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Atmospheric Chemistry and Physics Discussions

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 paper presents a comparison of error reduction in atmospheric CO2 inversions assuming three different satellite measurement systems: a thermal IR instrument represented by AIRS, a near IR instrument represented by SCIAMACHY, and a near IR with sun-glint utilization represented by the proposed OCO project. The authors examine the uncertainty reduction in retrieved carbon fluxes across a variety of different spatial and temporal scales and compare these results to an 89 station network of surface measurements represented by the current flask CO2 monitoring system. The smallest scale for which fluxes are returned are determined by the grid of the trans-

port model employed: 8x10 degrees. The methodology appears sound to the stated purpose and clearly explained.

My first question concerns the specification of the measurement precision. This is mainly a request for clarification: you indicate that an error of 3.6 ppm is applied to individual measurements. The ensemble has a minimum of 1 ppm. If 15 measurements typically make up an ensemble, are you not most times at this minimum (3.6/sqrt(15))? You indicate that the 1.0 minimum represents an approximation of systematic errors. But if it goes into the measurement error as stated in equation 3, it is a random error.

Figure 3: Is there a reason why the SCIAMACHY and AIRS(U) instruments are not included in this figure?

In the discussion concerning figure 2, the authors allude to the fact that the patterns of fractional change in flux uncertainty are somewhat different when one examines particular months rather than the change in uncertainty per year. Though I do understand that the number of figures has to be kept to a reasonable level, an examination of the information in figure 2 and 3 but for a summer versus winter months may be important in comparing the instrument performance. I would imagine that the instruments that have a vertical weighting function with some emphasis towards the surface will outperform in Winter, but not have a particular advantage during summer.

In relation to my last comment, Figures 5 and 6 show sensitivity to the temporal scale but I am assuming the fractional change in error is represented as an integral or an average of the year? Perhaps some clarification of the units for the lines in these figures would help.

Figure 7: I assume that this figure reflects the PW/SC case? Do the crossovers change when the evenly-weighted priors are used? More broadly, I think that the vertical axis in Figure 7 should be something other than the fractional change in the error. In particular, I think physical units such as the aggregated error in mass per unit area per unit time of some sort allow one to then compare the satellite error to something like a flux tower

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or other surface measurement. After all, just doing a better job than the existing flask network is not the most compelling goal. Though I don?t expect the authors to do this, I have always preferred adding the satellite systems to an existing flask network as a way to show these type of results. One can assume that the intercalibration work will be done, something I know those working on the OCO project are planning on devoting a lot of effort towards.

Abstract, line starting "Globally, rather challenging..." Would strike the word "rather"

Discussion section, 1st paragraph, sentence starting "However, our analyses also indicate..." Would use "required" rather than "requirement"

Last para of the discussion section, sentence starting "This may have important implications" would strike "in particular"

1st para of the Conclusions section, sentence starting "These performances are put in ..." would strike "with the performance" and replace with "to".

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 5237, 2003.