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Interactive comment on "Accuracy and precision of lower stratospheric polar reanalysis temperatures evaluated from A-train CALIOP and MLS, COSMIC GPS RO, and the equilibrium thermodynamics of supercooled ternary solutions and ice clouds" *by* Alyn Lambert and Michelle L. Santee

Anonymous Referee #1

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General Comments

In this new paper Lambert and Santee evaluate lower stratospheric polar reanalysis temperatures by comparison with CALIOP, MLS, and COSMIC GPS measurements. In particular, the study focuses on the analysis of ice and STS equilibrium temperatures of PSC particles. Overall, I found this to be an interesting and carefully conducted study.

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Discussion paper

The paper is well-written, concise, and fits in the scope of ACP. I would recommend it for publication once the specific comments listed below have been addressed.

Specific Comments

abstract: The abstract is a bit long and could possibly be shortened. At p2, l21-22 a copyright statement and acknowledgement was introduced, which seems to be out of place?

p3, l13-15: At this point the reader might wonder why NAT is not considered in this study? The reason is given later in the manuscript (at the begin of Sect. 3), but I would like to suggest to move the explanation a bit forward.

p4, l6-10: Regarding the long-duration balloon observations there is a number of other studies using them for evaluation of meteorological reanalyses in the polar stratosphere (some of the papers being part of the recent S-RIP special issue in ACP):

Boccara et al., Accuracy of NCEP/NCAR reanalyses and ECMWF analyses in the lower stratosphere over Antarctica in 2005, J. Geophys. Res.-Atmos., 113, D20115, doi:10.1029/2008jd010116, 2008.

Friedrich et al., A comparison of Loon balloon observations and stratospheric reanalysis products, Atmos. Chem. Phys., 17, 855-866, https://doi.org/10.5194/acp-17-855-2017, 2017

Hoffmann et al., Intercomparison of meteorological analyses and trajectories in the Antarctic lower stratosphere with Concordiasi superpressure balloon observations, Atmos. Chem. Phys., 17, 8045-8061, https://doi.org/10.5194/acp-17-8045-2017, 2017.

I would also suggest to rephrase "Independent datasets such as radiosondes, satellite observations,...", simply because the data sets may have been subject to data assimilation at some of the centers and therefore might not be "independent" in a strict sense.

p5, l9: Add a reference for the MLS measurements?

p5, l10: So far I had not associated ESA with the "A-train" constellation?

p5, l19-30: Maybe summarize this information in a table? It might be worthwhile adding information on the vertical resolution of the data sets in the height range relevant for this study.

p6, l4-5: How was the interpolation performed, linearly with respect to log(p)?

p6, l27-p7, l2: Maybe add a few words regarding the uncertainties of T_STS (or say that this will be discussed later in the paper)?

p7, l11: "Even though the RO measurements _per se_ are SI-traceable..." What does this mean?

p7, l21-31: Not sure if this detailed explanation on how the noise was estimated is really needed? Maybe just summarize the findings of Staten and Reichler?

p8, l1: "...and -0.5K above 35 km" does not seem to be relevant for this study?

p8, l9-12: What are typical values of z_Ttop, z_Sbot, and H_Sbot? Naively, I was thinking z_Ttop and z_Sbot are the same?

p8, l22-28: What is causing the differences between the "dry" and "wet" GPS profiles? The differences do not seem to be related to the water vapor correction at these height levels?

p9, l20: Another prominent reference in addition to Doernbrack et al. is:

Carslaw et al., Increased stratospheric ozone depletion due to mountain-induced atmospheric waves, Nature, 391, 675–678, 1998.

p11, l15-20: The patterns might also be compared with Hindley et al. (2015), who used COSMIC data:

Hindley et al., The southern stratospheric gravity wave hot spot: individual waves and their momentum fluxes measured by COSMIC GPS-RO, Atmos. Chem. Phys., 15, 7797-7818, https://doi.org/10.5194/acp-15-7797-2015, 2015.

p11, 21-32: I found it a bit difficult to follow the text in this paragraph, in particular this sentence: "A decrease in the spread of the temperature differences of reanalysis data compared to COSMIC should be detectable by rejecting the temperature profiles from consideration that are expected to have lower fidelity to the true atmosphere." Maybe rephrase this sentence/paragraph a bit, using easier words?

The message is (as I understood) that you are excluding those COSMIC profiles from the statistical analysis that contain gravity waves, because those are generally not captured well by the reanalyses because of their limited spatial resolution? This makes sense, I think, as it puts emphasis on the evaluation of the large-scale state.

p12, l4: I would suggest to add 1-2 sentences regarding the motivation for the orographic case study at the begin of this section. Reading the paper the first time, I found it a bit strange that at the end of Sect. 4.1 your are saying you are excluding gravity wave-affected data from the analysis and then the next section is showing a gravity wave case study.

Overall, the case study is very interesting, I think, as it clearly illustrates the difficulties related to the representation of gravity waves in reanalyses data. Perhaps the findings could be put into context with other recent work discussing this aspect? The studies listed below might be worthwhile considering. They found that meteorological analyses and forecasts generally tend to show both orographic and non-orographic gravity waves patterns in the correct locations. However, wave amplitudes are significantly underestimated and wavelengths are overestimated (as can be seen also in your Fig. 4), which is attributed to the limited spatial resolution or truncation of the forecast models.

Schroeder et al., Gravity waves resolved in ECMWF and measured by SABER, Geophys. Res. Lett., 36, L10805, doi:10.1029/2008GL037054.

Jewtoukoff, et al., Comparison of Gravity Waves in the Southern Hemisphere Derived

from Balloon Observations and the ECMWF Analyses. J. Atmos. Sci., 72, 3449–3468, https://doi.org/10.1175/JAS-D-14-0324.1, 2015.

Hoffmann et al., A decadal satellite record of gravity wave activity in the lower stratosphere to study polar stratospheric cloud formation, Atmos. Chem. Phys., 17, 2901-2920, https://doi.org/10.5194/acp-17-2901-2017, 2017.

Hoffmann et al., Intercomparison of meteorological analyses and trajectories in the Antarctic lower stratosphere with Concordiasi superpressure balloon observations, Atmos. Chem. Phys., 17, 8045-8061, https://doi.org/10.5194/acp-17-8045-2017, 2017.

p12, l11-15: Which height level are the wind speeds of 25-35 m/s referring to? For estimation of gravity wave vertical wavelengths by means of the dispersion relation the winds at the observational level should be considered. There might be significant Doppler shifting and change in wavelength towards longer wavelengths if the stratospheric winds are stronger than the low-level winds.

p12, l18-20: Are these differences in wave amplitude correlated with the horizontal resolution of the reanalyses data sets?

p13, l11-15: How do these findings regarding the biases of the reanalyses relate to other studies on the topic? Earlier studies found quite significant warm or cold biases of reanalyses data (varying with height) in the polar winter stratosphere, I think.

p13, l18-19: I am not so familiar with the CALIOP classification of PSCs. Do MIX1, MIX2, and MIX2-enh include or are mainly dominated by NAT?

p14, l19-p15, l2: Section 4.6 does not seem to present any results, but recapitulates the method? Maybe merge with Sect. 4.7?

p16, l13-16: I would not directly expect the distance of reanalysis grid points to observations being a reason for causing two distinct modes of temperature precision because I would expect those distances to vary continuously over the study area?

p17, l15-16: I am afraid, do not fully understand this sentence: "The standard deviations result from the combination of the quadrature addition of the precisions of the reanalyses and the reference points." What does "precision of the reference points" refer to?

p18, l24-25: However, the standard deviations with respect to the reanalyses are only similar, because gravity wave-affected profiles showing small-scale temperature fluctuations have been excluded in your analysis, right? It might be good to repeat this here.

p19, l8-9: I would agree that this study showed much better agreement between the reanalyses and observations than earlier work. However, I am not sure if you really demonstrated that point in the paper (see also comment regarding p13, l11-15)? The discussion of this aspect could be extended a bit.

p31, Fig. 3: Maybe indicate the noise variance of the COSMIC data in Figs 3a and 3d? In the caption the unit "K^2" is missing in a few places, e.g. "0.1" -> "0.1 K^2" and so on.

p33, Fig. 5: Does this analysis consider filtering of small-scale fluctuations due to gravity waves? There seem to be notable differences in the medians between the reanalyses. Is this related to COSMIC data being or not being assimilated in some of the reanalysis? How do these results compare to other studies?

Technical Corrections

p11, l4: Remove units "J Kg^-1" from equation (3)? And "Kg" should be "kg" in other places?

p12, l10: peninsula -> Peninsula (maybe use either "Palmer Peninsula" or "Antarctic Peninsula" throughout the manuscript for consistency?)

p12, l11: lambda_v -> lambda_z

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Discussion paper

p14, l31: ERA-I -> ERA-Interim (and in other places)

p36, Fig. 8 and p37, Fig. 9: y-axis labels are partly missing

p44, Fig. 16: fix "standard (triangles) deviations"

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