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Biogeosciences Discussions

Interactive comment on "CO_2 budgeting at the regional scale using a Lagrangian experimental strategy and meso-scale modeling" *by* C. Sarrat et al.

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RECOMMENDATION This is a good paper and I recommend that it be published with minor changes as described below.

GENERAL The paper compares three techniques for estimating the regional scale CO2 flux over an area of mixed land use in south- western France. The techniques compared are results from a coupled meso-scale meteorological/land surface model, the area-weighted average of ground-based measurements and the results from two Lagrangian budget flights by separate aircraft. The results are for single day of the CERES program and so represent a test of the methods in only one set of conditions.

However, this reflects the difficulty of collecting the data required for such an analysis and the complexity of its analysis.

The paper concludes that the results from the two Eularian approaches (the coupled meso-scale/land surface model and the area-weighted average of ground-based measurements) are in close agreement but that the results from the aircraft Lagrangian budget flights differ significantly from these. The results from one of the aircraft Lagrangian budget flights (Dimona) is significantly higher than the Eulerian approaches, the results from the second aircraft flight (Piper Aztec) is significantly lower. The differences are attributed to differing integration times, uncertainties in the CO2 vertical profile measurements from the aircraft and non-stationary meteorological conditions during the budget flights.

SPECIFIC POINTS There are a number of specific points that, if addressed, would, in my opinion, significantly improve the paper. These are set out below.

1) LEAF AREA INDEX Section 3.1.2 mentions the LAI field used in the coupled meso-scale meteorological/land surface model but there is no description as to the source of this data. LAI is a fundamental driver of ecosystem gross primary production (GPP) and hence net ecosystem exchange (NEE). It would be very useful if the authors gave more details on the source of the LAI data used in the modeling. For example, did it come from a global data set, from MODIS imagery or from measurements (optical, harvest etc) at each site? This would help other researchers to decide on the best strategy for obtaining LAI data for their own work.

2) MODEL VERSUS OBSERVATION COMPARISON Section 3.1.2 and Figure 3 describe the very good agreement between the CO2 flux calculated by the coupled meso-scale meteorological/land surface model and the ground-based observations but no details are given as to the level of tuning required, particularly of the land surface model (LSM) to achieve the these results. It would be very useful to other researchers to know what parameters were estimated from site specific data and how this was done.

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For example, were generic values of the model parameters used or were they obtained by some parameter estimation technique using the observations? If the latter, was the parameter estimation technique used on a subset of the observations or on all of the observations? This is a problem faced by all researchers in this field and any guidance the authors can offer will be of help to many.

A related point in this context is agreement between modeled and measured sensible and latent heat fluxes. The authors comment that the agreement between these is weaker than for the CO2 flux but do not show any data or elaborate on the differences. Yet this is an important point as it directly relates to the correct simulation of the surface energy balance. It would help other researchers if the results for sensible and latent heat flux were also presented, even though the main thrust of the paper is CO2 fluxes. This would help others place the work presented in the context of their own studies. The current paper is not overly long and I think space could be devoted to this comparison.

The next point follows on from the previous one. With regard to Figure 3, it is not stated if the data presented is for a single day or averaged over several days. From the variability in the observed CO2 fluxes, it seems likely that the data refers to a single day but the period that applies to the data should be explicitly stated. In the context of estimating regional scale CO2 fluxes from ground-based measurements, it would be very useful if the authors could use the available data to estimate how many flux sites (equivalently what fraction of their total area needs to be sampled) are needed to provide reliable estimates of the regional scale CO2 flux. This would help other researchers to decide on an optimal sampling strategy for their own projects.

3) ENTRAINMENT FLUX I think that the treatment of the entrainment flux term in the budget techniques needs to be better developed. This would certainly address my greatest concern about the paper.

The entrainment flux is first mentioned at line 5 on page 2940 in a description of the results from the Meso-NH model. The authors assert that the entrainment flux is neg-

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ligible for the periods covering both the Dimona and Piper Aztec flights. I’ll use results from the Dimona flight but the same comments could be made about the Piper Aztec flight.

Figure 7 shows the modeled CO2 concentration profile at 1000 and 1700 UTC. During this time the CO2 concentration in the mixed layer is 1.5 to 2 ppm less than the CO2 concentration in the free troposphere above. During this time the boundary layer deepens from 1000m to 1250m. The CO2 concentration jump across zi and the growth of the boundary layer imply an entrainment flux exists. Even if the boundary layer height remains constant, there will still be an exchange of air between the CO2 depleted mixed layer and the comparatively CO2 rich air in the free troposphere. It should be possible to estimate the entrainment flux from the available observations and some assumptions regarding the mixing at the top of the boundary-layer. Giving this value in the paper would considerably strengthen the case for asserting that the entrainment flux is negligible.

A short description of how the coupled Meso-NH/LSM model simulates the entrainment flux would also be useful. For example, are the dynamics of the mixing at the top of the PBL solved for explicitly or are they parameterized?

Finally, in a paper in Boundary-layer Meteorology published in 2004, Cleugh et al give a very cogent appraisal of the importance of the entrainment term in Eularian budget techniques. In particular they discuss the size of the entrainment term they found necessary to get fluxes calculated using the CBL budget technique to agree with the surface observations. They also give a clear description of the difficulties caused by fact that the surface and entrainment fluxes are of the same sign for both H2O and CO2. This means that the change in mixed layer concentration over time is the difference of two terms, causing the uncertainties to compound. This is different to the case for sensible heat where the surface and entrainment fluxes are of opposite sign. Their paper may be of interest to the authors, if they are not already familiar with it.

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4) BUDGET TECHNIQUES The main focus of the paper is estimating regional scale CO2 fluxes and, to some extent, this dictates the results that are best presented. However, it would be extremely useful if the authors also compared results from the three techniques for sensible and latent heat fluxes. These results may even offer some insight as to why the CO2 fluxes estimated by the Lagrangian budget technique are significantly different from the two Eularian approaches. For example, are the sensible and latent heat fluxes obtained for the Dimona flight larger than the modeled and observed, as is the case with the CO2 flux? This would also broaden the appeal of the paper. For example, many hydrologists are searching for methods to estimate regional scale evapotranspiration.

5) CONCLUSIONS SECTION The authors have obviously done a great deal of work for this paper, in both the collection and analysis of the experimental and modeling data. I think that the conclusions section could be strengthened to make better use of this work.

The discussion of the errors and uncertainties associated with each technique could be enhanced and made into a separate section. This would help other researchers to assess which of the three techniques are best suited to their own applications. It would also help to place the differences in the results from the three techniques into context. For example, what is the uncertainty in the Lagrangian budget results? Does this explain the difference between the Lagrangian and Eulerian techniques?

Perhaps the main addition to the conclusion section would be some guidance as to which method of estimating regional scale CO2 flux is best for a given situation. As a result of the work undertaken for this paper, the authors are certainly in a strong position make such recommendations. A discussion of the uncertainties associated with each method would go a long way to answering this question.

6) EDITORIAL There are several minor editorial corrections that would make the paper easier to read. I would be happy to provide a list of these if the authors feel that they

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would be of use.

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