment on this? It seems to me that the spread in the models are still large. A more detailed discussion around this (strengths and weaknesses with the current models) would be welcomed. Also, there is no figure showing the simulated spatial AMV patterns. I think this is required.
4. Climate models and the signal-to-noise paradox. I think for a Nature paper on this topic it would be really beneficial if your multi-model analysis could be put into context with the so called «signal-to-noise paradox» (Scaife and Smith 2018), i.e. the widespread inconsistency between the low strength of predictable signals in climate models and the relatively high level of agreement they exhibit with observed variability of the atmospheric circulation.
5. The title. Redefining AMV is a pretty strong statement. How do you justify this? The damped oscillation framework has been suggested before (Omrani et al. 2022). The importance of external forcing for AMV is also not new. So how are you actually redefining AMV?
6. External forcing vs intrinsic ocean circulation dynamics. On line 82-83 you state that the AMV is strongly imprinted by external forcing. How does this manifest itself? Direct radiative forcing of say tropical Atlantic through aerosols, or indirectly through changes in ocean circulation (say AMOC)? Overall I would like to see some figures and discussion about the role of ocean circulation, e..g. the Atlantic Merdional Overtruning Circulation (AMOC), in the models. Are AMOC and AMV in phase? Out-of-phase? Or doesn't the ocean circulation matter at all for AMV?
7. Damped oscillation. Related to the previous point. The coupled damped oscillation framework was recently published in npj (Omrani et al. 2022) with the main author of this paper as one of the co-authors. In that framework, the coupled stratosphere-troposphere-ocean pathway is key to the mechanism, linking a wave-induced downward propagating positive stratosphere/troposhere-coupled Northern Annular Mode with AMOC. If the AMV is purely external forced, why talk about a damped oscillation at all? Or is the external forcing imprinting on the AMV in a different way, through phaselocking or something? What if so, could be the mechanism?
8. My final point is one thing that annoys me about this paper, and which I will have to insist be rectified in order for me to accept this paper. Why do you only use the SSP5-8.5 scenario for the models? It is now well established that this is an implausible scenario (Hausfather Z and Peters GP 2020; Pielke Jr, R. et al. 2022; IPCC 2021, and many others), so in my opinion there is no excuse for climate modellers to keep sticking to this scenario. If you want to use, as you say in the abstract, «realistic» climate simulations for the near future then SSP2-4.5 is a much better option. You might be right that «other scenarios are not expected to differ significantly from the ssp585 scenario ...», but then why not just show it? You open yourself up for a lot of criticism by sticking to this scenario only.

That a side, I would like to thank the authors for a nice work and a very interesting paper. I will be happy to look at a revised version.

References:

Sutton RT, et al. (2018): Atlantic multidecadal variability and the UK ACSIS program, BAMS.
Kravtsov S, et al. (2018): Global-scale multidecadal variability missing in state-of-the-art climate models, npj Climate and Atmospheric Science.
Scaife A and Smith D (2018): A signal-to-noise paradox in climate science, npj Climate and Atmosphere Science.
Omrani NE, et al. (2022): Coupled stratosphere-troposphere-Atlantic multidecadal oscillation and its importance for near-future climate projection, npj Climate and Atmosphere Science.
Hausfather Z and Peters GP (2020): RCP8.5 is a problematic scenario for near-term emissions, Proc. Natl Acad. Sci., 117, 27791-2.
Pielke Jr. R, et al. (2022): Plausible 2005-2050 emission scenarios project between 2 degC and 3 degC of warming by 2100, Environ. Res. Lett., 17, 024027.
IPCC (2021): Climate Change 2021: The physical science basis. Contribution of Working Group I to the sixth assessment report of the intergovermental panel on climate change.

Reviewer #2 (Remarks to the Author):

This study examines the relative contribution of the Atlantic Multidecadal Variability (AMV) and warming trend due to increasing greenhouse gases to the North Atlantic (NA) SST warming during the early 21st century using both observations and CMIP6 simulations. The authors conclude that the contribution of AMV to the total NA SST warming is smaller than previously thought. They further suggest that, based on analyses of the CMIP6 simulations, AMV fluctuations are strongly damped under warming.

The findings of this study potentially improve our understanding of AMV dynamics in the future under global warming. However, The authors’ conclusions heavily rely on statistical methods and they do not provide mechanistic explanations. Also, there is a question mark regarding the methodologies used for the CMIP6 simulation analyses. Therefore, the results of the study are not yet fully convincing because of the reasons detailed below. Therefore, I recommend a major revision of the manuscript before considered for publication.

Major comments:
My major concerns are around the statistical methods that authors used to extract AMV from the CMIP6 simulations and the interpretation of the extracted AMV signal. The authors used a statistical model (DLM) to estimate AMV from the simulations. It is understandable to use a statical model to do so given a small number of realizations for some models. However, there are also a few models that have, at least, 25 realizations, which should be enough to obtain the “true” warming signals. Is the warming signal estimated from DLM close enough to these true signals? Most models in Fig. 2a show an almost linear trend during the second half of the 21st century, but the stochastic trends from DLM shown in Fig. 2b are mostly nonlinear and become flattened toward 2100. So, it is not surprising to see some low-frequency variability when the stochastic trends are removed from NA SST. This could be problematic for the sub-ensemble because there are only 18 realizations. That is, even after the ensemble average, because of the small ensemble size, some residual can remain. How do authors make sure that this is a “real” AMV signal and there is an oscillatory behavior?

Even if the oscillatory signal is real, what is the mechanism that generates the oscillations? The phase of the sub-ensemble and full ensemble is very close during the 20th century; amplitude is just larger in the sub-ensemble. This suggests that the sub-ensemble is a mix of both externally forced and internally generated signals. One possible explanation seems to be that in these realizations, the internally generated AMV is synchronized with externally forced AMV. Then, could the damped warm AMV phase during the early 21st century be just due to internally driven AMV? In any case, some explanations along with supporting analyses seem to be required. As it is, the damped oscillation of AMV is too hand-wavy due to the uncertainty in the statistical method used to remove the trends and the lack of mechanistic explanations.

Authors argue that AMV is better defined after a quadratic fit is removed than a linear fit based on a few metrics without much detail of these metrics. How these metrics are calculated and why do higher scores mean better performance of the quadratic fit than the linear fit? More details of these metrics should be given (ideally in the method section) and the justification of the quadratic fit over the linear fit needs to be provided.

Minor suggestions:
ll. 44-48: I can’t follow this sentence. It is too long and seems to be broken.
ll. 34-35: There are also studies that contend the aerosols-driven AMV cold phase (Zhang et al. 2013; Kim et al. 2018)
ll. 40-41: There are studies (Robson et al. 2018; Yeager 2020) that predict a cooling in the upper ocean heat content in the subpolar North Atlantic during the early 21st century.
l. 91: Fig. 2d -> Fig. 1c?

References
Kim, W. M., Yeager, S. G. & Danabasoglu, G. Key Role of Internal Ocean Dynamics in Atlantic Multidecadal Variability During the Last Half Century. Geophys. Res. Lett., 45, 13449-13457 (2018).
Robson, J., Polo, I., Hodson, D. L. R., Stevens, D. P. & Shaffrey, L. C. Decadal prediction of the North Atlantic subpolar gyre in the HiGEM high-resolution climate model. Clim. Dyn., 50, 921-937.
Yeager, S. The abyssal origins of North Atlantic decadal predictability. Clim. Dyn., 55, 2253–2271 (2020).
Zhang, R., et al. Have aerosols caused the observed Atlantic multidecadal variability? J. Atmos. Sci., 70, 1135– 1144 (2013).

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Version 1:

Decision Letter:

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Dear Professor Zanchettin,

Your manuscript titled "Accelerated North Atlantic surface warming reshapes the Atlantic Multidecadal 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,

Heike Langenberg, PhD
Chief Editor
Communications Earth & Environment

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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

In the revised manuscript "Accelerated North Atlantic surface warming reshapes the Atlantic Multidecadal Variability" the authors have addressed all of my main concerns from the original review. I am especially pleased to see a more thorough discussion about the uncertainties in the models, the role of ocean circulation (AMOC) and the inclusion of the SSP245 scenario in the analysis. Based on this I am therefore happy to recommend the paper for publication.

I have just a few minor comments/corrections for the authors to consider:

Line 56: I don't like a sentence starting with a number. I suggest something like: "A total of 176 simulations from 29 models are considered."
Line 93: It is not clear here that "deviation of North Atlantic SST from the global-mean SST" is actually deviation from global mean SST EXCLUDING the AMV domain. This is correctly described in the Methods section, but is unclear here. Please correct.
Line 128: "...what done for..." should be "...what is done for...".
Line 216: "...what obtained for ssp585..." should be "...what is obtained for ssp585 ..."
Line 237-241: I am not sure about the use of colon and semi-colon here. I'd rather just separate the sentences with a period.
Line 333: The ssp245 simulations are not mentioned at all here (the AMV sub-ensemble section). They clearly should. Please correct that.

Congratulations to the authors for a well-written and very interesting paper :)

Reviewer #1 (Remarks to the Author):

Comments to authors:

In their paper titled «Accelerated North Atlantic surface warming redefines the Atlantic Multidecadal Variability», the authors examine the Atlantic Multidecadal Variability (AMV) by comparing observed sea surface temperature observations over the 20th century with multimodel 20th century and future simulations from CMIP6 climate models. They suggest that natural fluctuations of AMV is currently damped meaning that natural variations in AMV will be overwhelmed by long-term emission-driven warming. The paper address the nature of the AMV, including the relative role of external forcings or intrinsic factors, which is an important topic that feeds directly into the current climate change debate. As such the paper is certainly fitting for this journal. The paper is well written in it current form, and the statistical methodology is valid. However, I have some critical comments that lead me to suggest major revision.

We thank Reviewer #1 for their appreciation of our manuscript and for the constructive criticism and helpful comments. We respond below to the specific remarks by the Reviewer.

1. Is the Atlantic SST accelerating? A key premise for this paper is the claim that observations show an ongoing acceleration of the warming. While this certainly may be true, the observational basis for this is still very short (a few years) as seen in Fig. 1a. You mention potential short term events (like Honga Tonga eruption) in the Introduction. Couldn't this «acceleration» be due to short-term events like this? Or what about ENSO? Some more elaboration around this would be nice.

Thanks for this important comment. It seems to have been stimulated by our reference to the possible causes of the 2023 global anomaly stated in the introduction of our original manuscript, which was just meant to be a general premise to the broader focus of the paper. In order to investigate whether short-terms events could be responsible for the found acceleration, we reinforced the trend analysis by comparing skills of quadratic and linear trends for periods excluding the most recent years. The analysis shows that a quadratic trend, suggestive of acceleration, is preferable even if the last ten years of data are removed from the analysis. We have added the following statement in lines 78-80, reporting as an example the results for the 1870-2013 period:

"Similar results are obtained excluding from the analysis the most recent years, which indicates that this acceleration does not merely result from a short sequence of exceptionally warm years (e.g., for the period 1870-2018, quadratic trend: $R^2 = 0.518$; AIC = -490; SBC = -481; linear trend: $R^2 = 0.474$; AIC = -480; SBC = -474)."

Whereas in the revised manuscript we did not include a detailed analysis on the role of specific phenomena such as ENSO or the Hunga Tonga eruption for the recent state of North Atlantic SST, we briefly discuss in the final section of the revised manuscript that remote forcing factors and teleconnections driving the AMV have implications for its predictions, see lines 416-419 of the revised manuscript:

"Analog uncertainties stem from remote sources of North Atlantic SST variability, such as El Niño-Southern Oscillation[i], that might have significantly contributed to the recent warm anomaly of the AMV and will continue shaping its future evolution."

2. Detrending of Atlantic SST. This is a long standing issue regarding how to examine AMV. As you mention in the paper the way you detrend can impact how much the recent NA SST trend can be attributed to AMV. In the paper you only look at linear and quadratic fit of the Atlantic SST. Given the importance of this, I would really like to see this explored a bit further. For instance, Sutton et al. 2018 use a different definition of AMV where they take Atlantic mean temperaure minus the global mean excluding the North Atlantic. This definition has been used extensively in the literature. This definition should therefore also be included and tested.

Thanks for this comment. The Sutton et al. (2018) definition is inherently different from the definitions that we adopted and its consideration is a worthy addition to our study. Whereas the definitions we originally employed in our study are based on different assumptions about the shape of the trend (long-term temporal evolution) of North Atlantic SST and neglect the role of global SST, the definition by Sutten et al. (2018) is based on the assumption that the AMV can be (linearly) separated from the global SST and defined as a residual from global variability, without making any assumption about the shape of the trend of North Atlantic SST. Therefore, it provides a complementary perspective on the AMV with respect to our original approach.

We have added an analysis of an AMV index based on the Sutton et al. (2018) definition in our revised manuscript. The results on global-mean SST (North Atlantic excluded) and on the "Sutton AMV" indicate that the hypothesis of a damped nature of the AMV in historical times is indeed rejected if one considers the AMV to be independent from the global SST. We present and discuss this result in lines 69-71 and 137-145 of the revised manuscript, referring to studies that indicate the AMV as a source of global SST variability, so that both signals cannot be easily separated. The text in lines 137-145 is reported below:

"Trends of observed global-mean SST supports the hypothesis that acceleration of North Atlantic surface warming is part of a global phenomenon (for the period 1870-2023, quadratic trend: $R^2 = 0.965$; AIC = -750; SBC = -741; linear trend: $R^2 = 0.778$; AIC = -676; SBC = -670). Accordingly, an AMV index calculated as deviation of North Atlantic SST from the global-mean SST yields a mid-20th century AMV anomaly (0.135 °C for the period 1930-1960) that is slightly warmer than 21st century AMV anomaly (0.095 °C for the period 1995-2023), both warm periods being not significantly different ($p = 0.26$). The damped nature of the AMV is therefore rejected if assuming that the AMV has no active role in surface warming of the global ocean - a hypothesis that, however, contrasts with model-based evidence that the AMV is a source of decadal global SST variability[ii,iii,iv]."

We have also added the following text in lines 419-422 of revised manuscript to acknowledge the limitations of our approach that neglects the connection between AMV and global SST:

"Our stochastic trend approach neglects an explicit representation of the relationships between the AMV and the global SST, as we acknowledge that discerning both signals is difficult due to their co-phase[ii] and a clear-cut separation, as done for some AMV definitions**Error! Bookmark not defined.**, implicates tight assumptions about the AMV as a source of global variability."

3. Climate models and skill. I find the large diversity of skills in how models represent the AMV quite interesting. Are there characteristics about the «skillful» sub-ensemble models that stands out? Higher resolution? Better resolved stratosphere? Others? Or is this just «luck of the draw»? Kravtsov et al. 2018 stated the following about CMIP5 models: «While climate models exhibit various levels of decadal variability and some similarities to observations, none of the simulations *considered match the observed signal in terms of its magnitude, spatial patterns and their sequential time development». Can you comment on this? It seems to me that the spread in the models are still large. A more detailed discussion around this (strengths and weaknesses with the current models) would be welcomed. Also, there is no figure showing the simulated spatial AMV patterns. I think this is required.*

Thanks, we agree that a figure with the patterns is necessary. The original manuscript was prepared as a brief communication, hence the limited amount of results illustrated there. Observed and simulated AMV spatial patterns are now provided in new Fig. 2 of the revised manuscript, that also includes a panel illustrating the skills of individual realizations within the multi-model ensemble. This shows that the spread of spatiotemporal skills for single-model ensembles is large, even for the case of models contributing to the AMV sub-ensemble. This suggests that in these models the AMV has a substantial intrinsic component. In addition to describing the observed spatial pattern, we have added the following sentence in lines 299-302:

"Overall, despite a good general performance of some models (most noticeably ACCESS-ESM1-5 and CanESM5), the scattered skills for all single-model ensembles (Fig. 2d) prevent identifying key model specificities that contribute to realism of the simulated AMV and rather point to a substantial contribution by intrinsic variability."

Thanks also for pointing to the Kravtsov et al. paper: it confirms that the North Atlantic emerges as a source of global multidecadal variability (the "global stadium wave"), which again supports our statement above that separating the AMV from the global SST should be done with caution. We cite this paper in line 145 of the revised manuscript

4. Climate models and the signal-to-noise paradox. I think for a Nature paper on this topic it would be really beneficial if your multi-model analysis could be put into context with the so called «signal-to-noise paradox» (Scaife and Smith 2018), i.e. the widespread inconsistency between the low strength of predictable signals in climate models and the relatively high level of agreement they exhibit with observed variability of the atmospheric circulation.

Thanks for pointing to the Scaife and Smith paper. The signal-to-noise paradox, despite mostly developed for atmospheric variability, may indeed be applicable to our case. We have included a reference to the paradox in the final discussion paragraph at lines 422-427:

"Finally, the signal-to-noise paradox[v] provides an additional framework to interpret the developing incoherences across the AMV scenario evolutions: accordingly, if models underestimates the observed signal-to-noise ratio of the AMV[iv], then simulations contain a smaller proportion of predictable variance than the observations and hence the real world is more predictable than the models. In this case, the near-future evolution of the AMV will provide clearer information about its damped character than what is discernible by current climate simulations."

5. The title. Redefining AMV is a pretty strong statement. How do you justify this? The damped oscillation framework has been suggested before (Omrani et al. 2022). The importance of external forcing for AMV is also not new. So how are you actually redefining AMV?

We agree that our title was possibly an overstatement, but we thought that identifying the AMV as a strongly damped oscillator was enough to call for the verb "redefine". We have changed this to "reshapes" in the revised title, as indeed what matters most is how the temporal evolution of the

AMV is reshaped according to our index (whereas the spatial pattern as shown in new figure 2 remains very similar to the typical AMV pattern).

6. External forcing vs intrinsic ocean circulation dynamics. On line 82-83 you state that the AMV is strongly imprinted by external forcing. How does this manifest itself? Direct radiative forcing of say tropical Atlantic through aerosols, or indirectly through changes in ocean circulation (say AMOC)? Overall I would like to see some figures and discussion about the role of ocean circulation, e..g. the Atlantic Merdional Overtruning Circulation (AMOC), in the models. Are AMOC and AMV in phase? Out-of-phase? Or doesn't the ocean circulation matter at all for AMV?

We agree with the Reviewer that the AMV-AMOC connection is a critical aspect for the understanding of the AMV and its simulation. We have added a figure (new Figure 4 in the revised manuscript) illustrating the diversity of relation between the AMV and the meridional overturning circulation in the Atlantic. We have added the following text on lines 309-322 of the revised manuscript:

"The high skills and coherency of simulations within the AMV sub-ensemble are not tied to their representation of ocean dynamics: Whereas models within the AMV sub-ensemble provide a strongly consistent spatial structure of the Atlantic meridional overturning circulation (AMOC) (Fig. 4a), the AMV imprint on the AMOC is largely incoherent across models, except for the shallow mid-altitude and polar North Atlantic (Fig. 4b). The large spread of cross-correlation profiles between AMV and two AMOC indices for individual simulations within the AMV sub-ensemble confirms the diversity of behavior across models and across realizations with the same model (Fig. 4d). For instance, a near co-phase between AMV and AMOC is consistently found in two realizations from different versions of the MPI-ESM model, or in most realizations with CanESM2, whereas it is impossible to discern a preferred phase-relation within ACCESS-ESM1-5 or CNRM-CM6-1 in terms of sign, strength or timing. This diversity of coupling between AMV and ocean circulation stems from several potential causes, including model specificities regarding timescale and amplitude of intrinsic AMOC variability[vi] and the disruption of intrinsic AMV-AMOC dynamics by external forcing**Error! Bookmark not defined.**. Therefore, assuming a role of ocean circulation for the AMV, no dominant mechanism emerges to explain the ensemble historical AMV evolution."

7. Damped oscillation. Related to the previous point. The coupled damped oscillation framework was recently published in npj (Omrani et al. 2022) with the main author of this paper as one of the co-authors. In that framework, the coupled stratosphere-troposphere-ocean pathway is key to the mechanism, linking a wave-induced downward propagating positive stratosphere/troposhere-coupled Northern Annular Mode with AMOC. If the AMV is purely external forced, why talk about a damped oscillation at all? Or is the external forcing imprinting on the AMV in a different way, through phase locking or something? What if so, could be the mechanism?

We do not claim that the AMV is merely forced, rather we agree with Omrani et al. (2022) that the AMV can be described as an intrinsic oscillatory mode (possibly involving different compartments of the Earth system as in Omrani et al., 2022) susceptible to external forcing. Compared to the damped oscillation described by Omrani et al. (2022), our analysis suggests that the damping of the component is stronger than previously thought and indicated by Omrani et al. (2022). This means that, for instance, the potentially approaching hiatus induced by an upcoming negative phase of the AMV as described by Omrani et al. (2022) might not occur with the amplitudes described in those papers. In one of the conclusive paragraphs of the revised manuscript we more explicitly refer to Omrani et al. (2022) as follows to indicate that our results are not necessarily in contrast with their work (see lines 405-413):

"The found diversity of AMV-AMOC connections within the AMV sub-ensemble reveals a variety of expressions of simulated dynamics that necessarily have different levels of realism; furthermore, only in individual realizations the intrinsic component of the AMV is fully expressed. Based on individual simulations and accounting for scenario uncertainty - that is merging the *ssp585* and *ssp245* AMV sub-ensembles -, a neutral AMV phase (2040-2060 average within ±0.1 °C) by the mid-21st century is the most probable (66%) outcome, with a cold phase being slightly less probable than a warm phase (14% and 20%, respectively). Simulations yielding a pronounced mid-21st century cold phase of the AMV (ACCESS-CM2 r2; CNRM-CM6-1 r2; MRI-ESM2-0 r3) are compatible with recently published AMV scenarios**Error! Bookmark not defined.**, but disagree on amplitude as well as timing and duration of the anomaly."

Concerning the mechanism(s), we discuss at least the AMV-AMOC connection when discussing new Figure 4, and we refer to our response above for specific changes in the text.

8. My final point is one thing that annoys me about this paper, and which I will have to insist be rectified in order for me to accept this paper. Why do you only use the SSP5-8.5 scenario for the models? It is now well established that this is an implausible scenario (Hausfather Z and Peters GP 2020; Pielke Jr, R. et al. 2022; IPCC 2021, and many others), so in my opinion there is no excuse for climate modellers to keep sticking to this scenario. If you want to use, as you say in the abstract, «realistic» climate simulations for the near future then SSP2-4.5 is a much better option. You might be right that «other scenarios are not expected to differ significantly from the ssp585 scenario ...», but then why not just show it? You open yourself up for a lot of criticism by sticking to this scenario only.

Thanks for this comment. Indeed, in the early phases of our study we also investigated the ssp245 scenario. However, despite more realistic in terms of near-future evolution of emissions and forcing, this scenario is more complex than the ssp585 we eventually chose, because of the temporal development during the 21st century of forcing and simulated North Atlantic warming. Specifically, following the forcing, North Atlantic warming in the ssp245 scenario is characterized by reduced trends in the second compared to the first half of the 21st century. This multidecadal signal, which we interpret as direct radiative response to the forcing, superposes on the typical AMV timescales and complicates the separation of AMV from the global/forced response.

In the revised manuscript we have included results for the ssp245 scenario in new Figure 5, and we discuss them considering the results obtained for ssp585, which we maintain as reference scenario for the main analysis. As shown in Figure 5, both scenarios support our conclusions of an acceleration of north Atlantic surface warming in the second half of the 20th century and of a strongly damped character of the AMV with only weak (or neutral) AMV conditions in recent decades.

The ssp245 results are discussed in several parts of the revised manuscript, we only report here one part of the text added in lines 369-387:

"The *ssp585* scenario provides for a sustained warming signal throughout the 21st century but is considered implausible by recent assessments[vii,viii]. The milder *ssp245* scenario provides for more realistic projected climates, but it also makes the long-term trend and AMV components of 21st century evolution of North Atlantic SST less discernible, as the warming trend weakens before the mid of the century following the applied forcing pathway (Fig. 5a,d). The multidecadal time scale of this forced evolution superposes on the typical AMV time scale, complicating the separation between variability arising from North Atlantic dynamics and transient response to radiative forcing. In the scenario period, forced trends from the AMV sub-ensemble still indicate sustained North Atlantic surface warming in the next couple of decades for *ssp245*, like what obtained for *ssp585* (Fig. 5e). Larger trends are also consistently pictured by both scenarios since the 1970s: an ongoing acceleration of North Atlantic warming is therefore a robust feature across simulations, models and scenarios.

In the historical period, the dynamic linear model yields a slightly stronger warming trend around the mid-20th century followed by stronger cold AMV phase in the *ssp245* compared to the *ssp585* scenario (Fig. 5a,b). Therefore, a statistical separation between direct radiative response to forcing, dynamical responses and intrinsic variability cannot be clear cut. The amplitude of this uncertainty is comparable to that found for different trend models accounting for accelerated warming in observations (Fig. 1c). Despite these uncertainties, the AMV sub-ensembles from both scenarios agree on the stark contrast between large and coherent historical AMV fluctuations, and large spread in projected AMV evolution yielding vanishing expected anomalies in the scenario period (Fig 5b,e)."

That a side, I would like to thank the authors for a nice work and a very interesting paper. I will be happy to look at a revised version.

Thank you, we are confident that the revised version of our manuscript will convince you about the soundness of our analysis and robustness of our conclusions.

Reviewer #2 (Remarks to the Author):

This study examines the relative contribution of the Atlantic Multidecadal Variability (AMV) and warming trend due to increasing greenhouse gases to the North Atlantic (NA) SST warming during the early 21st century using both observations and CMIP6 simulations. The authors conclude that the contribution of AMV to the total NA SST warming is smaller than previously thought. They further suggest that, based on analyses of the CMIP6 simulations, AMV fluctuations are strongly damped under warming.

The findings of this study potentially improve our understanding of AMV dynamics in the future under global warming. However, The authors' conclusions heavily rely on statistical methods and they do not provide mechanistic explanations. Also, there is a question mark regarding the methodologies used for the CMIP6 simulation analyses. Therefore, the results of the study are not yet fully convincing because of the reasons detailed below. Therefore, I recommend a major revision of the manuscript before considered for publication.

We thank Reviewer #2 for their appreciation of our manuscript and for the constructive criticism and helpful comments. We respond below to the specific remarks by the Reviewer. Our revised manuscript has been substantially improved compared to the original submission, which was prepared for a brief communication format. Therefore, the revised manuscript contains expanded analyses that also concerns the physical interpretation of the AMV, particularly regarding its connection with ocean circulation and the robustness of its simulated representation across state of the art models.

Major comments:

My major concerns are around the statistical methods that authors used to extract AMV from the CMIP6 simulations and the interpretation of the extracted AMV signal. The authors used a statistical model (DLM) to estimate AMV from the simulations. It is understandable to use a statical model to do so given a small number of realizations for some models. However, there are also a few models that have, at least, 25 realizations, which should be enough to obtain the "true" warming signals. Is the warming signal estimated from DLM close enough to these true signals? Most models in Fig. 2a show an almost linear trend during the second half of the 21st century, but the stochastic trends from DLM shown in Fig. 2b are mostly nonlinear and become flattened toward 2100. So, it is not surprising to see some low-frequency variability when the stochastic trends are removed from NA SST. This could be problematic for the sub-ensemble because there are only 18 realizations. That is, even after the ensemble average, because of the small ensemble size, some residual can remain. How do authors make sure that this is a "real" AMV signal and there is an oscillatory behavior?

Even if the oscillatory signal is real, what is the mechanism that generates the oscillations? The phase of the sub-ensemble and full ensemble is very close during the 20th century; amplitude is just larger in the sub-ensemble. This suggests that the sub-ensemble is a mix of both externally forced and internally generated signals. One possible explanation seems to be that in these realizations, the internally generated AMV is synchronized with externally forced AMV. Then, could the damped warm AMV phase during the early 21st century be just due to internally driven AMV? In any case, some explanations along with supporting analyses seem to be required. As it is, the damped oscillation of AMV is too hand-wavy due to the uncertainty in the statistical method used to remove the trends and the lack of mechanistic explanations.

Thank you for this comment that allows us to clarify our methodology and improve its description in the revised manuscript. In fact, we followed the method described by the Reviewer already in the original analysis, that is, we consider the ensemble mean as reference for the expected forced response in the case multiple realizations are available for a certain model. We have added the following text in the methods section (dynamic linear model, lines 637-640):

"The analysis is performed for each model separately, where M is the raw time series of North Atlantic average SST in the case of a single realization, and the ensemble-mean time series if multiple realizations are available, so that δ and β more accurately capture the forced response."

We agree with the Reviewer that there is unavoidably a superposition of AMV and forced (direct radiative response) signals, and that there might be mutual dynamical dependences between AMV and global dynamics, so that a statistical separation between both signals cannot be clear cut. Having added an analysis on the ssp245 scenario following a recommendation from Reviewer #1, we discuss this issue more explicitly in the revised version of the manuscript. In particular, we acknowledge (lines 382-383) that "[…] a statistical separation between direct radiative response to forcing, dynamical responses and intrinsic variability cannot be clear-cut".

We have enlarged the sub-ensemble, which now accounts for 25 simulations and provides a more solid assessment of its emerging signals.

Concerning the last question about the oscillatory nature of the AMV, we respond by tackling two critical aspects of the analysis.

First, our interpretation is first of all bound to our initial hypothesis, which we express as "If North Atlantic surface warming has accelerated, then the AMV has a damped nature." We agree that this even more fundamentally traces back on the separability between different processes contributing to total variability, namely the long-term global warming and its imprint on North Atlantic SST, and the AMV, which is a phenomenon arising from ocean dynamics in the North Atlantic and has likely both an externally forced and intrinsic nature. But such a concern about the separability between these two processes is pervasive across all proposed definitions of the AMV in scientific literature. We are convinced that our approach to describe the long-term trend is an improvement compared to previous methods - and we provide several evidence supporting our approach that are based on both observations and simulations -, and therefore allows for a more realistic "extraction" of the AMV signal form the raw North Atlantic SST data. The revised version of the manuscript contains an expanded discussion about the available definitions of the AMV and the different characterizations of the observed AMV evolution they generate in the historical period (see also revised Figure 1).

Then, we support the hypothesis of a damped oscillatory character of AMV fitting a delayed oscillator model to the simulated AMV data. We agree with the Reviewer that this aspect of the analysis could benefit from further investigations, and we therefore toned down the associated conclusions, in particular removing reference about "damped fluctuations" in the title. Explicit reference to the possible damped oscillatory character of the AMV is now confined to the following text (lines 342 onwards):

"This, again, points to a strongly damped nature of the phenomenon. Accordingly, the simulated AMV is consistent with a range of damped oscillators with time delays from interannual to interdecadal (blue shading in Fig. 3c, see methods), suggesting that the AMV is part of a self-excited damped mode of climate variability."

Authors argue that AMV is better defined after a quadratic fit is removed than a linear fit based on a few metrics without much detail of these metrics. How these metrics are calculated and why do higher scores mean better performance of the quadratic fit than the linear fit? More details of these metrics should be given (ideally in the method section) and the justification of the quadratic fit over the linear fit needs to be provided.

We now provide a description on how the considered metrics for model performance skills are calculated and used to assess the shape of the trend in observed North Atlantic SST data (lines 583-589).

"The skills of statistical models with least-square fit to the data to describe the trend in average North Atlantic SST are evaluated based on the following metrics: the coefficient of determination (R^2), the Akaike information criterion (AIC), Schwarz's Bayesian criterion (SBC) and the corrected AIC. Based on the model's sum of squared erros *sse*, number of data *n* and number of parameters *p*, AIC is calculated as $n \cdot \log(sse/n)+p$, SBC is calculated as $n \cdot \log(sse/n)+p \cdot \log(n)$ and AICc is calculated as $n \cdot \log(sse/n)+(n+p)/(1-(p+2)/n)$. The best model is characterized by the highest value of R^2 and the lowest values of AIC, AICc and SBC."

We remark that the quadratic trend is highlighted because it allows for a simple description of accelerated warming, and is therefore instrumental in demonstrating that a trend model including an acceleration in the rate of warming explains better the data than a trend model with constant rate of warming. Our revised analysis then focuses on the stochastic trend approach to generalize the shape of the trend, without the need for more specific hypothesis about the shape of the trend.

Minor suggestions:

ll. 44-48: I can't follow this sentence. It is too long and seems to be broken.

We have removed the last part of the sentence ("with weak cold anomalies whose amplitude is too small to significantly modulate projected warming trends")

ll. 34-35: There are also studies that contend the aerosols-driven AMV cold phase (Zhang et al. 2013; Kim et al. 2018)

We have rephrased as follows, also citing the two mentioned papers:
"Whether the historical AMV is predominantly externally forced or intrinsically induced remains debated, in particular regarding the role of tropospheric aerosols at least for the most recent cold AMV period"

ll. 40-41: There are studies (Robson et al. 2018; Yeager 2020) that predict a cooling in the upper ocean heat content in the subpolar North Atlantic during the early 21st century.

We have rephrased as follows, also citing the two mentioned papers: "[…] model-based evidence for an imminent cooling of the upper North Atlantic remains limited to the subpolar gyre region[ix,x].

l. 91: Fig. 2d -> Fig. 1c?

Correct. The sentence was largely rewritten, also referring to the new figure with spatial patterns in observations and simulations

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We thank Reviewer #1 for their appreciation of our revised manuscript and for the editorial comments and corrections in this second round of revision. We have accounted for all the suggestions by the Reviewer, as detailed below (original comments by the Reviewer in italics.

The revised manuscript "Accelerated North Atlantic surface warming reshapes the Atlantic Multidecadal Variability" the authors have addressed all of my main concerns from the original review. I am especially pleased to see a more thorough discussion about the uncertainties in the models, the role of ocean circulation (AMOC) and the inclusion of the SSP245 scenario in the analysis. Based on this I am therefore happy to recommend the paper for publication.

Thank you for recommending the paper for publication!

I have just a few minor comments/corrections for the authors to consider:

Line 56: I don't like a sentence starting with a number. I suggest something like: "A total of 176 simulations from 29 models are considered."
Done, thanks

Line 93: It is not clear here that "deviation of North Atlantic SST from the global-mean SST" is actually deviation from global mean SST EXCLUDING the AMV domain. This is correctly described in the Methods section, but is unclear here. Please correct.
We now specify in both Figure 1 caption and in the text that the index is calculated as “deviation of North Atlantic SST from the global-mean SST excluding the AMV domain”

Line 128: "...what done for..." should be "...what is done for...".
Done, thanks

Line 216: "...what obtained for ssp585..." should be "...what is obtained for ssp585 ..."

Done, thanks

Line 237-241: I am not sure about the use of colon and semi-colon here. I'd rather just separate the sentences with a period.

We just substituted colon and semicolon with full stops.

Line 333: The ssp245 simulations are not mentioned at all here (the AMV sub-ensemble section). They clearly should. Please correct that.
We now specify that “The AMV sub-ensembles of historical and ssp585/ssp245 simulations [...]” and further that “For each of the considered future scenarios, the AMV sub-ensemble accordingly consists of [...]”

Congratulations to the authors for a well-written and very interesting paper :)

Thank you, your input contributed to make our study stronger.