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Discussions

Interactive comment on "Exploring the utility of quantitative network design in evaluating Arctic sea-ice thickness sampling strategies" *by* T. Kaminski et al.

Anonymous Referee #2

Recommendation

Major Revision

Comments to Author

The authors explore the utility of two Icebridge type aircraft trajectories for observing ice and snow thickness for constraining sea ice forecasts in Chukchi sea and along the

Alaskan Coast using a variational assimilation framework along with a quantitative network design investigation. The study is extremely innovative, introducing a framework to easily discern the usefulness of a given set of observations, here ice and snow thickness observations, for forecasts of ice. The authors do a good job of introducing their technique, but I found the description of the results somewhat light. Their results section covers only 7 paragraphs, but attempts to describe 9 different figures – although admittedly 3 of those figures (Figs. 6,9, 12) are additional information for previous figures (Fig. 5, 8, 11). I think the results section could be significantly enhanced, eliminating some of the unnessary (and not referenced) figures, concentrating on better explaining the figures they have deemed most relevant. With the exception of the results for the Bering Strait to Prudhoe Bay (BS2PB) region, the authors choose to study uncertainty and parameter sensitivity to single day forecasts, even at 3 and 5 months lead time. I believe at these timescales, one should concentrate on the sensitivity to time integrated forecasts of at least one month (at minimum one week) duration. At the very least, the authors should discuss how their results would be dependent on such a short period of forecast interest at such long range lead times.

Major Comments

1. I found the description of the model too brief. Presumably it is described fully in Kauker (2003), but here I would think the thickness of the top ocean layer and a slightly more detailed (currently a single sentence) description of the sea ice, including a description of how snow is handled would be in order.

2. The authors concentrate on a summer period – 1 April to 31 August 2007 when no satellite observations of sea ice thickness are available as this is the period of primary interest to navigation and other interested parties in the Arctic. Some additional comments on why this particular period was chosen, along with some

detail of observation deficiencies inherent to it would be useful.

3. For the prior uncertainty of the initial state, the authors used the initial state for a given calendar day over a twenty year period. Similarly, they use the standard deviation of the atmospheric forcing (for the 5 month period) over a twenty year period as the uncertainty in the atmospheric forcing. This is effectively climatology. Without a detailed analysis of the surface atmospheric state that can be provided by a seasonal forecast model – and undoubtedly, even with such a detailed analysis – this is probably a good assumption of the uncertainty in atmospheric forcing. However, this would seem a rather harsh assessment of the uncertainty in initial conditions. Satellite observations of ice concentration, and winter observations of ice thickness, while still having significant uncertainty, presumably would constrain the initial condition uncertainty better than (model) climatatology. A statement detailing the significance of these assumptions in the discussion of the results I think would be warranted.

4. The sensitivity diagrams (Figs. 5, 8, 11) are extremely hard to interpret. I assume each of the control variables in Table 1 could be on the x-axis of the figure, with the forcing and initial conditions further divided among 9 regions of Figure 2. This is obviously a huge amount of information, which presumably only a subset of which is represented in the figures. The authors have attempted to label which control variable is being represented on the plot, but this can just as often be confusing to follow as well. To further complicate things, figures like Figure 5c are completely empty. The solitary Figure 8 is the only plot that is remotely decipherable. I have no definitive suggestions how this large amount of data can be adequately relayed to the reader, except to perhaps present less of it. Perhaps separate figures for parameter, forcing and initial condition space would be a start. Empty figures such as Figure 5c should be eliminated, replaced with commentary on why there is a null result (I don't know the answer).

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5. The impacts on the Chukchi and North of Barrow regions discusses the impacts for day 91 and day 153 of the forecast. Surely this in nonsensical. At these time scales any skill to be had, and any desire for forecasts, would be for integrated periods of time. Users of the forecasts would presumably not be interested in conditions on 30 June and 31 August, but rather either on average conditions throughout June, July, or August, or the probability of a large ice event during the period. Presumably, a lot of the dependency on the control variables would transfer through to forecast quantities integrated over longer periods, but this should at least be discussed. Indeed, the navigational requirements discussed through the BS2PB target region are temporally integrated quantities, so they have considered this in the context of that.

6. I disagree with the authors final comment, that the concept can be applied on smaller regional scales. While, technically this is true, the authors own results clearly show that local results can be dependent – and indeed more dependent – on both non-local forcing and observations. At time scales of months, it will be critical to consider the Arctic system as a whole, and not solely some local region. Indeed, conditions outside the Arctic may be equally influential.

Minor Comments

1. p. 1737, l. 2: I was always taught that what follows after a colon (:) constitutes a complete sentence. "First" should be capitalized.

2. I assume SD is standard deviation – it is never defined. First appearance p. 1743, l. 23.

3. p. 1739, l. 25: "T is the transposed." I would prefer "T is the transpose operator."

4. Figure 1 & 3: I think Figure 1 & 3 could be combined into a single plot. Both plots have a relatively sparse amount of information on them, plus placing the transects in relationship to the regions of interest in forecast skill would be instructive.

5. Figure 4b: Airborne measurements of ice and snow thickness along the B2F transect seem to have an undue remote influence on ice concentration in the Chukchi region on timescales of only 10 days. Is there a physically identifiable reason for this?

6. Figures 6, 9, 12: Can the target region be included in the figure?