

We thank the reviewer for the comments and suggestions and revised the manuscript accordingly. All changes are indicated with Latexdiff. Below, we added a point-by-point response to the major concerns. Some of the major concerns are related to the publications of Lubis et al., 2017 and Bosilovich et al., 2015. The later includes a more generic form of equation 1. Our nomenclature and also some statements that are of concern were introduced in Lubis et al. 2017. We rephrased the paragraphs and added additional explanation.

Major

issues:

Comment:

Lines 109-112: the upper limit of the vertical ranges of the two ozone data-sets assimilated into MERRA-2 is of at least as much interest here as the lower limit, as this should limit the usability of the MERRA-2 ozone to higher altitudes. So please state these upper limits as well. To the best of my knowledge, MLS ozone is assimilated up to 0.02 hPa, so the rather large disagreement between MLS and MERRA-2 ozone above 0.1 hPa is a bit strange. You can't resolve this issue in this paper, but you should discuss it in some way.

Reply:

We added the upper assimilation limit in the bracket. The differences above 0.1 hPa are explicitly mentioned in the discussion section.

Comment:

Line 124: As I already pointed out in my previous review, in the upper mesosphere, odd oxygen is dominated by atomic oxygen, not by ozone; this means that in the upper mesosphere, MERRA-2 "ozone" is actually atomic oxygen, not ozone. As there is much more atomic oxygen in the upper mesosphere than ozone, this could explain the large discrepancy above 0.1 hPa observed compared to MLS and GROMOS ozone. Again, this is not an issue that you can resolve, but you should discuss it.

Reply:

We explicitly added the suggested discussion to the MERRA2 section and expanded the description.

Comment:

Line 124: I am not quite sure the term "prognostic variable" is used correctly here. As I understood equation (1) in this context, the production and loss terms of the assimilation model are derived for odd oxygen, not for ozone. As particularly the chemical production and loss terms are much simpler for odd oxygen than for ozone, this approach makes sense, particularly in the stratosphere where odd oxygen is dominated by ozone.

Reply:

The term prognostic was introduced in Ludis et al 2017 (page 2439, first paragraph left column (top)). However, we understand the concern of the reviewer and have changed the name to 'diagnostic'. The odd-oxygen model is used to estimate the state of the ozone rather its temporal evolution or a future prediction in the context of the data assimilation.

Comment:

Lines 127-129: again, I am not clear whether "vertically integrated" is the correct term here, either for ozone itself, or the ozone tendencies. My understanding is that MERRA-2 assimilates both column ozone and ozone profiles. For column ozone, it might make sense to use vertically integrated tendencies, but it does not make sense when ozone profiles are assimilated. Why integrate vertically, if the information is vertically resolved? You would lose information about the altitude profiles. Also, later on

(e.g., figures 6 and 8 and discussion) you yourself use the terms given in equation (1) for specific pressure levels, not vertically integrated.

Reply:

The term vertically integrated refers to the nabla operator in the first term of the right-hand side. The continuity equation includes a z-derivative that is usually vertically integrated, which means numerically estimated from the layer above and below. In so far vertically refers only to the 3DVAR data assimilation, which couples the layers above and below and is not meant as vertical integrated quantity for ozone (dobsen units).

Comment:

Line 129, equation (1): I did not find this equation in either of the references you provide, also not in Wargan et al, 2015. Please clarify where this equation is from, clearly providing the reference and equation number within this reference.

Reply:

The equation was presented in Lubis et al 2017 (page 2439, first paragraph left column (top)) and this paper refers to Bosilovich et al., 2015. Both references are included.

Comment:

Figures 3 and 4: why show GROMOS and MLS up to 0.01 hPa, but not MERRA-2? Presumably, the reason is that the agreement is not good. However, you can't evade a discussion of this point by just not showing this altitude region for MERRA-2. I see two options. Either exclude the region 0.1-0.01 hPa completely. Than you shouldn't show it here or in any other figure for any of the data-sets, and make a clear statement in the methods description why this region is excluded from the analysis. Or you can show the region here; than you should include results from MERRA-2 here and in the following for this region as well, and discuss here why they don't agree well with MLS (despite MLS being assimilated into MERRA-2 up to 0.02 hPa) and GROMOS.

Reply:

Figure 3 and 4 show all data sets up to an altitude of 0.01 hPa. Only Figure 2 is limited to 0.1 hPa.

Minor Comment:

Lines 10-11: it is not clear to me in this context what this means. Unfortunately this also still does not become clear in the description of methods in Section 2.3/2.4. I think what you mean is “..we contrast results from the continuity equation using MERRA-2 reanalysis data with the terms of the ozone tendencies as used in the MERRA-2 ozone assimilation model.”?

Reply:

We rephrased this sentence and replaced “contrast” by “compare”.

Comment:

Line 50-53: It is unclear what you are trying to say in this sentence. In particular, “In the mesosphere/lower thermosphere (MLT) region, ... in the mesosphere ...and the altitude region” is unclear - do you mean “in the MLT” or “in the mesosphere”? Which altitude region?

Reply:

We rephrased this sentence and made the altitude region less ambiguous.

Comment:

Lines 109-112: “are used to estimate ozone in MERRA-2” they are assimilated into MERRA-2. Please clarify.

Reply:

The manuscript uses assimilated:

“The retrieved ozone profiles from the Solar Backscatter Ultraviolet Radiometer (SBUV, 1980 to 2004) and the MLS (since August 2004, down to 177 hPa until 2015, down to 215 hPa after 2015 and up to 0.02 hPa) and TCO from SBUV (1980 to 2004) and the Ozone Monitoring Instrument (OMI) (since 2004) are assimilated into MERRA-2 (Gelaro et al., 2017).”

Comment:

Lines 204-207: “our analysis reveals a qualitative agreement ...” this is maybe mostly a matter of style, but as this is a conclusion from the results shown in this section, this statement should be at the end of the section, not at the beginning.

Reply:

We did draw this conclusion here in this section to justify the more detailed analysis that followed in section 5. As this was a recommendation of another reviewer, we would like to keep it.