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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Year-round record of near-surface ozone and "O_3 enhancement events" (OEEs) at Dome A, East Antarctica" *by* Minghu Ding et al.

Anonymous Referee #3

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This paper is within the scope of ACP and presents a potentially interesting observational record by investigating an interesting scientific topic. However, the used methodologies are not adequate (in some cases - see the use of PV - wrong) and, also for these reasons, the conclusions are far to be robust and mostly based on qualitative and arbitrary interpretation of data. Moreover, the manuscript suffers of strong deficiencies in the vocabulary and in the quality of figures. Finally, I did not see any acknowledgments to people or Institutions providing the ozone data from South Pole station: from which data repository has been this dataset obtained?

Thus, I'm sorry but I have to recommend rejection or a complete re-submission of a

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Discussion paper

new manuscript when the following shortcoming will be fixed.

SPECIFIC COMMENTS:

Abstract: line 25: “To explain this unique finding, the occurrence of stratospheric intrusion (stratosphere-to-troposphere, STT) events was studied with the Stratosphere-to-Troposphere Exchange Flux (STEFLUX) tool”. Here the author claimed STEFLUX was used in this work: unfortunately this is not the case (see also the related comment by one of the STEFLUX’s authors). STEFLUX is a code developed by Putero et al (2016), see https://www.atmos-chem-phys.net/16/14203/2016/acp-16-14203-2016.pdf. No indication of the real use of STEFLUX can be found along the paper. At some point the authors claimed they selected back-trajectories coming from region with PV > 2 pvu (by the way: in the southern polar stratosphere the PV is negative, so this is wrong!): this is a rather simple filtering of back-trajectories far to be the application (or a replication) of STELFLUX. Thus, this sentence should be removed from the abstract.

Line 58 -72 (page 4): this section is almost a “cut-and-paste” from a paper by other authors (Cristofanelli et al., Analysis of multi-year near-surface ozone observations at the WMO/GAW “Concordia” station).

Section 2.1: the experiments is not well described. As an instance no information are provided about the set-up of the measurement system as well as used materials. No information about the application of a Quality Assurance strategy and good/standard practices. No reference to the adoption of (international) calibration scale. No details about the execution of the intercomparison with the travelling standard Thermo 49i-PS: the linear correlation coefficient is not sufficient to assess the overall quality of measurement (what about the total uncertainty)?

Line 113: vocabulary issue: “Model-49iPS UV absorptive ozone calibrator” should be “UV-absorption ozone calibrator Thermo 49i-PS”

Line 128: why data filtering? In general, I feel dangerous to automatically eliminate data without motivation (i.e. error codes in the internal diagnostic, extremely inconsistent values,. . .).

Section 2.2 Meteorological simulations should be renamed as “Air mass back-trajectory calculations”

Section 2.2: a very poor description of the methodology (and strategy) for back-trajectory calculation is provided (no indication about time resolution of back-trajectories and frequency of their calculation). A discussion about usability and limitation of the use of these kind of back-trajectories based on coarse meteorological data is missing (e..g. it seems that the authors did not perform any sensitivity study to evaluate the impact of selecting different altitude or geographical position of the trajectory arrival point, which is a rather common practice to evaluate associated uncertainty). The authors mentioned that a clustering has been performed but they not provide any information about the clustering methodology nor provided evidences for cluster calculation in the paper. Vocabulary issue: “and the back-up time was 120 h”. What is the back-up time?

Line 170: the assigned weights look arbitrary. No explanation or motivation provided.

Line 185: this sentence is not clear at all

Line 194: “In Antarctica, the emissions of ozone precursors are generally less than those at mid and low latitudes”. Which precursors are emitted in Antarctica? By which process? What do you mean with “generic less”? Please try to be quantitative.

Line 207: “Specifically, the largest standard deviation was observed in October at DA because of multiple influences, including photochemical reactions by ozone precursors and ultraviolet radiation, photolysis reactions by strengthened ultraviolet radiation, and external air masses from the coast.” These are only assumptions: not proofs are provided by the authors

Line 216: vocabulary issue: “Is there ozone exchange happening?” Ozone is transported not “exchanged”.

Section 3: Overall, I think that the analysis of diurnal variation is not well executed in Section 3. What “normal days” are? If the authors’ goal was to investigate the diurnal variability of ozone some relative measure should be used instead of actual mixing ratio (see for instance the earlier work by Helmig et al., 2007: "A review of surface ozone in the polar regions").

Line 225: “Because of the limited number of normal days, the diurnal concentration fluctuated”. If the dashed area represents a confidence interval (not explained in the figure), the diel (not “diurnal”: vocabulary issue) cycle looks well consistent and not “erratic”, instead. On the contrary, it is the green series that looks more “noisy”. Please avoid using this kind of background colors in the Figure 4 plots. Is time expressed as UTC or what else in Figure 4? The diel variability of ozone (even when evident, see green line in plot 4a or black line in plot 4c) is not explained or motivated enough by the authors.

Line 230: I do not agree. This can suggest that local photochemistry cannot have a role. But probably, if you consider the transport time, the integrated contribution of photochemistry related with snowpack NOx emissions can be relevant. This should be better assessed in the paper.

Line 241 – 245: This part is confuse and the description of cycle leading to ozone production is not correct. Sorry but I cannot really understand why the cold environment can motivate the daytime variability at NA.

Line 254 – 264: again, this is mostly a cut-and-past from an already published paper.

Figure 6: The analysis and interpretation of back-trajectory analysis presented in Figure 6 is not robust at all. Firstly the conditional probability should be calculated for winter and summer, if you want to demonstrate a prominent role of STT versus other processes in winter. From Figure 6, it looks that only a small number of TRJ are used

for this analysis (how much?): unfortunately this strongly limits the statistical robustness of results (that, in any case, do not support STT occurrence). Moreover, I'm not able to see any difference between back-trajectories in polar night/day that you used for motivate the role of STT during the winter.

Line 339: "Here, we use STEFLUX to 340 identify STT events and define the height of tropospheric potential vorticity PVU = 2." You did not use STEFLUX, actually. Moreover, in the Southern Hemisphere medium-high latitude, stratospheric air-masses can be traced setting PV < - 2 pvu and not PV > 2 pvu!

Line 370: "To quantitatively analyse the influence of STT events on OEEs, we examined 370 the appearance of STT events above DA and found that STT events (550 hPa PV>2 PVU) accounted for 55% of the polar night in 2016." This is wrong: firstly, to trace stratospheric air-masses, you should detect PV values lower than – 2 pvu. Secondly, as clearly see by Figure 7 at 550 hPa the PV variability is affected by non-adiabatic process occurring near the surface and thus it cannot be used to trace STE.

From Figure 7 is not possible to see any obvious correlation between ozone at DA and the downward transport of stratospheric air masses: the supposed link between high near-surface O3 at DA and occurrence of STE is not supported by a quantitative analysis (only a qualitative comment to Figure 7 is provided). Moreover, the wrong detection methodology used to identify the STE events brings an evident overestimation of STT occurrence: all the winter period (except August) appeared to be affected by STT (even without effect on near-surface O3, see e.g. the period from 6/20 to 7/15 which not support your hypothesis).

Finally, Figure 8 does not provide any reasonable support to the hypothesis that STE are driving O3 variability during winter. I do not see any evident differences between OEE and NOEE. It is not clear why using the rate of change of the hourly O3 m.r. instead of the actual O3 m.r.

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