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Discussions

Interactive comment on "The impact of observing characteristics on the ability to predict ozone under varying polluted photochemical regimes" by P. D. Hamer et al.

Anonymous Referee #2

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General: The paper by Hamer et al. is in general well written with a good introduction and careful formulation of the conclusions. The study is limited to the chemical processes that transform emissions to pollutant concentrations. Nevertheless, the sensitivity results and dependence on chemical regime are useful background information for understanding efforts that aim to assimilate satellite and surface observations for multiple species. I am in favour of publication. However, I have several minor comments which I propose the authors deal with in their paper before it is published.

Detailed comments:

Abstract: There are several formulations in the abstract which somewhat obscure the scientific content. Examples are "a variety of analyses"; "characteristics of "; "to support"; "various sets of"; "our principle method"; "which is the primary focus of this work"; "simple but key"; "our framework's ability"; "These questions are designed to examine"; "establish the robustness". The clarity of the abstract may be improved by removing several of these phrases.

Abstract: "complimentary"

Introduction: In general the introduction is well written. I found the content a bit too focussed on the USA, and the authors may consider to add 1-2 lines to balance this a bit more. A reference to MACC is missing, e.g. the recent GMDD paper by Marecal is relevant.

p4915, l7: "simplistic". This is a very negative word.

Scenarios: why do all scenarios include CO ? A scenario with O3, NO2 and HCHO would make sense to me, given the techniques to measure these compounds with satellites. Would that make any change to the ozone forecasts?

p4918, l2: "averaging kernel and DFS". Readers may associate "averaging kernels" with satellite retrievals. It is good to make clear that emission averaging kernels are meant here.

p4918, l14: What is a 1D box model. For me, a box model is 0D. If 1D, how many layers? Or does the 1D refer to time?

p4919, l1: Isoprene emissions and concentration: please give the reader an impression what this corresponds to (e.g. "typical concentrations for Summertime North-East USA, Summertime Southern California"?). Similar for the anthropogenic VOC emissions: is this typical for urbanised regions? (Is mentioned later, but good to mention it here as well)

p4919, l10: Again it is unclear what the "box" in the box model represents. Is it the

entire boundary layer?

p4919, l22: Can emissions be adjusted with an hourly time step, or longer (e.g. daily) ?

p4922, l1: remove subscript at end of the line.

p4922, eq 10: S_n^{-1}

p4923: I do not understand eq 11. Does "xt" mean "true state" ?

p4923: What does "x" mean in this case. Again, is this the "true" state? It seems "x" has a different meaning here as in eq.8 ?

p4924, l12: "characterise the uncertainties on x~ and q~". But I thought "x~' is the uncertainty. So, the sentence reads like "characterise the uncertainty of the uncertaity". Is this what is meant?

p4925, l8: "z = O3 …". Should this be "y = O3 …" ?

Caption fig 4: $q_Z(x,t)/dx_{NO}$ is repeated 3 times. What are the three colors?

Fig 9: lower is NO and upper is VOC ?!

p 4933, top: For Fig 11 it would be interesting to understand if the error reduction is due to the diurnal sampling, or to the reduction of the noise. More observations (n) effectively implies a 1/sqrt(n) decrease of the error. Would the same reduction be obtained if all observations were taken on the same hour? Figure 10 shown that the time of observation is crucial. How does this relate to fig.11 ? For instance: for a sampling distance of 12h, what are these two hours?

Table 3, 4, 5: what is the unit of the numbers presented?

3.2.2. Table 6 not easy to understand. What does "ozone prediction error - standard true state" mean? Error-minus-state does not make sense.

p4936, top: I do not understand the message behind the comparison in Fig. 12. Evi-

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dently there is a clear weekly cycle. However, on top of that there is the full day-to-day variability of weather-related processes and emission variability which complicate real-life comparisons as compared to the simplified box model approach. In fact, for me Fig 12 is not really useful for this study and may be removed.

p4937, l27: "demonstrated our framework's relevance" I do not understand what is meant by demonstrated here. Clearly many issues, such as the various modelling uncertainties, role of vertical distribution, as well as the ground and satellite observation characteristics (kernels, representativity) are not discussed.

p4942, l 14-15: Apart from future 4D-Var, do the authors think that (ensemble) Kalman filter approaches could deliver similar results?

p4943, l2: "are the first to demonstrate this novel approach" Is this true? For instance, Miyazaki et al. (doi 10.5194/acp-12-9545-2012) assimilate ozone and NO2, and the system adjusts the emissions.

p4944, l14: "non of the current generation of LEO satellites possesses a reliable means of attaining instrument sensitivity to the boundary layer for these gases." Is this true? In particular in the UV and SWIR spectral ranges there is sensitivity to the ground, and the signals measured with LEO instruments show a clear signal in NO2 and HCHO (in fact also CO) originating from the BL.

p4945, l6: Perhaps good to mention the night-time mixing (of ozone) between the boundary layer and free troposphere.