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Review of the manuscript "Global total ozone recovery trends derived from five merged ozone datasets" by M. Weber

Anonymous Referee #2

Referee comment on "Global total ozone recovery trends attributed to ozone-depleting substance (ODS) changes derived from five merged ozone datasets" by Mark Weber et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-1058-RC1>, 2022

The manuscript "Global total ozone recovery trends derived from five merged ozone datasets" by M. Weber provides an update to a study published by the first author in 2018, with four more years of data added to the five analyzed datasets (four satellite datasets and one dataset comprised of ground-based measurements). A multiple linear regression is applied to annual mean data from the period 1979 to 2020 to determine total column ozone (TCO) trends in different broad latitudinal bands for the period in which concentrations of ozone depleting substances (ODSs) increased in the atmosphere, and for the period after the peak concentrations had been reached. The multiple linear regression includes next to the typical proxies also several dynamical variables (e.g. a proxy for the Brewer Dobson circulation (BDC) or the Antarctic/Arctic Oscillation (AAO/AO)) which is one of the main differences to other trend analyses based on TCO data. The authors find with this method significant positive trends (related to the reduction in ODSs in the atmosphere) for the period 1997-2020 for the near global mean (60S-60N), as well as for the Northern hemisphere mid-latitudes for which the trend is near zero if the dynamical proxies are not included in the regression.

The manuscript is very well written and well structured, mostly the data and methods are explained in enough detail to allow the reader to understand what is going on (in a few cases I found the description slightly too short and I have mentioned them in the details below), and the topic lays clearly within the scope of the ACP journal. There are a few minor things that I commented about below that are easy to fix, but there are two main points that I think need careful adjustment of the manuscript or some additional thought.

I recommend the publication of the manuscript after revisions.

Two main points:

- Attempting an attribution with a multiple linear regression that includes non-orthogonal proxies is tricky. Especially if several proxies include a trend. The hope then is, that the regression is able to separate the trend contribution from the different proxies based on the additional variability the proxies provide. However, it is possible that trends are not assigned correctly to the different proxies which would falsify the signal of the trend that if of interest, in this case here, the trend caused by ODSs and not by changes in dynamical variables. The authors argue that with the addition of the dynamical proxies the variability of the time series' are matched better by the regression results. There are two points that make me somewhat doubtful of this statement: (1) the pre-1996 trends change clearly with the introduction of the dynamical proxies (Figures 3 and 4) although the main trend signal should be coming from ODS-related changes in this period; (2) the signal from the SH Brewer Dobson circulation proxy in the NH polar regions that cannot really be explained. I think the manuscript needs more discussion of these points to strengthen the claim that the addition of the dynamical proxies can indeed robustly isolate the ODS-related trends. For the first point I raised I would suggest to check the older literature about regression results for the pre-1996 period where dynamical proxies have been used. I have added two references in the comments below that might be worth checking out. And there might even be more that could be checked and where the results could be compared to the pre-1996 ODS-related trends calculated here. For the second point I raised I think it would be helpful to do some sensitivity test to check the robustness of the trend results and the contribution of the individual proxies: (I) not using the trend proxy but JUST the dynamical proxies, how do their contributions change if at all; (II) use some of the dynamical proxies only in the regions where they occur, e.g. AAO only in the SH, AO only in the NH, etc.; how does the contribution of these proxies change (if at all), and how does the ODS-related trend change? I think these sensitivity test will go a long way to show the robustness of the results presented here in this manuscript.
- I think it is really important to clarify throughout the manuscript (including the title!) what kind of trends the authors talk about. Mostly, the trends that are discussed are the trends that are attributed to the reduction of ODSs in the atmosphere WITHOUT any contribution of dynamics to the trend. In many places this is not totally clear since the trends are only called "recovery trends". However, for me this is the main point of the manuscript and the difference to other studies. It would therefore be extremely important and very helpful if the authors could be more specific in how they name the trends throughout the manuscript (e.g. instead of referring in the abstract in line 11 to "The near global trend of the median of all datasets..." it would be better to be more specific and refer to "The near global ODS-related trends ...", and specifying this in the title like "Global total ozone recovery trends attributed to ODS changes derived from five merged ozone datasets")

Minor comments:

- Line 10: "... is indeed on slowly recovering..." – remove the "on"
- Line 16: data from which phase of CCMI? Please specify.
- Line 71: It is not clear in this section what the spatial coverage of the described datasets is. I assume 90S-90N since also polar regions are analyzed. Please add this information to the dataset descriptions.
- Line 72: "ground-based" instead of "ground"
- Line 78: "ground based Brewers, ..." - remove the "ground based" since it is already mentioned at the beginning of the sentence.
- Line 80: Add also here the information from which phase of the CCMI project simulations were analyzed.
- Line 129: It is not clear how and by whom the ground-based dataset was updated. The references for the dataset are relatively old, therefore it would be good to add a few words on how the dataset was updated to the year 2020.
- Line 135: The word "belt" is used here, although it is only explained in the following sentence what exactly is meant by it. This should be switched to make it clearer for the reader what is meant with "belt".
- Line 154: the data were bias corrected. It would be nice to give here a range of biases that needed to be adjusted. I understand that the biases can be different for the broad latitude bands and datasets, but some kind of number/range would be nice here.
- Line 169: "applies" should be "apply"
- Line 175: "." is missing after the parenthesis.
- Line 176: The year 1996 is the time for maximum EESC concentrations for which region of the globe? Tropics? Everything beside the polar regions?
- Line 177: It would be good to give the exact latitude ranges here which define the polar regions.
- Line 190: The end of the sentence is slightly misleading. I would add "for these years" before "were calculated" to clarify that only for the years with too many missing data no annual means were calculated.
- Line 226: What about the pre-1996 trends? Did they stay very similar to W18 as well?
- Line 248: "agree" instead of "agrees"
- Line 255-257: It might be nice to add here a table with the trends reported from W18 and calculated here. It would provide a nice overview of things that changed and things that stayed roughly the same (just for the multi-observational median, not each individual dataset)
- Line 269: "ground-based" instead of "ground"?
- Line 285: Are there any studies that report on trends pre-1996 based on regression methods that use also dynamical proxies? There is one looking at ozone soundings at Payerne (Weiss et al., JGR, Vol. 106, D19, 22685-22694, 2001), and one looking at individual TCO station measurements (Maeder et al., 2007, <https://doi.org/10.1029/2006JD007694>) but there might be even more analyzing total column ozone data with dynamical proxies. As mentioned above, I think it would be helpful to provide an estimate how well the ODS-related trends compare with earlier findings for the pre-1996 period since they did change quite a bit with the introduction of the dynamical proxies.
- Line 305/306: Couldn't this signal be a spurious regression result where the attribution did not work properly between the trend proxy and the dynamical proxies also including a trend? I think some sensitivity test (as mentioned above) would be helpful here to test the robustness of this signal.
- Line 316: "." missing after the parenthesis.
- Line 331: "have" instead of "has"
- Line 366-368. This sentence seems somehow out of place here. I think it needs a little more explanation and detail.