

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2022-247

Anonymous Referee #2

Referee comment on "Ozone depletion in the Arctic and Antarctic stratosphere induced by wildfire smoke" by Albert Ansmann et al., Atmos. Chem. Phys. Discuss.,
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The paper aims to highlight and quantify the role of extreme events of wildfire smoke to the ozone depletion in the Arctic and Antarctic. The study is based on lidar measurements concerning the evolution of the smoke in the stratosphere and on ozonesondes concerning the vertical distribution of ozone. The authors further analyze a unique lidar dataset, already presented in previous studies, concerning the presence of wildfire smoke in the polar stratosphere and aim to associate this with ozone depletion in the Arctic and Antarctic. They provide a very comprehensive review on the impact of various types of stratospheric aerosols on PSC formation and attempt to describe the potential impact of aged smoke on PSC formation. They also provide a very detailed analysis of the characteristics of the aerosol layers and the ozone departures at various layers from ozonesonde measurements. The paper is very interesting, well written and structured and it is of high importance, concerning the potential impact of possible increasing extreme wildfire events on ozone loss. The paper should be accepted for publication but some open questions/issues should be addressed or considered in a revised version.

The authors present vertically and temporally resolved ozone anomalies using the 2010-2019 period as a reference. Is the choice of this period arbitrary? They don't provide any significance of these anomalies and it is hard to judge to what extent the event they present are unique and unexpected.

The authors associate ozone anomalies with ozone loss but they don't mention what is the contribution of dynamics in the observed anomalies. I believe that the authors claim that there is an excess chemical loss due to the presence of wildfire smoke compared to previous years. How do they exclude the impact of different dynamics. How significant are such changes (in dynamics) compared to the climatology during the years under study? These is not clear in the analysis.

The authors show relative occurrence of PSCs from CALIPSO. How does this compare to average values? Is this frequency larger than the previous years? And if yes how is this associated solely to the presence of smoke? Please comment on this.