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## Reply on RC2

Christophe Genthon et al.

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Author comment on "10 years of temperature and wind observation on a 45 m tower at Dome C, East Antarctic plateau" by Christophe Genthon et al., Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2021-204-AC2>, 2021

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### General

This paper describes temperature and wind data measured over ten years at six levels on a 42 m tower near Concordia station, which is located on the high interior plateau of the East Antarctic Ice Sheet. The dataset is unique – no other comparable measurements exist from this part of Antarctica – and is thus of immense value for validating the performance of climate and weather prediction models in this region and also for the development and validation of remote sensing techniques. Furthermore, the Dome C region provides an ideal “natural laboratory” for studying the atmospheric boundary layer (ABL) under conditions of strong static stability so these measurements also have application in the development of parametrisations of the ABL under such conditions, which occur widely across the polar regions and even in mid-latitudes. I am very pleased that the authors have chosen to make these data freely available and have documented them carefully in this paper. The paper provides users of the data with essentially all of the metadata that they would need to access and analyse the data. The description of the quality control procedures applied could possibly be a little more quantitative (see my detailed comments below) but this is not a major issue.

As well as providing a detailed description of the dataset, the paper also presents some basic climatological analysis of the data (section 3) and uses the data to validate the ERA5 reanalysis (section 4). These excellent studies are a substantial part of the paper and would be worthy of publication in their own right in an atmospheric science journal. It is, of course, useful to see examples of the use of the data in a dataset description paper but the Editors may wish to consider whether the balance is right for ESSD. This issue aside, I believe that the paper is suitable for publication following attention to the specific points raised below.

=> We thank the reviewer for insightful and very pertinent comments to improve the paper. Here is how we have taken comments into account and thus hopefully fulfilled the reviewer’s requirements. Please note, line numbering is that of the original manuscript, obviously changed in the new manuscript. The new manuscript also accounts for comments by the other reviewer and changes made in response to these comments. Concerning the final point raised above, we think that the comparison is of interest not only to illustrate the value of the observation dataset we make available to the

community, but also because meteorological (re)analyses are often used as surrogates for unavailable observation, a view which is particularly questionable for the Antarctic plateau atmospheric boundary layer. We need real observations even when they are hard to obtain.

### **Specific points**

Lines 54-56: You could also mention measurements made at Halley (King, 1990, 10.1002/qj.49711649208) and the Alexander Tall Tower! (Mateling et al., 2018, 10.1175/jamc-d-17-0017.1)

=> Although the 2 towers are located on peripheral shelves near sea-level rather than on the high antarctic interior plateau, they have definitely proved very useful to study the peculiarities of the antarctic atmospheric boundary layer. We now mention the towers and list the corresponding references.

Lines 92-93: I presume you mean "...upwind of the station in the prevailing wind direction."?

=> Right, this is reformulated

Line 94: "extension" rather than "expansion"?

=> OK, taken

Line 138: "mean" rather than "modulus"?

Actually, a coma was missing which made it confusing, coma now provided

Line 143: "power outages" rather than "blackouts"?

=> OK, taken

Lines 151-152: These are reasonable limits for quality control purposes but I don't think

that it is correct to say that temperature "cannot" be outside these limits.

=> OK, this is reformulated

Lines 154-155: "Data outside of those ranges, or showing suspicious vertical variations or unrealistically steep changes, are eliminated." Did you apply objective criteria on vertical gradients or temporal changes? If so, state clearly what these are. If this was done subjectively, then make this clear.

=> There is no "objective" data quality analysis and processing in the mathematical sense. On the other hand, the data are 1<sup>st</sup> subject to automatic filtering (automatic detection / removal of unrealistic profiles, unrealistic steps, etc), completed by visual inspection and manual filtering for particular cases. This is further stated in the paper.

Line 155: "rapid" rather than "steep"? (I assume you are referring to temporal change here?)

=> OK taken

Lines 199-201: This sentence isn't very clear. Are you talking about elevated inversion layers? Not sure what you mean by "nonlinear turbulent diffusion" – turbulence is an inherently nonlinear phenomenon.

=> Yes we are talking about elevated inversion layers, building at the top of the weakly stable boundary layer. Turbulence is indeed nonlinear but we want to emphasize that the increase in inversion strength can only be explained by a non-linear process (Estourneland Guedalia 1985) Linear diffusive processes such as radiation tend to smooth gradients out. We have clarified the sentence as follows:

Strong elevated temperature inversions within the tower height generally build-up at the top of the boundary-layer when non-linear diffusion by turbulence vertically transports cold air from the surface, e.g. when the stable boundary layer transits from a very stable (with a very strong near surface-based inversion) to a weakly stable regime (Vignon et al. 2017)

Figure 3: Use symbols to mark the measurement levels and possibly add horizontal bars to indicate measurement uncertainty. Part (a) shows a suspiciously large superadiabatic temperature gradient between the 25 m and 33 m levels.

=> Done using circle symbol, with diameter reflecting height uncertainty due to snow accumulation during the period of observation

Lines 229-230: You could establish how much of the difference between AWS and tower temperature was due to sensor height difference by extrapolating the tower measurements to AWS sensor height (either linearly or using a more sophisticated extrapolation).

=> Yes but the elevation difference varies in time and is not even known at a given time. Considering the combined uncertainties in elevation difference and with extrapolation method, we don't think this would bring much additional insight.

12: Figure 6 (and other power spectra). Give units on the "Power" axis.

=> The unit for of power spectrum is the square of the variable ( $(^{\circ}\text{C})^2$  or  $(\text{m/s})^2$ ) multiplied by time sampling (48 samples per day)). This does not make for a very handy unit, so we do not report this on the figures themselves but now mention in the text.

13: Figure 7 caption: "tower level", not "model level".

=> Done

14: Figure 9: Y-axis caption needs correction.

=> Done

15: Line 328-329, figure 10(a): It is interesting that you don't observe significant power at the inertial frequency.

=> Actually, the spectra do show power near inertial frequency (approx  $\frac{1}{2}$  day) but this is very small compared to annual, semi-annual and diurnal. We do not make a case of the inertial waves in the paper because they are intermittent, one would have to "chase" them, and this is beyond the scope of the paper. The text is corrected to mention that gravity waves show on spectra.

16: Section 4: I think it would be useful to summarise the results (mean bias, rms bias, correlation coefficients) of this section in a table, maybe broken down by season?

=> We appreciate this suggestion. We think that graphical representation (figs 12 and 13) is a good way of conveying important information on the agreement between ERA5 and observation. We provide numbers (correlations, rms) on the figure and in the text. Whether this needs to be further summarized in a table is questionable. In her/his general comment, the reviewer wonders if an ESSD paper is the right place for an evaluation of ERA5. We argue that this is used as an illustration of the value of our data but to remain focuses may be we should not expand the presentation of the results further than we currently do (numbers, correlations, rms, are in the text or on the figures, do we need to further summarize in a table in this context?)

17: Lines 373-374: I don't understand this sentence. Correlation coefficients don't tell you anything about the relative amplitudes of variations in the two series that are being correlated.

=> It does tell about the relative amplitude of variations along the series. In terms of timing, the 2 series at <https://www.lmd.jussieu.fr/~cgenthon/temporaire/illusrcorr.jpg> (sorry for the link, the system here does not allow to directly insert figures) are perfectly coherent in timing of events but the correlation coefficient is less than 1 because the amplitude of the events is not the same.

18: Line 380: The temperature profile has a log-linear form under stable conditions, with the linear component increasing with increasing stability.

=> Right. "linear" changed to "log-linear"

19: Lines 391-392: It might be clearer to say "the reanalysis product has a cold bias at higher temperatures and a large cold bias at the lowest temperatures". Weidner et al. (QJRMS 2021, 10.1002/qj.3901) give a striking example of the failure of ERA-Interim and ERA5 to accurately reproduce an extreme low temperature and associated extreme surface inversion over Greenland.

=> We don't think that this is fully equivalent to what we intend to express, as we also carry a point on the vertical structure of biases. We thus stick to our original formulation.

20: Line 493: Typo "to overestimate".

=> Corrected

21: References, line 573: I don't think that the Ekman paper is referenced anywhere in the text.

=> Right. Reference removed

### **A note on the datasets**

The datasets described in the paper can be easily downloaded from the Pangaea data centre as tab-delimited text files with some basic metadata provided in the file headers. Temperature and wind speed measurements are provided in separate files. However, wind direction measurements (which are described in the paper) are not made available. As wind direction is a cyclic variable there can be some ambiguity (alluded to in the paper) when calculating 30-minute means, but I would strongly encourage the authors to deposit a third file in Pangaea, containing either 30-minute vector mean wind directions or, alternatively, 30-minute mean u- and v-components of the wind.

=> This is a good suggestion. However PANGAEA currently informs that it is overwhelmed with data accommodation requests and is very slow to answer. Concerning the the temperature and wind speed data for this paper, waiting for PANGAEA's DOIs has significantly delayed the process of paper submission. Waiting for PANGAEA to approve and provide DOI for an additional data set would further delay acceptance and publication by several months. We will consider depositing 30-minute U- and V- components in the future but in order to not delay further the paper we do not advertise this in the paper. On the other hand, at the same time we mention that the minute instant directions shown in the paper are available on request to authors, we add that the 30-minute average U and V are also available on request.