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 (ESSD-2021-414)

(Our answers in red)

General

This paper documents an important new dataset of near-surface humidity profiles from Dome C on the East Antarctic Plateau, obtained using a novel hygrometer previously described by Genthon et al, 2017. Unlike many conventional hygrometers, the new instrument is capable of accurately measuring humidities which are supersaturated with respect to ice. Since ice supersaturation occurs frequently at Dome C this dataset provides for the first time an accurate climatological description of near-surface humidity profiles and their variability on daily to annual time scales at this station.

The paper provides a clear description of the measurements and the means by which they were obtained. Some basic climatological analysis is presented, which gives useful insight into the processes that control humidity at Dome C. I have a few comments on the paper (set out below) that require attention but these are all relatively minor and should all be easily addressed by the authors before final publication.

Thank you John King

Specific comments

1. Throughout the paper, the term “supersaturation” is used without qualification, and is generally used to imply supersaturation with respect to ice. There is thus some ambiguity in the use of this term and I would recommend that it should always be qualified by “wrt ice” or “wrt liquid water” as appropriate unless it is absolutely clear from the context which of these is being meant. Examples of places where clarification is definitely needed include the caption to fig.3, line 167 and line 473.

Agree. In several instances we use the term “supersaturation” to compactly express both senses, wrt ice and wrt water. In those cases we do not change the formulation. In other cases such as those specifically raised here, we reformulate to make it clear.

2. Lines 23-24: Make it clear that these temperatures are for Dome C, not averages for the whole plateau.

Done

3. Lines 78-79: Strictly speaking, the humidity gradient isn't the origin of the turbulence (which is generated by wind shear or convection). Maybe say "...because it enables the calculation of vertical moisture transport and exchange with the surface."?

Formulation is changed to “because gradient is the origin of the fact that turbulence can transport moisture vertically and exchange with surface”

4. Line 163: I'm not sure that this is the "traditional" view. Cloud physicists have known for a long time that ice supersaturation occurs in mixed-phase clouds - it is the basis of the Bergeron-Findeisen process, formulated in the 1930s. However, the occurrence of near-surface supersaturation wrt ice seems to have been largely overlooked until appropriate measurements (King and Anderson, 1999; Genthon et al, 2017) became available).

Right, here meaning, the traditional view in general, outside the specific cloud community which is “at the cutting edge” in the respect. Even in the climate modeling community, just 30 years ago, clouds were parameterized when water content reaches above 100%: there was no provision at all for supersaturation. This is still the case for some models including some participating in IPCC. But yes, this is clearly an outdated point of view. Here “traditional” replace by “old-time”.

5. Figure 8: Is “Hour” local time? Give the difference to UTC.

Yes local time, which is UTC + 8, now reported in the legend.

6. Figure 9: There is very little information on this figure. You could make it more informative. Maybe show seasonal mean profiles?

We suspect that this comment is about figure 10, as an other reviewer made a similar comment on figure 10. The figure is now removed, the information being now conveyed in the text (“we looked at individual times to confirm it is not a result of this averaging”)

7. Lines 326-327: King et al (2001, DOI: https://doi.org/10.3189/172756501781832548) present estimates of water vapour fluxes for Halley from humidity profiles.

OK but not sure what to do with this info. This sentence is to highlight what models do, not to discuss possible methods to calculate fluxes from gradients.

8. Lines 373-374: Not sure what you mean here - that water vapour profiles cannot be used to diagnose convection?

We mean that the information of water vapor profiles is no added value over the temperature gradient to evaluate thermal convection. It does contribute when phase change significantly affect the temperature profiles (in moist convection) but it is not the case here.

9. Lines 475-476: “Thus, it is no wonder…” Do you have a reference that shows that models with appropriate microphysical parametrisations do produce ice supersaturation near the surface at Dome C?

Yes, this is in Genthon et al. 2017, section 4.2 is dedicated to just this. Genthon et al. 2017 is cited.

10. Section 4: Also using a heated hygrometer, King and Anderson (1999) observed frequent ice supersaturation at Halley but no saturation wrt liquid water. They suggested that this indicated that, even in the clean polar air, cloud condensation nuclei, which will initiate droplet formation at very low supersaturations wrt liquid, are relatively abundant, while ice nucleating particles are rare.

OK this is now reported in the text

Minor points and typographical corrections

1. Line 76: “operational”

OK

2. Line 115: Capital “D” for Dome C

OK

3. Line 144: Delete space in “humidity”

OK

4. Line 183: “divergence” (not convergence)

It depends on point of view, here referring to convergence in the free atmosphere, admittedly associated with divergence at the surface

5. Lines 308-310: I don’t understand this, please clarify.

This is slightly rewritten and hopefully now clarified. We mean that he native relative humidity data (such as distributed with the paper) show a profile similar to the middle plot of figure 9. It takes to to transform to partial partial to see that the mean profile is actually conventional

6. Line 335: “upward” (not downward)

OK

7. Lines 467-470: Insert “increasing” before “upward” and “downward”

We don’t think that this is correct: the gradient (vector) is oriented upward or downward, not (necessarily) increasing in any direction.