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The Cryosphere Discussions

Interactive comment on "Near-surface climate and surface energy budget of Larsen C ice shelf, Antarctic Peninsula" *by* P. Kuipers Munneke et al.

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We would like to thank Ms. Dadic for her thorough and helpful assessment of our discussion paper. Below, we reply on the comments and suggestions for improvement she offered us in her review. 'Q' here denotes the queries from the reviewer, 'A' is the response from the authors.

Q: [...] The paper would benefit from a more detailed discussion in some places, such as discussing the implication of the subsurface solar radiation for different seasons (see comments below). Also, it would be interesting to see a discussion about the snow conditions (from the many available snow profiles), which would add very useful

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information for understanding of the surface energy balance.

A: We agree that in a few instances, the topics of subsurface penetration of radiation and changing snow conditions can be improved. It should be noted that the snow pits we dug were solely used for density and liquid water measurements, and as such, they were not snow profiles in the classical sense, i.e. no detailed stratigraphy was made of these pits. However, even these measurements can be used to improve this paper further in a few instances. We deliberately chose not to make this a snow microphysics paper, so we will try to keep the treatment of the snow density profiles limited to the instances where it is relevant for the surface energy budget.

SPECIFIC COMMENTS:

All comments in which textual improvements are suggested will be taken care of in the revised manuscript.

Q: P2668, L27: It is not clear what has been interpolated. Was there a data-gap so the data needed to be interpolated. Or was there some sort of spatial interpolation. Please clarify.

A: This will be clarified. When only ARGOS-transmitted data are available, there may be data gaps since the ARGOS satellites pass over at irregular times, and the signal is not always picked up. This can result in data gaps up to a few hours. The interpolation is therefore temporal.

Q: P2669, L8ff: If wind speeds and conditions at the two AWSs are similar, so I expected the riming to occur at both sites at the same time, which seems to be the case for only a few days. Could it be that while one of the sensors is completely rimed, the other has heavy rime on it, but is still slightly moving? Is there something in the literature about wind speeds and conditions at which those sensor start riming. It would be helpful to write something about the uncertainties that are caused by riming (e.g. slower turning wind vanes) and to estimate the associated uncertainty, if possible.

A: While the meteorological conditions at both stations are similar, riming does not always happen at the same time. Riming is a strongly non-linear effect: once a wind vane stopped circulating, more rime will generally accrete. A small initial difference in riming may cause the wind sensor at one station to stop while the other still continues to rotate. The stopped wind sensor will freeze even more, and it will require quite some solar energy and wind to remove the rime. Therefore, the riming does not occur simultaneously in general. We are not aware of any literature discussing in general terms the conditions for which riming of wind sensors occurs. The wind props are so light and the friction from the bearings so low that partial riming will quickly lead to a complete stop of rotation. It is difficult to estimate the uncertainties due to riming, but we will perform a sensitivity experiment in which the fixed wind speed of 1 m/s during rime events is replaced by a 5 m/s wind.

Q: P2670, L8: It is not clear, in which part of the model the iteration happens. Is it in the calculation of surface/ subsurface temperature? And what does it mean "until the energy budget is closed": is that when the modeled surface temperature is close to the one estimated using LW_up. In that case, I am not if the LW_up, which is used for getting the surface temperature right, should be used to verify the surface temperatures. Please explain in more detail.

A: We will take care of an improved model description in the revised manuscript. The input fluxes for the model are LW_down and the SW fluxes. All other fluxes are a direct or indirect function of surface temperature. By iteration, the model finds the surface temperature for which all energy terms add up to zero. Then, the outgoing longwave radiation corresponding to the surface temperature that was found in the iteration, can be compared to the observed outgoing LW.

Q: P2670, L16: Where do the 100 um come from? Also, it would be interesting if you showed the average density profile and discussed how those 60 pits differ. it is a lot of pits and a lot of very interesting data, that I am sure is worth discussing more in detail.

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A: We did not measure the grain sizes in the snow pits. The 100 um are merely a choice based on the agreement of observed snow temperature with runs where different values for the grain size were used.

Q: P2673, L16ff: It would be interesting to see if the effect that the thicker cloud has on the net shortwave radiation is larger through increased albedo or through reduced incoming shortwave radiation. Also, it would be interesting to see what fraction of the albedo change is caused by the increased melt (and changing of the surface properties) and what fraction by the thicker cloud cover. Consider doing some simple model experiments to show the relative influences of these different processes.

A: In the revised manuscript, we will not only discuss the differences in albedo, but also of the incoming solar flux. This will elucidate the role of these processes.

Q: P2674, L13: This is not always necessary the case. Imagine a cold snowpack, where the incoming shortwave radiation penetrates into the snowpack only to slightly warm up the snow (not bringing it to melting point). Without partitioning the melt, the energy gets concentrated at the surface, increasing the surface temperature to the melting point, and causing melt. Please discuss why in the case of your location, this is not the case (e.g. already warm snowpack). This could also have different effects in spring (when the snowpack is colder) than in summer, when the snowpack in warmer.

A: We thank the reviewer for this addition. We fully agree and adapt the text accordingly.

Q: P2674, L26ff: I is not clear why you assume that subsurface melt does not lead to surface lowering. Please explain.

A: Our manuscript apparently causes some misunderstanding on this point. We will clarify the text to avoid any misinterpretation.

Q: P2675, L3: Considering that you have around 60 pit-observations in January 2011, it is not clear why you need to use a constant (with depth) snow density. You should have quite a good idea about density, which you could use for the comparison with the

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sonic ranger.

A: We will incorporate the different density profiles here.

Q: P2675, L7: It would be interesting if you explained why you have low density snow at the surface. I would think that the melting surface has a higher density than the underlying surface. For this discussion it would be useful to show the snow density profiles that you have obtained. See comment to P2670, L16.

A: Also here, we will insert some discussion on the observed density profiles. It should be noted that the snowpack was a wet and melting snowpack which had been melting for a month already. So the lower density near the surface can probably be explained by the fact that the snow was younger and had not undergone as many melt-refreeze cycles.

Q: P2677, L20–21: It is not only the dries, but also the warmer air that favors sublimation.

A: We agree.

Q: P2678, L7–9: The implications of subsurface solar radiation should be discussed more in detail. See also comment to P2674, L13.

A: We will reiterate the implication of subsurface solar radiation at this place.

Q: P2678, L14: Summer is not necessary calmer. Figure 5 and Table 1 show the opposite, summer is actually windier than winter. Please correct or rephrase.

A: We did not state that summer is calmer than the other seasons. We just note that in summer, there are many calm and cloudy days. In the other seasons, the occurrence of calm and cloudy days is likely similar, or even more frequent.

Q: Figure 5: Could be removed, because it does not have any necessary information and is not important for understanding the paper.

A: We will consider the reviewer's request to remove this graph in the revised version.

Q: Figure 7: It would be interesting to see the same figure for the surface temperature.

A: We will include a panel in this figure showing the surface temperature for these days.

Interactive comment on The Cryosphere Discuss., 5, 2665, 2011.

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