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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "A regional CO_2 observing system simulation experiment for the ASCENDS Satellite Mission" *by* J. S. Wang et al.

Anonymous Referee #1

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1 General comments

Overall a good paper, well structured and written, which proposes a useful analysis of the constrains brought by future remote-sensing CO2 measurements with high spatiotemporal resolution on regional-scale CO2 fluxes. It also presents an interesting discussion about the target and threshold requirements to answer key carbon cycle questions (Section 4). One concern is that while the authors point out the limitations of the method used, and in particular the impact of assuming perfect boundary conditions or incorrect prior error statistics, they do not try to assess (at least partially) the sensitivity of their results to those assumptions. This would be especially interesting here since the high spatiotemporal density of the ASCENDS observations could

actually result in the inversion being only weakly sensitive to the prior information. In addition, the explanations for the difference in posterior errors obtained with the global inversion and the regional one are not always well explained. This question of inversion technique, while interesting, is somewhat tangential to the main question of instrument design, and seems to raise more questions than answers, so the authors might consider removing section 3.2 and saving the topic for more complete treatment at a later date.

2 Detailed comments

- p. 12823, l. 6-8: I don't follow this explanation. In both the Eulerian and the Lagrangian simulations interpolated meteorological fields are used. The ability of the Lagrangian model to better simulate filamentation processes compared to the Eulerian one stems from the strong diffusion/dilution effects when using Eulerian simulations with coarse resolution.
- p. 12822, l. 18: Also, Deng et al., ACP, 2014.
- p. 12823, l. 12-16: Articles from Brioude et al. (2011, 2012) (maybe some others from the same author) should be cited here.
- p. 12824, l. 14:"...uncertainty levels in constraining the fluxes that ASCENDS observations..."
- p. 12826, l. 10-11: It is not very clear what "...the measurements errors at each location are scaled to two possible performance levels: 0.5 ppm and 1.0 ppm error..." means. Do you use only those constant error values in this study (with differences only due to the number of observations within each pixel)? It seems like from the reading of the next sections, but it should be better clarified here.

- p. 12830, l. 16: Not clear over what the average is done here.
- Section 3.1: I think this section should be simplified a bit. The posterior error reduction always results from the combined effects of the observation sensitivities (Jacobian), observational errors, and prior errors. Here the authors focus on describing the relative contribution of each of them to explain the uncertainty patterns observed. I would rather put more emphasis on the implication of the error reduction spatial distributions in term of constraints on specific CO2 sources/sinks sectors for instance.
- p. 12831, l.6: The recent satellite-based regional CH4 inversion by Wecht et al. (JGR, 2014) discusses and treats the issue of boundary conditions explicitly. This aspect is a critical factor in the derivation of regional constraints for CH4, and thus one must assume that it is an even greater factor for CO2. That the issue is only raised here as part of the discussion of uncertainty in 4.2, but not factored into the actual results, is of considerable concern. At the very least, this potentially large limitation should be mentioned in the abstract to qualify the estimated inversion performance.
- p. 12833, l. 27-28: I don't agree with this statement: " The reason for this is that longer a priori error correlation lengths result in fewer "unknowns" to be constrained by the observations". Longer error correlations essentially better transfer the observational information throughout the control vector elements (the fluxes here), which results in stronger constraints for each flux in average. Although it mechanically results in fewer "unknowns" to be solved for, saying the latter is the cause for the larger uncertainty reduction is confusing I think.
- p. 12834, l.18: Please specify what model is used here.
- p. 12834, l.26-28: Are you using the method described in Chevallier et al. (2012) (Appendix B)? If yes, please explicitly refer to this paper.

- p. 12835, l.10: It would be good to explain what is the basic principle of this (estimate-truth) statistics methodology.
- p. 12835, l.12: "in results"<–>"in error reductions"
- p. 12835, l. 11-28: The explanations given for the higher error reductions obtained with the global inversion compared to the regional one are not clear. Are the models/meteorological fields used in both simulations the same (could have a great impact)? How much might the different means of calculating (Lagrangian) versus estimating (variational) the uncertainties play a role? Assuming the same model is used, and that only the resolution is different from the two inversions, the only scale-dependent errors I can see are the aggregation errors (the authors should cite and refer to Bocquet et al. (2011) here for the definition of this concept). Assuming the observation information is the same (i.e. same errors), an increase in uncertainty reduction could happen if the aggregated prior errors are higher than those at fine resolution for instance. I think the authors need to substantially expand upon their explanations here, or consider removing this section.
- p. 12836, l. 15 -17: Not necessary.
- p. 12837, l.23: "... the comparison is not totally consistent..."
- p. 12838, l.23-end: That's a good point. However, it would be useful to quantify explicitly the relative contribution of the observational information to the meeting of the target requirement (i.e. where is the prior error already very close to the target level?). A map showing this relative contribution might be useful here.
- Section 4.2: Given the high spatiotemporal density of the ASCENDS data, it would be interesting to assess how much the uncertainty reduction depend on the prior errors , which are often incorrectly specified. I think it is a key question

in general for such inversions to understand how much we depend on our prior information.

- p. 12842, l. 7: for all wavelengths?
- p. 12842, l.11-12: "... it has fever unknowns to be solved for...". Again, this argument is not clear.
- p. 12842, l.24-28: Although this could be left for future investigations, I think testing at least 2 different sets of boundary conditions as well as two different prior error scenarios would strengthen this study.
- Figure 3: What is F here? One could think F is the flux and therefore σ_F/F unitless. Please clarify.

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