I have two general comments on this manuscript that I think raise important issues that should be addressed before publication:

Inadequate citation of and discussion of relationships to previously published papers on ozone loss and meteorology in the Arctic 2019/2020 winter:

There are at least about a dozen peer-reviewed papers already published on the 2019/2020 winter, including one comprehensive overview of the meteorology and its relationships to ozone loss (Lawrence et al 2020) and many that discuss and / or model chemical ozone loss in the Arctic vortex and the record low ozone values. Only two of these papers (the Manney et al, 2020 and Wohltmann et al, 2020 papers listed) are cited here. Many, but not all, of these are in the JGR/GRL special issue,
https://agupubs.onlinelibrary.wiley.com/doi/toc/10.1002/(ISSN)1944-8007.ARCTICSPV
In which the first papers were published online in July 2020 and all except two recent ones published in or before November 2020. All of these contain material that would be useful to cite (though a couple of the dynamical ones possibly only briefly for context) in this paper, and some of them seem critical to cite. In particular, Lawrence et al (2020) needs to be cited for the discussion of the meteorology leading to the exceptional ozone loss. The material in Figures 1 and 2 of the current manuscript are, as far as I can tell, completely covered by Lawrence et al (2020), Wohltmann et al (2020), and Dameris et al (2021, ACP, https://doi.org/10.5194/acp-21-617-2021), so if they are to be included in the final paper, the authors need to highlight something that is new in their presentation of the material. (In that discussion it would also be worth citing DeLand et al (2020) for actual PSC observations.) All of the following papers include discussion of anomalous column ozone and its implications, and should be cited in addition to Wohltmann et al. (2020): Rao and Garfinkel (2020), Inness et al. (2020), Bernhard et al. (2020), Dameris et al (2021, ACP), Feng et al (2021), Weber et al (2021). Several of these papers (as well as Manney et al, 2020 and Wohltmann et al, 2020) include estimates from data and/or modeling of amounts of chemical ozone loss in 2019/2020 in relation to previous years (including especially 2010/2011), and the results in this paper should be discussed in the context of those in these papers, and what is new in this paper clearly highlighted.

Inadvisably casual use of the term "ozone hole" for the Arctic:

There are many reasons (first discussed extensively in relation to the 2010/2011 winter, e.g., see Solomon et al, 2014, https://doi.org/10.1073/pnas.1319307111) why one should be very careful and precise about applying the term "ozone hole" to the Arctic. There is some discussion of this in Wohltmann et al (2020), and I do not want to go through all of the detailed arguments again, so I **strongly** urge the authors of this manuscript to read the reviews of (especially the one by Dr. Wohltmann) and the SC by Grooß & Manney in the discussion of Dameris et al (2021, ACP) for a comprehensive discussion of this point, and use these cautions to consider and revise the presentation of the results in this paper accordingly.