The Cryosphere Discuss., 8, C1779–C1782, 2014
www.the-cryosphere-discuss.net/8/C1779/2014/

The Cryosphere Discussions
Open Access

Interactive comment on "Cloud and precipitation properties from ground-based remote sensing instruments in East Antarctica" *by* I. V. Gorodetskaya et al.

Anonymous Referee #1

Received and published: 10 September 2014

General Impression. This manuscript summarizes atmospheric measurements made at the Princess Elisabeth base in East Antarctica. These measurements of cloud, precipitation, and radiation are some of the first of their kind in Antarctica and certainly hold great value for the Antarctic research community. This paper serves to introduce the measurements and to provide an initial perspective on the types of results that can be drawn from them. In these regards, this paper is certainly worthy of being published. The scientific results themselves are relatively thin, which may be expected for an introductory paper of this nature. I would like to see a little more done to make the results more useful for the scientific community. Most of my comments will be relatively straight forward to address. One of them may take a little additional analysis.

I recommend acceptance of this manuscript subject to these generally minor, and one significant, comments.

Specific Comments 1) There are some places that need better descriptions:

*P4199, top: Should note that this facility is similar to many others globally in N. America, Europe, and elsewhere.

*P4206, line 9. What Z-S relationship was actually used? If not one then why multiple relationships and when are the different ones applied? Please specify exactly what was done.

*P4213, line 9-10. I understand the threshold for identifying liquid water from the ceilometer. But how are mixed-phase conditions identified? Based on earlier discussions in the methods section I think you use the same backscatter threshold. But you must also use some other information to identify that ice is also present?

2) I do not agree with some interpretations and/or I think that certain topics need clarification or further justification.

* p4200, line 8-11. Satellite measurements are indeed an important step forward for cloud observations. However, there are some very important limitations for polar cloud observations from satellite, especially the important low-level clouds. These limitations should not be neglected.

* p4204, line 13-14. This is not exactly true. The effective emission height is at some depth into the cloud that is above the cloud base, even if the full depth of the cloud has an emissivity of 1.

* p4205, line 18-20. Qualitative photographs of ice crystals are not a great way to get their maximum size. Also, at what size of crystals will there start to be non-Rayleigh effects on the MRR?

*p4208, line 13 and forward. I have a hard time believing that “the extended layer of

TCD
8, C1779–C1782, 2014

ice observed below the liquid layer on 7 Feb lead to a substantial increase in Tpyr. . .” Or an increase in LWin (line 24). This is especially true since there was not that much ice mass present; MRR didn't see anything. The text is true that mixed-phase clouds have important radiative effects, but these are typically driven by the liquid water that is present. It is most likely that the variability in Tpyr and LWin during this case was linked to variability in the LWP. Furthermore, the apparent increase in ice production later in the case is probably a consequence of increased LWP, although this last point is purely speculative without actual retrievals of LWP.

*P4213, lines 24 and after. Signal attenuation is important for lidars/ceilometers and prevents viewing of higher cloud layers. What is the impact of attenuation on Fig. 6b? A statement addressing this issue should be added.

*P4217, Line 22 and beyond. Three of the elements in this list refer to surface radiative fluxes and radiative forcing. It is important here to note that this is restricted to only IR or LW radiative effects because that is all that is studied. SW fluxes can also be quite important and typically act in the opposite direction from LW fluxes.

*P4218, line 11-12. Why are liquid-containing clouds only observed in the 1-3 km range? Why are there none observed <1 km? When considering polar clouds in general, there are certainly a lot of liquid-containing clouds <1km. What is special about this location and/or data set? Is there a mechanism that can explain this? Generally the temperatures will be a bit warmer towards the surface, so that is probably not a good explanation. In any case, something clearly needs to be explained about this point because it goes in the face of our general understanding of polar clouds.

*p4218, line 13-14. Ice clouds are not necessarily most frequent in polar regions. There are Arctic data sets that show liquid water occurring just as often as ice, especially in the summer season.

3) Since there is very little information of this nature over Antarctica and East Antarctica, these results will be quite interesting to the research community. With that in mind

TCD
8, C1779–C1782, 2014

it is important that the results are put in the proper context so that they can be most useful. One of the most challenging aspects of the interpretation is the seasonal bias to the data set. How does this impact the overall results? What information is available on winter conditions? Even if it is difficult to quantify the impact of the seasonal bias to the data set, this point should at least be discussed clearly so that these results are not incorrectly taken to describe the full annual cycle of conditions in the region.

4) As argued in the text, attribution of surface accumulation is very important. The paper discusses the different mechanisms for impacting net surface accumulation and argues that snowfall events are the most important for surface accumulation. This could indeed be the case; however, snowfall is estimated from a Z-S power-law relationship that was derived from a different location with likely different snow conditions. There is no effort here to evaluate the quality or applicability of the applied snowfall retrieval. Thus, it is not clear that this is even a good retrieval. What is the retrieval uncertainty? To address this issue in an aggregate sense an accumulation closure analysis should be included in the paper. Give the measured surface accumulation and the knowledge of the other mechanisms, do these equate? How close are they? Do they indeed suggest that the snowfall retrieval is any good? It is entirely conceivable that the snowfall retrieval is significantly wrong. How would that impact the results that distinguish falling from blowing snow? I have a hard time believing these important qualitative results regarding the surface accumulation without a more in depth closure analysis and some sort of evaluation of the retrieval uncertainty.

Technical Corrections.

P4199, line 24 and p4200, line 3: “year-round”

P4202, line 25: “instrument”

P 4212, line 11: “by and large”

Interactive comment on The Cryosphere Discuss., 8, 4195, 2014.