Response to Reviewer #1 comments

The authors thank Reviewer #1 for their comments. Responses to specific comments are below (reviewer responses in blue).

Snow thickness is difficult to simulate in sea ice models. The unreliable precipitation and the lack of robust observations could be attributed to as possible reasons. However, since the conversion from the freeboard to thickness relies majorly on how well the snow thickness is simulated, I wonder if the authors could provide some sentences to discuss the sensitivity of the forecast results to the simulated snow thickness.

Added further discussion, from line 147 now reads:

Currently, CPOM makes use of a modified snow depth climatology, based on Warren et al. (1999) and halved over first-year ice, for processing CryoSat-2 sea ice freeboard retrievals and conversion to SIT (Tilling et al., 2015). This approach is also used by other centres processing CryoSat-2 freeboard observations: Alfred Wegener Institute (AWI; Ricker et al., 2014) and NASA (Kwok and Cunningham, 2015). Instead, here the FOAM modelled snow depth is used. Modelled snow depth has a greater spatial and temporal variability than can be obtained from a climatology, as demonstrated by Mallett et al. (2021) and illustrated on Fig. 1. Using this method also maintains consistency between SIT and snow depth within the FOAM model. A preliminary validation indicates that the FOAM snow depth is somewhat thinner than the modified climatology of Warren et al. (1999), as shown on Fig. 1, particularly over multi-year ice. Tuning experiments demonstrate that simply increasing the snow depth in the model does not result in better evaluation of the SIT analysis against independent observations, owing to feedbacks in the model and between the SIT assimilation and the snow depth itself.

Snow depth uncertainty is a large source of error in radar altimetry sea ice measurements, both in the retrievals of freeboard and the subsequent conversion to SIT (e.g. Giles et al., 2007; Ricker et al., 2015). Due to the linear relationship between SIT and snow depth (Eqns. 1 and 2), an underestimation of the snow depth would lead to an underestimate in the SIT. Large uncertainties in the snow depth may apply whether it has been modelled or taken from climatology. Additional uncertainty is also introduced in Eqn. 1 through lack of knowledge of the snow and sea ice densities which, although constants in the CICE model used here, are spatially and temporally varying in reality (e.g. Alexandrov et al., 2010; Kern et al., 2015). Uncertainties due to variations in water density can be neglected (Ricker et al., 2014; Kurtz et al., 2014). In order to quantify and reduce the uncertainty in the FOAM modelled snow depth, future plans will include the assimilation of satellite snow depth observations.

Reference: Mallett et al., (2021): The Cryosphere, 15, 2429–2450. https://doi.org/10.5194/tc-15-2429-2021

Other references as already cited in the paper.

L142: make -> makes Changed

Eq (3): The threshold for thickness is 0.7m when having an uncertainty of 8. I realize that the authors have explained how the shape of these functions are obtained, but I feel curious of why 0.7m is used. If it was arbitrarily selected, then this information is necessarily to be present in the context. 8 m was the arbitrarily selected value as the maximum uncertainty for thin ice observations, being a very large value compared to the observations themselves. 0.7 m is simply where the function happens to reach 8 m. This has been clarified in the text:

"8 m is an arbitrarily-selected value of the maximum uncertainty for thin ice observations, being a very large value compared to the SIT observations themselves. This ensures that these data do not influence the analysis. 0.7 m is where the function reaches this value."

Paragraph L370: About the unexpected improvement in the mean differences. The authors state that it is caused by the spatial noise introduced during data assimilation. That could be one of the reasons, but from my side, I tend to believe it is caused by the systematic errors of the model. In Figure 5b,c, the model shows negative thickness bias, which indicates a slower growth during the freezing period. However, as suggested in Figure 9a, slightly thicker ice than observations are generally found. That is to say, the assimilation introduced thickness increment is faster than the ice growth by the model physics, i.e., ice grows faster in observations than in models. Could that be the case? I have no evidence about that.

Yes, this could also be an explanation. Have added to line 373: "This may also be a result of observed ice growing faster than in the model, which has a systematic error towards thinner ice in the control (figure 5(b); figure 11(b))."

The discussion and conclusions could be made compact. I currently read it feeling too much redundant information. I would suggest one or two future plans are already enough for wrap-up. Changes have been made to cut down this section. However, since this the aim of this paper is to demonstrate that the assimilation is feasible, and emphasises that the work is not complete, it is useful to summarise the future work that is required. It is also important to communicate to SIT observation data producers what is needed from them in order to move forwards.