

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

Anonymous Referee #3

Received and published: 6 October 2016

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 supesaturation 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

C1

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 oputcome 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 superaturation, 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.

C2

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.

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?

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 whisker plots as to not present potential confusion that this is supposed to be an actual climatology.

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.

13: “Superaturation. . .is frequent” 14: “but is very” 23 “leaving” 24: “supersaturation” 32 “can be obtained more easily at surface” 46: “against observations” 49: “in situ 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. 90: here and elsewhere “Antarctic” sometimes not capitalized 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. 92: Sueprsaturation here and elsewhere should be singular. Supersaturation, in my opinion, is a process not an event. 99: “plateau seem to be capped. . .” 109: “such as the vertical resolution.” 109: “If both models produce” 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. 119: “revisited” not “reminded” 129, Again capitalize Antarctic. 137: “impact on the series” 140: “another” (not “an other”) 142: extra comma after Kampfer reference 144: “were measured” 152: “2 contrasting years” 173: “energy, they have moving parts, and the

C3

mirror. . .” 174: “disfunction” 201: How much is the inlet heated? Constant wattage or controlled to maintain a temperature above ambient? 202: “perform in cold” 203: “According to the manufacturer, the” 211: “for a large fraction” 238: There is a contradiction in this sentence. “up to 200%...or even more”. It is either “up to” or “even more” 245: “note, that” 303: “by each instrument, but”. Also, it is unclear what is meant by “reported to the atmospheric temperature of the HMP” 309: “The latter” 311: “Warmest part of the day” Figure 3: What are range or error bars on these diurnal cycles? 331: rephrase, “where classical interpolation relationships are valid”. Unclear. 342 “nighttime” 346: “non-linear” 353: What is meant by: “At synoptic times” 356: “produces levels of supersaturation, which are larger than the observaions. . .” 381: What is meant by, “According to instrument reports. . .”? 384: “reaches” 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? 396: “as expected,” 412: “lose” not “loose” 421: “coldest period of” 457: “for measurements in such” 474 “the” 480: “et” to “and” 511 w.e. and also “water equivalent” is written out not as an acronym on 514 520 “associated with low” 558: “and climate models” 562: “the the” 564: “than the other” 581: “The applicability is limited to the Antarctic Plateau. . .”

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-670, 2016.

C4