nature portfolio

Peer Review File

Atlantic overturning inferred from air-sea heat fluxes indicates no decline since the 1960s

Corresponding Author: Dr Jens Terhaar

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The manuscript entitled "Recent Atlantic overturning inferred from air-sea heat fluxes dominated by decadal variability rather than trend" studies the relationship between AMOC strength and heat budget components in the North Atlantic Ocean. The authors show that the previously proposed relationship between SST in the SPG and AMOC strength by Caesar et al. (2018) for CMIP5 GCMs does not hold up for CMIP6 GCMs. The authors demonstrate that meridional heat transport at the southern boundary (MHTsouth) of the North Atlantic region of study and surface heat fluxes are the main components of the heat budget in the region. As MHTsouth is strongly related to AMOC, surface heat fluxes in the surface region could be used for AMOC reconstruction. Using this relationship, the authors show that the AMOC strength, reconstructed using reanalysis data, has not declined.

In my opinion, this paper provides a very important perspective on the AMOC slowdown debate, and I would recommend it for publication in Nature Communications journal. However, my major comment is that the authors should clarify their choice of historical scenario output used in their analysis of heat budget components (Fig.5) rather than preindustrial control output. It seems that in Figure 5, the second half of the 20th century shows a large trend due to GHG emissions. Additionally, some models exhibit noticeable variability in heat storage changes, especially before 1950. Would the results from preindustrial control simulations show a different balance between MHTsouth, MHTarctic, heat storage, and surface heat flux? Will it change the results from Lines 181-186? Lines 181-186 describe the major components of the North Atlantic heat budget, but it is not clear from which panel the authors derived the values (1.6 ZJ yr^-1, 1.4 ZJ yr^-1, 0.7 ZJ yr^-1, 0.4 ZJ yr^-1) for these anomalies. Are these values from panel (a)? It seems it is an important part of the analysis, and it would be beneficial to clarify this part.

Additionally, would it be useful to use multiple regression to regress AMOC onto a combination of these four variables to better understand the relationship between the heat budget of the North Atlantic and AMOC? Could some of the variables show high significance for AMOC even if the magnitude of their anomalies is smaller than that of other variables?

Minor comments:
Fig.1 – would it be beneficial to show each ESM on a plot with different markers?
Fig.5 - panel a - no y axis label

Reviewer #2

(Remarks to the Author)
After showing that the relationship between SPG SST index and AMOC anomalies at 26.5N is not supported by CMIP6 models, the manuscript discusses the relationship between the AMOC at 26.5N and the heat budget integrated north of the section. The main conclusion of the manuscript is that air-sea heat flux anomalies are a close proxy for AMOC anomalies on decadal to centennial timescales. Based on this result, the authors used the air-sea heat fluxes to reconstruct the AMOC at different latitudes in the CMIP6 models and in the observation-based reanalysis ERA5 and JRA55.

The study provides a valuable evaluation of the relationship between the AMOC and the heat budget in the North Atlantic in several CMIP6 Earth System Models, but it currently suffers from shortcomings that cast doubt on some of the results obtained, as indicated in the major comments below. I thus recommend publishing the manuscript subject to major revisions.

I hope the authors find my comments and suggestions useful when revising the manuscript.

Major Comments:

1) Major comment 1: Sloppy conclusions in the reconstruction analysis. I find the conclusion of the reconstruction analysis (lines 280-284) a bit hasty because the AMOC reconstruction is not well reproduced across all models. The authors acknowledge (lines 292-303) that the AMOC reductions since ~1990 is not well reproduced by the air-sea heat fluxes in several models and even falls out of 2sigma for some of them. In addition, I find that the reconstructed decadal variability is often stronger than the directly simulated AMOC, as in Figures 8(r,i). Thus, I am not convinced that these mismatches are an "exception", as claimed by the authors (lines 302-303), and they should be further discussed (e.g., through an evaluation of the model biases, the impact of resolution, etc..) to make the proxy more robust. I agree that excessive ECS can be one explanation for the differences in the AMOC decline (not for the AMOC decadal variability though), but the authors should develop this argument and show some convincing figures for these models.

2) Following on this major comment, the title and main conclusion of this paper (lines 487-490) is misleading and should be rephrased to emphasize the important limitations associated with the method. Instead of providing an estimate of the net AMOC strength, the AMOC reconstructions based on air-sea heat fluxes provides an estimate of the "upper limit for the AMOC decline" in the historical period, as acknowledge by the authors in the results (lines 318-319). Moreover, the strong decrease in the coefficient of determination r2 with increasing latitudes (Table 1) shows that a proxy based on the air-sea heat fluxes is not robust at all latitudes. Then, I suggest modifying the conclusion and title accordingly.

3) Major comment 2: Most of the study relies on correlation estimates between two variables, but there is no indication of significance for these correlations throughout the analysis.

4) Major comment 3: The evaluation of the SPG SST index is based on the same method than described in Caesar et al. (2018), and the conclusion is that the relationship between AMOC and this index is not robust in the ESM CMIP6 models because the SPG region covers a larger area than the 'warming holes', which introduce noise in the SST anomalies. I understand the logic of using the same area than in Caesar et al. (2018), for consistency with this previous study, but I suspect that this SPG area has been defined based on SST patterns from CMIP5 models and might need to be adjusted in the CMIP6 models.

Specific Comments:

Line 20: Consider removing the term "evolution" at the beginning of the sentence.

Line 41: Consider replacing the term "become" into "form".

Lines 92-94: The sentence is confusing, please clarify. How the air-sea heat fluxes relate more to conservation of energy than SST?

Line 94: "We then combine […]": Consider replacing by "We then apply […]".

Lines 107-109: Consider indicating the slopes obtained from your CMIP6 analysis and the one obtained by Caesar et al. (2018) from CMIP5 (if indicated in their paper) for a clearer comparison.

Line 111, Figure 1: For clarity, I think that the trend of -1.7 Sv per century found in CMIP5 should be indicated in Figure 1 with a horizontal grey bar.

Lines 125-126: The sentence is confusing, please rephrase.

Line 134: It is difficult to read the numbers in the current version of the Extended Figure 1. All the subplots of this figure should be bigger.

Lines 138-141: Can you indicate the correlation coefficients for these two Figures (Fig.3 b-c)? Related to major comment (2), can you also indicate the significance of these correlations (here and for all correlations in the study)?

Lines 144-145: The sentence is confusing, please clarify by removing "based on reconstructed annual AMOC timeseries".

Lines 157-159: Please clarify that the strong relationship is only found at decadal and centennial timescales but not at annual timescale.

Lines 175-176: "MHTarctic refers to the MHT into the Arctic". Consider indicating "northward MHT into the Arctic".

Line 179, Figure 5: Did you consider that the OHC change is different from zero when evaluating the low correlations at annual timescale, and it is why the evaluation of the mechanisms/causal relationships between the AMOC and air-sea heat flux at annual timescale is left to another study? I am also wondering why the OHC change (blue lines on Figure 5) is different from 0 after ~1950 in the multi-model mean, even after smoothing the timeseries with a 11-year running mean.

Lines 183-184, Figure 5: There is a large difference in the variability of these timeseries with, for example, some models showing larger decadal variability than others. It would be interesting to further discuss these differences with regards to the existing literature.

Lines 214-215: Consider indicating that it is related to the “variability of northward MHT” at the same latitude.

Lines 220-225: I am not sure to understand the argument here. Does the change in OHC refers to the statement at line 179, the equilibrium is not observed at annual timescale, or to a relatively stronger impact of the changes in MHT into the Arctic? Please clarify.
It can also be due to the area over which the air-sea heat fluxes are integrated – the correlation might be better if the area of integration was localised over the northern part of the subpolar gyre (Irminger and Labrador seas) where the dense water is formed, with possibly some lag.

Lines 242-244: It is not the only difference with the Walin framework. The Walin framework consider both the air-sea heat and freshwater fluxes. The freshwater fluxes (evaporation and precipitation) are relatively important for the AMOC variability, and I am now wondering if the authors tried to evaluate the relationship between AMOC and buoyancy fluxes instead of the heat fluxes alone. Using buoyancy fluxes (and thus including freshwater fluxes in the equation) can possibly lead to less mismatch between the reconstructed and simulated AMOC (see major comment (1)) and, then, to a more robust proxy.

Lines 245-246: I am confused, why is the correlation at 0-year lag (Extended Figure 4) different from the 0.22 indicated on Figure 3a?

Lines 257-260: I am not sure to understand your argument here, are you referring to the difference in the coefficient of determination over the Labrador Sea seen in Figure 7 (a-b)? If not, what can explain this difference over the Labrador Sea? Please clarify.

Line 333: Remove “Fig. 9B”. The AMOC reconstruction for JRA55 at 26N is shown in Fig. 9A.

Lines 361-366: Although the reconstructed decadal variability at 26N cannot be compared with the too short RAPID timeseries, the reconstructed long-term trends can be evaluated as compared with the observed one. Can you indicate the observed trend for comparison?

Lines 370-372: Figure 9 does not show a reconstruction of the AMOC at the OSNAP array, which is not at constant latitudes, or am I missing a figure?

Lines 459-460: It is not robust after ~1990, see major comment (1).

Reviewer #3

(Remarks to the Author)
The paper by J. Terhaar et al. tests - using 24 different CMIP6 models - in how far air-sea flux anomalies can be used to recreated AMOC strength. They find that this works better than the previously used SPG SST anomalies, albeit is only able to represent AMOC changes on decadal to centennial time scales.
The paper is well written and fairly easy to understand, but has several flaws in the underlying reasoning. These are the well-known overestimation of the aerosol forcing in the CMIP6 models, the discrepancy in the evolution of the historical AMOC in CMIP5 vs CMIP6 models, ignoring the atmospheric influence on the air-sea forcing on longer time scales, not considering the observed OHC changes when reconstruction the "real-world" AMOC, and some others (please see comments on the pdf for more details).
I would therefore suggest that all these issues need to be answered first, the analysis needs to be extended and the results double-checked before a publication can be considered.
In principle I believe that considering air-sea fluxes to determine AMOC strength can be an asset, but they should be considered in addition to changes in OHC/SST not as the only source of information.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The manuscript "Atlantic overturning inferred from air-sea heat fluxes indicates no decline since 1960s" provides important results for the debate on AMOC slowdown, as the authors use a new approach to reconstruct AMOC anomalies based on air-sea heat fluxes and show that AMOC hasn't declined during 1963 to 2017 period. I believe the current version of the manuscript has been significantly improved. Therefore, I recommend its publication, pending some minor revisions.

LN 90-91: "either the mechanism underlying the assumption was incorrect" - reference?

LN 93: "have altered or even broken the assumed relationship" - reference?

Fig.1 Reference cited in figure caption is #34, reference displayed on figure is #36

LN 170-171: "The larger intercept might be a consequence of the impact of aerosols on the AMOC that do not affect the SPG SSTs"
not sure if the cited paper supports this conclusion. Here is the paper from Zhu et al., 2023, where authors show that AER forcing affects AMOC and Temperature in SPG (their Fig.4)

Zhu, C., Liu, Z., Zhang, S., & Wu, L. (2023). Likely accelerated weakening of Atlantic overturning circulation emerges in optimal salinity fingerprint. Nature Communications, 14(1), 1245.

Fig.5 Maybe make "panel a" bigger with the display of mean anomaly values and show the single GCMs as smaller subplots?

Fig.5 "The missing 6 climate models from the total ensemble of 24 climate models did not provide the northward heat transport and are therefore not shown here."
This information should be in methods as well

Extended Data Fig.5 - Also maybe since there is a weak yearly relationship between surface heat flux and AMOC, you can apply a running mean of 5 to 10 years to compute lagged correlations? Will it show stronger correlation?

LN 354-359 and rest of the text: It might be helpful to use the subscripts AMOC_r for reconstructed anomalies and AMOC_ds for directly simulated anomalies, as the wording becomes quite confusing throughout the text.

LN 366: reconstructed AMOC

LN 422-425: Please consider rewriting the sentence for clarity, as it is currently hard to follow.

LN 459-461: I’m not sure if it can be accurately described as a decline.

LN 548 I would add at the end: strong relationship between air-sea heat flux anomalies and AMOC anomalies on decadal and longer time scales.

LN 900-905: you should mention that 6 models didn't provide northward heat transport.

LN 971-977: Please consider rewriting the paragraph for clarity, as it is currently hard to follow.

Extended data Fig.3: colorbar?

Extended data Fig.8: do you mean difference between reconstructed and directly simulated AMOC here?

Reviewer #2

(Remarks to the Author)
The authors responded to my concerns by clarifying the text on their main conclusion, i.e., the limitation of the AMOC reconstruction analysis (comment 2.1-2.2). It improved the manuscript by making the conclusion more robust, to my opinion. The description of the emergent constraint approach is also welcomed to clarify the underlying assumption associated with this method (comment 2.4).

I still think that an evaluation of the buoyancy fluxes (including freshwater fluxes in the equation), instead than air-sea heat fluxes alone, could be an interesting addition to the manuscript, possibly leading to less mismatch between the reconstructed and simulated AMOC (comment 2.21), but I understand that it might be beyond the scope of the paper. Note, however, that there are large uncertainties in both air-sea heat and freshwater fluxes between reanalyses, and because the authors interpret the range between the two reanalyses as additional uncertainties from the observation-based estimates of heat fluxes, something similar could be done for the freshwater fluxes.

The manuscript now stands well by itself and can be published in the present form.

Reviewer #3

(Remarks to the Author)
Overall the authors made a good effort in responding to my comments, I in general I am in favor of publication. Nevertheless, I think there are a two areas where the paper would benefit from a clearer justifications and/or additional methodological detail:

Clarification of Methodology: While the authors give good justifications for using air-sea fluxes, I am missing a clear explained how this choice impacts the reliability of AMOC reconstructions, i.e. a more detailed explanation of why air-sea fluxes alone are sufficient could strengthened the discussions.

Addressing CMIP5 vs. CMIP6 Discrepancies: The authors responded to my concerns about differences between CMIP5 and CMIP6 trends and aerosol forcing, but I would like a more in depth discussion on the implications of these differences (and why the authors feel that these discrepancies do not undermine their findings).

1 Response to reviewer 1

General Comments

The manuscript entitled "Recent Atlantic overturning inferred from air-sea heat fluxes dominated by decadal variability rather than trend" studies the relationship between AMOC strength and heat budget components in the North Atlantic Ocean. The authors show that the previously proposed relationship between SST in the SPG and AMOC strength by Caesar et al. (2018) for CMIP5 GCMs does not hold up for CMIP6 GCMs. The authors demonstrate that meridional heat transport at the southern boundary (MHTsouth) of the North Atlantic region of study and surface heat fluxes are the main components of the heat budget in the region. As MHTsouth is strongly related to AMOC, surface heat fluxes in the surface region could be used for AMOC reconstruction. Using this relationship, the authors show that the AMOC strength, reconstructed using reanalysis data, has not declined.

In my opinion, this paper provides a very important perspective on the AMOC slowdown debate, and I would recommend it for publication in Nature Communications journal.

Response:

We thank the reviewer for their positive evaluation and the additional constructive and helpful comments, which have substantially improved our manuscript. We have taken each comment into account, provide responses to each point below, and adapted the manuscript accordingly.

Comment 1.1

However, my major comment is that the authors should clarify their choice of historical scenario output used in their analysis of heat budget components (Fig.5) rather than preindustrial control output. It seems that in Figure 5, the second half of the 20th century shows a large trend due to GHG emissions. Additionally, some models exhibit noticeable variability in heat storage changes, especially before 1950. Would the results from preindustrial control simulations show a different balance between MHTsouth, MHTarctic, heat storage, and surface heat flux? Will it change the results from Lines 181-186? Lines 181-186 describe the major components of the North Atlantic heat budget, but it is not clear from which panel the authors derived the values (1.6 ZJ yr^{-1}, 1.4 ZJ yr^{-1}, 0.7 ZJ yr^{-1}, 0.4 ZJ yr^{-1}) for these anomalies. Are these values from panel (a)? It seems it is an important part of the analysis, and it would be beneficial to clarify this part.

Response:

The numbers presented in lines 181-186 are indeed the mean of the absolute values from panel (a) in Figure 5 as hypothesized by the reviewer. We have now clarified this in the revised manuscript:

"The largest anomalies across the components of the North Atlantic heat budget over the historical period are simulated in the southern MHT (1.6 ZJ yr^{-1}, mean of absolute values between 1850 and 2014) and surface heat flux (1.4 ZJ yr^{-1}) terms, while anomalies in heat storage (0.7 ZJ yr^{-1}) and Arctic MHT (0.4 ZJ yr^{-1}) are smaller (Fig. 5)."

We have chosen the historical period here as we aim to reconstruct the AMOC over this historical period. It is thus important that our main assumption, namely that AMOC changes are mainly related to changes in the air-sea heat flux, holds even during warming in the historical period.

This primary relationship that underlies the premise of this paper holds in both historical and preindustrial runs. In our original submission, we did a parallel analysis of the preindustrial runs (see Extended Data Figures 7, 8, and 11), and found the same relationship between the AMOC and air-sea heat fluxes. On decadal time scales, the r^2 between the air-sea heat flux anomaly and the AMOC anomaly is 0.67, compared to 0.74 in the historical runs. Similarly, we found that the relationship is weak to non-existent on annual time scales (r2=0.06 in pre-industrial runs and r2=0.22 in historical runs). The slope of the relationship on decadal time scales does change between preindustrial and historical runs (a steeper negative slope in historical runs) due to the trend that the reviewer points out is apparent in the historical runs. This is the reason why we explain on lines 377-406 that this methodology provides an upper bound on the calculated trends, and is thus a robust indicator of AMOC decline.

Comment 1.2

Additionally, would it be useful to use multiple regression to regress AMOC onto a combination of these four variables to better understand the relationship between the heat budget of the North Atlantic and AMOC? Could some of the variables show high significance for AMOC even if the magnitude of their anomalies is smaller than that of other variables?

Response:

As suggested by the reviewer, we had also considered multiple regression similar to a previous studies by one of the main authors (Terhaar et al., 2022). In this case, however, we believe that such an approach is not possible for two reasons. First, a multiple regression is problematic in the presence of correlated predictor variables. In this case however, the air-sea heat flux influences the export of heat to the Arctic and also the heat content and is hence correlated with both on decadal timescales (r^2=0.46 for air-sea heat flux and OHC, and r^2=0.53 for air-sea heat flux and northward heat transport to the Arctic Ocean). The correlation makes it impossible to robustly fit a multiple regression to these predictor variables. Second, the heat content change throughout the entire North Atlantic as well as the transport of heat to the Arctic through the Barents Sea Opening, the Fram Strait, and the CAA are not well observable on monthly timescales making it impossible to use these variables in order to exploit a multiple regression.

While it is hence not possible to use the suggested variables for a multiple regression, we also believe that the advantage of a multilpe regression would be minor at best in this study. Figs. 3, 5, and 6 show that the air-sea heat fluxes offset the northward heat flux anomaly, and both fluxes are related by a very strong relationship that explains most of the AMOC anomaly on decadal and centannial timescales. Furthermore, Figs. 6 and 8 demonstrate the robustness of this relationship in magnitude (Fig. 6) and it's capability to reconstruct the time evolution of the AMOC (Fig. 8).

Overall, the already strong relationship and the impossibility to use the mentioned variables to further constrain the AMOC anomaly unfortunately leads us to decide not to apply a multiple regression in this special case.

Terhaar, J., Frölicher, T. L., and Joos, F.: Observation-constrained estimates of the global ocean carbon sink from Earth system models, Biogeosciences, 19, 4431–4457, https://doi.org/10.5194/bg-19-4431-2022, 2022.

Comment 1.3

Minor comments: Fig.1 – would it be beneficial to show each ESM on a plot with different markers?

Response:

We now allow each model in Fig. 1 to be identified as suggested by the reviewer. However, instead of using different markers, which are hard to identify, we added letters to each dot and a corresponding legend.

Comment 1.4

Fig.5 - panel a - no y axis label

Response:

We thank the reviewer for pointing out this omission – we have now added an axis label.

2 Response to reviewer 2

General Comments

After showing that the relationship between SPG SST index and AMOC anomalies at 26.5N is not supported by CMIP6 models, the manuscript discusses the relationship between the AMOC at 26.5N and the heat budget integrated north of the section. The main conclusion of the manuscript is that air-sea heat flux anomalies are a close proxy for AMOC anomalies on decadal to centennial timescales. Based on this result, the authors used the air-sea heat fluxes to reconstruct the AMOC at different latitudes in the CMIP6 models and in the observation-based reanalysis ERA5 and JRA55.

The study provides a valuable evaluation of the relationship between the AMOC and the heat budget in the North Atlantic in several CMIP6 Earth System Models, but it currently suffers from shortcomings that cast doubt on some of the results obtained, as indicated in the major comments below. I thus recommend publishing the manuscript subject to major revisions.

I hope the authors find my comments and suggestions useful when revising the manuscript.

Response:

We thank the reviewer for their positive evaluation and the indeed very constructive and helpful comments, which have substantially improved the manuscript. We have taken each comment into account, provide responses to each point below, and adapted the manuscript accordingly.

Comment 2.1

Major comment 1: Sloppy conclusions in the reconstruction analysis. I find the conclusion of the reconstruction analysis (lines 280-284) a bit hasty because the AMOC reconstruction is not well reproduced across all models. The authors acknowledge (lines 292-303) that the AMOC reductions since 1990 is not well reproduced by the air-sea heat fluxes in several models and even falls out of 2sigma for some of them. In addition, I find that the reconstructed decadal variability is often stronger than the directly simulated AMOC, as in Figures 8(r,i). Thus, I am not convinced that these mismatches are an "exception", as claimed by the authors (lines 302-303), and they should be further discussed (e.g., through an evaluation of the model biases, the impact of resolution, etc..) to make the proxy more robust. I agree that excessive ECS can be one explanation for the differences in the AMOC decline (not for the AMOC decadal variability though), but the authors should develop this argument and show some convincing figures for these models.

Response:

We agree with the reviewer that the wording used to describe the mismatches between the AMOC reconstructed from air-sea fluxes and the directly simulated AMOC did not accurately describe the results. However, we disagree with the reviewer that the exception we describe and the explanation we provide are not convincing. First, we want to highlight the remarkable agreement with the directly simulated AMOC shown in Fig. 8 and Extended Data Figure 5. Until the end of the historical period in CMIP6 in 2014, the number of times when the reconstructed AMOC falls out of the 2-sigma range is less than 5%, as expected from a normal distribution with equal means. After 1990 to 2000, a small bias towards too low AMOCs arises in some models. As this bias does not occur in pre-industrial control simulations (Extended Data Figures 7 and 8), the bias occurs very likely due to atmospheric warming and consequent air-sea heat uptake by the North Atlantic. Furthermore, this bias after 1990 to 2000 is larger than the 2-sigma range in the CanESM5 models and the IPSL-CM6A-LR model. These two models have in common that they have exceptionally high warming trends over the beginning of the 21st century, as well as an exceptionally high ECS and TCR (Tokarska et al., 2020), again suggesting that it is indeed the warming that drives this bias. Hence, we believe that our explanation for the slight bias and for the exceptions is roubst.

Following the reviewers comments and our imprecise wording in the previously submitted manuscript, we have now carefully reworked the text and specified that the reconstruction works well until around 1990 and that it provides an upper limit afterwards:

"The comparison between the reconstructed and directly simulated AMOC demonstrates that the air-sea heat flux anomalies can indeed be used to reconstruct the net AMOC anomalies until the 1990s and that the reconstruction provides an upper limit that diverges at most 1Sv from the net AMOC anomaly afterwards."

and

"After 1990 to 2000, the air-sea heat flux-based reconstructions provide an upper limit as indicated by a too weak reconstructed AMOC in comparison to the directly simulated AMOC in some historical simulations after 1990 to 2000, when ocean heat uptake started to accelerate[95].

This additional air-sea heat flux anomaly due to anthropogenic warming incorrectly results in a decline of the reconstructed AMOC in these climate models (Fig. 8), and is hence not visible in the reconstructions across the pre-industrial control simulations (Extended Data Figures 7 and 8). Such a difference between the directly simulated and the reconstructed AMOC is larger than 1Sv and exceeds the 2-sigma standard deviation in models with high equilibrium climate sensitivities (ECSs), for example CanESM5, CanESM5-CanOE, IPSL-CM6A-LR, or UKESM1-0-LL (Fig. 8 and Extended Data Figure 6)[96]. These models all share an ECS that exceeds the recently assessed likely range[97]. The excessive ECS in these climate models leads to an overly strong warming over the recent decades[96]. This strong atmospheric warming then leads to an additional air-sea heat flux into the Atlantic Ocean (Fig. 5 & Extended Data Figure 10). As the historic warming and the ECSs are outside of observed and assessed ranges[96−98], we interpret this mismatch as an exception, rather than the rule of the surface flux reconstruction method. Hence, we conclude that the identified relationship across climate models between AMOC and air-sea heat flux anomalies can be used with confidence until 1990-2000 but may be biased low by up to 1 Sv over the last 20 to 30 years."

and

"In the future, stronger atmospheric warming and associated ocean heat uptake (Figs. 3b, 3c, and Extended Data Figure 10) will further increase the difference between the simulated and reconstructed AMOC based on air-sea heat flux anomalies. While these reconstructions still provide an upper limit for an AMOC decline, air-sea heat fluxes due to atmospheric warming will need to be considered in the coming decades when determining not only an upper limit of the AMOC anomaly based on air-sea heat fluxes but the precise AMOC anomaly."

We have also now mentioned, as suggested by the reviewer, that the reconstructed AMOC variability can be overestimated (e.g., CESM2) or underestimated (e.g., GISS-ES2-1-G). We also want to emphasize here that the r^2 value for this relationship is 0.74 (Fig. 3), thus the relationship is strong but not perfect, so we would not expect the relationship to reproduce all the variability present in the directly simulated AMOC time series. The text in the revised manuscript was thus changed to:

"The comparison between the reconstructed and directly simulated AMOC demonstrates that the air-sea heat flux anomalies can indeed be used to reconstruct the net AMOC anomalies until the 1990s and that the reconstruction provides an upper limit that diverges at most 1Sv from the net AMOC anomaly afterwards. The heat fluxes detect (1) long-term decadal AMOC variabilities as in CMCC-CM2-SR5 (Fig. 8C), (2) AMOC increases around 2000 as in HadGEM3-GC31-LL (Fig. 8M), and (3) AMOC reductions in recent decades as in GFDL-CM4 (Fig. 8W). While the reconstruction is not perfect, e.g., the variability of the reconstructed can be too low (e.g., Fig. 8 i,r) or too high (Fig. 8 s,t), the directly simulated AMOC is within the 2-sigma range of the reconstructed AMOC for 95% of all years, which is expected for a normal distribution."

We further followed up on the reviewers' suggestion that the model resolution might be a factor for a potential bias. In our model ensemble, we have four models that are run at different resolutions: The GFDL models (GFDL-CM4 at high resolution and GFDL-ESM4M at lower resolution), the MPI models (MPI-ESM1-2-HR at high resolution and MPI-ESM1-2-LR at lower resolution), the CNRM models (CNRM-CM6-1-HR at high resolution and CMRM-CM6-1 and CNRM-ESM2-1 at lower resolution), and the HadGEM models (HadGEM3-GC31-MM at higher resolution and HadGEM3-GC31-LL at lower resolution). Across these models there exists no common difference between higher or lower resolution in the capacity to reconstruct the directly simulated AMOC in the historical simulations (Extended Data Figure 5) or in the pre-industrial control simulation (Extended Data Figure 8). We now mention this in the revised manuscript:

"Also, horizontal model resolution does not appear to change the ability to reconstruct the AMOC, as differences between the directly simulated and the reconstructed AMOC are not substantially different between models of different resolutions (see for example the GFDL, MPI, CNRM, and HadGEM models in Fig. 8 and Extended Data Figure 6)."

We hope that these adjustments have made the conclusion more robust and less sloppy.

Tokarska, K. B. et al. Past warming trend constrains future warming in CMIP6 models. Sci. Adv. 6, eaaz9549 (2020).

Comment 2.2

Following on this major comment, the title and main conclusion of this paper (lines 487-490) is misleading and should be rephrased to emphasize the important limitations associated with the method. Instead of providing an estimate of the net AMOC strength, the AMOC reconstructions based on air-sea heat fluxes provides an estimate of the "upper limit for the AMOC decline" in the historical period, as acknowledge by the authors in the results (lines 318-319). Moreover, the strong decrease in the coefficient of determination r2 with increasing latitudes (Table 1) shows that a proxy based on the air-sea heat fluxes is not robust at all latitudes. Then, I suggest modifying the conclusion and title accordingly.

Response:

Following the reviewers suggestion, we have changed the title to:

"Atlantic overturning inferred from air-sea heat fluxes indicates no decline since 1960s"

In addition, we have adjusted the Conclusion to account for the slightly weaker relationship between the AMOC and air-sea heat fluxes at higher latitudes and the strong decrease in the coefficient of determination above 50°N, as suggested by the reviewer:

"The air-sea heat flux derived AMOC estimates are best suited to reconstruct AMOC variability in the subtropical North Atlantic, but also closely track the northward meridional heat transport in the subtropical and subpolar North Atlantic up to 50°N. "

Comment 2.3

Major comment 2: Most of the study relies on correlation estimates between two variables, but there is no indication of significance for these correlations throughout the analysis.

Response:

We thank the reviewer for pointing out this omission. We had not shown all p values as they are in all but one case several orders of magnitudes smaller than 0.05. In the revised manuscript, we now make note of any correlations that are not statistically significant ($p<0.05$) and added the following sentence to the Methods:

"As almost all regressions are statistically significant ($p<0.05$), we only highlight the non-significant relationships throughout the manuscript."

Comment 2.4

Major comment 3: The evaluation of the SPG SST index is based on the same method than described in Caesar et al. (2018), and the conclusion is that the relationship between AMOC and this index is not robust in the ESM CMIP6 models because the SPG region covers a larger area than the 'warming holes', which introduce noise in the SST anomalies. I understand the logic of using the same area than in Caesar et al. (2018), for consistency with this previous study, but I suspect that this SPG area has been defined based on SST patterns from CMIP5 models and might need to be adjusted in the CMIP6 models.

Response:

We might be able to slightly improve the SPG SST index by adapting the region for the CMIP6 ensemble. However, in order to exploit the relationship, a common region would have to work in both the CMIP5 and CMIP6 ensembles. Adapting the region to CMIP6 would very likely worsen the SPG SST index for CMIP5 models. If a given region does not work in one ensemble, one cannot have confidence that this region is the right region to measure the SPG SST index in the real world. Following this comment, and comments from reviewer #3, we have now added a paragraph about emergent constraints to the Introduction that explains the underlying assumptions of that approach to avoid misunderstandings by the reader:

"This concept of exploiting a relationship between an observable and a non-observable variable - in this case, AMOC strength and SPG SST anomalies - with observations has been widely applied in climate science under the name of emergent constraints[22,45˘49]. Emergent constraints allow to reduce uncertainties of projections[48˘51] and to estimate past and present variables or processes that cannot be directly observed[22,34,48]. In the emergent constraint approach models are not used to determine the unknown variable but to determine the slope and intercept of a relationship between two variables. Thus, each model has equal weight in determining that relationship even if a model does not correctly reproduce the historical trajectory of each variable. In fact, the larger the spread in the variables across the model is, the more robustly one can estimate the underlying relationship that is postulated to link the two variables. In the past, however, a number of such relationships that were identified across CMIP5 model ensembles have been later discarded as they were not reproducible across CMIP6 model ensembles[49,52]. These relationships were not reproducible because either the mechanism underlying the assumption was incorrect and the statistical relationship occurred by chance or newly included processes in CMIP6 models, which had not or not adequately been included in CMIP5 models, have altered or even broken the assumed relationship. Given the large amount of data and models across CMIP ensembles, it is not unlikely that relationships can emerge by chance[53˘55]. Consequently, it has been postulated that a relationship can only be exploited with observations if that relationship is mechanistically plausible, if the underlying mechanism is verifiable, and if the relationship is replicable in any other model ensemble[56,57]."

In addition, we also successfully tested our here proposed AMOC reconstructions in CMIP5 (new Extended Data Figure 9) and now mention the successful test in the Results and Discussion.

Comment 2.5

Line 20: Consider removing the term "evolution" at the beginning of the sentence.

Response:

Changed as suggested.

Comment 2.6

Line 41: Consider replacing the term "become" into "form".

Response:

Changed as suggested.

Comment 2.7

Lines 92-94: The sentence is confusing, please clarify. How the air-sea heat fluxes relate more to conservation of energy than SST?

Response:

We agree and have removed this sentence in the process of rewriting this paragraph.

Comment 2.8

Line 94: "We then combine [. . .]": Consider replacing by "We then apply [. . .]".

Response:

As suggested by the reviewer, we have replaced "We then combine [. . .]". However, we wrote "We then exploit [. . .]" instead as we think it fits even better.

Comment 2.9

Lines 107-109: Consider indicating the slopes obtained from your CMIP6 analysis and the one obtained by Caesar et al. (2018) from CMIP5 (if indicated in their paper) for a clearer comparison.

Response:

As suggested, we now include those slopes and also the intercepts in the text. In addition, we added the CMIP5 slope from Caesar et al. (2018) to Fig. 1 to also allow for a visual comparison.

Comment 2.10

Line 111, Figure 1: For clarity, I think that the trend of -1.7 Sv per century found in CMIP5 should be indicated in Figure 1 with a horizontal grey bar.

Response:

Based on this comment and the previous comment, we have added the slope from CMIP5 to Fig. 1. The intercept of that slope with the observed trend of the SPG SST index now indicates the trend of -1.7 Sv per century found in CMIP5 as suggested by the reviewer.

Comment 2.11

Lines 125-126: The sentence is confusing, please rephrase.

Response:

As suggested, we have rephrased and hopefully clarified the sentence:

"Even if the AMOC and SPG SST trends are weak on centennial timescales, strong variability in the AMOC might permit tighter constraints on the relationship between the AMOC and SPG SST index, and allow SST-based reconstructions of the past AMOC on annual or decadal timescales[39]."

Comment 2.12

Line 134: It is difficult to read the numbers in the current version of the Extended Figure 1. All the subplots of this figure should be bigger.

Response:

The size of the subplots was increased as suggested by the reviewer.

Comment 2.13

Lines 138-141: Can you indicate the correlation coefficients for these two Figures (Fig.3 b-c)? Related to major comment (2), can you also indicate the significance of these correlations (here and for all correlations in the study)?

Response:

We now include the r^2 values in panels 3b and 3c. In addition, we now highlight when correlations are not significant and write in the manuscript correlations are significant if nothing else is mentioned (please see response to major comment (2)).

Comment 2.14

Lines 144-145: The sentence is confusing, please clarify by removing "based on reconstructed annual AMOC timeseries".

Response:

As suggested, we have now removed that part of the sentence.

Comment 2.15

Lines 157-159: Please clarify that the strong relationship is only found at decadal and centennial timescales but not at annual timescale.

Response:

As suggested, we now clarify that this relationship holds on decadal and centennial time scales:

"While there appears to be no robust relationship between anomalies of the AMOC and the SPG SST index on annual, decadal, or centennial timescales, a strong relationship between AMOC anomalies at a given latitude and air-sea heat flux anomalies in the North Atlantic north of that latitude exists on decadal to centennial time scales (Fig. 3)."

Comment 2.16

Lines 175-176: "MHTarctic refers to the MHT into the Arctic". Consider indicating "northward MHT into the Arctic".

Response:

Changed as suggested.

Comment 2.17

Line 179, Figure 5: Did you consider that the OHC change is different from zero when evaluating the low correlations at annual timescale, and it is why the evaluation of the mechanisms/causal relationships between the AMOC and air-sea heat flux at annual timescale is left to another study? I am also wondering why the OHC change (blue lines on Figure 5) is different from 0 after 1950 in the multi-model mean, even after smoothing the timeseries with a 11-year running mean.

Response:

The OHC change is different from zero at all timescales (see Fig. 5). Our point on line 179 was that if the system were in equilibrium, i.e., without global warming, the ocean would not have a net heat uptake and OHC would not change. In that case, the three other fluxes would compensate for each other. We have now clarified this in the revised manuscript to avoid further misunderstanding:

"In equilibrium, i.e., without global warming, long-term averaged $\frac{dOHC}{dt}$ is zero and the three fluxes compensate each other"

In reality, the change in OHC is not zero but its magnitude and variability is relatively small compared to the magnitude and variability of the fluxes. The positive OHC due to air-sea heat fluxes lead to the small biases in the reconstructed AMOC by around 1Sv that are discussed in the manuscript and in our response to Comment 2.1.

We also tested if OHC changes can explain the weak variability on annual timescales, as suggested by the reviewer. However, we found no improvement in the relationship when relating the AMOC anomalies to the air-sea heat fluxes and the OHC anomalies. There is likely no improvement as anomalies of the northward MHT across 26.5°N are not related to anomalies of the OHC on annual timescales (r^2=0.04). In the previously submitted manuscript we had therefore already written in the abstract:

"On annual timescales, however, air-sea heat flux anomalies are mostly altered by atmospheric variability and less by AMOC anomalies."

and in the main text:

"On annual timescales, however, the southern MHT anomaly is only very weakly linked to the variability of the air-sea heat flux in the North Atlantic (r^2=0.19). This weak link on annual timescales suggests that other terms such as the change in OHC could be important, and atmospheric dynamics override the overall importance of southern MHT from the tropical Atlantic Ocean[70,82,83]."

Comment 2.18

Lines 183-184, Figure 5: There is a large difference in the variability of these timeseries with, for example, some models showing larger decadal variability than others. It would be interesting to further discuss these differences with regards to the existing literature.

Response:

As suggested by the reviwer we now dicuss these differences in the revised manuscript:

"The southern MHT and surface heat fluxes are the dominant heat budget components in all models, although individual models simulate different magnitudes and frequencies of the variability. These differences might be explained by a different base state of the North Atlantic across models, as the projected decline in the AMOC is also state dependent[36,73], or by different internal variability, for example internal changes in ocean circulation[74], or sensitivities to external drivers, such as atmospheric driven air-sea heat fluxes, across the model ensemble[75−77]. Independent of the driver and magnitude of the variability of the components of the North Atlantic heat budget, a change in the southern MHT over the historical period is largely compensated in climate models by a change in surface heat fluxes and less so by a change in the ocean heat content or the heat flux toward the Arctic Ocean[78]."

Discussing individual models, however, would go beyond the extent and scope of this manuscript.

Comment 2.19

Lines 214-215: Consider indicating that it is related to the "variability of northward MHT" at the same latitude.

Response:

As suggested, we now indicate that it is related to the variability of northward MHT at the same latitude.

Comment 2.20

Lines 220-225: I am not sure to understand the argument here. Does the change in OHC refers to the statement at line 179, the equilibrium is not observed at annual timescale, or to a relatively stronger impact of the changes in MHT into the Arctic? Please clarify. It can also be due to the area over which the air-sea heat fluxes are integrated – the correlation might be better if the area of integration was localised over the northern part of the subpolar gyre (Irminger and Labrador seas) where the dense water is formed, with possibly some lag.

Response:

Here we posit that the lack of a relationship between the air-sea heat fluxes and the MHT anomaly across the southern face on annual time scales could be due to one of two reasons: (1) changes in the OHC on annual time scales could be more important than they are on decadal and longer time scales (i.e., the "equilibrium" the reviewer references does not hold), or (2) the air-sea fluxes are governed by atmospheric variability rather than oceanic variability at annual time scales. Both mechanisms are likely at work: Piecuch et al. (2017) (citation #58) demonstrate the former holds in the subpolar North Atlantic since the early 1990s, and Gulev et al. (2013) (citation #64) demonstrate the latter. We do not see evidence to support the reviewer's suggestions that the air-sea heat fluxes could be localized to the northern part of the subpolar gyre where dense water is formed, and thus we have not pursued this mechanism. Notably, this lack of agreement on annual time scales is not a major focus of this paper because we are primarily interested in characterizing the long-term variability and stability of the AMOC.

Comment 2.21

Lines 242-244: It is not the only difference with the Walin framework. The Walin framework consider both the air-sea heat and freshwater fluxes. The freshwater fluxes (evaporation and precipitation) are relatively important for the AMOC variability, and I am now wondering if the authors tried to evaluate the relationship between AMOC and buoyancy fluxes instead of the heat fluxes alone. Using buoyancy fluxes (and thus including freshwater fluxes in the equation) can possibly lead to less mismatch between the reconstructed and simulated AMOC (see major comment (1)) and, then, to a more robust proxy.

Response:

We thank the reviewer for pointing out this additional difference between the methodology of this paper and the Walin framework, and we now explain this on lines 315-321:

"The heat budget method is distinct from the frequently used Walin framework[84˘86], which infers ocean circulation from surface buoyancy fluxes. The Walin framework aggregates the water mass transformation between sets of outcropping isopycnals, and implies a causal relationship in which the surface buoyancy fluxes force the ocean dynamics[86˘91]. As opposed to the Walin framework, the heat budget mechanism described here relies on surface heat fluxes and does not provide information about a causal relationship; these fluxes are simply connected through equation (2)."

We also agree that using surface buoyancy fluxes could be an avenue of further exploration, but due to large uncertainties in the freshwater fluxes (precipitation and evaporation), especially over the subpolar North Atlantic (e.g. Yu, 2019), such a relationship could not have been robustly exploited with observations so that we have not pursued it here.

Yu, L. Global Air–Sea Fluxes of Heat, Fresh Water, and Momentum: Energy Budget Closure and Unanswered Questions. Ann. Rev. Mar. Sci. 11, 227–248 (2019).

Comment 2.22

Lines 245-246: I am confused, why is the correlation at 0-year lag (Extended Figure 4) different from the 0.22 indicated on Figure 3a?

Response:

In Extended Data Figure 4, we first calculated the correlations for each model independently and then showed for each lag the multi-model average. The idea is to show the spread of the model ensemble. The 0.22 in Figure 3a is the correlation that is calculated from all annual values from all models at the same time. Hence, the correlation is different.

Comment 2.23

Lines 257-260: I am not sure to understand your argument here, are you referring to the difference in the coefficient of determination over the Labrador Sea seen in Figure 7 (a-b)? If not, what can explain this difference over the Labrador Sea? Please clarify.

Response:

Yes, we are referring to the difference between Figs. 7a and 7b, and indicating that the difference over the Labrador Sea can be explained by air-sea fluxes dampening SST variability in the western subpolar gyre (i.e., the Labrador Sea). We have now made this connection of the text to the figure more clear:

"The similarity between the spatial patterns underlines that changes in air-sea heat fluxes and SPG SST are linked. However, the local r^2 is higher when using heat fluxes instead of SST (compare panels a and b in Fig. 7), suggesting that air-sea heat fluxes dampen the SST changes and are thus more tightly connected to the AMOC."

Thank you for pointing out this lack of clarity.

Comment 2.24

Line 333: Remove "Fig. 9B". The AMOC reconstruction for JRA55 at 26N is shown in Fig. 9A.

Response:

Changed as suggested.

Comment 2.25

Lines 361-366: Although the reconstructed decadal variability at 26N cannot be compared with the too short RAPID timeseries, the reconstructed long-term trends can be evaluated as compared with the observed one. Can you indicate the observed trend for comparison?

Response:

As suggested by the reviewer, we now show these decadal averages from 2009 to 2018 based on the RAPID array in Figure 9a. Unfortunately, we cannot compare the trends quantitatively as we only reconstruct decadal averages. For a proper comparison, only decadal averages ($\pm$5 years) of observed values exist only from 2004 to 2023 so that decadal averages can only be obtained between 2009 and 2017. Over such a short period, any linear trend would be highly uncertain.

Comment 2.26

Lines 370-372: Figure 9 does not show a reconstruction of the AMOC at the OSNAP array, which is not at constant latitudes, or am I missing a figure?

Response:

We had intended to use one of the latitudes an approximation of the location of the OSNAP array, which is indeed not correct as highlighted by the reviewer. We apologize for this oversight – we have now removed this sentence.

Comment 2.27

Lines 459-460: It is not robust after 1990, see major comment (1).

Response:

As described in our response to major comment (1), we have addressed this issue at many places in the manuscript. In this particular place, we have changed the text as follows to account for the reviewers suggestions:

"These tests were successful for all climate models in the pre-industrial control simulations and for most climate models in the historical simulations. Due to anthropogenic warming in the last decades a small bias of 1Sv towards too low reconstructed AMOCs may arise once anthropogenic warming leads to additional air-sea heat flux anomalies that are not related to the AMOC. However, that bias is well within the uncertainties. Only the climate models with excessive ECS have reconstructed AMOC anomalies that are more than 2-sigma different from the directly simulated AMOC over the more recent decades. As only two models with unrealistic warming failed the test, the relationship or emergent constraint proposed here seems indeed to be robust but may lead to a small bias towards too negative AMOC anomalies since around 1990 to 2000."

3 Response to reviewer 3

General Comments

The paper by J. Terhaar et al. tests - using 24 different CMIP6 models - in how far air-sea flux anomalies can be used to recreated AMOC strength. They find that this works better than the previously used SPG SST anomalies, albeit is only able to represent AMOC changes on decadal to centennial time scales.

The paper is well written and fairly easy to understand, but has several flaws in the underlying reasoning. These are the well-known overestimation of the aerosol forcing in the CMIP6 models, the discrepancy in the evolution of the historical AMOC in CMIP5 vs CMIP6 models, ignoring the atmospheric influence on the air-sea forcing on longer time scales, not considering the observed OHC changes when reconstruction the "real-world" AMOC, and some others (please see comments on the pdf for more details).

I would therefore suggest that all these issues need to be answered first, the analysis needs to be extended and the results double-checked before a publication can be considered.

In principle I believe that considering air-sea fluxes to determine AMOC strength can be an asset, but they should be considered in addition to changes in OHC/SST not as the only source of information.

Response:

We thank the reviewer for their evaluation and comments, to which we have responded point-by-point. We believe that there were some misunderstandings and disagreements that we have addressed in detail below. Overall, we have taken each comment into account, provide responses to each point below, and adapted the manuscript accordingly.

Comment 3.1

1. You cite a paper (Li et al., 2022) that states that the long-term cooling in the subpolar gyre region is driven by the atmosphere (i.e. the air-sea flux) which makes SPG SST a bad AMOC indicator yet argue at the same time that this air-sea flux is a better AMOC indicator than the SSTs - how does that work?

Response:

In this sentence we are citing papers that have questioned the connection between SST and the AMOC. Some of these results are themselves seemingly mutually exclusive - our goal in this portion of the text is not to question those results but to demonstrate to the reader that we are not the first to find cracks in the SPG SST-AMOC relationship. However, we briefly explain here why our results and the Li et al. (2022) results are not mutually exclusive as suggested by the reviewer:

Here, we demonstrate a robust relationship between the AMOC and air-sea heat flux anomalies in the North Atlantic. We further show that changes in the OHC (or SST) are a minor contributor to the total heat budget on decadal and centennial, leaving the two fluxes (lateral heat convergence) and surface heat fluxes to compensate one another. The central result of Li et al. (2022) is that the trend in OHC (or SST), which is small compared to the trend in overall air-sea heat fluxes, can be explained by air-sea heat fluxes. As the largest anomalies in the air-sea heat flux seem to be related to the AMOC, this does not exclude that the comparatively rather small anomalies in the SST/OHC are driven by smaller air-sea heat flux anomalies that are superimposed on the large AMOC-related air-sea heat flux anomalies. Hence, the results presented herein and the results from Li et al (2022) can both be true, and thus we do not see the paradox that the reviewer seems to reference.

Comment 3.2

2. Zhu et al. 2023 also use a salinity fingerprint and even argue for an accelerated weakening of the AMOC - you should mention this.

Response:

As suggested, we now reference the study by Zhu et al., 2023. Following the reviewers suggestion, we now emphasize even more prominently that other proxies also estimate a decline over the past decades.

Comment 3.3

3. True, but again: all these papers say is that SPG SSTs are influenced by the ocean and the atmosphere (the latter via air-sea fluxes) and now you say that this air-sea fluxes are a better AMOC indicator than those SSTs - doesn't seem logical to me.

Response:

Again, we are confused by what paradox the reviewer is referencing. Below, we show that most of the northward heat flux anomalies and hence to the AMOC anomalies are linked to air-sea heat flux anomalies. As the relationship between the AMOC anomalies and the heat flux anomalies is strong but not perfect, this does not exclude that the AMOC anomalies can also influence the SSTs at the same time, which they do. In addition, it also does not exclude that there is part of the air-sea heat flux anomaly that influences the SSTs, which it also does. Hence, we do not understand the argument why it is not possible that most of the AMOC induced northward heat flux anomaly results in air-sea heat flux anomalies while at the same time SSTs can be weakly linked to the AMOC (as shown in Figure 2), and be influenced by the atmosphere and the ocean. Based on this line of argument, which we explained in detail in the previously submitted manuscript, tracking the fluxes and not the SST/OHC is a logical outcome of these results.

Comment 3.4

4. Sorry, but again doesn't sound logical to me. That would as like saying that, e.g., the power of the radiator in my apartment is more closely linked to the heat loss from my apartment to the outside than to the actual temperature in my apartment. While the former of course also influences the resulting temperature (greater heat loss in winter, of my door is open, etc) - the relationship between heater and temperature is still more direct.

Response:

We enjoyed thinking of this analogy and thank the reviewer for bringing it up. While we understand the reviewer's point that the temperature in a room is clearly influenced by the heat flux out of a radiator, when one averages over long enough time scales (e.g. annual averaging) the temperature of the room is largely unchanged, while the radiator puts out heat throughout the year. Thus the balance in this case is between the heat flux from the radiator and the heat flux to the surroundings, while the change in heat content of the room is negligible. This result of time-scale sensitivity is directly analogous to this manuscript – we do not find that the balance between lateral heat convergence and air-sea heat fluxes holds on short time scales but does hold on decadal and longer time scales.

Comment 3.5

5. False conclusion - indeed there are a few papers (e.g. Gong et al., 2022) that indicate that the AMOC in CMIP6 models agrees less with the RAPID observations than the ones in the CMIP5 models, i.e. it is possible that CMIP6 models do a poorer job in simulating the AMOC. E.g. Hassan et al, 2021

Response:

We believe that this is a misunderstanding. The underlying method that Rahmstorf et al. (2015) and Caesar et al. (2018) have used is referred to as emergent constraint when used to reduce uncertainties in climate projections (see for example Cox et al. (2018), Terhaar et al. (2020) or Terhaar et al. (2021)). This concept thus does not rely on a model to simulate a particularly good evolution of the AMOC but on the fact that all models show the same link between two variables, i.e., a relative cooling of the North Atlantic or a heat flux anomaly into the North Atlantic in response to a weakening AMOC. Following this misunderstanding and also based on a comment from reviewer #2, we have added a new paragraph to the Introduction to introduce this concept and the underlying assumptions:

"This concept of exploiting a relationship between an observable and a non-observable variable - in this case, AMOC strength and SPG SST anomalies - with observations has been widely applied in climate science under the name of emergent constraints[22,45˘49]. Emergent constraints allow to reduce uncertainties of projections[48˘51] and to estimate past and present variables or processes that cannot be directly observed[22,34,48]. In the emergent constraint approach models are not used to determine the unknown variable but to determine the slope and intercept of a relationship between two variables. Thus, each model has equal weight in determining that relationship even if a model does not correctly reproduce the historical trajectory of each variable. In fact, the larger the spread in the variables across the model is, the more robustly one can estimate the underlying relationship that is postulated to link the two variables. In the past, however, a number of such relationships that were identified across CMIP5 model ensembles have been later discarded as they were not reproducible across CMIP6 model ensembles[49,52]. These relationships were not reproducible because either the mechanism underlying the assumption was incorrect and the statistical relationship occurred by chance or newly included processes in CMIP6 models, which had not or not adequately been included in CMIP5 models, have altered or even broken the assumed relationship. Given the large amount of data and models across CMIP ensembles, it is not unlikely that relationships can emerge by chance[53˘55]. Consequently, it has been postulated that a relationship can only be exploited with observations if that relationship is mechanistically plausible, if the underlying mechanism is verifiable, and if the relationship is replicable in any other model ensemble[56,57]."

In addition, we also successfully tested our here proposed AMOC reconstructions in CMIP5 (new Extended Data Figure 9) and now mention the successful test in the Results and Discussion.

Caesar, L., Rahmstorf, S., Robinson, A. et al. Observed fingerprint of a weakening Atlantic Ocean overturning circulation. Nature 556, 191–196 (2018). https://doi.org/10.1038/s41586-018-0006-5

Cox, P., Huntingford, C. & Williamson, M. Emergent constraint on equilibrium climate sensitivity from global temperature variability. Nature 553, 319–322 (2018). https://doi.org/10.1038/nature25450

Eyring, V., Cox, P.M., Flato, G.M. et al. Taking climate model evaluation to the next level. Nature Clim Change 9, 102–110 (2019). https://doi.org/10.1038/s41558-018-0355-y

Hall, A., Cox, P., Huntingford, C. et al. Progressing emergent constraints on future climate change. Nat. Clim. Chang. 9, 269–278 (2019). https://doi.org/10.1038/s41558-019-0436-6

Rahmstorf, S., Box, J., Feulner, G. et al. Exceptional twentieth-century slowdown in Atlantic Ocean overturning circulation. Nature Clim Change 5, 475–480 (2015). https://doi.org/10.1038/nclimate2554

Schlund, M., Lauer, A., Gentine, P., Sherwood, S. C., and Eyring, V.: Emergent constraints on equilibrium climate sensitivity in CMIP5: do they hold for CMIP6?, Earth Syst. Dynam., 11, 1233–1258, https://doi.org/10.5194/esd-11-1233-2020, 2020.

Terhaar, J., Kwiatkowski, L. & Bopp, L. Emergent constraint on Arctic Ocean acidification in the twenty-first century. Nature 582, 379–383 (2020). https://doi.org/10.1038/s41586-020-2360-3

Terhaar, J., Frölicher, T. L. & Joos, F. Southern Ocean anthropogenic carbon sink constrained by sea surface salinity, Sci. Adv. 7, eabd5964 (2021).

Comment 3.6

6. This is actually not that surprinsing, as several studies have shown that CMIP6 models overestimate the Aerosol forcing which messes up the relationship between SPG SST and AMOC strength

Response:

Due to other changes in the paragraph (see next comment), the sentence does not exist anymore in this form in the revised manuscript.

Comment 3.7

7. This just highlight that the CMIP6 models do simulate a cooling in the SPG (-0.44K/century - that is similar to the CMIP5 cooling), but - likely due to the stronger Aerosol forcing - not an AMOC weakening.

Response:

We thank the reviewer for pointing out another example how SPG SSTs can change without an associated change in the AMOC, highlighting the weakness of the SPG SST index. Following the reviewers comment, we have added the following sentence to the manuscript:

"In the CMIP6 ensemble, 10 of the 24 climate models that have a statistically significant ($p<0.05$) trend in one variable and no statistically significant trend in the other (Fig. 1, Extended Data Figures 1 & 2). Some models show, for example, a change in the in the SPG SST index without an associated change in the AMOC, which might be a consequence of a potentially overly strong sensitivity to aerosol forcing in some CMIP6 models[67]. Although the aerosol sensitivity might be too strong in the CMIP6 models, the resulting SPG SST change should also result in an associated AMOC change if both variables were strongly linked. While aerosols are one example how the AMOC and the SPG SST index can be affected independently, there are likely others[61˘63], such as air-sea heat flux anomalies from the atmosphere and heat transport anomalies towards the Arctic Ocean[64]."

Comment 3.8

8. I've given quite a few reasons above that show that this deduction is not 100% correct. For a possible explanation see McCarthy & Caesar 2023.

Response:

We thank the reviewer for pointing us to McCarthy and Caesar (2023). We believe the reviewer is referring to this manuscript: https://doi.org/10.1098/rsta.2022.0193. We had read it with great interest when it was published and now read it again, but we could not find an additional argument. We would be grateful if the reviewer could help us finding it. Nevertheless, we would like to refer the reviewer to our response to comment 3.5 in which we explain that our approach does not reply on the capability of models to replicate the past but relies on the fact that the relationship between two variables is robust across models and different scenarios, which it is in the case of air-sea heat fluxes and the AMOC.

Comment 3.9

9. Over what depth are you calculating the OHC?

Response:

We now clarified in the revised manuscript that the OHC is integrated over the full depth of the ocean.

Comment 3.10

10. Theoretically there is also the heat flux from the deeper ocean layers - depends a little on what range you are looking at. Well here it depends a lot on what depth range you are looking at...

Response:

As described above, the OHC is integrated over the full depth.

Comment 3.11

11. But Piecuch et al. e.g. say that changes in ocean heat content in the subpolar gyre are driven by heat advection - this does not support your theory.

Response:

Indeed, the references do neither support or contradict our theory. While most of the northward heat transport anomaly results in an associated air-sea heat flux anomaly, this does not exclude that smaller OHC anomalies are also driven by smaller northward heat flux anomalies. Overall, the relationship between northward heat flux and air-sea heat flux anomalies is strong but nor perfect leaving room for other processes.

We thus agree with the reviewer that this reference and ref 59 were not good choices for this sentence, and have removed them.

Comment 3.12

12. So you look at air-sea flux over which region? 26.5N-90N or what do you mean with north of? Ah, it's 26.5-55°N - found it. Maybe link to the SM here.

Response:

We calculate air-sea fluxes over the region between the latitude mentioned in the text (ranging from 26.5°N to 55°N) and a northern boundary of Davis Strait, Fram Strait, and the Barents Sea Opening as shown in Figure 4. We now clarify this on line 780. Furthermore we have clarified the sentence in the figure legend mentioned by the reviewer and now write:

"The North Atlantic air-sea flux anomalies north of 26.5°N and south of the Arctic (see orange area in Fig. 4 and methods for a detailed definition of the region) are..."

Comment 3.13

13. Should probably not mark the area above 55°N in orange - in accordance with your calculations?

Response:

As noted above, this figure shows the correct region of integration for the air-sea heat fluxes.

Comment 3.14

14. 18? (Line 213)

Response:

We thank the reviewer for spotting this mistake, which has now been corrected as suggested.

Comment 3.15

15. Could you provide a plot with southern MHT anomaly and SST average over 26.5-55°N anomaly? Just to make sure that the better correlation is not just due to the larger area.

Response:

As suggested by the reviewer, we tested if the correlation gets better if SST anomalies are calculated over the entire North Atlantic region, the same region over which the air-sea heat fluxes were integrated. When using these average SSTs over the entire North Atlantic, the coefficients of determination are r^2=0.02 for annual averages, r^2=0.02 for decadal averages, and r^2=0.26 for centannial averages. Hence, increasing the size of the region does not improve the relationship and we decided to not add an additional figure.

Comment 3.16

16. 18? I though 6 ESMs don't provide MHT?

Response:

We apologize for this oversight and thank the reviewer for spotting this mistake, which has now been corrected as suggested.

Comment 3.17

17. But this means that you do not account for any changes in OHC/SST in your calculation going going from air-sea flux to AMOC strength, correct?

Response:

Yes this is correct; we do not consider changes in OHC/SST when assessing the relationship between the AMOC and the air-sea heat flux.

Comment 3.18

18. Would be interesting if you would plot SST anomalies with this.

Response:

Following the reviewers' suggestion, we now added an additional Extended Data Figure that shows the directly simulated AMOC and the reconstructed AMOC using the relationship between the decadal averages of the SPG SST index and the AMOC. As the SPG SSTs and the reconstructed AMOC are linearly related, the figure allows to compare the SST anomalies and the AMOC at the same time as it allows to demonstrate the large uncertainties in the reconstructed AMOC when using the SPG SSTs that results from the low coefficient of determination (r^2=0.30) of that relationship.

Comment 3.19

19. Some studies suggest that many CMIP6 models underestimate global warming (have a too small ECS), e.g. Smith & Forster, 2021

Response:

We have read the manuscript with great interest. However, this manuscript seems to treat the period before 2000 while the paragraph that the reviewer is referring to treats the years after 1990 to 2000. Moreover, Smith & Forster (2021) show that CMIP6 models have a larger climate sensitivity than CMIP5 models (see Table 1 in their manuscript). Other studies also found that the CMIP6 models have a higher sensitivity to climate forcing than CMIP5 models and that this sensitivity is too large (Tikarska et al. (2020) & Nijsse et al. (2020)). Unfortunately, we do not understand the comment and how we should add this reference and would be grateful for further explanations.

Nijsse, F. J. M. M., Cox, P. M., and Williamson, M. S.: Emergent constraints on transient climate response (TCR) and equilibrium climate sensitivity (ECS) from historical warming in CMIP5 and CMIP6 models, Earth Syst. Dynam., 11, 737–750, https://doi.org/10.5194/esd-11-737-2020, 2020.

Tokarska, K. B., Stolpe, M. B., Sippel, S., Fischer, E. M., Smith, C. J., Lehner, F., & Knutti, R. (2020). Past warming trend constrains future warming in CMIP6 models. Science Advance, 6, eaaz9549.

Comment 3.20

20. This works only if you assume that changes in atmoshpheric conditions can not have an impact on decadal trends in the air-sea flux which we know is not true (e.g. NAO).

Response:

We believe that this is a misunderstanding. In our manuscript, we do not claim a one-directional relationship, i.e., that only the AMOC is driving the air-sea heat fluxes or that only air-sea heat fluxes drive AMOC changes. Instead we assume that changes in the AMOC lead to changes in air-sea heat fluxes AND that changes in air-sea heat fluxes lead to AMOC changes. In the previously submitted version of the manuscript we had written:

"As opposed to the Walin framework, the heat budget mechanism described here relies on surface heat fluxes and does not provide information about a causal relationship; these fluxes are simply connected through equation (2). Although a lead-lag correlation analysis of the climate models from CMIP6 shows that air-sea heat fluxes lead the AMOC on average by around 3 years (Extended Data Figure 5), similar correlations exist also for a lead of air-sea heat fluxes by 10 years and for a lead by the AMOC by 7 years. As different mechanisms appear to be linking the AMOC and air-sea heat flux anomalies[86˘93], we do not attempt to infer a causal relationship and simply rely on the heat budget that averages over these lead/lag relationships, and holds on decadal time scales."

It is thus no contradiction that air-sea heat fluxes are affected by other drivers such as NAO. Based on our here provided relationship, such non-AMOC-driven decadal trends in the air-sea fluxes imprint on the AMOC. The NAO is likely responsible for some of the variability in the air-sea heat fluxes, which results in variability in the reconstructed AMOC in Fig. 8, which in turn is similar to the variability in the directly simulated AMOC. We have added a reference (Buckley et al., 2016) to the manuscript that describes this link:

Buckley, M. W. & Marshall, J. Observations, inferences, and mechanisms of the Atlantic Meridional Overturning Circulation: a review. Rev. Geophys. 54, 2015RG000493 (2016).

Comment 3.21

21. This is reason enough to not use this product over a region where there is ample evidence that the aerosol forcing is strong - like the North Atlantic.

Response:

Although JRA-55 does not prescribe aerosol changes explicitly, it incorporates variability in aerosol forcing indirectly through assimilation of observations, which are strongly affected by aerosols. Thus, we do not believe this is reason to discard it from our analysis. We now clarify this on lines 355-357:

"Instead, JRA-55 indirectly incorporates variability in aerosols through its data assimilation of observations, which are subject to realistic aerosol forcing."

Comment 3.22

22. The difference in the data products in early decades already suggest that you can likely only trust them post 1990, correct?

Response:

The uncertainties of the reconstructions are only based on the relationship between and not based on the uncertainty of the observation-based estimates of the air-sea heat fluxes. Hence, we interpret the range of these two products as the additional uncertainty from the observation-based estimates. This range becomes smaller over the recent decades as observations become more frequent, leading to a better agreement between the reanalysis products.

In addition, we would like to highlight that the products almost always agree within the uncertainties and that the magnitude of the difference between both estimates is relatively small compared to anomalies simulated by CMIP6 models (Fig. 8). As we have changed the range on the y-axis in Fig. 9, the relatively small difference between the products appears larger than it is.

We thus have confidence in the results before and after 1990.

Comment 3.23

23. The increase in heat flux from the ocean to the atmosphere is likely mainly related to the increase in atmospheric T - I would not read it as an fairly stable AMOC.

Response:

We do not follow the reviewer's logic here – an increase in atmospheric T would indicate an increased heat flux from the atmosphere to the ocean and not the other way round.

Nevertheless, we want to highlight that the small trends in the reconstructed AMOC were already mentioned in the previously submitted version of the manuscript:

"Across both reconstructions, a positive trend for the AMOC at 26.5°N can be found from 1963 to 2017 of 0.022±0.005 Sv per decade (ERA5) and 0.022±0.003 Sv per decade (JRA-55). "

In addition, the anomaly of the reconstructed AMOC always stays within ±1.5 Sv, which is fairly stable, especially compared to the large anomalies in CMIP6 models (Fig. 8) or the previously published AMOC changes, e.g., a decline of 3±1 Sv since the mid-20th century (Caesar et al., 2018).

Comment 3.24

24. Wait, you only showed the correlation between AMOC at 26.5 N and air-sea flux between 26.5-55°N; how did you estimate the others?

Response:

As stated above, the fluxes are integrated over the area between the latitude noted (25°N – 55°N) and the northern boundary of the Davis Strait, Fram Strait, and the Barents Sea Opening. We realized that the figure caption might have created this misunderstanding and have changed it to:

"Reconstructed decadal averaged AMOC anomalies with respect to the period from 1963 to 2017 at (**a**) 26.5°N, (**b**) 30°N, (**c**) 35°N, (**d**) 40°N, (**e**) 45°N, (**f**) 50°N, and (**g**) 55°N based on air-sea heat flux anomalies from ERA5 (blue) and JRA-55 (orange) integrated over the North Atlantic between that latitude and the Arctic Ocean (see Methods for detailed description of the regions boundaries)."

Comment 3.25

25. But, as you mention in the beginning, there are a lot of other AMOC reconstructions with no long-term trend, please compare to a few of those.

Response:

As suggested by the reviewer, we now added a paragraph to the Discussions in which we discuss our results in the broader context of other proxy reconstructions:

"Proxy-based reconstructions of AMOC variability at 26.5°N over the past century have yielded a range of possible time series, some that show a decline, and some that do not. Two recent review papers have attempted to detect any coherence in these records by combining AMOC variability across a range of proxies[44,119]. Unfortunately, these reviews disagree with one another: Moffa-Sanchez et al. (2019)[119] use a wide range of available proxies across the North Atlantic and find no coherent variability, while Caesar et al. (2021)[44] use a subset of the available proxies that the authors deem to be most robust and find a reduced AMOC strength starting in either the second half of the 19th century or the 1960s. The disagreement between these reviews revolves around which proxies should be considered[63,120] and whether using more proxies is appropriate. Our reconstructions cannot disentangle this issue but we do see limited evidence of a peak in the AMOC prior to the 1960s (Fig. 8) as proposed in Caesar et al. (2021)[44]. Similarly, we see no evidence of a long-term decline in the AMOC since the second half of the nineteenth century (Fig. 8) as proposed by Caesar et al. (2021)[44]."

Comment 3.26

26. but you never look at the observed changes in OHC?

Response:

Correct, we do not consider the observed changes in OHC. In Fig. 5, we demonstrate that this term is negligible on decadal and longer time scales in the CMIP6 models. Therefore, we cannot use observed OHC changes and our AMOC reconstruction is thus based on the air-sea heat fluxes.

1 Response to reviewer 1

General Comments

The manuscript "Atlantic overturning inferred from air-sea heat fluxes indicates no decline since 1960s" provides important results for the debate on AMOC slowdown, as the authors use a new approach to reconstruct AMOC anomalies based on air-sea heat fluxes and show that AMOC hasn't declined during 1963 to 2017 period. I believe the current version of the manuscript has been significantly improved. Therefore, I recommend its publication, pending some minor revisions.

Response:

We thank the reviewer for their positive evaluation and the additional minor comments. We have taken each comment into account, provide responses to each point below, and adapted the manuscript accordingly.

Comment 1.1

LN 90-91: "either the mechanism underlying the assumption was incorrect" - reference?

Response:

The references 49 and 52, which were already cited at other places in the paragraph, were added here as suggested by the reviewer at the end of the sentence.

Comment 1.2

LN 93: "have altered or even broken the assumed relationship" - reference?

Response:

The references that were added here are the same as the references for the lines mentioned in comment 1.1. As the passages that are highlighted by the reviewer occur in the same sentence, we added the references 49 and 52 at the end of this sentence as explained in the response to comment 1.1.

Comment 1.3

Fig.1 Reference cited in figure caption is #34, reference displayed on figure is #36

Response:

The reference in Fig. 1 has been adjusted as suggested.

Comment 1.4

LN 170-171: "The larger intercept might be a consequence of the impact of aerosols on the AMOC that do not affect the SPG SSTs" not sure if the cited paper supports this conclusion. Here is the paper from Zhu et al., 2023, where authors show that AER forcing affects AMOC and Temperature in SPG (their Fig.4)

Zhu, C., Liu, Z., Zhang, S., & Wu, L. (2023). Likely accelerated weakening of Atlantic overturning circulation emerges in optimal salinity fingerprint. Nature Communications, 14(1), 1245.

Response:

Following the reviewers comment, we now realize that our sentence was not well formulated as aerosols indeed affect SPG SSTs. Our intent was to highlight that aerosols strongly affect the AMOC but only weakly the SPG SSTs. Moreover, there appears to be no connection between both changes as the SPG SST anomalies are first negative and then positive while AMOC anomalies are always positive in the simulations where only aerosols change. In the revised manuscript, we corrected the sentence to better transfer the intended meaning behind the sentence:

"The larger intercept might be a consequence of the relatively large impact of aerosols on the AMOC and the relatively small or even opposite impact of aerosols on SPG SSTs[67]."

Comment 1.5

Fig.5 Maybe make "panel a" bigger with the display of mean anomaly values and show the single GCMs as smaller subplots?

Response:

We thank the reviewer for the suggestion. However, we prefer to keep the emphasis on the individual models by keeping the panels the same size as the multi-model mean. Making the multi-model mean larger would de-emphasize the internal climate variability that can be seen in the individual models, but is largely removed in the multi-model mean. As the internal climate variability in the individual models is what is used later to reconstruct the AMOC, we decided to keep the figure unchanged.

Comment 1.6

Fig.5 "The missing 6 climate models from the total ensemble of 24 climate models did not provide the northward heat transport and are therefore not shown here." This information should be in methods as well

Response:

The information has now been added to the methods as suggested by the reviewer:

"In addition, simulated northward heat transport in the North Atlantic was used to corroborate the proposed mechanism in the manuscript. This variable was only available for 18 of the 24 ESMs (Fig. 5)."

Comment 1.7

Extended Data Fig.5 - Also maybe since there is a weak yearly relationship between surface heat flux and AMOC, you can apply a running mean of 5 to 10 years to compute lagged correlations? Will it show stronger correlation?

Response:

As suggested, we now also calculated the correlation for a running mean of 10 years. The correlation is indeed stronger and Extended Data Fig. 5 was accordingly updated (Supplementary Fig. 5 in the revised manuscript).

Comment 1.8

LN 354-359 and rest of the text: It might be helpful to use the subscripts AMOC_r for reconstructed anomalies and AMOC_ds for directly simulated anomalies, as the wording becomes quite confusing throughout the text.

Response:

We had previously thought about introducing additional acronyms. However, too many acronyms might make it harder for the more general audience to follow. Therefore, we have decided to not use subscripts as suggested by the reviewer.

Comment 1.9

LN 366: reconstructed AMOC

Response:

We have added the word 'AMOC' as suggested by the reviewer.

Comment 1.10

LN 422-425: Please consider rewriting the sentence for clarity, as it is currently hard to follow.

Response:

As suggested by the reviewer, the sentence has been clarified and now reads as follows:

"The uncertainties (shading in Fig. 9) are solely based on the linear fit (Fig. 3) and do not include the uncertainties of the observation-based surface heat flux anomalies themselves[99]"

Comment 1.11

LN 459-461: I'm not sure if it can be accurately described as a decline.

Response:

As the decline is indeed very small in the observations, we have added the word 'small' in front of decline. We nevertheless kept the sentence as one of the reviewers had specifically asked for a comparison to the RAPID data in the last review round.

Comment 1.12

LN 548 I would add at the end: strong relationship between air-sea heat flux anomalies and AMOC anomalies on decadal and longer time scales.

Response:

As suggested by the reviewer, we have added "on decadal and longer time scales" at the end of the sentence.

Comment 1.13

LN 900-905: you should mention that 6 models didn't provide northward heat transport.

Response:

The information was mentioned as suggested. Please see response to comment 1.6.

Comment 1.14

LN 971-977: Please consider rewriting the paragraph for clarity, as it is currently hard to follow.

Response:

We understand that this explanation may be difficult to follow. However, we had already shortened the sentences as short as possible and had aimed for using the most precise words to describe the calculations accurately. Hence, we unfortunately do not know how we could increase the clarity of the paragraph.

Comment 1.15

Extended data Fig.3: colorbar?

Response:

The colorbar has been added as suggested.

Comment 1.16

Extended data Fig.8: do you mean difference between reconstructed and directly simulated AMOC here?

Response:

We have now added the words 'difference between the' to highlight that we meant indeed the difference between reconstructed and directly simulated AMOC here.

2 Response to reviewer 2

General Comments

The authors responded to my concerns by clarifying the text on their main conclusion, i.e., the limitation of the AMOC reconstruction analysis (comment 2.1-2.2). It improved the manuscript by making the conclusion more robust, to my opinion. The description of the emergent constraint approach is also welcomed to clarify the underlying assumption associated with this method (comment 2.4).

...

The manuscript now stands well by itself and can be published in the present form.

Response:

We thank the reviewer for their positive evaluation. We provide a response to the last outstanding point below, and adapted the manuscript accordingly.

Comment 2.1

I still think that an evaluation of the buoyancy fluxes (including freshwater fluxes in the equation), instead than air-sea heat fluxes alone, could be an interesting addition to the manuscript, possibly leading to less mismatch between the reconstructed and simulated AMOC (comment 2.21), but I understand that it might be beyond the scope of the paper. Note, however, that there are large uncertainties in both air-sea heat and freshwater fluxes between reanalyses, and because the authors interpret the range between the two reanalyses as additional uncertainties from the observation-based estimates of heat fluxes, something similar could be done for the freshwater fluxes.

Response:

We agree with the reviewer that an evaluation of the buoyancy fluxes is an interesting idea that merits further research. However, the aim of this paper was to build on the concept of Caesar and Rahmstorf and to account for the concept's weaknesses by introducing the idea of the heat budget in the North Atlantic. While adding freshwater fluxes is thus indeed beyond the scope of this manuscript, we have now added the following paragraph in line with comment 3.1 from the third reviewer:

"In addition to air-sea heat flux anomalies, other variables such as net surface buoyancy fluxes that incorporate both heat and freshwater fluxes could be used to improve the reconstructions of the AMOC. Multi-variable constraints[22] using heat and freshwater fluxes separately could also lead to further improvements. Here, we have not used freshwater fluxes due to large uncertainty in precipitation minus evaporation over the open ocean[99], and correlation between surface fluxes of heat and freshwater complicates the calibration of a multi-linear relationship using both of these flux products. Further research is needed to test if the addition of freshwater fluxes or salinity changes might further improve the reconstructed estimates of the AMOC anomaly."

3 Response to reviewer 3

General Comments

Overall the authors made a good effort in responding to my comments, I in general I am in favor of publication. Nevertheless, I think there are a two areas where the paper would benefit from a clearer justifications and/or additional methodological detail:

Response:

We thank the reviewer for their positive evaluation. We have taken the last two outstanding comments into account, provide responses to each point, and adapted the manuscript accordingly.

Comment 3.1

Clarification of Methodology: While the authors give good justifications for using air-sea fluxes, I am missing a clear explained how this choice impacts the reliability of AMOC reconstructions, i.e. a more detailed explanation of why air-sea fluxes alone are sufficient could strengthened the discussions.

Response:

Following the reviewers comment, we have added the following paragraph to the Discussion:

"In addition to air-sea heat flux anomalies, other variables such as net surface buoyancy fluxes that incorporate both heat and freshwater fluxes could be used to improve the reconstructions of the AMOC. Multi-variable constraints[22] using heat and freshwater fluxes separately could also lead to further improvements. Here, we have not used freshwater fluxes due to large uncertainty in precipitation minus evaporation over the open ocean[99], and correlation between surface fluxes of heat and freshwater complicates the calibration of a multi-linear relationship using both of these flux products. Further research is needed to test if the addition of freshwater fluxes or salinity changes might further improve the reconstructed estimates of the AMOC anomaly."

Comment 3.2

Addressing CMIP5 vs. CMIP6 Discrepancies: The authors responded to my concerns about differences between CMIP5 and CMIP6 trends and aerosol forcing, but I would like a more in depth discussion on the implications of these differences (and why the authors feel that these discrepancies do not undermine their findings).

Response:

We are a bit confused by the comment. In the last revisions we have shown that the reconstructions are robust across CMIP5 and CMIP6 models despite the different aerosol forcings. As there are no differences in the reconstructions of the AMOC between both model ensembles, we do not understand what implications could be discussed. We hope that the addition of the following sentence to the manuscript has appropriately addressed the comment of the reviewer:

"The consistency in the relationship between air-sea heat flux anomalies and AMOC anomalies across CMIP5 and CMIP6 model ensembles despite the difference in the effect of aerosol forcing between CMIP5 and CMIP6 models[67] further supports the finding that air-sea heat fluxes and the AMOC are indeed mechanistically connected."