We thank the reviewer for their diligent review and insightful feedback on our revised manuscript, which have significantly enhanced the quality of our manuscript. We have addressed all the recommendations, see details below (reviewer's comments in black, our replies in blue).

This manuscript evaluates the usage of the CICE sea ice model for seasonal forecasting in NOAA's Unified Forecast System by examining 12 month simulations of CICE driven by reanalysis atmospheric forcing and an ocean 1-D mixed layer model. Using root mean square errors in integrated quantities, the authors evaluate hemispheric and regional errors in sea ice extent and volume. My evaluation of this manuscript is to accept with minor technical revisions. The authors have commented on and taken into account in the text of the manuscript the major issues I had with regards to an earlier version of the manuscript. I now find the manuscript suitable for publication.

My recommendation is Accept with minor technical revisions.

Major Comments

1. My previous major comments have been addressed, and I thank the authors for taking them into account.

Thank you.

2. I do have some hesitation still with regards to the missing ocean dynamics with the use of an ocean mixed layer. In particular, the authors seem to acknowledge the only missing component of the ocean dynamics is missing heat transport by the ocean. While this is certainly important for the Barents and Kara Seas, an equally missing component is sea ice transport via ocean currents – as opposed to wind forcing, which is present in their simulations. Oceanic advection undoubtedly plays a role in their forecasting results in the Labrador Sea and East Greenland regions, but also likely has a large influence in the Beaufort and Chukchi seas – although here, the elimination of the sea ice thickness bias from CRSR initialization is by and large the most important driver of increased skill.

Thanks for pointing this out. We have added this to the summary:

*This positive bias in SIT is likely due to the absence of oceanic currents in the column mixed layer ocean model,which, although having a lesser impact compared to wind forcing, still plays a role in sea ice export.*
*Additionally, the cold bias in the modeled SST near the marginal ice zone may contribute to the positive bias in SIT. The cold bias is likely a consequence of neglecting poleward oceanic heat transport within the simplified ocean model used in the experiments.*

3. Instead of listing further major comments, I have added a (non-compulsary) list of additional comments that I and other readers might have considering the manuscript. I am not suggesting the authors need address them, but rather they take note of them for future reference.

We appreciate this list. We addressed them as much as we can here and will use it for future reference.

Minor Technical Comments
1. Abstract. In the abstract, can you be more specific then "the model's Arctic sea ice thickness has a positive bias"? Perhaps the model's Arctic sea ice initial conditions have too thick ice in the Beaufort Sea, leading to too large ice extents in the Arctic at 6 month lead forecasts and when observable, errors in all leadtime forecasts of ice volume.

I realize the situation with regards to forecasts of ice volume are perhaps more convoluted, but I was limiting my suggestion to a single sentence.

Thank you. We have revise this in the abstract:

*The model's initial conditions have too thick ice in the Beaufort Sea, resulting in excessive ice extent in the Arctic at 6-month lead forecasts and errors in ice volume at all lead times when compared to available observations.*

2. ll 40-41. The inserted (thank you) citations for the existing seasonal forecasts that use CICE are slightly confusing. Could you please separately cite the MetOffice GloSea system (specifically say the GloSea system) and the Canadian CanSIPS systems? It is also in this way perhaps fairer to the MetOffice system which pre-dates CanSIPS use of CICE by almost a decade. E.G. · · · for example, in the UK Met Office Global Seasonal (GloSea) forecast system (Arribas et al., 2011; MacLachlan et al., 2015) and in the Canadian Seasonal to Interannual Prediction System (CanSIPSv2) (Lin et al., 2020).

Thank you for your suggestion; we've incorporated it into the revision. During our work with the CICE5 source code, we noticed the valuable contributions made by the Met Office.

3. ll. 119-120. The statement "Lead times not ending in .5 are rounded up to the nearest integer month for simplicity (i.e., 11.5-month lead time is rounded up to 12-month lead time)" seems contradictory to me (i.e. by your first statement lead times ending in 0.5 should should not be rounded at all). I'm not sure the whole statement is necessary, and could simply be removed.

Thank you. We have removed this. We added a footnote of "*Rounded up to the nearest integer month for simplicity*".

4. Figure 7. The dotted (CS2 IC) lines seem like dashed lines to me?

Thank you: they are indeed dashed lines. We changed "dotted" to "dashed" in the manuscript.

Additional Comments to Consider

I am not asking for any action on any of these listed items. However, you may wish to consider how these are presented in the future.

1. ll. 53-59. While a complete set of CICE namelist options/values would be outside the scope of a Model Setup section (but might be part of open data supplementary material), one common aspect of CICE model setup beyond the EVP, ice thickness categories, and ice/snow thermodynamic layers is whether or not a meltpond parameterization scheme is used.

Thank you. We have added in the manuscript that no melt pond parameterization scheme is used in this study.

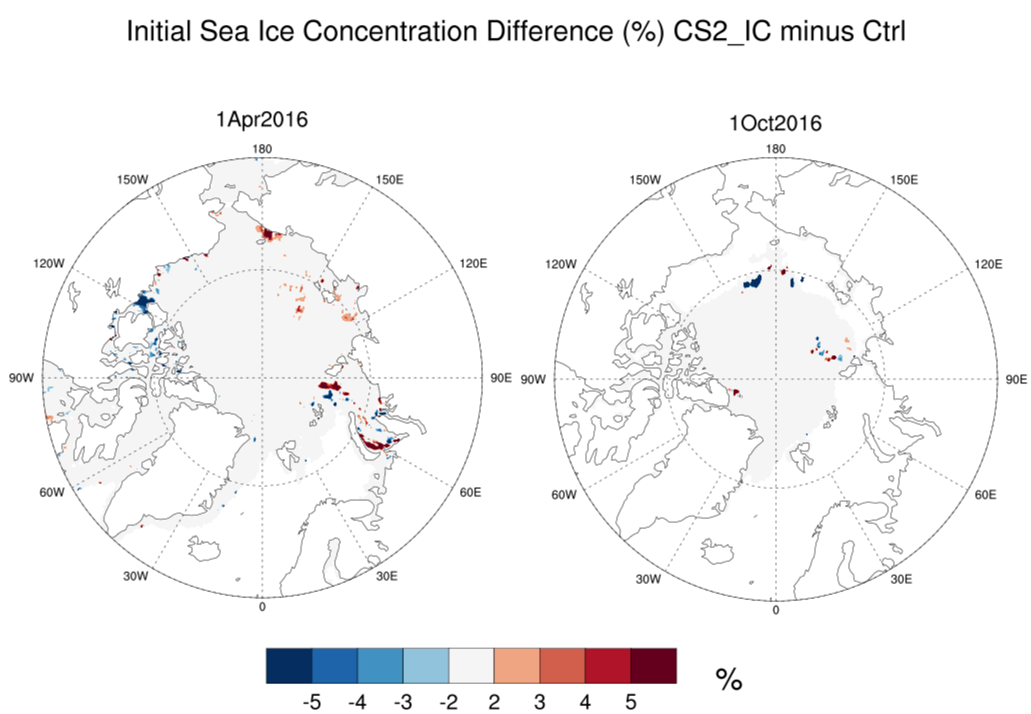
2. l. 86. The mixed layer in the Arctic varies considerably with season (Uotila et al. An assessment of ten ocean reanalyses in the polar regions. Clim Dyn 52, 1613?1650 (2019). https://doi.org/10.1007/s00382-018-4242-z; and observational citations within). I have no knowledge in terms of the capabilities of the mixed layer ocean scheme in CICE, but is a constant 20m choice for mixed layer depth a reasonable and realisitic choice. This might have 2nd order implications for the biases you observe in SST (Figure 9) – although the 1st order biases are obviously oceanic heat transport.

Thank you for the reference. We have added in the manuscript:

*While this choice is generally a reasonable approximation, it may not adequately represent the actual conditions in certain regions. Specifically, this fixed depth may be too shallow in the Amundsen and Makarov Basins during winter, as well as within the Antarctic Circumpolar Current, when compared to observations as shown in Uotila et al. (2019).*

3. ll 99-100. Assigning zero values to areas where CryoSat-2 observes no sea ice thickness – which might be anywhere with sea ice thickness below 1m – would seemingly reduce your initial condition sea ice concentrations and sea ice extent (although Figure 4 top row shows no evidence for this). Might this be worth expanding on? Are there areas in CS2 IC where sea ice concentration was removed relative to CNTR IC?

Thank you. Ricker et al. (2017) pointed out the uncertainty in the CryoSat-2 dataset regarding thin ice. However, this uncertainty in the thin ice region does not invariably result in a zero ice thickness. In the CS2_IC case, ice coverage is only reduced to zero for points with precisely zero ice thickness. Consequently, only a few points fall into this category, as shown in the sea ice concentration difference plot below.




4. ll 185-189. I find the oceanic heat transport explanation for positive bias in sea ice concentration in the Labrador Sea (and elsewhere) unsatisfying. An alternative, or likely easier explanation might be found in ocean current transport of ice not incorporated in the mixed layer ocean model. The Labrador Sea and along the east coast of Greenland are regions with a southward ocean current that moves and disperses sea ice southward (where it melts). A lack of ice transport could very easily explain the positive bias of sea ice concentration here – and perhaps in the southern ocean as well – although ice transport through atmospheric forcing plays a large role there. Overall, while the lack of ocean heat transport is obviously important – especially in the Barents and Kara Sea – but other than there, a lack of sea ice transport by ocean currents is likely to be just as big a missing factor imposed by the mixed layer representation of the ocean. And then there would also be the role played by the imposed depth of the mixed layer itself.

Thank you. We have added in the manuscript:

*Here, a one-dimensional column mixed layer ocean model is employed, without accounting for oceanic advection. Notably, in regions such as the Labrador Sea and along the east coast of Greenland,*
*the prevailing southward ocean currents play a significant role in transporting and dispersing sea ice towards the south. Consequently, neglecting sea ice export associated with ocean currents is likely to result in a positive bias in SIE, particularly over extended lead times.*

5. Figure 4 and spatial biases discussed in (previous) sub-section 3.1: Although I understand the plotting of ice concentration fields for October 6.5 month forecasts: It matches your ability to show similar plots with observation for ice thickness in Figure 5. However it is the April initialized forecast lead with the smallest bias (Figure 1a; Figure 11; are AMSR2 observations

really only available Oct-Apr?), which hides significant system biases, particularly in the Barents and Kara Seas not mentioned explicitly until sub-section 3.4. Some better co-referencing of spatial biases with hemispheric biases and a better coherence with regards to which start dates to concentration on could have been achieved in the text [and I should have taken more careful note in the first revision of this paper].

Thank you. This is a very good point. We added October initializations in Fig. 11 to show the results are similar to the April initializations. AMSR2 observations are available all year along, but not available before 2013 at the time of this study.

We added a discussion on Barents and Kara Seas in Sec.3.2:

*In both the control and CS2_IC experiments, a positive bias in SIT at 6-month lead time is seen in the Barents and Kara Seas, as shown in Fig. 5. This bias is likely attributed to the absence of southward ocean currents in the mixed layer ocean model and the subsequent southward drift of ice, as previously mentioned.*

6. Figure 9. Due to some of the lack of connections made in my previous point the plot of SST biases (Figure 9) lacks cohesion with the rest of the manuscript. The influence (or lack there-of) of oceanic heat transport is obvious from the figure, but then it is hard to then compare this with the sea ice results already shown– or to possibly link the contribution of other system biases (CFSR forcing / depth of mixed layer) to both SST biases and sea ice biases.

Thank you. We agree and have decided to remove the SST bias plot (Fig. 9), but we will retain the discussion of SST.