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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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This comment started out to be a review of this paper. I realized, on completing it, that I was focused more on the state of the science in this area than any particular failings of the paper itself. So it seemed better to divide the responses into those normally considered part of a review and this more general comment.

The 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. We have, thus far, shied away from the obvious question this raises. This can be cast approximately as “what are the nature of the systematic errors expected from such measurements and how do they impact inversion studies?” To address this we must consider not only the uncertainties returned by inversion calculations but the biases, that is the difference between sources produced by an inversion and the sources which generated the concentration field.

The problem of systematic errors is not just difficult to do, since it requires very close collaboration between two hitherto separate disciplines (remote sensing and atmospheric tracer inversion) but it is even difficult to formulate. In Rayner et al. (2002) we noticed that a particular systematic offset in the observations (biasing all land observations by 0.1 ppmv) had little impact on a root-mean-square measure of bias but a large impact on biasing the land-ocean partition. So whether this measurement bias was thought to be serious depended on the question one considered.

The approach we took in Rayner et al. (2002) was aimed more at highlighting the problem than a detailed investigation. This comes, partly, from the fact that O’Brien and Rayner (2002) did not show systematic biases in the simulated radiative retrieval. So while we could use the scatter of those retrievals as a guide to the error we should put on individual data, more subtle error characteristics were not available to us. The bias impact experiment surprised us but I now think I understand a little more of the underlying statistical behaviour. The problem arises, I think, whenever the statistical model we use for the data error and the behaviour of that error in reality differ too greatly. The behaviour of the error of sample means with sample size (under the assumption of independent errors) means this discrepancy looks rather different for the monthly mean over a given region than for the annual mean, say, over all land regions. For the control case in Rayner et al. (2002) we can sketch this roughly as follows:

Our error model is of independent errors with standard deviation 1.8 ppmv. There are approximately 234,000 pseudo-observations of which about 25% will occur over land.

We bias each of these by 0.1 ppmv. By the usual formula for the standard error of the mean, our error model implies we have confidence in the mean concentration over land with a standard deviation of $1.8/\sqrt{0.25*234,000} \approx 0.0074$. This is about a factor of 15 smaller than the bias we applied so it is reasonable that the estimated sources are seriously affected. For the root-mean-square bias of each monthly source the case is different. If we assume that the observations with most impact on the source estimate are those occurring inside the region we can repeat this calculation. We have 116 regions and 12 months thus 1392 source components to estimate. The 234,000 observations translates to an average of approximately 169 observations in each source region for each month. Again using the standard error of the mean, our error model suggests we can determine the concentration in such a region to $1.8/\sqrt{169} \approx 0.14$ ppmv. This is larger than the 0.1 ppmv bias we applied so the lack of impact is also statistically consistent.

The above trivial calculations are appealing but hardly conclusive. For example they do not consider the fact that not all points were affected by the bias or that the inversion is more affected by spatial gradients than by absolute biases. More sophisticated experiments with the impact of error models require us to really perform the inversion and check our reasoning. Unfortunately the set-up in Rayner et al. (2002) was ill-suited to such an investigation. A prerequisite is that we have a baseline calculation with a low bias. We could not achieve this for two reasons. Firstly the advection algorithm in the transport model we used (CRC-MATCH) is nonlinear. Thus, the linear green's function or matrix representation of the transport model is approximate. We cannot, then, expect to recover the original sources even if we obtained a perfect match to the pseudo-observations. Furthermore, although the 116 regions in the inversion is higher than most synthesis inversion studies, it is still insufficient to capture the structure of the source fields we wish to retrieve. The $8\times10^{\circ}$ resolution and linear advection scheme means the set-up used by Houweling et al. is ideally suited to generate an unbiased control and hence a set of meaningful tests of error models and systematic errors. In the best of cases these tests would include propagation of likely systematic effects

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from the radiative retrieval or instrument simulator through to their impacts on flux inversions. In the absence of the necessary collaborators, the tests can sketch a series of responses to possible systematic errors. Although it was not included in Rayner et al. (2002) , an obvious solution for the appearance of a bias of all land measurements is to include an unknown offset into the problem to absorb it. The outcome is likely to be the removal of the bias at the cost of increased uncertainty on the flux components we are actually interested in. Is this a more or less effective strategy than including correlations on the data error (the alternative approach)? Can we build generally robust error models that are resistant to likely biases but still return flux uncertainties small enough to be interesting? What are reasonable cross-validation strategies against the existing and planned well-calibrated surface networks?

These are, I think, the next set of important questions for tracer inverse specialists considering the satellite CO_2 problem. The authors have an ideal apparatus to consider such problems so I hope they can be persuaded to lead the way.

Two topics that arose in my review of the paper are immediately amenable to the kind of bias analysis I have outlined.

In the discussion section the authors comment on problems of calibration between surface and satellite measurements. The impact of these could be examined easily by biasing some of the pseudo-observations and either solving for or neglecting an offset representing possible calibration differences. See Law et al. (2003) for an example.

Similarly, the impact of errors in vertical transport can be mimicked, to some degree, by introducing differences in the weighting functions used to calculate the pseudo-observations and the response functions.

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