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Interactive comment on "Year-round Impact of Winter Sea Ice Thickness Observations on Seasonal Forecasts" *by* Beena Balan-Sarojini et al.

Anonymous Referee #1

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Balan-Sarojini and co-authors present a study examining the impact of sea ice thickness (SIT) assimilation on seasonal forecasts of the northern hemisphere sea ice cover. In its approach and scope the study covers new ground; several of the key findings are substantive and represent a significant advance in our understanding of sea ice predictability and performance of seasonal-scale forecasts. The authors make good use of newly available, state-of-the-art ice thickness fields and strike a nice balance between more fundamental questions of prediction system performance, and applied questions related to improving forecast skills of Arctic sea ice models.

The paper is well structured and makes good use of figures to illustrate key points. The first half of the paper (up to and including Section 3.2, Fig. 7) is particularly compelling and self-contained. The latter part of the manuscript, while interesting, is less compelling with some of the writing lacking clarity and the paper losing focus with respect to the goals laid out in the introduction and implicit in the title. If this part of the paper is retained in full, tightening the text and improving readability of sections 3.3-3.5 in particular would make the paper more accessible.

At the same time, a few aspects of the paper can be improved or require further thought, as outlined below.

First, given the central role the SMOS/Cryosat-2 data set plays in this study, one would like to see some discussion of how errors and uncertainties in the ice thickness data set may have impacted forecast skill and in particular some of the regional patterns observed in the thickness-assimilation runs. As shown in Ricker et al. (2017) uncertainties due to the fundamentally different retrieval approaches for SMOS and Cryosat-2, and to a lesser extent the optimal interpolation and data merging schemes, vary significantly by region. For example, over the Canadian Basin with mostly thick, multiyear ice the data product is dominated by bias/errors in Cryosat-2 data whereas in the Bering or Okhotsk Sea thinner ice weights errors towards those associated with SMOS thickness retrievals. It would be important to establish whether differences in thickness-field uncertainties have any impact on model performance and regional or temporal contrasts in forecast bias. This is also relevant for the integrated analyses of parameters such as the Integrated Ice Edge Error or ice volume at the pan-Arctic scale which may gloss over important regional contrasts in model performance.

Second, the paper lacks detail on the representation of ice thickness and key ice growth, melt and deformation processes in the LIM2 prognostic thermodynamic-dynamic sea ice model used in this study. It would be important to provide more detail, in particular as to whether any of the parameterizations that are part of the Fichefet & Morales Maqueda (1997) – FMM97 – model have been updated or changed. Of potential concern in FMM97 – based on description in their original paper – would be the limited representation of surface melt processes and their impact on ice albedo as well as physically unrealistic representation of internal ice melt (with internal "storage" of solar heat up to a 50% threshold). These shortcomings, which may have been addressed in updates but if so the paper needs to make this explicit, do not necessarily limit applicability of the model in the context of seasonal ice forecasts. However, they are problematic in diagnosing some of the linkages between surface forcing, energy storage and the seasonal ice cycle explored in Section 3.3, since FMM97 in its original form may be ill suited to examine in particular the spring-summer-fall transitions in terms of the surface radiation balance or rates of ice thinning and decay.

Given these potential concerns, it would be instructive in Section 3.3 to examine the proportion of up/downwelling shortwave fluxes (or albedo, for that matter) to get a better perspective on the sensitivity of sea ice as represented in FMM97 to variations in downwelling shortwave energy. Such a detailed analysis may well be beyond the scope of the present paper. If so, this may be an argument to remove the latter parts of the paper as the basis for a separate, more detailed study. The first part of the paper (up to Section 3.3) is substantive enough and fully in line with the title of the paper.

Third, starting with the discussion of sea ice volume at the pan-Arctic or northern hemisphere scale the paper began to veer off-course a bit in terms of the goals laid out in the introduction. While total ice volume is a great integrator and a relevant variable in a global context, I was not able to tell whether the authors were assuming that it can also serve as an integrated measure of model performance in terms of ice concentration/extent and ice thickness. Given the regional contrasts in model performance apparent in the early figures of the paper the wholesale discussion of ice volume is somewhat problematic. For example, the interpretation of the seasonal ice volume cycle in terms of a single "freezing rate" (p. 17, top paragraph) is too simplistic since increases in ice volume during fall and winter occur through a combination of ice deformation and ice growth inside the ice pack as well as advance of the ice edge in marginal seas. Without an in-depth analysis some of the earlier figures and a solid understanding of how well the sea ice model is capturing the relevant processes, Figures such as Fig. 8, don't add that much to the paper and could be relegated to supplemental materials or cut completely.

Finally, just a few minor points: - Comparing bias in ice thickness (Fig. 1) with bias in ice concentration (Fig. 3) it's striking that regions with near-zero bias in thickness (e.g. East Siberian Sea, Chukchi Sea in November) show up as having significant bias in ice concentration; moreover despite substantial contrasts in thickness biases between reference and ice thickness runs (Fig. 1c&d) the biases in ice concentration are near indistinguishable (Fig. 3 g&h). How can this be explained? - In regards to July ORA-SIT biases in ice concentration, it was striking to see much larger bias in the ORA-SIT than in the reference runs. Why would the simulations that performed (understandably) so much better in replicating ice thickness in March through assimilation of ice thickness data perform much worse in replicating ice concentration in July? Note that this finding also seems to contradict your statement in l. 185 that "The non-availability of the observations for the melt season in a way provides an opportunity to test the predictability of winter SIT from summer initial conditions." - You discuss your findings in terms of Arctic ice concentration and thickness but your figures include regions outside of the Arctic proper (such as the Okhotsk Sea). Please clarify whether both model output and assimilated data cover the entire northern hemisphere sea ice or a subset of that data. This is relevant in particular for figures like Fig. 5 which references "nh" in the figure label (for northern hemisphere?) but refers to Arctic sea ice area in the caption.

Minor comments & corrections

l. 2/3: change to "in its early stage"

l. 20 "near-surface temperature forecasts of early freezing season initialized in May": This phrase is confusing and not entirely clear, please revise to clarify what specifically is forecast with respect to "freezing season".

l. 25: change to "lasts into autumn"

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l. 80: “it is relevant as cross-check variables evaluation” – not entirely clear what’s referenced here – should it be “they are relevant because they allow for cross-checking between variables”? Please clarify.

l. 81: “SIT verification is also conducted as a sanity check of the nudging approach” – You lost me at “sanity check” – what exactly are you doing here? Please explain.

l. 91: change to “The Level-3”

l. 145: “LIM2 has a single sea ice category to represent sub-grid scale ice thickness distribution” – this needs further clarification. To calculate an effective conductive heat flux through the ice Fichefet and Morales Maqueda (1997) assumed a uniform thickness distribution bounded by zero and twice the average thickness. This parameterization was only applied in calculating heat fluxes through ice and lateral melt rate but did not enter into any of the ice dynamics components of the model. Given that ice thickness initialization is central to this manuscript, a clearer description of what exactly was implemented is needed.

l. 168: change to “differ in”

l. 233: “These results clearly show. . .” – Some clarification is needed here, since I interpret Fig. 4 as indicating that through May (but not the entire melt season), the effects of SIT assimilation are evident, beyond that the reference run performs better through the end of melt. In linking SIC increments to SIT assimilation please also consider the points raised in the general comments above.

l.238: “(units are. . .” – This should be part of the figure legend or caption, and not be buried in the main text.

l. 245: change to “melt season forecasts are considerably reduced”

l. 251: The top labels of the figure panels are cut off and it’s not clear that they’re actually needed (“bias for sia in area nh” – would need to be explained; also: is nh Northern Hemisphere? If so, what is the difference between this data for northern

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hemisphere and the Arctic sea ice area as indicated in the figure caption?); the color scale needs better labeling.

l. 265: insert “are” in “that are to be expected”

l. 268: Fig. 6 - This figure should be cleaned up a bit as well; there’s no need for two top labels (the upper one is more descriptive anyways, but even that’s not needed given the explanation in the caption); the color bar needs proper units. Fig 7: Same comments apply – the 1e12 and 1e11 squeezed right next to the figure panel label and disjunct from the axis label (with units of square meters) are less than ideal and need to be cleaned up.

l. 287: Fig. 8: It’s not clear to me how an axis label of 10ˆ1 3 mˆ3 translates into 10ˆ12 mˆ3 as the figure caption claims. Why not put an axis label in kmˆ3?

l. 361, Figure 11: same comments as for Fig 6 apply

l. 369: correct spelling of “Atlantic”

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