**Atmospheric
Chemistry
and Physics
Discussions**

Interactive
Comment

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

Anonymous Referee #1

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Hamer et al. conducted a variety of analyses based on simplified OSSEs with the aim of supporting geostationary satellite measurements of air quality. OSSEs are based on a photochemical box model and the assimilation is done using a Lagrangian 4D-variational system. Surface emissions and ozone concentrations are optimized during the process. The authors tested different "observing" scenarios (CO+NO2, CO+NO2+O3, CO+NO2+HCHO) with different conditions of emissions (from NOx-limited to VOC-limited conditions) and different errors on the pseudo-observations. They analyzed the impact of adding O3 or HCHO observations on the forecasting capabilities of the model. They show the importance of the chemical regime on the efficiency of the constraint added the additional observation.

GENERAL COMMENTS

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I think that major revisions are necessary in order that this work can be suitable for ACP publication. I detail the reasons below.

1) Supporting studies for future satellite missions are very important and essential, but I do not think the study of Hamer et al. achieve this aim, at least as far as the manuscript's title and the aim of the paper as defined by the authors would suggest. No observation requirements for geostationary mission are really quantified and discuss in details in the manuscript. The conclusions of the study regarding this aspect are qualitative, superficial and not discuss enough to be convincing. The authors conduct a lot of idealized experiments and show results (that are intrinsically interesting) concerning the constraint bring by different species (NO2, O3, HCHO) depending the chemical regime. However, in my opinion, the experiments are not connected enough with the real satellite measurements capabilities/specifications to be able to provide quantified requirements for a future mission. Nowhere the authors based their experiments on expected capabilities/specifications of GEOCAPE concerning the noise on the different species for instance. Without doing that, they could have prescribed the range of acceptable characteristics for each species to achieve a good forecasting performance. This point is very briefly mentioned at the end of the conclusion but discussed nowhere else in the text. On the contrary, I had the feeling reading the paper that the authors were forcing open doors at some points and do not put sufficiently enough in light the novelty of their results compared to what one knows and expects. What I would suggest to the authors to improve the paper:

a) if they do not want or cannot go further in their study, they should change the title and the aim "supporting future mission" of the manuscript to something maybe less ambitious but which represents better the presented results. Actually, interesting results are shown in the manuscript concerning the chemical regimes for instance and are of interest for publication (if point 2 of the general comments is addressed).

b) If the authors want to keep the guideline of the paper and can make additional experiments, (i) I would recommend to take into account the capabilities/specifications

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of planned GEO missions for their pseudo-observations. For instance, if I well understood, in the study presented here, the different experiments are conducted with the same noise for all the species (except HCHO at some points). Is it realistic? I guess not. The authors could propose experiments varying the noise on each species independently for instance and prescribe the range of acceptable noise to achieve a good forecast; (ii) I would recommend to go to quantified conclusions and recommendations: which range of noise, observing frequency, etc is needed to achieve good forecasting performances (to be defined).

2) The results concerning the impact of the chemical regimes on the constraint choice and the observing time and frequency would be discussed in light of what one already knows and what we expect according to literature, chemical reactions, etc. I exaggerate but saying that HCHO better constrains COV emissions and that O3 measurements in the afternoon offer a better constraint seem obvious and we can draw this conclusion without performing all these experiments. The authors should highlight more which results were expected according to previous studies on chemical regimes for instance, and show what they bring new (quantification of the effect?; does the behavior of the different constraint was expected for the transition regime? Etc). This kind of discussion will strongly reinforce the paper.

3) In this study, the utility of including CO in the measurement system is not discussed. If CO is not measured, what is the impact on the AQ forecasting? This point should be added.

4) The authors made efforts to be pedagogical in their explanations but sometimes this leads to repetitions and weigh the text. For example, the part 2.5 could be merged with the results part to avoid repetitions (2.5.2 merged with 3.1 and 2.5.3 with 3.2). Another example of repetition is p19312 – lines 4-9: all this was already written just few lines before. Moreover, adding a table that summarizes all the experiments conducted with the conditions (noise, xNO, etc) and numbering these experiment would be helpful for the reader.

SPECIFIC COMMENTS

p19294 – l15: check the resolution of GOME. It should be 40*320 km and not 40*40 km.

p19294 – l19-20: "limited to fewer chemical species". The authors should mention that the main difficulties with satellite remote sensing measurements are the vertical resolution and the poor sensitivity in the boundary layer.

p19295 – l29: MIPAS does not really measure tropospheric ozone like the other mentioned instrument (only upper tropospheric can be measured).

p19296 – l20: the authors should add IASI in the list.

Section 2.3: the variable x should be distinguished in the notation from the true state, currently also noted x in the manuscript. It can be confusing for the reader. Authors might replace x be xt when referring to the true state.

p19303 – l10: what do the authors mean by the true ozone variability?

p19303 – l18: what are the specified errors of x and xa?

p19305 – l14: G is not represented on Fig. 4. Correct the text.

p19308 – l11: the DOF is the trace of A and not the determinant of A

p19314 – l5-10: this should typically be said when the experiments are defined earlier in the text.

p19316 – l22-24: the sentence should be reformulated. This is true for CO but not completely for NO2 (the link with the emissions is more direct in this case)

Figure 4: does the figure represent a schematic as the previous figures or does it present experiment results? If the figure displays experiment results, it could be interesting to add the forecast without assimilation. Note also that Tau3 is not defined.

Figure 5: I do not understand why the observations in the first 2 days are so close to

the true state. Is some noise added to the observations?

TECHNICAL CORRECTIONS Annotations on the figures are usually too small to be comfortably read.

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