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3, S2211–S2216, 2003

Interactive Comment

Interactive comment on “Inverse modeling of CO_2 sources and sinks using satellite data: A synthetic inter-comparison of measurement techniques and their performance as a function of space and time” *by* S. Houweling et al.

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Before starting this review, I would like to point out that I have also written a short comment to accompany this paper in ACPD. The comment addresses more what this paper did *n*ot do rather than what it did. After several attempts to include that material in a review it seemed, finally, that this was not fair to the authors; if all papers were to be judged by what they did not include journals would be much thinner. Overall, I feel the paper makes an incremental but worthwhile contribution to the analysis of the theoretical utility of satellite CO_2 measurements. I suggest it be published although I hope it forms the first part of a larger study.

This paper forms the latest in a growing body of work attempting to establish the utility (or otherwise) of satellite measurements of CO_2 mixing ratio. With one exception these papers have come to roughly the same conclusion: Such measurements will be useful with precision requirements which are difficult but achievable *provided* a horde of systematic errors can be kept under control. The paper goes further to consider the relative utility of idealized versions of various proposed instruments. It considers the impact of the different atmospheric sampling behaviour of the different instruments on uncertainty reduction under a range of inverse set-ups. This is interesting enough but I don't quite see the practical need for it. Two of the instruments simulated in the study (AIRS and Sciamachy) are already flying and they are contrasted with a purpose-built instrument (OCO slated for launch in 2007. In a practical sense, ranking the relative utility of AIRS and SCIAMACHY will probably have little impact on the direction of research. There are already programs in place to retrieve CO_2 data from the spectra measured by both instruments and this would be the case independent of the results of this study. The finding, therefore, that OCO was the most useful of the three measurement approaches is, in my view, the most important outcome of the study. The finding occurs despite what I think is a relatively harsh treatment of OCO's capability. One would hope that a purpose-built CO_2 instrument would achieve more precise measurements than instruments not designed for this measurement. The results are quite sensitive to the magnitude of the data error used in the Bayesian inversion since, in this data rich regime, predicted uncertainties scale almost directly with the data uncertainty. How different would the relative performance of the instruments look if the authors used the 1 ppmv error sought by the designers of the OCO instrument? The difference is suppressed somewhat by the use of the 1 ppmv noise floor in the calculations.

Another difficulty with the instrument comparison is the chosen sampling strategy. One of the strengths of satellite measurements, it would appear, is their ability to sample synoptic structures in the concentration field and use these for the spatial location of sources. This is the reason quoted by Rayner et al. (2002) for much of the increased performance of that inversion cf the original Rayner and O'Brien (2001) study. The

ACPD

3, S2211–S2216, 2003

Interactive Comment

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Interactive Discussion

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forced conformance of all the sampling densities to weekly and $8\times10^\circ$ measurements probably eliminates some important differences between the instruments. In particular, when considering biases, the ability of the thermal IR technique to measure in day and night probably helps avoid biases associated with partial sampling of the diurnal cycle.

Some of the insights gained in carrying out the work for the paper are significant beyond the satellite problem. For example, the realization that equally weighted rather than area weighted uncertainties produce a fairer comparison of inversion behaviour at different latitudes is something I hadn't noticed. Similarly the seasonal differences in response of column-integrated CO_2 to a given source puts something of a bound on the impact of horizontal advection on rectification, a problem which has been discussed in transport model circles.

Specific Comments

I thank the authors for making a LATeX copy of the manuscript available to me. I will use the section labels and some quoted text to identify the context for my comments.

sect:inversion The Bayesian synthesis inversion procedure is described pretty much from scratch. Given that this is not a point of departure for the paper, i.e the authors aren't doing anything different from the rest of us here, I wonder whether this is really necessary.

sect:inversion I agree with the authors that, since they only consider uncertainties, they do not need to explain the calculation of the prior fluxes themselves. Unlike the authors this would lead me to delete this section since I believe the structure of the uncertainties can be explained without that for the fluxes themselves. I also think some indicative numbers on the prior uncertainties should be given, preferably in flux density units.

ACPD

3, S2211–S2216, 2003

Interactive Comment

sect:measure In the instrument section we again need indicative numbers showing the combined impact of the measurement density and precision on the weekly ensemble error statistics. Do these always meet the 1 ppmv minimum criterion?

sect:measure "It may seem that the vertical weighting functions for the near IR (SCIAMACHY and OCO) and thermal IR (AIRS) cannot be compared since they do not represent the same thing." I don't understand this paragraph. We don't compare weighting functions, they are just part of a transfer function from fluxes to measurements. If it's merely a matter of plotting them on a comparable scale, I think it would be ok to divide them both by their column-integrated weighting function. That is we can address the question of the vertical variation without considering the overall magnitude.

sect:measure I wonder if one of the opportunities this paper affords is to study improvements in surface measurements as well as the satellite measurements. We assume tremendous steps in technology for the satellite business but leave the surface network stuck in the same monthly mean sparse measurement pattern it's always been. I'm betting that by the time OCO flies, at least, many of these sites will have, at least, weekly measurements, either by augmentation of the flask network or new technology in surface measurement. How much difference will this make for the comparison? There's another reason for doing the comparison against weekly measurements too, one can then separate the impact of increased spatial and temporal sampling.

sect:maps "Next we will focus in more detail on the differences between the different satellite instruments." ... there follows an explanation of why the authors use the EWUC scenario. This is a very good point but I didn't find the explanation easy to grasp. I don't know if this is better but let me suggest the following: Weighting the uncertainties by area is tantamount to expressing the problem in flux units. Since the concentration data is in units of ppm this weighting has the effect of

ACPD
3, S2211–S2216, 2003

making the concentrations more sensitive to a unit change in fluxes at low latitudes than high (since low latitude grid cells are larger). Because the uncertainty reduction depends on this sensitivity, low latitude fluxes will incur a greater reduction in uncertainty for this rather trivial reason. We wish to isolate the impacts of atmospheric sampling and transport so use equally weighted uncertainties to eliminate this artifact.

sect:scales The relationship between uncertainty reduction and spatial correlation is, I think, pretty clear. Imagine a case where there is a large spatial correlation across, say a subcontinental region. This means that any measurement taken on that region will reduce the uncertainty for all of it. This will happen before any aggregation so there is no work left for the aggregation to do. Recall that there is some debate within the inversion community whether to use spatial correlations on the prior or aggregation after the inversion to increase the spatial reach of measurements.

sect:discuss In fact, I think combining measurements with different calibration is an important research topic. Provided the calibration offsets are included in the space of unknowns quite a bit can probably be done, at the expense of some loss of certainty. Single differences in calibration aren't that difficult except when they happen to coincide exactly with quantities we're interested in. for example, if the land-ocean partition is still a major focus (a questionable idea) then having to solve for a calibration offset between the predominantly marine NOAA-CMDL network and the continental measurements of Sciamachy may be a real problem.

sect:discuss I don't believe that the description in terms of the model resolution matrix rather than the posterior covariance adds any clarity? There will also be significant off-diagonal elements in the posterior covariance which will generate uncertainty cancellation as pixels are aggregated. I find this a clearer reason than explanations in terms of the MRM.

sect:discuss In the same paragraph there is an expressed concern about not exploiting the full information content of the observations with their binning to large grid-cells. I think this problem is much more severe in the time domain. (See earlier explanation). I think the authors should note this.

ACPD

3, S2211–S2216, 2003

Interactive Comment