Atmos. Chem. Phys. Discuss., 8, S1419–S1422, 2008
www.atmos-chem-phys-discuss.net/8/S1419/2008/


Atmospheric Chemistry and Physics Discussions



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

**Anonymous Referee #2**

Received and published: 8 April 2008

General comments:

The paper compare fluxes calculated with two models for Europe and compares concentrations calculated based on these fluxes and a Lagrangian transport model for three sites.

A simple comparison between the different fluxes, given the differences in the models including their resolution is not very helpful. Adding flux data would significantly enhance the value of such a comparison. The differences between the models would be clearer if a table would be included that specifically lists the differences and similarities between FACEM and SiB. A clear description should be given what FACEM is targeted at, i.e. prediction vs. analysis, spatial vs. temporal patterns, and which processes are the authors interested in and by what reason.

There are a number of cases discussed, however the motivation is not very clear. At first I would have suggested to drop cases 2 and 4, since it is clear that taking out $CO_2$ by photosynthesis without putting a large fraction of it back in the atmosphere due to respiration cannot lead to good estimates of atmospheric $CO_2$. However, adding some words to motivate the decision why these cases where calculated could help. Interestingly the r-square with no biospheric fluxes nearly as good as with biosphere for Cabauw, this should be discussed.

The presentation of the results in form of the many figures needs a lot of work. For example Figures 4-6 contain 36 plots with 7 lines each, i.e. a total of 252 time series, with no legend shown in the figures. This makes it extremely hard to read information from these figures and to relate the time series to the different cases.

Overall, the paper may be acceptable after these issues and the comments below have been addressed.

Specific comments: Pg 4118, line 7: Providing an r-square value in the abstract without mentioning the time scale is not that useful, since the r-square values depend a lot on whether fluxes are resolved on hourly or diurnal time scales.

Pg 4120, line 23: “uncertainty accumulation” is not a good term to use here, it implies that other researches using more sophisticated models do somehow a bad job. This should be reformulated using more objective terminology.

Pg 4120, line 23: It is unclear why “This design limits the application of both models to regions without complex orographic features.”

Pg 4121, line 14: “The GPP accounts only for the uptake of $CO_2$ due to photosynthesis.” This is the definition of GPP, here it sounds as if something was omitted in FACEM.

Pg 4123, section 2.2: The domain should be specified, for example within one of the



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many flux maps shown in Fig. 1-3.

Pg 4126, line 14: “The magnitude of a net local source or sink at a certain location is small, generally in the order of 10% or less.” This is unclear.

Pg 4126, line 26: I suggest starting the description of the six cases should start with the first case, and not with the exceptions made for case 4 and 6. Also there is no reference to Table 1.

Table 1: The Table is hard to read. Different columns for different kinds of fluxes (ocean, land biosphere, emissions) might help to see what is in common and what is different in the cases.

Pg 4127, line 21: “The anthropogenic and oceanic contributions (solid red) add relatively little to the variability of the modelled signals, suggesting a larger influence of the local terrestrial biosphere on the measured variability than the local anthropogenic sources.”: I don’t see this. All signals show in the figures are correlated with each other and with the measurements. For this one should not refer to a figure like this.

All figures: labels a), b) etc. are missing

Pg 4128, lines 4-11: GlobalView has a temporal resolution of about a month, much less than synoptic variability. Thus any synoptic changes in the background are not simulated. A given trajectory will pick up an average background value, but usually this background is modified due to synoptic distortions of the flow upstream of the trajectory models domain. This is likely to result in biases with synoptic scale temporal patterns. In this sense GlobalView is not suited as a boundary condition of a model resolving synoptic scale variability.

Pg 4128, line 12: “… the modelled GPP signal is concealed by heterotrophic respiration. The uptakes of $CO_2$ due to photosynthesis, that are clearly present in case 2 and 5 are barely discernible in the measured signals.” This



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is nearly impossible to reproduce using the table 1, figure 4, and the text. The authors should try to find a better way to formulate this or modify the figure to convey this message. What I see from the figures is that all combinations of tracers correlate well with the observations, and seem dominated by PBL development over the course of the day. What is meant by “concealed by heterotrophic respiration”? This would be the difference between NPP and NEP, so (if I got this right) the difference between the orange and the blue lines in Fig. 1. There seems to be a reasonable signal. May be it helps plotting the individual components rather than various combinations.

Pg 4128, line 12: “ will be difficult, if not impossible, to dissect the different contributions of the biosphere to the measurements using concentration measurements only…. “ Given the problems mentioned above it is impossible for me to judge or follow this statement. However, in order to come to such a conclusion, statistical arguments are needed that quantify the difference between the agreements of the different cases with the model. The first step to this is to assess whether the differences in correlation coefficients, biases and variance between the different cases are significant. Simply mentioning transport uncertainties without quantification or reference can not support such a statement.

Pg 4129, line 12 -19: Doubling the nocturnal mixing height suggests that there is room for 100 % uncertainty. This is not surprising and has been amply discussed elsewhere and I recommend the authors to refer to the literature in this case. This simply questions the approach to test different biospheric models against concentration measurements without properly simulating vertical mixing within the transport models. Concerning the improvement in agreement with the measurements, the table should be augmented to facilitate the comparison. Also, the significance level should be indicated.

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



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