Reply to Reviewer #1:

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 straightforward 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.

We thank the Reviewer for his/her critical review and useful comments, which helped to improve the manuscript. Regarding the comment about the introductory nature of the paper: that was exactly the aim of this manuscript - to introduce the observatory, demonstrate its application for a wide variety of processes (using case studies) and investigate basic cloud and precipitation properties. Other papers already published by our group and colleagues (see the project website http://ees.kuleuven.be/hydrant/pubs_links) and in preparation focus on particular processes. The data are freely available for the research community for further applications.

Below we address each point raised by the Reviewer in detail with our replies in blue and the *corrected/new text highlighted in magenta both in the replies below and in the revised manuscript*.

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.

We agree that such observatories exist around the world, while in Antarctica this is the first of its kind. We have added in the revised Introduction:

"While observatories including similar or more advanced ground-based remote sensing instruments, with long-term measurements of cloud and precipitation properties, exist in glaciated regions of mid latitudes and in the Arctic (e.g., Löhnert et al., 2011; Shupe et al., 2011) this was lacking in Antarctica until now."

*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.

The snowfall rate (S) uncertainty range was first calculated using six relationships derived by Matrosov (2007) for unrimed aggregate snowflakes modeled as spheroids. In the revised version (new Fig. 8 and Table 5), we have added also three relationships from Kulie and Bennartz (2009) for three different unrimed particle shapes (aggregates, three-bullet rosettes and low-density spherical snow particles) derived using recently published backscattering characteristics for each ice habit. These three relations from KB2009 represent the possible range of actually a much larger number of derived Ze-S relations, so we are estimating the Ze-S uncertainty with a large ensemble of particles. In Figure 8, the diamonds represent the mean values based on all Ze-S relationships, while the magenta bars indicate the entire range of retrieved S values (from min to max).

Applying multiple relationships is necessary to cover the uncertainty in S retrievals from radar Ze due to various backscattering properties of different snowfall particles and different particle size distributions. We don't have enough in-situ observations in this area to derive our own PE Ze-S relationship. However, we know that our location in Antarctica should receive dry snowfall, and the typical dry snow habits, which the Ze-S relationships used here are based on, have been observed in Antarctica (Konishi et al. 1998; Walden et al. 2003). Thus we believe that our ensemble of Ze-S relationships for dry snow covers the "true" Ze-S relation. We have clarified this in the revised text (please see the last paragraph of Section 3.2c).

Konishi, H., Wada, M., and Endoh, T.: Seasonal variations of cloud and precipitation at Syowa station, Antarctica, Annals of Glaciology, 27, 597–602, 1998.

Kulie, M. S. and Bennartz, R.: Utilizing Spaceborne Radars to Retrieve Dry Snowfall, Journal of Applied Meteorology and Climatology, 48, 2564–2580, doi:10.1175/2009JAMC2193.1, 2009.

Matrosov, S. Y.: Modeling Backscatter Properties of Snowfall at Millimeter Wavelengths, Journal of the Atmospheric Sciences, 64, 1727–1736, doi:10.1175/JAS3904.1, 2007.

Walden, V. P., Warren, S. G., and Tuttle, E.: Atmospheric Ice Crystals over the Antarc- tic Plateau in Winter, Journal of Applied Meteorology, 42, 1391–1405, doi:10.1175/1520-0450(2003)042<1391:AICOTA>2.0.CO;2, 2003.

*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?

Thank you for raising this very important point. Using a threshold applied to cloudy profiles without snowfall, we can identify if liquid was present. This can be a liquid layer alone without ice or a mixed phase cloud. We know whether these clouds are mixed-phase when doing case studies by the presence of ice in ceilometer backscatter signal below the liquid-containing layer, but we did not attempt to apply an automated algorithm to distinguish liquid-only from mixed-phase clouds for the entire time series analysis in Section 4.2. To make this clear, we use a definition 'liquid-containing' clouds, rather than 'mixed-phase' clouds throughout the revised text (except for the case studies where we did observe mixed-phase cloud structure with ice present below a liquid-containing layer).

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.

We agree and have added a comment to the revised Section 2 supporting it with references: "...*Despite tremendous progress in cloud observations from space, limitations in the characterization of low-level clouds and precipitation persist: they can remain undetected by CloudSat's radar (Marchand et al., 2008; Maahn et al., 2014), while CALIPSO's lidar is rapidly attenuated by cloud liquid water, leaving no information on atmospheric features occurring below the top of the liquid layer (Cesana et al., 2012)."*

Cesana, G., Kay, J. E., Chepfer, H., English, J. M., and de Boer, G.: Ubiquitous low-level liquid-containing Arctic clouds: New observations and climate model constraints from CALIPSO-GOCCP, Geophysical Research Letters, 39, L20 804, doi:10.1029/2012GL053385, 2012.

Maahn, M., Burgard, C., Crewell, S., Gorodetskaya, I. V., Kneifel, S., Lhermitte, S., Van Tricht, K., and van Lipzig, N. P. M.: How does the space-borne radar blind-zone affect derived surface snowfall statistics in polar regions?, Journal of Geophysical Research: Atmospheres, Accepted, doi:10.1002/2014JD022079

Marchand, R., Mace, G. G., Ackerman, T., and Stephens, G.: Hydrometeor Detection Using Cloud- sat — An Earth-Orbiting 94-GHz Cloud Radar, Journal of Atmospheric and Oceanic Technology, 25, 519–533, doi:10.1175/2007JTECHA1006.1.

* 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.

We agree with this precision and have reformulated the 2nd paragraph of Section 3.2b including a more extensive discussion of other factors influencing Tpyr:

"For optically thick clouds the pyrometer temperature (T_{pyr}, °C) is determined by the infrared emission within the lowest part of the cloud. Though this is typically slightly colder than the temperature at cloud base, compensation by atmospheric emission below cloud base allows us to approximate T_{pyr} to the cloud base temperature. At the same time, for clouds residing within a temperature inversion, as frequently occurring in both polar regions (e.g., Mahesh et al., 2001a, b; Sedlar et al., 2012), the cloud emitting layer is shifted towards the cloud top, in which case T_{pyr} will exceed the cloud base temperature. Rathke et al. (2002) showed overall good correspondence between the effective cloud radiative temperatures obtained from the geometric and spectral methods versus the lidar/radiosonde values of cloud base temperatures for Arctic stratus clouds. Deviations were attributed mostly to cloud inhomogeneity, in particular variable cloud water content, and to presence of strong in-cloud temperature gradients. For optically thinner clouds, both the temperature profile and variable cloud optical thickness strongly

influence cloud emitted radiances and therefore the observed T_{pyr}. Measurements in Antarctica show predominance of optically thin clouds (cloud optical depth < 1) over the Antarctic plateau (Mahesh et al., 2001b), and frequent optically thick clouds at the Antarctic coast (Ricchiazzi et al., 1995). Here, we use raw T_{pyr} measurements at 1-min temporal resolution without distinguishing between optically thin and thick clouds."

Mahesh, A., Walden, V. P., and Warren, S. G.: Ground-Based Infrared Remote Sensing of Cloud Properties over the Antarctic Plateau. Part II: Cloud Optical Depths and Particle Sizes, Journal of Applied Meteorology, 40, 1279–1294, 2001b.

Rathke, C., Neshyba, S., Shupe, M., Rowe, P., and Rivers, A.: Radiative and microphysical properties of Arctic stratus clouds from multiangle downwelling infrared radiances, Journal of Geophysical Re- search, 107, 4703, doi:10.1029/2001JD001545, http://doi.wiley.com/10.1029/2001JD001545, 2002.

Ricchiazzi, P., Gautier, C., and Lubin, D.: Cloud scattering optical depth and local surface albedo in the Antarctic : Simultaneous retrieval using ground-based radiometry C0–, Journal of Geophysical Research, 100, 21 091–21 104, 1995.

Sedlar, J., Shupe, M. D., and Tjernström, M.: On the Relationship between Thermodynamic Structure and Cloud Top, and Its Climate Significance in the Arctic, Journal of Cli- mate, 25, 2374–2393, doi:10.1175/JCLI-D-11-00186.1, http://journals.ametsoc.org/doi/abs/10.1175/ JCLI-D-11-00186.1, 2012.

* 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?

Photographs of freshly fallen snowflakes give an idea of their magnitude and shapes. The size limit where snow particles start to deviate from Rayleigh scatterers is not only dependent on size but also on it's habit or density. In the dry Antarctic environment processes enhancing snow particle density like riming or melting are very unlikely and from in-situ observations we know that most snowfall is comprised by low-density snow particles. For this kind of snow it can be expected from scattering theory that even larger snow particle sizes still behave as Rayleigh scatterers. In any case, these different scattering properties are covered in our selected Ze-S relationships, which include a wide range of habits, snow particle densities and PSDs. We only wanted to express that the snowfall particles observed at PE station (from photographing freshly fallen snow particles when possible) are in general not systematically different from the typical low-density Antarctic snowfall, supporting the argumentation that the usual dry-snow Ze-S relations can be applied to our observations.

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

We agree and have reformulated the statements in Section 4.1.2 as following:
"The extended layer of ice observed below the liquid layer on 7 February occurred together with a substantial increase in T_{pyr}*."...*
" LW_{in} *was also increased during the presence of an extended ice layer below the liquid layer on 7 February. Simultaneously enhanced ice production below a liquid layer and increased* T_{pyr} *and* LW_{in} *could be manifestations of increased cloud liquid water path."*

*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.

We have extended our statement about decreased sensitivity of ceilometer with height in Section 4.2.1:
"The decrease of frequency of occurrence with height for the low β_a *values can be related, as for the radar, to the decreasing sensitivity of ceilometer with height leading to a possible underestimation of high optically thin ice clouds (Van Tricht et al., 2014)."*

*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.

We have reformulated that the list rather shows *"the potential of the observatory"* for investigating both SW and LW radiative fluxes. We do show the effects of cloud properties on net SW fluxes (Fig. 4a), but don't investigate cloud radiative forcing in detail (this is the subject of our current research).

*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.

Indeed, observations in the Arctic show frequent liquid-containing cloud occurrence near the surface. However, our statistics concern the liquid-containing cloud LAYERS, and in the case of mixed-phase clouds the ice layer found below these liquid-containing layers can extend close to the surface. We have clarified this in the revised text (section 4.2.1):

" Combining these results with Fig. 6c points to a large vertical range of the ice layers, including both ice-only clouds and mixed phase clouds. As demonstrated by the case

studies (section 4.1), mixed-phase clouds at PE with liquid- containing layers above 1 km can have ice layers extending close to the surface."

A deeper investigation is required to understand mechanisms behind mixed-phase cloud properties at PE and is beyond the goals of this introductory paper. Preliminary analysis shows that location of liquid-containing cloud layers at 1-3 km agl can be related to the specifics of moisture advection to PE. Isentropic analysis of specific humidity (using ERA-Interim re-analysis data) showed that moisture fluxes tend to be stronger at higher levels (285 K isentropic surface compared to 275 K surface) and related to lower latitude moisture sources. Also, fast building-up of strong near-surface temperature inversions at PE can favor decoupling of the liquid-containing clouds from the surface.

*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.

We agree with this remark and deleted this statement, adding instead a comparison to the liquid-containing clouds occurrence in the Arctic to the revised Section 4.2.2:

" The frequency of liquid-containing clouds at PE is lower than found at various Arctic locations (32-56%, Shupe, 2011), or at Greenland summit (from 10% in winter up to 40-60% during summer, Shupe et al., 2013; Van Tricht et al., 2014)."

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 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.

Measurements gaps each winter due to the PE base power failures have been a real issue. The data availability has been already noted in the beginning of Section 4.2.1 *("...we provide a compilation of basic cloud and precipitation statistics derived from the remote-sensing instruments based on the available measurement periods during 2010–2013 (14 months for cloud measurements mainly in summer/autumn and 26 months of snowfall measurements including an entire year, see Table 1 for exact measurement periods for each instrument)*." We have also added a statement in the revised Conclusions that cloud statistics are *"restricted to summer and autumn/beginning of winter)."*

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.

As we already stated above, the snowfall rate (S) uncertainty range is now calculated using nine relationships for dry (unrimed) snow that is typical in Antarctica: six relationships derived by Matrosov (2007) for unrimed aggregate snowflakes modeled as spheroids and three relationships from Kulie and Bennartz (2009) for aggregates, three-bullet rosettes and low-density spherical snow particles. New Figure 8 shows much larger uncertainty compared to using only Matrosov 2007 relationships. The typical snow habits used by both M07 and KB2009 have been observed in Antarctica at Syowa and South Pole (Konishi et al. 1998; Walden et al. 2003). Although we don't have enough in-situ observations in our study region to derive our own PE Ze-S relationship, we believe that the chosen ensemble of Ze-S relationships for dry snow covers the "true" Ze-S relation, i.e. the real S lies within the uncertainty range for each snowfall event (new Fig. 8).

Further, we have addressed the request of the Reviewer to consider accumulation closure and compare other surface mass balance (SMB) components with snowfall retrievals (new Section 4.2.4). That was possible for 2012, when radar snowfall measurements were available continuously. At PE, SMB (accumulated snow height changes, *SH*) = *S* - *SUs* - *SUds* - *ERds*, where *S* is snowfall, *SUs* and *SUds* are surface and drifting snow sublimation, respectively, and *ERds* is the wind-driven erosion/deposition of drifting snow. *SUs, SUds* and *ERds* terms are typically negative (snow removal), but can be also positive (deposition) under specific conditions. Net SMB (*SH*) is measured by the AWS sonic height ranger. Sublimation terms are calculated using the AWS measurements as described by Thiery et al. (2012). Using radar-derived *S*, we estimate *ERds* as a residual SMB term. In the new section 4.2.4, we present year-total estimates of all SMB components giving their uncertainties, as well as cumulative daily evolution of SMB component during the year 2012 (new figure 9).

As was stated by many publications (e.g., Frezzotti et al. 2013, Groot Zwaaftnik et al. 2013, Thiery et al. 2012), the major challenge in polar science is attributing local snow accumulation to precipitation and/or the wind-driven snow erosion/deposition. This is especially true at PE, where snowfall is almost always accompanied by strong winds. Using the radar-measured snowfall rates at high temporal resolution is an important step in understanding SMB changes, despite the existing uncertainty in *Ze-S* relationships. At the same time, precise estimation of local wind-driven snow removal term (*ERds*) and thus a real closure analysis is not possible using only point AWS measurements and requires high-resolution regional climate modeling resolving snow transport and associated processes (which is beyond the goals of this study).

New and/or corrected text related to this subject can be found in the following sections of the revised manuscript: Abstract, Section 3.2c, Section 4.2.3, Section 4.2.4 (new section), and Conclusions. Figure 8 has been updated now showing a full range of expected S values. New Figure 9 has been added showing cumulative SMB components during 2012.

Frezzotti, M., Scarchilli, C., Becagli, S., Proposito, M., and Urbini, S.: A synthesis of the Antarctic surface mass balance during the last 800 yr, The Cryosphere, 7, 303–319, doi:10.5194/tc-7-303-2013, 2013.

Groot Zwaaftink, C. D., Cagnati, A., Crepaz, A., Fierz, C., Macelloni, G., Valt, M., and Lehning, M.: Event-driven deposition of snow on the Antarctic Plateau: analyzing field measurements with SNOWPACK, The Cryosphere, 7, 333–347, doi:10.5194/tc-7-333-2013, 2013.

Thiery, W., Gorodetskaya, I. V., Bintanja, R., Van Lipzig, N. P. M., Van den Broeke, M. R., Reijmer, C. H., and Kuipers Munneke, P.: Surface and snowdrift sublimation at Princess Elisabeth station, East Antarctica, The Cryosphere, 6, 841–857, doi:10.5194/tc-6-841-2012, 2012.

Technical Corrections. P4199, line 24 and p4200, line 3: “year-round” P4202, line 25: “instrument” P 4212, line 11: “by and large”

All technical corrections have been addressed.

The revised manuscript also contains some additional text improvements highlighted in magenta.