

## ***Interactive comment on “Uncertainties in Arctic sea ice thickness and volume: new estimates and implications for trends” by M. Zygmuntowska et al.***

**Anonymous Referee #1**

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The paper provides a new estimate of mean Arctic sea ice thickness and volume derived from ICESat. It tries to provide a detailed assessment of the overall uncertainty in these parameters and the relative importance of specific sources of uncertainty. The overall goal of the paper is a good one. Various estimates of ice thickness from satellite have come with sometimes sketchy estimates of the impact of assumptions and sources of uncertainty. Though none the error sources are new, other papers attempting the construction of time series of satellite derived ice thickness often touch on some but not all of the sources of uncertainty. They often also don't provide the most tractable trail for their error assessment because they are rather focused on the story they are telling. Error analysis is pretty boring stuff after all. The paper purports to fill the gaps in the error assessment. Unfortunately, I think it only partially succeeds. Some of it has to do with an unclear description of the methods and interpretation of the results. Fun-

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damentally, the biggest problem in determining the uncertainty in mean ice thickness and volume is that we do not know how well errors are correlated in space and time and with each other, and to what degree retrieval errors are random and thus reduced by spatial and temporal averaging. The paper claims to address this problem, but I don't see how it does so. This may be due to the lack of detail that is provided for the Monte Carlo simulation or how the errors are accumulated. It is not clear to me how this was done. Were parameters changed globally or randomly for each grid cell? How is the spatial auto-correlation of errors handled other than by the dependency of some of the parameters on ice type or thickness? How were total uncertainties calculated? What is really the “Monte Carlo” part here.. It appears that what is done here are simply separate sensitivity calculations resulting in a sensitivity of thickness/volume to each parameters as functions of ice thickness and ice type distributions.

The paper also could be helped by more clearly defining what is meant by the uncertainties. The authors state that the uncertainties are for “mean ice thickness” or “total volume” but don't state over what time averaging periods. Are those for individual IceSat campaigns or aggregate over multiple years.

I wonder if the paper could be restructured so a “best guess” estimate could be presented first, which is then followed by the uncertainty assessment of this specific first guess. I think that might help with reading this.

The paper suggests that the Laxon 2013 findings about the ice volume loss between the IceSat period and the CryoSat period are substantially affected by the different assumptions about ice density. Since IceSat draft retrievals have been directly compared with in-situ measurements, the question arises how those comparisons would look with different density assumptions. It is also interesting that the computed ice volume change using different density assumption is more in line with PIOMAS calculated ice loss cited by Laxon et. al. 2013, though notably the period is very short.

Are there other substantial differences in the Kwok et al. 2009 and Xi and Zwally data

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sets that need to be considered?

I suggest some rewriting and clarification prior to publication.

Specifics: 5053... lack of sufficient estimate of uncertainties.. Please clarify what is meant? What would suffice? 5043, spatial autocorrelation This is mentioned here it is a very good point but it is never really picked up again as far as I can tell 5055.. for comparison we also use the gridded data set from JPL I don't see this comparison anywhere except in maybe Fig 8. Are those the JPL thickness retrievals? If so, how is the mean volume calculated there. Is this "weighted" by ice concentration or not. 5057, 3, frequency spectrum This isn't strictly correct since many sea ice algorithm stake advantage of the polarization differences... so maybe more generically "frequency and polarization" signatures. 5059, A clearer definition of the "domain" over which mean ice thickness is calculated. Make it clear that subsequent estimates are in "effective ice thickness", are they? 5061... explain more clearly how this is done and specifically how this addresses the shortcomings of other error analyses (e.g. spatial auto-correlation of errors, correlation of errors in time, correlation of errors between parameters) 5062, 8, "behind" should be "beyond" 5064, 26 sub-grid scale variability How is this computed? What does this mean in this context. 5066,5 no increase in winter ice thickness This is interesting. Kwok and Rothrock found that the IceSat estimates substantially underestimate the seasonal cycle. Could this be part of the reason? Kwok uses model predicted accumulation? How different is that relative to the reweighted W99 climatology. 5065, 29 mean absolute uncertainty Clarify over which time period this is, single day, one campaign, multiple campaigns? 5068, 20, changing weather conditions influence snow fall Better check those references. I don't think they provide any evidence that changes in weather have contributed to snow depth/fall differences. The Kurtz reference s certainly don't. When multiple references exists for same year, use a,b or some identifier 5072, 28... rate of -385 km<sup>3</sup>/a.. This is much closer to the PIOMAS-derived volume loss than Kwok et al. or Laxon et al. Though the differences in length of time series should be noted. 5074, 16.. the lack of sensitivity to

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ice concentration errors is somewhat surprising, can this be explained in more detail? 5075, 15 However, What are you saying here? The uncertainties shown in this paper suggest that IceSat or Cryosat derived changes are nonsense? I may agree with that point but you don't make it very well. You show that there is about a 30% error in the volume trends. This is a quantitative conclusion. I think some rewording of the idea is needed. Figs 4, 5 Horizontally Spatially is probably meant here

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Interactive comment on The Cryosphere Discuss., 7, 5051, 2013.

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