The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC1
https://doi.org/10.5194/tc-2021-149-RC1, 2021

Comment on tc-2021-149

Anonymous Referee #1

Referee comment on "Retrieval and parameterisation of sea-ice bulk density from airborne multi-sensor measurements" by Arttu Jutila et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-149-RC1, 2021

This paper addresses a critical parameter of Arctic sea ice needed for the determination of sea ice thickness for satellite altimetry, and is timely considering the recent launch of ICESat-2. The results determined here are very likely to be widely used and be impactful. The authors have a tremendous data set to examine sea ice density, and the analysis is thorough and well described. The paper is well written and figures are of high quality. My comments are primarily on one point – the authors simplify all their analysis to a single empirical fit, while I believe it would be more useful to explore the variability of this relationship for different ice types/conditions in more detail. Based on what they show, I think this would be straightforward to do without too much effort. I recommend publication after my comments below are addressed.

Major comments

You went to a lot of work to examine the relationship between sea ice density and different ice types, and deformed vs level. You also indicate you looked at the relationship between density and other parameters besides ice freeboard. But in the end chose to present only a single relationship based on ice freeboard. This seems somewhat unsatisfying, given you do show that there are systematic differences and density for FYI and MYI, and it appears there may be a difference between level and deformed ice (judging from figure 4 – it would be nice to also have a plot of density vs deformation; or if just two categories, a plot of the density distribution for these two categories so the reader could tell if that was a significant difference or not). My guess is your work is going to be very highly cited and this relationship will be used for almost all future altimetric estimates of Arctic ice thickness, so this will have a big influence. It would be nice if it could either be refined a bit better, or shown that such refinement results in no significant difference. So, I would have liked to see this relationship (eq 7), presented for just FYI, and just MYI, and if possible, just deformed and just level. It would be really interesting to see if that makes any difference, or it's just within the bounds of the error. Perhaps the difference is not big enough to matter, maybe because the ice freeboard captures a lot of

the variability inherent in these different ice classes. If so, that is worth reporting, because that will save future authors from trying it, or even provide some more guidance on the kind of observations are needed to improve things more. I think this could be done with a quite modest amount of effort, since you have already identified which ice is in which class.

You also mention that for your fit you tried other parameters and they didn't have good correlations. That's good to know, but maybe provide more details? What parameters exactly, and how poor were the fits? Would a multiple regression that included more variables improve things. For example, would including ice freeboard and ice type improve it much, or not?

If indeed your relationship is the best, and trying other fancier parameterizations doesn't make much of a difference, then as I say, this is the one thing from your paper that everyone will use. In that case, maybe it is worth putting this relationship in the abstract itself? Ok, maybe interested readers shouldn't be so lazy.

Section 2.6 – you do a nice job of accounting for the uncertainties. But it seems like you are assuming they are all normally distributed here. But you noted a bias in the snow radar; maybe there are biases in the other measurements, too (e.g. a bias in the EM-bird for ridges). Did you correct the data for any of these biases so that the errors would be centered first? Another possible bias is suggested from the retrieval rates in table 1. Do you know if there is any ice types or thickness for which retrievals are less likely? I am thinking mostly of the snow radar, which I believe will get poor retrievals for thin snow, and possibly also in heavily deformed ice. This doesn't bias your data exactly, because this is excluded, but it may bias the types of ice that you measure (i.e. your data might not be an average representation for the whole Arctic, or even for your survey areas). Thus, your density fit might be biased to certain ice types. It would also be nice to have some more discussion and analysis of whether this relationship would have more error in different regions or ice types and conditions (this relates to the main points above about the simplified empirical fit).

Minor comments:

Line 3 "in the 1980s and earlier" I think reads a bit better.

Line 25 Perhaps change "Coming to the era of" to "At the start of the era of"

Line 35-44 – note that W99 was updated by Webster et al, and Blanchard-Wrigglesworth et al examined the spatial bias as well. Though not sure if these are the updates you are

referring to, but you should probably provide a cite for the updated product, and one or more of the reanalysis techniques.

Lines 50-64 – It may see obvious, but perhaps point out here that you are focusing on Arctic sea ice density. There have been a few studies that measured Antarctic sea ice density, which because of different properties may be expected to have different densities and effective densities (though they tend to span the same range as these Arctic observations).

Line 121 – How often do you get total thickness less than snow freeboard or snow depth? This is obviously a measurement error, so makes sense to exclude. But does it tell you something about your measurement error? i.e. when this happens you are getting an ice thickness error of ~100%, or, based on buoyancy, an error in ice thickness something like at least 3 times the snow freeboard. Is it possible you also have errors of this magnitude in the other direction (i.e. grossly overestimating ice thickness)?

Line 129-130 – Can you give a bit more detail? What is a typical spacing of these sea surface references? I am assuming it's pretty small so that linear interpolations between them works just fine.

Line 146 – "in snow depth"

Section 2.4 – different snow depth retrieval algorithms for ultrawideband radar have been tried, with differing results. I see you did some validation of your method and report errors, so that is good. Can you add a comment on how well your algorithm is expected to do versus others? It may be that yours works well enough for the ice type you validated against, but perhaps it might have larger errors elsewhere?

Section 2.6 I am a little confused on how these uncertainties come into the final analysis. It looks like in Table 3 and figures 6 and 8, you just use the standard deviation, so in the end these apriori uncertainties go away. I think this is actually ok if the uncertainties are normally distributed, but not if there are biases (such as in snow depth). I gather from section 4.2 that equation (4) is used to calculate the local (800m or 25 km) densities and their respective uncertainties (eq. 5 and 6). But then I assume these do not affect the values in Table 3 or the empirical fit? Or were the uncertainties explicitly used in the fitting procedure? Granted, they are quite small relative to the scatter, so I think they wouldn't affect the fit at all. I do see the discussion in section 4.2, so perhaps all that is needed is a sentence in section 2.6 to clarify how they are used.

Figure 5 – this figure is what really makes me want to see the differences in the

distribution of density for deformed vs undeformed and whether there is a statistically significant difference. I understand that you probably couldn't do it in 800 m along track averages.

Figure 8 – actually, relating to my top comment, this figure does show quite well the difference in density distributions for different ice types. I suspect that there is no statistical significant difference between the SYI and MYI distributions, but probably there is with FYI. Most of the FYI have low sea ice freeboard, so maybe the relationship is just us good if it was based on ice type? I suppose sea ice freeboard could be capturing that ice type relationship, but as I noted above, you might not get sea ice freeboard from altimetry, but you might get e.g. roughness.

Line 290-292 – This again argues for showing a relationship between density and deformation.

Equation 7 and figure 8 – you should state the uncertainty in the fit parameters in equation 7, and maybe show the confidence limits of the fit on figure 8.

Discussion/Conclusions –Your data are for April only. People will be tempted to use your relationship generally, which as you note might not be so valid (or over other areas, too). It is worth stressing this as a caution to users. Can you also speculate, based on your data and the literature, how the results might be different elsewhere or at other times? e.g. could it be that in the autumn densities might be higher because of saltier FYI and maybe less consolidated ridges? Do you think the scatter in the fit in figure 8 would capture the range of densities likely to be observed elsewhere and at other times?

Author contributions – contribution of CH is not specified.