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The Cryosphere
Discussions

Interactive comment on "Estimating the snow depth, the snow-ice interface temperature, and the effective temperature of Arctic sea ice using Advanced Microwave Scanning Radiometer 2 and Ice Mass Balance buoys data" *by* Lise Kilic et al.

Anonymous Referee #2

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Review of

Estimating the snow depth, the snow-ice interface temperature, and the effective temperature of Arctic sea ice using Advanced Microwave Scanning Radiometer 2 and Ice Mass Balance buoys data

by

Kilic, L., et al.

Summary: A suite of linear regressions is derived consecutively to derive i) an estimate

of snow depth, ii) an estimate of the snow-ice interface temperature and, finally, iii) of the effective temperature Teff - all from brightness temperature (TB) observations of the AMSR2 in the Arctic Ocean during winter time. This suite is developed with the aid of TB collocated with weather forecast data, OIB snow depth data and IMB snow depth and snow-ice interface temperature observations as well as with simulations of TB, snow-ice interface temperature and Teff with a thermodynamic model in combination with a microwave emission model. Observed and retrieved snow depths and snow-ice interface temperatures are compared by means of RMSD and correlation. Examples of retrievals of the Arctic-wide distribution of snow depth and snow-ice interface temperature are shown and discussed in the context of a multiyear ice concentration product.

This paper is an interesting contribution to the scientific literature in this field. Before it could become acceptable for publication the authors need to take care of several issues which are required to understand their methodology, to potentially re-do their analysis, and to better underline the new aspects of their work in front of the background of work done by others. Solving most of these issues will help the authors to reply to my suggestions to improve their discussion of the results achieved. I therefore hand this manuscript back to the authors, asking for major revisions. The general and specific comments will potentially aid in this process.

General comments GC1: The introduction needs a better structure: Relevance - previous work - shortcomings - what will you do and why. The introduction also requires an improved set of references to make clear the current state-of-the-art of snow-depth on sea ice retrieval and also snow-ice interface temperature retrieval.

GC2: The description and illustration of the methodology to retrieve the snow depth but also in particular the snow-ice interface temperature lacks important details for the understanding. See my specific comments with this regard.

GC3: The title "promises" retrieval of effective temperature but the paper kind of stops before having applied a method retrieving it from TB data and discussing any results into this direction.

GC4: I am a bit lost with regard to a critical discussion of the results. - I neither found a discussion about how accurate the automatically retrieved snow-ice interface temperatures are, nor did I find a discussion about the dependency of the different retrievals on the same data. For instance: The fact that in Figure 12 snow depth and multiyear ice concentration have a certain degree of correlation can partly be explained by using the same frequencies and polarizations (see eq 1 and the microwave data entering the MYI concentration maps). The same applies to Tsnow-ice, which is via its correction with the snow depth is also related to these frequencies. - Uncertainty estimates are missing in any of the retrievals presented. - A critical discussion about the physics behind the many linear regressions used would definitely add to the understanding of the paper and would give the approach more credibility.

Specific comments P2, L4-5: "Improved estimates of ... from satellite observations ..." implies that such estimates exist already. But they have not been mentioned yet.

P2, L8-19: I find this paragraph relatively weak and not suitable yet for this introduction. An improved paragraph would - more clearly separate between snow deoth and Tsnow-ice retrieval - find more references for both these parameters. Comiso et al (2003) for instance also refer to Tsnow-ice; there are other papers dealing with the application and evaluation of the Markus and Cavalieri (1998) (MandC98) approach in the Arctic; there are other papers discussing about the caveats of the MandC98 and suggesting improved retrieval, e.g. Markus et al., 2011; Kern and Ozsoy-Cicek, 2016. Isn't there a paper by Rostosky et al., 2018, also, where an alternative approach is proposed. Finally, there have been various conference contributions (Frost et al., various years) which results' possibly should not remain to be unmentioned here. All these, in my eyes, belong to the introduction. Here you motivate why you think that you approach brings added value to the research landscape in this topic. - The RRDP data set is a co-production of ESA and EU (SPICES project) activities and this needs to be mentioned. Also, to my knowledge, this data set has been published and is citable with

a doi. You might want to ask Leif Toudal Pedersen about this. - It should be mentioned that routinely processed data sets of snow depth and snow-ice interface temperature exist. It makes sense to not only check out the NSIDC data holding but also activites at JAXA and other institutions (metno for instance).

P2, L26/27: I guess it would not hurt to at least mention the substantial emissivity differences between first-year ice and multiyear ice here, i.e. the sensitivity to ice type.

P2, L31: On the one hand the "relationship ... is complicated at microwave frequencies > 18GHz" ... on the other hand "from 6 to 50 GHz [i.e. including those complicated frequencies] there is a high correlation between Teff and Tsnow-ice" This is a bit confusing and should be reformulated. Also the following statement that by "using linear regression" Teff can be estimated contradicts the previous mentioning of a complicated relationship.

P3, L12-15: - Please provide information about the product level which is used in this product and also detaiil whether swath data or gridded data are used. - For the co-location with the IMB as well as the OIB data sets it is important to know the search radius in space and time within which an IMB or OIB measurement is co-located with a satellite measurement.

P3, L15: See one of my previous comments. There should be a DOI and citable reference.

P3, L16-25: - 10 cm vertical resolution of the temperature measurements sounds a bit coarse. Please check whether there are not other (finer) vertical resolutions in different media. - How does the acoustic sounder penetrate the snow to measure the location of the snow-ice interface at 5 mm accuracy. So far I thought that these IMBs have an acoustic sounder looking downward to measure the position of the snow surface relative to the sounder and an acoustic sounder underneath the sea ice looking upwards, measuring the position of the ice underside; both together provides the total (sea ice + snow) thickness. The temperature measurements in the snow and sea ice are then used to figure out where (approximately) the snow-ice interface is located. - How are IMB measurements co-located? What is the sampling frequency? Was there any averaging performed?

P3, L26-31: - Please provide references about the OIB data and/or the OIB campaign. - The 408 observations ... are these the 50 km sections? Do these overlap or are these consecutive sections? - Why did you use data from 2013 only? - You give precision / accuracy estimates for the IMBs but not for the OIB data. Please provide such as well for OIB.

P3, L33: "neither interpolated or smoothed ..." okay. What is the sampling in time?

P4, Table 1: - What is the mean snow depth and ice thickness given in the last two columns? Is this an average over the entire time period the buoys lived ... or over the time period from which you used the data ... or are these the initial depth and thickness values at buoy deployment? Please be more specific. - The time periods given in the second column do not last from Dec. 1 to Apr. 1. Why?

P4, L1-2: I don't understand what you want to say with this sentence. I do not rate a difference of 1-4 K over 100km as a particularly good example to state something about how variable data from adjacent buoys could be.

P5, L2-5: This is a very short description. What are the skills and limitations of this model to simulate TBs and Teff for snow-covered sea ice? Can the model deal with liquid water in the snow and/or with melt-refreeze cycles? For which microwave frequencies and polarization the model can be applied? What are the input atmospheric data? Even though the simulations were part of an earlier study it would be very helpful to have some key elements listed here.

P5, L13-19: This summary part is not very clear. - What is "forward selection"? - IMB snow depth is expressed as a function of TB using multilinear regression. ... then the IMB training data set is used to perform the regression ... ??? - "centred

(avarage was subtracted)" –> I don't understand this. Centred between what? Which average was substracted? - What TB dependence are the TSnow-ice values corrected for? - Which snow depth data set is used here? The IMB training one? - Perhaps a schematic illustration with the data flow and the different regression steps would ease understanding of your method.

P5, L25 to P6, L6: - Please explain why you use the OIB product with the much better spatial coverage and hence representation of the satellite footprint conditions only for the forward selection. Would it have been more straightforward and logical to carry out both, the forward selection AND the regression using the OIB data? What is the added value using the IMB snow depth values? - Please provide an additional table in which the results of the statistical forward selection are summarized. - Please explain the statistical measures used in the forward selection. May I ask whether you tried all frequency and polarization combinations? How many in total did you try? - Please provide at least an example, e.g. a scatterplot or 2-dimensional histogram, in which you illustrate the relationship between the 3 channels used for the best retrieval and the OIB snow depth data. It would be very intriguing to see how much the measurements scatter around the regression lines.

P7, L7-12: - It is not entirely clear what were the input and the test data sets for these additional tests of the regression. Please be more specific about what you did. This goes back to the a schematic illustration which is missing. - How does the multi-linear regression work? Is it a stepwise linear regression? If not, how do you / the method assures that with the choosen parameter combination you end up in a minimum of the multi-dimensional RMSE "surface" (optimal parameter combination)? - Is there any uncertainty involved in your parameter estimation? Or, in other words, what is the uncertainty of the SD retrieved with equation (1) based on the multilinear regression?

P6, L14-20: - "snow depth estimate from MandC98" –> Did you compute this on your own? If yes, with which coefficients? If not, where did you take the snow depth information from? Without more detailed information about this it is not possible to properly evaluate the quality of your results. - What is the basis (in terms of time) for the intercomparison presented in this and the following paragraph? Are we talking about a 4-month average value?

P6, L21-27: - What do we know about the limitations of the MandC98 approach in terms of snow depth? - In L23/24 you kind of contradict your statement from L19/20. Please check. - I doubt that this particular buoy is located ON a ridge or hummock. In that case the local snow depth would probably not be very large because it can be expected that the wind blows the snow off the ridge and hummock. Maybe you wanted to write "nearby a ridge or hummock"? In that case your statement would be making more sense, I guess. Please check your hypothesis. - What happens with RMSE and correlation for MandC98 when skipping the data from 2013F? Please provide.

P6, L28 until P7, L2: - "uncertainties on OIB data" –> could you be a bit more specific what you mean here? How did you derive the uncertainties of the OIB data? Are these included in the product? Or are you referring to the difference between the satellite snow depth retrievals and the OIB data? - In this paragraph, as well as in the previous one and in Table 2 you are using the RMSE. In Figures 2 & 3 one can see that the difference between the IMB or OIB data on the one hand and the satellite data on the other hand can be quite large and therefore determine the RMSE. Did you try to compute an unbiased RMSE as well, by first subtracting the bias and then computing the RMSE? It might be worth a try. - The IMB data contain timeseries of snow depth which is derived from a relatively precise measurement of the location of the snow surface relative to the sounder (the downward looking acoustic sounder) and a relatively imprecise measurement of the ice-snow interface location by a temperature gradient method (to be described later in this paper apparently). Did you check the snow depth estimated from these two kinds of IMB measurements with the other data in the RRDP data set: precipitation (amount and type?) from ERA-Interim? It might be worth to do that to get a better feeling and the quality of the IMB snow depth data time series. - "Spatial scales are different" ... "the correlation is higher ..." –> yes, indeed the

scales are different. You could attempt to plot a typical satellite footprint and then, try to overplot in scale a typical OIB measurement and a typical acoustic sounder footprint. If you cannot visualize it, then it might help to quantify the difference scales again in this sentence. Another important thing which needs to be taken into account when understanding the statistics of the different data sets used is the temporal sampling which is yet not mentioned for the IMB data and which you did not specify further for the OIB data. One could argue that it is not the pure difference in spatial resolution but also and in particular the vast difference in single observations entering the one value compared between IMB, OIB and satellite data.

Table 2: - I suggest that you add the mean snow depth values as well as the standard deviation of the respective data set. The latter helps to figure out whether the data sets compared have a comparable statistics. - Am I correct assuming that the data shown in this table are only containing those IMB data which you did NOT use for the training of the method? If not that you could perhaps consider to leave these out and redo the computation. In any case it would be important to mention in the caption of the table data from which IMBs are included here.

Figure 2: - Is the length of the time series at the same scale for all IMBs? - You are also presenting the comparisong between the satellite data and the IMBs for the training data set. Is this done on purpose? - The box in the top right, annotating the figure, should be placed outside to see the full range of MandC98 snow depths. - In any case Figure 2 contains a lot information for discussion. For instance: There is not much varition in IMB snow depth for all 4 IMBs except a small step change for 2012G and a large step change for 2012H. While for 2012G both CandM98 and your approack agree with each other perfectly well, both show a clearly increasing snow depth for the other 3 IMBs, CandM98 more than your approach, an increase which in this form is not confirmed by the IMBs. - For 2013F, 2014D and 2004I your approach looks like an amplitude-dampened version of MandC98. Most of the ups and downs in the Mandc98 time series are also present in your snow depth time series. What do you think causes

the fact that the amplitude in short-terms (possibly unwanted snow depth variability) is so much smaller for your approach compared to that of MandC98? - How realistic do you think are step changes in IMB snow depth of 5 cm snow depth DECREASE as observed for 2013F and 2013G?

Figure 3: - While I doubt that an additional scatterplot with regression lines superposed does make sense for the IMB data sets I strongly recommend to add such kind of a figure here. For that it would be very good to obtain an estimate of the OIB snow depth retrieval uncertainty from the RRDP people and to estimate and uncertainty of both the MandC98 data and your approach, based on the uncertainties of the input data. Such a figure would add substantial value to the time series shown in Figure 3. - It might make sense to indicate in Figure 3 where data are over first-year ice and where over multiyear ice. - Important for Figure 2 and Figure 3 and in general all results which include MandC98 data is more information about how you used this data, i.e. whether you computed the snow depths on your own, whether you applied filters and if yes which, or whether you simply took the data out of a data base. This is important because in the products issued by NSIDC there are certain flags which, for instance flag multiyear ice because the MandC98 retrieval does not work properly there.

P7, L5 until P8, L4: - "nearly piecewise linear" sounds strange. I suggest to either write "nearly linear" or "piecewise linear". Any complicated curved profile one can approximate piecewise linear. I am not sure that this is what you wanted to express here. - "because of turbulent mixing" –> well, ok, but what if this is not existent? Then you have a strong air temperature gradient near the surface. - Is the gradient of 35K/m a typical value for the temperature gradient in snow? If so - do you have a reference? If not, then it might make sense to specify this a bit more here. Otherwise you might make the wrong assumptions in the subsequent analysis.

P8, L5-8 / Figure 4: - Only 2 of the IMBs you used show an average snow depth subtantially larger than 20-25 cm, i.e. in only two of the IMBs temperature profiles you will have more than just 2 or 3 locations where the temperature is sampled. This

does not sound a very safe method. - Figure 4 places the measurement locations exactly at the air-snow and ice-snow interfaces. I doubt that this is the case in reality. Please comment on that in the text - Figure 4 also reveals that the air-snow interface can be located quite accurately - at least in the shown setting - because the gradient change at this interface is indeed quite large. At the snow-ice interface however, the change in gradient is much smaller and almost not detectable - at least the way you plotted Figure 4. In other words, Figure 4 is not ideal to support / illustrate your method to derive the snow-ice interface from the IMB temperature profile data. See also my comment to Figure 5. - "if sea ice starts to melt" –> The isothermal state is something which is reached well after surface melt has commenced, am I right? It is hence first the temperature profile in the snow which changes before there is an isothermal state in the sea ice to be expected.

P9, L2-5: - "thermistor at the snow-ice interface" –> Please provide this detail - if confirmed by references - in the data section. It is an important detail. - "detected with our automated method" –> Did you also evaluate the success / skill of this method and can you provide a measure of its uncertainty? I'd say it is essential to know this because the high precision with which the acoustic sounder measures the location of the snow surface of 5 mm is kind of useless without knowing what the potential bias of your method to locate the ice-snow interface is. Looking at Figure 4 and the description of your method it is certainly fair to assume that a bias of 5 cm might not be uncommon.

P9, L6-12: - 89GHz TBs are highly correlated with the air-temperature –> you don't show this in any of the figures, am I right? What is this statement for? Does it add value and is it relevant for the outcome of the paper? If relevant - How well are TBs of the other frequencies correlated with the air temperature? - Yes, at 18.7 to 36.5 GHz there might be some scattering of microwave radiation in the snow. Actually it differs between 18.7 GHz and 36.5 GHz that much that it form the basis for the snow depth retrieval of the MandC98 approach. Did you know that? I would therefore - particularly because one can properly derive snow depths up to 40-50 cm depth not say that at these frequencies one has shallow penetration into the snow. I'd rather state that for all but one of your IMB penetration at these frequencies is deep enough to properly retrieve the snow depth. I suggest to reformulate this sentence therefore to avoid contradiction and misunderstandings. - "7.3 GHz is ignored" –> but you show it in Figure 5 nevertheless. Why? - Please try to provide an explanation why the horizontally polarized channels at $\sim$7 and $\sim$11GHz have a substantially lower correlation with Tsnow-ice. - It is more than likely that at these two frequencies (7 and 11 GHz) there is also substantial penetration into the sea ice - particularly if the underlying sea ice is multiyear ice and therefore has a close to zero salinity in its uppermost centimeters to a few decameters. Actually, taking Figure 4 and 5 together suggests that what you retrieve as the ice-snow interface temperature Tsnow-ice is not necessarily exact that temperature but rather a temperature of a sea ice layer underneath - that sea ice layer into which these two low frequency channel data penetrate. –> Temperature of the effective emitting layer.

P9, L14-19: - If I understand your concept of "centred data" correctly then what you basically do is working with anomalies and compute the linear regression between the anomalies of Tsnow-ice and anomalies of the TBs at the two frequencies selected. How valid / representative is in this case your correlation analysis which you based on the absolute values and not on the anomalies. Wouldn't it have been more straightforward to carry out the correlation analysis with the data you will use at the end for your retrieval?

P9, L20/21: - I agree about the dependency of Tsnow-ice on snow depth. I do not understand, however, why you can assume that only the offset of the linear regression changes while the slope is the same for each IMB. If I take Figure 6 and draw a linear regression for each of the four IMBs used I will get different offsets AND different slopes. Please explain. - Maass et al. (2013) is a reference which certainly cites itself older references about the mentioned isolating effect of snow. Could be that the book by Untersteiner is a more appropriate reference here. - Finally: Do we expect a linear

relationship?

Equation 2: I suggest to set up this equation in the same fashion as equation 3. The way done currently is confusing. I would stick with the notation that Tsnow-ice has the form ax + b + c where ax + b are originating from the linear regressions shown in Figure 6 and c is the correction faction based on the snow depth. That you are showing the content of Eq. (2) in Figure 7 is a different thing.

P10, L5-16 / Figure 7: - When I look at Figure 7 I do not necessarily "buy" that using the inverse SD leads to an underestimation of small snow depths. I would say that the majority of data pairs of IMB 2012L fits better to the 1/SD than to the log(SD) curve. The same could be said for 2013F and the SD curve. I suggest to first remove outliers and then compute the RMSD between the fitting curve and the SD values for each IMB for each of the three fits used to have a more objective measure of the skills of the fits. These values can easily be compiled in a Table. - By the same token I recommend to discuss the physical background using these different fits. Is there perhaps evidence that one of these is particularly suitable given what we know about the interaction of microwave radiation and snow on sea ice as well as about the relationship between microwave radiation, penetration depth and Tsnow-ice? - I would highlight in the caption of Figure 7 and once more in the text that IMBs from 2012 serve as training data and that IMB data from 2013 and 2014 are independent and serve as kind of a quality check of the fits shown in Figure 7. You might even want to highlight this by choosing either different symbols or different symbol sizes. - Finally, I guess you need to explain in a bit more detail how you switch from the linear regressions given on Page 9, Lines 18/19 to equation 2 and 3 because of three (addition to my comment farther up about why only the offset changes) reasons. 1) What happens to the offset of 0.020 and 0.019? 2) The regressions obtained from Figure 6 are computed using the TB and Tsnow-ice anomalies. If I am not mistaken, you need to use the TB anomalies in Equations 2 and 3 as well then ... this is not clear. It is particularly not clear whether the Tsnow-ice value obtained with equations 3 and 4 is just the anomaly or the "absolute" value and if the latter, where is the switch where you step back from anomaly to absolute value? 3) The snow depth you are using here ... is this the one you obtained yourself with Eq. 1 or is this (has this to be) an independent, externally provided snow depth? If it is the snow depth from Eq. 1 then at least Eq. 4 are not independent as both contain in some way information of the 6 GHz channel.

P10, L18 until P11, L7 / Figure 8 + 9: - Figure 8 and 9 only partly answer my point (3) farther up whether you need an independent (external) snow depth estimate or you can use the one retrieved with your method. - I guess it is important to discuss Figure 8 and 9 in detail. Figure 8 uses the IMB observed (or better derived) snow depth. The agreement between computed and observed (better estimated) Tsnow-ice is certainly better than in Figure 9. This needs to be stated - potentiall also in form of mean differences and standard deviations in a separate table. - I cannot see in Fig. 8 that 2012L is particularly bad. It is actually together with 2012H the IMB with the best agreement. 2012G has a positve bias (Tsnow-ice retrieved > Tsnow-ice "observed"), 2012J a negative one. 2013F and 2014F both have a negative bias while 2013G and especially 2014I have a positive bias. Is this reflected by Figure 7? - Please remain critical. Do you believe in the decrease in IMB Tsnow-ice for 2014I to -20degC until the end of the period at a mean snow depth of > 20 cm? - Is the difference between Figure 8 and 9 for 2013F and 2014I in line with the differences in retrieved snow depth versus IMB snow depth? The negative (2013F) and positive (2014I) biases become larger when going from Fig. 8 to 9. Hence the regressed snow depth has to be larger than IMB snow depth for 2013F and smaller than IMB snow depth for 2014I. Is this the case? - Since IMB snow depth estimation requires IMB Tsnow-ice, these two quantities are not independent. How useful is it then, to compare a remote sensing product which uses IMB snow depth (as a function of IMB Tsnow-ice) with the IMB Tsnow-ice itself?

P12, L3-8: - What is the ultimate goal to compare model results, which are seemingly completely independent of the observations in terms of ice type, snow depth /accumulation, and time period used (?), with your estimations of Tsnow-ice. Please provide

1-2 introductory sentences. Otherwise it pretty much sounds like comparing apples with oranges.

P12, L9-12: - Please use the same number of digits: 2.7 K and 2.1K instead of 2.07K. - The bias-corrected regressed Tsnow-ice values show a larger difference between 10V and 6V than found in the previous section. Why? What could be the reason? Is it because the model is capable to handle the relationship between the frequency-dependent penetration depth into the sea ice underneath the snow-ice interface and Tsnow-ice better than your estimations based on IMB-data based estimates of Tsnow-ice and its correlation with the TBs at the respective frequencies? (See my comment to figure 5).

P12, L13-15 / Figure 10: - In contrast to Figure 6 you use absolute TB and Tsnow-ice values here - while for the regressions shown in Figures 3 and 4 you (at least partly) used TB anomalies? Please explain i) why you can use the absolute values here and ii) why it is possible to use equations 3 and 4 also for the absolute values.

P13, Figure 11: Please check the caption; "at different frequencies" does not apply to the figure shown.

P13, L1/L6: "50V" ? Perhaps you write on P12, L17: "50 GHz at vertical polarization (50V)"? Then you have introduced this acronym.

P13, L3/4: I am not sure I would term this behaviour "sensitivity". It is possibly better to state - like you partly did - that if the slope of the regression is < 1 then Teff originates from below the snow-ice interface while when the regression is > 1 then Teff originates from above the snow-ice interface. This makes pretty much sense given the smaller penetration depth into snow and sea ice at 89GHz compared to the lower frequencies, i.e. 10GHz or 6 GHz.

P13, L7-9: "These linear regressions ... to retrieve the Teff ..." –> ok ... but how? Now we are at the point where I, as the reader, would like to see the "final" equation with which I can compute Teff based on (which?) TB with (which?) external or additional input data ... Here the paper kind of stops and does not go further ahead. Why? –> GC3

P14, Table 3: Please state in the caption what the source for Tsnow-ice and Teff are.

P14, L2-8 / Figure 12 - Why do you use SIC from a weather forecast model? This is not understandable given the multitude of products available in Bremen. - You use Eq. 3 and hence first need to compute the TB anomalies ...? - Am I right in assuming that the snow-depth input into Eq. 3 is the one computed with Eq. 1 and shown in the first row of Figure 12? - You use and show a multiyear ice concentration product ... why? Is this to demonstrate / illustrate that your approach is able to compute snow depth over multiyear ice as well? While it is certainly a valuable product one gets the impression that the multiyear ice area increases during winter. Even ice drift seems not capable to explain the substantial spread of multiyear ice into the Eastern Arctic Ocean. - What is the cut-off MYI concentration value used in Figure 12, last row? In other words: What is the minimum MYI concentration displayed? It seems not to be 1%. - Please provide a measure of the actual ice cover - for instance by providing the 15% sea-ice concentration isoline in all 9 images of Figure 12. - In almost all images in Figure 12 there are tiny, noisy white dots. Where to these come from? Can you remove them?

P14, L9-13 / Figure 12 - This paragraph needs to be rewritten. I have difficulties to follow the justifications about the larger snow depth and snow depth evolution north of Greenland and the Canadian Arctic Archipelago. Yes, we know Warren et al. (1999) but there are more recent papers to check that out. Since you have been using OIB snow depth data it would be fairly easy to look into respective papers (Webster et al.) in which these data were analysed and discussed. There has also been a recent update of the Warren et al. (1999) climatology by Shalina and Sandven. Even though its data are from the past as well it is certainly worth to take a look. In addition, since the paper lacks so far the justification why - now with the new regression - also snow depth retrieval over multiyear ice is potentially possible, it would be important to get back to

this issue here and to also mention the work done by other members of the group in Bremen (Rostosky et al., Frost et al.). Nothing is specifically stated about the snow depth (quality) in the rest of the Arctic. It is in particularly not understandable why large parts of the first-year ice cover have been omitted.

P14, L14-19: - "$\sim$-30degC" –> How do you know? Arctic wide? Which data source? - I would rethink about the November temperature you mentioned so explicitely. If it is -5 degC then the snow-ice interface temperature is colder than the atmosphere everywhere. - While it is correct that for Jan. and Apr. there are areas where a thick snow cover nicely aligns with warmer Tsnow-ice values there are also regions where a thick snow cover nicely aligns with particularly cold Tsnow-ice. This should be discussed further. - "Note that we can observe ..." –> If this is the case then this would be very confusing and I would strongly recommend to either remove or flag these areas using an appropriate sea-ice concentration threshold - appropriate in the sense that application of the flag allows a Tsnow-ice bias due to the open water of X Kelvin ... X = 2K? ..... Alternatively, you could - as has been done for the original snow depth retrieval (these people were smart) - correct the input TBs for the fraction of open water. Perhaps, by superposing 15% sea-ice concentration isolines on each image of Figure 12 helps to find out where these sea ice margins are located.

Page 15, L1 until P16, L6: - in L2: "highest values" –> of what? - in L6: "Under the same conditions, a higher ice thickness will lead to a lower Tsnow-ice value" –> really? Lets consider a 4 m thick, a 2 m thick and a 1 m thick ice flow, all at -30degC air temperature and all with 10 cm snow on top. Isn't the heat conduction through the snow the main driving factor for the ice-snow interface temperature? - In L1 next page: "positive correlation" ... I suggest to be more careful with this statement unless you can provide evidence that you indeed observe such a correlation by, e.g., picking specific subregions, compute correlations on a daily basis and present time series of these.

GC4

I give no comments to the conclusions yet as they might be rewritten after the revision.

Typos: P2, L1: "reduced" –> "reduces"

P2, L25: "of the" –> "at the"

P2, L26: "in the medium" –> perhaps better: "into the medium"?

P3 L1: "Secion" –> "Section"

P3, L9: "dataset" –> "datasets"

P14, L14 & 16: Add "C" behind the degree sign of the temperature.

P16, L5: "developped" –> "developed"

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