In this paper the authors present a comprehensive analysis of supersaturation at 3m above the surface at Dome C in Antarctica. The paper formally compares hygrometers and shows the need for heated inlets otherwise, not surprisingly, frosting on the inlet maintains RH at 100. The supersaturation is then compared against two atmospheric models with microphysical schemes, which yield similar distributions for supersaturation as the observations. Finally, the authors discuss how these corrected measurements influence latent heat flux from (or to) the surface. Although this paper had a number of grammatical errors, the overall flow and logic was good. Some of the conclusions regarding the importance of supersaturation in setting the latent heat flux are not well supported. Some other factors, including the isotopic effects are simply not adequately discussed.

Overall, this paper presents new and exciting measurements and provide important insight into the approach to measuring humidity in cold and dry places. Some suggestions are listed below but otherwise following revisions this paper seems suitable for publication. Sections 1 and 2 are very well written. For people new to this topic, the writing provides quite a good introduction. The authors do a great job explaining the pros and cons of the instruments in a way that is very practical and useful.

Larger comments: One conclusions that the authors draw is that by not considering supersaturation, estimates of moisture exchange are wrong. While this is strictly true, the outcome of considering or not considering this effect is so small it is hardly noticeable. Latent heat exchange accounts for two order less of water accumulation than precipitation (from this manuscript) and the difference between the the HMP and HMP- mod latent heat fluxes are not really measurable. Particularly when you consider a 100% possible uncertainty from the choices of stability functions. So, I think it would be more appropriate to say that supersaturation has no measurable impact on latent heat exchange or the moisture budget at Dome C. In my opinion the authors miss an opportunity to discuss a potentially larger impact of supersaturation, which is its effect on surface cloud formation, which greatly influences the radiation budget. So, if a model excludes supersaturation, it would be a lot cloudier. As opposed to focusing on the effect of supersaturation on latent heat exchange, I would focus on the indirect effect that considering or excluding supersaturation has on the radiative budget. There was no discussion or consideration of the uncertainty in Goff-Gatch, which could be shown by considering other alternative formulae. Are there any aerosol sources at Dome C associated with camp activity such as diesel burning. If so, the measurements made at Dome C represents a minimum supersaturation.

Replies and accounting of the larger comments:

We agree that initially concluding about the turbulent moisture flux that "the consequences (of occurrences of supersaturations) are limited on the antarctic plateau" is an understatement. We replace with "no measurable impact".

We also agree that occurrences and properly accounting in models of supersaturations have a strong impact on clouds and radiation – this is actually how we make our introduction in the topic, raising the issue of parameterization of high (cirrus) clouds that strongly affect the Earth energy budget. We did not raise the local cloud issue though. This is corrected in the conclusion but we do not attempt to estimate the impact on the radiation as this is much less straightforward than estimating turbulent fluxes: there is not bulk approach like Monim-Obukov formulations available, one would need to run models of both cloud formation and radiation transfer.

Finally, yes we also agree that there are uncertainties in existing empirical formulas to calculate saturation vapor pressure and thus supersaturations. As temperatures get colder, the differences between the formulas get larger (see e.g. http://cires1.colorado.edu/~voemel/vp.html). It is interesting that alternative formulations are generally evaluated against Goff-Gratch formulaes:

those are widely considered a reference mile stone. When mentioning that we use Goff and Gratch, we now also report that other formulas exist and that they can result in differences of up to 20% in the estimation of the saturation water vapor and thus on supersaturation for the coldest temperatures – which are alos the least frequent. Note that fro the HMP, from which humidity for the coldest cases are obteined, the conversion formulaees actually convert from RH with respect to liquid to RH with respect to solid, with a 5° (heating) correction, so this is largely a 2 way correction though which potential errors partially cancel.

Replies to the more detailed comments:

In Figure 7 when comparing the observed and modeled distributions, it would be good to reduce the resolution of the observations to 4 or 6 times daily, in order to fairly compare against the models. Does the change in resolution influence the distribution.

The figure below shows little difference whether all available observations or only observations at the synoptic times (0, 6, 12, 18 h TU, the sampling of the ECMWF data) are used to build the distributions. This is now reported in the the legend on figure 9, mentioning that the comparison between the model and observations is not affected. Because differences are so small, we prefer showing the distribution from the full time resolution of the observations and the models., reflecting the full information in the available data.

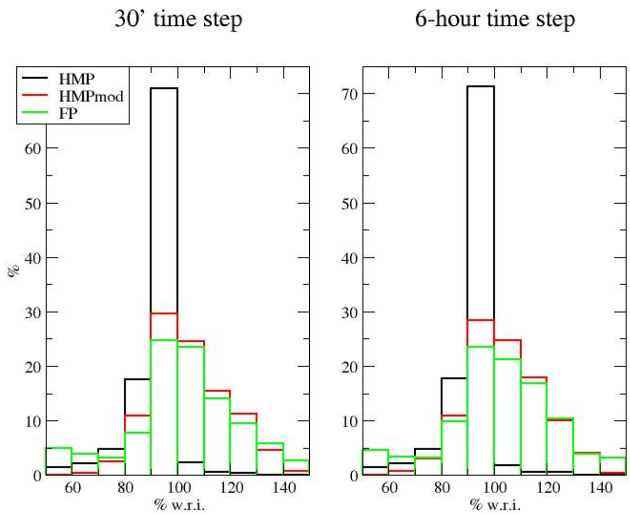

Figure: Distributions of Rhi from observations, horiginal (30') time sampling (left) and 6-hour subsampling (right)

On line 603: The authors say supersaturation at 200% is a game changer. I guess I would like to know a little more about what is implied here. What does this change?

"Game changer" is probably a little beyond our though. We change this to "invites some revision of our understanding...", considering in particular the issues raised concerning water isotopes.

In Figure 9 and related discussion. This looks like a kind of RH climatology, but in fact, as shown in the top panel a lot of data is excluded because Pa is too low. For this reason, I think it would better to show monthly distributions as box and whisper plots as to not present potential confusion that this is supposed to be an actual climatology.

Following comments by reviewer 2, we have straightened the discussion on the fact that we exclude *extreme (and extremely unlikely) values, so it is now clearer that the climatology is restricted to a specific range of values. We suppose that the reviewer refers to "box and whisker" rather than "box and whisper". It is not clear how box and whisker helps with a potential confusion with an actual climatology : showing plots for 3 instruments with and without restriction, we think that box and whisker actually add confusion to the figure.*

Small comments Throughout the paper "supersaturations" is used. As noted below, I believe supersaturation should be described as a state not an event. So I would change all supersaturations to supersaturation.

We appreciate the linguistic case made by the reviewer. However, we do find "supersaturations" used in various publications including at least 7 in ACP. Sticking to using "supersaturations", is possibly linguistically improper but scientifically convenient: it allows to indeed distinguish between the state (without s) and the recorded events (with s).

13: "Superaturation. . ..is frequent"

And yes, here we agree that "supersaturation" is more correct

14: "but is very"

OK

23 "leaving"

OK

24: "supersaturation"

OK here too

32 "can be obtained more easily at surface"

This part of the abstract was reformulated following comment by an other reviewer

46: "against observations"

OK

49: "in situ timeseries' "

OK, replaced by "in situ observation timeseries"

68-70: evaporation/sublimation/condensation the role that blowing snow has on evaporation, the process is still evaporation. I think blowing snow needs to be considered zero net.

The distinction between surface and blowing snow evaporation is generally made because, from a calculation and modeling perspective, these are different processes. We calculate surface evaporation in section 3.3 using Monin-Obukov similarity theory. This cannot apply to blowing snow evaporation (and we mention that blowing snow is not considered but is infrequent anyway).

90: here and elsewhere "Antarctic" sometimes not capitalized

Antarcica, as a region name, is capitalized, antarctic, as a adjective, is not.

92: “impact the reconstruction” Here some more information on how supersaturation impacts the ice core. Is this because precip is formed under supersaturation ? or is this through post deposition processes.

The impact is through kinetic effect each time the phase changes. This is mentioned in ther introduction (“Supersaturation leads to kinetic fractionation of the stable isotopic composition of water when it condenses”) Although the net budget is small, water is constantly exchanged between the snow surface and the atmosphere in one direction or the other. The cumulated impact on water isotopes may not be negligible. We are lighter on the isotopic issue in the present version, removing all equations, however this particular issues is now highlighted in the discussion section: “ it has important consequences for the formation of the isotopic signal of the snow. While the cumulated impact of water vapor exchange between the surface and the atmosphere may be small and contributes only ~10% of the surface mass balance, the asymmetry of the meteorological conditions (colder during condensation than during sublimation) leads to differences in the fractionation coefficients for the phase transition. As supersaturation during snow accumulation induces additional fractionation [Jouzel and Merlivat, 1984], we expect a significant impact of local supersaturation to the water isotopic signal recorded in the snow [Casado et al., 2016]”.

92: Supersaturation here and elsewhere should be singular. Supersaturation, in my opinion, is a process not an event.

Please see above

99: “plateau seem to be capped. . .”

We stick to the formulation “reach but seem to be capped” because it also conveys the additional information that the unmodified sensors can measure 100% relative humidity even in the Dome C conditions.

109: “such as the vertical resolution.”

OK done

109: “If both models produce”

OK done

112: Maybe not “decide between models” but diagnose the robustness of models”. Ultimately, it is not the goal to remove models but to improve them all.

Finding that one model does better than the other hints at a better approach to parameterize supersaturation. In that sense it is really “decide between models” just like in a model intercomparison exercise. However, it is true that the ultimate goal is to improve our modeling capabilities. Therefore, we change this to “decide between and improve models”.

119: “revisited” not “reminded”

129, Again capitalize Antarctic.

Please see above

137: “impact on the series”

OK

140: “another” (not “an other”)

OK

142: extra comma after Kampfer reference

OK

144: “were measured”

Actually, they are still being measured so we stick to “are measured”

152: “2 contrasting years”

OK

173: “energy, they have moving parts, and the mirror. . .”

*They have moving part **because** the mirror needs to be cleaned. This is now clarified, using “because” rather than “as”*

174: “disfunction”

Both dis and dys appear possible but OK for disfunction

201: How much is the inlet heated? Constant wattage or controlled to maintain a temperature above ambient?

This is a good question and is not mentioned in the manual. As we report in the paper, we never see any frost deposition so this is bound to be above saturation temperature.

202: “perform in cold”

OK

203: “According to the manufacturer, the”

OK

211: “for a large fraction”

OK

238: There is a contradiction in this sentence. "up to 200%...or even more”. It is either “up to” or “even more”

This part is reformulated following an other reviewer's comment

245: “note, that”

OK

303: “by each instrument, but”. Also, it is unclear what is meant by “reported to the atmospheric temperature of the HMP”

This is reformulated: “...reported to one same atmospheric temperature, that of the (unmodified)

HMP...”

309: “The latter”

OK

311: “Warmest part of the day” Figure 3: What are range or error bars on these diurnal cycles?

Because in summer all instruments perform within their nominal temperature range, the errors are the instrumental errors provided by the manufacturers as stated in section 2. This is very small compared to the amplitude of the diurnal cycle, is almost the same at all times, and is not considered essential for this figure.

331: rephrase, “where classical interpolation relationships are valid”. Unclear.

OK “classical” replaced with “gradient”.

342 “nighttime”

OK

346: “non-linear”

OK

353: What is meant by: “At synoptic times”

Right, this is meteorologist jargon but is now removed in the new text.

356: “produces levels of supersaturation, which are larger than the observations. . .”

OK

381: What is meant by, “According to instrument reports. . .”?

This was to distinguish between observation and model data, but following reviewer's 1 comment, the models no longer show in this part (they are moved to a specific model section 4) resolving any possible ambiguity. Thus “According to instruments reports” is simply removed.

384: “reaches”

OK

385: It is unclear the justification to limit the range between 50-150%. I see that this is 99% of observations but, still, why reject 1% of the data?

The justification for rejecting the most extreme data from the figure was that they are infrequent and using a linear vertical axis they would not clearly come out of the axis line itself. To resolve this problem, we now show 2 graphs side by side (new fig 6), one with a linear vertical axis restricted to 50-150% RHi, the other with a log vertical axis extending to 200%.

396: “as expected,”

OK

412: “lose” not “loose”

OK

421: “coldest period of”

OK (periods rather than period)

457: “for measurements in such”

OK

474 “the”

OK

480: “et” to “and”

OK

511 w.e. and also “water equivalent” is written out not as an acronym on

OK

514 520 “associated with low”

This is reformulated

558: “and climate models”

OK

562: “the the”

OK

564: “than the other”

OK

581: “The applicability is limited to the Antarctic Plateau. . ..”

We don't know whether this is a comment or a request for changing something in the text? We take this as a comment and, yes, this is limited to the conditions found on the antarctic plateau.

We are grateful to the reviewer for his/her very careful reading of the paper, many comments and suggestions which have definitely contributed produce a better paper.