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Some thoughts on the altimetry aspects of this paper

Robbie Mallett

Community comment on "On the 2011 record low Arctic sea ice thickness: a combination of dynamic and thermodynamic anomalies" by Xuewei Li et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2020-359-CC1>, 2021

I read this paper with great interest as a user of CryoSat-2 data, and have a couple of thoughts regarding the authors' use of this data in the spirit of TC discussion.

It seems that a headline result of this study is that "a sea ice thickness record minimum is confirmed occurring in autumn 2011" (I hope this is fair to say given that it's the second sentence of the abstract and first of the Summary/Discussion). I think to fully make this claim there should perhaps be a deeper consideration of the nature of this metric and the uncertainties in altimetry-derived SIT (particularly over thin ice).

To state the obvious, SIT is a local property of sea ice, whereas extent and volume are global properties of the ice pack. The impact of this is that mean SIT is sensitive to the area over which it's averaged (unlike the other two metrics). The approach in this paper is to just average over the "area of actual ice coverage" (L37). The decision to average over this area this has many implications.

For instance, are the authors including the sub-Arctic seas like the Baltic and Okhotsk Seas and Baffin and Hudson Bays? I believe the AWI product includes SIT values for these. If the SIT of these regions contributes to the 'mean SIT' statistic, then how relevant is their interpretation of the 2011 minimum in terms of the dynamic/thermodynamic budget which is only produced for the Arctic Ocean (and also only with reference to 2011-16).

Next the authors should probably acknowledge that CryoSat-2 doesn't do a good job of retrieving the thickness of thin ice (<0.5m; see Ricker et al., 2017). This is because this ice protrudes above the waterline by less than 5cm, and even less with snow cover. But here I think the authors are averaging over quite a lot of thin ice to generate their statistic. The merged CS2-SMOS product was developed with this limitation in mind, and (in my opinion) should be the product of choice for this calculation. I particularly think this because the CMST model assimilated this product, so it should perhaps be used for consistency's sake anyway. A related issue is that CS2 simply can't measure ice below a certain thickness. By just taking the average in places where it *can* measure, the authors are likely biasing their mean SIT statistic high. The size of this bias will depend on the extent of sea ice with unmeasurably low freeboard. I think they should state what fraction of the total sea ice area (as measured by a scatterometer or radiometer) is covered by the

altimetry data under consideration. It's possible that this fraction is very high and my concern isn't warranted, but I think it is relevant.

Finally I'm not really sure what the whole-Arctic mean SIT minimum is supposed to tell us. I can imagine a year where there's an early freezeup and therefore a lot of very thin FYI coverage. So volume could be up, but mean SIT down. (but what if this weren't measurable by CS2?) But I can also imagine a scenario where we're low on MYI, so volume could be down as well as mean SIT. So does whole-Arctic mean SIT mean anything? I think some more consideration of the relationship between the metric and sea ice volume is warranted.

(for instance just before the freeze-up there is less sea ice volume than afterwards. But thin FYI proliferates after the freeze-up. So the effect of the freeze-up (I think) is that sea ice volume goes up, but mean sea ice thickness goes down sharply. But then volume and mean-SIT both grow together). So the minimum occurs after freezeup but before the ice starts thickening in earnest. Does the day of minimum mean-SIT therefore occur before CS2 starts working? I'd be interested to know on what day of the year piomas predicts the minimum in whole-Arctic mean SIT, and for that matter what year has the lowest minimum in that data.

A couple of narrower points:

The authors state: "The mean sea ice thickness within the area of actual ice coverage in October 2011 reached the lowest record for that calendar month in any year of the satellite records". The authors should point out that they have not examined the whole satellite record, which includes pan-Arctic SIT snapshots from ICESAT (2003-2010), and coverage up to 81.5 degrees by Envisat. Would be perhaps worth confirming that 2011 is a record low when also compared to ICESAT derived thickness? In particular I'm thinking about winter 2007-8 after the SIE minimum.

I'm also not sure that it's right to cite the NSIDC Kurtz & Harbeck data as an ESA product. Given I think both Kurtz and Harbeck were at and still do work at NASA? Could be wrong about this though.

Figure 4(b): It looks a lot like FYI export from the FS is negative for almost all months here? So it's flowing Northwards? Maybe I have the sign convention wrong, but in that case isn't MYI flowing backwards? I think some explanation is warranted about why it looks like there's an ice-type-dependent flow direction.

Figure 5): "Wind anomalies". Does the length of the arrow represent the magnitude of the velocity vector anomaly? Or the magnitude of the wind speed anomaly? These can be quite different. If it's the first then a large arrow can represent wind of the same speed going in a very different direction. If it's the second, then a large arrow can represent wind blowing in the same direction but at a different speed. I suppose it must be the first, because you can't have a negative arrow size? Or maybe it could be, because the arrows could then point backwards. Worth clarifying.

Lastly since this work presents data from CS2 altimetry and a model (which assimilates a related product: the CS2-SMOS data), as a reader I'm interested to know how independent the model and the altimetry are. Does the SIT data 'force' the model behaviour? Or is it a weak influence that can be relatively ignored?