The Cryosphere

Discussions

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Interactive comment on "Improving Met Office seasonal forecasts of Arctic sea ice using assimilation of CryoSat-2 thickness" *by* Edward W. Blockley and K. Andrew Peterson

Anonymous Referee #1

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This paper explores the benefits of assimilating winter sea ice thickness estimates from CryoSat-2 into the Met Office FOAM/GloSea sea ice assimilation and seasonal forecast system. The approach builds on recent work demonstrating the utility of sea ice thickness assimilation in perfect model and forced model simulation studies. The authors briefly explore the atmospheric response to an improved representation of summer sea ice.

The approach taken was simple - nudging the simulated winter sea ice thickness towards the CryoSat-2 observations and assessing the impact on the simulated ice state. Much of this was unsurprising - the simple nudging scheme worked and reduced a

modeled thickness bias. The analysis of the potentially more interesting benefits of assimilating sea ice thickness were sometimes less clear, including the general aims of the paper beyond demonstrating this thickness bias reduction.

I think the paper could be worth publishing if several key improvements are made as detailed below. Note that I am not an expert in data assimilation so hopefully another reviewer can provide more input regarding the approach taken and their method of evaluation.

Also a general gripe of this study and similar ones: I find it really challenging to understand what you mean by the use of forecast and hindcast throughout the paper and they seem to be used interchangeably. If you are using prescribed atmospheric forcings that have assimilated real data then my view is anything using that is a hindcast not a forecast. Prediction is the more general term that could be appropriate but I see nothing in this paper that resembles a true forecast (no future knowledge), despite the title.

Main comments:

It's really not clear from the motivation what it is you are trying to achieve by assimilating sea ice thickness in this study. In some cases you say the ice area/extent impact is negligible (as you ignore lower thickness ice) but other times it seems you highlight big improvements in your ice edge 'forecast'. You should really present a hypothesis you are testing in this kind of study (i.e. which metrics you are assessing). In general I would think assimilating thin ice should be especially important for seasonal (spring/summer) forecasting as you want to correctly incorporate this into your model as this is the ice most likely to melt out. If you get that wrong, you get the summer melt wrong. It seems like you used the opposite reasoning to justify not using the AWI/SMOS data but your results suggest the opposite to be true if you care about both the summer ice thickness and ice edge.

I had a lot of issues with Section 2.2 (describing the CryoSat-2 data): - You need to

make clearer the various thickness datasets available and how a number of groups are now routinely producing sea ice thickness estimates, e.g. NASA/AWI/CPOM for CryoSat-2. - CPOM and NASA data were used in the study of Allard 2018. You need to make this clearer. It was in general unclear how your study differed from Allard and I think you need more discussion of their approach and results. - Is the pole hole and the data uncertainty really why these data haven't been used? It seems like any reasonable assimilation scheme shouldn't need complete coverage and can factor in data uncertainties. My guess is that the main reason was data availability, the fact these thickness products were in their infancy, and inertia. - On that note, how do you treat the fact you are unlikely to have complete coverage from the CryoSat-2 data? - After listing problems associated with generic thickness data you then say this is improved by the availability of CS-2 thickness data. This doesn't make much sense as it is written. - You should cite the relevant studies regarding uncertainty estimates, not just the Ricker/AWI reference and apply that to CS-2 data derived by other groups. There are strong differences in the retracking procedures which may have impacts on respective data uncertainties across the products. - I don't get what this extra quality checking of the CPOM CS-2 data is. The fact you have included a personal comms from one of the data producers of that dataset makes it seem like this is something they do too? What exactly do you mean by smearing?

Why not use daily along-track CryoSat-2 data? I thought this was the whole purpose of CPOM releasing the daily along-track data? Instead they grid the data, then you grid the data, then you interpolate to a get a daily thickness?

Hard to tell what this volume comparison really means. You compare with PIOMAS but then say that data is biased low (which I'm not actually sure is true when you look at more than one CS-2 estimate) so it is actually good that you are further from that data? You say this was expected but this seems like a hindsight statement to me. I agree PIOMAS data can provide useful context but I don't agree with how you've used it. I think you should just show the CS-2 data and say look, the assimilation does what it is

supposed to do..

P10 L10-20 and elsewhere: Very confusing to me if these are hindcasts or genuine forecasts. You use both labels interchangeably. How could you move forward to produce genuine forecasts?

Specific comments:

In the abstract: really the 'first time, we directly assess the impact of winter sea ice thickness initialisation on the skill of seasonal summer forecasts'? Do you mean in the Met Office model framework? I think Allard and others have done this and I also don't think you do assess forecast skill

Introduction - In general you need more updated references. A lot of this discussion is a bit outdated now. i.e.:

- Drop the Vaughan/IPCC refs and use the more specific refs. Try Serezze & Stroeve 2015 for a more recent seasonal sea ice trends citation?

- The Collins/IPCC is also a bit outdated. I think you can add some of the more recent references to sea ice projections - e.g. Jahn 2016, 2018.

P2 L1-11 - I think this is not useful information as it is not that relevant to seasonal forecasting and a lot of the references and discussion are outdated. Either update/improve or drop.

P2 L13 - change sentence ordering.

P2 L17 - the predictive skill sentence is confusing. SIPN haven't really assessed that.

P2 L19-23 - I don't think you've really said why it is interesting though! Either make a clearer point regarding its scientific interest (e.g. what the predictability/memory of the system is compared to other components of the climate system). Also I don't think it is clear that sea ice is now necessarily harder to predict. Having some enhanced variability may be useful. Your figure 4 doesn't show an increase in ice edge error for

instance!

P2 L27 - why exactly does a lack of observations make the forecasts harder? Less to assimilate in models or to validate? Below you list a number of observations that are available in the poles...

P2 L35 - include acronym definitions.

P3 L4 onwards - this is a bit of a confusing paragraph to me. What is the point you are trying to make? In general my view is that there is hope for dynamical models being used for skillful sea ice forecasting based on some of the perfect model studies that you cited. However the SIO has really shown that they are not currently performing much better than the linear trend in many cases (as shown by Stroeve et al., 2014). It still seems that dynamical models are lagging behind more simple statistical methods (e.g. Schroeder et al., 2014, Petty et al., 2017) despite their sophistication.

I think you need to add in some comments on the different forecast methods available, merge with the following paragraph about improving dynamical models and make clearer what the motivation of this study is! This should be the key paragraph of the introduction.

P3 l 24 - not sure how this point links to the above.

P4 L1-4 - but in the abstract you imply you are the first to do this?! I guess you meant in your fully coupled Met Office forecast framework. You need to make that clearer.

P5 L11-13 - why just mention the ocean reanalysis component here? Would be more understandable if you referred to GloSea as a reanalysis.

P5 L23 - I don't think these are the correct citations here. Link to relevant passive microwave concentration datasets instead or recent papers describing that long-term record (e.g. Parkinson/Comiso papers).

P5 L25 - should reference Kwok and Cunningham 2008 instead.

P6 L16-17 - what do you mean by sensitive here? I think you mean uncertain/challenging? Again, is there nothing in one of the CPOM papers that highlights this issue?

P6 L30 - I think this is a big guess. Do we really know much ice < 2 m melts away each summer? That Keen modeling study (Fig 2?) suggests some 40% of the ice less than 2 m (including ice and snow in that thickness) does not melt through in summer. A lot will have to do with how much snow there was on the ice and where/when melt onset occurs. Even if it does melt away, this seems to be crucial information for determining solar absorption that can drive SST increases and further sea ice melt. Also need to make the point here (and earlier) that AWI do produce a merged product!

Section 2.3 - Why do you need extent and concentration? Surely you are just assimilating sea ice concentration? This needs to be made much clearer here. - The use of CMEMS 'data' seems very confusing to me. Why not use observed ice concentration?! - The NSIDC sea ice index is just a monthly index of total ice extent. This isn't what you use, right?

P8 L6 - what is the size of your model grid (in kilometers?) how does this translate to the 5 km CPOM data?

P8 L14-24 - Think you should list out the CICE thickness categories. How else could you have done this? I think it would be worth presenting more sophisticate approaches for future work, however I get that you started with this simple approach.

Figure 2 - I think you should also show (maybe in the supplementary info?) what the pre nudged, and nudged thickness fields are. Could just do this for the mean October-April thickness and also update Figure 1 to show this longer season too. If the mean thickness was way off before it makes sense that assimilating the thickness will improve things..

Why the different start dates for the forecasts? Pretty confusing.

Figure 4 is hard to see. Maybe box plots of the recent years showing the variability in the different estimates in the different years?

P11 L10 - CMEMS isn't really an observational estimate, right? Based on the assimilation of OSI-SAF...

Pll L14 - not sure what you mean by 'building a picture' here. I see no value in showing that earlier data.

P11 L17 what is close

Figure 5 - include numbers on map. Plot the IEEE as a time series.

Figure 7 and 8 - I don't understand these maps. How exactly is the data shown in Figure 8 calculated? Also is Figure 8 averaged over the entire year, but figure 7 is September? Why are the pressure units different? Perhaps better to show Figure 7a and 8a together, then 8a and 8b.

The discussion of using ensemble members instead of the ensemble means and how this relates to assessing model bias was confusing and needs more description/clarification.

Should either enhance the CLIM analysis or drop. i.e. repeat with using a fixed (think FIXED_IC would be a better acronym) for all years of data available.

How do these results compare with Allard?

P14 L21 - not sure what you mean by work well. Maybe not gone wrong?

P14 L1 Why is this from dynamics not thermodynamics or the forcing being off?! Also drop 'so-called ice-ice force'