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Interactive comment on "On retrieving sea ice freeboard from ICESat laser altimeter" *by* K. Khvorostovsky and P. Rampal

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Summary: Basically two methods exist to retrieve total (sea ice + snow) freeboard and subsequently sea ice thickness from ICESat laser altimeter data in the Arctic Ocean. The overall difference in sea ice thickness between the two methods used, the so-called lowest-level-elevation (LLE) and the tie point (TP) method, has been estimated as 0.42 m. The main aim of the present paper is to figure out whether this bias is based on the different freeboard retrieval methods. For this purpose the authors re-construct the methods and investi-

gate the retrieved freeboard heights as a function of different geoid models and as a function of different settings required for the two methods used. These settings basically determine how well the sea surface height (SSH) is approximated against which the freeboard is referenced.

The authors demonstrate that shorter averaging scales are beneficial. They further quantify the difference in the two approaches due to the different settings and conclude that the TP method potentially is the one suited better. The authors identify, however, that for thin sea ice the linear relationship between surface roughness and freeboard, which is used in the TP method to approximate the SSH, breaks down below a certain freeboard value. The authors then develop a method how to reduce this bias and quantify the effect of this improvement, which is valid for first-year ice.

This is an interesting and well-written paper which - even though ICESat is not in orbit anymore - helps to better understand the limitations involved in sea ice thickness retrieval using this sensor. The authors managed to convincingly demonstrate that another correction, when applied to the ICESat freeboard retrieval using the so-called Tie point method, potentially improves freeboard and hence thickness retrieval even more - and particularly for first-year ice.

To fully benefit from this improvement and to fully understand the relevance of this improvement, I feel the paper would benefit from a number of clarifications which are detailed in the general comments and in the specific comments. In particular, a more clear and more focussed description of the goals of the paper, and a revision of the first-year ice versus multiyear ice discrimination seems to be beneficial for the paper. I therefore recommend to give the authors the chance for major revisions and optimize the paper further.

General comments: 1) I have the feeling that the authors could work on the motivation and the principal aim of the paper. What are the new findings of this paper? How relevant are these? How globally applicable are these? Since ICESat-1 is not in orbit anymore one could ask whether it is worth to apply the new approach presented and what a potential user would gain from that. To me it seems as if the main improvement is 1-2 cm smaller bias for basically first-year ice (FYI). This might be not enough to trigger a potential user to switch to the new, optimal approach. One could, however, use this paper to give another, additional evidence for

how difficult it is to derive sea ice thickness from satellite laser altimetry.

2) A relatively large number of the results roots on the QuikSCAT data based discrimination between FYI and multiyear ice (MYI). The authors used a 50% MYI (or FYI) isoline to separate between both ice types. I have the feeling that the authors could enhance their results by reducing the number of mixed pixels and instead of using 50% defining FYI and MYI areas as follows: FYI: all grid cells with MYI fraction below 10%; MYI: all grid cells with MYI fraction above 90%. I could imagine that the difference in the freeboard and in the effect the authors are describing becomes even more clear in that case. And in addition the authors would concentrate their ice-type based analysis on grid cells where the dicrimination into these surface types is more reliable and the relative error contribution is smaller than when using a 50% threshold. Yes, by this the sum of the grid cells analysed (FYI plus MYI) would not be the total anymore, but I don't think that this would have a significant influence on results and interpretation.

3) The authors test two different, constant gain settings. I am wondering whether they

are

aware of the paper by Yi et al., 2011, Annals of Glaciology, where it is stated that due to

the instability in the power emitted by the GLAS a constant gain setting might not be

appropriate. Yi et al. (2011) suggest to use a variable gain setting - which was also used

by Kern and Spreen, 2015, for instance. Donghui Yi was one of the main principal scientists

working with ICESat data in the background and I doubt that there are many more who have

more experience with the nitty-gritty technical details of this sensor than Donghui.

4) All maps seem to be blurry compared to the histograms. I am wondering whether the authors

could increase the dpi for these maps. In addition, the color legends in all maps would

benefit from a title and a unit.

Specific comments: Page 1, Line 13: "by up to 15 cm". In the next line the authors give a mean and an "up to"

value. Therefore I would find it fair to see a mean value here as well.

Page 1, Line 15: Perhaps add "average" before "difference"?

Page 1, Line 19: I would not write "is very large and". Here it comes to the main message

the authors wish to give to the readership, i.e. whether the authors wish to give a list of

potential uncertainties and biases or whether the authors wish to promote their im-

provement.

Page 1, Line 23: One could argue that this statement is not so new but I am wondering

whether authors could take a look the parameters used for the freeboard-to-thickness

conversion. Doesn't the JPL approach use a specifically designed ECMWF data based snow depth

data set?

Page 2, Line 2: I am wondering whether the authors would like to weaken that "ten times" by

an "approximately". Currently this contradicts with the notion given on Page 12, lines 30-

32.

Page 2, Line 1-5: Even though the authors refer to the Zygmuntowska et al (2014) paper later

I suggest that it could be mentioned here as well.

Page 2, Line 8: I am aware of this 2 cm, but wasn't this obtained for a very ideal case of

sea ice (smooth, extremely level young ice in the Ross Ice Shelf polynya)? I am wondering

whether the authors also would like to mention the single-shot accuracy of 13.8 cm.

Page 2, Line 27: "their mean represents" I don't think this is entirely correct. If within a

ground track segment of 550 km the LLE provides 3 elevations then these are used to

approximate the SSH ... maybe by simply computing a mean in the Yi and Zwally (2009) version of the LLE but not in the version proposed by Spreen et al. (2006) and Kern and Spreen, 2015, where the SSH at that location is approximated by fitting a polygon through the elevations identified as minima.

Page 2, Lines 28/29: This statement is true for the missing leads but if the LLE is set to, e.g. 2% and there are fewer elevations than a certain predefined number (I guess 3 in Kern and Spreen, 2015; how is this solved in Yi and Zwally?), then for that specific laser shot no SSH is approximated and it will be instead estimated via interpolation from the neighboring estimates. The fact that a moving ground track segment is used here ensures that inconsistencies / jumps are smoothed along track.

Page 2, Line 34: "particular to the chosen value for ice density": Is this true? I thought that Kwok (et al.), I guess in their 2007 paper, carried out a sensitivity / uncertainty analysis and figured out that in fact the snow depth is the second most important error contribution, after the uncertainties in freeboard.

Page 3, Line 10: "is also quantified": I am wondering whether the authors could state which version of the JPL prodcuts they reproduce: the simple first version or the optimized one. It is not clear from this paragraph.

Page 3, Line 13: I suggest to add "elevation" behind "level 2"

Page 3, Lines 13-15: Please explain the meaning of "FM" and "MA" as well.

Page 3, Line 16: Is the Yi and Zwally data set also a level 2 data set? The authors could add this information.

Page 3, Lines 18-21: Please note on which grid projection these two data sets are given. While the NSIDC data set is publicly available - and may even have a DOI? - the one based on QuikSCAT seems to be an in-house (NERSC) product. Is this correct? Would it be possible to give more information about this data set? At the end this is relatively cruicial for your work because a lot of the results you give are based on the discrimination of FYI and MYI.

Page 3, Lines 24/25: This sentence is not clear to me.

Page 4, Line 2: See general comment 3

Page 4, Line 12: I suggest to write: "... level in leads we evaluate the effect of using a different geoid on the results in section 3.2" instead of using passive voice.

Page 4, Line 17/18: I sugget to write: "We evaluate and discuss the effect of ..." instead of using passive voice.

Page 4, Lines 16 & 25: I am wondering whether the authors would consider to add a table in which they list the different approaches, different scales and different settings used. I can imagine that this would increase the readibility of their paper.

Page 4, Line 24: I suggest to write: "... availability. The distance between ICESat along track samples is 172 m. If we ..."

Page 4, Line 25: It is just a minor detail but I suggest to write "about 580" instead of "about 600".

Page 4, Line 29: I suggest to give more information about the type of satellite images used by Kwok et al.

Page 5, Line 5: What is R overbar?

Page 5, Lines 9-11: I have difficulties to understand this sentence. What is meant with "their agreement"?

Page 5, Lines 11-12: I have difficulties to understand this sentence as well. Why is the number of tie points not sufficient for basin wide studies?

Page 5, Lines 14-15: I suggest to write: " ... the surface roughness of a given sample ..."

Page 5, Line 15: What is meant by "to be for this sample"?

Page 5, Lines 18-19: I suggest to write: "We discuss the influence of the regression model in Section 3.3"

Page 6, Lines 1-3: I am aware of this correction. In the frame-work of this paper and these

studies, I suggest the authors could spend 1-2 sentences more on this issue. The TP method is the one you are finally after here. Now, if the TP method has - as one of the caveats - that it identifies snow covered, refrozen leads as potential tiepoints, then one could doubt its reliability. Thin ice, to carry snow on it, needs to have a certain thickness. Could it be that this correction is also done to take the effect of frostflowers growing on thin ice into account. These increase the reflectivity as well. In this context the authors could include also a sentence or two into the discussion because this is an obvious limitation of the TP - or in general of methods using laser altimetry where the surface reflectivity of the sea ice plays a major role in the SSH approximation.

Page 6, Lines 7-8: I suggest to formulate this more clearly in the last sentence of this paragraph. The correction does not increase directly with freeboard height because the latter is not part of the correction factor. Therefore it might be better to write about a relative increase. In contrast, the reflectivity R is part of the correction factor and directly influences it.

Page 6, Line 29: Comments to Table 1: Does this table include values from the entire maps shown in the various figures or did the authors focussed on a certain region? I am asking because the work of Kwok et al. usually focusses on the Arctic Ocean, omitting seas such as the Hudson Bay, Greenland Sea, etc. I am wondering whether it would make sense to do the same in the present study - and if not what could be a good reason to not limit the area. If the authors keep the region like they did, then I suggest that they mention this in the discussion of the results when they are referring to differences between the different methods in terms of the retrieved sea ice thickness.

Page 6, Line 31: "differences between data releases" I suggest the authors try to be less global here and to give an approximate estimate of a) what causes these differences and b) how large these are. This might also help to explain why the standard deviations of line 1 in Table 1 are relatively large.

Page 7, Line 1: "up to 10 cm" Just a comment: This is indeed a lot for sea ice thinner than 15 cm. This is a bias of up to 2/3 of the sea ice thickness and in terms of sea ice thickness derived from the biased freeboard this would amount to an up to 300% relative error. That is something.

Page 7, Lines 2-4: See my general comment 3.

Page 7, Line 13: "This explains ..." It seems a bit strange that the usage of a constant

(possibly wrong) gain is selectively causing biases for specific ICESat periods. Again see general comment 3.

Page 7, Line 24: The authors could add that the main purpose of this along-track averaging is to get rid of the large-scale fluctuations in the elevations caused by the geoid used.

Page 7, Line 29: "Using ... into ..." I don't understand the meaning here.

Page 7, Line 30: The authors might want to state why they switched to the EGM08 geoid here.

Page 7, Line 31: "from the use of" ... I suggest to write that the authors have obtained these; these are not just used, right?

Page 8, Line 1: "Freeboard ..." The authors could note that positive biases also occur along the coasts - something which also confirms the results of Kern and Spreen (2015).

Page 8, Lines 1-2: "patterns are similar" I don't agree here because, e.g., in the Greenland Sea, the Fram Strait area and the Kara Sea the pattern differ.

Page 8, Lines 3-6: Why should we note the roughness here?

Page 8, Lines 7-8: The text refers to one map in Figure 2d) but the figure actually shows two maps.

Page 8, Line 11: The authors could mark the location of these ridges in the respective map.

Page 9, Lines 7-11: Why are the authors doing this? I guess I missed the motivation for

discriminating between FYI and MYI. See also my general comment 2.

Page 9, Lines 19-21: I don't understand this sentence. Basin wide and MYI seem to contract

each other. And if the TP method does not provide (enough) freeboard measurements in a 25 km

grid box (or track segment) then there are no data from the TP method, right, and can

therefore not be compared with data from the other method?!?

Page 9, Line 22: (i) can be formulated more clearly perhaps. Isn't the main reason here that

the LLE identifies re-frozen leads, where the refreezing has happened some time ago and

hence the lead is not covered by thin ice anymore, compared to leads with open water or

young ice?

Page 9, Line 23: What does absence of local detection mean? Aren't SSH values interpolated

across gaps?

Page 9, Lines 25-27: The statement that more leads are found over FYI area than in MYI areas

could perhaps be confirmed from the available literature: Willmes and Heinemann, Remote

Sensing, 2016; Ivanova et al., The Cryosphere, 2016; Röhrs et al. The Cryosphere, 2012;

Brohan and Kaleschke, Remote Sensing, 2014.

Page 10, Line 5: "We checked that our results ..." How did the authors do that? What was the

measure used to do this?

Page 10, Line 6: The regression curves mentioned are not shown, right?

Page 10, Line 7: Why "thick MYI"? Isn't simply "MYI" sufficient here?

Page 10, Line 10: Contributions of tie points with "larger distances" dominate? Why larger?

Page 10, Line 11: I am a bit confused here. I thought that sigma_25 is computed for non-

overlapping 25-km segments in the TP method. Did you change this and now use a running 25 km

segment?

Page 10, Line 12: Why do tie points with lower h_r contribute more than those with larger

h_r. I don't fully understand this.

Page 10, Lines 14-15: The authors write about the missing linear relationship between

sigma_25 and freeboard for freeboard values approaching 0 and refer to Figure 6 a) already.

I suggest the authors spend more effort to first discuss Figure 4 in more detail and then

explain what they do and why, by using Figure 5 and 6. Figure 4 did not yet get too much

attention. See also my comments to Figure 4.

Page 10, Line 18: "contribute more than the other" Why?

Page 10, Lines 28-29: "For example, differences ..." I don't see this.

Page 11, Line 5: "15 samples" as are shown in Figure 5?

Page 11, Line 12: Why is this? The authors could not yet convince me. The dark blue curves,

for instance, seem to be the least noisy ones, for instance.

Page 11, Lines 13-19: The correction is about 1 cm (1-2 cm for FYI) ... is this worth the

effort? Perhaps a more proper discrimination of MYI and FYI would enhance the results (see

general comment 2).

Page 11, Line 18: "differences remain unchanged" This is not true if one looks at Figure 7.

Page 11, general comment to section 3.3.2: I have the feeling that a better description and

discussion of Figure 4 would form a better basis onto which this section can root. I have

the feeling that this section would be more easy to read once the information is structured

a bit better and in chronological order, and by underlining the contribution of the authors

more.

Page 11, Line 24-25: "rather uniformly distributed" Why is this the case? Does this make

sense in the eyes of the authors?

Page 11, Line 29: "from and after ON05" This could be gain related - see general comment 3.

Page 12, Lines 1-2: I am wondering whether it would make sense to show these values in Table

2 as well ... or in a new Table 3 (see below).

Page 12, Line 3: "and over the ICESat period" What is meant here? I would add an "is" before "ranging".

Page 12, general comment: I am wondering why the authors do not show maps generated with

their own "optimal" approach?

Page 12, Lines 8-13: I would find it helpful to have the colors used in Figure 9 to be

written in parentheses in the text after the respective variable. Does the variation of valid grid cells have an influence on your results?

Page 12, Line 19: "it may underestimate ..." Why? Where did the authors demonstrate this? A

link to the respective figure in this paper would help.

Page 12, Line 21: "30 cm" I would read 35 cm from Figure 9.

Page 12, Line 22-23: Are you referring to Figures 4 and 6 here?

Page 12, Line 23: "have greater weight" Why?

Page 12, Line 26: neighboring to leads ... ICESat data." I don't understand how this worked

out without additional reference data of freeboard; were airborne data used?

Page 12, Line 26-27: "However, the difference between freeboards retrieved ..." Is this the

desired goal? It sounds like GSFC is the truth against which your method can and is

referenced. Is this the case? What I recommend the authors to do is to also discuss the resuls of using the GSFC approach

(LLE) with 25 km window. Figure 9 illustrates that the "regular" GSFC and the 25 km GSFC

differ quite a bit. The authors also mentioned that presumably by using 25 km windowing one

can get more realistic results than using 100 km windowing. I feel that this part of the

discussion is currently missing in the paper.

Page 12, Line 30: The authors please check whether they are indeed referring to "sea ice

freeboard".

Page 12, Lines 30-32: 7 cm ... 6 ... 0.42 m ... these numbers and/or conclusions are

contradicting the notion of the "ten times" mentioned in the introduction.

Page 13, Line 2: "$\sim$ 1 cm" I am suggesting that the authors add another table in which they

show at one glance the results of this paper in comparison to the other results. I guess it

would increase the readability of the paper and would also help the authors as well as

the

readership to estimate the relevance of the optimizations carried out by the authors.

Page 13, Lines 3-7: These lines read as a repetition of information which is also given in

the conclusions.

Page 13, Line 13: Please mention what the database and region for which this number is valid

for.

Page 13, Lines 28-29: "inadequate weighing" To which method does this refer?

Page 13, Lines 29-32: This reads a bit strange. The authors improve the TP such that it

agrees better with the GSFC_100km - over FYI basically - and then recommend to use the TP.

One could ask why not simply use the GSFC? Perhaps the authors can stress more why GSFC

potentially has more uncertainties / biases than the TP ... and the authors could comment on

GSFC_25 km as well.

Page 17, Table 1: I suggest to use +/- instead of the "/" to separate mean and standard

deviations.

Page 18, Table 2: What is the unit? What is the second number in each cell? How do these

results relate to your correction? What is meant by "adjustment" here - or in other words:

What is the reference? In the second row of the table I would write: "Lead width"

Page 19, Figure 1: - In the maps in b) and c) there is a line artefact in the Fram Strait? The authors could

comment on this. - The histograms might benefit from a vertical line denoting zero difference.

Page 21, Figure 2: - The maps in d) lack the annotation which geoid is used.

Page 23, Figure 3 - Why is the difference between LLE and TP on average smaller for FM periods than for ON

periods? If the positive elevation bias due to re-frozen leads instead of open leads would

be the reason, then I would assume that it is easier to hit an open lead / lead with just

very thin ice during ON periods than during FM periods. And therefore I would assume that

the effect of leads covered with a finite sea ice thickness causing a bias in the LLE and in

turn causing an notable difference between LLE and TP to be larger during FM than during ON.

But apparently the opposite is the case.

Page 25, Figure 4: - The images lacks annnotations a) and b) - I would find it helpful to see lines of the regression used in the TP for every ICESat

period to understand better the limitations of the TP, to understand whether there are

differences between ON and FM periods and learn, where on average, the linear rela-

tionship

starts to fail. One could do it via dashed lines like in Figure 6. - I would be good to understand the motivation why the authors did chose the periods shown

in Figure 6. - How would these plots look when FYI and MYI were discriminated?

Page 26, Figure 5: What is the motivation for showing these periods?

Page 27, Figure 6: - The caption lacks description of what the dashed lines refer to. - For images c) I suggest to use the same scale at the y-axis and to perhaps also give the

total number of valid values used in each image (in c).

Page 31, Figure 9: - The thin dashed lines show the results of your optimization. I am wondering why the

authors are not highlighting their results more - see general comment 1. - I am wondering whether it makes sense to show the "old" original TP version results when

the two corrections mentioned (lead fraction in FoV and snow depth / reflectivity) are

needed to come up with "reasonable" results using the TP? One could, instead of showing

results from the TP without these two corrections show only the final versions of the TP. - The LLE does not require any of these two corrections, right?

Typos: Page 1, Line 11: ICEsat –> ICESat

Page 3, Line 15: denoted –> denote

Page 4, Line 28: accordingly –> according

Page 5, Line 7: Kwok et al., –> Kwok et al.

Page 12, Line 25: "in average" –> "on average"

Page 13, Line 20 & 27: "in average" –> "on average"

Page 14, Lines 3-4: "in average" –> "on average"

Page 14, Line 17: "6(69)" –> "56(69"

Page 14, Line 19: Please add "-Oceans" in the Journal title.

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