REPLIES TO THE REFEREE COMMENTS

In their revised manuscript, Kuttippurath et al. have addressed the earlier comments. Most importantly, it wasn't really clear what this study adds, that was not known from other published studies on the Arctic winter 2019/20. Nevertheless, the present study is scientifically sound, appropriately acknowledges earlier studies and provides a comprehensive overview. So all in all I recommend publication after consideration of some additional, most minor, comments:
We thank the reviewers for the positive comments and suggestions for improving the paper. Our point-by-point responses to the reviewer comments are detailed below in blue text, and the changes are shown in the version of the manuscript with track changes.

Specific comments:

l.64: "The occurrence of extreme events is a feature of climate change": I'm having problems with this statement as this is too general here: There are always extreme events, even without climate change. I suggest to drop the new sentences "The occurrence of extreme events is a feature of climate change (e.g. IPCC, 2007). Therefore, the extremely cold winters with large loss in ozone could also be a harbinger of climate change." And continue as before with "Previous studies have postulated …", which is much more to the point.
Done, the statement is removed as suggested by the referee.

l.71: "However, in this study, we use different data sets, different loss estimation methods, and several assessment parameters together to study the polar processing and ozone loss in the Arctic winter 2020, and such an analysis is never done for this winter. This is particularly important as the winter was very cold with the largest ozone loss in the observational record and experienced the total column ozone (TCO) values below 220 DU for several days in the vortex, for the first time."

I suggest to avoid statements like "never done" and "for the first time" and just state (matter of fact): "In this study, we use different data sets and several parameters together, to investigate the large ozone loss in the Arctic winter 2020, and the conditions that led to the record low total column ozone (TCO) values below 220 DU in this winter." (Or similar)
Done. We have removed the statement and revised as suggested by the referee in lines 71-73

l.252: The finding of high amounts of ClO in air masses associated with the mini holes are very surprising, and I believe it was already suggested in the previous review round that it may be worth to further investigate that. Unfortunately, this is not further addressed here.
Done. We are very sorry that we cannot discuss this further in this paper as it involves more detailed analysis. This paper is already a very long (13000+ words and 8 figures). As suggested, we would do a separate study in this regard. We hope that the referee and the editor would find it as an appropriate decision. Thank you.

On the other hand, similar studies were also done in the past on ozone mini-holes. For instance, Weber et al. (2002) state the temperature can be below PSC threshold and thus, high ClO can be there. Feng (2006) also demonstrate large areas of PSCs and high ClO in the mini-hole region. These are mentioned in lines 258-260.

l.451: "The appearance of a threshold in TCO below 220 DU for several weeks demonstrates that Arctic winters may enter a new era of ozone depletion events, and signal significant changes in the climate of the region": I don't agree with this statement. It is fine to say that the Arctic may have entered a new level of ozone depletion, but whether or not this signals significant climate change or

may be seen as a new era remains to be shown. This is not supported here in this study or by the given references.
Done. We have deleted " … and signal significant changes in the climate of the region" in lines 448.

Minor comments
l.30 "Montreal protocol" -> "the Montreal protocol"
Done. Please find it line 30

l.40: "high temperatures" -> "higher temperatures"
Done. Please find it line 40

l.45: "the ground-based" -> "ground-based"
Done. Please find it line 45

l.45-48: "ground -based measurements show about 15–20% of loss in most Antarctic winters", I'm confused at this point: is this a typo and should read "in most Arctic winters"?
Done. Thank you for correcting this. Please find the "Arctic" in line 46

l.81: I suggest to move the statement on the uncertainty of the ozone sonde measurements to the end of the next paragraph, after the statement on the satellite uncertainty. Maybe also moving the list of species from MLS up to l.77/78.
Done. The specie added to line 78 and the ozonesonde uncertainty added to the next paragraph end, in line 108

l.163-168: "The diagnosis with heat flux and the eddy heat flux associated with waves demonstrates that the momentum transported from the troposphere to stratosphere was very weak in 2020 (in the range of -20 to 30 Km s -1), and the heat flux values are zero or negative (e.g. -10 Km s -1 in February) during most part of the winter. These results are also in agreement with the eddy heat flux computed for the waves, as they also show smaller wave momentum to the stratosphere. In short, the eddy heat flux and wave heat flux show smaller values in January-April; indicating the reason for the less disturbed long-lasting vortex in 2020."

I find these lines confusing. As I understand both panels in Fig.1 show eddy heat flux, one calculated for all waves, the other for waves 1-3, right? Maybe you could make this clearer.
Done. As suggested, we have specifically mentioned the total or net heat flux and eddy heat flux associated with planetary waves 1, 2, and 3 in lines 163-168

l.194: "We use the profile descent method using the trace of air motions N2O and is a widely used method for ozone loss estimation": This sentence is not very clear.
Done. We have revised it as "We estimate the descent rate from the tracer N_2O inside the polar vortex, then assume the averaged profile descent rate is identical to the dynamical ozone, so that the chemical ozone loss can be derived (e.g., Griffin et al., 2019). This is a widely used method for chemical ozone loss estimation. Please find it in lines 195-197

l.221: "and there is no sunlight in the Arctic vortex in early winter": this statement contradicts what is said a few lines further down. Maybe write "…and one would not expect significant amounts of sunlight in the early winter Arctic vortex…"
Done. Thank you and we have revised as suggested in line 221

l.252: "TCO transported": better say "ozone transported" to link to the discussion on profile changes in the next lines.

Done, please find it in line 253.

l.315: “A very similar conclusion is also presented in the study of Grooß and Müller (2021).” -> “…higher than that of any previous Arctic winter (Grooß and Müller, 2021).”
Done. Please find it in line 315.

l.338: “record-breaking ice PSCs” -> “record-breaking extent of ice PSCs” (or whatever quantity you are referring to here)
Done. Please find it lines 337-338

l.343: “We also looked at the changes in EESC during the period (2005-2020) and there has been continuous decline in EESC during the period (Fig. 6, top panel). The rate of change of EESC during the period is about 246.16 ppt per year (e.g. WMO, 2018); suggesting a consistent reduction in stratospheric halogen loading in 2020 (e.g. Grooß and Müller, 2021).” This is a bit wordy and can be expressed more compact as “During the period 2005-2020 there has been a continuous decline in EESC by about 246.16ppt per year (e.g. WMO, 2018).” However, how can WMO (2018) be cited for changes up to 2020? I don’t understand why this suggests a consistent reduction in halogen loading: this is the change in stratospheric halogen loading. Or do I miss something here?

Done. We have revised the text as suggested by the referee now in lines 343-345. “We also looked at the changes in EESC during the period (2005-2020) and there has been a continuous decline in EESC during the period (Fig. 6, top panel). The predicted rate of change of EESC during the period is about 246.16 ppt per year (e.g. WMO, 2018); suggesting a reduction in stratospheric halogen loading in 2020 compared to the peak loading by about 10% (e.g. Grooß and Müller, 2021).”

l.436: “column ozone loss ever measured”: I’m having problems with the statement, that the column ozone loss was measured, as we cannot directly measure column ozone LOSS. We can only measure ozone columns or profiles and derive chemical loss using certain assumptions (as described in this paper).
Done. Agree, it should be estimated or calculated. Rephased as “deduced hitherto” in line 434