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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Mesoscale inversion: first results from the CERES campaign with synthetic data" *by* T. Lauvaux et al.

Anonymous Referee #3

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This paper presents an inverse mesoscale modelling system to estimate the 4-day average CO2 flux at high spatial resolution (8 km) over a 300 km x 300 km domain in the South West of France, using high-frequency observations from ground-based and aircraft measurements during the CERES measurement campaign.

While the setup of this inverse modelling system is certainly an important achievement, the presented paper appears very preliminary.

In my opinion, the most severe deficiency of the paper is the neglecting of diurnal CO2 flux variations. However, diurnal variations of CO2 fluxes from the biosphere (assimilation during day vs. respiration during night) can be much larger than the daily average CO2 flux. Therefore, the retrieval of average CO2 fluxes requires the realistic

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modelling of diurnal variations of CO2 fluxes, and the realistic modelling of diurnal variations of the atmospheric boundary layer.

The approach presented in the paper may be valid for trace gases with small/neglibible diurnal variations of emissions (e.g. CH4, or SF6), but seems questionable for CO2.

The authors do not discuss at all this issue. They do not even describe the applied biosphere models or inventories to calculate the J matrix.

A further limitation of the paper is that only calculated flux error reductions are presented (equation 4), but not the inverted fluxes themselves. Will this be presented in a companion paper, or is this considered not feasible/reliable ? In principle all information should be available to solve equation (2). The paper would be much more convincing if it included a detailed comparison with observations. This would allow a much better assessment of model performance.

On page 10454 the authors discuss the weaker vertical mixing in the model compared to (meteorological ?) observations. How severe is this problem ?. Again, it would be very useful to see some tracer simulations and comparison with observations.

Furthermore, the general presentation of the paper should be further improved, in particular the text of sections 6, 7, and 8, which is partly cumbersome to read (e.g. page 10449, lines 8-12: "During the 26 May, as for the 27, a sea breeze starts around noon, affecting the Biscarosse tower (Fig. 2 10 (b)). This situation appears on the 27th whereas the Autan wind was dominating the two towers (Fig. 2a), i.e. a strong south eastern wind amplified by the valley between the Corbieres mountains and the Montagne Noire.".).

Some further comments:

Title "... synthetic data": As I understand, the authors do not apply any synthetic data (i.e. observations generated by an atmospheric model), but avoid the use of observational data and use only their estimated uncertainty.

ACPD

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page 10446, lines 17-18: why is the maximum number of observations 8140 x 102 x 2 + 8140 x 2040 x 10 ? I do not understand the factor 8140 here (which is the number of optimized surface fluxes + boundary conditions)

page 10447, line 18: the applied observation error of 4 ppm is of course crucial for the derived error reduction. Estimating the observations error (which should include the model representativeness error) from the comparison with aircraft data may significantly underestimate the potential model representativeness error for the surface measurements, in particular during night.

page 10452 ("virtual Bicarosse tower of 300m"): I find the discussion of this experiment very poor. What are the concrete conclusions from this experiment ? What would be the optimal measurement height for the use in the inversion system ?

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 10439, 2007.