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## Comment on acp-2021-1058

Anonymous Referee #1

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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-RC2>, 2022

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Review of 'Global total ozone recovery trends derived from five merged ozone datasets', Weber et al., ACPD, 2021

### 1. Short resume

Weber et al present a comprehensive analysis of trends in total ozone, focusing primarily on the period since the turnaround in ozone-depleting substances. This is an update and extension of earlier work published in 2018. In contrast to latter publication, the authors now claim the detection of increases ( $0.4\%$ /decade) in near-global (60S--60N) total ozone since 1996, with high confidence ( $>3\text{--}4\sigma$ ). Positive trends over broad mid-latitude region in both hemispheres (35N--60N and 35S--60S), about  $0.5\text{--}0.7\%$ /decade, are significant as well although close to the 2 sigma detection threshold.

The dynamical proces terms (Arctic and Antarctic Oscillation, Brewer-Dobson circulation) in the regression model play a central role in this detection, especially at northern mid-latitudes. The authors deliberately chose not to detrend the dynamical terms prior to regression, in order to account for any long-term changes in AO, AAO and BDC. In doing so, they find that trends become less negative before 1996 and more positive since 1996 across large regions of the low- and mid-latitudes. This more complete attribution results in a higher significance of the trends, especially in the northern hemisphere where the  $2\sigma$  detection threshold was passed. Hence, the authors conclude that dynamical changes appear to counterbalance the recovery of ozone in the mid-latitude NH.

The authors furthermore explain the positive recovery trend of total ozone as a result of changes in ozone-depleting substances. Indeed, the ratio of the rate of increase and decrease in ODS concentrations is consistent with the rate of depletion and recovery of total ozone across all  $5^\circ$  latitude bands between 60S--60N.

## 2. Recommendation

This paper provides an important update to previous assessments of long-term changes in total ozone. It is very well written and accessible to a large scientific audience. The methodology is sound and the presented results support the claims made by the authors. I highly recommend publication of this work in ACP if my remarks below have been addressed.

## 3. Major comments

Ordered in order of appearance in the text.

### *3.1 Extension of GOME-type backwards in time (Sect. 2.7)*

I understand the importance of covering a sufficiently long period, but is this backwards extension for GOME-type data records still needed now that more than two solar cycles have been completed since 1995? Doesn't this break the independence between SBUV and GOME-type estimates? By how much does the negative trend in the SBUV period influence the recovery trend estimates during the GOME-type period?

Have you tested the sensitivity of the resulting trend to the choice of NASA COH or NASA MOD, and without the extension?

Avoiding data gaps is important but preserving data quality / stability is perhaps even more important under high aerosol backgrounds. Could you elaborate why gaps are more important or, if that is not the case, comment about the stability of both SBUV records after Pinatubo?

### *3.2 No reference to how trend errors are estimated (Sect. 3)*

Many trend estimates (Fig. 3) are close to the  $2\sigma$  threshold. The computation of MLR coefficient uncertainties therefore deserves some attention, this is missing right now. Please explain how MLR parameter errors are computed or refer to relevant publications.

Somewhat related to this, was there any consideration of including reported measurement errors in the regression?

### *3.3 Annual time series*

p.7, l.180: Could you motivate the choice for analysis of annual mean time series instead of monthly mean data? Is there an impact on the trend estimates and their significance? Please refer to relevant publications.

### *3.4 Robustness of attribution to dynamical processes (Sect. 5)*

Previous work by the authors (Weber et al, 2018) also considered terms for dynamical processes in the MLR. At the time, however, no significant positive trends were detected (Fig. 9).

It would be enlightening to discuss whether the four additional years of data have truly helped to attribute ozone changes more robustly to dynamical changes. Or, whether it is plausible that the current attribution is subject to geophysical variability (and measurement uncertainty).

## **4. Minor comments**

p.1, l.12-13: Near-global trend values disagree with quoted values in Section 4. Please revise.

p.3, l.82: "Annual mean timeseries of all five merged datasets are in very good agreement". Somewhat subjective, please add a number.

p.5, l.132: The evolution in satellite quality has been described adequately. This is missing in the WOUDC section. Surely, there must have been progress in the calibration of these instruments or the coherence of the network since the work by Fioletov in 2008. If so, could you update this section accordingly?

p.6, l.142-143: "[...] can be estimated with a precision comparable with satellite-based data sets ( $\sim 1\%$ )." A reference would be appropriate.

p.6, l.150: Remove "from the past into the future" as the statement "between 1960 and 2100" is more than sufficient.

p.6, l.154-156: I am sorry, I did not get the point of "The multi-dataset mean was then

added back to each dataset, such that all bias corrected timeseries are provided in units of the total column amounts (W18). However, the trend results derived from them are identical to those derived using anomaly timeseries." Could this be clarified a bit better for the non-expert?

p.6, l.154: "to the mean". The 1998-2008 mean at the global or local level?

p.6, l.165: See comment below, the second term in Eq. 1 should be  $b_1(t-t_0)$ .

p.6, l.166: "coefficients  $b_1$  and [...]" This is inconsistent with the notation in Eq 1. Sign of first trend term  $(t_0-t)$  implies that positive  $b_1$  values represent a decline in ozone. Please change this. The factors  $X_1(t)$  and  $X_2(t)$  define the decline/recovery periods.

p.6, Eq.2 and 3: Figure 1 suggests that the "recovery" period starts in 1996, so the turnaround is defined as  $t_0=1996$ . If this is correct, then the notation in Eq. 2 and 3 should be changed to  $X_1(t)=1$  for  $t < t_0$  and  $X_2(t)=1$  if  $t \leq t_0$  (and vice versa for  $X_i=0$ ). The trend model is not continuous at  $t_0$ , hence  $<$  or  $\leq$  do make a difference.

p.7, l.185-187: Is there any particular reason why you haven't used GloSSAC v2 (Kovilakam et al., 2020)?

p.8, Table 2: EHF is missing from this list. Where can it be downloaded?

p.8, Eq.4: "BDCn" and "BDCs" should be explained in the text.

p.8, l.208: "the linear trend terms best approximate EESC related trends". Can a match between ozone trend and EESC expectations really validate the choice of terms in the MLR? There is risk of a circle reasoning here. If the improved agreement with EESC expectations is motivating the choice of terms in the MLR model then you can't use this same agreement again to conclude a causal relation between trend and EESC.

p.8, l.215-216: This phrase is not entirely clear on whether or not you use the detrended proxy. This choice is so central to this paper that it must be very clearly stated.

p.9, Fig. 1:  $\chi^2$  is the sum of "the squared differences median timeseries minus MLR"

p.9, l.219: "MLR prediction after fitting" would be clearer than "MLR result from applying".

p.9, l.220: To me, "after 1996" suggests 1996 is not included. What about replacing "after 1996" by "since 1996" throughout the manuscript?

p.9, l.224: "recovery from reductions in ODS" would be more clear on the effect of ODS on ozone.

p.11, l.260: Replace "from applying" by "when applying"?

p.11, l.260: It is somewhat unexpected to regress a "super"merged timeseries rather than average the trends from individual records. What is the rationale? Also, the sample size is just  $N=3$ , for 1979-1995, so won't the "super"merge-then-regress method lead to more uncertainty in the MLR parameters than the regress-then-average approach?

p.12, Table 3 (caption): The periods in the caption are inconsistent with information in Figs 1 and 2. The first trend period stops in 1995, the second starts in 1996. Hence, it should be  $1979-1995$  and  $1996-2000$ .

p.12, Table 3: For each latitude belt, the occurrences of "mean/median trend  $>1996$ " should be  $\geq 1996$ , in order to be in line with Fig. 1 and 2.

p.12, Table 3: The error notation was confusing for me, I haven't seen this specific notation very often. For instance, what does  $-1.9(13)$  mean? Is it  $-1.9 \pm 0.13$  or  $-1.9 \pm 1.3$  or ...? I find an explicit notation such as  $+0.4 \pm 0.2$  much more effective. I recommend using this throughout this table and also the manuscript.

p.12, l.265: "One notable change from W18 is that the tropical trends during the ODS rising phase are now more negative (down to  $-1\%$ /decade) while before they were mainly close to zero. This may be caused by the additional proxy terms used in this study". The pre-1996 data have been available for a very long time now. Has this effect never been looked into before? If so, please refer to relevant work.

p.12, l.270: Please replace the "maybe" (conditional) by an "is" (certainty). Trend uncertainty scales with  $n^{-3/2}$  (e.g., Weatherhead et al., 2000) so the eight more years in the recovery period already lead to  $\sim 45\%$  smaller trend error. This seems not too far from the observed factor  $2\times$  reduction of the error in Table 3 and Fig. 3.

p.13, l.274: "The expected tropical recovery [...]". Estimated mid-lat NH recovery trends are too small compared to EESC prediction as well.

p.15, l.320: "NH total ozone has been steadily declining..." conflicts with the first phrase of this paragraph "stable ozone levels in NH since 2000". Please clarify the text.

p.15, l.324: "with larger springtime polar ozone losses"?

p.15, l.325: Remove "recent" from "A recent downward trend". Perhaps you meant that this was recently reported? Ball et al report a continuous decline since the 1980s, not a recent decline.

p.18, l.332: Quoted recovery trend value ( $11\%/decade$ ) conflicts with that in Figure 7 ( $12\%/decade$ ). Please correct.

p.19, Table 4: Same comment on error notation as in Table 3 (p.12).

p.19, l.367: The Gaudel paper is about differences between tropospheric ozone data records. So probably not the best reference when the message is about consistency between tropo/strato/total ozone.

## **5. Technical corrections**

p.1, l.10: Remove "on" from "[...] is indeed on slowly [...]".

p.1, l.12: Remove "in absolute numbers".

p.1, l.15: Add "-" to "chemistry-climate models".

p.2, l.30: Typo "stratosphere".

p.2, l.38: Remove "agreement" from "Montreal Protocol agreement".

p.3, l.75: Replace "in large part" by e.g. "largely".

p.3, l.79: Replace "Observations Zénithales" by "Observation Zénithale".

p.4, l.87: Replace "are processed using the same V8.7 retrieval algorithm" by e.g. "are retrieved using the same V8.7 algorithm".

p.4, l.108: Type "[...] shift to an equivalent [...]".

p.5, l.130-132: Double occurrence of ground-based. First one could be removed, e.g. "The WOUDC zonal mean ...".

p.7, l.175: Add "." after "W18)".

p.7, l.189: Replace "there are not sufficient number of months" by e.g. "there are not enough months" or "there is not a sufficient number of months".

p.7, l.194: Replace MLR "equation" by MLR "model"?

p.8, l.212: Remove "the possibility", as it is a bit redundant.

p.8, l.212: Replace "MLR results" by "MLR fit residuals" perhaps? This is a bit clearer as the MLR parameter estimates are MLR results as well.

p.9, l.218: "five bias-corrected" instead of "bias-corrected five".

p.11, l.242-243: Maybe you forgot to remove the newline between paragraphs?

p.11, l.251: Add a "\$+" sign to the quoted values at start of this line.

p.11, l.256: Remove ' after "timeseries".

p.11, l.261: Add "/decade " after  $+0.5\%$ .

p.12, Table 3 (caption): Remove "and" from caption "[...] in bold have an absolute [...]"

p.12, Table 3 (caption): Add "prediction" at the end of "and mod $_i$  the MLR".

p.12, Table 3: Add "\$+" to trend value  $\geq 1996$  for median time series near-global.

p.12, Table 3: The quoted  $r^2$  value for WOUDC in 20S-20N band is single digit (0.7), should be double (0.70).

p.13, l.276: Remove "on" from "elucidate further on".

p.13, l.285: Type "Fig. 4a" should be "Fig. 4".

p.15, Fig.5 (caption): There is a missing word in "Negative values an anti-correlation [...]".

p.15, l.311: Add "s" to "chemical effect"?

p.15, l.316: Add full stop at end of phrase.



p.17, Fig.7 (caption): Capitalise "See".

p.18, l.331: "Earlier signs of ozone recovery have been", should be plural.

p.18, l.331: Add "," in between "Now with".

p.18, l.332-333: "During September, the Antarctic ozone hole usually grows and [...]".

p.18, l.340: Remove "as shown in Fig. 7". A bit redundant, you already referred to the figure in the previous phrase.

p.18, l.344: Replace "globally" by "global"?

p.18, l.352: Add "," in between "tropics recovery".

p.18, l.354: Add "," in between "Arctic large".

p.19, l.363: "chemistry-climate models".