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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Lagrangian transport modelling for CO_2 using two different biosphere models" *by* G. Pieterse et al.

H. Dolman (Referee)

han.dolman@geo.falw.vu.nl

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The paper addresses a number of issues, of which I take as the most important one the performance of the coupled FACEM-COMET model to back calculate sources and sinks from tall tower observation. The authors first try to establish the performance of the land surface model by comparing with Sib-3 which is a logical step. The finding that it is hard to extract more than the net signal (NEP) from the models is an important finding that could be more emphasized.

There are however a few fundamental problems with the paper that require significant effort before the paper would be acceptable.

The introduction paragraphs make a lot about the issue of complexity versus simplic-

ity, which are subsequently not very well followed up in the paper. I suggest either drastically shortening those parts in the introduction, or make a deliberate effort in the results and discussion section to clarify whether Sib or FACEM is better and why. Fundamentally is it a problem to determine” the crucial processes” (p4120 l 21), which would depend on the type and region of application . This apart from the philosophical issue whether it is uberhaupt possible to know everything. Certainly in current carbon cycle work, claiming that one knows all fundamental processes is not appropriate.

On the danger of being a reviewer that does not judge whether the authors have done a good job, and suggesting something complete different: I am concerned about the evaluation of the FACEM model with another model, at even lower resolution. Why do the authors not use site level fluxes and for instance fAPAR products to compare their model with. This would be much stronger than comparing with another model. The fact that different land use classes, different resolutions and different meteorological forcing (?) does not make this comparison easy, or straightforward. I have the strong feeling that the comparison with Sib is a remnant of an earlier study and I would suggest leaving it out altogether, and validate FACEM with fluxes and remotely sensed observation where possible. This is what they do partly in page 4125 line 20-25, and this is much more relevant.

The comparisons are made largely through the use of the correlation coefficients r2. It is nowhere stated whether an increase from say, 0.55 to 0.60 is significant. The authors should put some effort in quantifying the statistical difference and significance between their various runs.

Comparisons with COMET Cabauw. It should not come as a surprise that the Sib and FACEM models produce similar results, given the fact that the COMET trajectories are in the field where they agree.

Page 4132 The interaction between FACEM and the stable layer is something that I

ACPD

8, S1285–S1287, 2008

would really like to see expanded as this aspect is key to using the method to distinguish between GPP and Respiration.

I feel the finding that only the Net signal (NEP) can be retraced is very important and in fact disqualifies using SIB-3 that is constrained to have net balance.

The amount of graphs could be reduced. Particularly 4-6 are very hard to read, even on the screen.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 4117, 2008.