

## ***Interactive comment on “Monitoring high-ozone events in the US Intermountain West using TEMPO geostationary satellite observations” by P. Zoogman et al.***

**Anonymous Referee #1**

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This manuscript presents an Observing Simulation System Experiment (OSSE) to evaluate the potential impact of ozone observations made from geostationary orbit in improving monitoring and attribution of high ozone events in the Intermountain West of the USA, where there are limited surface measurements available. The manuscript highlights the improvement in monitoring such events brought about by assimilating UV-visible measurements made from a geostationary platform relative to IR measurements made from a low-Earth orbiting platform. While there is nothing particularly unique about the analysis presented in the manuscript, the authors make a compelling case for the role of planned future observations in monitoring and attribution of pollu-

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tion events in a specific region with limited availability of continuous measurements. The scope of the manuscript is suitable for publication in Atmospheric Chemistry and Physics after the general and specific comments set out below have been addressed.

General comments

Abstract: The abstract isn't particularly clear in its current form and I believe that significant rewriting will be of benefit to the reader. I have made some specific comments related to this below.

Figure labeling: it would be useful if individual plots in the figures were labelled (a), (b), (c) etc. which would help to clarify the figure captions and the descriptions given in the text, rather than “left”, “top left” and so on.

Specific comments

Page 33464, Line 4: “subsiding background influence” is a bit vague for the abstract, try to clarify what this means. Line 5: I suggest clarifying that you mean “surface measurement network”. Line 10: the combined use of UV and Vis measurements should provide improved retrieval sensitivity in the lower troposphere, I suggest changing “for sensitivity” to “to provide sensitivity”. Line 12-13: clarify what the three data sources are, it reads here as though CASTNET (needs defining) are the only available surface measurements, also in the next sentence you say the synthetic data are assimilated but it isn't clear which data these are (all data? just the satellite data? just TEMPO?). Line 15-16: the context for the error correlations isn't clear, I would suggest to remove this from the abstract unless it is a critical result. Line 16-21: it would be useful to the reader if some numbers could be included when you talk about improvements of using TEMPO for monitoring over just using the surface measurements or LEO IR instrument.

Page 33465, Line 2: I suggest clarifying that you mean “high-elevation measurement sites”. Line 11: clarify what you mean by “sparseness of satellite data” - observations have been made with relatively high spatial coverage by OMI for almost 10 years and

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GOME-2 for almost 7. Line 12-17: I suggest moving this sentence to the end of the introduction when you set out what the manuscript analyses as you also re-introduce TEMPO later on in the introduction. Line 18-27: it would be of benefit to the reader to transfer a lot of the information in this paragraph to earlier in the introduction. In particular, I think moving the definitions of North American background and Intermountain west would help with the flow of the introduction.

Page 33466: I recommend swapping the paragraph about the CTMs with that about background effects to help with the flow of the introduction. Also, you mention the stratospheric influence a couple of times here and I think it would be beneficial if you could say something about what constitutes the background ozone source earlier in the introduction.

Page 33467, Line 1-2: Could you say something about the limitations of LEO observations here? You mention about a 1-day return time for the orbit which is true for the orbit track, but a lot of these instruments scan across the track which can increase the time between repeat observations. Line 20-21: Another paper has been recently published, also applying an OSSE for geostationary measurements of European air quality, which could be useful to cite here: [www.atmos-meas-tech.net/7/391/2014/](http://www.atmos-meas-tech.net/7/391/2014/)

Page 33468: It looks as though a lot of the information in the second paragraph on this page and the first paragraph of Section 2.1 contain a lot of the same information, please check the consistency of these two paragraphs and reduce the amount of repeated information.

Page 33470, Line 4: the statement “now becoming operational” is a bit strange - instruments observing tropospheric composition from LEO platforms have been operational for 15 years since the launch of GOME. I suggest removing the statement. Line 7-8: please clarify that the IASI instrument measures at TIR wavelengths and ozone is retrieved from these measurements. Line 8-12: the final part of this paragraph doesn't seem to serve a very useful purpose to the flow of the paper and I would suggest mov-

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ing this to the concluding remarks. Line 25: in principal any retrieval approach could be used for these instruments, please clarify that you assume profiles retrieved using optimal estimation.

Page 33471, Line 14: have the latitudes and longitudes of the North American domain been given earlier in the manuscript? also, I recommend using “averaging kernels” rather than “averaging kernel matrix” here and throughout the manuscript. Line 27-29: in the final sentence you mention both IASI and LEO instrument (and IASI-3 elsewhere) - it would be of benefit to the reader if you chose one term (LEO?) and use that consistently throughout the manuscript.

Page 33472, Equations: an unusual notation is used in the equations for the data assimilation, typically H is used for observation operator and K for Kalman gain, is there a reason why the authors do not use these?

Page 33475, Line 12-13: when giving the horizontal resolution of the GEOS-Chem grid it would helpful to specify which is the longitude and which is the latitude.

Page 33476, Line 11-12: clarify that TEMPO makes continuous daytime observations, also clarify that the peak sensitivity of the averaging kernels (and DOFS?) indicates the potential for simultaneous sounding of free troposphere and boundary layer. Line 20: please check that this is the correct Lin et al. reference. Line 20-21: it would be useful to also show the CASTNET time-series in comparison with the other lines on Figure 6.

Page 33477, Line 1-2: clarify what the bottom left plot of Figure 7 shows - is it synthetic TEMPO data? Line 10-11: the last sentence looks out of context here, hasn't it been established in the literature that LEO observations can track pollution plumes? I suggest either removing this sentence or clarifying the advantage of the LEO instrument over TEMPO for monitoring high ozone events due to long-range pollution transport.

Figure 1 caption: it would look better if the plots of the maps were consistent with those in Figure 5, i.e., with labelled horizontal and vertical axes. Also, the black lines marking

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out the area of interest are not clear, I suggest redrawing them in a different colour.

Figure 2 caption: I recommend using “averaging kernels” rather than “averaging kernel matrix”. The statement “Lines are matrix rows for individual vertical levels” is unnecessary.

Figure 4 caption: the “data” of “data assimilation of ... observations” is unnecessary.

Figure 5 and 6 captions: the first sentences of these captions is unnecessary.

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