

Interactive comment on “Lagrangian transport modelling for CO₂ using two different biosphere models” by G. Pieterse et al.

G. Pieterse et al.

Received and published: 22 April 2008

Dear Prof. Dr. Han Dolman,

First of all I would like to thank you for your critical but constructive review. We will try to be as thorough as possible with the response to your comments and with the implementation of your suggestions into a new version of the manuscript, while also considering the input of the other referees. I have copied your comments into this document and added my responses.

With best regards, Gerben Pieterse.

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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

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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.

Response: I agree that it is an important finding. We address this finding in the final section of the discussion (pp 4132-4234) and try to assess possibilities to disentangle the different biospheric signals. We will extend the discussion in this respect.

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 simplicity, 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.

Response: The other referees had similar comments about this issue. We will add a section to clarify the intended application of the two models and clarify why complexity does not necessarily lead to improvement in accuracy. SiB is intended to provide the biosphere boundary conditions (carbon and heat fluxes) for past, present and future climate studies, whereas FACEM is primarily intended for provision of (proper) prior estimates for inverse modeling studies. Because of their different fields of application, we wanted to avoid a good versus bad discussion about the models focus here on the opportunities provided by the observed differences and their possible explanations for future improvements in the FACEM model.

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 überhaupt possible to know everything. Certainly in current carbon cycle work, claiming that one knows all fundamental processes is not appropriate.

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Response: It was not intended to portray our model as such and therefore we will try to rephrase accordingly. Indeed, the 'crucial processes' should be approached in terms such as 'candidate crucial processes'. But I do believe that there is a general scientific consensus about which processes should at least be included to reproduce the primary responses of the biosphere to the primary drivers. This is what I meant to say with 'the crucial processes';.

On the danger of being a reviewer that does not judge whether the authors have done a good job, and suggesting something completely 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.

Response: A thorough comparison of the FACEM model with site level measurements was presented in an earlier paper (Pieterse et al., 2007). I agree that part of the differences are due to different input parameters, but because both models contain many similar sub-parameterizations, such as the core of the photosynthesis schemes, we would end up comparing essentially the same schemes rather than the models in their normal setup of application. I think that it is very illustrative to show that the output of two different model frameworks can still be very different for essentially the same environmental conditions, showing the reader that there is still a lot to learn about the biosphere, and that we are still quite limited in describing the current response of the biosphere on even the main driving parameters, let alone describing it for future and past climate conditions.

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The comparisons are made largely through the use of the correlation coefficients r^2 . 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.

Response: I do not agree that the comparisons are made primarily based on the correlation. Throughout the paper, improvements in correlation are always shown in conjunction with two other important parameters; variability and bias. Also in my personal opinion, any correlation (R^2) less than 80% is statistically questionable and I think that any analysis of the strengths of relationships between variables should in principle also be accompanied with an assessment of statistical significance. Unfortunately, in this field of research we frequently have to content ourselves with the reality of much poorer correlations and to resort to qualitative statements about improvements in model performance. But I agree that we could inform the reader more explicitly about the fact that improvement should be assessed in terms of all three abovementioned parameters. In all comparisons not only R^2 , but also variability and bias are inter compared. In general differences in R^2 of the order of 5% are not significant considering the variances of the compared signals. The discussion will be extended on significance and significant differences will be marked in the relevant tables.

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

Response: These two comments basically address the current limitations of this (or should I say any?) transport model to represent the atmospheric conditions at different locations and this issue is addressed in the paper. We ran the COMET model for

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different lower limits for the planetary boundary layer (PBL) depth concluding that misrepresentation of the stability of the PBL depth can result in significant bias. One of the important conclusions stated in the final part of the paper (e.g. line 5, page 4133) is that, at present, transport modeling in conjunction with concentration measurements alone will not enable one to disentangle the different biosphere signals and that other measurement techniques will be required to provide additional information about the vast underdetermined system of unknowns we are trying to solve.

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.

Response: The SiB model is constrained to have net balance on an annual basis, based on the notion that the induced uncertainty is small compared to all other uncertainties involved (Denning et al., 1996). However, the timescales at which the different sub processes manifest themselves are quite distinguishable on the seasonal and diurnal time scales and the SiB model will still provide estimates for the magnitudes of the different fluxes to and from the biosphere. What our paper concludes is that it will be very hard to verify whether the magnitudes of the separate fluxes are correct at the hourly and diurnal timescale, for any biosphere model. The important message here is indeed that the total net flux can be extracted from the atmospheric signal.

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

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