Reply to Reviewer #2

Dear Dr. Leandro Ponsoni,

Thank you very much for reading our manuscript and providing very useful suggestions. We did the revision according to your and the other reviewer's comments and gave explanations in case we did not do it. See our detailed replies below (in blue).

This is my first review of the manuscript "Lasting impact of winds on the Arctic sea ice through the ocean's memory" by Q. Wang and collaborators. In this study, the authors show interesting results based on the fact that the ocean "stores" the impact of different patterns of atmosphere circulation (Arctic Oscillation, Arctic Dipole Anomaly, and Beaufort High modes) which, a few years after, will influence the Arctic sea ice drift, thickness, concentration, and extent. The authors argue that the ocean impact on sea ice occurs mainly through changes in the sea ice dynamics (compared to the thermodynamics). These, in turn, are forced by changes in the sea surface height and geostrophic ocean currents. I think the numerical experiments described in Sec. 2 are well-thought and -designed.

Although, I am not convinced of the meaning of the experiments described in Sec. 3.3 (please see below). Overall, the text is well written and pleasant to read, so that I do not have many "line-by-line" comments.

Since I am late with my review (for which I would like to apologize), it is worthwhile saying that I have seen that the first referee already plotted the comments online. To avoid getting biased by those comments, I haven't looked at them yet. Thus, regarding this aspect, my suggestions are independent.

In short, I think the manuscript is nearly ready. I encourage its publication. However, I do have three major comments that the authors might consider addressing and/or answering in case I have missed the point.

Thank you for your comments. See our replies below.

Major comments:

1. Since the authors propose to look at the ocean's memory, I feel that the study could provide further information on the memory's timescale. For how long the memory imposed by the different atmospheric modes is stored by the ocean? And, for how long it will impact the sea ice? In practical terms, how many years more are required for the lines displayed in Fig. 5a,b,c to converge to zero? I understand that this would require extra modeling effort what is not always straightforward (or even feasible), but I think the scientific community would benefit very much from that information. Interesting conclusions could be achieved. Among others, this effort can lead to findings such as "the negative phase of atmospheric mode X remains longer in the ocean compared with its positive phase (or the other way around)". Or, "the AO forcing has a higher and longer-lasting impact on SSH anomaly compared to the BH forcing". Etc.

As the changes in freshwater content and sea surface height with positive and negative wind perturbations are *roughly* antisymmetric (see Figure R2-1 below), the time scale of the recovery of the ocean state after the wind perturbations are switched off, that is, the length of the ocean memory, is expected to be similar to the duration of the prior wind perturbations. We added this short discussion on time scales to the revised paper (lines 366-368). In terms of the magnitude of the impact on sea ice, AO and BH perturbations are more important because they can induce larger changes in freshwater content and sea surface height, which is clear in the paper.

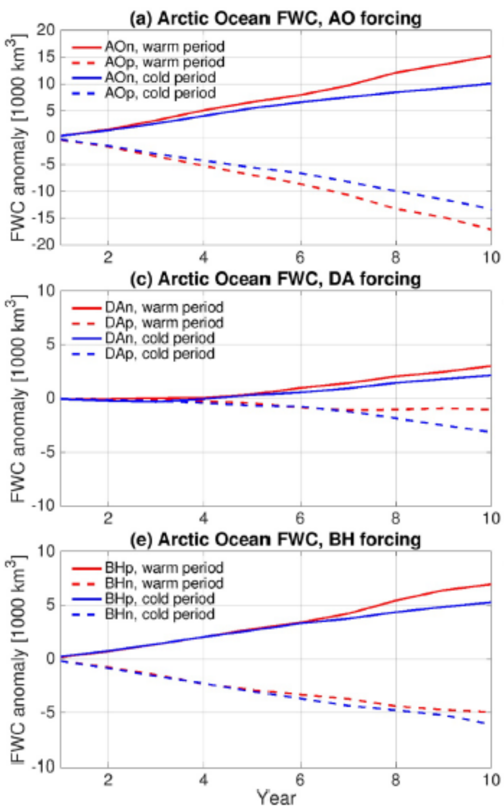

Figure R2-1: Changes in freshwater content in response to different wind perturbations and in different climate scenarios. The implication of these plots is that the time scale of the recovery of the ocean state after the wind perturbations are switched off is close to the duration of the prior wind perturbations. This figure is taken from Wang (2021) cited in the paper.

2. The authors argue that sea ice changes take place through sea ice dynamics. However, I guess they can provide a more comprehensive analysis regarding the thermodynamics aspect. For instance, by looking at other diagnostics such as providing a comparison in terms of ocean heat content between control and sensitivity experiments, and inspecting its relation with the sea ice changes.

Changes in temperature does not tell whether sea ice is indeed impacted by the ocean thermodynamically, while sea ice “thermodynamic growth rate” (the changing tendency of sea ice thickness) shown in the paper explicitly tells the effect of the ocean memory. In particular, the thermodynamic growth rate in Figure 10 shows to be relatively small anyway, so we do not need more attribution.

3. I am not convinced that the experiments described in Sec. 3.3 effectively disentangle the “ice-ocean stress” and “sea surface height gradient force” contributions. Since the geostrophic flow, and therefore the ice-ocean stress, is generated by gradients in the sea surface height, aren’t the second (ice-ocean stress) and third (sea surface height gradient force) terms on the right-hand-side of the Eq. 1 intrinsically related?
In other words, I am wondering whether statements such as in pg. 17, ls. 255–258
“They are the same as the original sensitivity simulations with prior wind perturbations of negative AO forcing, negative DA forcing and positive BH forcing, respectively, but with the sea surface height η in equation (1) replaced with that saved from the control run. In these experiments, the ocean influences sea ice drift only through ice-ocean stress.”
make sense since this change is impacting the geostrophic balance (at least the barotropic component). I mean, the geostrophic currents are a consequence of the horizontal gradients of sea surface height. What is the physical meaning of introducing a geostrophic circulation which is not in balance with the corresponding sea surface height? I have the feeling that the model will quickly adjust the geostrophic currents to the new sea surface height. I might be missing something, but I can’t see the meaning of these experiments. Could the authors say a few words on that?

We only modified the “sea ice” momentum equation in the sea ice model, not in the ocean model. So the geostrophic currents and SSH remained consistent in the ocean model.

These extra experiments are only intended to disentangle the two forcing terms in the sea ice momentum equation. Yes, these two forcing terms co-exist in reality, as we also addressed in the paper text. But we are interested to know whether both forcing terms are important.

Overall comment on the experiments’ description:
I found a bit confusing that the three main sensitivity experiments are described in Sec. 2 (Method and model setups) while the “additional experiments” are described in Sec. 3.3 (Attribution of the impact). While reading the manuscript, I wasn’t expecting new experiments “jumping” into the text when discussing the results. This comment might be biased by my personal taste, but I think that all experiments could be described upfront in Sec. 2. I leave that to the authors.

Now we briefly introduced the extra experiments in the Method section (L102-107), while keeping more details where these experiments are analyzed. In the Method section the motivation to do these experiments is less clear, so we provide more information just before showing their results.

pg. 1, l. 9: “We identified” → it seems that “identified” isn’t the right term here. Maybe “reproduced”?
Changed to “obtained”

pg. 2, l. 22: “pronounced interannual and multiyear variability” → "interannual" and "multiyear" sounds kind of the same.
Changed to “variability on different time scales” (L22)

pg. 4, ls. 72–73: “climatological sea ice derived from a previous simulation.” → a few more details on the “previous simulation” would be welcome.
We added the explanation: “that is, December sea ice averaged over 1970 - 1990 obtained from a simulation with the same model configuration” (L76)

pg. 4, ls. 75–76: “The mean values of Arctic sea ice volume and extent are slightly overestimated” → By curiosity, do the authors expect impact of this overstimation on their results? If so, what impacts?
We added a short comment that model biases could influence the quantitative results. “The regulation of the sea ice state on the impact of the ocean implies that an overestimated sea ice thickness in our model might lead to an underestimation in the induced sea ice changes in the sensitivity experiments. We also note that the idealized wind perturbations we used were intended to allow for easy interpretations of involved dynamical processes. Realistic wind forcing can produce more complicated spatial structures in the response of sea ice.” (L395-398)

pg. 4, l. 86: “deseasonalized monthly mean sea level pressure” → how did the authors deseasonalized the time series? Filtering, subtracting the seasonal and/or monthly means?
The mean seasonal cycle was removed. (L91)

pg. 4, l. 94: “Wind anomalies associated with three idealized SLP anomalies representing these modes were used:” → Also out of curiosity, how the idealized fields were created? I see that the idealized fields (Fig. 2b,e,h) do not perfectly match the modes shown in (Fig 2a,d,g)? For instance, maximum values are not co-collocated in space. Do the authors expect that these differences (even if small) impact the sea ice concentration patterns shown in Fig. 8 and the sea ice extent, for instance?
The idealized winds are intended to simplify the study, providing key information without being bothered by small details. As an example one can find more details about designing idealized wind forcing in Marshall et al. (2017) that is cited in our paper. In the revised paper, we added a short comment together with the possible impact of model biases mentioned in the reply above. (L395-398)

pg. 6, ls. 107–109: “The main dynamical processes changing Arctic freshwater content under the wind perturbations are Ekman transport of freshwater, although induced changes in sea ice thermodynamics also have certain contributions” → Even underneath the sea ice where the wind stress isn’t applied to the sea surface?

Yes, wind influences the sea ice directly, also the ocean through changing ocean-ice stress.

pg. 10, Fig. 6’s caption: Maybe it is worth mentioning that the scaling for blue and red velocity arrows is the same (If that is the case).

In the figure caption we mentioned “in each panel the same scaling is used for sea ice drift and geostrophic current”

Sincerely,
The authors