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Comment on acp-2021-658

Anonymous Referee #1

Referee comment on "Influence of total ozone column (TOC) on the occurrence of tropospheric ozone depletion events (ODEs) in the Antarctic" by Le Cao et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-658-RC1, 2021

The authors present a modeling study concerning tropospheric ozone depletion events in Antarctica. This is a well-known phenomenon occurring in both polar regions related to a complex interplay between meteorology, atmospheric chemistry, and sea ice. Despite a large amount of observations, the exact conditions necessary for starting and ending such events still remain elusive. One reason is that comprehensive models that are capable to simulate chemical processes in the polar atmosphere-sea ice-ocean system are still missing. Therefore, theoretical studies like the one presented here are still required to advance our knowledge. The authors concentrate here on a specific point concerning the impact that low total ozone columns (TOC) may have on the tropospheric depletion events. Since the chemical initiation of the ozone destruction near the surface is driven by photochemical processes, it can be expected that reducing the ozone column may have an accelerating effect due to enhanced UV radiation and enhanced photolysis rates, which is confirmed by the simulations. This result can constitute an important contribution to a better understanding of ozone depletion events. However, I have major concerns about some parts of the manuscript and the presentation of the simulations, which is too limited to support their reproducibility. My major concerns are described below.

Major comments:

In the title and at numerous occasions in the manuscript the authors claim to study “the influence of the change in TOC” on ozone depletion events. However, the manuscript only shows the influence of the TOC on the depletion events. This is most obvious in Fig. 1, with a caption referring to “… temporal change in TOCs”, while it shows time series of observed TOCs at different locations. Of course, the TOC varies over time. However, the authors should carefully check when they actually studied the influence of a changing TOC or when they studied different TOCs.

I find ch. 3.1 dealing with the observations not convincing. In the current form a relationship between the presented TOC and the ozone depletion near the surface is not apparent. However, why would you expect to find a strong relationship between a short-term phenomenon that lasts a couple of days at maximum and the monthly average of the TOC? Table 3 indicates that in the studied period at Halley the periods classified as depleted in ozone are always less than 10 % of the total time. In my opinion the monthly averages can easily hide any correlation. Moreover, the spatial representativeness of the two observations (surface ozone and TOC) are not discussed. Depending on the meteorological conditions the observed surface ozone may correspond to a very confined region, while it can also be influenced by the effective transport of air masses. In fact, it is well known that depletion events at coastal stations are in almost all cases related to the transport of air masses from sea ice-covered areas. This is not discussed at all in the manuscript. On the other hand, it remains unclear what a monthly average of TOC signifies in terms of spatial representativeness. I am not so familiar with the spatial and temporal variability of stratospheric ozone, but what does a monthly average of TOC represent? The use of monthly averages is even more surprising since it appears that higher resolution data are available. Why didn’t the authors use daily values of the TOC and the surface ozone to check for any correlations?

I find the modeling part of the manuscript interesting and useful. To my knowledge, the influence of the TOC on the depletion of surface ozone via its impact on the different photolysis rates has not been studied in such detail before. This gives useful new information on the processes governing the depletion of ozone. This also concerns the sensitivities as shown in ch. 3.4. Such information may also be helpful to understand why ozone depletions only appear during springtime and what processes are involved in the termination of depletion events. However, I am less convinced by the choice of the boundary conditions that are used for the simulations. It appears that the monthly averages of the TOC as presented in ch. 3.1 were used as well as a specific solar zenith angle SZA for each month. What is this SZA? The maximum, minimum, or average SZA for a given month or the SZA for the middle of the month? Was the diurnal cycle of the SZA considered in the simulations? Like in the case of the monthly averaged TOC for the correlations, I am not convinced that applying monthly values is useful and may even create artificial boundary conditions. Why didn’t the authors perform simulations for specific days (for example, for each analyzed month the days with the maximum and minimum TOC or selected days with similar TOC, but varying SZA)? In my opinion such simulations would be much more convincing since they would more closely correspond to conditions encountered at Halley. Moreover, they would be easier to characterize and also easier to reproduce.

A further issue that merits some discussion is the comparison between the observed and the simulated surface ozone concentrations. First, it would be good to have a figure (maybe in the supplement?) showing the surface ozone concentrations that were used and indicating also the periods that were identified as periods with depleted ozone according to Table 3. Second, with the low frequencies as shown in Table 3 (the highest value corresponds to less than 70h during a full month) it is impossible that any of the events corresponds to the simulations as shown for example in Fig. 3 with three full days of zero ozone. This point should be clarified.

The authors claim that they obtained the surface ozone measurements at Halley from the WDCGG. However, the WDCGG web page states: "Reactive gases measurement data (except for CO) have been agreed to be transferred under the responsibility of the newly established GAW World Data Centre for Reactive Gases (WDCRG) hosted by the Norwegian Institute for Air Research (NILU)." The source of the data should be verified.

The information about the parameters used in the different model components is not adequate since their description is too limited to support the reproducibility. For example, the TUV model (ch. 2.2.1) requires a range of further input parameters concerning the albedo, clouds, and aerosols. In the manuscript it is not sufficiently specified, which parameters were used. Moreover, according to equation (4) the KINAL model (ch.2.2.2) does not include deposition, which can be an important removal process for a number of the simulated species. This would be a serious weakness of the model.

Minor comments:

I'm not convinced that the direct transport of tropospheric ozone into the boundary layer has ever been demonstrated (l. 77ff). In any case, Kuang et al, 2017, reported only that an entrainment from the stratosphere occurred into the free troposphere at an altitude above 3000m.

In figure 4 it appears that the ozone concentrations in the simulations for October and November do not drop to zero. Is this realistic?