

Interactive comment on “High resolution modeling of CO₂ over Europe: implications for representation errors of satellite retrievals” by D. Pillai et al.

Anonymous Referee #3

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This manuscript investigates the representation errors that would arise from the application of small footprint satellite measurements in course resolution global inverse models by using the high-resolution meso-scale model WRF. It addresses one of several issues that need to be investigated before we can take optimal advantage of satellite CO₂ measurements. Overall, this contribution is original and useful. I see no reason to uphold publication, provided that the authors deal with a few issues that are listed below, and which should call for minor revisions only.

GENERAL COMMENTS

Somewhere it should be explained how the bias is defined. The way I understand it

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what is used is the within 1x1 grid variability of the monthly mean concentration. I see no way in which this measure of bias could separate between true bias and random error of the monthly averaged concentration. Certainly in cases when the monthly mean is only composed of a few cloud free measurements it is difficult to speak of a bias. This should be explained better. The easiest way to solve it would be to not speak of bias at all, but e.g. of averaged versus instantaneous measurements.

Regarding the linear correction of representation error that is being proposed, it is understandable that by fitting a few potential drivers to the data you may explain a substantial fraction of the variance. The question is how robust this fit is to the spatio-temporal distribution of the specific set of samples that was used. Clearly this step should precede any application. Within the context of this study it would be unfair to require extending the analysis to other regions, but I think an attempt in the direction of robustness testing should nevertheless be made (right know not even the uncertainty of the fitted coefficients is provided).

SPECIFIC COMMENTS

Section 3.2: It is not quite clear how the A-SCOPE measurements are defined. The 3.5 km samples were aggregated to 100 km. Then it is not clear what is meant by the 10 km samples that are mentioned. Also, since there is no reference, it is unclear if this sampling approach corresponds to what has been proposed for A-SCOPE.

Page 8: ‘These dominances . . . (not shown). It is straightforward to understand why the boundary layer variability is correlated with the variability in the total column. In the case of synoptic events, however, I would have expected the signal along the front to penetrate higher up into the atmosphere. With that in mind it doesn’t come natural that these events are ‘not strongly correlated to concentrations in the free troposphere around 4 Km’? In my opinion a clarification is needed here to help the reader.

Page 9: “mean mixing ratio near the surface” It is understandable that the representation error is related to the deviation of the concentration from the background, indicative

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of regional sources. It is not clear why the mean mixing ratio is taken as a parameter instead of something like “the absolute deviation from the background”. The latter would have the advantage that it accounts for regional sources and sinks. What is the motivation for using the mean mixing ratio?

Page 10: ‘Such a simple parameterization would likely reduce the impact of representation errors significantly’ In my opinion the authors do not provide a recipe for how to account for representation biases. Formally, the Bayesian framework cannot deal with biased random variables. Somehow the bias needs to be corrected for, which is much more complicated than adding a (random) contribution to the data covariance matrix. Therefore, if biases are important, it is not clear if the above-cited sentence is true. Some further clarification is needed here.

TECHNICAL COMMENTS

Table 5: Units are needed for ‘Resolution’ and ‘Intercept’.

Figure 7: Labels on the Y-axis are missing.

The text should be thoroughly checked for English spelling and grammar.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 20599, 2009.

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