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Interactive comment on "Trends in Global Tropospheric Ozone Inferred from a Composite Record of TOMS/OMI/MLS/OMPS Satellite Measurements and the MERRA-2 GMI Simulation" *by* Jerry R. Ziemke et al.

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General Comments

This paper presents satellite tropospheric column ozone measurements from various instruments over the period 1979-2016, and compares with a simulation from a chemistry transport model driven with reanalysis meteorology and best estimates of changing emissions. The comparison indicates similar upwards ozone trends with a similar regional time evolution (accelerating increases over Asia in recent years). The data

and model comparison are well presented and the overall story is convincing, and well worthy of publication. The model simulation details should be expanded a little (see below), to stress that the ozone increases are not simply due to increases in NO emissions. Indeed, it would be most interesting to extend the modelling work to more fully understand the drivers of the ozone increase (e.g. the components from methane, NOx, any changes in stratospheric contribution, any changes in ozone lifetime, e.g. due to changes in deposition or humidity), although I can imagine the authors will say this is beyond the scope of the current publication. Nevertheless, if they can say anything about attribution that would be most useful, especially from a policy perspective – we would like to understand the processes that have led to the increases in ozone seen, in order to reduce/reverse them in future. In a few places, the paper lapses into overly technical jargon, but on the whole it is clear and well written. If these points can be clarified, and the specific points below addressed, I fully recommend this paper for publication.

Specific Comments

L30 The GMI simulation is definitely not 'identical' to the satellite measurements. (It would be worrying if it was.)

Done.

L31 Define TCO.

Done.

L38 N Atlantic

Done.

L42 ...changes in emissions and concentrations of global pollutants, ...

Done.

L46 The Lin et al. (2017) study appears to focus on the US rather than being a truly global study (cf. Young et al., 2013, for example). [Young, P.J. et al. (2013) doi:10.5194/acp-13-2063-2013]

We added discussion of the Young er al. (2013) paper in this section in the revision.

L51 The Shepherd et al. (2014) paper is mainly about stratospheric, rather than tropospheric ozone, so also seems an odd choice at this point.

Shepherd et al. (2014) reference and discussion has been deleted in the revision.

L120 [I complained about this in my initial report, but it's still here!] "... developed within NASA Goddard Code 614 ..." I don't know what this means. Is it a building or an institute? Is it a protocol or some sort of NASA standard method that we are all supposed to know? It is technical jargon that should be decoded for the non-NASA general public readership. At least give us a reference.

This has been re-worded in the revision.

L143 What are "...in situ UV cloud pressures..."?

Re-worded.

L201 What is a "...de-trended cumulative mass flux ..."?

We have re-written this section to clarify with reference to the cumulative mass flux within the model.

L208 The model description doesn't mention several important aspects for ozone. How is methane handled? (The ozone trends will have been partly driven by methane trends, but it is not mentioned at all). The focus is on emissions – but what about ozone removal? What does the ozone deposition scheme look like? Is it related to land cover properties (e.g. Leaf Area Index, etc.), and does this change? How does effective stratospheric Cl loading vary?

These are good points which we have added discussion in the revision. We discuss

methane source and ozone deposition, etc. for the model.

L218 Most all?

We have added extensive discussion to clarify.

L225 What are 'TCO offset differences'? They are not explained. Is there an overlapping period of both TOMS and OMI/MLS in 2005?

Added text to clarify.

L233 'after 2016' is not very specific.

Re-worded to clarify.

L286 tropospheric NO emissions ...

Done.

L289 NO2 concentrations? Please clarify that emissions are not equivalent to, or to be used interchangeably with, concentrations. This is fundamentally important.

Re-worded now to clarify.

L296 NO emissions ...

Done.

L334, 335, 337 lower

Done.

L343 I don't think Figure 5 is referred to (either at all, or before Figure 6).

Thanks for seeing this. We didn't even discuss Figure 5 in the original draft. Figure 5 is now mentioned in the main text and has a corrected figure caption as well.

L388 CFC concentrations?

Done.

L448 most –> more

Done.

L725 Does Figure 5 (which, as noted above, is not referred to in the main text) really show trends in biomass burning emissions, as the caption indicates?

Very correct – the current figure is not just biomass burning NO, but all NO emissions just as in Figure 2.

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