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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparison of three vertically resolved ozone data bases: climatology, trends and radiative forcings" *by* B. Hassler et al.

Anonymous Referee #2

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This manuscript compares a new ozone climatology from Bodeker et al. (2012) to two somewhat older ozone climatologies by Randel and Wu (2007) and Cionni et al. (2011), which have been hitherto used to evaluate past ozone trends and prescribe ozone fields in global transport and tropospheric chemistry-climate models (used in the IPCC AR5) without full representation of stratospheric chemistry, respectively. Given that changes in stratospheric ozone are known to strongly impact surface climate, representation of a realistic ozone field in these models is crucial to understanding past climate change and variability. However, the reality of the available ozone climatologies is not yet known. In order to address this issue, the study presented uses multiple diagnostics such as climatology and variability of both total column and vertically resolved ozone in order to evaluate differences in the three available ozone data sets, derives long-term ozone trends and assesses the impact of the differences among those trends

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on radiative forcing. It is important to keep in mind for the interpretation of the results that the three regression-based climatologies were constructed using different sets of basis functions in order to serve different needs of the community and hence cannot be expected to reproduce all aspects of variability.

The presented detailed comparison of the three available data sets and the guidance provided for their use constitutes a very valuable contribution to ozone research and the study hence fits well within the scope of the ACP journal. The manuscript is generally well written. However, I got the impression that some evaluations have not been done as thoroughly as they should have been done. I hence recommend minor revisions before publication addressing the comments as outlined below.

Major comments

(1) Evaluation 3.1 'Integrated ozone': This evaluation and discussion thereof need to be revisited. While the authors may be right with their conclusions, their evaluations are not accurate enough to convince the reader. You cannot just add some rather arbitrary tropospheric ozone column value that is strictly valid only for the period between 2004-2010, since there are tropospheric trends between 1979 and 2010 that may make significant differences to your main conclusion (claim) that RW07 total ozone column is biased high. A first simple remedy is to compare total columns for all data sets in a consistent way, namely integrated from the monthly mean tropopause height up to 1 hPa, instead of simply integrating between 250 and 1 hPa. Secondly, as shown in your figure 3, BDBP shows too high values in the early period and too low values in the later period, so that the average of these values may just by pure coincidence agree better with TOMS. Using shorter time periods of averaging (e.g. 1979-1985, or 2000-2005) would avoid this issue and a 2000-2005 based climatology may be better comparable to a combined TOMS/Ziemke et al. (2011) climatology.

(2) Evaluation 3.2 'Vertically resolved ozone': The first paragraph should be based on a figure the reader has access to. Second paragraph onwards: your discussion on

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Figure 2 seems to talk about 4 data sets (the three data sets plus FK98), but I can only see three! Please revise figures or explain on which basis you discuss, but it cannot remain as it is.

(3) The study emphasizes most strongly the differences in the polar regions, where we know that RW07 has difficulties due to the limited data coverage of SAGE. I like to see similar evaluations as in Figs. 2 or 4/5/6 also for midlatitude regions to learn more about the validity of the different data sets in these regions.

(4) I suggest moving presentation and discussion of Figs. 9 and 10 into the Result section. The problem I have with showing them in the Conclusion section is that they are using the Hassler et al. (2008) gridded data climatologies for comparisons, which are also main input into the BDBP data set and hence can be expected to agree better with BDBP than with the other data sets. It would also allow more space for a more balanced discussion of the strength and weaknesses of the different data sets in the conclusion section, e.g., that the BDBP shows too strong Arctic trends and some artifacts in the SH upper stratosphere. Also, you may want to explain again why RW07 has problems in the polar regions, and that the SPARC data sets basically only provides information on the chlorine-related part of ozone changes.

(5) Evaluation 'Annual mean trends': P26581 L 11-13 you say that SAGE I could be the problem for the large trends seen in BDBP. However, RW07 includes these data as well and do not get a similar result? Also, given the rather too high/low anomalies at the beginning/end of the BDBP data set, I find it a strange argument on P26581 L19 to say that the trends derived by Randel and Thompson (2011) and Forster et al. (2007) cannot really be used to confirm the results, i.e. a better agreement with the trends of RW07 and SPARC and disagreement with BDBP due to the different time periods over which the trends are calculated. You have most of the data needed, so could adopt the periods over which the trends are calculated as used in these studies for comparison to confirm your argument.

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Minor comments

Abstract, L22: I do not agree that the uncertainty in our knowledge of ozone trends is large, since there are multiple other ways used to derive those trends (see latest ozone assessment WMO, 2011) than just using the here discussed climatologies. The problem to point out lies rather in how the different ozone climatologies are constructed, i.e., which particular basis functions are used for the respective regression models and how these influence what trends the climatologies pick up.

P26563 L15-18: Please rephrase, it seems an awkward sentence starting with 'Although. . .' Also, see previous comment, you say yourself that one should not use the climatologies to derive trends!?

P26574 L24: Why do you say BDBP Tier 1.4? Is this a different data set all of a sudden then used before?

Technical comments

- Introduction P26563 L2: Did you want to say 'in a changing climate'?

- P26564 L1: Please rephrase to 'that (i) show high vertical resolution, (ii) cover . . .'

- P26567 L16: Suggest to delete 'a manuscript by'

- P26568 L14: Use the abbreviation you defined earlier for this publication 'FK98'.

- P26572 L3: Please refrain from using 'significantly' unless you mean statistical significance.

- P26580 L17: Suggest using 'higher into the middle stratosphere' instead of 'atmosphere'

- P26582 L26: What are 'purely statistical uncertainties'?

- Figure 7: It is confusing to use different contour levels (and colors?) in these plots. I also find it somewhat confusing that the y-axis for the plots on the right is on the right

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instead of on the left. You don't gain space or comparability by doing so.

- Table 2: Please add latitude band of the different regions considered.

- Table 1: Please add highest pressure level also in km for the SPARC data set for consistency.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 26561, 2012.