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Interactive comment on "Quantifying the snowmelt-albedo feedback at Neumayer Station, East Antarctica" *by* Constantijn L. Jakobs et al.

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The paper proposed by C.L. Jakobs and colleagues addresses a very important subject that has received little attention for the Antarctic. It aims at demonstrating that snowmelt-albedo feedback is crucial to explain melt dynamics in the coastal Antarctic, which is expected but has never been demonstrated and quantified yet. For this, the paper uses a high-quality and long-term dataset of meteorological conditions from Neumayer station. The dataset is rich enough to allow investigating the surface energy budget in detail and the process underlying the snowmelt-albedo feedback. It is also very long for Antarctic standard (24 years) providing information on long-term changes, with an interesting climate perspective.

The paper is however difficult to read because of the structure, or maybe because some key sentences are missing. The English and style are in contrast excellent. The detailed comments below explain the issues. It is worth noting that they were written while reading the paper for the first time. I decided to keep them as is, despite the fact that some critically missing points were clarified further in the paper. Considering that most readers will read the paper from beginning to end, I think that the order of the comments is helpful to understand the necessary changes. I am optimistic that the authors will solve most of the problems by restructuring the paper and providing the key information early in the paper.

Another issue is the lack of robustness of the results on the feedback with respect to the methodological choices. There are a few questions and suggestions to improve this aspect below, but as a general matter, the paper should be improved by including more comparisons (with results from the literature) and with a proper discussion section putting the results in perspective with respect to other studies having the same aim, but from different regions. Melting snow in the coastal Antarctic is not so different from snow melting in other regions. This should help to consolidate the findings.

Given the great potential of the paper, I encourage the authors to undertake the improvements suggested in the following.

Detailed comments

Abstract: the information about the accumulation is missing to my point of view, in order to put in perspective the 46mm w.e., even though there is no direct link for the very specific objective of the study.

In my opinion, using kg/m2 for precipitation and melt is more correct and less confusing than mm w.e.

P2 L13: "Larger snow grains enhance forward scattering of photons". This is a bit incorrect, as it mixes two perspectives (radiative transfer and photon propagation). I

would say "Larger snow grains has reduced scattering relative to absorption" for a pure RT perspective, or "Larger snow grains reduce backward scattering of photons" for a purely photon propagation perspective. This is a detail.

P3L10: The thickness of the top layer is very high, and I suspect that the power of the snowmelt feedback is highly sensitive to this thickness in the range 1-10mm. Skin temperature can also be very different from temperature in the uppermost 4 cm. Using small layers adds complexity which may be inadequate for regional climate modeling, but the scope of the paper is local and process oriented. It is interesting to assess in such conditions how sensitive is the investigated effect to the numerical layer thickness. Tests should be performed to show how robust the results an conclusion are to the thickness of the uppermost layers.

P3L20-25: This simplification is surprising for a study on snowmelt albedo feedback. The effect of the penetration is precisely maximum in the case of coarse/melt grains as the greater absorption is due to a deeper penetration. This seems to me a too extreme simplification given the topic of the paper and past work in this research group. At the minimum this should be assessed, somehow, by a sensitivity analysis. This is also related to the previous comment on numerical layer thickness. The argument about temperature measurement is weak, as measuring temperature in the first centimeters is anyway nearly impossible and secondly because the effect of the penetration (solid greenhouse effect) can be visible in temperature at depth when high quality surface temperature/meteorological conditions are available, as it is the case here.

P3L30: I would remove the emissivity symbol because this equation is only complete for emissivity of 1 (as assumed here). A more correct equation is LWup = sigma eps Tsˆ4 + (1-eps) LWdown. This significantly reduces the sensitivity to eps (as much as the sky is covered by low clouds) compared to the incomplete equation, so would avoid the first part of the comment in P6L26.

P4L5: The approach is surprising, as explained and justified, but I guess this results

from an unsuccessful attempt to, conventionally, use SWdown ? If not, this should obviously be tested. If yes, a more direct explanation of what has been done should be presented with some developments. In particular, a detection and statistical study of riming would be interesting, if this is an important problem to collect the data, in particular on how it correlates with melt (I intuitively expect a negative correlation). This section is confusing.

P4L20: Since grain growth is very sensitive to liquid water content (cubic power) which comes from the melted mass (constrained by available energy) and the layer thickness, this growth is thus very sensitivity to layer thickness (inverse of cubic power?). Here again I suggest to perform a sensitivity to the numerical layer thickness. Exploring the range 1-5cm should be adequate for this aspect, to stay far from divergence at very small layer thicknesses. I'm afraid this sensitivity analysis could greatly affect the result section. . . and change the paper.

Figure 2: make the individual dots partially transparent (alpha parameter) to better represent the density of dots (or make the dots smaller but the effect is usually better with transparency). The actual representation can be misleading when the number of dots is huge (the case here) and the density is uneven.

Figure 3: The title seems incorrect. Is it right that a sensitivity analysis has been done using a Monte-Carlo approach (chose random pair of z0 and density, run the model and compute RMSE) ? If yes, the graph does not show the relationship between these two parameters, but instead the RMSE and bias as function of both parameters. Still if I understand well, I suggest as a small improvement (for a next paper) to use quasi-random generator instead of pseudo-random. A Voronoi interpolation would also improve the graph. This is not critical.

P6L19-20: This sentence is hard to follow without the formulations. Equations could be added in the method section.

Figure 4: I again suggest transparency on dots + remove non significant numbers for

R2, bias and RMSE (same for Fig 12).

P6L25: Maybe. It could also be a problem of calibration of the radiometer. In such case all the Ts,obs would be scaled down. I suggest to 1) show transparent dots to visualize if these cases are frequent or not, and 2) check that Ts>273.15K occurs mainly for low wind to support the proposed hypothesis of heating. Otherwise, consider to 'recalibrate' the radiometer by scaling down its efficiency to reduce the number of Ts over 273.15K. It may be necessary to use the complete LWup and emissivity close to 0.98 to make this test. Recalibration may lead to a significant effect on snowmelt simulations.

Figure 5: This figure is a bit complex to read despite its apparent simplicity, I have spent some time to understand why the steps and what is the black/red mixture. I suggest to show the grid (vertical dotted gray line on 1st Jan of each year or another way to visualize the summers). The ÂńÂăshaded red areaÂăÂż appears as a line, it would be better to remove it. The necessary info is in the text and is also next to the discussion P6L30-34 which is very good and give a more correct impression of the potential uncertainties than the red area. I'm also wondering about the interest of showing (only) the cumulative melt. I have spent some time to mentally derivate the curve to see the temporal trend and variability (then I realize later it is in Fig 8...). I suggest to add a plot with annual melt along with the cumulative time-series. The measurement error might be more visible on this plot.

Section 3.2. It is relatively disconnected from the remaining. This could be moved to the data section, or at least before Section 3.1

Figure 7: the color is not visible. Is it possible to make wind roses (showing wind speed and direction as e.g. in Champollion et al. 2013 in TC) for 2 or 3 classes of T2m-Ts (e.g. <5 and >5) ? In the end, is the information on temperature so useful ?

P7L19: Is it relative to water or ice ? Relative to ice is more relevant over the ice-sheet.

P7L29: I don't see in Fig 8 and Table 2 that SEB is dominated by SWnet. What does

this mean ? All the plots in Figure 8 have a different y-axis scaling, which makes difficult to judge the dominance of one or another terms.

P8L6: Ts could be shown in Fig 8 (along with Tair).

P8L27: "The difference in SWnet is caused solely by surface albedo". How to exclude the cloudiness as a cause ? Has the LWdown changed between the two years ? More generally how does this interact with the 'unconventional' approach use to compute the SW fluxes. Is it mainly an observational results or an intrinsic consequence of the model and approach ? On a one hand I'm impressed that SW down is equal for both years suggesting that the model predicts the right grain size that perfectly remove the albedo dependence from SWup. However a constant SWdown between both years is only expected if cloudiness has not changed. It is worth checking this, because this is an indirect validation of the approach and of the model grain size.

P9L6: Picard et al. 2012 (doi:10.1038/nclimate1590)Âămay be a useful citation at this point.

P9L17: It is not clear in the data section that SWdown was not excluded (due to riming) and used to compute observe albedo. This Section 4.1 should be moved in the Method section, because it is necessary to understand the previous section results (see my comment P8L27).

P9L25: Picard et al. 2012, Libois et al. 2015 and Picard et al. 2016 provide observed relationship between dry snow albedo and grain size.

P9L27: "to best match the cumulative melt using observed albedo.". I do not understand what has been done. It seems in contradiction with Section 2.2 which indicates that SWdown is not used because unreliable. How to compute valid albedo in these conditions ? In any case this kind of information is required in the method section before the result section Additionally, it seems relevant to show the observed albedo evolution if it exists.

P10L5: CNR4 are given for SZA>60°.

P10L19-20: are these metrics calculated over the summer or the year ?

Section 4.2: From here, I start to understand what I have missed before. It is not clear that the main simulation was done with measurements of SWdown and SWup because the Section 2.2 emphasizes the unconventional approach and the albedo parameterization. I let the previous comments written before reaching this section because they highlight the problem for who reads the paper linearly

Nevertheless, I'm still concerned by the interaction between the approach and the finding of the importance of the snowmelt-albedo feedback. The results seem to entirely rely on the calibration of the metamorphism and albedo parameterizations and their validation is to limited. For instance, over-estimating grain growth in wet conditions automatically leads to over-estimate the importance of SMAF. Ideally, comparison with data from the literature (even on seasonal snow, which is subject to comparable conditions when melting) would help to consolidate a little bit more the result. I was also expecting a discussion section comparing SMAF with the literature.

The discussion at the end of P10 confirms the lack of robustness. The sensitivity to the numerical layer thickness which I propose before is likely to further weaken the findings of this section.

A possible solution is to define SMAF from R0 and R1', where R1' uses the albedo at the end of the winter (and not the annual average of albedo). This would avoid to rely on the grain growth and grain-albedo parameterization, and would be more robust. At least, it should be checked that R1' is close and lower than R3. The main drawback of using R1' is neglecting the dependency on cosine(SZA) which tends to reduce albedo and increase melt during the summer, in parallel with the grain growth.

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