Review of "Research on the unusual spring 2020 Arctic stratospheric ozone depletion above Ny-Ålesund, Norway"

BY QIDI LI ET AL.

General

I think this manuscript could make a contribution eventually, but it needs work. The reader cannot see clearly what is the main message of the paper (see also below). What is new? My understanding is the following: the first purpose is to introduce the new DOAS total ozone measurements at Ny-Ålesund at Yellow River Station. Then these measurements are used to investigate Arctic ozone loss in 2020. If the authors agree, then this point should come across much clearer in the manuscript. And the new DOAS instrument needs to be better described in the manuscript.

Given the fact that so much has been published on the Arctic winter 2019/2020 already (see also below), it might be more appropriate to present this work in ACP as a *Measurement Report*.

Further, the authors need to understand the background of the science they are reporting better. Some examples in detail and suggestions for improvement are given below. But as an obvious example: the authors report (on some occasions) the NAT temperature as -195 K – there are no negative values if temperature in measured in K. Overall, I think that the manuscript contains publishable material but I am afraid that restructuring and rewriting large parts of the manuscript are necessary.

Comments

What are the main messages of the paper?

First: the paper states that ozone VCD from a ground-based instrument, the GOME-2 satellite, and the Brewer and SAOZ instruments agree rather well. However, this is not a very new conclusion and had been discussed in many (mostly more technically oriented) papers before (e.g., León-Luis et al., 2018; Fioletov et al., 2002; Fioletov, 2002; Fioletov et al., 2005; Weber et al., 2005, and references therein).

Second, the paper reports that substantial ozone depletion occurred in the Arctic vortex until mid-April 2020, consistent with changes in simulated HNO_3. Again this is today not very new information; there is a special issue in JGR/GRL (and some of the papers on the Arctic winter 2020 in this special issue are cited/discussed in this manuscript) but there are a few more papers on Arctic ozone in 2020 in the meantime (e.g., von der Gathen et al., 2021; Kuttippurath et al., 2021; Ardra et al., 2022).

Third, ozone and temperature profiles were simulated by SD-WACCM, with these sim-

ulations corresponding well with ozonesonde measurements (but how well? – see below). The study used SD-WACCM with meteorological parameters driven by Modern Era Retrospective-Analysis for Research and Applications version 2 data; thus the simulation of temperature profiles by SD-WACCM is expected – isn't it? The fact that the ozone sonde measurements can be reproduced by the model is good but should be stated more clearly and in particular more quantitatively.

Finally the paper closes with the statement that "observations of ozone VCDs over Ny-Ålesund will continue in order to monitor future ozone changes over the area.". This is very good of course but not a conclusion from this paper.

WACCM

Some results of the paper rely on the model WACCM. But it is not clear how these results are obtained. I presume (although this is not stated in the paper) that openly available WACCM results have been used. If this is the case it should be clearly stated. If not, the WACCM runs conducted by the authors should be described (see also details) and then the WACCM version used should be clear. Also the way how the WACCM source code can be obtained should then be documented. Further, section 2.3 cites Kunz et al. (2011) – this is a good paper, but the paper does not deal with MERRA 2, so this sentence is confusing.

Further, which chemical scheme has been used in these simulation? I assume the most recent JPL recommendation (Burkholder et al., 2019). Müller et al. (1994, cited) emphasize the importance of CH_3O_2 + ClO for Arctic ozone loss – is this reaction taken into account in the WACCM simulation? More importantly, in which reference is the list of reactions described that is employed in the described chemical simulation? This information should be given in the paper. I also note that 'atmospheric simulations' are not mentioned in the author contribution. In general, it should be clear from the paper how the WACCM results were obtained.

PSCs

Clearly PSCs are important to polar ozone loss. However, first, one has to discriminate between PSC 'formation' and 'existence'. For crystalline particles (NAT and ice) this is not the same thing. (see e.g. Tritscher et al 2021). Also the temperature threshold for the onset of heterogeneous chemistry is not the same thing as NAT existence (Drdla and Müller, 2012, see also Tritscher 2021;Solomon1999, cited in the paper).

Further, denitrification by sedimenting NAT particles is touched upon in the paper. It is not straightforward implementing sedimentation in a model and explain the observations of large NAT particles in the atmosphere (e.g., Grooß et al., 2005; Molleker et al., 2014; Fahey et al., 2001; Tritscher et al., 2019). As simulated removal of HNO_3 in the paper is mentioned, the paper should give some information how NAT sedimentation is implemented in WACCM.

Ozone from sondes and simulation

Figure 9 (top) shows an important comparison, namely ozone sonde measurements against simulated ozone. However I suggest not showing the region below about 10 km, which is not of interest here (it also shows basically a blue area). But I think it is important to also show a plot of the differences (observations minus model) which would reveal that the model does not very well simulate to observed ozone depletion in March between 15 and 20 km. Further questions: what is the meaning of negative ozone mixing ratios (top)? WACCM seems to overestimate temperatures at about 25 km – is this a real effect?

Formation of HCl

The presented WACCM results suggest that the deactivation in the Arctic in 2021 is partly caused by formation of HCl, This is the classic deactivation pathway in the Antarctic, but not in the Arctic (e.g. Crutzen et al., 1992; Douglass et al., 1995; Müller et al., 2018). The authors might want to comment on this point.

References

Several references have been cited in this review; hopefully they are helpful. The point is not that the authors should feel obliged to cite these references. However, the paper cites WMO (2014); I suggest that a more recent ozone assessment should be used in the paper (WMO, 2018). The most recent (2022) assessment has just been released (https://ozone.unep.org/science/assessment/sap) and might be helpful when revising this paper.

Data availability

The data availability statement in this paper is not good. I suggest making the DOAS observations at Ny-Ålesund available for download on a server that issues a doi and where the data are permanently archived. Such links are reported for (e.g.) SAOZ but not for the DOAS measurements presented in the paper. Further, the WACCM data need to be better described (see above). Making data available through e-mail request is no longer recommended.

Details

- Title: I suggest avoiding “Research on” in the title; isn’t this obvious? The title should rather reflect the fact that DOAS measurements from Ny-Ålesund are reported here.

- p. 1, l. 16: why this period? (I think this is the period when measurements are available, but this should be clear from the paper).
- p 1, l 21: what is a "normal year" in the Arctic?
- p. 1, l. 21: 44.3 % $\longrightarrow$ here and elsewhere in the paper: add an error estimate for the ozone loss.
- p. 1, l. 23: here ans elsewhere: PV and ozone depletion: is this only a complicated way of saying that there is no ozone loss outside the vortex? I think that Ny-Ålesund was located outside the vortex at about April 16 (see also Fig. 8.
- p. 1, l. 26: how new is the peak in ClO (chlorine activation)? Compare the papers in the JGR/GRL special issue?
- p. 2, l. 38: this is not a good description of halogen induced polar ozone loss (e.g., Müller et al., 2018, and Solomon 1999, Tritscher 2021, cited in the paper).
- p 2., l. 42: 'recovery' is an important issue, it is different in the polar regions and in mid.latitudes (WMO, 2018). See also further papers on the recovery of both the Antarctic ozone hole and global ozone levels (e.g., Kuttippurath and Nair, 2017; Strahan and Douglass, 2018; WMO, 2018; Bodeker and Kremser, 2021; Stone et al., 2021; Weber et al., 2022).
- p. 2, l. 49: there should be more citations here than just Hu 2020.
- p. 3, l. 75: Simpson is on boundary layer issues: this reference needs to be changed. There are several alternative citations, already cited in the paper and there are further modelling papers cited in this review.
- p. 3, l. 75: These citations focus on one particular model (CLaMS), which is okay. But I think you should have citations to other models here as well (e.g., Chipperfield, 1999; Khosrawi et al., 2009; Bekki et al., 2013; Chipperfield et al., 1994; Kinnison et al., 2007; Wohltmann and Rex, 2009; Wohltmann et al., 2010, []).
- p. 4, l. 99: You cannot start the Methods section with "the DOAS instrument". Which instrument? I think it is a new instrument that is described below – correct? This should be much clearer from the paper and the instrument needs to be described first before it can be "placed" somewhere. Further, given the fact that the DOAS technique is so prominent here (or should be) a bit more background on DOAS and citations (see perhaps, Hüneke et al., 2017) might be appropriate.
- p. 6, p. 140: this sentence starts with 'parameters' but the paper should state what was actually done regarding WACCM.
- p. 7, l. 166: ERA5 has 137 layers – is there a typo here?
- p. 7, l. 167: where have these measurements been done?
- p. 8, l. 173: what is a 'normal year'?

- p. 9, l. 199: what is the 'threshold temperature'? This is an important point that should be discussed in the paper.
- p. 9, l. 201: by definition the PV in the southern hemisphere is negative and positive in the northern hemisphere. This simple fact should be taken into account when making such statements.
- p. 10., l. 235: apparent $\longrightarrow$ obvious?
- p. 10, l. 243: 'recover' is problematic here, it is not the right word to use when taking about chlorine deactivation putting a halt to ozone loss.
- p. 10., l. 237: it is not only the reaction HCl + $ClONO_2$
- Figure 5: cold the errors of the individual measurements be used for weighting the data when calculating regression etc?
- Figure 6: Show error bars?
- Figure 7: the blue line shows 195 K, which is an approximation for the onset temperature for heterogeneous chemistry.
- Figure 8: show error bars?

References

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