Comment on acp-2021-880
Anonymous Referee #1

*We are thankful to the referee for the thoughtful comments and suggestions; we address these (in italics) in the text below under **"Answer"**, for each point made. Our revised document will clearly include the changes we point to in detail below.*

Referee comment on "Upper stratospheric ClO and HOCl trends (2005–2020): Aura Microwave Limb Sounder and model results" by Lucien Froidevaux et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-880-RC1, 2021

This is an excellent paper, using results both from MLS measurements and the WACCM model to obtain trend estimates of ClO and HOCl over the ~15 years for which MLS has been making measurements. With the exception of requiring a better explanation for a line used in two of the figures, this manuscript is certainly publication worthy in its present form.

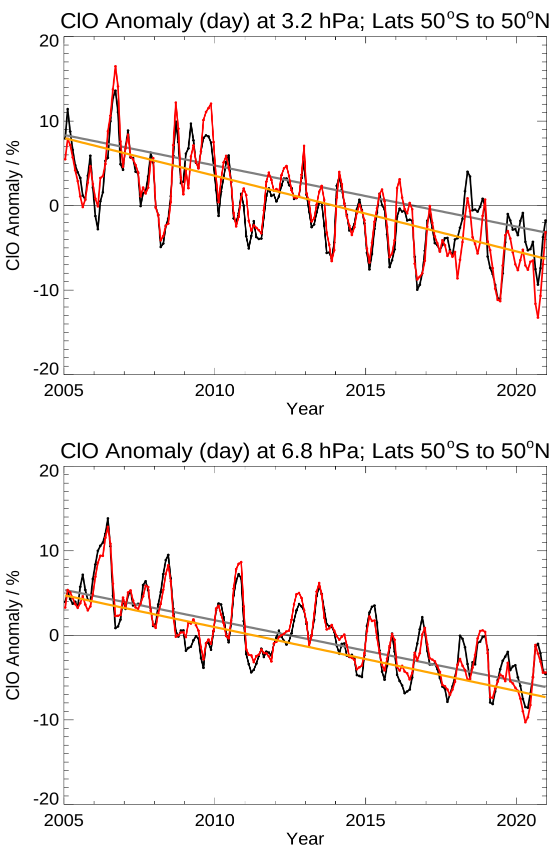
However, I do have to express disappointment with this study because almost all of the analysis of this wonderful MLS dataset and the sophisticated modeling study that accompanies it is reduced to plots of linear trends. Beyond some lines drawn through two figures (which are dominated by annual cycles) the reader is left with no sense of how well a linear trend actually fits this data. The authors use a quite complicated regression fit that includes some quite long-period terms such as ENSO and 11-year solar cycle terms. Before comparing the trend fits between model and measurement, it would be good to know how well these agree or whether they are significant, since differences in these terms could influence the calculated trends. If they don't make a difference please say so.

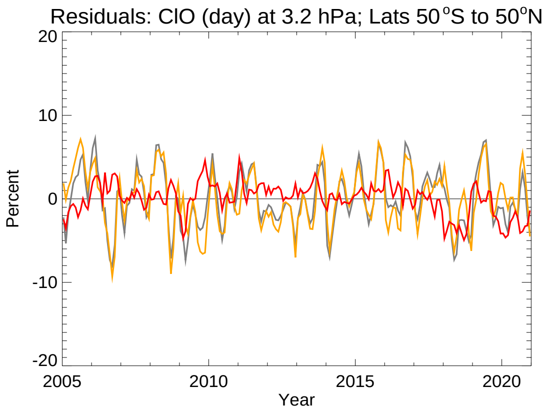
A few simple measurement (and possibly model) timeseries plots of annual ClO and HOCl anomalies (without ENSO, F10.7, or QBO fits) or something similar from 50S to 50N at a few of the altitudes shown in Figure 10 would be of great interest. It would visually help the reader to understand how easy it is to identify a linear trend in this data, would help to answer the question of the importance of the multi-year terms in the fit, and would provide some indication of the importance of endpoints. At present, only Figures 2 and 8 provide any timeseries information, and these are very cluttered and difficult to read, dominated by the annual cycle, and given only for very specific (for some reason different for the 2 species) locations.

Answer: *This is a reasonable discussion point (or disappointment), and we agree that showing some simpler time series of deseasonalized anomalies as a top-level view would indeed be a good addition to the Figures already shown (with associated residuals to be shown in the supplementary material, not to add too much in the main portion of the paper). An example of deseasonalized time series is shown below for near-global ClO anomalies at 2 upper stratospheric pressure levels; the QBO signal dominates, after the removal of the main (annual and semi-annual) cycles.*

In terms of the regression fits, we have used this functional form for other studies where the ENSO term, for example, is more important (for ozone or carbon monoxide in the upper troposphere). However, the ENSO and solar terms do not have a large impact in this upper stratospheric study of ClO and HOCl. We are adding a comment to this effect, i.e. "The largest components in the regression fits are, by far, the annual and semi-annual terms, with some dependence versus latitude and pressure regarding their relative impacts. About 70–80% of *the explained variance arises from these two terms, for both the observed and modeled (near-global) cases. The ENSO and solar terms typically account for less than a few percent of the explained variance, and the same is true for short-period (less than 6-month period) terms. The QBO signal is generally the largest component that remains, if one considers deseasonalized time series, as seen in the Figure [below] for ClO (50°S–50°N). The data and model fits generally behave in similar ways, although there can be some small differences between the two."*

Thus, it is unlikely that the near-global linear trend results would be significantly affected by changes to the regression model (e.g., by using slightly different functional forms or terms), although one can always strive to explain variability in better ways, in order to reduce the error bars to some extent; the linear trends should not be affected very much at all. Such additional analyses would be most useful at 32 hPa, where there is evidence for low frequency (multi-year) ClO variations, although this goes beyond the main purpose of our work (linear trend detection with a focus on the upper stratosphere). However, we did take this general comment to heart, and we are adding the information mentioned here.





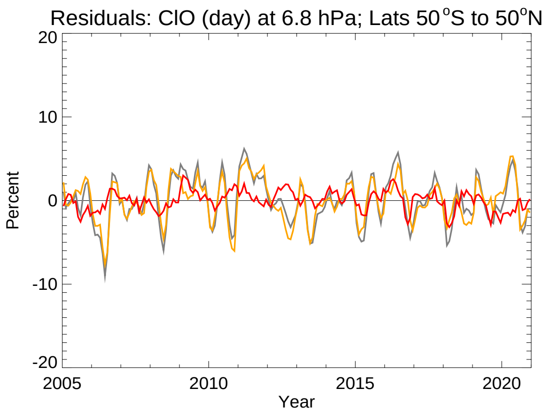


Figure 2 and Figure 8 – I'm afraid that I don't understand the meaning of "the model fit to MLS data". Is this just WACCM minus MLS? The authors seem to be using "fit" to refer to the regression. Line 246 seems to suggests that the pink line is just a difference: "WACCM time series actually fit the MLS data better than the regression fits do". I indeed hope that this line is just a model minus MLS difference with the bias removed, since this would seem to be the most useful and basic thing to plot.
Answer: *Agreed, the "debiased model minus MLS" wording should have been used for more clarity, so we have changed this accordingly in the Figure caption of Figure 2.*

Minor points:
Line 138 – "only" is an unnecessary word here. Also, somewhere in this paragraph it should be mentioned that additional details about the standard HOCl retrieval are included in the MLS data quality document (i.e. Livesey et al. 2020). I realize that is mentioned in 3.2.
Answer: *Yes, these minor points have been taken care of accordingly, even though we are focusing on the non-standard HOCl retrieval in this work.*

Lines 178-193, or thereabouts- I'm pretty sure that "the model" is always referring to WACCM, but it would be nice to spell that out somewhere in this long paragraph.
Answer: *Indeed, the model is always referring to WACCM; we have added a sentence to make this even more clear.*

Line 211 – Just curious as to why here the fitted component apparently follows a different solar model than that mentioned in 2.2.
Answer: *The solar flux model used in WACCM6 (and mentioned in Section 2.2) is based on a fit to solar flux data sources, so this is WACCM's historical use of daily solar variability at many wavelengths, including those that are needed for photolysis reactions in the photochemical treatment. Moreover, as mentioned by Gettelman et al. (2019), "It should be noted that beginning 1 January 2015, solar forcing data are projections based on historical solar cycles rather than from observations." The fits to the data are based on another approach, which uses the F10.7 monthly average solar flux data, also a "historical" choice for the regression routines used by the first author. Switching solar models (e.g., for the fits) has not been attempted in this work; given the high correlation one expects between these slightly different solar variability treatments, this will have a negligible effect on the trends and their error bars, given also the very small contribution from the solar term in these fits. Also, I would reiterate that both the model and MLS time series are treated the same way, in terms of the regression fits (including the solar term).*

Line 216 – "year-long blocks". I agree that this is probably a reasonable choice, but I'm just curious if the authors have any particular reason for this choice. I certainly don't insist on any change in the manuscript.
Answer: *The reason is, in part, to follow what others have done in the past, but also because it makes sense to preserve some of the interannual variability by doing so.*