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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The diurnal variation in stratospheric ozone from the MACC reanalysis, the ERA-Interim reanalysis, WACCM and Earth observation data: characteristics and intercomparison" *by* A. Schanz et al.

A. Schanz et al.

ansgar.schanz@iap.unibe.ch

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Dear Referees,

thank you for the review of the paper and the hints on how to improve the publication. In the following, we answer on your comments point-by-point:

Referee #3

1)

My major concern is the definition of 'diurnal variations' in Eq. (2). The problem is that D_{O3}s are calculated for each day and they are averaged over each month (Is my understanding true?). As for this definition, D_{O3} does have some value even in the case where there is no diurnal cycle but only seasonal variations (please consider a very simple case where O_3 amount increases linearly with time over a month). In this sense, the results and discussion throughout Sect. 3.2-3.4 are very misleading to the readers. I would suspect that large signals in polar region in winter may be the artificial ones due to the inappropriate definition. It would be highly recommended that the diurnal variations be extracted in the method such as the composite analysis over a month or so (as used for Fig. 1). The D_{O3} should be calculated afterward by using the composite results. All figures (except Figs. 1 and 7) and corresponding description need to be throughly revised based in the new results.

We agree that D_{O3} should be calculated by using the local time composite of a month. When D_{O3} is deduced day-by-day and averaged over a month, results can be misleading. In order to verify your concerns, we checked our analysis of the three model systems. We found a critical problem in the analysis routines of the ERA-I and MACC reanalysis. As you suspected, for these two data sets the relative fractions of D_{O3} has been deduced on a daily basis and averaged over a month which was not intended.

After reprocessing of the data and applying the composite method correctly, the magnitudes of the diurnal variation in winter are much weaker. Indeed, the very large signals are caused by the incorrect routines. However, the basic findings such as a significant diurnal variation at the polar circle in winter (after reprocessing: 8%) are still maintained and are an interesting new feature. In addition, the diurnal variation in the summer hemisphere is almost not affected by this incorrect analysis.

The addressed case of a linear increase is still a problem in the local time composite method. However, the biases from such trends are small. We did tests where the diurnal variation was isolated from synoptic or sub-seasonal variability by subtracting a 24-hour-running mean from the ozone time series. The resulting residuum diurnal variation is then binned to a local time composite of a month. Finally, D_{O3} is deduced. Subtracting the 24-hours running mean is similar to a simple high-pass filter and gives the diurnal variation without disturbances. We plan to apply this method to all the figures. Fig **??** shows preliminary results of these methods.

We briefly recap our new analysis of the diurnal variation:

- The global ozone time series $O_3(x_k, t)$ at position x_k is detrended to a residuum variation $O_3^{res}(x_k, t)$ by subtracting a 24-hours running mean $\overline{O}_3^{24h}(x_k, t)$ for each point in time. The residuum variation at position x_k is thus given by

$$O_3^{res}(x_k, t) = O_3(x_k, t) - \overline{O}_3^{24h}(x_k, t)\ . \qquad (1)$$

  In our case of a 6-hourly temporal resolution, the 24-hours interval of the running mean comprises five values I =: [t-12, t-6, t, t+6, t+12] to average over.

- The resulting residuum time series is split into periods of one months. Then the data is sorted and binned by time of day. For each time of a day the values are averaged. Finally these averaged values are put together to composite referred to as $O_3^{comp}(x_k, t)$. This composite represents a mean diurnal variation of the month under consideration.

- From this composite, the peak-to-valley difference D_{O3} is deduced by

$$D_{O3}(x_k) = max(O_3^{comp}(x_k, t)) - min(O_3^{comp}(x_k, t)) \qquad (2)$$

In a revised version, we present new figures and descriptions of the findings according to the described method. Thus, all percentages and drawn conclusions are going to be critically reassessed. Further, the definition of D_{O3} (32680, 6-13) needs to be updated. We are much obliged to you for your comment on the definition of the diurnal variation which lead to a correction and improvement of our analysis method.

2)

Figures 9-10 (Fig. 6 also?) are based solely on a particular day. In this case, not only diurnal variations but also all kinds of variations including synoptic eddies and sub-seasonal variations contribute to the results. Thus, the conclusion that advection is important for the diurnal cycle, is not convincing. Again, all analyses should be based on monthly average or so, to extract the diurnal cycle only (that is to remove synoptic or sub-seasonal variability).

Based on our reply on 1) the Figs. 9-10 were remade by monthly means without the disturbances of synoptic eddies and sub-seasonal variation. We think the known relative D_{O3} is a better choice of representation as the 18.00-6.00 figures. This will also help to improve the comprehensibility of the manuscript.

3)

The authors often describe like '... is due to photochemistry' and '...is due to advection' throughout the manuscript without any discussion (p32680 Lines 20, 27 and many others). I do not understand based on which results (or references) there explanation are made. Also, which kind of advection do the authors assume in this paper? (e.g. vertical? horizontal?).

The photochemical effects and the underlying reactions have been described by Schanz et al. (2014) including the strong accumulation effect during daytime at the polar circle in summer. The present manuscript gives a brief summary of the main findings of Schanz et al. (2014) in the introduction (32670, 12-23). We will carefully revise the manuscript in order to avoid any misleading comments and give a clear reference when needed. The advection leading to a diurnal variation can be both horizontal and vertical depending on composition gradients and wind strengths.

4)

[p32679- Line 23] I understand that Sakazaki et al.(2013) tried to reduce the effects from sampling issues by removing the seasonal ozone changes. For the present SMILES data, it seems that the authors did not do any analysis for reducing such effects (is my understanding true?). Generally, for satellite measurements, the sampling issue is critical for the detection of diurnal variations and several techniques have been used to reduce it for MLS/UARS and SABER measurements as well (e.g. Huang et al.(1997) for MLS; Huang et al.(2010) for SABER). Please address and take into account these backgrounds.

We use the SMILES climatology product of diurnal ozone variation as distributed on the NICT website. Kreyling et al. (2013) describe the minimization of the inhomogeneous data sampling by carefully choosing the size of averaging bins. To account different gradients of ozone, the climatologies have been separated into different latitude bands.

In Fig. 1, we present SMILES data for March-April as given by the SMILES climatology product. The climatologies are two-monthly due to the need of sufficient data for each local solar time bin. In contrast, the analysis of Sakazaki et al. (2013) uses the full

observation period of SMILES (about 7 months) and is therefore more affected by seasonal ozone changes. However, we agree that this error source exists even for a two-monthly climatology and should be mentioned in the paragraph where Fig. 1 is discussed.

5)

Please reconsider whether Sect. 3.3 is necessary for the present purpose. If diurnal variations are extracted based on monthly average or so (please see Comments 1-2), the effect from the fine structures (e.g. Fig. 6) might disappear because tides are global-scale phenomena.

We reevaluate Sect. 3.3 after the full analysis with the new results according to 1). The section gives additional information for modelers on potential problems with horizontal resolution. We agree that this is not critical for the present purpose and therefore can be withdrawn.

6)

[Figure 10] I would think potential vorticity analysis should be done on potential temperature coordinate, because air parcel moves along on the isentropic surface. Also, it is unclear how much of the abserved diurnal cycle can be explained by the PV 'advection'.

We did an analysis of potential vorticity at 1000K (about 5hPa) which showed marginal differences. We decided to keep the pressure coordinate in order to make comparisons to observations more convenient. However, that was before we identified the problems with the routine (see point 1). With the new results of diurnal variation a reevaluation is indispensable. An isentropic surface may then be a better choice.

7)

[Related to Comment 3] Throughout the manuscript, please describe clearly/separately what has been already known (with appropriate references) and what is new. These two are mixed in the manuscript so that I find it very difficult to follow the logics.

Thank you for calling our attention to this issue. The manuscript will be carefully revised taking into account the new and already known features of the diurnal variations.

Referee #2

1)

One of the main conclusions drawn by the authors cannot actually be derived from the results and analysis shown in this study. It is concluded that the CTM used to produce the MACC reanalysis is superior to the linear scheme used by ERA-Interim to simulate stratospheric ozone variability. However, there are several other important differences between MACC and ERA-Interim reanalyses that affect ozone, and it is not possible to disentangle the effect that each one of these differences has on the diurnal cycle of stratospheric ozone from the data and analyses shown here. Among these important differences are: different IFS model versions were used for each reanalysis, different observations were assimilated in each reanalysis and also MACC included an ozone

bias correction that ERA-Interim did not.

We agree, that the disentanglement of interacting causes (e.g. different CTM's) is not possible in detail. Aside the chemical schemes there are more differences of the model systems so that some conclusions should be revised and weakened.

We cannot state that the CTM of MACC is superior to the linear scheme in ERA-Interim in general. With respect to the diurnal variation in ozone we find differences to MACC and WACCM. Both have a very strong chemical representation and agree with the results of Schanz et al. (2014). Such differences of the model systems are interesting for many scientists working with reanalysis data. We will consider your objections and carefully revise the text. Please also consider that the study characterizes the diurnal variation depending on season and latitude and is not a model system intercomparison only.

2)

The authors also claim that because the ozone linear scheme used in ERA-Interim does not contain information on diurnal variability, then ERA-Interim ozone cannot show any diurnal variability. This is not entirely true, as the observations assimilated do contain some information on the diurnal cycle.

We agree that the data of ERA-Interim does contain some information of the diurnal cycle via assimilation. On the other hand the ERA-Interim does not show such strong diurnal variations in ozone at high latitudes in summer which is due to the photochemical response to insolation over daytime (Schanz et al., 2014). This effect is confirmed by comparison to the OZORAM measurements (cf Fig. 7).

However, we looked into the data to confirm this. In the period from 1. April to 31 August 2004 ozone profiles have not been assimilated into the MACC reanalysis (no MLS or MIPAS, Inness et al., 2013). We applied our diagnostics to that particular period and obtained very similar diurnal variations as for 2012 in the summer and winter hemisphere. The assimilated diurnal variation in ozone seems rather small which is reasonable when considering that MLS and MIPAS are aboard sun-synchronous satellites.

Both, Comment 1 and 2 will lead to a careful revision of all statements regarding this issue. Further, we will pay attention that our study cannot be misunderstood as a degradation of the valuable ERA-Interim reanalysis and its ozone scheme. This is truly not our intention.

3)

Almost all discussions in the manuscript are based on comparison for only a few months and locations, to derive conclusions about reanalysis datasets more global analyses should be shown.

The main focus is on diurnal variation at high latitudes with regard to potential biases in trends from satellite observations. A description of the diurnal variations at different seasons and latitudes is of high interest for these sampled satellite data.

We do not present various other locations since the diurnal variation in ozone in the tropics and midlatitudes has been studied by ground-based radiometers (e.g. Parrish et al., 2014; Studer et al., 2014). We think it is important to compare the model system to non-space borne instruments which observe and resolve the diurnal variation in ozone. At high latitude we present the OZORAM observations which have a good diurnal sampling.

4)

Overall, discussions are not put within the context of existing knowledge of ozone chemistry and dynamics, which makes it difficult to see what novelty the study actually offers.

In paragraph (p. 32670, l. 9 ff) we describe the context of existing knowledge about the diurnal variation. The last paragraph (p. 32671, l. 26 ff) describes the novelties such as the strong diurnal variation at the polar vortex in winter. In addition, we show observations of the OZORAM radiometer at high latitude. However, we agree that the manuscript needs to be improved in terms of describing the context of our results and the novelty of findings which was also criticized by Referee 3. We will truly work on this in the revised manuscript.

We would also like to address the examples:

I

If differences in the representation of diurnal variability were mainly due to the chemistry scheme then WACCM results and MACC reanalysis would be closer, Fig. 5 shows they clearly differ, and that differences between WACCM and MACC are larger than between MACC and ERA-Interim.

The WACCM model and the MACC reanalysis are close where the diurnal variation is due to a photochemical response in the stratosphere (Fig. 5). This does not apply for all seasons or latitudes. WACCM and the MACC reanalysis clearly differ in the polar winter where the diurnal variation is not due to photochemistry. Here, the ERA-Interim and MACC reanalyses agree well. This is because WACCM does not assimilate dynamics nor is it nudged.

It might be not applicable to compare these model systems as 'close' or not. It is better to put the data in the context of the underlying physics of the diurnal variation which change with season and latitude. To respect these seasonal features, Figs. 2-5 present different months and the annual evolution of the diurnal variation for all model systems.

II

[Figure 1] Why only three points are shown for ERA-Interim while four points are shown for MACC? Why do not these points correspond to the analysis times (x-axis)? I am surprised at the complete lack of variability shown by ERA-Interim at Mauna Loa. It might be a feature only for this location/period because a direct inspection of the ERA-Interim dataset show some variability over the tropics. So, no general conclusion can actually be drawn on this from the figure shown.

We apologize that the first marker for the ERA-Interim is covered by the marker of the MACC reanalysis in Fig. 1. By changing the marker style this will be improved. Further, the x-axis of Fig. 1 is given in local time, Hawaii. The points cannot match since the reanalyses data are given in universal time which do not correspond to 6:00, 12:00, 18:00 and 24:00 local time, Hawaii.

We considered the objection that ERA-Interim cannot show a complete lack of diurnal variation at Hawaii. Indeed, there is a mistake in the plot. As you suspected there is some diurnal variation of approximately 1% in the ERA-Interim reanalyis (comparable

to what can be seen in Fig. 1a for Bern). Thanks for this hint which will certainly improve the figure and the related paragraph.

III

[Figure 9] Panel a) and c) should be swapped, Fig. 6 would also confirm this. MACC reanalysis and ERA-Interim cannot be so different, and WACCM cannot agree so well with MACC in the Arctic and so poorly in the Antarctic. All the corresponding discussion in the main text (Sect. 3.4) would then be wrong.

Please consider, Fig. 6 (antarctic winter) and Fig. 9a and c (arctic summer) show totally different situations. The WACCM model and the MACC reanalysis can agree well when the diurnal variation is caused by a photochemical response to insolation during day time (cf. Fig 7 or EXP I). This situation is shown in the upper panels where the arctic region is shown (polar day, summer). It is not a matter of Arctic or Antarctic, it is a matter of what is the underlying effect of diurnal variation: photochemical response or dynamics.

We infer, the figures should be improved by better indication of the polar day and night areas so that it is easier to understand what is the main cause of the diurnal variation in the respective hemisphere. The presentation quality needs to be improved.

IV

While the definition used for the diurnal variation could be valid for comparing different systems/datasets, as it has been raised by Referee 3, using monthly averaged values will itself modify the diurnal cycle compared to observations and therefore, extreme *caution needs to be taken when drawing any conclusions about values and strength of the diurnal cycle (as it is done in Sect. 3.2 ﬁ).*

We kindly refer to the remarks on Referee #1 where we addressed the deduction of the diurnal variation.

V

Another example is the last sentence before Sect. 3.2 about current understanding of stratospheric ozone photochemistry. Even if this might be partly true, this is a very strong statement that cannot be supported by a reference that is more than 20 years old. In addition, it is the way ozone is represented in some models what is not sufficient, as well as issues in the sampling frequency of observation datasets. If the authors intent to support a statement like this, they should look into studies from recent multimodel intercomparisons, for multiple locations and periods of time.

We agree on your objection that the statement needs more than a 20 years old reference. In case this statement is maintained in a revised version we take into account your suggestions. Thanks for calling our attention to this problem.

We would like to optimize our manuscript in terms of the definition of the diurnal variations, the presented results, conclusions and the presentation quality. Therefore, we would like to send a revised manuscript according to your suggestions and remarks which is more concise and with monthly mean data only.

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Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C12656/2015/acpd-14-C12656-2015-supplement.zip

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 32667, 2014.