

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.

Anonymous Referee #3

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This paper compares the diurnal ozone variations in CTMs (SD-WACCM and MACC) and ERA-Interim reanalysis. The topic is interesting, however I would have several critical concerns particularly for the analysis method. Therefore, at this moment, I could not evaluate specific discussions in the manuscript. I would regret to say that the results and discussion in the present form may not be suitable for the publication in ACP. Please see specific major comments below:

1. My major concern is the definition of “diurnal variations” in Equation (2). The problem

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is that DO₃s are calculated for each day and they are averaged over each month (Is my understanding true?). As for this definition, DO₃ 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 O₃ amount increases linearly with time over a month). In this sense, the results and discussion throughout Sections 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 Figure 1). The DO₃ should be calculated afterward by using the composite results. All figures (except Figs. 1 and 7) and corresponding description need to be thoroughly revised based on the new results.

2. [Related to comment 1] Figures 9-10 (Figure 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).

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 explanations are made. Also, which kind of advection do the authors assume in this paper? (e.g., vertical? horizontal?).

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.,

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1997 for MLS; Huang et al. 2010 for SABER). Please address and take into account these backgrounds.

Refs: Huang et al. (1997), Ozone diurnal variations observed by UARS and their model simulation, JGR, 102(D11), 12971-12985

Huang et al. (2010), Ozone diurnal variations in the stratosphere and lower mesosphere, based on measurements from SABER on TIMED, JGR, 115, D24308, doi:10.1029/2010JD014484.

5. Please reconsider whether Section 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 effects from the fine structures (e.g., Figure 6) might disappear because tides are global-scale phenomena.

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 observed diurnal cycle can be explained by the PV "advection".

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.

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

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