

## ***Interactive comment on “Lagrangian transport modelling for CO<sub>2</sub> using two different biosphere models” by G. Pieterse et al.***

**G. Pieterse et al.**

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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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General comments: The paper compare fluxes calculated with two models for Europe and compares concentrations calculated based on these fluxes and a Lagrangian

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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.

Response: A thorough comparison of the FACEM model with site level measurements was presented in an earlier paper (Pieterse et al., 2007). We will include a short summary of the findings in this study.

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.

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 slightly different fields of application, I wanted to avoid a good versus bad discussion about the models and to focus on the opportunities provided by the observed differences and their possible explanations for future improvements in the FACEM model.

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<sub>2</sub> by photosynthesis without putting a large fraction of it back in the atmosphere due to respiration cannot lead to good estimates of atmospheric CO<sub>2</sub>. However, adding some words to motivate the decision why these cases were calculated could help.

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Response: The different cases were indeed added to show the less well informed reader that the measured signals are the result of large counteracting signals that can have very different magnitudes for different sites. We will include more explanatory text in the introduction of Section 3.2.

Interestingly the r-square with no biospheric fluxes nearly as good as with biosphere for Cabauw, this should be discussed.

Response: Cabauw is located in a very densely populated region where anthropogenic influence is measurable during day and nighttime conditions. When the nocturnal boundary layer is formed, the concentrations will increase significantly due to the anthropogenic and respiration sources. In fact, the variability due to anthropogenic activity (around 50%) was lower than we expected prior to running the model calculations. We will add text explaining the effect of anthropogenic sources on the nocturnal boundary layer concentrations.

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.

Response: Precisely because of the large amount of data, we decided to leave out the legends, to explain the different colors in the captions, and gave the figures for each site fixed scales to enable direct comparisons. But I agree that it is still quite hard to read the figures quickly. Therefore we will make the figures easier readable.

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

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resolved on hourly or diurnal time scales.

Response: Yes, the r-square values were calculated for hourly values. We will add the required explanatory text in the Table captions.

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.

Response: I agree that this issue should be addressed with care, and want to stress that we did not mean to portray sophisticated models as being uncertain. But mathematically, uncertainties do accumulate and although a model can be more sophisticated in terms of included processes, it can be more uncertain than a less sophisticated model with known overall uncertainty when the parameters of the included sub-process are not determined with sufficient accuracy. We will rephrase this sentence.

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

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.

Pg 4121, line 14: ‘The GPP accounts only for the uptake of CO<sub>2</sub> due to photosynthesis.’ This is the definition of GPP, here it sounds as if something was omitted in FACEM.

Response: We meant that GPP does not include autotrophic respiration, so we will change this sentence accordingly.

Pg 4123, section 2.2: The domain should be specified, for example within one of the many flux maps shown in Fig. 1-3.

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Response: We will specify the domain accordingly

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.

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 al., 1996).

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.

Response: Indeed, this passage is very unclear. There should be a reference to Table 1 here. We will rephrase this paragraph.

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.

Response: The table will be extended as suggested

Pg 4127, line 21: ‘The anthropogenic and oceanic contributions (solid red) add relatively little to the variability of the modeled 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.

Response: Yes, this finding is more a reflection of sentiment rather than observation. We expected an even larger influence. We will change this sentence because the anthropogenic influence is in fact of significant influence (around 50%), but much lower than we expected prior to running the model calculations.

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

Response: In the submitted latex manuscript, the figure labels were included by code but in the final proof the figure labels did not show. We learned that the labels should have been physically part of the figure files. We will include the labels in the final version of the manuscript.

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.

Response: I agree. The latest versions of the COMET model actually use coarser resolution global model results to initialize the trajectory calculations but these model versions were not available at the time of this study.

Pg 4128, line 12: "the modelled GPP signal is concealed by heterotrophic respiration. The uptakes of CO<sub>2</sub> due to photosynthesis, that are clearly present in case 2 and 5 are barely discernible in the measured signals." This 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.

Response: The text should say "the modeled GPP signal is con-

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cealed by the respiration processes (2008). Furthermore, we could clarify that the total uncertainty (including the uncertainties introduced by the different calibration scales that are used, see for example [http://www.esrl.noaa.gov/gmd/ccgg/globalview/co2/co2\\_method.html](http://www.esrl.noaa.gov/gmd/ccgg/globalview/co2/co2_method.html)) of atmospheric measurements of CO<sub>2</sub> is frequently in the order of ±1-5 ppm, meaning that the differences between GPP and NEP that could be discerned during daytime, in the order of 10-20 ppm, are not discernable accurately.

Pg 4128, line 12: It 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.

Response: Throughout the paper, improvements in correlation are shown in conjunction with two other important parameters; variability and bias. Improvements are always discussed mentioning these parameters together. (Also in my personal opinion, any correlation (R<sup>2</sup>) less than 80% is statistically questionable and I agree 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 should inform the reader more explicitly about the fact that improvement should be assessed in terms of all three abovementioned parameters.

Pg 4129, line 12 -19: Doubling the nocturnal mixing height suggests that there is room

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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.

Response: Indeed this is not a new finding. We will add the relevant references. With respect to the statistics, please refer to my response to your previous comment.

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

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