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Interactive comment on "On the Retrieval of Sea Ice Thickness and Snow Depth using Concurrent Laser Altimetry and L-Band Remote Sensing Data" *by* Lu Zhou et al.

Anonymous Referee #1

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General comments:

In this manuscript the authors present a new method for the retrieval of sea ice thickness and snow depth using combined laser altimetry and L-band remote sensing data. The paper address a relevant scientific question within the scope of TC. The authors introduce a method based on snow freeboard derived from airborne Operation Ice Bridge (OIB) and the 1.4 GHz brightness temperature from the SMOS satellite. For verification they use again the OIB data and show a "good match" between observed and retrieved parameters.

The combination of altimetry and radiometry could possibly be used to improve re-

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Discussion paper

trievals of sea ice parameters. However, I am very sceptical that the conclusions derived in this manuscript are valid. The manuscript lacks a certain level of detail and traceability. Moreover it seems that the conclusions are based on circular reasoning. At least it seems that there is no real validation because both the retrieval and verification are based on the same data set. Therefore, I can not recommend to accept the paper for publication in The Cryosphere.

The authors use laser-based snow freeboard and radar-based snow thickness as well as the sea ice thickness from OIB flights. One problem of the present manuscript arises from the fact that the sea ice thickness in the OIB product was derived using the snow freeboard and the radar-based snow thickness (Kurtz et al., 2013). Therefore, it is not very surprising that the authors find a very high covariance between snow depth, thickness and freeboard because the quantities can not be considered as results from independent measurements. The "verification" in Figure 7 seems to just exemplify this circular reasoning. Additional problems arises from the fact that different snow depth algorithms for OIB exist and that the instrumentation has changed from year to year (Kwok et al., 2017). The dependency of the used data sets and the lack of discussion of the used assumptions cast serious doubts on the validity of the conclusions.

Regarding the use of L-band brightness temperatures I found the Figure 1 misleading with emissions arising just from the surface and not from the ice volume or deeper layers. Some crucial model assumptions are not explained in the manuscript, e.g. the parameterization of ice salinity. It seems that a thickness dependent parameterization was used otherwise I can not explain the large sensitivity to ice thickness exceeding 3 meter. All this should be explained and discussed in the manuscript. This is perhaps described in the reference Zhou et al. (accepted) but is not yet available to me.

Another issue with the manuscript is the overall aim of the method. It is not yet clear what is the main advantage of combining laser altimetry and L-band radiometry? Is the method for the fusion of airborne and satellite data or for to be used for future satellite missions? For the ICESat period there are no L-band radiometer data available. For

the ICESat-2 period it is not clear if SMOS and/or SMAP are still in operation. What about the different spatial and temporal samplings and uncertainties? These practical considerations are not yet even mentioned.

Specific comments:

P4L7 The resolution of the radiometer depends mainly on the size of the antenna.

Units should not be in italics.

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