Response to Reviewer #2:

1-The description of the satellite instruments is almost nonexistent in the paper. This is crucial for the reader to know what these instruments are capable of measuring. I would suggest to add a paragraph or/and a table recapitulating the instrument characteristics. The figures with the averaging kernels are not sufficient to understand the impact of the data. For example, in Natraj et al, the averaging kernels are normalized averaging kernels and this is not specified in this paper. The instrument configuration used by Natraj et al. is probably different from the TEMPO one. How the averaging kernels presented in the paper are constructed? I haven't seen them in Natraj paper. I have a similar remark for the atmospheric (Temperature, species input, ..) and surface parameters (albedo, ..) used as input by Natraj et al. Are they relevant for the period and the surface of the OSSE? I would suggest the authors to comment this in the paper.

A comparison of TEMPO specifications has been added to the specifications from Natraj et al. : "The UV+Vis spectral ranges (290-340 nm, 560-620 nm) and spectral resolution (0.4 nm) assumed by Natraj et al. (2011) are comparable to the spectral ranges (290-490 nm, 540-740 nm) and spectral resolution (0.6 nm) planned for TEMPO."

The averaging kernel matrices are taken directly from the work by Natraj et al., as indicated in the paper.

We have added to the conclusion on the effect of using fixed averaging kernel matrices on our OSSE results (see response to point 4 below).

2-I found the use of the LEO data too quick to be convincing. I did not see if the authors used the nighttime data to conclude that LEO data do not add any significant contribution. The TIR should bring information during nighttime in the free troposphere and from long range transport. But the question is perhaps what is the information brought by the LEO satellite? For example what are the differences between the couple "ground based stations and TEMPO GEO" vs the couple "ground based stations and IASI-3 LEO"? and this for the two OSSEs proposed. I would suggest the authors to present the results of this OSSE to show the relevance of a GEO vs a LEO. We will see the real benefit of TEMPO vs the existing system.

We have attempted to make clear the use of nighttime LEO data twice in Section 2.2: "TIR has the

advantage of providing observations at night that will be complementary to TEMPO."

and "We similarly generate synthetic LEO IASI-3 observations over the North American domain twice a day (local noon and midnight)."

We have modified the statement regarding the information provided by a LEO instrument in addition to having TEMPO observations: "The LEO instrument will thus be valuable for tracking transpacific transport of ozone plumes even when TEMPO is operational"

3-For the high-ozone events in the Intermountain West OSSE, I did not understand why there is no data that cover California. In the CASTNet surface network, there are stations located in California. Are they representative of the background? if not, this is a pity because one or two stations in this region or in the Las Vegas area would be sufficient to give better results with only surface data assimilated. In addition, I find the results of GEOS-Chem model too different from the CCM. Why GEOS-Chem model is so different? By using such simulations, the improvements by assimilating synthetic observations are highlighted too much. Please comment on this in the paper.

We have added a sentence explaining why California CASTNet observations were not used: "CASTNet stations outside of the Intermountain West are not used as they do not provide useful constraints for the region."

Added comment on how the differences between the models affect the OSSE results (see response to point 4 below).

4-Finally, I think the different assumptions taken by the author make the OSSE very likely overoptimistic. Above all the fix averaging kernel for the full period and the entire West of US area without taking into account the heterogeneity of the surface (surface albedo, surface temperature, etc) for the GEO and the LEO is somehow questionable for the final results. Because if the OSSE is overoptimistic, how useful is the final result for concluding on a quantification of the benefit from GEO ozone measurements? I would suggest to comment on how overoptimistic (or pessimistic if it is the case) the OSSE could be.

We have added a paragraph to the conclusion on the effect of our assumptions on the OSSE results: "The use of invariant averaging kernel matrices is a limitation of this study. Preparation for TEMPO must include improved constraints on physical parameters, such as surface albedo, that can vary greatly over the North American domain and that affect the sensitivity of UV+Vis retrievals of near-surface ozone. Also, if the differences between the two models used in our OSSE are larger than future errors in modeled ozone, this study may overestimate the information TEMPO will provide."

Minor comments

In the introduction, the authors have cited Fishman et al., 2012 but they have forgotten the European and US authors for their work on Geostationary satellites for monitoring air quality (Lahoz et al., 2012). I would suggest to add this publication (see reference below).

Citation added to introduction.

Still in the introduction, the authors mentioned the different missions targeted at air quality over Europe with S-4 or GEMS over East Asia. I would suggest to add some information about their differences with TEMPO. For example I think S4-UVN or GEMS have only UV channels (no visible channel) without the possibility to have some sensitivity for ozone at the surface. It would be interesting to know how the global constellation of GEO satellites will be done for ozone to target air quality purposes. I would suggest to add a comment on this in the introduction.

Added clarification of Sentinel-4 and GEMS spectral coverage: “Sentinel-4 and GEMS will only measure ozone in the UV.”

In section 4, the period 12-15 is confusing. In the text the authors mentioned the 13 June but they mentioned the 14 June in Figure 7. I would suggest to add this latter date when describing Figure 7. Also, it would be interesting to see horizontal maps for this particular day to evaluate the impact of stratospheric ozone at the surface or in the free troposphere.

Rewrote the sentence containing June 12-15: “Actual observations at nearby CASTNet locations indicate ozone in excess of 75 ppbv during this modeled intrusion.” Caption of Figure 7 modified to 2100 MT (Mountain Time) 13 June from 0300 GMT 14 June for clarity.

In Figure 4, 5, 6 and 7: if LEO data are used in the assimilation process please indicate it either in the caption, and on the panels, and in the text.

Use of LEO data is now correctly included in the captions for Figures 4-7. We have left the panel labels as TEMPO for these figures for clarity as the LEO observations do not add information in addition to the TEMPO observations.