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.

Thank you John for raising the organization point, and for the review altogether. We certainly appreciate your suggestion of altering the flow of the paper However, it is not quite true that the FP has poor performance in all but the warmest months: the FP appears to work fine down to approximately -55°C, that is about half of the time including many days in winter. Thus I takes to presenting and discussing the winter data to raise the point. Therefore we feel necessary that, after a general presentation of the instruments (section 2), we first present summer observation which allows to asses and discard the non modified HMP, then turn to presenting the full year including winter which allows to detect the problem with the FP and limit its use in the climatology. We still feel that this is a good way to go. On the other hand, although the 3rd reviewer reports that the flow of the paper is good, we agree with you that an alternate defensible way is to push the model comparison to after a full presentation and discussion of the observations is given. We thus add a 4th section to the paper, which is fully dedicated to presenting the models including the cloud microphysics parameterizations in more details than before, then compare the models with the 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. 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?

Thanks for pointing that stating that Dome C is an exception for supersaturation in the surface atmosphere is an overstatement. We have moderated the statement (thus addressing a similar comment by reviewer 2), and we now report in the discussion section your observations with Phil Anderson at Halley station, compare with ours as suggested and adopt your interpretation almost verbatim (thank you!). We do not know of observations of IN at Dome C. There are lidar observations of aerosols, which cannot see so close to the surface as our measurements, and as far as we know do not see much besides higher clouds and diamond dust, and occasional pollution plumes from the station when the wind shifts from its main origin.

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.

A quick description of the 2 models with a particular focus on the cloud microphysics is now given in the new section 4.

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).

A paragraph is added in the discussion section to further address the issue of model biases, with reference to Wessem et al. This is reproduced below:

ECMWF and MAR supersaturation simulations are quiet different for several reasons. Water vapor concentration in the model first results from data assimilation while it is fully free to respond to model equations and parameterizations in the second. Parameterization of ice crystals nucleation play a particular role in the behavior of the supersaturation process. It is based on theoretical developments in ECMWF and in this case the number of crystals formed is rather insensitive to the aerosol physical properties. It results mainly from aircraft observations in the Arctic in MAR. The results at Dome C probably show that parametrization tuning is too narrow to properly account for the near surface conditions at Dome C, although temperature conditions probably play the most important role. Cloud ice processes are still poorly understood and the parameterizations used here must certainly be improved. This point is all the more important that a sensitivity tests of the RACMO microphysical scheme to the inclusion of supersaturation improves significantly the *performance of this model over the Antarctic [van Wessem et al., 2014].*

Minor points and typographical corrections
19: analyses

OK, changed throughout the text

23: leaving not living

OK

52: condense not condensate

OK, changed throughout

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

OK

88: condenses not condensates

OK corrected throughout

100: 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.

Yes, this is now reported in the discussion section

160: condense, not condensate

OK

171: referred TO as A ....

OK

174-178: Note that the system described by K&A 1999 did heat the aspirated air.

Yes, definitely (“Air was sampled at a nominal height of 4 m above the snow surface and taken to the unit through a 6 m long heated PTFE tube”) and it was a source of inspiration. This is now clearly reported.

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

Yes, corrected

290: Define “night” a bit more precisely?

Hard to define precisely since there is no night. Now referred as “broadly the coldest half of the day”.

309: “latter two”, not “latters”

OK done

311: warmest TIME of the day

OK changed

343: night-time

OK corrected

366 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).

As described in the response to major point 1, after the section 2 were we present the instruments, we stick in section 3 to presenting the data along with further discussing instrument performances (including calibrating HMPmod with FP in summer because this is when both instruments perform consistently) because we need the data to assess the performances. On the other hand, the model contributions to section 3 in the initial version are now fully moved to section 4. Thus the data section along with some discussion of the instrument performance is indeed more compact than before.

413-415: 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?

This occurs for low temperatures / low water content. As mentioned in section 2 when presenting the frost-point hygrometer, there in an issue with cooling the mirror to low enough temperature to reach condensation. This point is now reminded in section 3.2.

425 “at odds”

OK corrected

430-435: Need to be clear that what is being described here is not a humidity climatology but a conditional climatology for vapour pressure > 2 hPa.

OK, “When restricting to above 2 Pa” added.

l474: “...above thE surface...”

OK corrected

476: “is written”, not “writes”.

OK corrected

482: You should include an equation defining L.

we do not think that an equation of the MO length is really necessary since we refer to Vignon et al [2016] for the exact formulations of the stability functions. However, to fully make the point, we also add the reference to Stull [1990] from whom the formulation of L is obtained.

488: “...because OF the very low...”

This does not sound right because the sentence is “... because the very low vapor content of the atmosphere induces high uncertainties...”. Should we write “because of the very low vapor content, high uncertainties are induced?”

518-520 and fig 12: Maybe refer back to the equation on l469 to emphasize 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.

Reference to the equation is now given. Figure 3 was introduced in response to a comment by reviewer 2, showing RHi as a function of wind speed. Reference to new figure 3 is given here, answering part of the request for additional figures. As figure 3 illustrates the point for wind speed, we do not feel that the same figure for IR would add much to the discussion and this is omitted.

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

OK, changed

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

Yes, at Dome C, clarified

535-539: “devoid of ice nuclei” may be a bit strong, but the existence of high ice super- saturations 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?

There are evidences of liquid water in clouds aloft but no evidence of supercooled water droplets near the surface. Extensive dendricity clearly indicates that frost deposition is from inverse sublimation rather than riming. The text is changed to moderate the statement of “devoid ice nuclei”, citing Anderson [1993].

We thank John King for his thoughtful comments and suggestions which have definitely contributed enhancing the quality of the paper.