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Comment

Interactive comment on “Mesoscale modeling of the CO₂ interactions between the surface and the atmosphere applied to the April 2007 CERES field experiment” by C. Sarrat et al.

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The paper describes modelling of regional carbon fluxes and energy exchange using aircraft measurements for model parameterisation. It draws on a number of measurements that combine aircraft measurements, tower measurements, and suitable weather conditions. Capacity of the model to reproduce measurements is substantially improved after tuning with a number of measurement days. The analysis is useful, since the connection of ecosystem scale to regional scale surface-atmosphere exchange is still poorly understood. The paper deserves to be published but it requires major revisions before being acceptable.

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Abstract: this is very descriptive. A stand-alone abstract should (while still being relatively brief) include a study's chief results and conclusion.

Methods: please summarise the main features of ISBA-A-gs. The model is published and not each and every little detail needs to be reproduced. But the reader should be familiarised with its main aspects, particularly those that are most relevant to this study without having to read the referenced literature.

Fig. 2/pre and post calibration for winter crops: the calibration removes a minimum CO₂ east of Toulouse; after calibration a "hot spot" of CO₂ > 390ppm appears north of FAGR. I couldn't find this being discussed in great detail, what would be the reason?

The text on recalibration of soil respiration for the winter crop area must be clearer (page 522). You write that during the campaign, soil moisture was close to field capacity. The soil moisture module in ISBA-A-gs does not include a moisture-response of soil respiration; and as you discuss correctly, for this particular campaign this wouldn't have been an issue. But what is meant by "This was a possible reason for stimulating soil respiration" (line 20)? Are you trying to say here that the calibration which was done for a previous campaign under dry conditions didn't work and that the R25 had to be increased for the present simulation? The whole paragraph is a bit cumbersome. And what stopped you from introducing a simple soil moisture dependence of respiration in the first instance? There are a number of published parameterisations available that could be adopted. Finally, why do you use a relatively high value of 25°C as a reference temperature, for a soil temperature at 20cm depth?

Model results show good agreement with eddy latent and sensible heat fluxes, but daytime NEE is underestimated. One possibility would be the crude soil respiration parameterisation; if R25 is set too high, simulated net uptake (the difference between canopy assimilation and ecosystem respiration) would be too small. I am not so sure I agree with the notion that the model agrees better with observations at nighttime. To begin with, nighttime eddy fluxes can be very (unrealistically so) variable,

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seen possible at LAMA. Moreover, when expressed in relative terms the discrepancy between model and "average" (visual inspection) nighttime eddy fluxes to me does seem as large, if not larger, as during daytime. In fact it wasn't clear to me whether you used the same R25 value for each of the four ecosystems; there is no reason to assume that respiration rates (at same temperature) would be the same in these very different systems.

The discussion reads in places not as a discussion but as a summary of the previous result section (for instance, page 528, lines 11-23). I would have liked to see much more in-depth comments of why the model is capable to reproduce surface energy partitioning into latent and sensible heat fluxes fairly well, but clear discrepancies become obvious for CO₂ fluxes (although overall the performance is not at all hopeless for these as well). You mention the issue of spatial resolution, and topography, which are probably important aspects. But overall the discussion section stays too much on the surface. There is no information whether the model assigns different photosynthesis/conductance parameterisations to the various vegetation types (see also my question above regarding respiration rates in the different vegetation classes). Again, giving a bit more information in the method section, and taking some of it up in the discussion section could add to the paper substantially.

The manuscript needs a thorough proof-reading and correction of numerous errors. It is also in places wordy and paragraphs are phrased in awkward logic that is sometime difficult to follow. I list a few examples here, but these are really just examples, and the entire manuscript must be checked and revised carefully.

Abstract, line 2: space between "devoted" and "to"; line 4: "The four..": which 4 days?
Should read: four consecutive days

Check setting of brackets when citing literature

"of course" is typically not considered good style in manuscripts. "really very crude" is also spoken, rather than written English.

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Page 524, line 24 – you start two consecutive sentences with "Again", revise.

Fig. 1 replace "schematising" by "overview"

Fig. 2: explain abbreviations somewhere, not every reader of Biogeosciences will be familiar with the study area. And while TLS = Toulouse may be relatively straightforward to deduce I was left in limbo about other places (SMOS, LACS, etc). What are the boundaries indicated by the thin grey lines?

Fig. 5: you must use larger font size for axis labelling and numbers, the Figure is very difficult to read.

Figure 7 and 8: adjusts y-axis scale such that the figures are actually legible. For instance, there is no reason to have as minimum for the Bowen ratio scale minus (!) 4, and CO2 fluxes also never reach a value of plus 14.

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