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Comment on wcd-2022-5

Anonymous Referee #1

Referee comment on "Benefits and challenges of dynamic sea ice for weather forecasts" by Jonathan J. Day et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2022-5-RC1, 2022

Review of "Benefits and challenges of dynamic sea-ice for weather forecasts" by Day et al.

Weather and Climate Dynamics

The ECMWF have recently introduced a dynamic sea-ice model (as part of their coupled atmosphere-ocean dynamical models) into their operational forecasting suites for their medium-range forecasts. This study evaluates one northern hemisphere winter of 10-day forecasts using three experimental configurations: (i) An atmospheric model forecast using persistent SST and sea ice surface condition; (ii) An atmospheric model forecast using 'observed' (i.e., daily updated) SST and sea ice surface condition; (iii) A coupled forecast. (i) is essentially the previous operational configuration, (ii) provides a sort of upper limit on forecast quality (assuming perfect observations) and (iii) is the new operational configuration so including the dynamic sea ice. The results are largely positive, but there are some limitations in the findings and in the ability to evaluate the results that are also discussed.

I thought this was a really nice study, one of the first to evaluate the benefits of dynamic sea-ice on weather forecasts for an extended period. The paper is concise, well written, generally placed into context well and illustrated with high quality and appropriate figures. I have a few specific concerns, primarily around caveating the largely positive benefits of this step forward in forecasting, and some minor comments.

General Comments

(1) The overall benefits of a dynamic sea ice are clearly evident and are nicely illustrated in Figures 1, 2 (which is a striking illustration) and 4. However, these general (and seasonally averaged) plots do show some caveats. Fig 4 illustrates that over the first day, the persistent forecast has lower IIEE than the coupled forecast for the northern hemisphere, and that this is always true in the Labrador Sea region, while in the Sea of Okhotsk it is true until around day 6. I suspect the reason persistence is better for these seas is that they are relatively small and enclosed regions, with sea-ice that advances/retreats when the winds are along the sea, thus pick out the advancing/retreating problems discussed later. These findings are noted briefly in section 3.1 (e.g., L90-95), but I think further discussion is really needed in section 3.1. This is explored a bit in section 3.2, where I think Fig 6 is used to explain that IIEE and changes in ice concentration are related (especially so for the more enclosed seas), but this is not very well linked back to the key figures of 2 and 4. I suggest the authors work on improving the links between Figs 2-4 and Fig 6-7 and explaining the different qualitative results of Fig 4.

(2) An interesting fact is noted with regard to Fig 4, that the 'initialisation error (IIEE)' is approximately half of the final IIEE error at day 10. This is rightly mentioned (L100) but this striking fact is not discussed further in Section 4 or the abstract. The authors note this is related to initialisation challenges and the use of only weakly coupled data assimilation. I know this is also a problem at other centres and is likely to be an issue for a number of years for coupled forecasts. I wonder if this finding should receive more prominence in the paper.

(3) The other related issue, which is briefly mentioned, is the veracity of the sea-ice analysis. The authors point out there are uncertainties in the sea-ice analyses and this will affect initialisation and the size of the errors (P10, L325) and that "guidance … from the remote sensing community" is needed. I agree here and I would perhaps suggest this limitation is added to the abstract. At present the last two lines of the abstract are a bit vague. It might be worth expanding these to state explicitly that the quality of satellite

sea-ice products on daily to weekly timescales and on meso-scales (<500 km say) are not well characterised and this is a limitation for NWP.

Specific Comments

L42 – there is another recent idealised modelling study on the atmospheric response to sea-ice geometry and concentration that should be cited here:

Spensberger, C., & Spengler, T. (2021). Sensitivity of air-sea heat exchange in

cold-air outbreaks to model resolution and sea-ice distribution. Journal of

Geophysical Research: Atmospheres, 126, e2020JD033610. https://doi.

org/10.1029/2020JD033610

L49 – I am not an expert on the timelines here, but are you sure that ECMWF developed the first coupled global … system? Maybe for an ensemble? Not sure about NWP more generally. The Canadian ECCC have had a coupled forecast model for some time and this may pre-date the ECMWF development. You cite one paper for the Canadian system (Smith et al. 2018), but you should probably also cite earlier pioneering work that demonstrated the potential for improvement in atmospheric forecasts from such a coupled system withi NWP.

Pellerin P, Ritchie H, Saucier SJ, Roy F, Desjardins S, Valin M, Lee V. 2004.Impact of a two-way coupling between an atmospheric and an ocean – icemodel over the Gulf of St. Lawrence.Mon. Weather Rev.132: 1379 – 1398

Smith GC, Roy F, Brasnett B. 2013. Evaluation of an operational ice-ocean analysis and forecasting

system for the Gulf of St Lawrence. Q. J. R. Meteorol. Soc. 139: 419–433. DOI:10.1002/qj.1982

Smith, G.C., Roy, F., Reszka, M., Surcel Colan, D., He, Z., Deacu, D., Belanger, J.M., Skachko, S., Liu, Y., Dupont, F. and Lemieux, J.F., 2016. Sea ice forecast verification in the Canadian global ice ocean prediction system. *Quarterly Journal of the Royal Meteorological Society*, *142*(695), pp.659-671.

L79-80 – I was slightly confused on reading the explanation for the persisted surface conditions for the first time, because 'an anomaly is added each day'. On second reading I think this anomaly is only for the SST (not the sea ice)? Perhaps check for clarity here.

L128 – I think the Hersbach ERA5 reference is missing.

L142 – I'd replace "Atlantic coast' with Labrador Sea coast, as it isn't the main Atlantic basin.

L231 – ‘that region’ – it is unclear from this paragraph which region you are talking about. Maybe these lines should be merged into the previous paragraph?

L250-265 – this paragraph on internal boundary layer development at the ice edge is unreferenced – you could cite the Spensberger and Spengler 2021 paper here or the idealised 2D model of this internal BL development which also uses observations in

Renfrew, I.A. and King, J.C., 2000. A simple model of the convective internal boundary layer and its application to surface heat flux estimates within polynyas. *Boundary-layer meteorology*, *94*(3), pp.335-356.

L285 – It was useful context to point out the differences in specific humidity (in g/kg and that this was 10% of the total value). You could also have expressed this as % of the standard deviation of this variable or something? And done similar for the difference in temperature. I think it useful to have an idea of the magnitude of these forecast differences in the context of day to day variability. If you can easily do such a metric? This is just a suggestion, not necessary.

L307 – The final section is more of a “Conclusions and Discussion” section.

L355 “weakly” not weekly.

Figures

Figure 1 – I would recommend changing the colour scale to one with white in the middle. At present the whole North Atlantic (which has no sea ice) is pink. It looks odd!

Fig 2 – the font size of the labels and legend is too small to read. Nice figure though!

Fig 3 & 2 – would it make sense to try and have the same colour for Ocean5 in these figures – this is red in 2 and 4 but green in fig 3.

Fig 5 – these figures illustrate the large variability between forecasts. Fig 5 is only very briefly mentioned in section 3.2 – I wonder if you should add a sentence or two emphasising the large variability.