

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 Sir/Madam,

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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In this paper, the authors investigate the performance of the newly developed biosphere model FACEM by (1) comparing the simulated fluxes to results of a more complex

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biosphere model and (2) coupling the fluxes from both biosphere models with the lagrangian transport model COMET in order to compare the resulting atmospheric CO₂ concentration with observations at selected stations.

The authors present a feasible strategy for an additional evaluation of a biosphere model, although a comparison with flux measurements would be the first option. Obviously, the evaluation based on flux data was already published in a preceding paper (Pieterse et al., 2007). Nevertheless, a short summary of the findings should be given in the introduction.

Response: We will include a short summary of the findings in that paper in the introduction.

The inter comparison of biosphere models - in terms of fluxes and concentrations - is used to identify model shortcomings relative to a more sophisticated, well-established model as benchmark and indicate potential needs for refinement or improvement. A quantitative evaluation of biospheric fluxes through a comparison of the resulting atmospheric CO₂ concentration with observations is, however, always limited by the quality of the transport model. A further limitation in this study is that only three measurement sites could be used due to restrictions in the applicability of the transport model.

Response: We will elaborate more on the limitations of this and other transport models.

The paper is clear and well written. Model experiment set-up and results are presented in a structured and quite clear way. Nevertheless, the presentation and discussion of the "Model evaluation" and "Conclusions" should be condensed by avoiding some redundant repetitions.

Response: This was also noted by the other referees. We will try to condense these sections into the essentials.

The number of figures might well be reduced (see specific comments) in order to concentrate on the most relevant findings. Not all of them are needed to provide new

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insights.

Response: We agree that some figures do not add new information but we chose to add all figures to show all data unbiased to enable the reader to assess the full results rather than a pre-selected subset. However, the other referees also noted that a number of figures do not add new information. Therefore, we will remove the figures that are not further mentioned in the text.

Specific comments:

Page 4120, line 5-15: Certainly there are good reasons to prefer simple biosphere models for the simulation of biosphere-atmosphere interaction. But without a deeper understanding of the processes the application of these kind of models for prediction of future climate - carbon cycle interaction are only limited. The authors should more clearly state whether FACEM should be used for diagnostic simulations of the biosphere under current climate conditions or should be also applicable in climate change studies.

Response: Yes, I agree that we should clarify that FACEM is intended for providing decent prior estimates for inverse model studies for present climate conditions.

Page 4120, line 23-24: While it is clear from Vermeulen et al. (2006) that the applicability of COMET is restricted to regions without complex terrain it is not obvious why this should be the case for FACEM. The authors should (at some point in the manuscript) clearly state what the specific limits of the applicability of FACEM are?

Response: Indeed, the restrictions with respect to the orographic features of the terrain are mostly applicable to the transport model. Some of the limits of applicability of FACEM are discussed in Section 4.1 but we should elaborate more about specific limitations for the FACEM model. The limitations of FACEM were also discussed previously by Pieterse et al. 2007.

Page 4121, line 8: The models rather simulate or

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’model’ the concentrations than ’predict’ the future...

Response: OK, we will alter the formulation

Page 4124, line 15: What is meant by ’spatial correlation’? This seems to be rather the spatial pattern of the temporal correlation. Are correlation and standard deviation based on 1-hourly values? If so, the good correlation in spring, summer and autumn might be largely caused by the correlation of the diurnal cycle. Otherwise, if this is already taken into account, the authors should mention it.

Response: The correlations are based on 1-hourly values and we agree that correlation is not the single most important parameter describing the model performance, because it is in general easier to obtain higher correlation for signals with higher variability, e.g. during the biologically most active seasons of the year. However, the variability and bias are shown along with the correlation plots and added together, they give a complete overview of the model performance. Also in the discussion, the performance of the models is always measured against these three parameters simultaneously. I believe that we have made it sufficiently clear that the performance of the models should be measured with these three parameters rather than with correlation alone.

Page 4125, line 26: The differences between FACEM and SIB2 are partly attributed to differences in input data. These assumptions should be tested in sensitivity studies using the same input data in both models, preferably use SIB input (land use and AVHRR-NDVI) for FACEM.

Response: I agree that some 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 and 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 still a lot to learn about the biosphere, and that we are still quite limited in

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describing the current response of the biosphere on even the main driving parameters, let alone describing it for future and past climate conditions.

Page 4125, line 12: The impact of differences in horizontal resolution should be discussed (and explained) here in more detail because Section 3.2.3 is focusing on the comparison with point measurements at Mace Head - which is a slightly different issue. The impact of spatial resolution should be further substantiated by running FACEM on the coarser SIB3 grid and comparing it to original resolution FACEM fluxes.

Response: We agree that there are several ways to further substantiate the model comparisons, but for this manuscript we decided to show a more general comparison and model performance compared with large datasets rather than showing a very detailed comparison for small subsets of data. This approach comes at the cost of some depth but in its current form, the volume of this manuscript is already quite substantial and we believe that adding more detail will render the paper less readable.

Page 4126, line 3: The questions/comments concerning statistics of GPP apply as well to NEP. Response: OK, we will note this in the article.

Page 4126, line 9: "...average NEP ... is larger by more than 80% ." From Fig. 3 this seems to be only in winter.

Response: Indeed this is the case, and that is what is mentioned in the paragraph following this sentence, where frost conditions are recognized as a possible cause for these overestimations.

Page 4126, line 15: "... net local sources ... in the order of 10% ..." 10% of what? What is the magnitude of the annual NEP in the FACEM results in areas not affected by missing snow or frost?

Response: We meant to say that the net source or sink in a certain region is in general only 10% of the strength of the sources of the sinks in that region. Details can be found the reference mentioned in the paragraph following this sentence (Denning et

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al., 1996).

Page 4127-4130: Switching between text, tables 3-5 and figures 4-6 it is difficult not to confuse all the different cases in the evaluation. Please consider to additionally assign self-explaining names to the cases instead of numbers only.

Response: OK, we will add more explanatory text.

Page 4127, line21: That statement " The anthropogenic... add relatively little to the variability..." is not supported by Fig. 4. The amplitude of the diurnal cycle including only anthropogenic and oceanic fluxes (red line) is comparable to the amplitude caused by anthropogenic+oceanic+GPP (green) and explains approx. 50% of the amplitude caused by all fluxes (blue).

Response: Yes, this finding is more a reflection of sentiment rather than observation. We expected an even larger influence. We will change this sentence.

Page 4128, line 8: Could a non-representative background only cause an underestimation of the modeled concentrations and not an overestimation as well?

Response: Yes, but in the case of Cabauw this generally leads to underestimations. We will change the sentence and add a comment that representative continental background concentration measurements or global chemical transport model concentration fields could be used to improve bias for the Lagrangian approach upon which the COMET model is based.

Page 4129, line 7 and Fig. 5: A further explanation of the strong uptake signal in the early morning in summer should be given.

Response: We also observed this uptake signal and it might be explained by the startup of photosynthesis in the still stable nocturnal boundary layer.

Page 4129, line 16-19: How would the results change for the case using FACEM? What do we learn from this exercise if they do not improve?

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Response: It would of course deteriorate the results predicted using FACEM. Implicitly, this part of text says that, suppose that SiB3 results are considered to be correct, what would be the error introduced by COMET and consequently overestimations in the FACEM model predictions.

Page 4129, line25: Flux measurements are available at Hegyhátsál, see Haszpra et al. (2005). I would recommend making use of these flux measurements already in this study in order to further disentangle the contributions by the different biosphere models and the transport model.

Response: For similar reasons as for your earlier comment about Page 4125, line 12, we acknowledge the fact that at some points the analysis could be more detailed but for the sake of readability of this manuscript we wish to save more detailed analysis of measurements for future studies. Also, I believe that the interpretations in such detailed studies requires close cooperation with the people that actually performed these measurements. For your information, flux measurements are available at Cabauw as well, we would welcome studies focused on the comparisons of the local fluxes with biosphere models and the variability that can be explained in the atmospheric signal from these local fluxes.

Page 4131, line 4: What is meant by spatial correlation? (cf. comment to Page 4124, line 15)

Response: I agree this is unclear and will remove '‘spatial’,'

Page 4132, line 9: Again the question arises what would happen for the case with FACEM fluxes. This should be specified, at least briefly.

Response: OK, we will add a note to the text.

Page 4132, line10-12: There is no need to repeat all numbers here.

Response: OK, we will remove the numbers

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Page 4133, line 9: What is meant by "different parts of the biosphere"? Please rephrase to clarify this.

Response: We meant to distinct photosynthesis, autotrophic respiration, and heterotrophic respiration. We will rephrase this sentence.

Figure 1: The color scale for R2 should be expanded to show more details, e.g. by assigning it to values between 0 and +1. Negative values of R2 do not exist anyway.

Response: OK, we will try to improve on the color scale

Figures 4, 5, 6: Instead of showing the mean diurnal cycle for all month it would be sufficient to show only a selection of panels, e.g. every second month or one month per season. Response: OK, we will restrict ourselves to displaying only the results mentioned in the text.

Technical corrections:

Response: The technical corrections shown below will be implemented, and the reference will be added.

Page 4119, line 16: greenhouse gas budget

Page 4126, line 25: add explicit reference to Table 1 to the sentence

Page 4126, line 26: ... investigated separately for daytime (..) and nighttime results.

Page 4126, line 27: ... cases allow for ...

Page 4127, line 5: ... Hegyhátsál tall tower...

Page 4127, line 27: ; ;; instead of yellow

Page 4129, line 23: ; ;; missing

Page 4130, line 9: ; ;; instead of yellow

Page 4130, line 18: ; ;; missing

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Page 4131, line 8: "this region"; or "these regions";

Page 4131, line 12-16: the sentence "For regions... expected and observe;" is not complete.

Page 4133, line 17 and corresponding reference: Karstens

References: capitalization of geographical terms, e.g. Mace Head, Hegyhátság.

References: Haszpra, L., Barcza, Z., Davis, K. J., and Tarczay, K.: Long-term tall tower carbon dioxide flux monitoring over an area of mixed vegetation, *Agricultural and Forest Meteorology*, 132, 58-77, 2005.

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

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