Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "Study of the dependence of stratospheric ozone long-term trends on local solar time" *by* Eliane Maillard Barras et al.

Anonymous Referee #2

Received and published: 2 April 2020

The manuscript "Study of the dependence of stratospheric ozone long-term trends on local solar time" by Maillard Barras et al. describes homogenized long-term ozone time series from the SOMORA microwave radiometer in Payerne, Switzerland, including comparisons with other ground-based and satellite ozone datasets, and also simulations with the SOCOL v3.0 chemistry-climate model. Additionally, a trend analysis on all datasets is performed, with respect to local solar time, which becomes very important in the upper stratosphere and mesosphere. This analysis aims to answer the question if measurement timing could be the cause for trend variability within different observational datasets in the mesosphere, and therefore also aims to reduce the uncertainty on a possible ozone recovery signal in the regions of the atmosphere where the diurnal cycle in ozone is significant. The manuscript is very well written, and well

structured. Its analysis is relevant to the community and fully within the scope of ACP. There are only a few minor things I would recommend the authors to consider before the manuscript can be published.

General remarks:

- It might be helpful to describe the actual calculation of the bias a little more in detail. On page 2, lines 159-161 for example, it does not become totally clear if the bias correction was applied to each individual profile or an aggregated profile (e.g. monthly mean). And it is not clear if the bias actually is a value for each specific pressure level, or if it is time dependent.

- Section 2: the mixture between the description of the different data sets, and the homogenization work performed on the Payerne data set was slightly confusing. I think it might be better to separate the two, in a data set description only section (where all the other datasets are described as well), and a separate homogenization section.

- Is there a reason why the conversion from ozone number density to ppm was done with ECMWF temperature profiles rather than the temperature profiles provided with HARMOZ? It might be worth adding a sentence or two for the reasoning of this to the manuscript.

- Page 13, line 387: I think the description of "the good agreement between the Payerne MWR and SOCOL v3.0" is very optimistic when looking at Figure 4. The differences between Payerne MWR and SOCOL range from -12% to +15% in the altitude range from 30-50km with a strong gradient. I think it would be more realistic to describe the agreement a little less optimistic.

Specific remarks:

- Page 2, line 37: reference "Molina and S." seems to be abbreviated

- Page 2, line 38: reference "E. et al." seems to be abbreviated

- Page 6, line 173: "was" missing between "front-end changed"?

- Page 6, line 173: "was" missing between "oscillator repaired"?

- Page 7, line 189: brackets not necessary around "(Hubert et al., 2016; Hubert, 2019)".

- Page 7, line 192: not clear on what temporal resolution the offset application is based on.

- Page 8, line 243: remove "to" in "from to 2.5"

- Page 9, line 264: "converted" should be "conversion"?

- Page 10, line 279: the sentence part "chemical species interacting participating" seems weird and might need rewording.

- Page 11, line 334: typo in the word "satellite"

- Page 12, line 343: it might be helpful to actually give the latitude and longitude values of the box here to make it easier for the reader to understand the box' extension.

- Page 12, line 344: there might be a "that" missing in "means a"

- Page 12, Section 4.1.1.1: It would be helpful to mention here again that the description of the differences is still a description of the results presented in Figure 4.

- Page 13, line 375: remove "s" from "appears"

- Page 13, line 375: the differences being smaller in" NH winter than in summer" is not shown in any figure, right? Would be worth mentioning that here (e.g. adding a "not shown") if you do not want to show these differences.

- Page 13, line 389: "desagreement" should be "disagreement"

- Page 14, line 427-430: If I understand correctly, you used the MLR described in Section 3.2 for the analysis described in Section 4.2.2. Is there a good reason to include ENSO and NOA basis functions in an ozone regression in the mesosphere?

- Page 15, Section 4.2.3: It is not clear exactly what basis functions you used for NOx (and why they would be different for NOx and T for example). In line 445 you mention that ENSO is included as basis function, but in line 459 it is not mentioned anymore.