

Interactive comment on “Year-round record of near-surface ozone and “O₃ enhancement events” (OEEs) at Dome A ,East Antarctica” by Minghu Ding et al.

Anonymous Referee #1

Received and published: 29 August 2020

The manuscript is within the scope of ESSD. It presents scientifically significant material based on surface ozone measurements at three Antarctic stations. Of especial importance are data of measurements at Dome A, the highest Antarctic plateau (4000 m above sea level), which is one of the remotest areas on earth. The analysis of the data is reasonable and reliable, the data is unique. The authors should consider the following comments prior to publication.

General comments

1. I would appreciate it if the authors could please introduce more about the details of instruments and the measurements.

C1

2. Because the results of trajectory clustering analysis are important in the discussion, there should be more description of trajectory clustering method.

3. L64-70. The specific chemical reaction process of nitrate aerosol photodegradation on snow pack should be increased. It is necessary to clarify the effect of NO_x released by photodegradation on O₃ emission from snow pack if it is possible.

4. In Section 3.3, the author's statement is too simple and arbitrary. The standard deviation of the average daily concentration in Zhongshan station was significantly higher than that in the other two inland stations. L259-L264 completely said that every solar chemical reaction had little effect on the concentration variation characteristics of the three stations, which was not rigorous. This paper focuses on the influence of daily photochemical reaction on the concentration variation characteristics of Dome A, and the difference of average daily concentration fluctuation between coastal stations and inland stations also needs to be discussed briefly. For example, coastal stations are easily affected by halogen gas mass in summer, and ODE (Ozone Depletion Event) is triggered (A.E. Jones et al., 2009), which has obvious impact on the fluctuation of average daily concentration in summer. Section 3.3 needs to add relevant references to support the author's statement.

5. In Section 4, the influence of STT on OEE is discussed by STEFLUX. It is also mentioned that STT can be judged by atmospheric chemical model (such as GFDL-AM3 and CAM-CHEM). Can author try to use CAM-CHEM Model to analyze the STT events in Dome A and compare the results with the OEE. At the same time, results of the two methods may be compared if it is possible.

6. Table 1 is not necessary, I suggest to delete it or move it to supplementary.

7. Fig. or Figure, please use the unified one in the whole paper.

Specific comments 1. Line 22, what is DA.

2. Line 232, only “in this part”? Sept→Sep?

C2

3. Line 217-220, “concentration, molar ratio, mixing ratio”, please be consistent with each others.
4. Line 282, from the results above, “it can be seen that” SP was characterized. . .
5. Line 301, the wind of DA were “predominantly” from north and west. Prevalent may be better.
6. Line 317, have → has.
7. Line 361, As the station name has been abbreviatted, such as Amundsen-Scott →SP, all station names should be checked and properly used.
8. Line 375-376, the last sentence should be rewrite or removed.
9. I would suggest improving the readability of the label in Figure 4 if it is possible. It seems to not clear on my copy.
10. Figure 5, Standard deviations of mean diurnal variation in near-surface. . .
11. Figure 8, what are the error bars.
12. Figure 11, δ should be Δ ?

Interactive comment on Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2020-130>, 2020.