Response to Referee#1

We thank the anonymous referee for their valuable comments and constructive reviews. These comments certainly help us to identify the sections in the manuscript that required improvement. We highlight the major improvements as follows:

- We carried out an analysis of ocean heat transport convergence in two regions covering the tropical and the subtropical North Atlantic, adding two new panels for Fig. 4;
- We included a more detailed comparison of statistical significance of skill differences given the AMOC state, adding a new figure (Fig. 9);
- We rewrote major portions of the manuscript to make it clearer and improve the reading flow.

We took into consideration all suggestions made by the reviewer and we provide below a point-by-point response to each comment. Please note that the referee's comments are highlighted in **bold** font, while our answers are in regular font.

Primary concerns:

1. The study relies on the mechanism of Duchez et al. (2016) (D16), and much of the analysis is a repeat of that done in D16 using the MPI-ESM rather than observations. The mechanism suggests that (l.36-37) "a stronger than average AMOC at 26N advects more heat northward, leading to colder waters in the tropics and warmer water in the subtropics." This might be (and probably is) true, however no analysis is presented relating the SST variability and predictability to changes in ocean heat transport (OHT) convergence, and the analysis is only based on the correlation between SST and AMOC. In the model, the importance of OHT to SST variability could easily be quantified (for example by upper-ocean budgets for Box 1,2). This would also quantify the role of surface heat fluxes (as discussed in the manuscript). Similarly, a more quantitative analysis could be performed to firmly establish the link between the proposed mechanism and SST prediction skill (e.g., Yeager et al. 2020, https://doi.org/10.1007/s00382-020-05382-4). In summary, since much of this study repeats the analysis of D16, I think the authors should use the opportunity and tool available to add more to the understanding of the identified SST prediction skill.

We thank the reviewer for the suggestions. The panels added to Fig. 4 illustrate that oceanic heat convergence in longitudinal bands north and south of the AMOC latitude (defined as total heat inflow at the southern boundary minus total heat outflow at the northern boundary) match qualitatively the seesaw mechanism from D16, with increased heat convergence south of 26N after weak AMOC (indicated by a negative correlation to AMOC),

and increased heat convergence north of 26N after strong AMOC (i.e. positive correlation to AMOC). The imperfect correlation values indicate limitations of our analysis, which we now explicitly discuss in several sections of the manuscript (most prominently the discussion section). In essence, this analysis highlights the complexity of factors influencing North Atlantic SST, which we cannot fully capture with the simplified analysis performed here. While this clearly is a drawback of our analysis, the newly added Fig. 9 illustrates that significantly distinct skill estimates are obtained after strong and weak AMOC phases in both boxes during summer, so our mechanism appears to capture the dynamics sufficiently well to yield discernible skill estimates, illustrating its value.

2. Related to the above, I believe the discussion of the new findings could be improved and more balanced. In places, I find the discussion a bit "selective", i.e., focusing on where the results fit with the initial hypothesis (e.g., l.245-246). It would perhaps help if the difference in skill for different regions (Box1,2?) was highlighted in a separate figure. Also, what are the confidence intervals of the correlations in Fig.8; are the difference in skill significant?

In response to this comment, we carefully revised the discussion and made an effort to balance the text. In particular: i) we included remarks on the role of ocean heat transport convergence to the referred regions (Box1 and 2), ii) we included a new figure (Fig. 9) to illustrate the difference in skill (Box 1 and 2). This analysis illustrates that skill differences are significant during certain seasons, which is in line with the D16 mechanism (as discussed in the manuscript).

3. The text is in many places quite hard to follow (see specific comments below), and I think the authors should spend some time/effort in improving the general flow of the text.

We put a major effort in improving the text of the entire manuscript, which will hopefully become clear both in the responses to the reviewer's specific comments and the revised manuscript.

Specific comments

l.3 I don't think it should be necessary for the reader to be familiar with D16 to read the abstract. I think you should rather briefly explain the mechanism.

To make the manuscript self-sustained, we changed the sentence as follows: *'We test the dependence of SST predictive skill in initialised hindcasts on the phase of AMOC at 26°N, invoking a seesaw - like mechanism driven by AMOC fluctuations.'*

l.18 change to "have potential important socio-economic"?

We changed the text to: '*Seasonal SST anomalies (SSTAs) in the tropics have been linked to the intensity and genesis of tropical cyclones and heatwaves (Coumou and Rahmstorf, 2012; Duchez et al., 2016b; Arora and Dash, 2016), and to fluctuations of marine resources (Stock et al., 2015); all of which have potential important socio-economic consequences.'*

l.21-40 I think the authors should revisit the structure of these paragraphs. A suggestion would be to do l.24-33, then 21-23, and then 34-40.

We agree with the referee's suggestion and changed the structure as suggested.

l.21 D16's mechanism -> The mechanism in D16

We agree and we modified these lines as follows: *'Here, we examine the seesaw mechanism proposed by Duchez et al. (2016a) (henceforth D16), which links variations in strength of the Atlantic Meridional Overturning Circulation (AMOC) at 26N and North Atlantic SSTs on monthly time scales.'*

l.21 transition -> variations?

And stated in the previous response this line was modified as follows: *Here, we examine the seesaw mechanism proposed by Duchez et al. (2016a) (henceforth D16), which links variations in strength of the Atlantic Meridional Overturning Circulation (AMOC) at 26N and North Atlantic SSTs on monthly time scales.'*

l.24 "fluctuations in the atmosphere" – could you be more specific? E.g., atmospheric circulation? Also, are you trying to say that ASFs and Ekman-induced heat transport by atmospheric circulation variability are important to SSTs, or that "fluctuations in the atmosphere" are an additional driver?

Thanks for the question. We specifically refer to ASFs and Ekman-induced heat transport in the ocean, and their role in modulating SSTs. Therefore we rewrote the sentence as follows: '*Air-sea heat fluxes (ASFs) and Ekman-induced oceanic heat transport are important drivers of seasonal variability for SSTs (Bjerknes 1964).'*

l.27 "been additionally" -> also

We changed as suggested: *'Part of the North Atlantic seasonal SST variability has also been attributed to the AMOC (e.g. Bryden et al. (2014); Zhang et al. (2019)).'*

l.42 “two dominant mechanisms” – could you be more specific?

This entire paragraph was rewritten to improve clarity. We change the sentences as follows: *‘Recent studies have found improved hindcast skill in the North Atlantic region after considering known physical mechanisms into their seasonal prediction analysis. Mechanisms were invoked in two possible ways: by identifying and explaining times of low and high skill, including precursors of high skill, as so-called* windows of opportunity *(Borchert et al., 2018, Mariotti et al. 2020); or by establishing physical mechanisms in the hindcast ensemble by sub-selecting ensemble members that meet certain physical criteria, thus filtering atmospheric noise in the ensemble (Dobrynin et al., 2018, Neddermann et al., 2018). The present study focuses on oceanic processes that are arguably less noisy than atmospheric dynamics (Gulev et al. 2013).*

l.47-49 It is not clear how this paragraph relates to the previous.

In order to clarify, several modifications were done to the introduction text, with the referred lines now in lines 54-57. We hope that the transitions are now clearer.

l.51 “similar technique as Borchert et al. (2018)” – please elaborate

Adding detail here, we modified the sentences as follows: *‘Analysing an ensemble of yearly initialised hindcasts with MPI-ESM-LR covering 1901-2010, Borchert et al, 2018, Borchert et al, 2019 showed that the AMOC at 50°N influences the SST variability and predictability for several years, with higher skill after years of strong AMOC and vice versa. Borchert et al, 2018 perform a predictive skill analysis of SST conditioned to strong and weak OHT anomalies at 50°N separately, showing a robust influence of the ocean on windows of opportunity for decadal subpolar North Atlantic SST predictions.’*

l.64 “in its mixed resolution” – I am not sure what this means. Also, check sentence.

In MPI-ESM-MR, MR stands for mixed resolution: T63 with 95 levels in the atmosphere and 0.4° horizontal resolution with 40 levels in the ocean. This information is included in the manuscript in lines 74-79.

l.81 Statistical Methods should include a description of the significance test used (bootstrapping).

The manuscript now includes a description of the bootstrapping method in lines 128-130 as follows: *‘We calculate statistical significance using a Monte-Carlo bootstrapping method. The process consists of 1000 bootstraps with replacement on the time-dimension at the 95% confidence level.’*

l.83-85 check sentence

We rewrote the sentence as follows: *'We choose the assimilation experiment over observations because of the short observational record of AMOC from the RAPID/MOCHA array that is available only from April 2004 (Cunningham et al. 2007). Our method therefore allows to constrain the seasonal cycle more robustly.'*

l.90 (and in general) the manuscript contains numerous abbreviations. I think the text would be improved if the use of abbreviations was somewhat limited.

Thanks for the suggestion. We changed the sentence as follows: *'We evaluate the atmospheric contribution to the SST variability using the Ekman transport (EKM) and air-sea heat fluxes.'* We additionally remove from the manuscript the following abbreviation: MDR (hurricane main development region), and made sure to avoid agglomerations of abbreviations in the entire manuscript.

l.93: "fluxes over sea" -> fluxes over the ocean

Changed as requested.

l.99-100 is it necessary to use a 3-month running average when you work with sea sonal means? Also, "high frequency" is a relative term. What is the "high frequency" variability that you want to remove?

Thanks for your comment. We understand that this sentence was misleading, as we meant applying the low-pass filter only for plotting time series (e.g. Fig. 1, 4) , but not for any analysis of seasonal means. We opted for removing this sentence, since a description of the running average used can be seen in the respective figure caption.

l.109 verify -> evaluate

Thanks for your input regarding lines 109 and 110. We decided to delete these lines to improve the reading flow following a suggestion from referee#2.

l.109 change to "against observations... in our analysis"?

As stated in the comment above, we deleted these lines to improve the reading flow following a suggestion from referee#2.

l.110 “Statistics” - could you be more specific?

As stated in the comment above, we deleted these lines to improve the reading flow following a suggestion from referee#2.

l.113 when is the smoothing applied? The gray lines in Fig.1 look unfiltered. And again, why is it necessary to display and evaluate a smoothed seasonal cycle? Minimum and maximum values are also given for individual months.

The smoothing is applied to the grey lines in Fig.1, and not for the mean seasonal cycle (coloured lines). The grey lines in Fig. 1.a-f represent the variability within each analysed year. To make this sentence clearer we rephrased as follows: *‘To show the spread of the annual climatology, grey lines in Fig.1.a,c,e represent anomalies w.r.t. the mean transport of a given year calculated for the full time series (1979-2014), and smoothed with a 3-month running average.’*

l.113 check use of “c.f” throughout the text. I don’t think this is the appropriate use.

We agree and removed the abbreviation in the manuscript where inappropriate.

l.119 remove “lower”

Changed as suggested.

l.122 “and correlate with” -> “with a correlation of”

Changed as suggested.

l.125 check sentence

We rephrased the sentence as follows: *‘Here, we compare the observed AMOC fingerprints discussed in D16 with those present in the assimilation experiment for the period April 2004 to March 2014 (c.f. D16's Fig.3).’*

l.135 any western displacement is not easy to see. Also, to me it looks like the eastern displacement of maximum correlation between lag 4 to 7 stretches northeastward toward the coast of Spain/northern Africa. Is this consistent with advection by the sub-tropical gyre?

We agree that this needed rephrasing to point out more specifically how the northern lobe behaves, and we altered the sentences as follows: *'With increasing time lag (5-7 months specifically), the subtropical lobe of positive correlation shows a displacement towards the east along the approximate circulation path of the northern boundary of the subtropical gyre. This suggests a role for the subtropical gyre in advecting the northern lobe of the seasonal AMOC fingerprint eastward.'*

l.152 you could consider adding a panel to Fig.4 showing this for e.g., Box 1,2.'

Thanks for your suggestion. We included one animation as supplementary information which illustrates the time dependence discussed in this paragraph.

l.155 It is true that Fig.4 shows time series and correlations from box 1,2, but is it correct to say that "main spatial features" are displayed in Fig.4?

We agree that this needed rephrasing and removed the sentence.

l.174 I'm not sure I see why positive correlations over the subpolar region necessarily point to atmospheric forcing (without any additional analysis). Please elaborate.

We agree that this sentence needed rephrasing to bring out our point more clearly. We elaborate on the possibility of atmospheric contribution in section 3.2.4. We changed to: 'In contrast, we find that autumn and winter seasons lack a characteristic dipole pattern (Fig. 5.c,d), showing instead only a narrow region of negative correlations over the subtropics of the order of -0.2 (-0.1) for winter (autumn). The absence of a dipole pattern in autumn and winter may suggest the influence of atmospheric drivers that could potentially supersede the AMOC fingerprints during these seasons.'

l.182 Again, is atmospheric forcing the only other option? What about other oceanic forcing not captured by the AMOC?

As pointed out by the reviewer, it is entirely possible that other oceanic forcings contribute to seasonal SST variability as well, such as zonal or vertical heat advection in the ocean, or heat storage effects. Such contributions are not captured by AMOC variability. Since the aim of this work is to discern the AMOC effect on predictability, and the main counteracting factors come from the atmosphere (other oceanic drivers would likely contribute positively to predictability), we stand by the analysis we did. That being said, we changed the phrasing slightly so as to leave room for the other possible forcings to SST, as pointed out by the reviewer.

l.185-198 The analysis/discussion of ASFs and SST is based entirely on correlations, but nothing is said about the magnitude of the anomalous ASFs, and, hence, how much of the SST variability they are responsible for.

This is true and a valid concern raised by the reviewer. In this work, we consider that ASFs are strong drivers of SST whenever they significantly contribute to SST variability. This is measured by significant correlation between the two quantities. It is not our aim to perform a full heat budget which would require a quantification of the total contribution of ASFs to SST variability, mainly because such budgets are rarely closed in climate models to begin with. To make the aims of our analysis clearer, we rephrased this section and most prominently replaced "strong" with "significant" when writing about the ASF influence on SST variability.

l.196 "(Fig. 6e,f) - strong positive correlations only seen in (f)

We agree with the reviewer and corrected the reference to Fig. 6: *'In contrast, we note strong positive ASF-SST correlations 12 over the eastern part of the subtropical lobe of the AMOC-SST dipole for autumn (Fig.6f).'*

l.196 it is not easy to see from Fig. 6f how much the positive correlations overlap with the subtropical lobe of the AMOC fingerprint. To me it looks like the positive correlations are mainly further east. Please make clearer and/or quantify.

The text now specifies that this finding is mostly limited to the eastern part of the subtropical lobe of the AMOC fingerprint.

l.201 "internal AMOC signal" - please explain

We used the quantity 'AMOC-EKM', i.e. subtracting the Ekman transport from AMOC, to remove the short-term variability attributed to the Ekman component (e.g. Mielke et al. 2013). This leaves the non-Ekman part of the circulation, which we call the 'internal signal' of AMOC. Due to the confusion that this phrasing has caused, we removed it from the manuscript. For clarity, we rephrase this as follows: *'In addition to ASFs, Ekman transport is an important contributor to short-term SST variability (Frankignoul, 1985). EKM is the wind-driven component of the overturning in the ocean, forming the full AMOC signal together with the overturning in the ocean interior, to which usually most of the northward heat transport is attributed (Ferrari and Ferreira, 2011).'*

l.206-207 There are almost no significant correlations in the tropical lobe for AMOC-EKM at 2-4 month lag (Fig.7). So not sure I understand this sentence.

This sentence was indeed misleading, referencing a correlation analysis further out than 4 months that we conducted but do not show. To make the paper more succinct and to the point, we deleted this sentence.

l.204-205 "the AMOC-SST dipole for both autumn and winter" - on l.172 you state that there is no distinct dipole pattern in autumn and winter.

We agree that this needed rephrasing. We modified as follows: *'In the other seasons, SST variability seems to be less less influenced by EKM, as shown by the weak correlation pattern for EKM (Figs.7b,l) as well as the high similarity in the SST patterns for AMOC and AMOC-EKM (Figs.7a, c, j, m).'*

l.232 do your results change if you only consider AMOC stronger/weaker than e.g., 1Std

The essence of our results do not change when using 0.5 std as selection criterion (please see example below, Fig.2), and also remain robust when using 1 std. However, to avoid shortening the time series too much, we choose to consider positive and negative anomalies instead. We now mention this in the manuscript to illustrate the robustness of our findings (beginning of the section "The role of the AMOC").

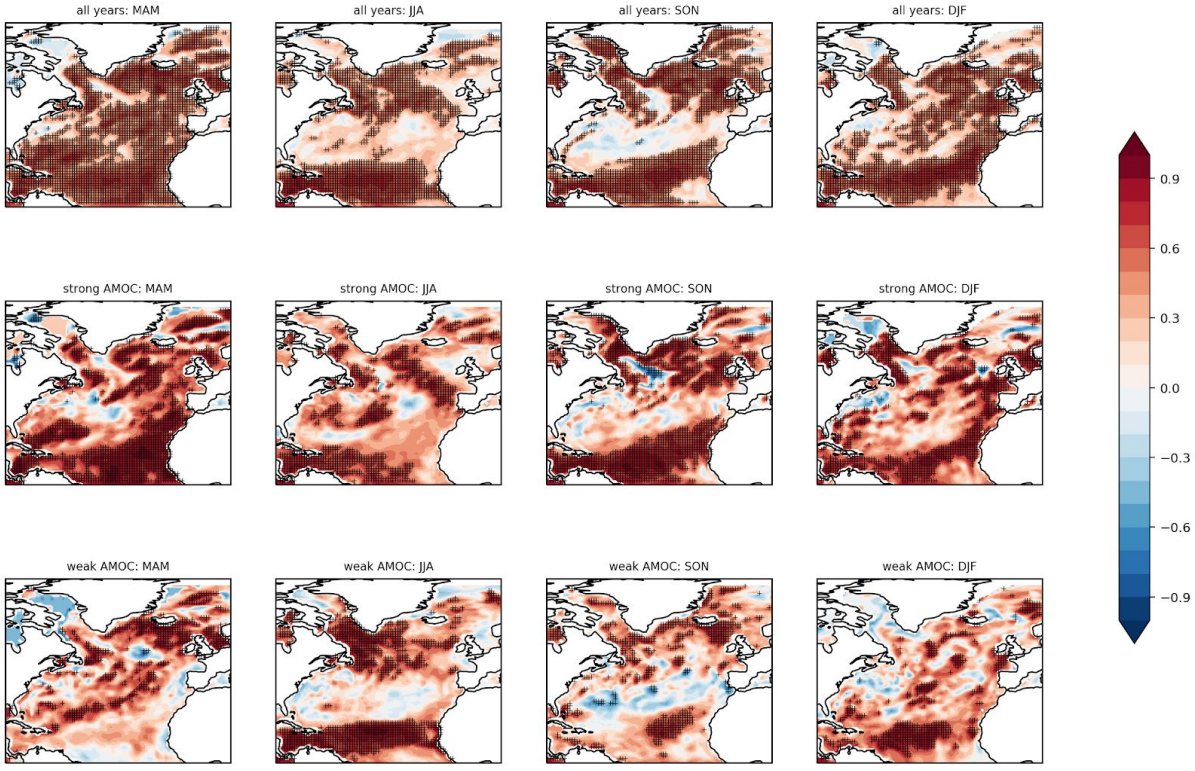


Fig. 2: Similar to Fig. 8 in the manuscript, but considering strong/weak phases of AMOC for a 0.5 std threshold.

l.233 "Atlantic Meridional Variability" - do you mean Atlantic Multidecadal Variability"? In any case, please explain/define AMV.

While rewriting, we decided to remove this sentence from the manuscript to make the discussion more focused on the AMOC strength as the chosen criteria. For the record, this is indeed what we meant.

l.237-241 This is where I think the authors need to strengthen their analysis, to demonstrate that the mechanism outlined here is actually what carries the added prediction Skill.

In rewriting and streamlining this manuscript, we moved this paragraph to the variability section. Please see our response to 'Primary concerns 1.' in the beginning of this document.

l.249 where is the MDR?

The hurricane main development region (MDR) is located in the tropical North Atlantic: 10–20°N, 30–60°W. We added this information and modified the sentence: '*While the mechanism does not solely explain the hindcast skill behaviour in the tropics, we find an improvement over the hurricane main development region, 10–20°N, 30–60°W, (e.g. Hallam et al. 2019) (...).*

l.266 no analysis of heat advection is performed

Please note that we now include an analysis of heat transport convergence associated with the AMOC in section 3.2.2 (Fig. 4.e,f) in lines 196-210.

l.272 Although decadal AMOC variability influences the AMV, I don't think it's correct to say "i.e., AMV".

We agree, thanks for the remark. We rewrote the sentence as follows: 'A possible reason for these differences could be decadal changes in AMOC variability and their imprint on SST (e.g. Ba et al. (2014); Knight et al. (2005)).'

l.273 Related to l.272, is the modeled AMOC also anomalously strong for the RAPID period (corresponding to a positive AMV phase)?

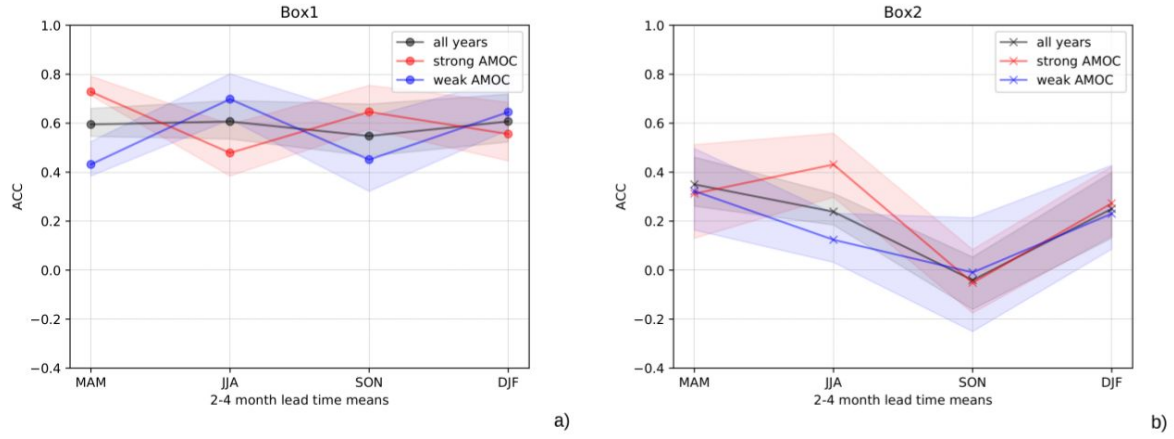
Observed and modelled AMOC are in reasonable agreement during the RAPID period (see fig. 1g), both in terms of variability and mean state. This statement is therefore true for both model and observations, and we added a short comment on that in place.

l.274 "multidecadal" - on l.272 you were talking about "decadal"

The AMOC changes referenced here are closer to a multidecadal mode than a decadal one. We adopted the use of "multidecadal" here and before.

l.297 "only explaining the improvements over the subtropics" - it is not easy to see the skill improvements in MAM, especially for the subtropics. As mentioned above It would perhaps help if the difference in skill for different regions (Box1,2?) was highlighted in a separate figure, including confidence intervals of the correlations.

Thanks for your suggestion. We include a new figure in the manuscript (Fig. 9) to illustrate the difference in skill for Box 1 and 2. This figure alongside new discussion of its contents now illustrates that the main season for skill improvement through the D16 mechanism is summer. This is reflected in the manuscript, also conveying more nuance when discussing the MAM fingerprint.



'Fig. 9. SST ACCs against ERA-Interim at 2-4 months lead time averaged over the regions shown in Fig. 4a. a) Box 1 (10.5◦ - 22.5◦N, 22◦ - 55◦W), and b) Box 2 (28.5◦ - 40.5◦N, 40◦ - 70◦W. Black lines represent the ACCs considering the full time series (1982-2014), red lines for strong, and blue lines for weak AMOC phases. The shaded areas indicate the interquartile ranges.'

l.298 check sentence

We entirely rewrote the paragraph. This led to a dissolution of this sentence into several new sentences, which hopefully clarified this issue.

l.309-311 And what would a model heat budget say?

In order to streamline the paper, we removed the entire paragraph as it did little to discuss the presented findings. As to the reviewer's question, we can only speculate what a careful model-based ocean heat budget would say, but from the heat convergence analysis added

to the paper (Fig. 4) and based on some work done by Kröger et al. (2018) on the subpolar gyre in a similar model setup, we suspect that a model heat budget would agree with Roberts et al. (2017) in that the ocean drives midlatitude heat content changes.

l.311 "active ocean dynamics" - please elaborate (what would inactive dynamics be?). Also does "active ocean dynamics" include Ekman-driven heat transport?

'Active' here refers to the oceanic response in contributing to the variability of temperatures in the surface mixed layer, as opposed to a 'passive' response to local atmospheric forcings. In the context of Roberts et al. (2017), 'active ocean dynamics' includes Ekman-driven heat transport. Please note, however, that when revising the manuscript we came to delete this sentence.

l.352 "following a convergence of OHT" - not shown

In response to the reviewer's comments, we added an analysis of oceanic heat convergence to the manuscript (Fig. 4). This part is therefore now explicitly assessed.

l.355 "large parts of the subtropics" - not sure I agree if this mainly refers to the aforementioned area around 40W.

We deleted "large", as we agree that this was an overstatement of our findings.

Figure 4. Add significance to (b)

We added the significance as suggested.

Figure 6. Figure title says "cumulated". Should be "cumulative"?

Based on published literature, we believe that both forms can be used (e.g. Duchez et al. Fig. 8) and we prefer to keep "cumulated" as it is.

References

Mielke, C., Frajka-Williams, E., & Baehr, J. (2013). Observed and simulated variability of the AMOC at 26 N and 41 N. Geophysical research letters, 40(6), 1159-1164.

Kröger, J., Pohlmann, H., Sienz, F., Marotzke, J., Baehr, J., Köhl, A., ... & Müller, W. A. (2018). Full-field initialized decadal predictions with the MPI earth system model: An initial shock in the North Atlantic. Climate Dynamics, 51(7-8), 2593-2608.