Answers Reviewer 1

The article investigates potential sources of error in sea ice thickness retrievals from snow depth and sea ice and water densities. It is an interesting investigation that draws on several approaches and datasets, but sometimes further discussion is needed about the selection, benefits and limitations of these to provide a full analysis and warrant the conclusions drawn.

There is a particularly interesting discussion of the water density variability and uncertainties which could be given higher visibility in the paper.

We Thank the reviewer for their insight full comments. For a detailed answer please find the answers under each remark.

Specific comments (line #)

8: “This study, evaluates” remove the comma

11: “The evaluation shows that the model derived snow thickness, sea ice density and water density compare better to observations than the associated parameters used in the CryoSat-2 sea ice thickness product.” Please briefly outline which observations are used for comparison.

Clarifying sentence was added beginning line 1.

12: “used in the CryoSat-2 sea ice thickness product” please explain which product (full information)

A sentence was added to specify which product is meant. More details can be found in the methods section 2.2.

72: . ‘It is known that the surface salinity varies’ – it would be helpful to clarify how deep “surface” is in this context

We specified how the surface is defined in line 112 in the methods.

76-77: “This study aims at analyzing how on average the geographical variability of sea ice density, snow thickness and water density influences the derived SIT.” What is meant here by “on average”?

Details of the averaging are included in the methods, section 2.5

78-79: “we find that the model sea ice density from Sievers et al. (2023) is not varying enough” do you mean you find in this study, or you found in Sievers et al. (2023)?

The sentence was reformulated in the revised manuscript.

79 – end of Introduction: much of the material here should be moved further into the paper. It is describing the methods in some detail, and even findings (I think, though not entirely clear – see previous comment) of the study, rather than introducing the topic and motivation.

We understand the comment, but choose to leave this section in the introduction. This section was added after the first review to clarify why we are using the data set we are using. This was in the first version of the paper only mentioned in the method and discussion section.

103: Section 2.1 Please clarify what is being modelled and for what purpose

The geophysical system of the ocean and ice are modeled.

104-106: a very brief (~ one sentence) introduction to NEMO and CICE would help here

A sentence was added aiming at addressing both this and the previous comment (line 107-111).

120: seems a word is missing (is it “set”?) in “and is to 882kgm−3 in C6N4”

116: “two parameterization” – should parameterization be plural?

144: There are several snow depth products (see e.g. Zhou et al. (2021)) so please explain the benefits of Garnier al. (2021) and any potential limitations or drawbacks, indicating why this was chosen.

See edited text in line 155 to 159.

174: is it also assumed to be at the freezing point?

It is unclear to us what the reviewer refers to.

Figure 1: what do the stars mean vs the dots? It would also be useful to briefly outline the discretization of points on the x-axis in adjacent text, and please also explain whether it is a problem that the colored areas are not spatially coincident with the datasets overlaid.

The meaning of the stars and dots was specified. What the reviewer means by the rest of this comment is not entirely clear to us. We added latitude and longitude labels to the figure. The colored areas and the stars and dots, are not suppose to be overlaying. They are simply plotted in the same figure. The data sets are discussed in the text, where the figure is referenced.

220: Section 3.1 Rather than only discussing the differences between the PDFs, please also discuss which you think are more likely to be realistic and why

Discussion of the results can be found in section 4.1 page 17

Figure 3: Is there a reason that CS2_AWI is italicized in the legend (also in Figure 4)?

This is an artifact of the plotting routine.

254 - 256 : Is it reasonable to plot the Ice Bird densities vs C6N4J21 when you then state: “The close agreement between the averaged sea ice density from C6N4J21 and the IceBird data in 019, and the fact that the 2019 IceBird data were used to derive the sea ice density parameterization in C6N4J21, calls for a comparison with independent reference observations” – perhaps this dependence should be noted in the figure caption?

The caption of the figure describes the content of the figure. The interpretation is done in the text. We believe that it is important to include the 2019 data for both model runs for comparison.

Figure 4: Please comment on the unusual CS2AWI value in early Jan 2020.

Please find the added comment in line 438-440.

296: Please comment on how reliable the WOA values are and how this affects your understanding of the comparisons between this and the other datasets.

The limitations of WOA are discussed in the discussion section 4.3 page 21

319 – 320: “and the CS2AWI data, leads to” remove comma

365 – 369: Zhou et al. (2021) compare snow depths so it is not clear what results from that study are supportive? How does Wagner (2022) support this?

This sentence is the summery of the discussion above. Zhou is mentioned in line 349 to 350 and Wagener in line 370 to 372 Further we made it more clear that ERA5 is the reanalysis forcing the model.

371: “products agrees better” change to “products agree better”

381 – 382: how reliable are the spatial variability and typical values from IceBird analysis and how does this affect your assertions?

Discussion of observation data was expended at the end of the section (line 433 to 436)

399: which ice is misclassified?

420: “:The” should not be captalized

427: should “track” be “tracks”?

444: “Particularly” should be “In particular”

447: “who’s mixing parameterization were” should be “whose mixing parameterization was”

453 remove comma after though

466: change “as” to “to”

474: add Mallett et al. (2021)

479: doesn’t this always depend on the snow thickness?

Sentence was deleted.

485: move the bracket to before “see”

501 – 502: this is really interesting and should be made more prominent e.g. highlighted in the abstract.

Sentence added in Abstract.

525: specify which observations

543: Figure numbers appear as “??”

575: “s” is missing from “interest”

588: the URL is not correct in Feltham (2006) reference

Answers Reviewer 2

The manuscript has improved considerably. I am still skeptical about the large variability of the water density in the Arctic Ocean in the model (the model “mixed” layer is too deep) and I don’t think that the WOA climatology can be used as an observational “truth”. See comment below. Ocean salinity (CTD) measurements in the Arctic Ocean are really sparse and are mostly done during summer.

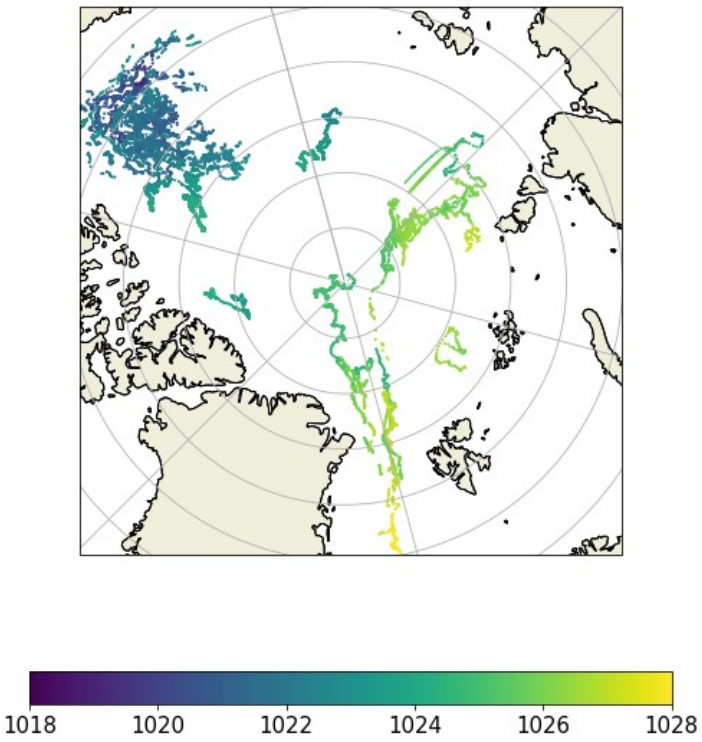
We Thank the reviewer for their insight full comments.
Since it was not clear, we added a sentence in line 183 specifying that I only used the month October to March to calculate the WOA mean.

Some specific comments:
Around L37: The parameters affecting the hydrostatic balance of the floe is not the only source of uncertainty when deriving the ice thickness from the radar freeboard. This could be mentioned here with a reference or two.

We considered the comment, but refrained from adding any changes to the manuscript since it would have added no additional information relevant to this study and, in our opinion, would have interrupted the flow of the introduction.

L255: How many data points is the WOA climatology in the Arctic ocean based on? Please include a description of the data density in the WOA climatology.

We could not find a precise figure of amount of observations, for the month we used, but we double checked the ice tethered profiles (Toole et al. 2016) available in the month October to March. ITPs are part of the version of the WOA we used and therefore the WOA in the Arctic must consist of more than this observations. In the month October to March there are a total of 44 ITPs active with a total of 26669 surface observations (observations above 10m). Graph showing density from ITPs for locations attached below:



Sources:
Toole, JM, RA Krishfield, JK O'Brien, A Houk, ST Cole, and the Woods Hole Oceanographic Institution Ice-Tethered Profiler Program (2016), Ice-Tethered Profiler observations: Vertical profiles of temperature, salinity, oxygen, and ocean velocity from an Ice-Tethered buoy system. NOAA National Centers for Environmental Information.
Dataset, https://doi.org/10.7289/v5mw2f7x. Accessed [05.02.2024].

L1005: Names are typed with capital letters, please correct.

We could not find the names this comment refers to. Line 1005 does not exist and Line 105 includes no names typed in lower case.