

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

Anonymous Referee #3

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General comments:

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 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

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in the introduction. The intercomparison 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.

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.

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 insights.

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.

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?

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Page 4121, line 8: The models rather 'simulate' or 'model' the concentrations than 'predict' the future...

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.

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.

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 focussing 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.

Page 4126, line 3: The questions/comments concerning statistics of GPP apply as well to NEP.

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

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?

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.

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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).

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

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

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?

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.

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

Page 4132, line9: Again the question arises what would happen for the case with FACEM fluxes. This should be specified, at least briefly.

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

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

Figure 1: The colour 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.

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.

Technical corrections:

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: 'orange' instead of yellow

Page 4129, line 23: ')' missing

Page 4130, line 9: 'orange' instead of yellow

Page 4130, line 18: ')' missing

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, Hegyhátsál.

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.

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