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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "An update on ozone profile trends for the period 2000 to 2016" *by* Wolfgang Steinbrecht et al.

Anonymous Referee #2

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Overall, the manuscript represents an important and timely update to the question of the magnitude and significance of trends in stratospheric ozone, which are an important indicator of the efficacy of the Montreal Protocol. The paper is generally well-written and the analysis is robust. There needs to be, however, a clearer, more focused discussion of the relationship between this paper and Harris et al. [2015] as a thread that one can follow throughout the paper. Instead, I found myself having to re-read Harris et al. to understand the primary difference between their analysis and WMO2014, and then having to read between the lines here to understand whether this was an apples-to-apples type update to the Harris et al. analysis or a simple extension of the WMO2014 analysis. The issue is easily fixed by being more clear about the differences between WMO2014 and Harris et al. (2015) early in the paper and by guiding the reader to

understand that the analysis here uses the same uncertainty assessment (based on the J-distribution) that led to the larger uncertainties in Harris et al. (2015) relative to WMO2014. I therefore recommend publication in Atmospheric Physics and Chemistry with minor revisions.

Specific Comments:

Page 2

Line 2: It would be clearer to say "ground-based data [collected or measured] by four techniques....

Lines 6-8: This sentence should be rearranged to clarify that "more years of observations and updated data sets" refers to a comparison to WMO 2014 and Harris et al. 2015. (Suggested: "This study confirms positive trends already reported in…using three to four more years…."

Lines 8-10: Here it would be helpful if the authors were specific regarding the reduction in uncertainty relative to Harris et al. that is the result of the improved datasets, which have a lower inferred drift.

Line 29: It seems to me that a brief mention of confounding factors is warranted here. We do indeed expect ozone to increase, but it is important to be clear that variability and trends in the circulation, temperature changes, etc, can easily mask those increases and lead to large uncertainties on calculated trends even for ideal data records.

Page 3

Lines 4-6: A more in-depth discussion of the differences between Harris et al. and Hubert et al. and the WMO 2014 results is needed here. What drove the larger uncertainties in those studies relative to WMO? Are the differences in uncertainties something that can be at least partially addressed by longer and / or improved records?

Section 2: The last paragraph of the introduction mentions "improved and additional datasets". A brief summary of which datasets have been improved or added to the analysis since WMO 2014 would be beneficial in Section 2.

Line 13: For people with less familiarity with these stratospheric trend studies, it might not be clear why there is a focus on records starting before 1990 given that your analysis looks at the 2000-2016 period. Lines 23-26: This is confusing if one is unaware that there were two SAGE instruments. Line 29: Improved in what way?

Page 4

Lines 1-3: A brief mention of the different assumptions used in SWOOSH and GOZCARDS would be helpful to a reader trying to understand how independent these datasets are.

Lines 17-28: The role of the ground-based data in this study is not quite clear to me. As far as I understand, they are not used in the average trend analysis, but they are also not used to quantitatively evaluate individual satellite records. It would be helpful if the authors could provide the rationale for their inclusion.

Lines 25-27: Is the fact that they are coherent over a wide range of altitudes and latitudes based on high vertical resolution profiles from satellites? Or on models? A reference would be useful here, since Harris et al. (2015) did not use ground-based profiles because of concerns about representativeness and the "coherency over latitude" would seem to negate that issue.

Lines 29-35 and Page 5, lines 1-3: These drifts, while not statistically significant, are of the same order of magnitude, if not larger than, the trends reported here. It is clear from the abstract that a more detailed analysis of these drifts is being undertaken by LOTUS, but a somewhat deeper discussion of their relevance to results presented here is needed, particularly given that the differences between Harris et al. and these results seems to stem in part from lower drift in the records used here.

Page 5

Lines 5-6: What is the rationale for using the 1998-2008 climatology for normalization?

Lines 14-16: It seems this point could be made more clearly by referring to sparser spatial and temporal sampling rather than "sparser sampling" meaning temporal and "geophysical differences" referring to the spatial sampling.

Figure 1: Presumably the grey line in Figure 1 refers the multi-model mean of the CCMVal2 models? This point should be clear in the caption and in the text in Section 2, and the authors should consider providing the full envelope of the models, as the range is fairly large.

Line 27: Is there something missing here between "solar cycle" and "Reisel"?

Section 3: It is clear from Figure 1 that there are data gaps in at least some of the ground-based records. How are these handled in the trend analysis?

Page 6

Lines 18-25: I found this explanation confusing. It is unclear to me from this description how the first 2 regression terms are used. This seems to imply that only the last 4 terms are used and then a linear trend for 2000-2016 is fitted to the remaining residuals – if so then why are the first 2 regression terms included at all?

Lines 20-23: On what years of data was the initial regression step performed?

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Lines 2-3: The authors might want to refer to the Tegtmeier et al. paper on the SPARC Data Initiative ozone climatologies here.

Figure 3: What uncertainty was used for the CCMVal 2 results? Is it based on the model range for all of the models or just on the ensemble mean? Please specify.

Page 8

Line 3: Why was SBUV only used above 40 hPa?

Figure 5: The caption needs several clarifications. It states that "uncertainty bars and yellow shading" give the +/- 2 x sigma values for all individual trends and seems to refer to the datasets used here (though it is unclear how both the uncertainty bars and yellow shading show the uncertainties for a single dataset), but then states that the yellow lines and shading show results from Harris et al. The WMO trend is apparently shown, but the color is not specified – is it the blue line? Finally, the clarification is again required for the model simulations – is this the ensemble mean? How are the uncertainties derived? I think perhaps things could be clarified if the sentence about uncertainty bars and shading were moved later in the paragraph.

Lines 20-21: Strictly speaking, a drift analysis requires comparison to independent datasets. It is unclear whether the authors are saying here that such an analysis has been performed and that the drift in the upper stratosphere has been determined to be 1-2% rather than the 6% used in Harris et al., or whether they are simply relying on the J-distribution analysis to argue for a small drift estimate. It is also unclear how this estimate relates to the estimates provide in Section 2, bottom of page 4, which describe drift estimates of 2-5% for the individual satellite records that make up the merged datasets.

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Lines 16-21: For completeness, a brief discussion of the attribution of trends should be provided here.

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