Review on «Monitoring high-ozone events in the US Intermountain West using TEMPO geostationary satellite observations" by P. Zoogman et al

General comments :

This paper presents an observing system simulation experiment (OSSE) to quantify the benefit of TEMPO (Tropospheric Emissions: Monitoring of Pollution) ozone in a general observing system composed of ground based data and Low Earth Orbit (LEO) data over the west part of US. TEMPO is a future US geostationary satellite, the LEO used in this experiment is the next generation of IASI and the ground based data network is the CASTNet surface network. The authors used the GFDL AM3 CCM and GEOS-Chem CTM as the different models to construct the OSSE. The CCM simulations are considered as the truth while the CTM assimilates the different synthetic observations from this truth considering the characteristics of the instruments used in the OSSE (ground based station, future TEMPO and next IASI). The authors applied their OSSE system to high-ozone events in the Intermountain West and a stratospheric intrusion to evaluate the benefit of TEMPO for these situations. The first OSSE is performed on April-June 2010 and the second on a shorter period between 12-15 June 2010.

This paper has the advantage to present a complete OSSE using different future observations but it seems to me lots of details are missing to convince me totally. I would suggest to answer these following points :

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.

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.

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.

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.

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).

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..

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.

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.

reference :

Lahoz, W.A., V.-H. Peuch, J. Orphal, J.-L. Attié, K. Chance, X. Liu, D. Edwards, H. Elbern, J.-M. Flaud, M. Claeyman and L. El Amraoui, 2012 : Monitoring air quality from space : the case for the geostationary platform. Bull. Amer. Meteor. Soc. doi :10.1175/BAMS-D-11-00045.1