First of all, I would like to express that I appreciate that the authors have worked on their manuscript! I think the structure and thus readability have really improved, and it is easier to understand what has been done here. The motivation behind and main idea of the paper are of course still very relevant and would serve the scientific community. And I think that the continuous year-long snow and ice time series produced here would be really useful. However, I think that the authors should either focus on producing the most realistic estimation of a continuous snow and ice time series assimilating the model to the observations and "do the best they can" to achieve this OR focus on "praising" their model and show how well the simulations agree with observations (see my comments 1 and 3) . Here, it looks a bit like they are trying to do both… In addition, there are still inconsistencies (see comment 2), and it is still unclear to me which of the described observations have been used (and which not) and why (comment 4). As I think that most of these concerns can mainly be addressed by modifying the description instead of the actual work, I would consider these minor revisions.

1. The authors (still) claim that the SnowModel LG reproduces the snow evolution accurately (p. 21, l. 309-401 and l. 420-421*[1] and p. 28, l. 588-589) during the time periods where there are no observations. The difference D (in terms of SWE) between the simulations and the observations has been subtracted from the simulations to match the observations (especially important for the two SYI cases), so of course the time series agrees well with the observations but we do not have information for the time periods in between, so how can the authors judge on this...? An exception is the good agreement between simulations and observations in June/July, which were not assimilated, which is why - here - the model can be "evaluated" (as is separately mentioned on p.21, 402-405). But note also my comment 3 as the starting point of the model might have been adjusted to potentially match these observations (or the ice thickness observations) better (?).
*[1]Additional remark: I had already mentioned this in the first review round, pointing at the sentence that is now found on l. 420-21 (before: l. 394) and the authors replied "Will be corrected", which was obviously not done as the exact same sentence is still in the manuscript.

2. The authors write that D (= the difference between observations and simulations, assumed to be related to deformation) is small throughout the simulations (p.22, l. 443) and that "its accumulated effect by 7 May (the last winter observation) was 10% in Nloop". I guess they mean 10% of the precipitated SWE value (which still means that the simulated SWE at this point in time has to be reduced by ~50% to match the observations). However, based on Fig. 8 it should be ~15% (visual inspection)... The number of 10% is also given in the abstract, should be checked.

3. The authors did not explain (although I asked for it in the first review) why the surface freeze-up date (= start of snow accumulation) for the fresh ice ("deformed SYI") was determined using the (3 hourly) air temperature time series, while for the saline ice ("ponded SYI") it was determined using the 3 day running mean of that time series. (In their reply they explained why different thresholds were used, which was clear (different freezing temperatures of fresh and saline ice) but not why the 3 day running mean was used in one case but not the other). If this cannot be explained, I (have to) guess it was because the results (e.g. regarding the match of simulations and not assimilated observations in June/July and/or the agreement between simulated and observed ice thicknesses) fit better when doing it like this...? I think it is ok to use a more "subjectively" determined starting date for the simulations (as compared to using identically - and thus more objectively – derived dates for both) but this has to be communicated. Especially, as it is the authors themselves who stress that "The correct initialization of our simulations proved to be the most critical aspect of our work." (p. 28, l. 603).

4. In Section 3.4, the MagnaProbe snow depth measurements along the transects during MOSAiC are described but I cannot see where they are used in this study. In Section 5.1, the authors write that they use SWE observations (described in the section on snow density 3.5) to assimilate the

Snow Model LG model. And in a second step they use the snow density evolution over time (eq. 1, snow density is a function of days since 25 Oct 2019) to assimilate snow density. The simulated snow depths are then compared to the snow depths*[2] that result from the SWE observations and this snow density evolution (snow_depth = SWE * density_water/density_snow) and NOT to any (directly) observed snow depths. Thus, I guess snow depth observations have not been used for SnowModel LG simulations or assimilations (?). Later on, the authors write that "Our simulations used observed MOSAiC snow depths ... to drive our ice growth simulations" (p. 19, l. 370-371). Here, it sounds like (directly) observed snow depths have been used, but this would contradict the statement a few lines earlier, where it says that "we performed ... SnowModel-LG simulations ... to create the snow forcing for the HIGHTSI sea ice simulations… using SWE observations" (p. 19, l. 359-362). Only in Fig. 9c (directly) observed snow depths are shown, but only the ones from coring and the "Ridge Ranch". Are the snow depths measured along the transects with MagnaProbes shown or used anywhere?
Another aspect that is unclear to me: The SWE observations are introduced in section 3.5 (‘Snow density’) as being measured in the snow pits. Figures 7, 8 and 9 show time series for the three ‘transects’ (Nloop, Sloop, Runway). On page 6 the locations of the snow pits are given (l. 128 ff). Nloop and Runway are among these. Did you use only SWE measurements located at these transects or all of them? For Sloop, did you take the closest ones (temporally and/or spatially) or all the measurements at any time on level and ponded SYI? Wouldn’t it be interesting to see how the snow depths measured along the transects compare with the simulations and the SWE-based snow depth estimations? Or maybe the snow depth observations along the transects ARE used somewhere but I cannot find it…
*[2] It is confusing that the snow depths used in this context are called ‘snow depth observations’, as they are rather ‘snow depth estimates based on SWE observations’, especially because snow depth observations have actually been collected, too.

p. 8, l. 161: analysis of (Boisvert et al., 2018) --> remove parentheses
p. 14, l. 272: So far, mainly "Central Observatory" was used, here you use "CO" + "These periods and marked...": and -> are?
p. 15, eq. (3): In the equation "M" is used, while in the text "S_M" is used.
p.25, l. 511-512: "... led to younger and thinner sea ice thicknesses ... equal to that of the oldest and thickness ice types" -> still does not make sense to me, did you mean "oldest and thickest ice type"?