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Interactive comment on "Inter-comparison of snow depth over sea ice from multiple methods" *by* Lu Zhou et al.

Anonymous Referee #1

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Review of Inter-comparison of snow depth over sea ice from multiple methods by Zhou, Stroeve and others.

In this paper the authors explore how well various methods of determining the snow cover (mostly depth) across the sea ice of the Arctic Basin match each other, and where possible, match in-situ data. The methods consist of a) in-situ depth values from two ice buoy systems, b) Ice Bridge airborne measurements from radar, c) the Warren et al. (1999) and newer climatologies based on Russian measurements, d) satellite retrievals and e) physical models driven from reanalysis products. The authors find similarities between products in some geographic areas and at some periods of the winter, but also large discrepancies in both absolute values and patterns of depth. This comes as no surprise given the diversity of the ways in which the snow fields are derived, and the limitations within the various methods.

This is a useful paper, and quite an impressive job in merely handling the massive data sets involved, but it does not go far enough in examining the "whys" of the discrepancies, and for that reason I think needs another round of revision. To simply state that radical differences in footprint sizes and spatial scales of the various snow depth products are at the root of the observed differences, while perhaps correct, seems of little practical use.

Part of the problem with the paper seems to start with this statement, given as a motivation for the study: (line 48) [to] provide an inter-comparison of these products so that recommendations can be made to the science community as to which data product best suits their needs.

I think I understand what the authors mean here, but it is not what they actually wrote. One would hope that the science community is not only looking for products that suit their needs, but in fact, are as accurate as possible. So while we all can accept that with footprint size and scaling issues, snow depth "truth" may be elusive, conceptionally it exists and it really is what users, and snow product developers, need to strive for. This murkiness in purpose reappears throughout the paper in that none of the models or methods are ever labeled as "wrong," even when the results seem to be utterly improbable. I understand no one wants to "slag" a model in print, but clearly one conclusion from the paper is that some of the models, in some situations, should be avoided for now (until improved) and the authors could be more explicit in saying so when that conclusion is clear (e.g. The PMW-DMI model has 13 cm as the end-of-winter average across the entire Basin: Fig. 3 left).

Before getting to the heart of my review, I wanted to raise a point that I am not an expert in but I believe is important. In any inter-comparison of models, the comparison will be skewed when the models are being forced by different reanalysis products, with the precipitation forcing, notoriously difficult to get right in the Arctic, varying a lot in both time and space. How did the authors sort out input reanalysis precipitation differences from model biases. It seems to me that the models need to be run on the same input.

I found the results section of the paper quite good, and the graphics, of which perhaps there are a few too many, both useful and explanatory. If some shortening were to occur here, I would suggest the spatial trends in Autumn (Fig. 1), and the temporal trends over the period of record (Fig. 5) could be deleted. Autumn is a tough time to model or map snow, since open water (no platform) determines snow depth to a large extent, depending how it is handled in the model or with the satellite products. Figure 5, while interesting, is a dangerous figure to put out there, as it is likely to be cited as depicting real changes, when in fact the point of the paper is that the models and methods need significant improvement, as attested to in Figures 6, 7, 8, 9, 10 and 11. Personally, I would remove Figure 5. But if it is to be left in (see the next paragraph for this same point), then it would be more useful to explore why all of the models/methods converge in getting increasing snow depth in the Greenland-Canadian sector, while the Russian-Siberia sector is losing snow then to just put it out there as “trends.” Can this convergence be traced back to some basic climate variables that enters all of the various models? What drives the convergence?

And that type of analysis is what I think the paper currently lacks. For example, starting with Figure 3, depth histograms offer a wealth of interpretive information if examined closely. SnowModel and NESOSIM show a distinct drift shoulder (deeper snow) in Autumn that indicates they allow the snow to drift immediately, while CPOM and DuST show little or none. So this feature is about drifting: is it real? Should it be there? Strangely, the drift shoulder disappears by Spring for SnowModel and NESOSIM, yet appears in UW and DuST. Therein lies some behavioral differences that could provide insights for model/method improvement.

Figure 4 offers similar analysis possibilities. A lot is known at least at local scales about the patterns of snow build up on sea ice in various locations. The slope of these seasonal trajectories, and whether they curve off late in the season or not, is a diagnostic that could readily provide insight into what is or isn't working. The W99 curve is suggestive; only the CPOM curves seem to carry that shape, yet these curves fail to reach reasonable depth values by Spring. That should tell us something. Finally, considering Figure 10, which is fascinating, the paragraph (lines 426-431) discussing it barely scratches what could be gleaned from the data. Not only is W99 significantly deeper than the model/method products, in some cases by more than 2X, but also the histogram shapes are so different as to look like they are from totally different fields. SnowModel, DuST, and DESS all have a zero snow fraction, while the others do not. PMW-DMI is as peaked as the W99 data, but is about 1/3rd as deep. Surely, contained in this plot, which took considerable effort to develop, are many useful suggestions for model and method improvement (most of the same comments apply to Fig. 11 and SS18), but to get to them requires thinking about why the histograms have the shape they do. In the old days, a lot of emphasis was place on interpreting skewness and kurtosis, and that literature might be of use here.

In summary, I recommend this manuscript be returned to the authors for revisions without acceptance, that they be commended for undertaking a very useful and difficult set of analyses, and that they be urged now to reap the rewards of that analyses by gleaning more pertinent and useful information from their work. That that could prove very useful in improving existing and future models and methods of extrapolating snow over the Arctic Basin.

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Discussion paper