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Biogeosciences Discussions

Interactive comment on "Analyzing the causes and spatial pattern of the European 2003 carbon flux anomaly in Europe using seven models" *by* M. Vetter et al.

M. Vetter et al.

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Dear Editor,

we are very grateful for the very constructive and detailed comments to our paper. The paper has been now edited by the English speaking coauthors. Furthermore, we have removed some of the repetitions and we believe that the paper has benefited from this. We have also included a more detailed description of the different model parameterizations, as well as a more detailed analysis of the model differences (see comment below). The paper shows that there is a need for a model development especially regarding the respiration parameterization as well as developing a crop-land parameterization which includes the special crop-phenology and management, which

are still lacking in most global models. This paper aims to assist the model-developers to prioritize the next model-development stages.

Response to reviewer number 1: Page S805 Line 8: *There is now need of repeating so many of the numbers provided in the table in the accompanying text.*

The text was really too long, and the repeating the numbers provided in the tables in the text were redundant. We have therefore removed them and just refer to the tables.

Page S805 Line 9: *Section 3.1 could be reduced in length considerably.*

We have rewritten the text considerably but describing the results of the major carbon fluxes for seven different models is not necessarily done in a short way. We feel that the text is now easy to follow and also one of the English speaking co-authors have read thoroughly through the manuscript.

Page S806 Line 1ff: *The authors state that the overall model agreement on negative NEP anomaly give high confidence in the modeled net flux response. But surely, by now terrestrial carbon models really should be expected to get the sign of the response to a severe soil moisture deficit, hot temperatures and high VPD, right?*

We agree that the models should show the expected reduction in the net carbon flux. Our point was that the reasons for the reduction in the net carbon flux were different. The response of the models differs in the fluxes such as GPP and Reco. Some models show an increase in Reco, and less reduction in GPP. Others show that the most important response was the reduction in GPP and not the reduction in Reco. The models have mostly been optimized against measured NEE. The models differ strongly in the way crop-land is parameterized. This also shows up in the GPP and Reco fluxes. Only one model explicitly uses a crop-phenology and harvest. The removed biomass was left to decompose at the end of the year. We have added a figure illustrating very different behavior of the models over the year for the crop plant functional type: demonstrating the need of further model development. This figure shows

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that the conifer plant functional type is much more similar in both seasonal variation as well as in annual maximum. Only one model showed a much lower annual maximum.

Page S806, Line 10: *What I found much more noteworthy is the notable difference in the absolute values of GPP, R, NEE between the models.*

The great difference in the absolute numbers of GPP and Reco among the models was partly due to an error in the parameterization scheme of one model (JULES). We have updated the results for this model and the absolute values are now in the same range. Especially the crop/grass areas improved. Since crops are the dominant land cover type of Europe. The total sums of GPP and Reco of Jules moved towards the total sums of the other models. Therefore we do not go into a more detailed analysis of the absolute numbers.

Page S806, Line 11 and the following lines: *Overall the paper would improve considerably by a more in-depth analysis of what causes the model differences.*

We have improved the in-depth analysis, as we have written a very detailed description of the model parameterization and also how croplands are treated in the different models. These descriptions are outlined in Section 2 (Methods), as well as in very detailed Tables (A1-A4) in the appendix. Furthermore, we have selected two regions (one dominated with crops, and one dominated by conifers), for which we show the seasonal variation among the models, and the dependence on the climate parameters and the model calculated soil-water content (only possible for the processes oriented models) in these regions (see Figure 5), and the discussion in section 3.3. Because of the large number of models and also the needed detailed description of the model differences, it was not possible to go into detail on all aspects of the model differences. We choose thus to focus on the net carbon-flux and its component fluxes.

Page S806, Line 14ff: *How are fertilized grasses represented in models with no N-cycle?*

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The only model having an explicit phenology and also including management is LPJ. Biome-BGC treats croplands as intensively fertilized natural grasses (without harvesting), ORCHIDEE treats the crops as especially productive natural grasses, also without harvesting. JULES represent crops as natural grasses (without harvesting). This is outlined in section 2, the expression “super-grasses“ has been removed from the text.

Page S806, Line 16ff: *What is the ORCHIDEE anomaly for the months July-September in this study (by contrast to Ciais et al. 2005)?*

We included the number of the NEP of the July - September NEP 2003 anomaly for ORCHIDEE as suggested by the reviewer for a better comparison between the two studies (the anomaly July to September 2003 was estimated to be -0.58 Pg whereas Ciais et al 2005 estimated the anomaly to be -0.50 Pg). The question regarding the different climate drivers (REMO versus ECMWF) have been addressed this using a similar modeling set-up for the model Biome-BGC (Jung et al. (2007). There, we found that the effect of different climate drivers contributed to 20% of the interannual variation (based on annual values, GPP), and that the use of different climate-drivers have a large effect on the simulated interannual variability of the gross and net carbon fluxes.

Comments to Page S806/S807, Lines 20ff: *The language is by and large acceptable, but the manuscript will require a very thorough editorial reading by one of the native English speakers that are listed as co-authors.*

The paper has been read thoroughly through by one of the native English speaking co-authors, and the language has been corrected as far as the native speaker found reasonable. We have edited the manuscript in such a way that the comments regarding the language are now mostly redundant. The adjective “on the other hand“ is a common way of stating an opinion from another point of view. We have therefore left this expression in the manuscript even if the reviewer has another opinion about this. *Comment for Page 1213 line 11:* The text has been rewritten.

Comment for Page 1214 line 1: We dont see why this should be unclear.

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Comment for Page 1216 line 12 13: The text has been rewritten and doesnt contain the phrase “was estimated to“ anymore.

Comment for Page 1216 line 15: Text has been changed to “PIXGRO estimated a NEP over the growing season 2003 close to 0.“

Comment on Page 1217 Line 13: We have changed droughts to drought. Further we use the expression “drought“ on purpose, as we see the carbon anomaly not only as an effect of soil water deficit, but as a combination of soil water deficit, hot temperatures and high VPD which can be described with one expression: drought.

Comment for Page 1218 line 4-9: Text has been rewritten.

Comment for Page 1211 line 21/22: “forests“ has been deleted one time.

Comment for Page 1212 line 11-12: “estimated“ has been deleted one time.

Comment for Page 1210 line 11-19: The paragraph has been rewritten.

Response to reviewer number 2:

S714, Paragraph 1: *Indeed most of the carbon budget estimation are carried out at the year basis and it would be interesting to see if the biodiversity of functional types at European scale counterbalance or smooth out the effect when considering the whole year period.*

We have added a table with annual estimates of NEP, GPP, Reco to the paper (Table 5). We have also included a discussion of the model differences, and the differences between the annual numbers compared with the seasonal numbers (Section 3.3) We have added a detailed description of the model parameterizations in the appendix, as well as a more detailed analysis of the carbon fluxes for two small regions (one dominated by crop, one dominated by conifers) (Section 3.3). We claim that the largest differences among the modeled NEE, GPP, and Reco are due to the parameterizations of the crop-land, which is the dominant land-cover in Europe.

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S715, Paragraph 2: *I would discuss differences not only as the outputs of different models, but also trying to understand the differences in the representation of processes in the models. Otherwise there is really no value in model intercomparison if we do not learn from differences.*

We added a Figure 5 which also shows on an average basis the very different behavior of the models for the crop plant functional type, whereas more similar behavior for the conifer plant functional type. This does indeed show that the most critical part of the terrestrial carbon flux models is their ability to simulate crop-land and crop-management. This will also in the future be one of the most important issues for improving the estimation of the European carbon balance. Further the model community will need a regional and continental scale database of crop-specific data for use as input in the models.

Interactive comment on Biogeosciences Discuss., 4, 1201, 2007.