Reply to Reviewer #1

Dear Dr. Céline Heuzé,

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).

I'll be brief: Interesting paper that would fit very well within this journal, but some crucial methodological points are currently missing.
Major comments, by order of appearance in the text:

1. The temporal resolution of the forcings and of the model itself are never discussed. Monthly and annual results are shown, so I assume "monthly" was the highest frequency analysed. If so, the authors should at least discuss how different the results would be with daily/5-daily output (thinking of sea ice drift in particular).

We added a short discussion on possible impact of the ocean on sea ice on shorter time scales in Section 4.3 (lines 391-394): "We analyzed the dynamic impact of the ocean on sea ice on monthly and longer time scales in this study. As the strength of the impact is very sensitive to the temporal changes in sea ice as revealed by the strong seasonal variability of the impact, regional variability of sea ice internal stress on shorter time scales could then allow for high frequency variability in the impact of the ocean on sea ice drift, thickness and concentration". We also added the information on the temporal resolution of the model and atmosphere forcing in the method section (lines 71, 73-75).

2. Likewise, I am missing a discussion on whether the run lengths are sufficient. Is a 6-year perturbation enough? Is a 4-year period without forcing enough? Or are we, throughout the paper, looking at transient responses?

The Arctic Ocean in reality varies with atmospheric forcing, not being in a steady state. In Discussion Section 4.3, we provided some background on the observed natural variability of Arctic freshwater content (5 - 7 years) and the recent trend in the last two decades. As the period of the variability changes with time in reality, we just choose a reasonable perturbation length for dynamics understanding, the main purpose of this study.

3. [my biggest issue] The relevance of liquid freshwater, instead of e.g. integrated salt content, is more and more debated within the physical oceanography community. This is particularly problematic for a Pan-Arctic study, to investigate a change that will impact the salinity, using a model. So first, please quantify your model's (potential) biases in upper-ocean salinity. Furthermore, you're not integrating over a fixed depth but only until the depth of the reference salinity. Again, show how well that depth is represented in your model AND how the depth changes throughout the basin and throughout your runs. Now, taking both comments into account, I'd also like to see an evaluation of the robustness of your results by comparing isohaline

vs fixed depth, and reference salinity vs integrated salt content. It can be as simple as showing maps of the mean FWC in the control run for all four options.

To avoid concern, we replaced the freshwater content with halosteric height in the figures (Figure 3 and Figure S3).

The changes in sea surface height can be explained by the changes in halosteric height, as shown by the comparison between figure 4 and figure 3. The latter (changes in halosteric height) are associated with the changes in freshwater content. The very good spatial correlation of freshwater content (was shown in the old paper version) and sea surface height is a direct illustration. As showing halosteric height already serves the purpose, we do not show freshwater content in figures in the revised version.

4. You never explain how sea surface height is computed. In particular, is it produced by the model, or did you have to compute it afterwards e.g. from the temperature and salinity fields?

The sea surface height is taken directly from the simulations. We added in line 111 "(the dynamical sea level simulated by the model)". It is different from the term "steric height".

5. [my second biggest issue] As per point 3, I would like to see an assessment of the model's sea ice concentration, thickness, drift speed and seasonal cycle in the control run before you move to investigating the potential differences coming from the different forcings.

We notice that your concern is due to missing seasonality in the "SSH anomaly" from your specific comments below. As we replied below, the SSH does have strong seasonality in each simulation, although the SSH difference between the sensitivity runs and the control run does not have significant seasonality. Therefore, there is no issue with the simulated seasonality of related fields in the model.

Anyway, we added a few sea ice assessments below (Figures R1-1, R1-2, R1-3). Sea ice concentration, thickness and drift were reasonably reproduced. Sea ice thickness is overestimated north of Greenland, but the model results are within the observation uncertainty range in most of the areas. We added Figure R1-3 to the online supporting information (SI, Figure S1). The original Figure S2 already shows integral assessment of sea ice (as extent and volume), so we did not further add Figures R1-1 and R1-2 to the paper.

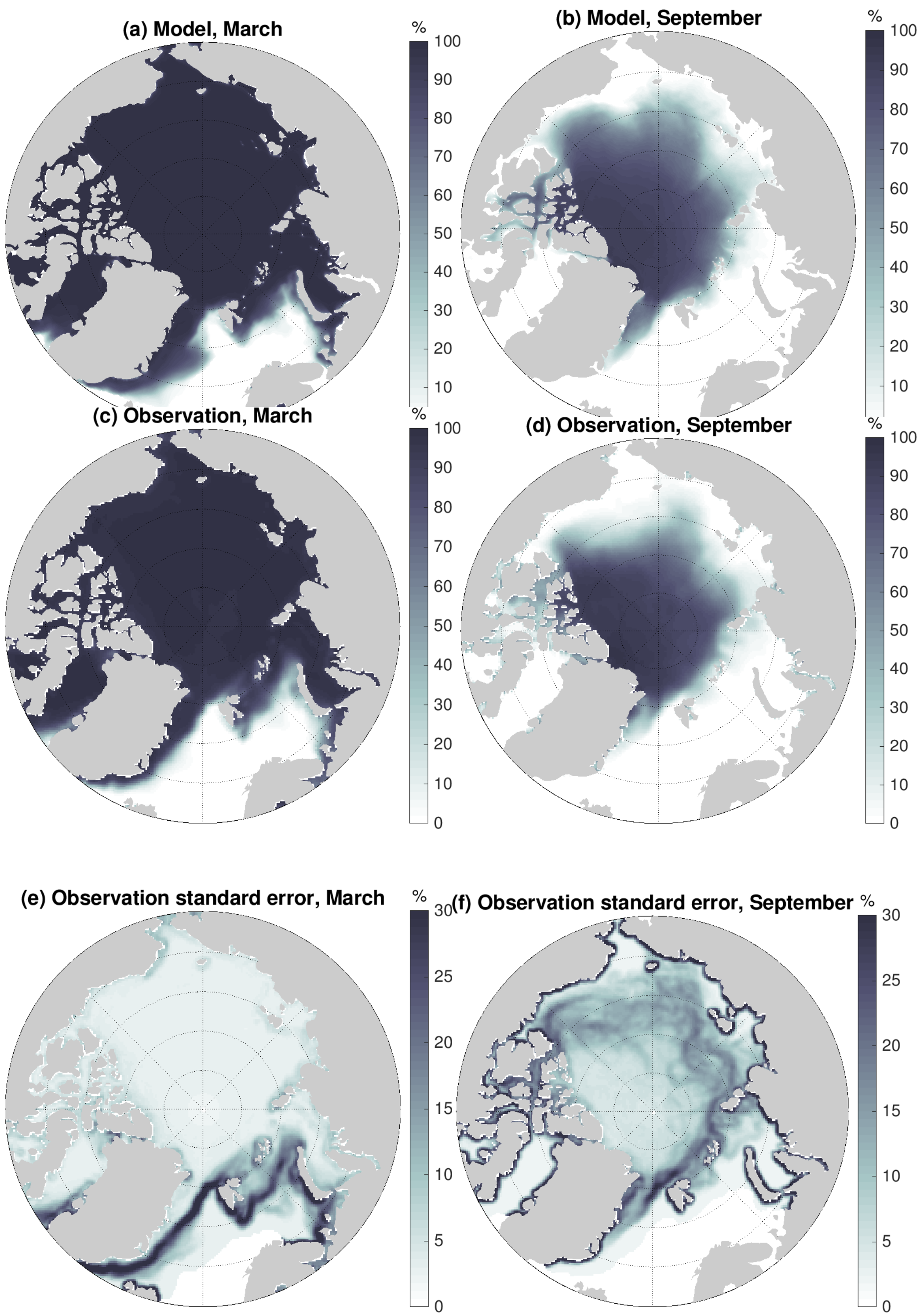

Figure R1-1. (upper) Simulated sea ice concentration in (a) March and (b) September. (middle) OSI-SAF observed sea ice concentration in (c) March and (d) September. The average over 2000-2019 is shown. The total observational uncertainty is shown in (e) for March and (f) for September. Observation reference: Lavergne, T., Sørensen, A. M., Kern, S., Tonboe, R., Notz, D., Aaboe, S., Bell, L., Dybkjær, G., Eastwood, S., Gabarro, C., Heygster, G., Killie, M. A., Brandt

Kreiner, M., Lavelle, J., Saldo, R., Sandven, S., and Pedersen, L. T.: *Version 2 of the EUMETSAT OSI SAF and ESA CCI sea-ice concentration climate data records*, The Cryosphere, 13, 49-78, doi:10.5194/tc-13-49-2019, 2019.

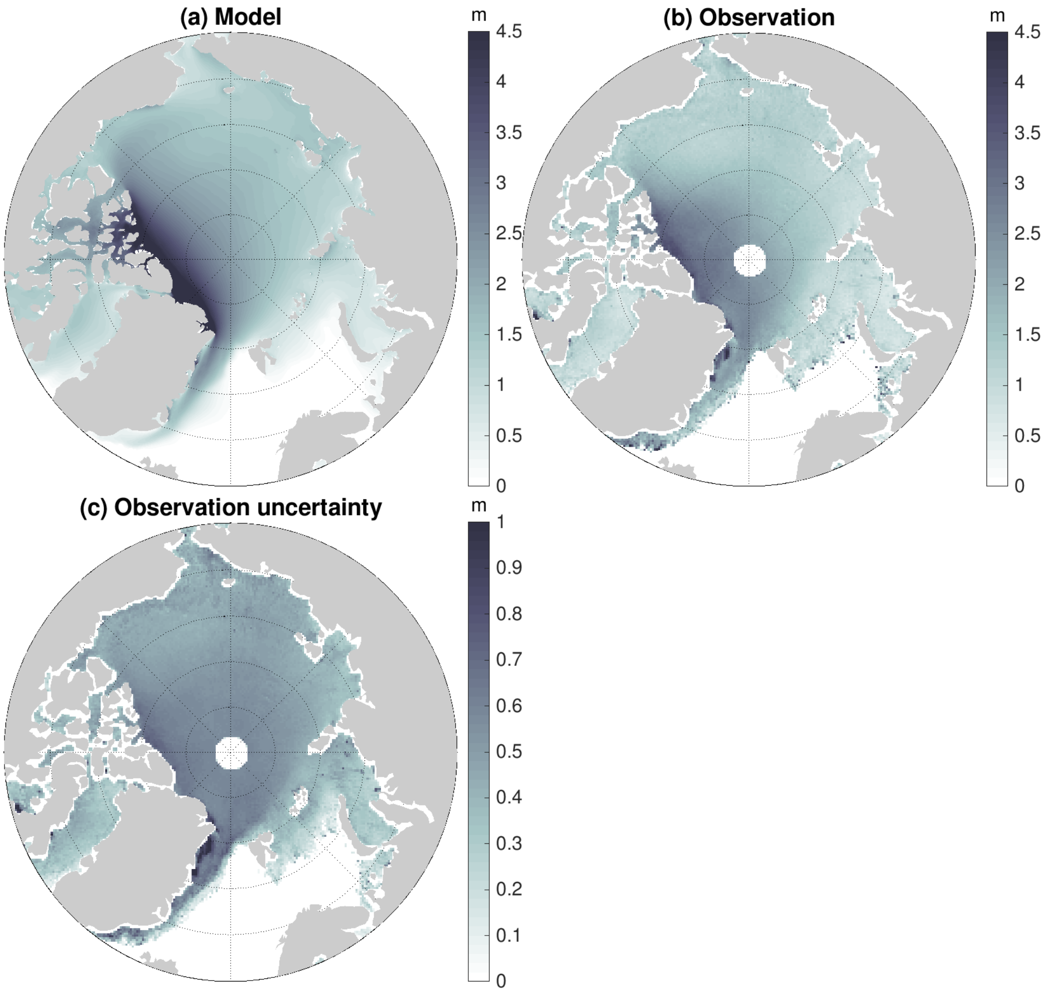

Figure R1-2. (a) Simulated and (b) CryoSat-2 observed sea ice thickness averaged over months with observations available (October to April) from 2011 to 2019. (c) The CryoSat-2 observation uncertainty. Observation reference: Hendricks, S. and Ricker, R. (2019): Product User Guide & Algorithm Specification: AWI CryoSat-2 Sea Ice Thickness (version 2.2), hdl:10013/epic.8eb07093-4042-40ab-bfb8-e0c72c1567de

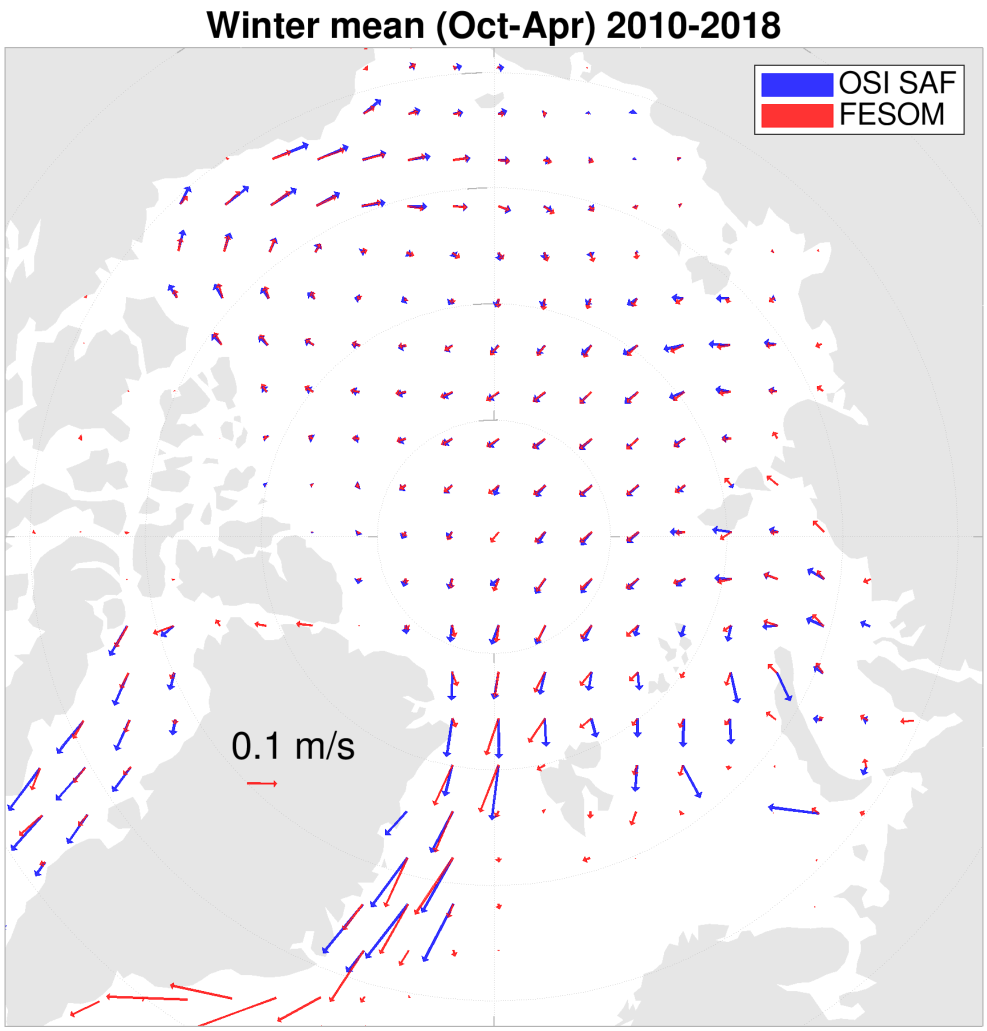

Figure R1-3. Sea ice drift in OSI-SAF observation and model simulation. The average is taken over cold seasons (October to April) in the period of 2010-2018. Observation is more accurate in the cold season (shown here) than in summer time (Lavergne et al., 2010). Reference: Lavergne, T., Eastwood, S., Teffah, Z., Schyberg, H. and L.-A. Breivik (2010), Sea ice motion from low resolution satellite sensors: an alternative method and its validation in the Arctic. *J. Geophys. Res.*, 115, C10032, doi:10.1029/2009JC005958.

6. Overall, there are many instances of inconsistencies or lack of precision that force the reader to guess what you really are showing. I am giving examples in the next part of this review, but throughout the manuscript verify that your text, captions and colorbars are not showing different things (velocities vs direction, anomalies vs actual values).

We did the proofreading and addressed your specific comments as replied below.

Specific comments:

- line 74: liquid freshwater content is only defined line 98. At least say here that the definition comes below.

As replied above, we do not show freshwater content in figures any more and the definition is thus removed. Only in the discussion section where we discuss recent changes in freshwater content, we mentioned the reference salinity in calculating freshwater content from the model results.

- line 86 – 97: this paragraph falls out of nowhere. Start line 86 with e.g. "to design the perturbations, we look at..."

We added "The wind perturbations were designed as described below." (L90)

- Figs 3 and 4: show the control as well – as you did for Fig 6.

As replied above, we replaced freshwater content with halosteric height in figure 3 now, which has no sense with showing its mean value. Furthermore, the standard deviation on interannual and 5-yr time scales in Figure 6 is just provided to show how significant the induced sea ice changes are compared with the strength of the variability on these time scales, while there is no such motivation in figure 3 and 4. The variability of sea surface height was found to be informative only after decomposition using for example EOF analysis (Koldunov et al., 2015; Xiao et al., 2020), which is not the focus in the current paper.

Ref:

Koldunov, N. V., Serra, N., Köhl, A., Stammer, D., Henry, O., Cazenave, A., Prandi, P., Knudsen, P., Andersen, O. B., Gao, Y., and Johannessen, J.: Multimodel simulations of Arctic Ocean sea surface height variability in the period 1970–2009, Journal of Geophysical Research: Oceans, 119, 8936–8954, 2014.

Xiao, K., Chen, M., Wang, Q., Wang, X., and Zhang, W.: Low-frequency sea level variability and impact of recent sea ice decline on the sea level trend in the Arctic Ocean from a high-resolution simulation, Ocean Dynamics, 70, 787–802, 2020.

- Fig 5: I suspect that the lack of seasonality of SSH is either the result of inadapted axis limits (giving the values in the text would help) or potentially too high a sea ice cover year-round (see major point 5).

Fig 5 shows the SSH difference between the perturbed runs and the control run, not SSH in an individual run. SSH in each individual run does have clear seasonality (see Figure R1-4 below). The SSH difference between the runs reflects the lasting ocean memory of prior wind perturbations. We added this information in the figure caption of Fig. 5 in the revised version to avoid confusion.

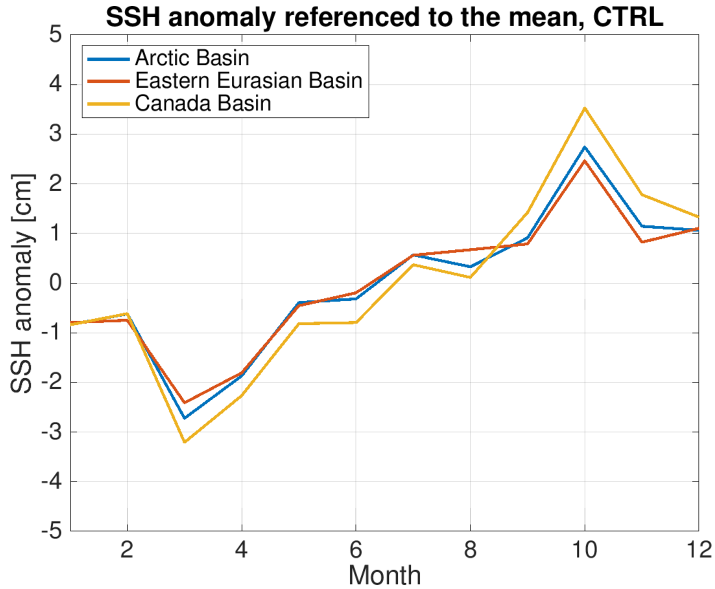

Figure R1-4: Monthly mean SSH in the control run averaged in different regions. The anomaly referenced to the respective mean value is shown.

- Fig 6: what is meant by "anomaly in drift or current", as in, what is subtracted from what? It would be clearer to show the arrows of each experiment, and let the reader compare to the control panel. And shading that has only positive values suggests that all experiments have a faster drift than control (shading described as an anomaly, see also major point 6)

Arrows show the anomalies of vectors (ice drift and ocean current), referenced to the control run as mentioned in the figure caption. The anomalies of ice drift and ocean current are shown together to illustrate the impact of the ocean current anomaly on sea ice drift anomaly. This information would be less obvious if we show the vectors of each simulation, and the plots would also become too busy with four arrows put together.

- It is confusing that Fig 7 shows thickness but Fig 8 shows concentration, when Fig 8 is presented as the seasonal version of the Fig 7 discussion. Potentially show thickness only, or combine both diagnostics into sea ice volume.

Both the impacts on sea ice thickness and concentration are interesting, so we would like to show them separately. However, the impact on sea ice concentration is mainly along the ice edge and in summer. If we show the temporal mean in Figure 8, this information will be masked by small impacts in other seasons and by (seasonal and interannual) changes in sea ice edge locations. We added in the revised paper: "Because the impact on sea ice concentration is only significant in summer and close to sea ice edges, whose locations vary strongly in time, averaging the sea ice concentration anomalies over the four model years would mask the ocean impact. Therefore, in Figure 8 we showed the September sea ice concentration anomaly in one particular year." (lines 195-198)

- Fig 9: RMS of drift = specify that it is of the drift speed

Added in the figure caption.

- line 257: show this SSH saved from the control run, including its seasonal cycle.

As shown in Figure R1-4 above, there is clear seasonality in the SSH in an individual run.

- line 295: show this result (that the sea ice volume export through Fram Strait is not impacted by sensitivity experiments), that's an important one.

In Discussion Section 4.4 we mentioned the results in more detail and the figure is actually shown as an SI figure (Figure S12). At the place of the original line295 (in line 305 in the revised version), we add “(see further discussion below in Section 4.4)”.

Sincerely,
The authors