Review of "Water vapor in cold and clean atmosphere: a 3-year data set in the boundary layer of Dome C, East Antarctic Plateau" by Genthon et al.

Our answers in red

This manuscript describes humidity data generated from a tower over Antarctica. This is an important data set in a unique environment. The authors do a good job of describing the relevance of the data. The manuscript is generally well written. It spends most of its time doing basic analysis of the data, with some interesting results. It is quite curious that there is no supersaturation with respect to liquid seen, even if the authors imply it from qualitative observations of possible droplets. I have a few major concerns, and minor comments.

Thanks

My major concerns are:

1. It seems odd to me that the manuscript pays more attention to analysis than to description of the sensors, calibration and errors. That seems appropriate for ESSD, while the scientific analysis perhaps belongs somewhere else. Section 2 should be expanded with more detail on the error characterization.

The description of sensors, calibrations and errors is admittedly limited because the sensors proper are commercial ones (HMP155). Thus basic information on calibrations and accuracies is provided by the manufacturer (Vaisala) and merely reproduced in the paper (section 2, lines 143-144). The dataset which is distributed on PANGAEA in association with the paper is the native data from HMP155, thus manufacturer information reported in section 2 directly applies and we feel there is no need to add much on this.

Section 2 also presents the methods to calculate elaborated data from the native HMP155 data. Obviously, this adds uncertainty on top of the native instrumental uncertainty. We suspect that this is the uncertainty the reviewer is interested in. This cannot be straightforwardly estimated as the added uncertainty results from the empirical formulations of the Clausius Clapeyron relations. There are several of them “on the market” and one way for an order of magnitude of the uncertainty would be to compare the results of different empirical formulations. However, Murphy and Koop report that results from those for the saturation vapor pressure over ice are all within 1% (except one which is not that used in the paper). This is now reported in the text. An additional intercomparison here would not add much.

In the next comment, the reviewer suggest that we use e. g. Murphy and Koop or show several different one, particularly for relative humidity over liquid. We now show and discuss relative humidity with respect over liquid using both GG and Murphy and Koop.

2. In particular, I think the paper should use the best available conversions for water vapor saturation pressure (e.g. Murphy and Koop 2005), or show several different ones. Particularly for Relative Humidity over Liquid.

Please see response to previous comment

3. In general the plot quality for line plots is not that great. As noted, some of the plots are redundant (Figure 7, Figure 10).

Yes, poor quality is due to converting native postscript images into jpg for inclusion in the Word document required to submit the manuscript. If / when the paper is accepted for publication, the optimal quality postscript images will be provided.

The july secondary maximum of temperature in the averaged seasonal cycle (figure 6) shows rather strongly and we think that figure 7 is needed to convince this is a result of interannnual variability in a relatively short record and has not climatological significance. Figure 7 has been reorganized and size reduced.

Figure 10 is removed

4. Some of the plots could be improved. The PDF historgrams at different heights (Figure 5, 13) would be easier to interpret with overlaid transparent filled colors for the bars.

5. There should be more discussion of possible errors in section 2 or in section 4. This seems strange given the discussion of potential supercooled liquid water, but no evidence of RH liquid > 100%.

Yes, there is room for possible errors and uncertainties in the discussions of data elaborated from the native instrumental reports, due to the elaboration process itself. We now somewhat discuss this issue in section 4 . However, a firm assessment of the errors is difficult here as, as Murphy and Koop themselves mention that formulations for vapor over solid are all within 1% and admit for the liquid the limit of any formulation due to “uncertainties on the saturation vapor pressure with respect to liquid at very cold temperatures”, which are not quantified.

6. Links: not sure why the DOI refers to 3 more DOIs, one at each level. But I guess that is the Authors' choice.

The files were initially submitted as one 9-column file (plus time) but PANGAEA suggested to make it 3 separate 3-column files (plus time), one for each level, . We abide with PANGAEA's suggestion, thus the 3 files.

Specific comments:

Page 5, L106: what is this reference to Genthon et al 2022: it's not in the references. Is it supposed to be the data itself?

Right, this should be 2021, now corrected

Page 6, L144: Does the Humicap calibration assume a saturation vapor pressure relationship? Seems like it must in the empirical calibration somewhere. Please elaborate and specify what is used or why it is not used.

Not sure what Humicap assumes but this is definitely an empirical relation, as already stated in section 5. Humicap implemented in HMP155 is used as "black box" here: it is up to Vaisala to devise calibration function, the end user merely measures a voltage which translates into relative humidity with respect to liquid even below 0°C. As a note, Humicaps are routinely used in similar black box mode in radiosondes worldwide.

Page 7, L163: Is the modified sensor in 3b the heated inlet on the right side of figure 1? Please clarify.

No, it is the full set up of figure 1. To correctly measure moisture, one needs both the unheated temperature and the heated temperature and moisture measurement. "instrument" replaced by "set of instruments"

Page 8, L167: does "adapted" mean "heated"?

No it means the full set, see above

Page 9, L181-8: Most of the discussion of mechanisms here is speculative. Do you have data that can bear on this? Figure 3b supports this figure, but not the mechanisms. Maybe show low winds with low temperatures? Or other measures of subsidence? Subsidence would lead to drying, but also warming, so pushing air out of those temperature ranges. Air aloft is going to have a higher potential temperature than surface Air, so without large radiative cooling it would be warmer.

Yes this is speculative. This is 1st of all a data paper. The discussion is meant to illustrate that the new data raise new issues; admittedly we do not solve all issues here. There is no direct measurement of subsidence as this is very slow: subsidence is mainly deduced from conservation issues (e.g. Vignon et al.). Subsidence may have a dominant impact on relative humidity only if lateral air and moisture advection are limited, that is, when the air is coldest: associating subsidence with warm(er than usual) air is thus not that straightforward.

Page 11, L240: Figure 7 doesn't really add new information. Couldn't you add standard deviations as colored shading to figure 6? Then you could do it for RHi and PPW too…

The standard deviation reported figure 7 is that over 10 years of temperature observation, to demonstrate that the July relative maximum has no climatological significance. Reporting on figure 6 could be misleading, as this is not the same time period, 3 years (fig 6) vs 10 years (fig 7). In fact, the reviewer suggests to show the same standard deviation for RHi and PPW as for temperature but this is not possible because we only have 3 years of data for the correct moisture variables. We thus feel it useful to separate what can be done over 10 years (temperature, figure 7) and over 3 years only (moisture), the 3 years of interest here (figure 6).

Page 15, L320: Figure 10. What does this figure show that is different from figure 9? I don't think this figure adds value to the paper and could be removed.

Following similar comment by reviewer 3, this figure has been removed

Page 18, L410: might want to note that since RhL < RHi then if RhL > 100% then RHi > 100%. That's the logic here right? If not, then how do you get supercooled liquid if RhL < 100%?

OK, we add that since RHl < Rhi, if RHl is close to 100% then RHi is necessary over 100%

Page 19, L445: Was the observed haze liquid or ice? That would seem to be an important distinction. Was there ever evidence of supercooled liquid at Dome C?

It is hard to assess whether near-surface haze is liquid or solid. We do not know of direct observation that can answer this question. On the other hand, at higher levels, there is definitely supercooled liquid water above Dome C as evidenced by polarized lidar observations (Ricaud et al., 2020. Supercooled liquid water cloud observed, analysed, and modelled at the top of the planetary boundary layer above Dome C, Antarctica. *ACP* 20 (7), pp.4167-4191. ⟨10.5194/acp-20-4167-2020). Whith similar conditions within and above the boundary layer, one may assume that supercooled liquid haze is likely down to near the surface.

Page 19, L447: if you used a different saturation vapor pressure like Murphy and Koop would you get a different answer? That should reduce conversion inaccuracies.

We now use Murphy and Koop and we do get a similar answer as reported in the new text.

Page 19, L449: But earlier you argued that Dome C was pretty homogeneous? How does that match with the heterogeneity you imply here.

The site setting is very homogeneous on the large scale, the atmosphere is not necessarily on small scales, if only due to the sharp vertical gradients and turbulence from which any vertical mixing can induce local air inhomogeneity

Page 22, L508: This is the same repository and doi as Genthon et al 2021? Just checking that is supposed to be the case. What is the reference to Genthon et al 2022 noted above?

Yes, there was confusion here, corrected