

Interactive comment on “CO₂ budgeting at the regional scale using a Lagrangian experimental strategy and meso-scale modeling” by C. Sarrat et al.

Anonymous Referee #3

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This paper by Sarrat et al. describes the observations and analyses of a CO₂ experiment in France from one day in June 2005. Three different methods to derive the carbon budget of the region are compared.

The paper is of average quality and makes use of established modelling tools and methodology. The language and presentation is more-or-less clear. However, other than presenting new observations from a single day, the study lacks novel aspects and fails to derive implications for the wider community. The uncertainties involved in the three different methods are also not discussed well, neglecting potential errors in the Eulerian and bottom-up methods.

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Hence if the study is to be published, I suggest the following major revisions: (1) Greatly expand the discussion of uncertainties in the three carbon budgeting methods. The authors appear to place confidence in the Eulerian model budget and the bottom-up method primarily because they agreed with one another. This agreement should not preclude a discussion of their uncertainties. For instance, biospheric models may miss biospheric processes significant for carbon fluxes, the bottom-up scaling requires that a few eddy covariance flux measurements are representative of the entire land type. Also, could it be that the Eulerian and bottom-up methods agreed better because the biospheric model has already been fitted to the eddy covariance data, and the fluxes are scaled up over the landscape using the exact same land database?

I am by no means suggesting that the Lagrangian budgeting method is the best; it is just that all three methods are subject to uncertainties and ought to be mentioned. I urge the authors to try quantifying the uncertainties as well.

(2) Extend the Discussion/Conclusion sections with messages drawn from the study that are of interest to the wider carbon science community. In other words: given the study's results, what is its greater significance? I can see that the meso-scale model may have some capabilities to reproduce observations, and that the Lagrangian measurements may have missed sampling parts of the atmosphere, but neither seems to be particularly of significance.

(3) Clarify the relative roles of the Piper-Aztec and Dimona aircrafts. In Sect. 2.1 ("The Lagrangian Experiment"), the Dimona was not mentioned at all. What were the specific sampling strategies of the two aircrafts? I suggest showing the flight tracks of the two aircrafts on the same plot (e.g., Fig. 1b) so that the two aircrafts can be related to one another. Secondly, even though Sect. 2.1 mentioned the use of constant volume balloons, they were not brought up again in the rest of the paper. Why not show the balloons' trajectories in Fig. 1b?

(4) Deal with the discrepancy between the modeled versus measured CO₂ profiles at

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1000UTC over the Dimona sub-domain, as seen in Fig. 8 (left) versus Fig. 7a. The modelled CO₂ appears to be 2 ppm lower than the observed values in the ABL. Why is this the case? The manuscript does not mention this discrepancy.

Other comments are as follows: (1) From which altitude in the model is the simulated CO₂ concentration plotted in Fig. 5a taken from?

(2) I assume that the straight lines in Fig. 8 (right; the green, 1700 UTC curve sampled by Piper-Aztec) indicate missing data? If so, I suggest removing the straight lines; otherwise the reader may find them misleading.

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