Review of “Atmospheric moisture supersaturation in the near-surface atmosphere at Dome C, antarctic plateau” by C. Genthon et al. MS No.: acp-2016-670 This manuscript reports surface humidity observations from Concordia station in Antarctica. It intercompares a heated humidity sensor with a frost point hygrometer and then also compares the results to models. The goal is to look at ice supersaturation. There are some comments about isotope effects and surface fluxes and how they might be affected by the results. The paper needs major revision. The data analysis is not complete: there are high values that are eliminated and claimed to be important. I am not convinced that there may not be evaporation of ice crystals in the heated inlet, or blowing snow, leading to anomalously high ice supersaturation measurements. There are also low biases eliminated without explanation why. I think that is because the frost point has a limited dew point, but I am not sure. Also, the effect of ice supersaturation on isotopic fractionation is mentioned as motivation, but there is now real information here, except some passing discussion (which I do not think is correct).

Surface fluxes are also noted as an important reason for measuring near surface ice supersaturation, and some calculations are made, but these show no effect of the difference in ice supersaturation. That null result should be more prominently stated.

Replies and accounting of general comments:

1) High values are not merely eliminated (they are mentioned in the text) but they were not initially illustrated on figures because occurrence is comparatively rare and displaying requires using logarithmic vertical axis which somewhat distorts the overall figure rendering. To answer the reviewer's comment while keeping readable figures, the distributions both to 150 and to 200% are now shown on figure 5, the later with a logarithmic scale. This is reproduced below.

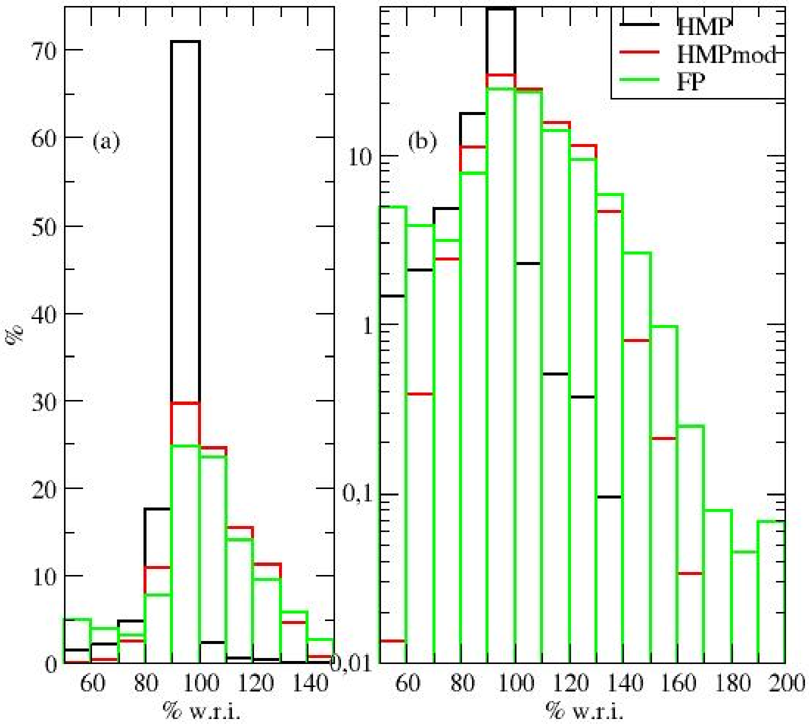

Figure 1: Observed distribution of Rhi in 2015

2) The reviewer raises an important concern which we have admittedly omitted: that some of the supersaturations we measure may be artifacts resulting from the evaporation of solid particles in *the heated inlet. This is a major issue and we do not have measurements of ice particles at the level of the instrument to directly rule this out. However, if supersaturation results from evaporation of blowing snow particles, occurrence of supersaturation should correlate with wind speed as blowing snow only occurs with higher wind speed. Figure 2 below shows that supersaturation actually anticorrelates with wind speed. Strong winds which can erode snow from the surface are associated with the intrusion of oceanic air masses carrying comparatively larger quantities of aerosols preventing supersaturation, thus probably the anticorrelation.*

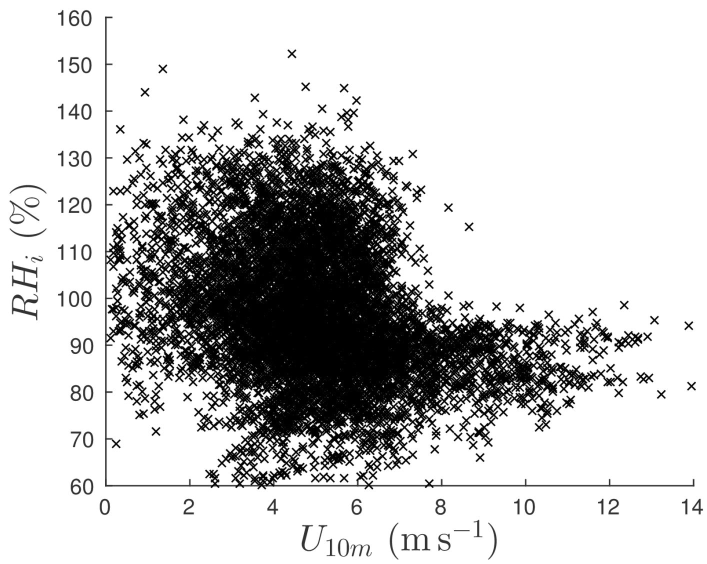

Figure 2: scatter plot of measured Rhi versus measured 10-m wind speed.

Concerning pecipitation particles, even diamond dust strongly affects the sky downwelling IR radiation (Town et al, 2006, Cloud cover over south pole from visual observation, satellite retrieval, and surface based infrared radiation measurement, J. Clim., 20, 544-559; Galllée and Gorodetskaya, 2010, see paper liste of references). Then again, if solid particles from precipitation bias our reports of supersauration, supersaturation should correlate with IR. Figure 3 below shows the opposite. More supersaturation when IR decreases probably largely reflects that relative humidity increases if total humidity is conserved while the atmosphere radiatively cools.

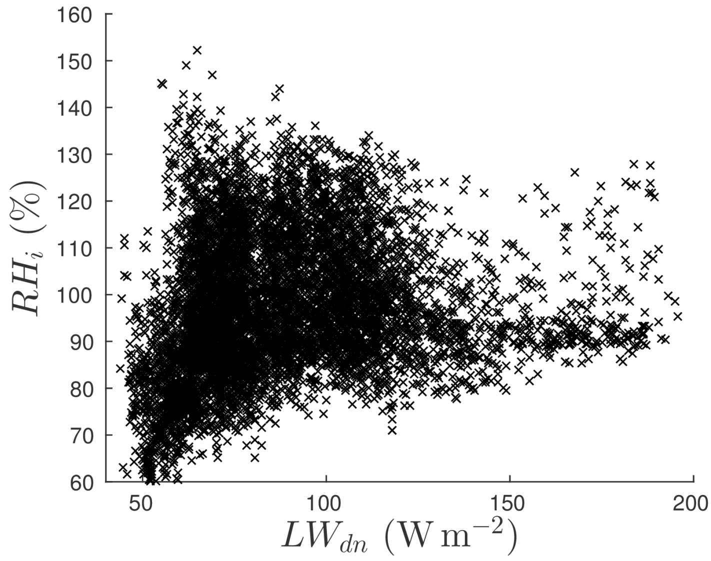

Figure 3: scatter plot of measured Rhi versus measured downward longwave radiation.

The point raised by the reviewer is nonetheless very important and is now discussed in section 2, and although only one of the 2 figures above is shown for the sake of conciseness the 2 anticorrelations in support of a limited impact are now reported.

3) Concerning the unrealistically low values, the reviewer is correct, they are eliminated because they result from the hygrometer having a limited frost point temperature. This limit was initially mentioned in section 2 presenting the instruments and methods but not mirrored in section 3.2 where the unrealistically low values are identified. This is now corrected.

4) Isotopes are admittedly not a major motivation for the present study. It remains that supersaturations may have consequences on the use of water isotopes to interpret ice core recors, a point which we think worth reporting although we do not address it really. As the other reviewers do not complain about our mentioning the water isotopes issue we keep in an abridged for droping all numerics and formula. The reviewer does not detail why he/she "does not think the discussion is correct". We hope the new discussion also resolves this issue.

5) It is not true that ignoring supersaturations has no effect on calculations of sublimation. However,the effect is small and this is prominently stated in the abstract: "This is unlikely to strongly affect estimations of sublimation".

Detailed comments:
Page 2, L15: in general polar regions are an exception, even high latitudes of the S. ocean. It is not just Antarctica.

OK. "An exception" is replaced by "one exception" and In response to reviewer 1 we now discuss observations in coastal Antarctica that also report supersaturations. We thus moderate our statement and report that supersaturations are observed at other places in polar regions.

Page 2, L30: last sentence of abstract is awkward. Maybe state this as an implication of these results?

OK, the sentence is reformulated. This is an implication of the fact that we demonstrate that supersaturations are indeed strong and very frequent at the surface at Dome C, and they can be comparatively easily measured providing extensive samples to test models.

Page 4, L57: ice supersaturation is common at low altitudes at high latitudes, particularly in stable environments.

Although we are not convinced that ice supersaturation is "common" (there is not much litterature that highlights the fact), it certainly occurs at in the high latitudes at low elevation. We have moderated our statement and now report that the antarctic high plateau is "relative" exception. We cite King and Anderson [1999] who report a distribution of relative humidity with significant occurrences of supersaturation at an antarctic station near the coast, and compare our distribution with theirs. We adopt the interpretation by the 1st reviewer that, although there are more ice nuclei at the coast, they may be less active than at Dome C because of the warmer temperature and thus allow significant supersaturation.

Page 4, L60: how close to what tropopause and when? Summer Antarctic conditions still feature mixed phase clouds and supercooled liquid to -30C (Lawson and Gettelman, 2014, PNAS). Be more specific. Lawson, R. Paul, and Andrew Gettelman. "Impact of Antarctic mixed-phase clouds on climate." Proceedings of the National Academy of Sciences 111.51 (2014): 18156- 18161.

This part reads: “Conditions close to the”tropopause are however found over the antarctic ice sheet both in terms of temperature and humidity levels. Because of the distance from the nearest coasts and the high elevation, the antarctic plateau is also particularly secluded from sources of aerosols”. We believe this says it all: there are similarities not only with respect to temperature and humidity but also isolation from sources of impurities (although admittedly not everywhere, as deep and frequent convection brings impurities from the surface in the tropics). The point is that we are discussing a surface atmosphere here, where in situ observations are easier than he upper trosphere and which directly iteracts with the surface. We are not sure here what is the reviewer's point concerning the mixed phase clouds.

Page 5, L84: this is a good point that highlights the uniqueness of Antarctic supersaturation.

OK

Page 6, L112: using direct in situ measurements…

OK

Page 7, L130: has hosted a

OK

Page 9, L177: heating will however evaporate any ice crystals in the air, making this a total water measurement. Do you have a particle counter too? Do you know whether there are particles being evaporated? This is a critical point.

Right, this is a critical point. We do not have a particle counter but we provide arguments above (also now in the paper) in support of a limited impact if any.

Page 12, L243: is accuracy related to the level of RH? I.e. What if it is extremely dry?

The data sheet (http://www.vaisala.fi/Vaisala%20Documents/Brochures%20and%20Datasheets/HMP155-Datasheet-B210752EN-E-LoRes.pdf) specifies different accuracy for different humidity range only for the warmer temperatures. It may be assumed that at cold temperature, the air is quite dry anyway and the main sensitivity is to temperature.

Page 15, L300: all times during the day

OK

Page 15, L303: ,but. (Correct)

OK

Page 15, L309: both of the latter

replaced by “the latter 2” following an other reviewer.

Page 18, L345: if the temperature and humidity do not match (the errors do not match) then is there a process problem with the ECMWF model?

Not necessarily. The relation of humidity with temperature is a complex one both in the real world and in a model, much less direct and much more non linear than a mere Clausius – Clapeyron.

Page 20, L386: what is happening for RHi > 200% ? That seems like an error. Might that affect other measurements below 200%?

We do not think the 200% is an error, but because they are infrequent, it did not seem practical to show the distributions to the extent of the 200% occurrences. This is now done by showing the the distributions both with a linear and a log scale for relative frequency of occurrence (figure 6).

Page 21, L407: has the filtering been done only on the observations? I.e. If the models produce over 150% but the observations do not, has that been reported? It should be reported .

Yes, both models can produce over 150% saturation although the MAR model does that much more (too) frequently. This is now reported, particularly raisong the issue for the MAR model.

Page 21, L414: is the difference because the frost point is too low as the air gets dry?

Yes. The issue is raised when presenting the instruments in section 2, it is now reminded here.

Page 22, L419: but there are also deviations at high moisture content. Why is that?

Deviations are quite small at high moisture content. Some deviations show at intermediate moisture content and we do not know why. It is only in the dryest cases that the deviations kill any significant regression.

Page 22, L425: at odds... Reflect instrument limitations

OK

Page 22, L435: how do you know if it is correct to remove here low points? Is this a problem with the frost point? You should know if you hit the minimum dew point.

This most probably is a problem with the FP as the other hygrometer and the models do not report values any close to these. The FP needs to cool a mirror using a Peltier cell. There is a limit to the cooling a Peltier cell can provide considering how efficient heat extraction is. The hygrometer documentation loosely refers to a -65°C operational limit which we suspect is related to the theoretical ability to cool the mirror down to -65°C but then if the real relative humidity less than 100% the limit can be reached at ambient temperature above -65°C.

Page 25, L453: direct estimates of

OK

Page 29, L540: but you haven’t shown them and filtered them out. Are they an error or not?

They are now shown figure 6.

Page 31, L572: why is the flux wrong? In the previous section you have shown super-saturation does not matter for the surface fluxes. Please explain.

“Wrong” is a bit too strong, this is now replaced by “erroneous”. Even though this makes little difference for the annual integrated turbulent moisture exchange, this results from alternation of frost deposition and sublimation, particularly in summer when sublimation during the day alternates with deposition during the night. At such time scale the issue may be more important.

Page 32, L601 : most isotope schemes in models do account for kinetic effects. I don't know that this discussion of isotopes helps the manuscript very much, you only discuss it in the intro and conclusions.

Isotope models allow for kinetic effects but kinetic effects related to supersaturation can only proceed if supersaturation is there. However, we agree with the reviewer, in the previous version we wrote either too much or to little about water isotopes. We do think it is important to highlight a potential issue for isotopes, but we now reduce this to men,tioning it in the introduction and a quick reminder in the discussion.

Page 32, L604: again, you eliminated these from the analysis and I am not convinced they are not an error. Please show them if you are going to discuss them. What instruments showed this and how do you know it was not blowing snow/ice?

We do show them now on figure 6. We do now provide some evidence that ice particles are not a major issue with our measurements. We nonetheless agree that particularly with the most infrequent extreme values such as 200% RHi an ice particle artifact cannot be ruled out. This is now reported in the new text.

We thank the reviewer for his/her thoughtful remarks and suggestions, particularly with raising the ice particles issue which definitely needs to be acknowledged and adressed.