

Reviewer #1, Dr Ellen Buckley

General Comments

This work presented in this manuscript addresses important concepts and questions in sea ice altimetry. The methodology is described clearly and with reproducible details. However, there are some points in the results and discussion that are confusing and seems to be drawing conclusions from very weak relationships. I also think there is too much confidence put into ICESat-2 as the “truth” when summer sea ice segments have not been validated. Generally, the use of the phrase “CryoSat-2 underestimates ICESat-2 heights” suggests that ICESat-2 is right and CryoSat-2 is wrong. Although some limitations of the study are mentioned, I think there are significant assumptions that need to be addressed and further discussed. There are specific comments in the next section. Overall though I think it is an important study and this line of work needs to be further investigated.

Thank you for your comments. We agree that more work needs to be done in this area. However as highlighted and discussed in our work, we are limited by the lack of external validation data and understanding of the satellite’s performance over summer sea ice. We agree that our language implied that we have put too much confidence in ICESat-2 heights, and have amended that (L 131, 490, 497). We have also added more to the discussion highlighting the potential biases in ICESat-2.

Specific Comments

throughout: check in text citations- a lot of incorrect punctuation

We have checked the citations.

50: is no snow always the case? in all regions? is there a reference for this? also what do you define as ‘mid-summer’ here? I see you address the snow assumption in line 124, perhaps move up.

We have not included the line ‘which is the case in mid-summer months’ to remove any ambiguity

56: is SAMOSA an acronym? if so, define. I see it is defined in line 141, move up to first instance.

Done

62: ‘likely often’ is redundant and vague. consider rewording

Removed ‘often’

78: newer studies say 11 m footprint. see:

Lori A. Magruder, Kelly Brunt, Thomas Neumann, et al. Passive ground-based optical techniques for monitoring the on-orbit ICESat-2 altimeter geolocation and footprint

diameter. *ESS Open Archive*. October 27, 2020.

DOI: 10.1002/essoar.10504571.1

[Thank you for highlighting this reference](#)

80-87ish: consider including these studies:

Buckley, E. M., Farrell, S. L., Herzfeld, U. C., Webster, M. A., Trantow, T., Baney, O. N., Duncan, K. A., Han, H., and Lawson, M.: Observing the Evolution of Summer Melt on Multiyear Sea Ice with ICESat-2 and Sentinel-2, *EGUsphere* [preprint], <https://doi.org/10.5194/egusphere-2023-189>, 2023.

Herzfeld, Ute; Trantow, Thomas; Han, Huilin; Buckley, Ellen; Farrell, Sinead; Lawson, Matthew (2022): Automated Detection and Depth Determination of Melt Ponds on Sea Ice in ICESat-2 ATLAS Data — The Density-Dimension Algorithm for Bifurcating Sea-Ice Reflectors (DDA-bifurcate-seaice). *TechRxiv*. Preprint. <https://doi.org/10.36227/techrxiv.21300153.v1>

[Thank you we have now considered and included these references.](#)

95: what about ICESat-2 Summer Sea Ice Campaign- cryo2ice underflights included in this campaign

Data from the 2022 ICESat-2 Summer Sea Ice Campaign were not yet available when the work for this paper was completed. We now see the airborne LVIS observations are available, but without the data from complimentary instruments. Analysing the CryoSat-2 and ICESat-2 summer freeboards against these 2022 airborne data will be the subject of our future research.

100: regarding error propagation, consider discussing and referencing this:

Giles, K. A., Laxon, S. W., Wingham, D. J., Wallis, D. W., Krabill, W. B., Leuschen, C. J., ... & Raney, R. K. (2007). Combined airborne laser and radar altimeter measurements over the Fram Strait in May 2002. *Remote Sensing of Environment*, 111(2-3), 182-194.

We did not include any error propagation in this study as in the reference, as this is mostly related to the conversion from freeboard to ice thickness, which we do not do.

135: “they”- who is they

[Amended](#)

151-160: this is confusing. the atl07 product does not sample 0.7 m along track. I think here you are mixing up atl07 and alt03 products. describe one at a time: ICESat-2 sampling, then what atl03 is, then how atl07 is created. where does 6 m come from? something you calculated? if not reference. seems very short. see reference: Kwok, R., Markus, T., Kurtz, N. T., Petty, A. A., Neumann, T. A., Farrell, S. L., et al. (2019). Surface height and sea ice freeboard of the Arctic Ocean from ICESat-2: Characteristics and early results. *Journal of Geophysical Research: Oceans*, 124, 6942– 6959. <https://doi.org/10.1029/2019JC015486>

“The length of the strong beam height segments (L_s) varies between ~10 and 200 m, while those from the weak beams are between ~40 and 800 m.”

Yes, you are correct, we meant the atl03 product. The 6 m is also incorrect in this statement, and we have amended to be ~30 m and ~75 m for the strong and weak beams respectively, based on previous studies (Tilling et al. 2020)

153: same as line 78 comment (11 m)

Amended

165: no hyperlinks within text, move to references

Done

163-170: would this be better in a table form?

We did not include a table here as it would be an incomplete copy of the table in Bagnardi et al. (2021). It is better to refer the reader to that table.

175: does interpolating the MSS introduce new errors?

That’s a good question, as yes it would introduce small but negligible error.

190: typo “known [to] classify significantly [fewer]”

Done

191: do you think you need to define what snagging is or is that a well enough known term?

We have modified the sentence to ‘by highly reflective melt ponds causing off-nadir snagging’ to make it easier to understand.

192: flow - -> floe

Done

206: no links within text- should just be in data accessibility section

Done

215: awkward sentence, reword

Done

216: “typically..” what do you mean, what percentage of the time, did you not use any CRYO2ICE tracks then?

The orbits for CRYO2ICE have a greater than 3 hour time difference. Therefore, we did not use any of the tracks. We have amended this sentence.

223: again, where is this 6 m value from? reference, or mention that you calculated that.

Amended

249: be careful of the use of the word ‘underestimate’ (here and throughout this paragraph) I don’t think it’s an estimate of ICESat-2 you are producing and ICESat-2 isn’t the ‘truth’

We agree and have changed to ‘lower than’, here and elsewhere throughout the manuscript

264 Figure 3: note that this is height not thickness in fig caption or axis label.

Done

279: figure 4, can you add total count of data points to the statistics displayed. also you need the y axis values for a)

Done

283: less --> fewer

Done

300: is there really more variability? the distance between the MAD lines seem almost steady. if there is in fact more variability, quantify.

Yes, there is more variability with the CryoSat-2 backscatter having a MAD of 3.2 dB for points from the 9th July to 16th August compared to 7.7 dB before 9th July and after 16th August, while the ICESat-2 photon rate has a MAD of 1.4 for points between the 9th July and 16th August and 1.9 for points before 9th July and after 16th August. This detail has been added to the text.

Deleted: point

Deleted: point

305: 18th august or 16th august (as in Figure 5)

It is the 16th August

314: figure 5 caption- describe why 9th July and 16th august are marked

Done

320: fig 6 caption. don’t the box plots also show the 50th percentile, add that: “2nd, 25th, 50th, ...” Also what does the line show? best fit?

Yes and the caption has been amended

322: misspell CryoSat-2 also in line 329

Amended

323: what do you mean ‘overwhelming’ – use the word significant (if applicable)

Amended

340: I wouldn't say "has an impact." they are related, but for example it is not clear if the altimeter difference is affected by the photon rate or if the difference is affected by the surface type which also affects the photon rate.

We agree and have amended the statement

342: I think you mean Figure 8, not 7.

Amended

343: "Here we observe a negative relationship between elevation difference and both photon rate and backscatter, inferring that ice floes with a higher coverage of surface water produce a lower height bias" – this is not what you see in figure 7 though- you see a positive relationship between backscatter and the difference.

This statement was confusing, so we have changed it to 'Here we observe that the largest elevation differences occur over rougher surfaces with relatively low photon rate and backscatter, inferring that ice floes with a lower coverage of surface water produce a larger height difference'

353: misspell ICESat-2

Amended

361: regarding relationship between backscatter and photon rate and melt pond fraction. Why don't you show it? could add another panel to figure 5 showing the melt pond fraction evolution in the central arctic.

Thank you for the suggestion, we have now added this to Figure 5, and we observe highest melt pond fractions from mid-July as expected.

366: typos in the Figure 8 caption, missing spaces, Sentinel-3

Amended

436: unit for the bias

Amended

Reviewer #2

This study compares elevation retrievals from CryoSat-2 using various retracers to elevation retrievals from ICESat-2 during periods of overlap in the summer months. The methodology is sound and I believe the authors have done a good job in the comparisons. As such, I think this is worth publishing and have only noted a few minor comments and suggestions below.

However, my main concern with the manuscript is that the results and understanding thereof may be a bit premature. As noted by reviewer Ellen Buckley, the lack of validation and knowledge with regard to the ICESat-2 summer returns represents a significant problem for interpretation of the results. Thus, it is difficult to say which retracker from CryoSat-2 is performing better, how well ICESat-2 is doing compared to CryoSat-2, or to go into much detail about physical interpretation of the results. That said, I do believe the authors have done quite well in their comparisons given the limits of our present knowledge, as such this represents an important first look at such comparisons during the summer months. It is just helpful to acknowledge the limitations accordingly, and I believe this is largely done.

We thank the reviewer for their comments. We acknowledge that these results are difficult to interpret due to the unknown nature of both measurements. We also agree that it is difficult to say which retracker is performing better, and we did not do the comparison to work this out. Instead, we use three retrackers to test the sensitivity of the bias between CS2 and IS2 as a function of the retracking method. We observe the same behaviour for all three retrackers, which gives us confidence that the bias is not *exclusively* a function of the CS2 retracking. We have further highlighted this in the introduction (L 131) and discussion (L 490, 497).

L32 I suggest citing the work of Kwok et al, 2020 here as well:
<https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2020GL088209>

Thank you for the reference, this is now included

Figure 1: I suggest stating what f_p and σ refer to in the figure caption to make it a bit more self contained.

Amended

L101: I think this ignores the fact that the 532 nm light from ICESat-2 also penetrates the water surface which leads to a presently unknown bias in the retrievals.

The laser can penetrate the melt pond surface, however the dominant signal will typically be from the reflection from the top of the ice/water (Farrell et al., 2020; Tilling et al., 2020), and this is what we assume is measured in the ATL07 data we use here. However, we have included this in our discussion on the potential biases in ICESat-2 in line 490.

L117-118: What is meant by not valid here? I believe the ATL10 freeboard heights are available for the summer months.

To our knowledge there is no reliable ATL10 freeboard heights available in the summer, as explained in Tilling et al. 2020. We have changed the word "valid" to reliable.

L155 and 223: Where does the value of 6 m come from here? I'm not sure what this is referring to.

To this has been mentioned by the first reviewer as well and we have amended the value.

L158: What is meant by normally distributed around 0 here? The difference with CryoSat-2 or with respect to each beam?

The difference is between each ICESat-2 beam. We have clarified the text in the sentence.

L183-184: Also it means that some values with cloud contamination will be included as well.

Thank you for pointing this out. Yes the data still could be contaminated by clouds, we have amended this sentence.

L185: There is a surface type flag in the products but it is likely not applicable for this study.

Yes, it is not applicable in our study

L390-398: This may be where independent validation of the ICESat-2 data is needed as raised by reviewer Ellen Buckley. There are multiple factors still unknown in the processing of summer sea ice returns from ICESat-2, and I think another factor that is not mentioned here would be how well ICESat-2 retrieves the surface height from melt ponds or if the penetration of the laser signal biases the retrievals in a way that represents a mixture of the melt pond surface and bottom.

Yes we agree, and we have strengthened this statement regarding unknown biases in the ICESat-2 data

References

Farrell, S. L., Duncan, K., Buckley, E. M., Richter-Menge, J., and Li, R.: Mapping Sea Ice Surface Topography in High Fidelity With ICESat-2. *Geophys. Res. Lett.*, 47, e2020GL090708, <https://doi.org/10.1029/2020GL090708>, 2020.

Tilling, R., Kurtz, N. T., Bagnardi, M., Petty, A. A., and Kwok, R.: Detection of melt ponds on Arctic summer sea ice from ICESat-2. *Geophys. Res. Lett.*, 47, e2020GL090644. <https://doi.org/10.1029/2020GL090644>, 2020.