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Interactive comment on "Atmospheric moisture supersaturation in the near-surface atmosphere at Dome C, antarctic plateau" *by* Christophe Genthon et al.

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General

Making accurate measurements of atmospheric humidity in the cold, dry environment of the Antarctic plateau is challenging. This paper reports new measurements of humidity at a plateau site, Dome C, made using novel instruments that were specifically designed for accurate humidity measurement in this environment. The authors present a humidity climatology for the site and show that large supersaturations with respect to ice are frequently observed. The observations are compared with humidity simulated using both a global and a regional model and significant biases are noted in both models. The impact of the observed supersaturations on calculated surface water vapour fluxes is examined but is found to be small when compared to climatological values of this flux.

The paper is a valuable contribution to our knowledge and understanding of near-surface atmospheric humidity over the high plateau of Antarctica. It is suitable for publication in ACP but I think that it could be improved by some restructuring. I make some suggestions on this below and list a number of other points (mostly minor) that require attention.

Major points

1.) There are three main areas of work presented in the paper: (1) comparison of different techniques for measuring humidity at Dome C, (2) presentation of a humidity climatology for Dome C and (3) use of these measurements to validate humidity in atmospheric models. At the moment, these three topics are presented partly in section 2 and partly across section 3. For example, the poor performance of the FP instrument in all but the warmest months doesn't get mentioned until section 3.2, when the year-round humidity climatology is presented. In my view, it would be more logical to first present the intercomparison of the instruments under all conditions before moving on to present the climatology and, finally, the comparison of the models with observations.

2.) In the conclusions section (lines 539-549) you state that this is the first time that ice supersaturations of up to 200% have been reported in near-surface measurements. While this may be true, King and Anderson (1999) observed occasional ice supersaturations of 150% or more, and a significant frequency of ice supersaturation of 120% or more at the coastal Antarctic station, Halley. Indeed, the climatological frequency distribution of RHice at Dome C (fig. 7a) appears quite similar to that at Halley (see King and Anderson 1999, fig. 2). This might seem surprising as one would expect to see a higher concentration of ice nuclei (IN) at a low-altitude coastal site than at Dome C and hence might expect supersaturations to be significantly lower at Halley. However, the number of active IN is a strong function of temperature (see, e.g. DeMott et al., 2010, doi:10.1073/pnas.0910818107). Thus it is possible that, while aerosol concentrations are lower at Dome C than at Halley, the concentration of active IN at the two sites is similar because of the lower temperatures at Dome C. A comparison of supersaturation statistics for the two stations would make a useful addition to the discussion. Have any direct measurements of aerosol (particularly IN) been made at Dome C?

3.) The methods section (section 2) should include a short description of the ECMWF and MAR models with a particular focus on the representation of cloud microphysics.

4.) There is no further discussion of the biases seen in the models in the discussion and conclusions section (section 4). I appreciate that a detailed analysis of the origins of these biases is beyond the scope of the paper, but some discussion of the significance of these findings is required. It might be worth noting here that incorporating a microphysics scheme that allowed supersaturation significantly improved the performance of the RACMO model over the Antarctic (van Wessem et al, 2014, doi: 10.5194/tc-8-125-2014).

Minor points and typographical corrections

l19: analyses

l23: leaving not living

l52: condense not condensate

l58: “Conditions close to those occurring at the tropopause...”

l88: condenses not condensates

l100: King and Anderson (1999, n.b. not King et al.) report measurements at Halley, which is a coastal (not plateau) station and show that RHi is frequently between 100 and 120% at that station.

l160: condense, not condensate

l171: referred TO as A ....

l174-178: Note that the system described by K&A 1999 did heat the aspirated air.

l288: “completely sets” rather than “really sets”?

l290: Define “night” a bit more precisely?

l309: “latter two”, not “latters”

l311: warmest TIME of the day

l343: night-time

l366 onwards and fig 6: These plots and discussion might fit better earlier in the section – just after fig 3, as they relate to the comparison of the instruments carried out in the first part of this section (see also major point 1).

l413-415: Some discussion of why the FP appears to sometimes give unrealistically low readings at low frost point would be useful. Possibly the air is so clean that a very high supersaturation is required to establish a frost deposit on the mirror?

l425 “at odds”

l430-435: Need to be clear that what is being described here is not a humidity climatology but a conditional climatology for vapour pressure > 2 hPa. l474: “...above thE surface...”

l476: “is written”, not “writes”.

l482: You should include an equation defining L.

l488: “...because OF the very low...”

l518-520 and fig 12: Maybe refer back to the equation on l469 to emphasise this point. You could replace fig 12 with two histograms showing frequency of occurrence of supersaturation as a function of (a) wind speed and (b) temperature to make your point more clearly.

l533: “... conditions THAT ARE close ...”.

l535: “in the field” – not clear if you are talking about at Dome C or elsewhere.

l535-539: “devoid of ice nuclei” may be a bit strong, but the existence of high ice supersaturations certainly suggest very low concentrations of IN (as suggested by King and Anderson, 1999). The statement about homogeneous freezing seems a little speculative. Is there evidence for the existence of supercooled water droplets at Dome C, e.g. from observations of rime accumulation on structures?

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