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The Cryosphere
Discussions

Interactive comment on "Arctic sea ice thickness loss determined using subsurface, aircraft, and satellite observations" *by* R. Lindsay and A. Schweiger

Anonymous Referee #2

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Review – Lindsay & Schweiger, Arctic sea ice thickness loss determined using subsurface, aircraft, and satellite observations, The Cryosphere Discussions

The authors present a study into systematic differences between Arctic sea ice thickness datasets obtained from various observation systems. Using a least-squares multiple regression model, they find close agreement between five of the systems while others give significantly thinner or thicker ice. Combining all observations, the authors derive substantial negative trends for annual mean ice thickness. The study is an interesting and timely attempt at solving the issue of comparing and combining sea ice thickness measurements made at different locations and different times. The manuscript is well written but could be improved especially in the error assessment. I

recommend moderate revisions.

General comments: - I miss a comment/discussion on the use of a mean value for ice thickness. Given the usual non-symmetric shape of an ice thickness distribution, the mean can be misleading and the mode of the distribution is more reliable. How does this affect the biases in the different datasets and the systematic differences? - All datasets are heavily affected by how snow is treated during the processing or in the conversion of draft, freeboard, or total snow&ice thickness to ice thickness. In the descriptions of the datasets, the authors mention the different snow thickness estimates that have been used for the different datasets, but do not include a discussion on the implications of using the different snow data for the the systematic differences (e.g. using the Warren climatology which is likely not representative for the Arctic sea ice regime in the 2000s). While I agree that a full discussion is beyond the scope of the study, it is an important point that deserves at least a qualitative, short discussion in the error assessment. It might be worth including the Webster et al., JGR 2014 paper. - On a similar note, I think the authors underestimate the effect of open water in the ice thickness estimates of some of the datasets, and the footprint issue, especially when creating and comparing 50km averages from measurements with very different footprint size and shape. While a full statistical analysis of both is needed which is obviously beyond this study, a proper acknowledgement of these sources of errors should be included in the discussion.

Specific comments: - The title does not really reflect the main component of the paper, ie. the assessment of systematic difference between the datasets - p. 4546, l. 8: make clear that you mean the sources used in this study – there are lots and lots of on-ice measurements (e.g. Renner et al., 2014) - Introduction: It should at least be mentioned and acknowledged that thickness has been measured for a long time and in many regions using drilling and on-ice methods (e.g. groundbased electromagnetics, buoys) - p. 4547, l. 24-26: There is a CryoSat-2 ice thickness product available from AWI at http://www.meereisportal.de/de/datenportal.html ? - p. 4549, l. 3-5: The

averaging is unclear: I guess the mooring have been averaged only in time? Did e.g. IceBridge or airborne EM flights get averaged over a month too? Also, how do you deal with oversampling by overlapping footprints in the averaging? - p. 4549 ff: I assume the submarine and the morring data do not include open water in their thickness estimates? The Air-EM data have an open water bias when open water is in the measurement footprint. What about the other measurements? There's some inconsistency in the dataset descriptions; would be good to have the same information (footprint size, known biases like the underestimation of ridges, distance between measurement points etc) for all datasets. - p. 4550, l. 28: What are the characteristics of this profiler? - p. 4551, l. 15 (and other occasions): What do you mean by "clustered"? - p. 4552, l. 7-11: I'm not sure I understand your argument here. The numbers of observations are still highly variable between the different datasets, simply because of the varying spatial and temporal resolution and coverage. Why do you need to subsample here and not in other cases? - p. 4556, l. 4: There seems to be some spatial structure in the residuals with high values close to coast lines. Any thoughts why that is? Issues with the reference datasets due to the proximity to the coast? - p. 4557, l. 10: Here and throughout the paper: Given the uncertainties in the datasets which often are around 10 cm or more, does it really make sense to include the second decimal in the analyses of the differences? - p. 4559, l. 2: How do the biases change regionally? - p. 4559, l. 6: The ICESat data around the North Pole are not really observations, are they? It seems strange to me to include them in this part when the thickness estimates most of the area of the North Pole are based on interpolations, not actual measurements. - p. 4564, l. 13-17: Do the relative magnitudes also change a lot when other datasets are used as reference (IceBridge, Air-EM)? Otherwise this makes me wonder how reliable the spatial distribution in the submarine data is. . . - p. 4564, l. 27: replace "does" with "do" - p. 4565, l. 2: This would be one of the reasons to use the mode of the thickness distribution. - p. 4564, l. 19 – p. 4565, l. 5: At least note that there is in situ data? - p. 4565, l. 18: the largest negative value – relative to ICESat-G - p. 4566, l. 13: delete "from"

Comments to the figures: - Figure 1: The maps are too small to recognise anything. I realise that that is partly due to the formatting of TCD, but even if blowing up the figure on the computer screen it is difficult to spot details. Also, which ice thicknesses are plotted? All observations or averages? It looks like all observations and I wonder if it really makes any sense to plot measurements from different seasons on the same map. This creates patterns that are due to the seasonal cycle and not the geographical distribution. To me, the maps do not add any information that is not covered by the graphs in the right column and Fig. 2 a.

- Figure 2: Continuing on from the previous comment, Fig 2 a gives valuable information about the geographical distribution of the different datasets, however, it is impossible to spot the moorings. In panels a, b, d, and f, it is very difficult to distinguish the colours (almost impossible for colourblind readers). The two greens for the IceSAT datasets are too similar, and same for IOS-CHK and IceBridge. I suggest pulling out Fig 2 a into a separate, larger figure, and use different symbols and colours for different datasets. Regarding Fig. 2 b a similar question as for the maps in Fig 1 applies: are these average thicknesses?

- Figure 3: Again, colours are difficult to distinguish on the map; black and dark blue look almost the same when only a thin line. Typo in the legend (SCICEX Box)

- Figure 4: The dots in Fig 4 b are so tiny, it is difficult to see them at all. Instead of a cloud of dots, it would be more interesting to see distributions, e.g. annual. That would also give more information how "representative" the mean (vs the mode) is. Same comment to Figure 5.

Reference: Webster, M. A., I. G. Rigor, S. V. Nghiem, N. T. Kurtz, S. L. Farrell, D. K. Perovich, and M. Sturm (2014), Interdecadal changes in snow depth on Arctic sea ice, J. Geophys. Res. Oceans, 119, 5395–5406, doi:10.1002/2014JC009985.

Interactive comment on The Cryosphere Discuss., 8, 4545, 2014.