Atmos. Chem. Phys. Discuss., 14, C9388–C9392, 2014
www.atmos-chem-phys-discuss.net/14/C9388/2014/
© Author(s) 2014. This work is distributed under
the Creative Commons Attribute 3.0 License.

Atmospheric Chemistry and Physics Discussions

Open Access

ACPD

14, C9388–C9392, 2014

Interactive Comment

Interactive comment on "Intercomparison of vertically resolved merged satellite ozone data sets: interannual variability and long-term trends" *by* F. Tummon et al.

T. von Clarmann (Referee)

thomas.clarmann@kit.edu

Received and published: 21 November 2014

In the paper by Tummon et al. vertically resolved merged satellite ozone data sets are compared in order to provide a characterization of these data sets. This leads directly to the main problem of the paper: With this scope the paper would perfectly fit in the sister journal Atmospheric Measurement Techniques but for publication in ACP it lacks discussion of the atmospheric science related to the data sets presented and intercompared.

p25693 l8: Here it is not distinguished between vertical resolution and vertical grid,

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

although these are not always the same. As a consequence of this, the subsequent statements remain vague.

p25693 l17ff: For a technical comparison paper focused on data characterization it may be justified to restrict the comparison to data sets which have a common technical characteristic, e.g. the same temporal coverage. For a scientific paper, however, the results have to be discussed in the context of all relevant existing work. E.g. for linear trends the shorter temporal coverage of a data set is not a good excuse to ignore it, because it is inherent to the assumption of a linear trend that the trend does not vary with time and thus trends determined from shorter data sets should be comparable to those inferred from longer data sets. The same applies to annual variability: The assumption that the amplitudes of the annual variation and its overtones is assumed time-independent implies that comparison to annual cycles of shorter data sets should still be meaningful.

p25694 l7: I do not understand the dichotomy ("either or"). I think that hybrid approaches are possible and thus suggest to delete "either".

p25695 l22: The resolution increases to 15 km. It is the resolving power which decreases. Since the correct technical wording is admittedly counter-intuitive I suggest to use an unambiguous verb here, e.g. "degrades". The same applies to line 26.

p25696 l2: Here broad averaging kernels are mentioned. Averaging kernels do not only carry information on the vertical resolution of a profile but also on the content of prior information in the data set.I consider this kind of information important: Depending on the choice of the prior information of the retrieval (constant or time-dependent) annual cycles, trends, etc. can be damped. Thus knowledge of these issues might be crucial to understand related differences discussed in the later sections.

p25701 l4: With the multiple occurrence of SAGE the MDM is certainly not an "unbiased estimate".

ACPD

14, C9388–C9392, 2014

p25702 l13: Is it adequate to talk of Fourier terms in the context of all coefficients A-H? Shouldn't the attribute "Fourier" be limited to the harmonic components? The treatment of seasonality is not quite clear. I might have missed the point here but a clearer and more detailed description of the model would be helpful.

Eq 1: I do not understand why R(t) appears in the equation which is supposed the MODELED data. Is the residual deterministic such that it can be predicted by the model? I think the residual is the difference between the measured and the modeled data and thus should NOT appear in the equation defining the model.

p25703 l17: I do not understand this sentence, particularly what does "not coefficient A" mean?

p25704 l9: The statement explaining the similarities of the annual cycles is quite vague ("This is perhaps due to ..."). The issue of having merged data sets relying partly on the same parent data sets needs a more thorough discussion. A more quantitative estimate on the reduction of differences due to this issue is needed. The statement as made in the current version, viz. that the occurrence of the same parent data sets in multiple merged data sets reduces the differences appears quite trivial to me.

p25707 l14 ff: I disagree with the use of the standard deviations: The standard deviation is a measure of the expected deviation of, e.g., one particular February from the mean of all Februaries, i.e. it is a measure of the variability of the February-value. For comparison of multiple averages the adequate diagnostic is the standard error of the mean (for uncorrelated data it is the standard deviation divided by the square root of the sample size). To judge how well the mean annual cycles agree, the latter would be the applicable quantity (with the caveat that the multiple use of parent data sets in some merged data sets adds further complication). I do not understand what can be concluded from the fact that the standard deviations overlap.

Summary Section 3.1: This section is restricted to the annual cycle as a technical diagnostic of the merged data sets. Is there nothing to say about the annual cycle in terms

ACPD
14, C9388–C9392, 2014

CC BY

of atmospheric sciences? Are the annual cycles as represented by the merged data sets in agreement with our expectations and with other analyses found in the literature? In other words, can the annual cycles be explained with our current knowledge on atmospheric processes? Publication in ACP requires discussion of such issues beyond the pure technical description.

Summary Sections 3.2 and 3.3: Here the same applies as for Section 3.1. The discussion is limited to the technical comparison. For an ACP paper the anomalies should be identified and as far as possible attributed to certain events (e.g. Pinatubo). A statement is needed which anomalies can be explained with current knowledge and if/where any unexplained issues are detected. This does not mean that the authors have to carry out quantitative analyses or model calculations themselves, but at least the data sets under investigation (and the events recognized in them) shall be put into the context of the existing literature. Although dealing mostly with column ozone, Shepherd et al (Nature Geoscience 443-449, 2014) might be useful in this context.

p25711 l 14: How is significance defined in this context, and how is significance evaluated, facing the multiple occurrence of certain parent data sets in the merged data sets which are then averaged?

Summary Section 4: In the last paragraph of this section, the authors put their data in the context of independent work. This is certainly a step into the right direction but this discussion needs to be extended to make the manuscript suitable for ACP. More detailed suggestions follow below:

p25713: I disagree with the explanation that the discrepancy between the trends found by Gebhardt et al. and those assessed here can be attributed to their shorter data set. As said above, the assumption of a linear trend implies that consistence with trends inferred from subsets of the period should be expected (when represented in Delta vmr/Delta t instead of %/Delta t). Further, Eckert et al. (ACP 14, 2571-2589, 2014) find trends in a similar short period which fit much better to those trends discussed in the

ACPD

14, C9388–C9392, 2014

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

CC BY

paper. This suggests that the discrepancy w.r.t. Gebhardt et al. is not to be explained by the shorter time period but that it is a particular characteristic of this data set. The work by Eckert et al. should be included here, not only because it belongs into this context per se, but also because it helps to solve the problem with the comparison to the Gebhardt trends.

The main results of the additional discussions to be included in Sections 3 and 4 should be summarized in the Conclusions and the Abstract.

The figures are too small (particularly, in Figure 1 the structures can hardly be seen, in Figure 4 and 8 the lines are hardly discernible. All labels are very small.

My critical review results from the fact that the current content of the paper is much more suitable for AMT than ACP. After consideration of the issues discussed above, the paper to my judgement still has a fair chance to meet the criteria for publication also in ACP. I recommend publication in ACP after major revision. Redirection to AMT (which is also linked to the special issue on SI2N) would be the more straight forward option but I guess the decision in favour of ACP has already been made.

Thomas von Clarmann

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 25687, 2014.