**Atmospheric
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and Physics
Discussions**

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Comment

Interactive comment on "High resolution modeling of CO$_2$ over Europe: implications for representation errors of satellite retrievals" *by* D. Pillai et al.

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We would like to thank anonymous referee for his comments. Authors' responses to these comments are as follows:

GENERAL COMMENTS (1) Somewhere it should be explained how the bias is defined. The way I understand it 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

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solve it would be to not speak of bias at all, but e.g. of averaged versus instantaneous measurements.

Response: We modified. See Page 6: (1st and 2nd Paragraph). Also please see our responses to F.M Breon and 2nd reviewer.

(2) 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 spatiotemporal 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).

Response: The standard error of coefficients (in curly brackets) is now included in Table5.

SPECIFIC COMMENTS (3) 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.

Response: We used algorithm provided by François-Marie Bréon to calculate the A-SCOPE track and to select cloud free pixels. The reference is included in the manuscript as "(F.M Bréon, Laboratoire des Sciences du Climat et de l'Environnement, Personal Communication)"

(4) 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

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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.

Response: Modified the statement by including correlation coefficients-"These dominances can be significant during synoptic scale events, where CO2 column variability is strongly correlated (squared correlation coefficient, R2= 0.37) to boundary layer concentrations (see Figure 1), but not strongly correlated (squared correlation coefficient, R2= 0.12) to concentrations in the free troposphere around 4 km (not shown)."

(5) 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 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?

Response: Unclear what is meant by "the absolute deviation from the background". We took what would be available from global models. Not clear how to get "the absolute deviation from the background" from a global model. If it is a global offset, this would be uniform, and would not add anything to the spatial structure of the representation error

(6) 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.

Response: Biases can be accommodated in a Bayesian framework by allowing for

corresponding temporal covariances.

Some further clarification is needed here.

TECHNICAL COMMENTS

(7) Table 5: Units are needed for 'Resolution' and 'Intercept'. Response: Added

(8) Figure 7: Labels on the Y-axes are missing.

Response: It is mentioned in the Figure description.

Please also note the Supplement to this comment.

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

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