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# *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 #2**

Received and published: 19 February 2015

The manuscript compares the representation of the diurnal cycle of stratospheric ozone in three different systems, the WACCM free running model and two reanalysis datasets, the MACC reanalysis and ERA-Interim.

Interest in the diurnal cycle of ozone in the stratosphere is rapidly increasing with the appearance of models with higher vertical range, wider modelling applications and new observation datasets, thus system intercomparisons are very welcome in this field. Nevertheless, the conclusions drawn in this study in its present form are not robust

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due to inconsistencies in results and discussions; therefore, unfortunately I cannot recommend the publication of the current manuscript.

Please find here below my major comments.

Major comments:

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 reanalyses 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.

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.

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

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.

Some examples of inconsistencies in the results and discussions shown:

-If differences in the representation of diurnal variability were mainly due to the chem-

istry scheme then WACCM results and MACC reanalysis would be closer, Figure 5 shows they clearly differ, and that differences between WACCM and MACC are larger than between MACC and ERA-Interim.

-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 does show some variability over the tropics. So, no general conclusion can actually be drawn on this from the Figure shown.

-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.

-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 conclusion about values and strength of the diurnal cycle (as it is done in Sect 3.2 f.i.).

-Another example is the last sentence before Sect. 3.2 about our 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 intend to support a statement like this, they should look into studies from recent multimodel intercomparisons, for multiple locations and periods of time.

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