Reviewer #1:

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.

We have reworked the abstract following the suggested changes.

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.

We appreciate the suggestion of adding letter labels to the plots but feel that the text labels are clear and prefer the cleanliness of the current graphics.

Specific comments

Page 33464, Line 4: "subsiding background influence" is a bit vague for the abstract, try to clarify what this means.

Changed to "…with subsiding air from the free troposphere."

Line 5: I suggest clarifying that you mean "surface measurement network".

Changed to "current network of surface measurements"

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".

So changed.

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?).

We have clarified that all these data are assimilated.

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.

This result is crucial for proper assimilation of the observations, particularly the surface measurements.

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.

We do not use one quantity for the monitoring capacity of observing systems, so we cannot quantify this in one number so succinctly.

Page 33465, Line 2: I suggest clarifying that you mean “high-elevation measurement sites”.

So changed.

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 GOME-2 for almost 7.

Sentence modified: “This capability has been limited so far by the temporal sparseness of satellite data and low sensitivity to the surface.”

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.

In response to the above three comments we have modified the ordering of the introduction and added an earlier description of the different contributions to the North American background.

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.

An additional citation has been added (Lahoz et al., 2012) to supplement Fishman et al. (2012) on the limitations of LEO 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/

Citation added.

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.

We have eliminated repetition between the paragraphs.

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.

Removed.

Line 7-8: please clarify that the IASI instrument measures at TIR wavelengths and ozone is retrieved from these measurements.

Changed to: “IASI retrieves ozone in the thermal infrared (TIR)”

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 moving this to the concluding remarks.

We believe this is important for explaining our choice of characteristics for our synthetic LEO observations.

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

Changed to: “TEMPO and IASI-3 are both nadir viewing satellite instruments, with retrieval of vertical concentration profiles to be made by optimal estimation (Rodgers, 2000).”

Page 33471, Line 14: have the latitudes and longitudes of the North American domain been given earlier in the manuscript?

They have been, domain bounds now removed from this location.

also, I recommend using “averaging kernels” rather than “averaging kernel matrix” here and throughout the manuscript.

Averaging kernel matrix is the correct terminology.

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.

We believe it necessary to describe the current (IASI) and future (IASI-3) TIR instrumentation as the basis for the characteristics of the LEO instrument in our OSSE. We have added clarification to guide the reader earlier in this section: “We similarly generate synthetic LEO IASI-3 (henceforth LEO) observations…”

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?

We follow the notation used by Rodgers (2000).

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.

Now specified in section 2.1 (Simulation Models) for AM3, as it is described first. The ordering is consistent for all model resolutions described.

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.

Sentence clarified: "TEMPO will provide continuous daytime observation in the free troposphere as well as in the boundary layer, with separation between the two (Figure 2)."

Line 20: please check that this is the correct Lin et al. reference.

Corrected to (Lin et al., 2012a).

Line 20-21: it would be useful to also show the CASTNET time-series in comparison with the other lines on Figure 6.

This point is not located at a CASTNet site and thus there is no CASTNet data to be plotted.

Page 33477, Line 1-2: clarify what the bottom left plot of Figure 7 shows - is it synthetic TEMPO data?

Modified to read: "Synthetic satellite measurement imagery from TEMPO…"

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.

Clarified: "The LEO instrument will thus be valuable for tracking transpacific transport of ozone plumes even when TEMPO is operational."

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

We tried different colors for the area of interest but did not find any that made the figure easier to read; we have instead increased the thickness of the lines.

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.
We prefer the detail of the current language for readers less familiar with plots of averaging kernel matrices.

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

Removed.

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

We believe these descriptive sentences are useful for guiding the reader.

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.