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Interactive comment on "Faster decline and higher variability in the sea ice thickness of the marginal Arctic seas" *by* Robbie D. C. Mallett et al.

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Review of

Faster decline and higher variability in the sea ice thickness of the marginal Arctic seas

by

Mallett, R. D. C., et al.

Summary: This very interesting paper illustrates the potential improvement in the credibility of trends in and inter-annual / intra-basin variations of Arctic sea-ice thickness estimates from satellite radar altimetry. This is achieved by a comprehensive inter-comparison of the contribution of snow on sea ice on the retrieval of sea-ice thickness

from radar freeboard when using the Warren et al. (1999) snow climatology on the one hand and a physical model for snow properties driven by atmospheric reanalyses' precipitation and other relevant meteorological parameters on the other hand. As expected, the inter-annual variability of the snow contributions based on the model data is considerably larger than the one based on the Warren et al. (1999) data. The paper further convincingly demonstrates that the more realistic inter-annual variation and trend in the snow parameters obtained with the model yields a new overall picture of the variability and spatio-temporal development of the Arctic Ocean sea-ice thickness.

The paper is generally well written and will have considerable impact on the scientific community. It would benefit from some re-organization (see GC1). It is furthermore quite light when it comes to descriptions of data and methodologies used (GC2). Currently, one would not be able to re-produce the work done. The inclusion of Kara and Barents Sea I find quite a hypothetical move based on the data availability and suggest to consider removing those from the analysis (GC3). Finally, there is a number of open points to discuss when it comes to the illustration and interpretation of the results presented.

In the following you will find my list of general comments (GC), specific comments and some suggestions to mitigate typos and editoral issues - all for the main manuscript - followed by a short list of things I found worth to consider in the supplementary material.

General comments:

Title: While your main conclusion supports the title in general, it is in some way misleading. The main focus of the paper is on the illustration that a snow depth climatology is not well suited to compute credible trends in sea-ice thickness estimates derived from satellite altimetry with such a snow depth as input for the freeboard to thickness conversion. In your paper, this is illustrated by usage of data from a numerical model which has experienced limited validation. Hence, albeit the improvement using these model data is obvious it is not necessarily the truth either. Hence, instead of formulating the title as a fact I suggest to include points of the above-stated.

GC1: I strongly recommend to re-organize the paper. Most of the explanations / motivations given in the subsections 1.1 and 1.2 are tied relatively close to Section 3 and should be combined with that section. In addition, subsections 1.1 and 1.2 refer to data and regions denoted in Section 2. Hence: Remove 1.1 and 1.2 and put it into Section 3. Let Section 2 start right behind the "true" introduction. That way the data sets used in 1.1 and 1.2 would be introduced adequately beforehand which eases reading and which reduces the number of open questions.

GC2: Both, the description of the data used as well as of the methodologies used lack some clarity and/or do not contain all information required. One good example: The ESA-CCI radar freeboard data set used comprises data of two different satellites with some overlap. It is not clear from the description in the data how long the Envisat and how long the Cryosat-2 part of the data used is - plus a motivation of the choice made - plus a discussion about the biases between the radar freeboards of these two satellites, which have a different sign based upon the region. Some of the descriptions also appear to contain errors which ask for re-phrasing.

GC3: The overall credibility of the paper would benefit from a more critical consideration of the application area of the Warren et al. (1999) climatology. Sampling density, number of observations, as well as the distribution of the snow depth observations over time combined with the usage of a polynomial fit limits the usefulness of these observations in the regions Kara Sea and Barents Sea. One good solution would be to omit these regions.

Specific comments

Line 18: " ... it determines whether floes ridge or raft ... " –> My take on this would be that this happens at rather small ice thicknesses, i.e. around 20-30 cm. I am therefore not so sure whether this is such an important physical role of the sea-ice thickness.

Line 21: "... with thin ice favoring melt pond formation ..." Why is that? Because there is little snow, which melts away more quickly than on thick is with a thicker snow cover? Otherwise I don't see a pressing reason why melt pond formation, which is basically driven by downward short- and longwave radiation should occur more easily on thin than on thick sea ice.

Line 76: "below the waterline" –> Why this part of the total SIT is from below the waterline? I find this addition confusing because it implies that the sea ice is thicker when it is snow covered - which is not necessarily the case. Equation (2) refers to a SIT which is similar in both cases, bare or with snow cover. It could be 1 m, 2 m, whatsoever, with or without snow. The quantities that change are the sea-ice freeboard and the radar freeboard. It seems you want to express that in case of a snow cover the part of the sea ice that is below the waterline is larger than in case of bare ice. Usually this part below the waterline is called draft. It might hence make sense to re-phrase this sentence a bit to avoid confusion.

Line 83: "assumes total radar penetration of overlying snow" –> How about penetration of the Ku-Band signal into the sea ice? Given the different near-surface sea-ice salinity and densities of MYI compared to FYI one might need to also make a comment on this issue?

Lines 109/110: "as quadratic fits ... without corresponding fits of density." –> I don't agree. Warren et al. (1999), page 8, writes about 2-dimensional density fits. Yes, only a May map is shown but maps are derived for all months. And it is the SWE which is computed, not the other way round. Please rewrite this paragraph accordingly.

Lines 121-124: I suggest to provide information whether and to which extend this modification of W99 has been implemented by follow-on studies, e.g. Tilling et al. ? Kwok and Cunningham 2015 ...

Lines 126-127: "are not currently used in sea ice thickness retrievals" –> I am wondering whether these variabilities are input into the uncertainty estimates provided along with the ESA-CCI SIT products? It might be worth to check.

Lines 130-133: "As such, ... values." –> Will this described in more detail in section 2? No it will not. Are positions of real drift stations used? If yes, which? If not: Isn't taking into account ALL ice covered grid cells of a 25 km grid providing a substantially different statistics - compared to the few drift stations used in the W99 climatology (their Fig. 1)? What is the time period considered? The description of this analysis step is lacking key details and should be re-written.

Figure 2: Fig. 2 could be connected more easily to Fig. 1 if you'd use snow depth instead of SWE. It is not entirely clear how these maps are derived. I note that the ice edge in the Kara / Barents Seas looks a bit weird for months DEC and JAN.

Line 136: "this results" –> Which? Not clear to what this refers.

Line 138: "where the ice type typically varies from year to year" –> How did you define this?

Lines 157-162: "We find ... (S3)." –> One question upfront: What is exactly the region you are considering here? The central Arctic Ocean? Laptev Sea included? Kara Sea? I am just asking because at a certain point in winter the entire region considered should be ice covered and the FYI fraction hence be only a function of the MYI extent.

I buy that there is a the decreasing fraction of FYI relative to the total ice extent in October due to later freeze-up. I don't agree, however, that this is the sole reason for your observation with respect to the trend mW99 snow depth and SWE. I believe an issue to consider is that the MYI coverage retreats more and more to those regions where W99 has maximum snow depth. Hence the relative fraction of MYI grid cells with comparably thick snow is increasing which to my opinion can result in a higher mean W99 snow depth (and SWE) for the MYI part of the sea ice in October.

Lines 164-166: "Several ... year to year. –> I don't find this formulation particularly clear. I find the "cannot accumulate snow from year to year" not to well chosen in the

light of mostly complete snow melt during summer - also on multiyear ice. I'd state that there are two reasons for the observations with SnowModel-LG: 1) The MYI area shrinks. Hence your sentence about "a lower [smaller] ice area is exposed to snowfall in September/October fits well. 2) Freeze-up commences later, hence new seasonal ice either has not yet formed or is too thin to carry / accumulate snow resulting in a substantial amount of the precipitation falling as snow being dumped into open water. In short also here your sentence about "a lower ice areas ..." applies, meaning you can, to my opinion, delete that extension "also the later ... year."

Line 167: "Webster et al. ..." –> I am wondering whether the "in situ sources" mentioned in the context of Webster et al. (2014) are i) also representing FYI and ii) aren't complemented with information from airborne operation ice bridge data [in which case these are not "in situ" anymore]. Please check! If their data indeed represent FYI and MYI then it might be worth to mention that explicitly in your manuscript.

Lines 187-189: "Where sea ice ..." –> Could you please comment on whether this second data set is similar to / consistent with the OSI-SAF one? What is the basis? Given the fact that you investigate quite short time series in your paper and put quite some weight on different ice types it is important that thes two data sets are consistent to each other, i.e. provide a seamless continuous spatial FYI/MYI fraction distribution without a jump in total regional FYI and MYI extent from, e.g., Feb 2005 to March 2005.

I note that you also use NSIDC ice-age data and one could ask the question: why didn't you use ice-age data throughout the entire study?

Section 2.3: Please comment on two issues. 1) The sampling on which W99 is based has large regional variations with substantial differences between marginal seas such as the Kara or Barents Seas compared to the central Arctic. How does the lack of reliability of W99 in these partly undersampled regions influence your results - particularly in the two regions mentioned above? 2) Snow depths / SWE in the Kara / Barents Sea do - for the same reason - depend a lot on the extrapolation / fit function used. How does this influence your results? Aren't the snow depth values in these regions too hypothetical to be adequately used in your study? Wouldn't it make sense to exclude the Kara and Barents Seas? To my opinion it would make your study considerably more credible. And it would potentially safe some space.

Section 2.4: Isn't the EASE grid a polar aspect of the Lambert Azimuthal Equal area grid? I'd suspect no re-gridding is required. What is the grid resolution of the radar freeboard data? What is the time period (years, months of the year) for which these data are available and used by you? How did you treat the overlap of Envisat and Cryosat-2? Key information is lacking here. It might also be worthwhile to take a look into the validation report of the SIT / freeboard data set used (see e.g.: https://icdc.cen.uni-hamburg.de/fileadmin/user_upload/ESA_Sea-Ice-ECV_Phase2/SICCI_P2_PVIR-SIT_D4.1_Issue_1.1.pdf). It provides some information about how "consistent" the two "merged" data sets are. Taking this information into account and discussing the potential biases (which still exist) I rate mandatory for a paper which so much relies on the analysis of this 17-year long time-series with a change of sensor right in the middle of the time series.

Line 206: "ice motion vectors" –> Which ice motion vectors? Please provide this information - including the temporal resolution and the version of that ice motion data set used - in your manuscript. In addition: "pan-Arctic snow depth and density distributions" –> Please provide a spatial and a temporal resolution as well as the domain. While the paper focuses a lot on snow depth I am wondering how snow densities obtained with SnowModel-LG compare to W99 ones?

Line 210: "snow-ice accumulation" –> Please explain what "snow-ice accumulation" is. Do you refer to snow-ice formation at the basal snow layer?

Lines 216/217: "snow depth differences ... than 5 cm" –> This is a quite global statement. Is this an Arctic mean value? Is this the mean difference in SnowModel-LS realizations just for the grid cells co-located with the OIB data? What is the standard

deviation of this difference? Do the OIB data used to tune SnowModel-LG represent FYI conditions adequately?

Section 2.6: For a better understanding it might make sense to explicitly state whether precipitation and/or snow fall are assimilated into NESOSIM as well. The snow pack initialisation, is this covering both snow depth and density? As W99 data are monthly values, is this initialisation only done monthly, or are monthly values interpolated to daily values with which the model is initialised henceforth? You explicitly mention depth-hoar and wind-packed layers in the context of NESOSIM. Does this imply that SnowModel-LG does not represent such features? If not, then I suggest to be more specific in the description of what SnowModel-LG can do and what not.

Section 3.2: Please provide more details. How many grid cells with valid SIT measurements are requied to compute a regional mean SIT value? How about regional means of radar freeboard and snow depth / SWE? Did you compute these as well? How many valid observations are required for the results broken down into ice types (see Figures S4 and S5)? Please provide a reference for the "Wald test".

Line 249: I suggest to stress here once more what "Snow_overbar" is, that it is not the snow depth but the snow-depth contribution to the SIT retrieved from altimetry

Line 259: "individual year, regions and months" –> Not clear what you did. You used detrended time-series of monthly, region-mean values of RF and snow and computed the correlation between these time series separately for every month and every region?

Lines 263/264: "The Barents Sea ... correlation." –> I suspect this observation is based on two completely different causes. For the Central Arctic the time series is just 9 years long. For the Barents Sea, neither is mW99 overly reliable nor are RF values overly reliable - especially during the Envisat period. Another, more general comment: The RF data for region Central Arctic are considerably more robust in terms of the number of valid observations contributing to the RF values used.

Figure 6: Not clear what is shown magnitude-wise on x- and y-axes. The same applies to Figures S6 and S7.

Line 286: "but analysis ... regions." –> Not clear what you mean here.

Line 287: "The covariability ... contribution" –> This discussion focuses on radar freeboard. It does not comment on the observation that at the beginning of winter (Oct.) the fraction of SIT IAV that is explained by RF-Snow covariance is larger than snow IAV.

Figure 7: Can you please check whether your representation of "Fraction of Total Variance (%)" is correct? I mean, ok, if the dimensionless factor rho is negative then the covariance term in Eq (5) gets a negative sign. Therefore you plot negative bars in panels (b). However, looking at the Central Arctic, November, this results in a fraction of radar freeboard IAV of about 110%, also the one for October is larger than 100%. I get a headache with this because a fraction cannot be negative (have you ever had a negative piece of cake?) and it can also not be larger than 100%, i.e. larger than the total (only if you order a medium size Pizza and get a large one instead). This applies then also to Figure S15 where the deviations from 100% are even larger. Again, I can see from Eq. 5 that it is mathematically correct. However, a positive covariability means that sigma_RF and sigma_snow are positively correlated while a negative one means that these quantities are anti-correlated. If we assume a very strong negative covariability of, say -90%, does that mean that the IAVs of RF and snow need to sum up to a fraction of the total of 190%?

Lines 327/328: "Perhaps more significantly, ..." –> This I don't find too convincing - also given the unknown uncertainty of these regional mean SIT values. I suggest to only mention these three new trends but do not hypothesize about the main reason.

Line 350: "... truncated SIT distribution ... thicker ice." –> This relatively global statement is not supported by Fig. S10 for all months. Particularly, I would not use the word "truncated". Truncated means that below or above a certain SIT values the area

occupied by these SIT bins is abruptly zero.

Lines 351/352: Please see my comment at Figure S10: You need to provide more details about how you derived this Figure. What is missing are binsizes and borders as well as the time-period for which the Figure is valid (2010-2018 I assume) as well as a statement here that this Figure is now showing a classical pdf but expresses the distribution in form of sea-ice area. In order to avoid confusion with the classical definition of sea-ice area which is sum of the area of ice covered grid cells weighed with the actual sea-ice concentration, you might want to rename your y-variable.

Line 353: "The regional, seasonal growth rate ..." –> What is the period considered?

Lines 372-374: I suggest to refer to the Boisvert et al. (2018) paper here (about the difference between Merra-2 and ERA-Interim) and in addition take into acount this paper: https://doi.org/10.1029/2019GL086426 . I am wondering whether Figure S12 is required. After all it confirms that your choice combining SnowModel-LG runs of ERA5 and Merra-2 is a good one.

Line 387: "replicates the higher contribution ..." –> Even though for the Central Arctic the data are just based on the period 2010-2015 = 6 years.

Linees 390/391: "underlying trends ... period" –> Ok ... but at the same time you use a shorter time period for the Central Arctic anyways. So this argument is not conclusive.

Lines 394-399: "As such ... 2018)." –> I suggest to shorten this part and perhaps either delete Figure 11 or move it as well to the Appendix. - which however, already contains a lot of additional material. But I feel that the paper would be still understandable with a few sentences highlighting the common findings between NESOSIM and SnowModel-LG

Line 427-428: "... these investigations ... warmer temperatures ..." –> I do not quite agree to this statement because, according to my knowledge, among the cases investigated and presented in Nandan et al. (2017) as well as in the later paper by Nandan et al. (2020, https://doi.org/10.1109/JSTARS.2020.2966432) is a sufficiently high number of cases with cold snow; hence the observation of a rising scattering horizon is not uniquely tied to warmer temperatures.

Lines 434-440: "Knowledge ... depth." –> I can follow the physical reasoning here and also how it is connected with Equation (2). However, it might not be as straightforward as it is formulated, given the fact that equation (2) transforms into equation (4) in Kwok and Cunningham (2008, https://doi.org/10.1029/2008JC004753) for a lidar - hence introducing a different factor in front of the snow depth. Perhaps formulating these lines more like being your own hypothesis than being fact might be more appropriate?

Line 443: In the context of the paragraph ending here you could also comment on the impact of i) the increased likelihood of a flooded basal snow layer in regions of comparably thin sea ice / thick snow / high SWE as, e.g. the Barents Sea and part of the region Central Arctic facing the Atlantic and ii) of a potentially increased likelihood for enhanced snow metamorphism, like is typical for the Antarctic, even in the middle of winter due to intrusions of marine air-masses into the Arctic, e.g. via the Fram Strait.

Line 469: "in the high precipitation months ..." –> One could argue, though, that thanks to later freeze-up and the concomitant change in atmospheric moisture content (and perhaps also circulation) also shifts the maximum of the precipitation to a later time - and with that the seasonal sea ice would still be able to accumulate a fair amount of snow - particularly supported by the fact that a warmer atmosphere can hold more moisture and with that can lead to increased precipitation rates. This is hypothetical of course but among the possible scenarios.

Lines 504/505: "negative ... this is not seen." –> I thought the negative covariances between RF and Snow shown in Figures 7 and S15 are an indication of exactly this observation?! The fact that these only occur in early winter makes a lot of sense as well.

Lines 505-507: "... snow is a highly ... weeks." –> I am wondering whether it would

make sense to to have a thought experiment to check whether the magnitudes of the the changes involved fit this hypothesis. While Snow_overbar can be easily computed based on the snow properties and is independent of the sea-ice thickness and its radar freeboard, RF_overbar is not. It is a function of both, ice growth and snow load. An experiment one could think of is, e.g. an initially 80 cm thick ice floe with i) 5 cm snow depth and ii) 20 cm snow depth (and similar snow densities) grows at -30 degC over a month. What is the RF for both cases at the beginning and what is the change in RF over the month? Without further snow accumulation Snow_overbar remains constant. How would the change in RF be modified if one would add another 5 cm of snow in the middle of the month and again at the end of the month? My hypothesis is that RF_overbar and Ssnow_overbar are correlated well in case a thin to moderately deep, slowly increasing snow cover allows adequate ice thickening so that Snow_overbar increases over time but RF increases as well over time. Further, my hypothesis is that RF_overbar and Snow_overbar are not well correlated in case an already thick snow cover hampers adequate ice thickening while it further deepens over time. In that case Snow_overbar increase over time like in the above-mentioned example, but the RF increase by increasing SIT and hence sea-ice freeboard is counterbalanced by an increasing radar range such that the increase is considerably slower than in the first example or even zero.

Lines 513-517: "Freeze-up ... further study" –> I doubt that for the months you consider this is an issue. Melt ponds in the Central Arctic begin to freeze over in mid August, latest in September; hence any snow falling in October falls on solid ice. I suggest to delete this part.

Typos / Editoral remarks

Lines 5/6 " ... with the conventional method ... " –> I suggest to tie this better the usage of a snow climatology.

Line 91: I suggest to use a different letter for the dimensionless factor than rho to avoid confusion with a density. Also "sigma_SIT_overbar" should possibly read "sigma_Snow_overbar"

Line 110: "radar speed" –> "radar wave speed" or "speed of the radar waves"

Line 118: If I am not mistaken, then Giles et al. (2008b) is purely dealing with the Antarctic, hence application of the W99 climatology appears to be unlikely.

Line 119: I agree that Eq. (2) contains the snow contribution to the sea-ice thickness; however, the term Snow_overbar is used in Eq. (4).

Line 134: "average half of the value" –> In order to allow a more fluent reading I suggest to write "about 50%" instead of "average half of the value" in Line 134. That way it will be easier to connect this finding with the SnowModel findings.

Line 139 and 140: "consistent" –> What is a consistent ice type in this context? Could it be that you wanted say "constant" or "unchanged"?

Line 187: "2005" –> "March 2005"

Lines 205: "assimilates reanalysis weather data" –> "is capable to assimilate meteorological data from different atmospheric reanalyses (see below)"

Lines 214: "results of reanalysis" –> "representation of the actual distribution of relevant meteorological parameters by atmospheric reanalyses"

Line 228: "Eq. (2)" –> Same issue as for Line 119 (see above)

Line 238: "The Central Arctic region exists above the latitudinal limit of the Envisat orbit" –> "The Central Arctic region is not sufficiently well observed by the Envisat radar altimeter (see Fig. 4)"

Line 255: "SnowModel's" –> I suggest to always keep the full name.

Line 257: "Having calculated ..." –> I suggest to refer to Figure 5 and the standard deviation shown therein. How about showing the detrended time series of RF_overbar?

Line 260: "between" –> "between detrended"

Line 278: "in for each" –> "for each"

Lines 286/287: "grouping by comparison to" –> "grouping in comparison to"

Lines 287-289: I suggest to add "radar" to all mentioning of "freeboard"

Figure 7 caption: For better readability of the figure and the text refering to it I suggest to somewhere re-introduce IAV here as the inter-annual variability. The last time IAV was used was in Figure 2. I cannot (a) and (b) in the Figure. I suggest to write "panel" instead of "figure in Line 293.

Line 294: "continuous" –> "contiguous"

Line 296: "was the" –> "was in the"

Line 306 "2002 - 2018": I suggest to add something like: "except for the Central Arctic: 2010-2018" in this caption as well as in all other figure captions where the Central Arctic region is shown along with results of the other regions.

Line 315: "Fig. 9" –> I suggest to refer to the panels highlighted in green.

Line 320: "declining" –> "negative"

Line 324: "mW99" –> Suggest to refer to Fig. 9 and the red panels

Line 325: "declining months to four" –> "months with a decline in SIT to four"

Line 326: "after 2006" –> I suggest to write "after 2003, except 2006"

Lines 333/334: "are only ones" –> "are the only ones"

Line 337: "Laptev Sea." –> I suggest to add "when using SnowModel-LG instead of mW99."

Line 340: "statistically significant months" –> When SIT is computed using SnowModel-LG?

Line 344: "snow" –> "show"

Figure 9, caption: I suggest to add the notion that y-axes of Central Arctic and East Siberian Sea differs from all other regions. I suggest to make "Where trends are ... superimposed" the second sentence of the caption. I suggest to add a note that unlike all other regions Central Arctic data are based solely on Cryosat-2 data.

Line 356: "Siberian there" –> "Siberian Sea there"

Line 404: "Chukchi" –> "Chukchi seas"

Line 405: "one month" –> "only one month"

Line 427: "raise" –> "rise"

Line 442: "diminishing show cover" –> I suggest to add: ", i.e. actual values of snow depth and SWE that are smaller than the climatological values"

Line 457: "EM" –> Has EM been explained already ...?

Line 489: "... truncated" –> Please re-phrase. It is not the radar altimetry time series that is truncated. There is a region where simply no measurements could be taken.

Line 511: "has longer to" –> "has longer time to"

Comments for the supplementary material:

Line 19: Please refer to Figure S1 here. Otherwise it is completely unclear how you ended up with the expression in Eq. (S8). You might want to replace the "=" by an "is approximated by ... as illustrated in Figure S1."

Line 23: "reformulated as" –> Which value is used for rho_water?

Figure S6, caption, line 3: "five" –> "four"

Figure S10, caption "... SnowModel-LG data." –> I suggest to add something like ... "expressed as total sea ice area of all grid cells falling into a specific SIT bin." In

addition: What is the bin size? What the bin borders? Since it includes the region "Central Arctic" this figure is based on years 2010-2018 only, correct?

Figure S11: There are two identical panels denoted (b). Please make a note in the caption about the differences in the y-axis range. Please make a note about the error bars. i.e. what these represent.

Figure S11, caption: "The SnowModel-LG contribution ..." –> While this statement is undoubtly correct also the aggregated marginal seas start low and end high. So why not also commenting on those regions? Again, I note that you need to provide the information whether all regions but the Central Arctic are based on the full period 2002-2018 or whether indeed all regions only used data from 2010-2018.

Figure S13: Again the notion about the smaller time period covered for region Central Arctic is missing.

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