Author's response to "Review" by dr. Déry

W. Thiery, I.V. Gorodetskaya, R. Bintanja, N.P.M. Van Lipzig, M.R. Van den Broeke, C.H. Reijmer, and P. Kuipers Munneke

The authors would like to thank dr. Déry for the time devoted to review the manuscript and for his useful and constructive comments. All comments by the referee were carefully addressed and the manuscript has substantially benefited from the proposed changes. We would like to clarify our changes regarding six comments:

> **General Comment 1**: "It is at times difficult to infer what the study period is. Such information could be clarified in, for instance, the Abstract and the Data and Methods sections. If data are available for up to 13 years at some sites, are the results then presented over the respective periods of data availability (e.g., Table 3)? If so, then the table caption needs to clarify this."

Unless stated otherwise, results presented in the manuscript are indeed for the complete periods of data availability. To clarify this, the periods of data availability were added to the caption of table 3, the Abstract and the Data and methods section.

> **General Comment 2**: "Does snowdrift erosion/deposition affect the surface mass balance of any of the instrumented sites presented in this study? This may be more of a concern for AWS 16 at Princess Elisabeth where the site is sheltered by Utsteinen ridge and may lead to a deceleration of winds and hence blowing snow deposition."

To the author's knowledge, no method exists to quantify the snowdrift erosion/deposition mass flux (ER_{ds}) based on single point observations of meteorological variables. Therefore, in the original manuscript only a qualitative argument was built for the statement that ER_{ds} can be neglected with respect to the solid precipitation (PR) in the residual term ($ER_{ds} + PR$ = 161 mm w.e. yr^{-1} at AWS 16) (P 1503 line25 – P 1504 line 15). If one wishes to quantify ER_{ds}, 2D- or 3D-modelling of snowdrift convergence/divergence including realistic topography is required, but this is beyond the scope of this study. In a recent 3D-modelling study using a regional climate model, Lenaerts and Van den Broeke (2012) predict snow drift deposition of ~10 mm w.e. yr^{-1} for the lee side of the Sør Rondane mountains. However, as local topographic elements such as Utsteinen ridge might affect this value at the AWS site, and since these features are not yet resolved in current modelling studies, it remains at the moment difficult to assess ER_{ds} at AWS 16 itself. We therefore believe that future research is useful in this context. To clarify this in the manuscript, section 3.3 and the conclusion were adaptded.

> **Specific comment 3**: "P. 1495, line 11: Rephrase this sentence to something as "The Princess Elisabeth AWE was deployed on…" Why was the AWS deployed in the shelter of Utsteinen ridge rather than in an open area?"

The considered sentence was rephrased. The location of the AWS 16 (300 m east of the Utsteinen ridge) was chosen considering that it should be (i) reachable but still far enough from the scientific station Princess Elisabeth to avoid disturbance from station activities, (ii) on the upwind side of the station, (iii) not in the tongue of an outlet glacier, (iv) far enough from any rock outcroppings, and (v) in the accumulation area. Since AWS 16 is part of a larger project dedicated to cloud and precipitation measurements using ground-based remote sensing instruments installed on the roof of the Princess Elisabeth base, the AWS measurements should be representative of the conditions of the escarpment zone sheltered by the Sør Rondane mountains.

> **Specific comment 7**: "P. 1503, lines 17-18: This sentence is confusing – how can 299 mm w.e. for a 2-year period correspond to 161 mm w.e. yr-1 contribution to the surface mass balance?"

Due to the presence of 2 data gaps at AWS 16, all annual mass fluxes at this station presented in the original manuscript were obtained by dividing the total cumulative mass flux by ~1.85 instead of 2. However, the referee correctly points to the fact that this reasoning does not hold for the net accumulation, since net accumulation during data gaps is accounted for by the total height change during this period. Hence, in the current version of the manuscript we assume zero sublimation mass flux during the data gaps in table 3 and from this consistently compute annual mass fluxes by dividing the respective total cumulative mass fluxes by a factor 2.

> **Specific comment 17**: "P. 1518, Table 3: Perhaps to provide further context to the surface mass balance and energy balance results, could the mean air temperature, relative humidity with respect to ice, and wind speed over the periods of record be added here?"

Inclusion of the mean values for the observational period of these meteorological variables is indeed instructive for the mass and energy balance results. It was opted to include the numbers in table 1, rather than table 3, for two reasons: (i) to preserve the readabilty of table 3 and (ii) because they record climatological characteristics of the location rather than representing a result of the conducted analysis.

> **Specific comment 18**: "P. 1519, Table 4: Could the estimate of the surface mass balance with attempts to fill in gaps in the meteorological data at AWS 16 be added here?"

As suggested by the referee, the numbers were added to table 4.

References

- Lenaerts, J., and Van den Broeke, M.R.: Modelling drifting snow in Antarctica with a regional climate model: 2. Results, Journal of Geophysical research, 117(D05109), 1-11, doi:10.1029/2010JD015419, 2012.