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Ocean Science Discussions

Interactive comment on "Assessment of responses of North Atlantic winter SST to the NAO in 13 CMIP5 models on the interannual scale" *by* Yujie Jing et al.

Anonymous Referee #1

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General comments:

The manuscript addresses the response of winter SST in the North Atlantic to NAO forcing in 13 CMIP5 models. Patterns of observation and models are compared. Because the patterns of NAO-driven SSTs look different between models and observation, the manuscript further investigates the link between the surface heat fluxes and the SST in the North Atlantic. Also, the link between the meridional ocean velocity and the NAO is presented. The authors suggest that it is the overestimated role of the ocean that causes an unrealistic relation between heat fluxes and SST, that finally cause the model to simulate a NAO-driven SST response that differs from the observed

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pattern.

The basic motivation for the authors of this study is that the observed tripolar SST response to the NAO is simulated only by 7 models (as they write). However, I would say that more than only 7 model reproduce a tripolar pattern associated with the NAO on this timescale, even though the centres may be partly displaced, or not providing the correct amplitude (Fig. 4). I agree that there are models that cannot reproduce a realistic pattern, but most of them simulate a tripolar pattern. Especially, the positive centre near the American coast, is reproduced by all models. So, for me the research question would rather be, why are the subpolar (negative) centres displaced.

I would hypothesize, that the displacement of the location could be explained by: the wrong location of the NAO-driven heat flux forcing, a different mean ocean circulation in the models, a different response of the circulation to the NAO, or a combination of these aspects. The authors suggest that the cause for the unrealistic SST response is the incorrect response of SST to heat flux forcing, or as they write in the abstract for the subpolar North Atlantic 'most of the models simulate a positive response of SST to the turbulent heat flux'. And here I see a fundamental problem: When positive flux anomalies (ocean to atmosphere) are correlated with positive SST anomalies, then the SST is the driver for this link, not vice versa. Therefore, the regression that their conclusions are based on (Fig. 8 / 9), do simply not reflect the 'response of SST to the NAO via heat flux forcing'. The regressions seem, instead, to pick up something else, which may or may not be indirectly related to the NAO (for example through an ocean feedback). It could be that for some models / regions on the analysed timescales the dominant link between SST and heat fluxes is not the NAO-heat fluxes forcing the SST. To really extract the response of the SST to NAO-driven heat flux anomalies, I would compute regressions of SST on a pattern of the heat fluxes that has been shown to be is NAO-driven for each model (maybe an index representing the typical structure as seen in Fig. 7).

In summary, I see a fundamental problem with the interpretation of the results and

based on that also not enough evidence for the conclusions presented here.

Another issue is, that the motivation for this study are the differences in the SST-response to the NAO (Fig. 4). But already within the same SST dataset, there are differences depending on how the NAO index was calculated (the first two panels in the first row of Fig. 4). Next, if I understood correctly, the NAO index in the models is calculated with another (third) method. So, it can be assumed that a part of the differences is explained by how the NAO index was calculated.

Furthermore, the entire manuscript would need substantial improvements regarding grammar / language in general. Therefore, I didn't list all the language issues or unclear formulations, because there were just too many.

Based mainly on the concern that I have regarding the approach / interpretation of the results, I cannot recommend this manuscript for publication.

Specific comments:

I suggest to modify the title to not have 'CMIP5 models on the interannual scale' together. So maybe 'Assessment of responses of North Atlantic winter SST to the NAO on the interannual scale in 13 CMIP5 models'.

15: Please clarify on the word 'obvious', in observations or models?

20: For the sub-tropical region an 'incorrect positive response' is mentioned. Why is it 'incorrect' when this subtropical centre of the tripolar pattern should be positive? Further down it is also written 'models can simulate the realistic positive response of SST anomalies to the NAO in the subtropical NA'. which seems to be a contradiction.

Overall, I find the abstract hard to understand.

The timescales are not made clear in the abstract.

59/60: 'In recent years, more and more people have realized that the evaluation of the CMIP5 Earth System Models (CMIP5-ESMs) is the basis for study by these models.' I

am not sure what the authors are trying to say here?

64/65: ‘unreasonable simulation of AMOC’. In which way unreasonable?

Why were these 13 models chosen? I would assume that SST and heat fluxes are widely available across CMIP5 models. Still, it seems that even out of this 13, two do not provide wind speed (Fig. 5 and 6).

What has been done with trends in the data, especially, when computing regressions?

Why do the ‘regression coefficients’ not have units? Are we actually looking at correlation coefficients here? What about the units for the covariances shown in Fig. 8b and 9b?

Is heat flux computed manually or is it a model output? And are the heat flux measures of observations and models derived /computed in a consistent way?

100: Please explained how the ‘site-based’ index is computed. Because it causes different regressions patterns (as seen in Fig. 4).

145: How exactly ‘normalized’?

157: I don’t understand this sentence: ‘Because the locations of the NAO action centers simulated by most of the CMIP5 ESMs in different NAO phases do not show the movements illustrated by the observation (the figure is omitted), the differences between the models are not caused by the NAO period or the phase of the initial sign, but are only related to the structures of models.’ So in observations the NAO pattern is not symmetric? And in the models it is symmetric? Or are the patterns also not symmetric, but differing from the observations? Also, I would argue that it is enough to say that - given the long period of 108 years and the rather short-timescale behaviour of the NAO - there is no reason to think the initial state would matter. But maybe I understood wrong what this sections was supposed to say.

179 / Fig. 3b: I do not see the value of analysing the power spectra of the SST averaged

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over 0-65°N in the North Atlantic in this case. The NAO fingerprint on SST is tripolar, and even if not perfectly tripolar in the models, it is non-uniform. Thus, SST variability that is associated with the NAO when analysing this area-average, is at least partly averaged out. Such an index would rather yield he AMV influence. I suggest to use a different SST index or remove this panel.

185: 'Based on the above analysis, simulated periods of the NAO indexes and area-averaged SST anomalies on the decadal scale are different from the results of observation.' I don't see the data that clearly support this statement. It would be helpful to see the individual power spectra, instead of only the periods of the peaks.

187: 'mainly reflected [. . .] on the interannual scales'. But aren't Eden and Jung 2001 focusing on the inter-decadal scale of the SST response to the NAO and the role of ocean dynamics?

194: Six models are named which a have a positive response in the subpolar region. However, some of these have also an area with a negative response in the subpolar latitudes (besides the positive one). So, if being generous with the exact location of the subpolar centre, nearly all models (maybe except for IPSL-CM5A-MR and MPI-ESM-MR) show some kind of tripolar response to the NAO. Please comment on that.

199: This last sentence in this paragraph should be revised. Both GFDL models have a similarly strong positive centre as in the obs_Gong panel. But again, I really recommend using the same method to compute the NAO index.

215: Is the SHF and LHF computed or is it model output? This is not clear, because in 130 it is only write 'usually calculated' and equations are provided. Based on that, are the observational heat flux data obtained in the same way?

219 / Fig. S2: I cannot agree on the statement that all models overestimate the SHF north of 50°N. First of all, the observations do not cover the area in the Labrador Sea, which seems to be the area of maximum ocean heat loss in the observations, which

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also seems to be the case for some of the models. So, I would say that, for example, both MPI models are doing quite well in reproducing the observed heat flux. Next, it is interesting that specifically the IPSL-CM5B-LR model is the one that is least 'overestimating' – I would rather say 'underestimating' the heat flux. And as mentioned before, I find this model least capable of reproducing the tripolar SST pattern associated with the NAO. In summary, I don't find it convincing that an overestimated heat flux (in the mean state) might be the cause for an unrealistic SST fingerprint in the models.

Another issue is, that I am not sure how robust the 'observations' regarding their heat flux mean states are (Fig. S2). I recommend to test that through showing only the heat fluxes during the last decades when higher quality and quantity of observations were available, and also showing the mean state of a different reanalysis product. It could be that they are indeed robust. It just needs to be shown, because the model performances are evaluated based on these results.

224: For SHF I would even say 30°-65°N. And also in the Gulf of Mexico and in the Caribbean. But again, it should be shown that these results based on the reanalysis product starting around the year 1900 are robust.

Fig. 6: I don't think Fig. 6 is useful. Naturally, increased wind speed tends to increase the heat flux (whether from ocean to atmosphere or atmosphere to ocean). When trying to explain the differences in the response of SST to the NAO, it would be more useful to compare the differences in quantities regressed onto the NAO index (like Figure 7), because as shown before (in Fig. 5), the wind-speed response to the NAO is non-uniform.

238: Please also comment on the comparison of models.

Fig. 8a: In the models with unrealistic positive correlations, is the atmospheric forcing (NAO) maybe too weak compared to other models / observations? It might be like that – when I compare the explained variances from Fig. 1.

310: ‘NAO-driven SHF / LHF anomalies’: Regression between SST and HF without a direct relation to NAO were shown. Therefore, it is not justified to say ‘NAO-driven’. This could only be said if the regressions had been done on an index (e.g., PC-based) that is related to the NAO.

336-339: I cannot agree on this statement, because: Fig. 7 (for heat fluxes) is the analogous version to Fig. 10 (for the meridional ocean velocity). Both figures show that the models reproduce the observations. Based on that only it is not justified to say that the root for an unrealistic SST response to the NAO are the heat fluxes. Indeed, the regressions of heat fluxes and SST in Fig. 8 and 9 show that the heat flux / SST relations are not realistic. But an analogous analysis for the meridional surface ocean velocity / SST is not presented. Even here there might be differences. And then one cannot say that unrealistic aspects in the SST response are caused only by the wrong heat flux response.

348: ‘Because there is a deviation between the simulated and observed periods of the NAO indexes / area averaged SST on the decadal scale , . . .’ What is meant by ‘period’ and ‘deviation’ here exactly? The most dominant timescales of variability from the power spectra?

363: ‘LHF and SST is mainly related.’ What is meant by ‘mainly related’?

364: When the response to LHF is ‘unreasonably positive’, how can contribute to a too weak positive response in the subtropical NA (as mentioned further above in l. 355)?

366-367: ‘have a significant positive response to the NAO’. This only applies to the subtropics, not the subpolar region.

As a supplementary document already exists, I would suggest to also show the individual power spectra from which Figure 3 is derived. For the power spectra please also provide the information about the window that is used to compute them.

Technical corrections:

28: First sentence: A ‘relationship’ is not an ‘event’.

220: By ‘MPI-ESM1’ you probably mean MRI-ESM1?

352: ‘along the meridian’ – which one?

355: ‘weaker positive responses’. I would add ‘than observed’

366: ‘observed meridional velocities’. Please add the information that it is the surface ocean velocity.

‘Constant field value is 10’ is not a good annotation for a panel where model data are not available.

In the figures with subpanels for the different models, sometimes there is one, sometimes there are two observational panels. That moves the position of the model panels and it is hard to compare them across figures. I suggest to have the observations as the last panels, or leave the second panel position free for the case there is no second observation panel.

Please increase the resolution of Figs. 1 and 3.

I think the SCCs in Figure 2 are not a very representative measure, because they hardly vary despite the model differences (as seen on the maps, or in the RMSE).

Figure 3: Please explain the meaning of the horizontal lines / areas.

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