Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2017-391-RC1, 2017

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.

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Received and published: 31 May 2017

The paper provides an update and a valuable summary of recent studies of upper stratospheric ozone trends. This work is relevant in the context of the Montreal Protocol (1987) as positive ozone trends in the upper stratosphere (in extra tropics) are viewed as clear indication of decreasing anthropogenic ozone depletion caused by ozone depleting substances (ODS) which is also seen in numerical simulation. The paper is an extension of earlier work of last WMO/UNEP Ozone Assessment (2014). Quasi global ozone trends need to be determined from merged satellite series, but it turned out that the construction of merged satellite series from individual measurement series is a major challenge for the community both in terms of concept and available data. An additional challenge is the way how to combine different merged satellite series to obtain appropriate long-term trends series including determination of correct

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Discussion paper

uncertainties. Particularly useful is in my view the synopsis of recent activities including work of the past SPARC-activity SI2N (e.g. Harris et al., 2015). General comment: I recommend to discuss in some more detail the (conceptual) differences between Harris et al., 2015 and WMO, 2014 (2014), see below. Specific comments: Abstract: 1. p. 2, line 10/11: I agree that a "detailed attribution of the observed increases to declining ozone depleting substances and to stratospheric cooling" is required: From a formal point of view a suggest to mention this point in the conclusions as well. Introduction: 2. p. 2, Line 21/22: I don't believe, that the Montreal protocol was (only) signed because of the ozone hole, this is a too strong oversimplification for me: Indeed the ozone hole was very important to enhance public awareness but in the same period the results of the International Ozone Trend Panel Report were elaborated showing first time significant negative trends in northern mid latitudes which was certainly important for the signature (and gradual strengthening) of the Montreal Protocol too. Ozone profile data records: 3. I suggest to clarify whether additional data were used compared to WMO (2014) and for which data sets important revisions were made. 4. I think it would be useful to clarify whether (all) satellite merged series used in this paper were used in Harris et al. (2015) and vice versa: I think in this paper SAGE-GOMOS merged series are not used. Is there a particular reason not to use these data ? please explain 5. Second last para on page 4: I would have preferred to put the sentence "Table 2 summarizes the ground-based stations used in the present study" at the beginning of the para. 6. p. 4, line 29: I suggest to extend the paragraph staring on p. 4, line 29 about the comparison of Hubert et al., 2016: Which NDACC measurements were used ? 7. Figure 1, legend: I am not sure, whether Umkehr measurements belong to "NDACC ground-based stations" – Umkehr measurements at least started earlier than NDACC exists. 8. Figure 1: Is there an explanation why upper stratospheric ozone decrease in extra tropics in the first years of the 1980s seems considerably larger in available measurements than in numerical simulations ? 9. Figure 1: what's the reason for missing data in the black curve 1983-1985 in the upper panel (northern midatitude) ? are the Umkehr data missing ? 10. p. 5, line 7 ff: The reference Eyring et al., 2010 seems rather old. Are no more recent publications available ? The data after 2010 are predictions in Eyring et al., 2010 Ozone profile trends: 11. Fig. 2: How is the significance levels determined for numerical simulations ? Is this (directly) comparable with significant trends in measurements ? Please explain From individual data sets trends to the average trend: 12. p. 7., line 22 ff: The use of weighting with inverse squared uncertainty might be viewed as scientifically arbitrary. I believe, that weighting with inverse squared uncertainty of the individual data series tends to increase magnitude of trends of the ensemble. Please comment 13. I suggest to extend the first para of this section, I think this discussion is important for the community 14. Fig. 5: I am wondering whether it is justified to show the uncertainty of Harris et al., 2015 (yellow shading). The main result of this study seems to me that the uncertainty of Harris et al, 2015 no longer corresponds to the present knowledge which is shown in Table 4 namely because of longer series and improved data.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2017-391, 2017.