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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observing requirements for geostationary satellites to enable ozone air quality prediction" *by* P. D. Hamer et al.

Anonymous Referee #2

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The paper of Hamer et al. discusses the ability of observations of NOx, CO, O3, and HCHO to constrain ozone and precursor emissions. The paper contains interesting material, but, to my opinion, it needs to be rewritten in a major way before it is acceptable for publication in ACP.

The main reason for this judgement is in fact the title of the paper, "Observing requirements for geostationary satellites to enable ozone air quality prediction". The authors present the work as an OSSE, which to my opinion is claiming much more than the authors bring.

An OSSE is normally based on a state-of-the-art 3D prognostic modelling system with data assimilation capabilities. In the OSSE one would study the added value of, in this case, geostationary satellite observations in improving the existing analyses, based

on e.g. surface observations. Such a study is based on synthetic observations which show geometry, error covariances and vertical observation kernels which are realistic for the future instruments. Such observations are ideally obtained from an independent but realistic model. The study should include a range of realistic situations, e.g. be conducted on a larger regional domain over longer time periods.

In contrast, the authors present simple box-model results focussing solely on the chemical Jacobians that relate concentrations to emissions, for one particular ozone pollution episode. Synthetic observations are obtained from the same model. Several assumptions are made, such as the time-independent emission hypothesis and the perfect model assumption. Subtleties of retrieval vertical sensitivity profiles and 3D distribution of trace gases in the atmosphere, and impact of clouds, aerosol, surface albedo on retrieval errors etc is missing. No attempt is made to provide realistic error bars on CO, NO2, HCHO and ozone observations. For instance the expected sensitivity of the satellite ozone observations in free troposphere versus boundary layer is very important but not discussed.

At several places in the paper the authors try to motivate (not very convincingly) the relevance of their work as a cheap OSSE for geostationary missions. From the paper I did not learn much about the real impact of Geostationary AQ measurements on state-of-the-art prognostic models and data assimilation systems.

Nevertheless, the core of the paper contains interesting results (following from the chemistry Jacobians) which can be published, and which have a relevance for future satellite missions. I would advise the authors to change the title, remove much of the text referring to the GEO missions, and rewrite the abstract, introduction and conclusions. An alternative title could be: "Importance of NOx, CO, O3, HCHO observations and diurnal sampling to constrain ozone and precursor emissions in air quality models".

The approach followed by the authors does not allow to constrain the diurnal profile of the emissions, which could be done by generalising the 4D-Var approach used. The authors refer to this possibility, which would provide relevant extra information. Please consider to extend the analysis in this way.

A few general observations:

- The introduction can be shortened when the results are no longer presented as an OSSE for geostationary observations.

- All of section 2: This section is long, and may be shortened considerably. Figure 6 (Jacobian) is a nice and useful plot, and is the major result in this section. I would personally reduce figures 2-5 to one single figure (e.g. figure 5 only).

- Please motivate the scenarios CN, OCN, and HCN, 9x NO based on the capabilities of the geostationary satellite instruments (GEOCAPE, SENTINEL, GEMS).

- An interesting special scenario would be only two observations at the most sensitive times, e.g. hours 9 and 15. Could a well-placed low Earth orbit mission provide part of the information?

Apart from the above comments, the paper is generally well written, clear and complete. But the text can be shortened without loosing information.

Specific comments:

Introduction:

Well written in general, with useful references. But focussing too much on (geostationary) satellites (see above).

p94, l15: "40x40" The footprint of GOME is larger

p 94: No references to the work of Hendrik Elbern and his group in Cologne, who set up a 4D-Var for a full regional AQ model.

p95, bottom: I miss a reference to the important (operational) GOME-2 mission on MetOp. It is good to distinguish GOME-2 from GOME.

Methodology:

p00: "the isoprene emission variability is parameterized to correlate to solar zenith angle offset by 2 h to consider both temperature and photon flux effects." I do not understand why an offset is applied - please explain or provide reference.

p01, l16: "Note then that all of the species emissions, Ei(t), share the same temporal variability." Why is this? VOC will have a different diurnal cycle than NO2 or CO.

p01, l9: "The emission inversion solves for, xi , the time independent emission scaling factors" Why this restriction? It is possible to set up a 4D-Var system which will solve also the temporal variability of the hourly emissions, and it would be very interesting to see if enough information is available in the data to constrain this diurnal profile. There seems to be an assumption that the true emission scaling factor is time independent and also applies to the forecast. In reality this will not be the case, and it is in fact an important question how much the analysed emissions are valid for the forecast period. This can only be answered by real observations (e.g. like Fig 16, see below).

p02, above 2.3: It is perhaps better to put this information in a table.

p02, l23: "Three observing scenarios" Please motivate these. Is this related to the payloads of the three future geostationary satellites?

p03, eq 5: replace "r.n." by a single symbol. Normally "\epsilon" is used for noise.

p05, top: Figures 2-5 provide similar information. For me only figure 5 would be sufficient.

p10, sec 2.5.3: Why is the emission (time) profile fixed? It seems to me that with e.g. 3-hourly or hourly observations one may extract information about this time dependence of the emissions.

Results:

I find the results section a bit long. The message can be conveyed with less words and less figures. In total 17 figures is a lot. Legends inside the plots may be removed (same text appears in figure caption).

p11,12: Figures 7, 8, 9, 11 are interesting, and the results are well discussed. The results could perhaps be combined into one single multi-panel plot.

p13: Figure 10. One could call this the HOCN scenario!? Separate plot not really needed for the paper: just mention that the combined scenario is not much better than OCN/HCN.

p11-13: How important is the perfect model assumption in the whole discussion? Please make a remark about this.

Sec 3.1.2: This is a different way of displaying but does not really contain new information.

Sec 3.1.3: Interesting, and one of the main results of the paper. Which part of the error reduction can be understood by simply the number of observations ? (The combined error of 24 uncorrelated observations is sqrt(24) smaller than the error of one observation) Are real observations really uncorrelated?

p16, l23: "lower sensitivity of CO and NO2" -> lower sensitivity of ozone ?

p17: The results in table 5 are very variable. Are there local minima, or is this related to the noise added to the observations?

p19, Fig 16: The authors use one example to argue relatively constant emissions. This is based on one station for one particular week, and is not very convincing therefore. I would prefer to see results for a large number of stations, averaged over longer periods.

sec 3.2.3: Why is this analysis limited to ethane and ethene? It would be nice to see a list of VOCs and the ability of the three scenarios to constrain them.

Discussion

p20,21: Several of the general findings are following "chemical common sense" and are not really new.

Sec 4.3: Following the general comments above: the chemical part of the impact study is OK, and I can believe these findings will be valid to a full 3D analysis system. However, the study is not a full OSSE since all other aspects (3D distribution; vertical sensitivity profile of the actual GEO observations; influence of other modelling aspects like deposition, transport, vertical mixing, lifetime; realistic model aspects; impact of surface observations compared to satellite data . . .) are not discussed, but will have a major impact on the OSSE results. This is not acknowledged by the authors.

p23, l4: It would be interesting if the study could be extended to a number of other (most relevant) VOC species (see sec 3.2.3).

p23, l22: These time-dependent scale factors are important, and I would very much like to see the results of such an analysis. This is very relevant to motivate the availability of multiple observations per day, as provided by GEO missions.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 19291, 2011.