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# *Interactive comment on* "Model-based Climatology of Diurnal Variability in Stratospheric Ozone as a Data Analysis Tool" *by* Stacey M. Frith et al.

**Anonymous Referee #3**

Received and published: 7 November 2019

Referee Report

Model-based Climatology of Diurnal Variability in Stratospheric Ozone as a Data Analysis Tool, by Frith et al.

This manuscript describes the (GEOS-GMI) global model climatology (12 monthly sets) regarding ozone diurnal change as a function of local time for various latitude bins and pressure values. The chosen time step (resolution) is a half-hour. Model values are compared to various data sets, mostly from satellite-based ozone measurements with different spatio-temporal samplings. Most of the comparisons seem to validate the model results, even if there are a few discrepancies that are not completely explained. This model climatology is publicly accessible (or will be), and this offers a useful tool for other investigators, to try to improve certain upper stratospheric and mesospheric

ozone comparisons.

General Comments

The paper is generally well-written, clear enough, and fairly thorough in the set of comparisons that are provided for validation. It does not purport to solve in detail every intercomparison's discrepancies. However, the lack of error bar discussion does raise some concerns, regarding the applicability for users; while the comparisons do indicate that the model provides a good representation of the true diurnal changes for ozone, the small differences that come up in terms of inter-instrument trend comparisons, for example, might still be "explained away" by uncertainties in model-based corrections, even after diurnal adjustments. Other uncertainties involve actual line-of-sight gradient issues, not just for the model results, but also for satellite-based retrievals, in particular, for solar occultation results (for which some attempts have been made to adjust for such gradients, but not as a general rule). These issues are the more difficult aspects, but this does not preclude, in my view, publication of this sort of manuscript. I ask for minor clarifications and some attempts (at least) at a better discussion regarding uncertainties, see my specific comments below. I also provide editorial-type comments, mostly as suggestions or corrections.

Specific Comments

1) One somewhat confusing detail has to do with the normalization time. For example, pg. 4, line 20, and pg. 6, line 4 refer to midnight as a normalization time. The Fig. 1 caption agrees with this description. However, the caption for Fig. 2 refers to 1:30 am as the normalization value, and so does Fig. S9. It would be good to clarify why there are these different normalization times, or if they should be the same. It probably does not matter too much, if different Figures are normalized slightly differently, but I found this confusing, so if something is written in error there, please correct.

2) Error bars are not always described (e.g., for Fig. 3), or justified (e.g., why not use standard error in the mean rather than standard deviation for Fig. 5 and Fig. 7, and

similar Figures in the Supplement?). When using a very large data set (e.g., 2004-2018 Aura MLS data in Fig. 5), the random source of error will basically disappear. As an aside, geophysical variability probably accounts for some of the year-to-year differences; differences in day/night temperature or $H_2O$ ratios, for example, could have some impact on $O_3$ abundances and $O_3$ diurnal change. In the mid- to upper stratosphere, $N_2O$ day/night variability from year-to-year (or month-to-month) could impact ozone and its day/night ratios. Some comments about why the authors chose to use standard deviations rather than errors in the mean would be welcome (is it to try to encompass such geophysical variability, which would be ignored in a standard error minimum type of error representation?). Maybe the standard deviation is indeed a more acceptable way to try to encompass sources of error, but I would welcome a brief comment regarding this point somewhere.

3) In some places, there is a mention of vertical "integration" of MLS data to match the vertical resolution of SBUV. This sort of smoothing is best done via the use of MLS Averaging Kernels (and MLS a priori data), although this can be somewhat tedious. The details are not mentioned here, but probably some indication of the "smoothing" or averaging process should be provided. Is there no smoothing in the Figure 5 results? Maybe errors in this, or omission of this, could lead to differences or discrepancies in the results (?). [It would also make more sense to smooth the MLS data sets for day and night and then calculate the ratios, than to smooth the MLS ratios, not that this is what was done].

4) For Figure 6 in particular, the model could be used, in theory at least, to calculate line-of-sight differences in ozone signal for a solar occultation measurement, using small time steps for such a "ray-tracing" calculation, including height-dependence. Comparisons to a case assuming homogeneous line-of-sight ozone abundances, which is often assumed in retrievals, could be made. In theory, the sign of the differences in this case (model versus observations) could thus be ascertained. The authors could at least expand on this by stating that these comparisons are difficult be-

cause of not only the model calculation aspects but also the satellite retrieval aspects (they do mention the model, it seems, but not the satellite retrievals explicitly). It is alright to state that such detailed analyses are needed to better ascertain whether the model and data really disagree, even if the more detailed work is not pursued in this manuscript. Also, I wonder if one would not need finer sampling of the model in local time to match the fast changes at sunrise or sunset...(I am not asking to necessarily carry this research out in detail here).

5) Error bars: I would note that there are no error bars in Figure 6, so either they are too small, or just not calculated (as a standard deviation of the ratios, as done in other Figures), probably the latter. Including such error bars would make sense, however. Also, the error bars in Fig. 8 seem to be indicated by dashed lines, a different format, but please explain these ranges in the caption. Also, in Figure 9, maybe a standard error in the mean values as a function of time here would be more appropriate, but no error bars are shown; some comments regarding this (or actual error bars) would be appreciated as well. I expect that the volume of data used for these comparisons (for each specific month) is large enough to ensure that random errors become negligible.

Editorial-type Comments / Suggestions

- Page 1

L14, add a comma after "this issue".

L16, change "applied in" to "applied to".

- Page 2

L3, decide if use ODSs or ODS (I would follow the WMO Report type of writing, so probably ODSs for plural, elsewhere also)

L6, change "has been" to "have been".

L24, "to analyze the ozone diurnal cycle at ..."

L28, change "Atmospheric" to "Atmosphere".

- Page 3

L3, "non-sun-synchronous"

L6, change "source" to "sources".

L7, I suggest "Also, these missions do not provide full global coverage."

L25, "as well as to that from..."

- Page 4

L19, it seems that "semi-hourly" should replace "hourly" here, since you use 30 minute model time steps.

- Page 5, L7. You mention OMPS NP and OMPS LP. You also later refer to OMPS profile data and mention NP (top of page 12). Please clarify which data set is being used, NP or LP (or both?), as this was not quite clear enough; maybe this mainly requires a change on page 12. If the datasets are used as mentioned (LP for one plot, NP for another), please clarify (briefly) why one should use LP versus NP or vice-versa (what are advantages/disadvantages of NP versus LP?).

- Page 6

L8, add commas "...very little, if any, variation..."

L12, add a comma after "Parrish et al. [2014]".

L15/16. However, SMILES data also suggest that ozone is decreasing..."

L22, add a comma after "Figure 4a]".

L28, either say "variations greater than" or "variations of more than"

- Page 7

L2, authors suggest that the

L8, Delete "Supplemental"

L9/10, matches the higher summertime amplitude model diurnal cycle reported by Studer...

L11, panels of Fig. 1 show the diurnal cycle...

L14, change "greater" to "more".

L18, but with larger afternoon values at 3 hPa

L22, delete "Supplemental" [also, it is a bit strange to refer to S1 after you referred to S2 earlier]

- Page 8

L20, relative maxima.

L22, relatively high ozone value.

- Page 9

L7, add a comma after "this comparison"

L8, shows the ratios of daytime to nighttime averages

L17, amplitude of those in the MLS data, with ratios generally ...

L18, near 1 hPa, we note a local minimum in ...

L19, local minimum

- Page 10

L2, delete "Supplemental"

L23 and L26, (maybe) change middle latitudes to midlatitudes

- Page 11

L3, change "site" to "sight"

L18, add a comma after OMPS NP. also, please state briefly how the conversion for MLS O3 profiles from pressure to altitude is made.

L20, change "show" to "shows"

L24, influence of the diurnal cycle on such analyses

- Page 12

L11, please add a sentence or two describing the "known bias pattern" for nadir UV instruments... Not everyone is familiar with what this means, and readers should not have to try to dig this out from other references (top-level information at least); "bias pattern" versus what? (in general?).

- Page 13

L10, please specify which instrument's results show a larger (or smaller) amplitude, is it MLS or SBUV, since the differences do not provide the reader with this information. One would think that the finer resolution instrument might provide a larger amplitude, although the broader vertical extent of the SBUV views means that this is actually not obvious.

L13, delete "Supplemental"

L24, change ozone levels to ozone values

L25, expressed as ratios to the value at midnight

- Page 14

L11, change "depicts" to "exhibits"

L27, suggesting that the representation...

- Page 21, change Froidevaux to Froidevaux et al.; also change Livesay to Livesey.

- Figure 1: the caption says "30 hPa to 0.3 hPa" but the plots seem to go down to 50 hPa. Please clarify.

- Many of the Figures spell "AURA" rather than "Aura", which is the correct spelling (it is not an acronym), as spelled correctly in most of the manuscript. It would be good to correct the Figures for this. Also, Fig. 4, Fig. 5, and others in the Supplement have Day/Night Ratio as plot titles, but show ASC/DSC (for Fig. 5) in the axis labels... In reality, day and night during polar summer or winter does not make sense, as it is always either day or night, so it is more correct to state ASC/DSC as what is being calculated, if I am not mistaken. If this is true, the Day/Night labels should more properly be written as ASC/DSC, and for consistency with axis labels... At most latitudes, of course, this is the same thing... In Fig. 5 (and others like it) there are confusing y-axis tick marks on the right side; it would be best to delete the altitude tickmarks there. Also, in Fig. 9, the last sentence could be rewritten a bit as "adjusted to *a* common time of 1:30 pm, to coincide with *the* Aura MLS measurement time." Figure S10: change "output" to "outputs" in the last sentence; it would have been nice to indicate what mostly contributes to the differences between the 2017 and 2018 runs (is it geophysical variability in the model, or were there also some sampling differences in how this was calculated, if matching SAGE sampling patterns?).

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