tc-2021-149
Retrieval and parametrisation of sea-ice bulk density from airborne multi-sensor measurements

An item-by-item response to referees' comments

Arttu Jutila et al.

8th November, 2021

In the following, you can find the *referee comments in italic bullet points* and our responses in regular font in an item-by-item fashion.

Anonymous Referee #1

- *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.*

On behalf of all authors, I would like to thank the referee for their time and effort in reviewing our manuscript and for the constructive feedback, which we have considered carefully. We are very grateful for the very positive evaluation by the referee, and we are confident that with the referee's help the manuscript will improve. I am hopeful that we are able to meet their expectations and eliminate all their concerns.

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.*

Our objective, perhaps not communicated well enough, was to find a simple, single-variable, functional relationship between sea-ice bulk density, including deformed ice, and a parameter observable from space. As this is the first study with this data set, we wanted to keep the parametrisation simple and provide one, good, all-around tool instead of up to half a dozen equations depending on the ice type (FYI/SYI/MYI/level/deformed). A single-variable parametrisation is directly comparable to the existing density parametrisations in Ackley et al. (1976) and Kovacs (1997) (line 369–373). To address the issue raised by the referee, we will be more explicit about our objective at the end of the introduction section when describing it in the revised manuscript. Further investigations on possible relationships with other or multiple parameters and more advanced parametrisations will be a topic of a future study, as proposed at the end of discussion (line 377–380). The full resolution data will be made public to the scientific community together with this paper.

The referee asks for a figure of density vs. deformation or density distributions of level and deformed ice. However, we cannot provide this with the along-track averaged (800 m) data, where such a length scale can already include a mixture of ice types and data points are not classified as level and deformed anymore. That is also evident from Fig. 5, to which we think the referee is referring instead of Fig. 4 (backtracking). We would be extremely cautious to use the non-averaged data to analyse this, as the assumption of isostatic equilibrium may not be valid locally at the nominal resolution, especially for deformed ice. However, to answer the referee's request, we provide such plots only for this response document in Fig. 1 below. It can be seen that the distributions of level and deformed ice densities are similar with mean values differing 5–6 kg m^{-3}, whereas deformation increases the spread of values only in sea-ice freeboard.

The referee also asks to see the parametrisation of Eq. (7) for just FYI and just MYI, which we provide here in Fig. 2 of this document below. It is clear that the parametrisation split to different ice types leads to coefficient of determination (R^2) values worse than for the complete data set. We will report this in the revised manuscript but keep the single parametrisation for the complete data set.

- *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?*

The other available parameters were total thickness, sea-ice thickness, snow depth, snow freeboard, and surface temperature, each including also their minimum, maximum, and standard deviation values. None of them showed significant linear, exponential, or power law dependency to density. For example, Eq. (2) would imply also linear anti-correlation between density and snow depth, but the result was an obscure cloud of data points with a correlation coefficient of only −0.33 compared to −0.62 of sea-ice freeboard.

We agree with the referee that multiple regression has potential to improve the parametrisation. Therefore, at the very end of the discussion section (line 377–380), we propose future studies to apply multi-variable approaches and machine learning to explain more of the variability in density.

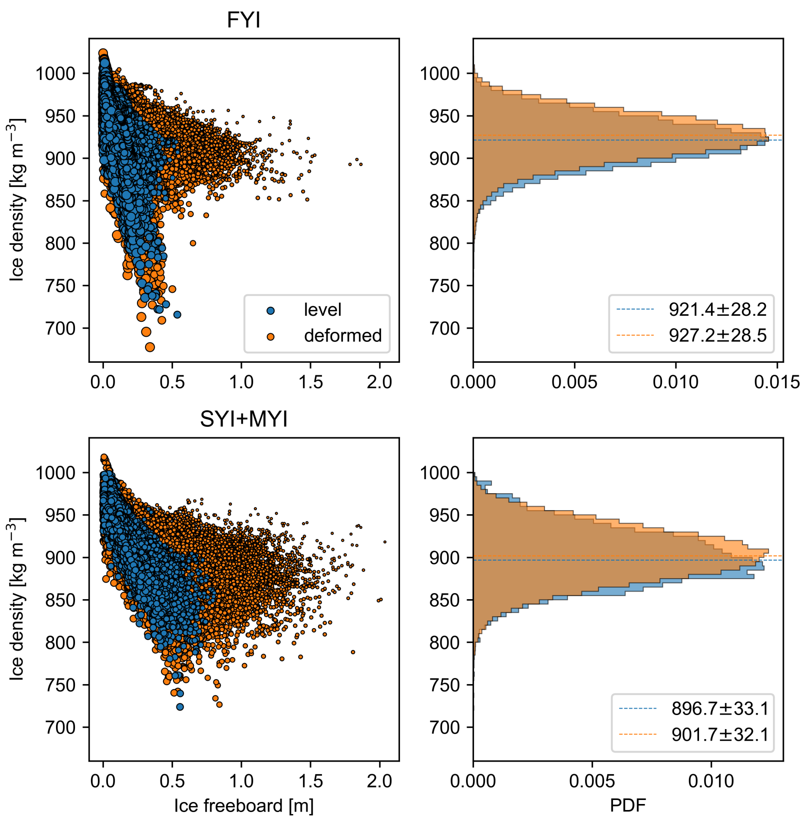

Figure 1: The nominal resolution, i.e., non-averaged, data showing the density distributions of level (blue) and deformed (orange) ice in the FYI (top) and old ice (bottom) regimes. The horizontal dashed lines over the histograms show the respective mean values indicated in the legends in the lower right corners with standard deviation. PDF stands for probability density function.

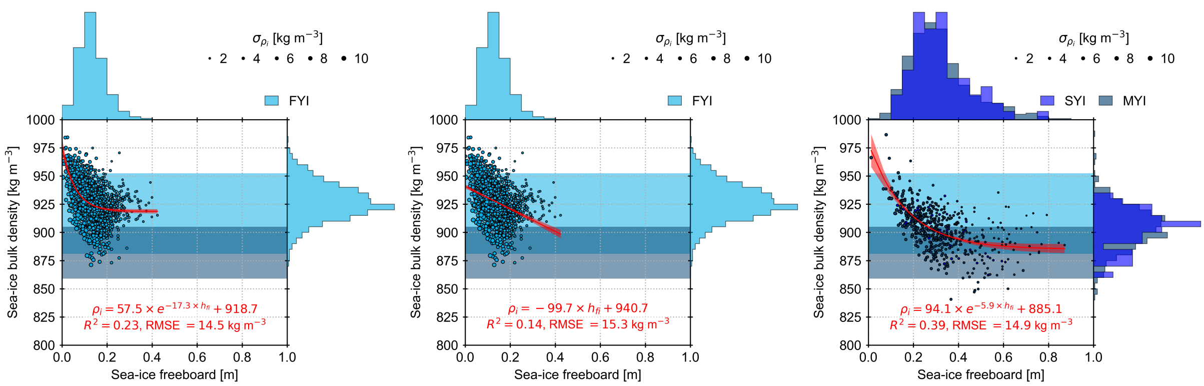

Figure 2: Density parametrisation of Eq. (7) and Fig. 8 of the manuscript split into different ice types and fits: FYI exponential (left) and linear (middle) fit and old ice (SYI+MYI) exponential fit (right).

- *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.*

While we agree that the parametrisation is a key result of the paper, putting it in the abstract would require adding also explanations of the variables used as required by the journal's instructions. Our current abstract is already at the journal's upper limit of length and therefore, we refrain from adding it to the abstract. As also pointed out by the referee, we hope that interested readers can find it from the text or in Fig. 8.

- *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).*

We did not correct the data for any biases but assumed that the errors are normally distributed and uncorrelated. The mean bias of 0.86 cm in the snow radar is below the sensor resolution and within the accuracy of the ground truth data, on which this value is based, and thus, negligible (Jutila et al., 2021). Regarding the snow radar retrievals, please see our response below to the referee comment about Section 2.4.

Regarding the retrieval rates of Table 1, data gaps exist due to the following main reasons:

- EM-Bird (total thickness)
  - Brief ascents every 15-20 minutes to monitor the sensor drift during post-processing (line 116)
- ALS (snow freeboard)
  - No freeboard conversion before (after) first (last) lead tie point, especially over landfast sea-ice
  - For example, the beginning of the survey on 2 April 2019 is over the landfast sea-ice of the Nansen Sound.
- Snow Radar (snow depth)
  - EM-Bird calibration ascents
  - Anomalously low retrieval rate on 7 April 2019 is due to a momentary malfunction of the instrument
  - Characteristics of the snow depth retrieval algorithm, see below our response to the referee comment about Section 2.4

Therefore, we agree with the referee that our density fit is somewhat biased as it does not include landfast sea-ice. Other than that no specific ice type is excluded in our analysis.

Minor comments:

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

Thank you for the suggestion. We agree and it will be corrected for the revised manuscript.

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

We will replace it with "At the beginning of the era of" to avoid repeating the word "start" later in the sentence.

- *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.*

Since the focus of this paper is not on the different snow depth products, we decided to refer to the inter-comparison study of Zhou et al. (2021) to avoid adding considerable length to the introduction by including a comprehensive list of citations. If the referee means the snow reconstruction study of Blanchard-Wrigglesworth et al. (2018), it is also included in the paper of Zhou et al. We do not see it fair to mention and thus highlight only one updated product or reanalysis technique study.

To address this issue, we will move the following sentence earlier in the text and add more explicit expressions to lead the reader to the descriptions first: "Descriptions of the different snow depth products currently available can be found in the inter-comparison study of Zhou et al. (2021, **and references therein**) **and in broad outlines below**."

We do not agree with the referee's note about W99 being updated by Webster et al., presumably referring to the paper Webster et al. (2014) describing the interdecadal changes in snow depth. To our understanding, Webster et al. studied the change in regional springtime snow depth by comparing W99 and airborne snow radar measurements from Operation IceBridge 2009–2013 interpolated using the same two-dimensional quadratic method as in W99. They provide estimates of how much the snow cover has been thinning, partly in keeping with the results of Kurtz and Farrell (2011), but that is hardly a snow depth product in the same sense as the ones included in Zhou et al.

- *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).*

While the review articles on sea-ice density we refer to (Timco and Frederking, 1996; Timco and Weeks, 2010) do not distinguish between Arctic and Antarctic sea ice, we agree with the referee that it is important to point out that majority of the measurements originate from Arctic sites. We will add the following sentence to the end of the respective paragraph: "It has to be noted that majority of the density measurements originate from Arctic sites, which is the case also in our study. Different properties and processes of Antarctic sea ice could lead to different densities values."

In connection to this, we noticed that the abstract does not mention the geographical location of our study. It will be added to the revised version of the manuscript (see below the section Additional corrections by authors).

- *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)?*

Total thickness less than snow freeboard or snow depth occurs only when total thickness is close to or below 0.1 m, i.e., the accuracy of the EM-Bird instrument, or when calculated sea-ice freeboard is negative. These data points comprise less than 0.3 % of the entire data set, but they are disregarded through filtering before analysis. In general, these measurements have the largest relative uncertainties. We do not think that our approach could lead to overestimated ice thickness nor have we observed any such indications.

- *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.*

The spacing between leads is diverse and depends on the ice regime. The typical spacing is up to 10 km. Over FYI in the Beaufort Sea and Chukchi Sea, spacing is often below 10 km. In contrast, the MYI north of the Canadian Archipelago is densely packed and the spacing can be more than 30 km.

We apply a spline interpolation between the tie points. Data before the first lead detection and after the last lead detection during a survey flight are discarded to avoid extrapolation errors.

- *Line 146 – "in snow depth"*

Typically, radar range resolution is given in relation to a medium in which the radar wave is propagating, such as, free-space/air or snow. Here, we refer to range resolution in the said medium, snow, and not in the measured parameter. To make it clearer, we will change the expression "range resolution was 1.14 cm in snow" to "range resolution in snow was 1.14 cm".

- *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?*

As we state on lines 150–152, the workflow and the retrieval algorithm are described in detail in our recent publication Jutila et al. (2021). There we compared our peakiness algorithm against the wavelet-based algorithm of Newman et al. (2014). We found that due to the characteristics of the wavelet method (detected interfaces are both on the leading edge of the radar waveform) it was prone to both overestimation and underestimation of snow depth compared to our algorithm and in situ data. To our knowledge, other retrieval algorithms are not publicly available as open source for comparison.

Another aspect to consider, in addition to the retrieval algorithm, is the CReSIS Snow Radar itself. Since its first deployment on the Operation IceBridge campaign in 2009, the radar has been continuously developed (e.g., Yan et al., 2017; Arnold et al., 2020; MacGregor et al., 2021). The lack of stability in radar properties and design over the years has somewhat hampered developing algorithms but in turn revealed differences in radar return power and sidelobe levels between campaigns (Kurtz et al., 2013; Kwok and Haas, 2015; Kwok et al., 2017). What makes the comparison against our study difficult is the fact that our radar version is similar to the latest one used on Operation IceBridge, but that specific version is not used anywhere else.

We agree with the referee that our retrieval algorithm was validated only for a specific ice type, namely level and landfast FYI. Increase of errors could be expected over rougher sea-ice surfaces, but we are confident that our approach is valid also in such sea-ice environments for two reasons. First, the low altitude of our IceBird surveys results in a radar footprint with an approximately 2 m diameter that is only 3–7 % in size of the earlier high-altitude acquisitions (lines 146–149). This considerably smaller radar footprint size decreases the amount and possibility of off-nadir reflections that could lead to potentially erroneous snow depth retrievals. Second, we require that a surface topography ("roughness") estimate is $h_{topo} \leq 0.5$ m within the radar footprint to filter out retrievals over very rough surfaces where our method is not validated (lines 156–159). In a single survey, less than 2 % of the values are disregarded due to this threshold. In a small area such as the radar footprint, surfaces rougher than the used threshold value correspond to sharp sails of pressure ridges, which are often snow-free due to wind erosion.

Further validation opportunities against in situ measurements on other ice types are not available to this date, because they were not realised due to poor weather (campaign in 2019) or cancelled altogether due to the global COVID-19 pandemic (campaign in 2021). However, we will pursue further validation opportunities over a range of different sea-ice surfaces in future campaigns.

- *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.*

We think this confusion stems from us using the term "inverse-variance weighted mean" to describe the along-track averaging method, when we in fact mean that we used the inverse of squared individual *uncertainties* as weights. We apologise for this mix-up of terms and we will make appropriate corrections for the revised manuscript. All mean sea-ice density values presented in this study — averaged over a length scale, survey, or ice type — are weighted with uncertainties according to Eqs. (5) and (6). Therefore, the uncertainty information is not lost but included in the averaging.

As explained in the figure caption, Fig. 6 features interquartile range, which is not equal to standard deviation. Fig. 8 shows local (800 m, Eq. (6)) uncertainties as explained in the figure caption. In Table 3, we report standard deviation, as explained in the table caption, to enable direct comparison with the values in Alexandrov et al. (2010). The local (800 m) densities have respective uncertainty values that are used for the uncertainty-weighted averaging of the survey or the entire ice type. Therefore, sea-ice density uncertainties are used in calculating the uncertainty-weighted means according to Eq. (5) again and not lost. Maybe that is the missing sentence from Section 2.6. More confusion is perhaps created as the ice type averaged density mentioned in Table 3 is not an arithmetic mean calculated from the individual survey means but again an uncertainty-weighted mean of all 800 m averaged density measurements of the ice type in question. The uncertainties of the local (800 m) densities are not used in finding the exponential parametrisation, because they did not significantly affect the fit as the referee already pointed out.

- *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.*

That is correct, we don't distinguish between level and deformed ice after averaging over the 800 m length scale. Please see our response to the referee's major comment above.

- *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.*

Yes, Fig. 8 shows density distributions for FYI, SYI, and MYI. We don't understand the referee's comment about a relationship based on ice type, which is not a continuous parameter and does not provide a functional relationship. We provide average density estimates based on ice type in Table 3. Lastly, while not all altimeters are able to measure sea-ice freeboard, it can be derived from Ku band radar altimeters, which can be found onboard the current CryoSat-2 or the future CRISTAL satellite missions. In contrast, laser altimeters, such as onboard the current ICESat-2 satellite, are not able to measure sea-ice freeboard but snow freeboard. For them, surface roughness could potentially reveal a relationship to density, especially between level and deformed ice (line 379–380).

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

We cannot show the relationship with the 800 m averaged data, as the referee already noted above.

Before averaging, the assumption of isostatic equilibrium may not be valid. Please see our response to the referee's major comment above.

- *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.*

We agree with the referee and the requested information will be added to the revised manuscript. The parameters of the exponential fit $\rho_i = a \times e^{b \times h_{fi}} + c$ are the following: $a = 72.0 \pm 2.4$ kg m^{-3}, $b = -3.7 \pm 0.4$ m^{-1}, and $c = 881.8 \pm 3.1$ kg m^{-3}. The 95 % confidence band of the fit is updated and shown in Fig. 3 of this response document below.

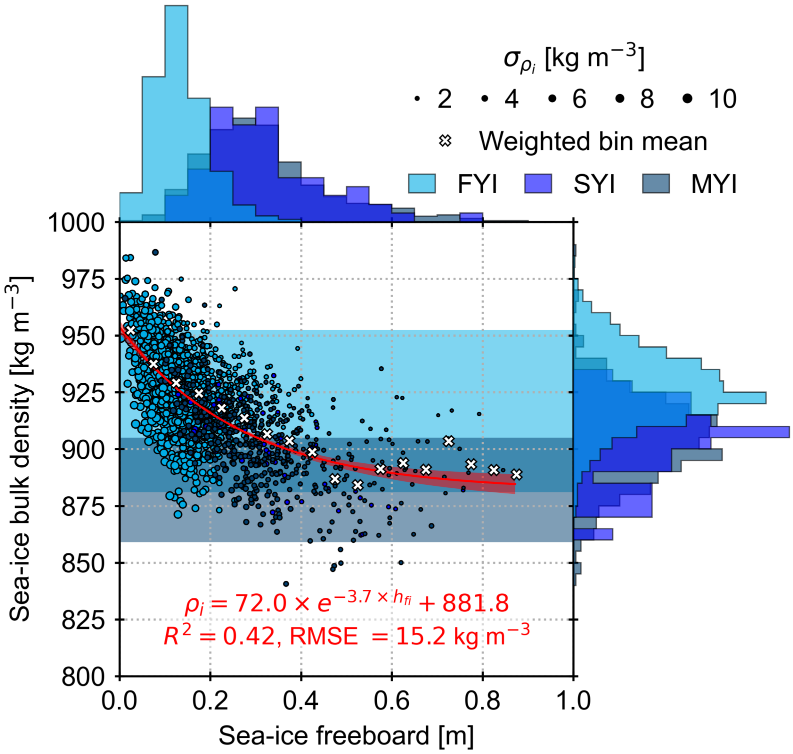

Figure 3: Figure 8 of the manuscript updated with the 95 % confidence band of the fit (red shading).

- *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?*

Thank you for raising this valid point. We agree with the referee that our data originates only from April and it is also regionally restricted. We discuss it on lines 330–331 together with other limitations and uncertainties and we will stress it further where appropriate in the revised manuscript. We further agree that it is worthwhile to emphasise, as the currently widely used density values derived in Alexandrov et al. (2010) share the same limitations. Their FYI measurements come from the airborne Sever expeditions that "took place mainly from mid March to early May, when landing on ice floes was possible. Thus, the data represent late winter conditions before melting starts." In addition, their FYI measurements are from level ice only. Despite these limitations, they are currently widely in use by various satellite altimetry algorithms across seasons and the Arctic regions (e.g., Sallila et al., 2019).

Regarding the speculation on differing results elsewhere and at other times, we share the referees thoughts on denser ice in newly formed and more saline FYI as well as unconsolidated ridges. Timco and Frederking (1996) report a range of density values of 720–940 kg m^{-3} that includes also our observations. Compared to Timco and Frederking, our density range is missing the lowest values that originate perhaps from brine-drained or rotten summer sea-ice measurements. However, we would prefer to base our discussion on actual measurements and results.

- *Author contributions – contribution of CH is not specified.*

We will include the contribution of CH explicitly as follows: "AJ [- -] prepared the manuscript with input from SH, RR, LvA, TK, and CH."

Additional corrections by authors

In the abstract, we will add a description of the study area to complement the study period: "Our sea-ice density measurements are based on over 3000 km of high-resolution collocated airborne sea-ice and snow thickness and freeboard measurements **in the western Arctic Ocean** in 2017 and 2019."

Data availability

The data products related to this paper have been prepared by PANGAEA and they are ready to be published together with the paper. Corresponding DOIs will be updated to the revised manuscript.

- Jutila, A., Hendricks, S., Ricker, R., von Albedyll, L., Haas, C.: Airborne sea ice parameters during the PAMARCMIP2017 campaign in the Arctic Ocean, Version 1, *PANGAEA*, https://doi.pangaea.de/10.1594/PANGAEA.933883, in review, 2021.
- Jutila, A., Hendricks, S., Ricker, R., von Albedyll, L., Haas, C.: Airborne sea ice parameters during the IceBird Winter 2019 campaign in the Arctic Ocean, Version 1, *PANGAEA*, https://doi.pangaea.de/10.1594/PANGAEA.933912, in review, 2021.

References

Ackley, S. F., Hibler, W. D., Kugzruk, F. K., Kovacs, A., and Weeks, W. F.: Thickness and roughness variations of Arctic multiyear sea ice, Tech. Rep. 76-18, Cold Regions Research and Engineering Laboratory, 1976.

Alexandrov, V., Sandven, S., Wahlin, J., and Johannessen, O. M.: The relation between sea ice thickness and freeboard in the Arctic, The Cryosphere, 4, 373–380, https://doi.org/10.5194/tc-4-373-2010, 2010.

Arnold, E., Leuschen, C., Rodriguez-Morales, F., Li, J., Paden, J., Hale, R., and Keshmiri, S.: CReSIS airborne radars and platforms for ice and snow sounding, Annals of Glaciology, 61, 58–67, https://doi.org/10.1017/AOG.2019.37, 2020.

Blanchard-Wrigglesworth, E., Webster, M. A., Farrell, S. L., and Bitz, C. M.: Reconstruction of Snow on Arctic Sea Ice, Journal of Geophysical Research: Oceans, 123, 3588–3602, https://doi.org/10.1002/2017JC013364, 2018.

Jutila, A., King, J., Paden, J., Ricker, R., Hendricks, S., Polashenski, C., Helm, V., Binder, T., and Haas, C.: High-Resolution Snow Depth on Arctic Sea Ice From Low-Altitude Airborne Microwave Radar Data, IEEE Transactions on Geoscience and Remote Sensing, pp. 1–16, https://doi.org/10.1109/TGRS.2021.3063756, 2021.

Kovacs, A.: Estimating the full-scale flexural and compressive strength of first-year sea ice, Journal of Geophysical Research: Oceans, 102, 8681–8689, https://doi.org/10.1029/96JC02738, 1997.

Kurtz, N. T. and Farrell, S. L.: Large-scale surveys of snow depth on Arctic sea ice from Operation IceBridge, Geophysical Research Letters, 38, L20 505, https://doi.org/10.1029/2011GL049216, 2011.

Kurtz, N. T., Farrell, S. L., Studinger, M., Galin, N., Harbeck, J. P., Lindsay, R., Onana, V. D., Panzer, B., and Sonntag, J. G.: Sea ice thickness, freeboard, and snow depth products from Operation IceBridge airborne data, The Cryosphere, 7, 1035–1056, https://doi.org/10.5194/tc-7-1035-2013, 2013.

Kwok, R. and Haas, C.: Effects of radar side-lobes on snow depth retrievals from Operation IceBridge, Journal of Glaciology, 61, 576–584, https://doi.org/10.3189/2015JoG14J229, 2015.

Kwok, R., Kurtz, N. T., Brucker, L., Ivanoff, A., Newman, T., Farrell, S. L., King, J., Howell, S., Webster, M. A., Paden, J., Leuschen, C., MacGregor, J. A., Richter-Menge, J., Harbeck, J., and Tschudi, M.: Intercomparison of snow depth retrievals over Arctic sea ice from radar data acquired by Operation IceBridge, The Cryosphere, 11, 2571–2593, https://doi.org/10.5194/tc-11-2571-2017, 2017.

MacGregor, J. A., Boisvert, L. N., Medley, B., Petty, A. A., Harbeck, J. P., Bell, R. E., Blair, J. B., Blanchard-Wrigglesworth, E., Buckley, E. M., Christoffersen, M. S., Cochran, J. R., Csathó, B. M., De Marco, E. L., Dominguez, R. T., Fahnestock, M. A., Farrell, S. L., Gogineni, S. P., Greenbaum, J. S., Hansen, C. M., Hofton, M. A., Holt, J. W., Jezek, K. C., Koenig, L. S., Kurtz, N. T., Kwok, R., Larsen, C. F., Leuschen, C. J., Locke, C. D., Manizade, S. S., Martin, S., Neumann, T. A., Nowicki, S. M., Paden, J. D., Richter-Menge, J. A., Rignot, E. J., Rodríguez-Morales, F., Siegfried, M. R., Smith, B. E., Sonntag, J. G., Studinger, M., Tinto, K. J., Truffer, M., Wagner, T. P., Woods, J. E., Young, D. A., and Yungel, J. K.: The scientific legacy of NASA's Operation IceBridge, Reviews of Geophysics, 59, e2020RG000 712, https://doi.org/10.1029/2020RG000712, 2021.

Newman, T., Farrell, S. L., Richter-Menge, J., Connor, L. N., Kurtz, N. T., Elder, B. C., and McAdoo, D.: Assessment of radar-derived snow depth over Arctic sea ice, J. Geophys. Res. Ocean., 119, 8578–8602, https://doi.org/10.1002/2014JC010284, 2014.

Sallila, H., Farrell, S. L., McCurry, J., and Rinne, E.: Assessment of contemporary satellite sea ice thickness products for Arctic sea ice, The Cryosphere, 13, 1187–1213, https://doi.org/10.5194/tc-13-1187-2019, 2019.

Timco, G. W. and Frederking, R. M.: A review of sea ice density, Cold Regions Science and Technology, 24, 1–6, https://doi.org/10.1016/0165-232X(95)00007-X, 1996.

Timco, G. W. and Weeks, W. F.: A review of the engineering properties of sea ice, Cold Regions Science and Technology, 60, 107–129, https://doi.org/10.1016/j.coldregions.2009.10.003, 2010.

Webster, M. A., Rigor, I. G., Nghiem, S. V., Kurtz, N. T., Farrell, S. L., Perovich, D. K., and Sturm, M.: Interdecadal changes in snow depth on Arctic sea ice, Journal of Geophysical Research: Oceans, 119, 5395–5406, https://doi.org/10.1002/2014JC009985, 2014.

Yan, J.-B., Gogineni, S., Rodriguez-Morales, F., Gomez-Garcia, D., Paden, J., Li, J., Leuschen, C. J., Braaten, D. A., Richter-Menge, J. A., Farrell, S. L., Brozena, J., and Hale, R. D.: Airborne Measurements of Snow Thickness: Using ultrawide-band frequency-modulated-continuous-wave radars, IEEE Geoscience and Remote Sensing Magazine, 5, 57–76, https://doi.org/10.1109/MGRS.2017.2663325, 2017.

Zhou, L., Stroeve, J., Xu, S., Petty, A., Tilling, R., Winstrup, M., Rostosky, P., Lawrence, I. R., Liston, G. E., Ridout, A., Tsamados, M., and Nandan, V.: Inter-comparison of snow depth over Arctic sea ice from reanalysis reconstructions and satellite retrieval, The Cryosphere, 15, 345–367, https://doi.org/10.5194/tc-15-345-2021, 2021.