Comment on tc-2021-353
Anonymous Referee #2

Referee comment on "Seasonal Sea Ice Prediction with the CICE Model and Positive Impact of CryoSat-2 Ice Thickness Initialization" by Shan Sun and Amy Solomon, The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-353-RC2, 2022

Review of "Seasonal Sea Ice Prediction with the CICE Model and Positive Impact of CryoSat-2 Ice Thickness Initialization" by Shan Sun and Amy Solomon.

This manuscript investigates the forecast skill of the sea-ice model CICE in stand-alone simulations. The analysis includes both the Arctic and Antarctic. The model CICE is shown to adequately reproduce the seasonality of the sea ice extent and volume, but with a larger difficulty in forecasting the winter and spring conditions. A particular interest is placed on the influence of the initial ice thickness on the forecast skill. Specifically, the use of the CFSR ice thicknesses, which over-estimates the ice thickness, is shown to decrease the forecast skills regardless of the initiation month. This is largely improved when ice thickness from Cryo-Sat2 is used instead. The simulations are presented as a baseline for future studies on the forecast skill of coupled models using CICE as the sea-ice component.

This manuscript is relatively well written, although many sentences are too long and confusingly constructed. The results are interesting and will interest many in sea-ice modelling community. Nonetheless, the manuscript suffers from an unclear problem statement in the introduction and from a tendency to describe figures without much in depth interpretations. This is especially important given that other studies have looked into the impact of ice thickness on forecast skills.

I believe this manuscript have potential for publication, but require major revisions.
Major points:

- There is a tendency (mostly in the introduction) in lumping too many ideas in complicated sentences. The text should be revised in that regard.
- The introduction lacks a problem statement and should be revised to clearly identify the scientific questions that the analysis aims to answer. A problem statement is vaguely formulated at L37-45 but mixed with the broader context. I believe that adding a paragraph devoted to the problem statement (mainly on the influence of the initial ice thickness on the forecast skills) would largely clarify the scope of the manuscript. In particular, it should clarify what information the stand-alone CICE simulations can bring that has not yet been documented.
- There is very little mention of the sea-ice dynamics (in the experiment setup, results and discussion) although it should largely affect the sea-ice extent, especially in a year-long simulation. The thermodynamics and dynamics contribution to the SIE is briefly investigated in Figure 7, although this analysis not clear (the methods are not described) and needs to be clarified.
- Much of the results are very descriptive and not thoroughly discussed. I believe that a comprehensive assessment needs to be include before publication. For instance, many statements are vague and general (e.g. the forecast skill is reduced in the alt-init simulations), despite the figures presenting much information. More in depth analysis of the results could include, for instance, explaining the differences in the SIC and SIV forecast skill patterns, and how it relates to the initial ice thickness.

Specific points:

L17-19: This sentence is confusingly constructed. Perhaps dividing it in smaller sentences would be clearer.

L20-22: This sentence is currently confusing as it covers too much while being too vague. For instance, what “forecast” and “predictability” are we talking about (weather conditions? Ocean? Climate?). The vague reference to the impact of sea ice conditions on teleconnections also needs expanding.

L23: It is not clear what we are talking about here. Weather?

L23: I am not sure that “leading” is the right verb... Perhaps “Driving”?
L29: The use of “In particular” is confusing here, as we were discussing the influence of sea ice on weather predictability, but now jump to sea ice forecasting.

L37-39: Do I understand that here, you validate the sea ice model component of a fully coupled ice-ocean-atm model, in a first step towards investigating how it feedbacks with the other components?

L37-45: The structure of this paragraph makes it difficult to understand the scope of the paper. It first indicates that the aim is to isolate feedback processes between coupled model components, then that it is to validate the sea-ice model used in NOAA UFS, in stand-alone simulations. However, I believe that the real goal here is to assess the influence of initial thicknesses on the ice predictability within CICE. This needs to be clarified.

L51-53: This sentence needs revisions.

L55: I suggest starting a new sentence after “experiment”.

Section 2: Some information on the dynamical component should be provided (e.g., I assume it is the standard EVP rheology and strength parameters in CICE?).

L66-69: This long sentence could be improved.

Section 3: What is defined as a “reliable forecast”? This statement is made at various places throughout the result section, but sounds rather subjective.

L82: It should be specified here (not later) that Figure 1 only shows simulations from 2014.

L84: There is very significant inter-annual variability in Arctic (and Antarctic) sea ice extent, yet here you say that the inter-annual differences are small? This needs to be clarified. For instance, you show 2014, a year where the winter maximum was relatively small (~14.9 million km^2) and the summer minimum remarkably large (5.0 million km^2). It is possible that conclusions drawns from Figure 1 are not representative of different years, such as 2012.
This result is very surprising to me, as the summer minimum is usually described as being more difficult to forecast and more dependent on the early summer meteorological conditions.

Together, these results are confusing and should come with some analysis and explanation. The ice extent results seem to indicate that the model overestimates the ice growth, but that does not show in the SIV results. On the contrary, the SIV results seem to indicate biases in the spring and summer melt, but this does not show in the ice extent results. Why? Is this expected? Also, changing the initial thickness improves the overall skill but does not seem to change the temporal patterns. Does this imply that the thickness influences the error magnitude but not the predictability patterns?

Figure 2: A couple things that are concerning in these figures:

- Why is there a low concentration spots at the North Pole, but only for the top-middle and bottom-right panels, and of different size?
- What is the line of low concentration North of Franz Josef Land, running from the Laptev Sea to Svalbard? It is a very strange location and orientation for such an LKF, and it is seen both in April and October. More confusing, it is seen in the observations but only in October. Is it real or is it an artefact?

On the initial thickness being the dominant source of error: how did you determine this dominance? There are no results on the contribution from other sources (dynamics mass balance, thermodynamics mass balances). This statement is also contradictory with the fact that the errors are large in the winter sea ice at all lead time: they are thus are likely dominated by other factors than initial ice thickness. The use of CryoSat also does not seem to change this pattern.

This sentence is confusing and needs to be re-organised.

I would edit to: “the SIE and SIV in the alt-init run in the fall ended up closer to NSIDC and PIOMAS than in the control run.”

How are the dynamics and thermodynamics contribution measured? I find it somewhat surprising that the dynamics tendency is exclusively negative. Also unclear to me is how do you define the thermodynamics tendency in sea ice extent? The thermodynamics is usually defined by column physics, and not directly related to changes
in area.

L138-144: How can the top melt be influenced by the ice thickness? It is more intuitive for the bottom melt, as the reduced thickness would also reduce the insulation, but it could also be mentioned.

L140: remove “apparently”.