

Response to Interactive comment: “Characterizing Arctic sea ice topography using high-resolution IceBridge data” by A. A. Petty et al.

Reviewer comments in black, our response in blue.

A difference document is also included as an author comment highlighting the changes made based on the reviewer comments (PETTY2015_TCD_DIFF.pdf).

Anonymous Referee #1 Received and published: 7 January 2016

General comment

This paper presents an analysis of sea ice topography data collected as part of the IceBridge mission between 2009 and 2014. The approach adopted combines conically scanning laser altimeter data, optical imageries and snow radar data to obtain a 3D product of sea ice topography. The authors present statistics of the height of sea-ice topographic features in the line with many previous studies and, as a novelty of this paper, a geometrical characterization and statistics of the volume of surface features. The main findings of this study are the strong spatial and interannual variability of sea ice topography, also linked to ice age (MYI/FYI), the dependence of ice topography on the proximity to the coast and the strong correlation between surface feature height and ice thickness. The paper reads well throughout. The data analysis is complex, complete and well presented. The novelty of this work, i.e. the 3D study of sea ice surface features, brings a new insight in the sea-ice surface topography. An outlook presents future development and interesting application of this work. Nevertheless, in very few points I would like to see a more detailed discussion of the results (see specific comments).

We thank the Reviewer for their comments and efforts that will greatly improve the manuscript. We agree that more detailed discussion is needed and have discussed this in response to the specific comments below.

Specific comments SEA ICE TOPOGRAPHY CHARACTERIZATION: at page 6503 lines 3-5, the authors refer to future work when mentioning differences between the present study and former linear profiling studies. I do agree that the material presented here is already enough, but I still think that few words about expected differences will make the present study immediately comparable with former studies.

This comment is dealt with somewhat in our response to the comment below, where the discussion at the start of 6503 provides more detail surrounding the benefits of this 3D study. We have added some more information here to make this clearer, as detailed below.

We have also added the following sentence to make it clear that our distributions are expected to be different, but presented in a way that enable comparison with earlier work:

‘The feature height distributions in this study are thus likely to differ from those presented previously [e.g. Wadhams, 1980], however, the distributions will be presented in a similar manner to enable comparison with these earlier analyses.’

In this 3D analysis the height of a topographic feature is actually the maximum (peak) height of it (as explained at pg. 6506, lines 22-25), whereas a 2D profiling along the same transect would more likely detect a maximum that is not the peak of the surface feature. This would lead to a systematic underestimation, when dealing with linear profiling studies, that should be taken into account when comparing results. This is mentioned at page 6510, lines 16-19, but it should be

mentioned already before. This would also put more stress on the improvements of a 3D analysis compared to previous studies.

We agree this is an important component of this work that needs stressing. We do also discuss this idea back on pg. 6502 of the original manuscript:

‘It is worth noting that these features will likely differ from those detected using linear profiling. For one, the Rayleigh Criterion (separating peaks by measuring the depth of the crest between them) is not employed in this study, due to the three-dimensional nature of the data. The relatively wide (~200-300 m) swath width also means we are much more likely picking the peaks of the entire surface feature, as opposed to linear profiling studies, which detect the peak of the surface feature along a random (linear) profile. These differences in approach, and the impact on the resultant sail heights especially, are discussed in more detail by Lensu [2003].’

We have added another line to this discussion to add a further benefit of this 3D study over previous 2D (linear transect) studies:

‘In regions where surface features are sparse, the two-dimensional ATM scan makes it much more likely that we will detect a surface feature (higher than the chosen elevation threshold).’

We have also changed the start of L16-19 to reference this discussion: ‘As discussed earlier (Section 3)’...

RESULTS: I find the discussion of Figure 5 (pages 6508-6509) a bit lacking. For example, in the years 2012, 2013 and 2014 (when there is enough spatial coverage in the BC region) on one hand we see a similar pattern in the CA, on the other hand in the BC the differences between the three years are very strong, and not simply due to the presence of MYI/FYI ice. Did you investigate possible causes? As it is mentioned at page 6515, lines 10-12, the winds in this region are mainly oriented parallel to the coast and can push the ice away from this region towards the Bering Strait. Thus it could be that differences in atmospheric forcing caused such differences in ice topography. Please elaborate this point. In both the height and the volume of features there is a decrease from 2009 to 2013 and then an increase in 2014, did you investigate possible causes?

To make the discussion of both the feature height and volume variability clearer we have moved the discussion to a new section (**4.1.3 Potential causes of feature height and volume variability**) and have embedded the feature height/volume results in two sections before this (in **4.1 Sea ice topography characterization**).

We have added the following sentence to the new Section 4.1.1: ‘Note that a discussion of potential causes of this interannual variability is provided later, in Section 4.1.3.’

We have elaborated on the discussion that was previously contained at the end of the volume results discussion to include more information regarding the 2009-2014 interannual variability. We have mainly focused on providing a more complete discussion surrounding existing studies that attempted more to understand the causes, rather than impacts of ice dynamics. We refer the reviewer to this new section.

The BC mean surface high variability between 2011-2014 is still relatively small compared to the large spread (standard deviation) in feature heights. We did analyze the ice drift vectors from NSIDC, but didn’t observe any obvious link between the drift through the preceding months and feature height variability so decided not to elaborate further, as this would likely add unnecessary confusion. We have modified the discussion surrounding the complex interplay between Beaufort

Gyre ice drift and melting out of ice in the BC region, which impacts the presence of MYI in the region, and added information about the recovery of old ice throughout the Arctic in 2014. We refer the reviewer to this new discussion section (4.1.3).

In both abstract and conclusions I read “crucial information regarding the tail of the ice thickness distribution” but I somehow miss the point. Do you refer to figure 7 or to paragraph 4.4? In the first case, only few rows in the paper are reserved to the discussion of Figure 7 (p. 6510 lines 12-21 and pg. 6511 lines 6-8). I think the authors need to justify more this statement.

When we mention the tail of ice thickness distribution, we are referring to the surface features detected in this study, which contribute to the thicker end of the *overall* ice thickness distribution. We are not referring to the tail of the surface feature distribution (e.g. Figure 7).

I find the discussion of Figure 9 (pg. 6512, lines 9-13) a bit lacking. For example, the shape of the distribution is very different between the CA and the BC, any possible explanation?

We have attempted to provide more information about the causes of differences in distributions, as discussed in the reply to the comment above.

Page/Line comments

- 6496/26: "... various spatial scale ..." please add range of variability of ice floes.

We have changed:

‘Sea ice is a heterogeneous medium consisting of level and/or deformed ice floes of various spatial scales, separated by cracks and leads (regions of open water).’

To:

‘Sea ice is a heterogeneous medium consisting of level and/or deformed ice floes of various spatial scales (hundreds of meters to several kilometers in diameter), separated by cracks and leads.’

- 6497/6: Ridges do extend vertically but also horizontally for tens/hundreds of meters. This should be mentioned already here, since in a 3D study such characteristic is important. To my mind, this is one of the reasons why the Rayleigh criterion cannot be used in a 3D study, as mentioned at pg. 6502 lines 25-28.

Agreed. We have added:

P3/L5-6: ‘Distinct pressure ridges can often extend laterally for tens/hundreds of meters across an ice floe.’

- 6497/15: "In the winter Arctic ice pack ..." this is not truly correct because sail and keel density is high not only in winter but also in regions characterize by high convergence. Please rephrase to "In Arctic regions where sail and keel density is high ..." or similar.

Agreed. Replaced: ‘In the winter Arctic ice pack where...’ with ‘In regions where...’

- 6499/19 Add the exact time in the year when the data were collected, either here or in Table 1.

Added this to Table 1 based on comment below.

- 6503/4-6: See specific comment above.

See above.

- 6507/21: I guess you mean "...mean height OF all the points..." .

Yes, thanks. We have changed this.

- 6509/7: See comment above regarding Figure 5.

Figures 5/6 uses the peak height of the features, not the mean of all points within each feature. This is explained in the previous methods section, which we think is enough of a clarification.

- 6512/6-8: I find this sentence a bit confusing, please rephrase.

Replaced:

'The regional variability in V_f appears stronger than the feature height variability, implying variability in the areal coverage of the features consistent with the feature height variability.'

With:

'The regional variability in V_f appears stronger than the feature height (h_f) variability. Repeating the analysis for both the mean areal coverage and mean height of features (not shown) demonstrates a roughly equal contribution to the regional volume variability from each term (features increasing in area and height concurrently).'

- 6512/24-26: to my mind, this sentence points to the importance of such 3D study. Stress this point more.

Agreed. We have added:

This highlights how the three-dimensional surface feature volume analysis presented is a more useful indicator of the total ice topography variability, compared to linear transects of peak feature heights, as discussed in Section 3.1.4.

- 6515/21-24: same as previous comment.

Agreed, we have added the following:

'This suggests that the three-dimensional surface feature volume data is a more useful measure of coastal topographic variability compared to the surface feature heights, especially compared to data compiled from linear transects.'

- 6520/1: "crucial information", see specific comment above.

We have replied to this in the comment above.

- 6522/30: the link is wrong: <https://aaltodoc.aalto.fi/handle/123456789/2097>.

We have added this website to the reference. The old url was added by the Copernicus editorial staff.

- Table 1: Add the exact period of the year when the data were collected (see also comment above).

OK, we have added start and end dates of the sea ice campaigns to the table.

- Table 3: Should "2015" be "All" ?

Yes, thanks. We have changed this.

- Figure 2, 5, 8, 13, 14: add lat, lon to the maps.

We have added lat/lon labels to these figures as requested.

- Figure S6 (Supplement): the caption is inconsistent with the figures and with what is written in the paper at pg. 6511/9-10: it is not a comparison between the two regions but between MYI and FYI.

Yes, this was wrong. Thanks for pointing this out. We have changed the caption to read:

'...for all features classified as either (a) first-year ice or (b) multi-year ice (ice type mask shown in Figure~S4).'

Some references are missing the second name of authors, in particular:

Abdalati et al., 2010
Hibler III et al., 1972
Hibler III et al., 1974
Krabill et al., 2002
Maneewongvatana, S. and Mount, D. M., 1999
Mock et al., 1972
Onana et al., 2013
Timco, G. and Weeks, W. F., 2010
Tucker III, W. B. and Govoni, J.W., 1981
Tucker III et al., 1979
Tucker III et al., 1984

We have gone through and made additions/corrections to the references. Some of the references listed above seemed to be already complete, however.