Atmos. Chem. Phys. Discuss., 13, C8977–C8984, 2013
www.atmos-chem-phys-discuss.net/13/C8977/2013/


Atmospheric Chemistry and Physics
Discussions

# *Interactive comment on* "Drift-corrected trends and periodic variations in MIPAS IMK/IAA ozone measurements" *by* E. Eckert et al.

**E. Eckert et al.**

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Received and published: 10 November 2013

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Drifts, Trends and Periodic Variations in MIPAS Ozone E. Eckert et al.

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# Authors reply:
# Drift-corrected Trends and Periodic Variations in MIPAS IMK/IAA Ozone Measurements

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10 November 2013

The authors would like to thank the reviewers for their helpful comments. In the following we give a point-by-point reply. The original reviews are included in bolds face; our reply is typeset in normal face.



## 1 Review #1

**This study is focused on obtaining accurate ozone trends from MIPAS measurements obtained from 2002 to 2012. The methods used and the findings of the study are described by the authors. Estimates of a possible drift in the MIPAS measurements were obtained via comparisons with several coincident ozone datasets, including from ACE-FTS, Aura MLS, Odin OSIRIS, and ground-based lidar. The authors then go on to correct for that analysed drift, prior to obtaining their final ozone trends. However, I am unconvinced from the analyses herein that there really is any significant drift in the measurements of MIPAS ozone.**

**My skepticism is as follows. In Section 2 the authors need to say right away what ozone quantities are being compared to obtain the MIPAS drift and are then analysed for the ozone trends. For example, the AURA MLS data and the lidar data were converted to ozone mixing ratio (MR) versus altitude. Is that the primary ozone quantity from MIPAS?**

Yes, volume mixing ratio is the primary ozone quantity from MIPAS. This will be clearly stated in the revised version.

**Were the ACE and OSIRIS ozone profiles also converted to MR versus altitude before comparison with MIPAS?**

ACE ozone profiles were provided in terms of volume mixing ratio. According to the OSIRIS user guide the ozone profiles are derived from the inversion algorithm in terms of number density. They are then recalculated to volume mixing ratio using ECMWF data. Both products are provided for the user, ozone in number density as well as in volume mixing ratio.

**Specifically, the authors say that they used temperature profile data from ECMWF for their conversions. Yet, those operational temperature profiles are derived from nadir radiances that have a much lower vertical resolution than the retrieved**

**ozone of the middle and upper stratosphere. As a result, periodic variations in ECMWF temperatures are damped, most likely, compared with the corresponding atmospheric oscillations that are affecting the observed ozone from the several satellite instruments (p. 17860, line 23). Trends in the ECMWF temperatures are likely incorrect, as well, or they carry their own uncertainties.**

We refer to our author reply of 22 July where we have replied to this comment in detail. For the revised manuscript we will go a step further and present the entire analysis of the drift between MIPAS and Aura MLS in pressure coordinates under consideration of the MIPAS averaging kernels. For this conversion temperatures and pressures as retrieved by MIPAS have been used to become independent from ECMWF.

**One clue that temperature may be a problem is the finding that the analysed drift increases with altitude (p. 17865, lines 12-15).**

In our recent author reply we have provided evidence that the increase of the MIPAS ozone drift with altitude can well be explained by the drift resulting from changing MIPAS detector nonlinearity. This issue will be discussed in more detail in the revised version.

**Thus, temperature trend errors may be significant and ought to be mentioned. At the very least, they represent a separate source of uncertainty that ought to be addressed and explained to the reader before I can recommend publication of your manuscript.**

This problem will be by-passed by performing the analysis on a pressure grid.

## 2 Review #2

**The authors of this paper perform a time-series analysis of monthly, zonal means of MIPAS IMK/IAA ozone data and look at the resulting components of the regres-**



**sion. The methodology is straightforward and widely used. The authors then go on to determine the magnitude, if any, of drift of the MIPAS data relative to other instruments (e.g. MLS). This is accomplished via the same regression to the difference of coincident pairs of events between MIPAS and other instruments. I, however, have a few questions/concerns regarding this analysis technique.**

**The authors compute component terms from a regression to data, while drifts in trends come from a regression to differences of coincident pairs. Any time monthly, zonal means are taken, a potential sampling bias can be introduced, whereby the data is not evenly spaced throughout the month and is thus not necessarily representative of the middle of that month.**

Drifts have been analysed on the basis of actual collocations, not on the basis of bin averages (i.e. bin averages have been calculated AFTER calculation of the collocated differences), so sampling is no particular issue here, because latitudinal sampling differences cancel out. For the trend analysis, we take advantage of the fact that since 2005 MIPAS has a fixed latitudinal sampling pattern which excludes latitudinal sampling artefacts. Trends calculated for 2005-2012 do not noticeably differ from trends calculated for 2002-2012, indicating there is no latitudinal sampling problem with the early results, either.

**Table 2 provides the coincidence criteria, as well as the total number of coincidences, but it does not provide a breakdown of coincidences by latitudinal band. While it is likely that a sampling bias does not exist given the large number of coincident pairs between MIPAS and MLS, it would be good to ensure one is not present given the difference in the retrieval of trends and drifts in trends.**

Since the drift analysis is based on coincident pairs and not on bin averages, it is not quite clear how any relative sampling drift should affect the relative ozone drift. Further, the number of pairs determines the error bar of the bin-average of the differences. Data points with large error bars have less weight in the drift analysis.

**The authors state that the reason for regressing to the differences between instruments "is to account for possible dependence of the differences on the atmospheric state" (pg. 17860, line 27). However, it appears that the authors assume that all of the differences can be accounted for by the atmospheric state. If the differences cannot be entirely explained by the atmospheric state, then any lacking ability of the regression model to fit to the data, particularly at the edges of the time periods, can bias the linear term. This would be readily apparent as large differences in separate fits (e.g. at different latitude bands) and could explain the banding structure seen in Figures 3 and 4. This can be better determined by varying the time period of the regression (e.g. less one year at one or both ends) to see how the phase of the oscillation of the residuals at the edges affects the retrieved trend term.**

First, we have tested different time intervals and have not found any substantial difference (compare, e.g. Fig 4 to A2). Second, only the MLS-MIPAS drift is used for quantitative correction, and there we do not see the pronounced band structure visible in the MIPAS-OSIRIS drifts.

**This can also apply to the retrieval of the trend term itself (i.e. from just MIPAS data), and could perhaps contribute to the strong correlations seen between Figures 8 and 10.**

First, also for the trends, the use of data periods of different length did not change a lot. Second, QBO coefficients have been considered in the fit and thus can hardly map onto the trend. Third, our consideration of autocorrelation of errors should account for related uncertainties.

**It would be interesting to compare the drifts in trends computed via the method outlined in this paper, and by outright regressing to the other instruments and deriving a trend term and comparing.**

In the context of drift analysis, we consider the direct comparison of trends from differ-



ent instruments inferior to the comparison of coincidences because the direct comparisons are prone to sampling artefacts (e.g. via variation of measurements geolocation etc.). In contrast, these effects cancel out in first order when the drift analysis is based on coincident pairs of measurements. Nevertheless, comparison of ozone trends is interesting in its own right and is a part of a parallel study of a separate project which is currently under way.