This paper analyses the added value of IASI O3 measurements by assimilating six years of IASI tropospheric columns and of MLS stratospheric profiles in the MOCAGE chemistry and transport model, by comparing that IASI-MLS analysis with the MLS analysis and the direct model, and by validating these analyses against ozonesondes in the tropical bands. Ozone ENSO Indices (OEI) are derived from the analyses and compared with the one computed for the OMI-MLS measurements and the Nino 3.4 index. This paper is an interesting and substantial work. The manuscript is well written, clearly structured and the figures illustrate well the key points. I recommend publication of the paper subject to the minor revisions detailed below.

General comments:

Some of the individual analyses related to the ENSO influence are similar to what is described in Wespes et al. (2017, JGR) which analyzes nine years of TOC observations from the IASI instrument with geophysical drivers. Although Wespes et al. (2017) analyzes the influence of the ENSO on TOC one step further by examining the ENSO-related tropospheric O_3 responses over tropical and extra-tropical regions, this earlier study should be acknowledged in the paper as the first one reporting ENSO-related O3 variations in IASI TOC measurements and should be discussed accordingly.

I realize that I do not feel qualified to the rigor of assimilation techniques, but I do not fully understand the added value of the assimilation for analyzing the ENSO-related variability in IASI TOC. The IASI dataset has been shown to be huge enough in previous studies to perform that kind of analysis. It could therefore be interesting to compute an OEI from the direct IASI TOC and to compare it with the IASI-a index derived in this paper. Could you bring that additional information and better discussed the added value of the assimilation technique in this study?

In addition, given that IASI provides vertical information on stratospheric O3, I'm not sure neither about the added value of using stratospheric O3 from MLS (for IASI-a) instead of the whole IASI profile. I understand that the MLS vertical profile in the stratosphere is better resolved that the one of IASI, but the focus is given on TOCs and one could think that IASI would constrain the model enough in the stratosphere for assessing comprehensive TOC analysis. Could you please clarify?
Furthermore, It is obvious that IASI-a is more appropriate than MLS-a for analyzing variation in TOCs and computing an OEI given that MLS does not sound the troposphere and that "little information is brought by the assimilation of MLS data" (cfr p.13, l.2). I'm also wandering in what way the different biases from IASI and MLS would impact on the IASI-MLS analysis. Could you specifically explain in the text the advantage of assimilating MLS in the stratosphere for the purpose of this study?

In Section 3.2.1, figure 6: you validate the IASI-a (IASI-MLS) analysis with the OMI-MLS residual method, meaning that MLS measurements are used in both sides. One could think that you turn around here. I guess here that you want to leave out the effect of the stratospheric O3 variation from the validation of IASI-a TOC. If correct, it would deserve to be clearly mentioned in the text.

Specific comments:

p. 2, l. 24-26: That sentence which refers to the increasing biomass burning in Indonesia, not to convection, should be moved after the following sentence in l.26-27.

p. 3, l.12-14: It should be mentioned that O3 sensitivity to ENSO has been already studied with IASI as well (Wespes et al. 2017).

p.3, l.20-21: The added value of using both IASI in the troposphere and MLS in the stratosphere to obtain direct evaluation of tropospheric O3 is not clear to me and should be specifically explained.

p.4, l.6-8: The 6-years reanalysis is here presented as the first IASI dataset suitable to perform analyses of O3 variations in the tropics. It is obvious that if the analysis of O3 variability can be performed from the direct IASI measurements, the reanalysis dataset is also suitable for that study. The added value of using the reanalysis is not clear. Please explain.

p.5, l.18-20: The values here are discussed in terms of accuracy, precision or bias. All these terms are used depending on the altitude layers. I think the reported values refer here to bias only. Please clarify.
What is the bias in stratospheric O3 from MLS in comparison with IASI?

p.6, l.27: The exact portion of the IASI profile "(1000-345hPa)" which is assimilated in the model should be defined earlier in the abstract and in the IASI measurements section 2.1.1. I thought that the whole IASI profile was assimilated with the MLS stratospheric profile. I only get the information later in Section 2.3.2 (p.7).

Why using 1000hPa for the bottom level and not the surface?

In section 2.1.2, it is written that you use the MLS data only between 12.12 hPa and 177.83 hPa, which means that no satellite measurements between 345 hPa and 177.83 hPa are assimilated and hence that there is no direct constrain on UTLS O3. How would it affect the assimilated TOC from 1000 to 100 hPa that are later validated and analyzed?

p. 9, l. 12 (validation section): I do not understand why you validate the TOC ranging from 1000 to 100 hPa. This range covers more than the troposphere (and than the assimilated IASI TOC) including the UTLS and, hence, it does not seem the most appropriate column for the IASI-a validation. It may mask a part of the added value of IASI. That would be fully achieved by validating the 1000-345 hPa column from IASI-a vs from MLS-a.

p. 9, l. 28-32 (figures 3c and 3d): The authors explain the larger biases from IASI-a than from MLS-a in the boundary layer by the weaker sensitivity of IASI in that region. However, MLS does not even sound the boundary layer at all. How could you explain that MLS-a better reproduces the O3 sonde observations than IASI-a? Please clarify.

p. 10, l. 24-26: What could explain the peak observed over Indonesia in October 2011 (Fig. 4c) in the O3 sonde dataset only and not in IASI-a?

p. 13, l. 25: "… Nino 3.4 is calculated from SST anomalies in the Pacific Ocean": One reference or the source of the available dataset is missing here.

Technical corrections:

p.6, l.13: “identical with” → “identical to”.

p. 10, l. 17-25, figure 4: According to the text and the figure caption, it seems that the y-axis is strongly annotated. It should be “1000-100hPa” everywhere. In addition, please indicate the regions in the corresponding panels.