

## ***Interactive comment on “Characterizing Arctic sea ice topography using high-resolution IceBridge data” by A. A. Petty et al.***

**Anonymous Referee #1**

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### **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

C2673

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).

### **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. 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.

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

C2674

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?

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.

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?

#### Page/Line comments

- 6496/26: "... various spatial scale ..." please add range of variability of ice floes.
- 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.
- 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  
C2675

convergence. Please rephrase to "In Arctic regions where sail and keel density is high ... " or similar.

- 6499/19 Add the exact time in the year when the data were collected, either here or in Table 1.
- 6503/4-6: See specific comment above.
- 6507/21: I guess you mean "...mean height OF all the points..." .
- 6509/7: See comment above regarding Figure 5.
- 6512/6-8: I find this sentence a bit confusing, please rephrase.
- 6512/24-26: to my mind, this sentence points to the importance of such 3D study. Stress this point more.
- 6515/21-24: same as previous comment.
- 6520/1: "crucial information", see specific comment above.
- 6522/30: the link is wrong: <https://aaltodoc.aalto.fi/handle/123456789/2097>.
- Table 1: Add the exact period of the year when the data were collected (see also comment above).
- Table 3: Should "2015" be "All" ?
- Figure 2, 5, 8, 13, 14: add lat, lon to the maps.
- 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.

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