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Interactive comment on "AMOC fingerprints influence seasonal SST predictability in the North Atlantic" *by* Julianna C. Oliveira et al.

Anonymous Referee #1

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Review of manuscript wcd-2020-45 submitted to Weather and Climate Dynamics

AMOC fingerprints influence seasonal SST predictability in the North Atlantic

by Oliveira et al.

This study investigates the impact of the strength of the AMOC on the seasonal prediction skill of North Atlantic SSTs using the MPI-ESM. The authors find that, consistent with previous work (Duchez et al. 2016), the AMOC leads a dipole SST pattern in the tropical and subtropical North Atlantic. The study then assesses whether considering different phases of the AMOC leads to improved seasonal prediction skill of SST.

In general, I find the study interesting, and the results should be of interest to the readers of Weather and Climate Dynamics and especially to those in the emerging

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field of climate prediction. However, I believe this submission requires major revisions before beings suitable for publication, as outlined below.

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.

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?

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.

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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.

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

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.

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

l.21 transition -> variations?

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?

l.27 "been additionally" -> also

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

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

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

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

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

l.83-85 check sentence

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.

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

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

l.109 verify -> evaluate

l.109 change to “against observations... in our analysis”?

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

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.

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

l.119 remove “lower”

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

l.125 check sentence

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 subtropical gyre?

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

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?

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.

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

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.

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

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.

l.201 “internal AMOC signal” - please explain

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.

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.

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

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

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.

l.249 where is the MDR?

l.266 no analysis of heat advection is performed

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

l.273 Related to l.272, is the modeled AMOC also anomalously strong for the RAPID

period (corresponding to a positive AMV phase)?

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

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.

l.298 check sentence

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

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

l.352 “following a convergence of OHT” - not shown

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

Figure 4. Add significance to (b)

Figure 6. Figure title says “cumulated”. Should be “cumulative”?

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