

Response to review #2

We thank the referee for their review of our manuscript. Answers to the specific comments in the review are listed inline below. The review comments are in black italics and our response is in red.

Specific Comments

Page 3 Line 1: Add Cummings and Smedstad (2014) for another coupled ocean-ice modeling system with data assimilation, here is a full reference: Cummings, J. A. and O. M. Smedstad, 2014: Ocean Data Impacts in Global HYCOM, Journal of Atmospheric and Oceanic Technology, 31, doi:10.1175/JTECH-D-14-00011.1

The articles cited here (i.e., Tonani et al., Martin et al., Balmaseda et al.) are all large multi-centre papers which describe all of the global ocean/ice analysis + forecasting systems that contribute to the GODAE OceanView project. This includes HYCOM-based systems such as that documented in Cummings and Smedstad (2014). Jim Cummings is part of the author list for one of these, and several Cummings papers are cited. However, this exact paper is not one of those referenced in the papers we cite here and so we shall add this citation to the revised manuscript.

Page 7 line 31: why couldn't a longer period, say from 2010-2017 or at least 2010-2016 be used?

This work was started in 2016 and so the analysis was performed up to September 2015, which was the most recent summer period at that time. This is true for all the control runs that we used in this project as well as the CS2 runs that we performed ourselves. Whilst undertaking this work, we were only able to perform such a large ensemble of seasonal predictions because the delay finalising our model configuration for CMIP6 meant that there was a good chunk of computational resource available. This is no longer the case and CMIP6 runs are in full swing now – meaning that we do not have the computational resources available to extend this to 2016 or 2017 at this time.

Page 8 line 14: which version of CICE (v4.0, 4.1) is used. Are melt ponds used in this study?

CICE vn5.1.2 is used here and the configuration includes topographic melt ponds. The coupled model version used here (GC3) is documented in Williams et al. (2017) and the sea ice component therein (GSI8) is documented in more detail in Ridley et al. (2018). Therefore we do not intend to include lots of technical information about the sea ice model used here.

However we agree with the referee that more information is required here and so we shall expand the text at the end of Section 2.1 to include key features of the sea ice model component (such as multi-layer thermodynamics, 5 thickness categories, prognostic melt-ponds, etc.).

Page 14 line 17-18: can you quantify bias reduction with some range or percentages?

Yes we will do this in the revised manuscript. This will take the form of quantifying mean SIT differences – as a value or a percentage –within the Atlantic and Pacific sides of the Arctic. We shall also extend Figure 2 to include plots of mean thickness for the control and thkDA runs to help illustrate this.

However, we feel that this would sit better at the end of page 9 where the bias/difference is first discussed rather than on page 14.

Figure 4: Please add an inset for both plots showing a blow-up for the period 2010-2015? It's a bit difficult to see with the longer data record shown.

Reformatting Figure 4 was also requested by referee #1. We are trialling different options for this at present – using insets and/or additional panels. The revised manuscript will include an improved Figure 4 that allows the results to be more clearly displayed. More space on the plot will be devoted to the key time-period of 2011-2015, and we will ensure that the individual ensemble member values are more easily distinguished.

There is no mention of ice drift in the paper. Could you analyze IABP ice drift data (Pan-Arctic domain) to determine the impact of assimilating CS2 data into the seasonal forecasts? This would complement your existing study. SIDFex is presently examining modeling center's skill in making long-term ice drift trajectory forecasts.

We have compared the May and September sea ice velocity fields from our two main experiments (CTRL and thkDA). We find that the May velocities are virtually indistinguishable in each of the 5 years (2011-2015). This is consistent with the findings of Allard et al. (2018) who show little impact on ice drift in their reanalysis comparisons.

The September velocity fields are also very similar. Although slight differences arise from the differing ice coverage, the ice drift is broadly/qualitatively unchanged in the experiment using sea ice thickness initialisation.

Thanks for the pointer to SIDFEx, which is something we've been meaning to get involved with for some time. We are in the process of setting things up to contribute to SIDFEx using our FOAM and coupled NWP systems (but sadly not from the GloSea seasonal prediction system because that only outputs monthly-mean ice drift).

I would like to see the ice edge error metric used to examine the regional differences seen from use of the CS2 data. Can it be divided into the following (or similar) basins (Beaufort/Chukchi/Bering Sea, Canadian Archipelago, Greenland Sea, Laptev Sea, Barents Sea, East Siberian Sea)?

Good idea. This would be a useful to underline the key points of this work. We have calculated the IIEE for several Arctic regionals and are exploring ways to display this information. This will most likely lead to an additional figure along the lines of Figure 1 (at the end of this document).

These results are consistent with the rest of the results here: runs with CS2 initialisation have decreased extent in the Beaufort & Chukchi Seas and increased extent everywhere else. In both cases, this brings us closer to the observations/reanalysis and lowers the ice-edge error (IIEE). Improvements are most notable in the central Arctic region - and particularly the Atlantic sector.

No comparisons are made against ice thickness observations from either ice mass balance buoys and/or moored ULS data. I recommend inclusion of some time series plots of the modeled ice thickness beginning with the Apr/May initializations through September for 2010-2015, with the control run included. The ensemble spread can be shown as well. This should clearly show the impact of the inclusion of the CS2 data.

While comparison with ULS would be interesting, we feel it is out of scope for this study where we are heavily focussed on improvement to sea ice cover (extent and edge location). However, this sort of comparison is something we would wish to do before implementing a proper 3D-Var sea ice thickness assimilation scheme – and in fact is currently being undertaken within the H2020 SEDNA project.

Lots of acronyms are used without spelling them out. A partial list is shown below. Perhaps a list or table of acronyms would be useful.

We apologise for this oversight. The revised manuscript will ensure that acronyms are spelled out at the point of first use. We do not think that this manuscript is “acronym heavy” enough to need a glossary appendix/table though. However, we can include one if the referee (or the journal typesetters) feels strongly about this.

Technical Corrections

Page 2 line 6: replace “Better knowledge” with “Improved knowledge”

Page 2 line 34: define SLA here

Page 3 line 22: define CFSv2

Page 3 line 25: replace “find” with “found”

Page 3 Line 34: Yang et al. reference not listed in References

Page 4 line 3: spell out NRL (Naval Research Laboratory)

Page 5 line 3: spell out FGAT

Page 5 lines 12-14: “used” appears in sentence 3 times. Perhaps change second mention of this word to “utilized”

Page 5 line 26: reword statement “have been around for some years”

Page 5 line 27: replace “main” with “primary”

Page 5 line 32: spell out SIRAL

Page 6 line 22: replace “was” to “is”

Page 7 line 16: spell out SSMI/S

Page 7 line 28: Ridley reference says (2017, in review); reference section states 2018

Page 9 line 24: reword to “although IT has”.

Page 10 line 4: delete “the fact”

Page 10 line 7: reword “amongst other things” what things?

Page 10 line 17: Williams reference in references section says 2018

Page 10 line 25: MacLachlan reference says 2014 in references section

Page 12 line 2: delete “down”

Page 13 line 26: reword “doing things this way”

Page 14 line 28: spell out SEDNA

Page 15 line 4: Neither Tsamodos reference is listed in References section

Page 15 line 17: replace “down” with “due”

Page 21: Peterson reference should be 2015 not 2014

Many thanks for providing such a thorough list of technical changes. We shall of course incorporate all of these changes in the revised version of the manuscript.

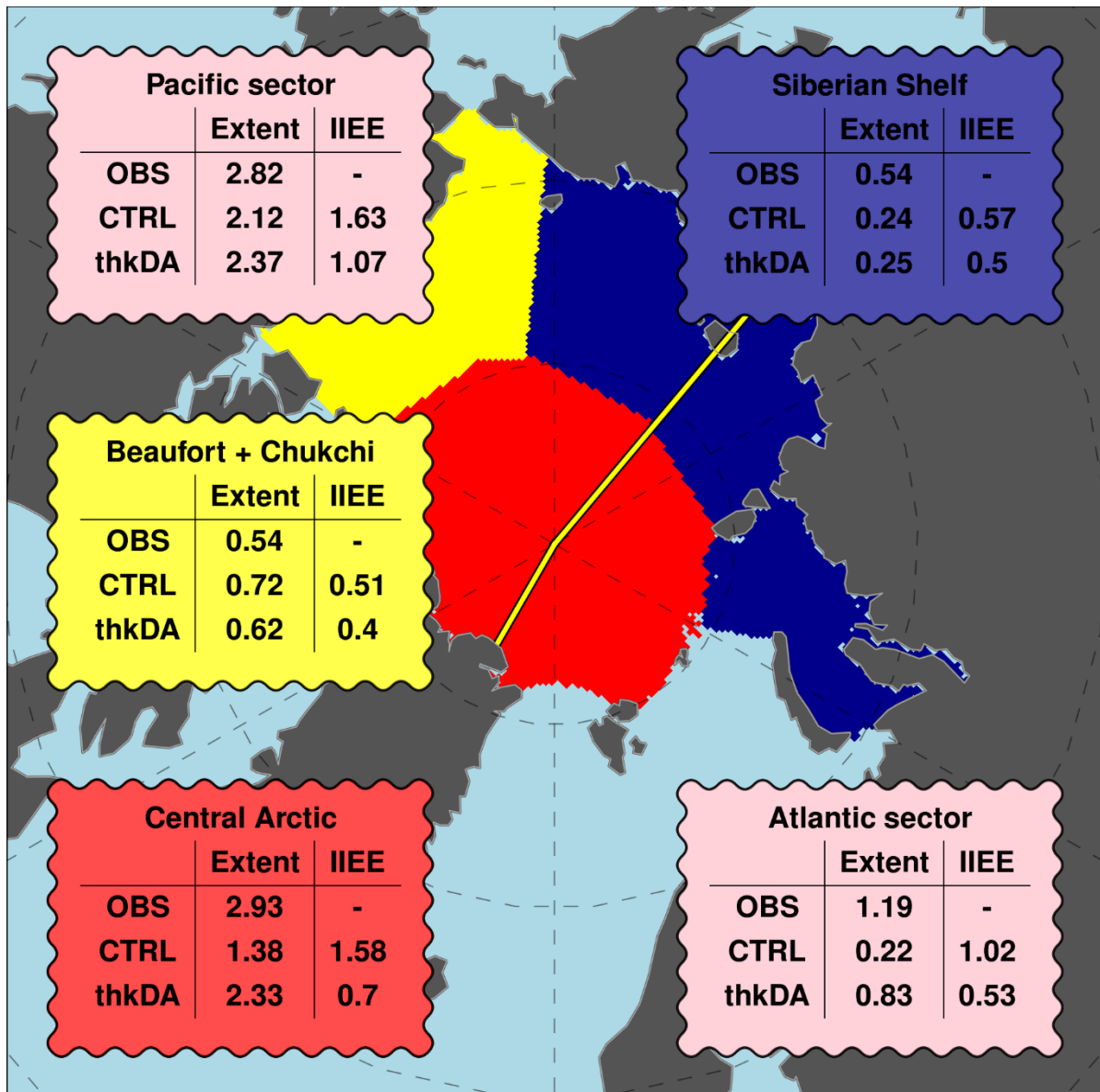


Figure 1: September-mean Arctic sea ice extent for the CMEMS reanalysis (using OSI-SAF) compared with modelled extent and ice edge error (IIEE) from the control (CTRL) and thickness assimilating (thkDA) seasonal predictions. Data are shown for 3 regions – distinguished by the underlying shading and corresponding box colours – as follows: combined Beaufort + Chukchi Seas (yellow), combined Kara, Laptev and East Siberian Seas (dark blue) and the central Arctic (red). Areas are defined using the standard NSIDC regions and units are millions of square km. Also shown (pink boxes) are corresponding statistics for the Atlantic and Pacific sectors of the Arctic Ocean. These are defined by splitting the union of the three coloured regions (i.e., red + yellow + blue) in two along the 30W and 140E longitude lines (yellow line) – which roughly follows the Lomonosov Ridge.