

Atmos. Chem. Phys. Discuss., referee comment RC1  
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## Comment on acp-2020-1313

Anonymous Referee #2

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Referee comment on "Exceptional loss in ozone in the Arctic winter/spring of 2019/2020" by Jayanarayanan Kuttippurath et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-1313-RC1>, 2021

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I struggled with this review because while the paper presents a lot of information, and the authors have done a lot of work, it is difficult to see what is new. The major discoveries announced in the abstract, for example, "Sporadic occurrences of ozone hole values ... record-breaking ozone loss of about 2.0–3.4 ppmv ... unprecedented chlorine activation ... first-ever appearance of loss [near] saturation in the Arctic..." have been made, and published, by Manney et al. (2020) or Woltmann et al. (2020). The title ("on the verge") suggests that the authors think we will see a real ozone hole, or more ozone holes in the future. But they fail to present any dynamical arguments for that. Figure 1a of Woltmann et al. (2020) in fact suggests this, so it would be interesting to see a climate model prediction of the like, or more long-term analysis of trends in dynamical parameters.

Instead, the paper waffles back and forth about whether there was an "ozone hole" in 2020, even contradicting itself, e.g., "all the methods, data, and parameters converge to provide an undeniable fact of the first-ever ozone hole" and then "the ozone loss in the Arctic cannot not be called as "an ozone hole"." In any case, this is not an important scientific argument, but a quibble about terminology.

Figure 3 is very nice, with a lot of important information, but that information is already in Manney et al. (2020). The ozonesondes, and the degree to which loss saturation was approached, are thoroughly presented in Woltmann et al. (2020). Loss saturation was never reached, in fact: Antarctic ozone hole profiles frequently show ozone below the detection limit of the sondes ( $\sim 1$  ppbv), while the lowest observed last spring in the Arctic was 125 ppbv. Sondes can measure ozone levels below 100 ppbv with good accuracy (as they do in the troposphere).

I did, however, find the discussion of the ozone mini-holes in December 2019 and January 2020 quite intriguing, especially the observation that they contained high ClO. Mini-holes are generally regarded as dynamic phenomena, so the suggestion that heterogeneous chemistry is occurring is interesting. It might be interesting to explore this further: are they also becoming more common? Do they affect the overall loss of ozone? See also Stenke and Grewe (2003). I noted that in Figure 7 the mini-holes were the only point where TCO fell below 220 DU. That seems worth exploring.

I also appreciated the long-term comparison with previous years in Figure 6. Perhaps this could be expanded, along with an analysis of the long-term changes in vortex temperature, V\_PSC, wave disturbances/stability...

Minor points:

Line 23: "provided the stratospheric chlorine levels still stay high there." I don't think there is much uncertainty about future Cl levels.

Line 34: "because"? Perhaps "possibly because". This is far from certain, or we wouldn't still be producing ozone assessments. In fact a lot of data show the opposite (decline since 1997).

Lines 58-59: " Here, we show that the Arctic winter in 2020 ... met the condition for an ozone hole for the first time". What condition? This disagrees with most other assessments (e.g. Woltmann et al., 2020; Manney et al., 2020; Wilka et al., 2021).

Lines 60-65: Should indicate where the data were obtained. Uncertainties are quoted but no citation is given.

Line 80: "The missing values in satellite measurements were filled with linear interpolation (poison\_grid\_fill)." What is "poison\_grid\_fill"? How does it work? What are the criteria used for filling?

Line 120: A lot of this paragraph is confusing, but this line especially. T\_NAT is 195, not 200 K.

Line 133-134: "This is the largest ice PSC ever observed in terms of its area, volume and number of days of appearance (i.e. frequency) in the Arctic and the area is twice that of the winter 2011." So what? This information is never used for anything.

Lines 151-156: This is interesting, potentially, but vague and hand-waving. It could be really valuable to have an analysis that looks at the evolution and variation of the Arctic vortex over the last 20+ years.

Lines 210-212: This analysis might make an interesting paper, if expanded.

Lines 360-364: This interpretation is incorrect. The sondes do indeed have an uncertainty of about 10%, but that means that the minima of 0.125 (or 0.200) ppmv would have error bars of  $\pm 0.012$  (or  $\pm 0.020$ ). That is not consistent with zero, or even 0.1 ppmv.

Reference

Stenke, A., and V. Grewe (2003), Impact of ozone mini-holes on the heterogeneous destruction of stratospheric ozone, *Chemosphere*, 50, 177-190, [https://doi.org/10.1016/S0045-6535\(02\)00599-4](https://doi.org/10.1016/S0045-6535(02)00599-4)