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Interactive Comment

Interactive comment on "The inter-annual variability of Africa's ecosystem productivity: a multi-model analysis" *by* U. Weber et al.

U. Weber et al.

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Professor Fortunat Joos, Editor

Re. MS-NR: bgd-2008-0109

The interannual variability of Africas ecosystem productivity: a multi-model analysis

Dear Prof. Joos,

We thank all reviewers for there constructive criticism that helped to improve the manuscript substantially. We have followed the suggestions of the reviewers in most cases. All relevant parts of the manuscript have been revised in order to clarify issues raised by the referees. Moreover, we have conducted additional analysis regarding the comparison of the atmospheric inversions and modeled NEP, and rewrote the respective section to clarify on the issue of fire emissions raised by the reviewers. We hope that our manuscript is now acceptable for publication in Biogeosciences. Please find below our detailed replies to the reviewers comments.

On behalf of my co-authors,

Yours sincerely, Ulrich Weber

Remarks of Referee 1

1) In equation 2 it’s not clear what you use in the denominator for years without CRU data (2004-2006). Equation 2 was subject to a typing error and has been corrected in the final version of the manuscript. The harmonization of the TRMM period is based on TRMM data only. Therefore the denominator is NCEPy,m instead of CRUy,m.

2) Section 2.4.1 "resulting in a grid a IAV": Should this say "resulting in a grid of IAV"? This part of the sentence has been subject to a typing error. The correct meaning is: ‘resulting in a grid of IAV’, and was corrected in the final version of the manuscript.

3) Section 2.4.3: I don’t know much about PCA. I got the gist of it when I also saw Table 3. Would it help to add more info in an appendix? We believe that additional material about principal component analysis is not needed in this paper given that it’s a standard multivariate technique to reduce the dimensionality of data cubes.

4) Section 3.1: I suggest adding references after the various GPP and NPP estimates. See reply on the same subject for Referee 3

5) Section 3.3: Was the acronym "TER" defined? The definition for TER has been included in the final version of the manuscript.

6) Section 3.4: I think that you refer to Fig. 11 before referring to Fig. 10. Better to

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reorder them. We very much appreciate this comment. Initially, the order of the figures follows the concept of analysis, where first we evaluate the correlation between modeled GPP and PCAmode1 (Figure 9) before looking at modeled GPP and PCAmode2 (Figure 10). A comparison with observational data for both PCA modes (1 and 2) is shown in Figure 11. We have reordered Figure 11 and Figure 10 in the final version of the manuscript.

7) Fig. 6: Do I understand correctly that "pluses" and "minuses" cancel each other out in this depiction of model agreement? This is a very good point. Positive values of model agreement in Figure 6 did overlay negative values if present, but did not cancel each other out. In context with section 3.2 we have simplified Figures 6 and 7 (left) to display only regions of large interannual variability of GPP and NEP.

Remarks of Referee 2

The variability of NEP in dry regions is mostly driven by fire emission. Could you indicate why fire is not taken into account in the analysis, while 3 out of 4 models are including a fire model.

Referee 2 addresses a relevant part of the carbon cycle of Africa. We agree with reviewer 2 that fire may control the carbon balance in dry regions on the stand scale level but to what extent fire losses control the variability on the continental scale is speculative because contributions of burned and not burned areas cancel. We have investigated the effect of fire on the continental scale in section 3.1 and concluded that it constitutes only a minor contribution to the continental scale variability. A detailed analysis and discussion of the role of fires is given by Lehsten et al. in this special issue.

Further the comparison of NEP with atmospheric inversion is difficult to evaluate. In many cases NEP changes the sign by fire emissions. Has fire been simulated in these 3 models? See reply on the same subject for Referee#3

Have all models been running with potential vegetation? This should be mentioned. We had stated in section 2.3 that “Vegetation distribution was dynamically simulated by the models.”. To make it fully clear we have changed this sentence to: “Potential vegetation distribution was dynamically simulated by the models.”

I miss the influence of vegetation distribution on the carbon cycle. Are there great displacements within the investigation period? Changing vegetation distribution has potentially a strong effect on the variability of the carbon cycle on longer time scales (100y-1000y). Given that our study is based on 25 years we have decided not to present an analysis of this effect to keep the paper concise.

Specific comments 4039, 3: What is the meaning of a heuristic approach for simulating vegetation distribution. The heuristic approach used by Triffid to simulate vegetation coverage is as follows: Triffid uses the knowledge gained about the large scale land-surface processes in determining the distribution of the volume of carbon to the various PFTs. This volume of carbon in the terrestrial vegetation is assigned by solving a set of first-order differential equations.

4044, 14: Does ORCHIDEE and JULES represent shrubs or a similar PFT to confirm this argument? This note of referee 2 reflects a misinterpretation of our manuscript at this point. The sentence (“High LPJ-DGVM GPP estimates for all regions except the Central Tropical Forest can be attributed to the missing representation of shrubs and savannah ecosystems in the model.”) in the manuscript has therefore been removed. As is now stated on page 4043, we attribute large continental numbers of NPP for ORCHIDEE and LPJ-DGVM to ‘too extensive forest cover’ (see also our reply to reviewer 3).

4044, 24: That could be explained by uniform distributed rainfall within the year in the inner-tropics. The good agreement of the four models and the remotely sensed data, as well as the seemingly heterogenous region of Madagascar should be emphasized

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here. The seemingly uniform distribution of rainfall accounts for the known absence of water limitation in the inner tropical rainforest (Nemani et al. 2003), and is further evaluated by the response of simulated gross primary production to driver meteorological conditions. Hence, prevailing moisture conditions create climatic circumstances for plant growth, but do not cause photosynthetic variability. The region of Madagascar is very heterogeneous. We assume this as a major source of disagreement between the four models in this region, compared to others. Extensive anthropogenic land degradation causes disagreement between modeled results and observed FAPAR data We have revised this part of section 3.1 to state the specific characteristics in the inner tropical region. “… Both, remotely sensed FAPAR and simulated GPP show that the seasonality of photosynthesis varies in concert with rainfall in all regions except for the inner tropical forest, where low intra-annual variation of rainfall creates climatic conditions without water limitation, reducing the effect of precipitation on photosynthesis. The heterogeneous vegetation of Madagascar together with known extensive anthropogenic transformation (Green & Sussman 1990) account for the disagreement between the models and between models and remotely sensed FAPAR”

4045, 8: Are fire emissions the driving part? See our response to similar comments of referee 3 regarding the role of fire.

4046, 3: Again: Fire influence variability of NEP is strong. Because fire occurs not regularly but rather due to different moisture conditions. See stated arguments on fire influence (referee 3)

4048, 4: Why should light limitation only occur where phosphorous limits productivity? This is a misunderstanding. We speculate that the opposite is the case, i.e. light limitation may occur when nutrients are not limiting. (Section 3.4 Page 4048: “Possibly, light limitation in the African tropics occurs where less phosphorous depleted soils are present since phosphorous is believed to be generally the main limiting factor for productivity in the tropics (Vitousek 1984).”

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Technical comments PCA is not explained The acronym PCA is explained by the heading of section 2.4.3, but has additionally be explained when mentioned the first time in the manuscript.

Fig.11 (S.4046) is mentioned before Fig. 10 (S.4047). See reply on the same subject for Referee 1

PCA Mode 3 and Mode 4 are not further analysed. The first 2 modes of the PCA explain 80 percent of the variability of the meteorological driver data, and are related to a moisture and temperature/radiation gradient respectively. We feel that not much additional information can be extracted from the analysis of higher order modes with little variances and therefore we discarded them to keep the analysis balanced.

Fig. 8 is not convincing and difficult to read. We agree that this figure is largely redundant to table 2 and therefore followed the suggestion of reviewer 2 and removed figure 8.

Remarks of Referee 3

In particular, reasons for the different DGVM responses should be discussed, for example, LPJ, LPJ-GUESS and ORCHIDEE have common elements yet their responses are quite different. It should be mentioned that most models including some applied here do not perform well in water limited regions (see Morales et al., Global Change Biology). Referee 3 makes a good comment here and we have addressed it in the Introduction section: “[…] however, these models are also associated with large uncertainties (e.g. McGuire et al 2001, Friedlingstein et al. 2006) in particular for water limited conditions (e.g. Morales et al 2005, Jung et al. 2007), and in addition have generally not been tested and parameterized specifically for Africa.”

Minor comments: P 4037 line 21 are the models really different? The models that are participating in our analysis are from the same class of ecosystem models, namely process-oriented ecosystem models developed for large regions. They share a com-

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mon structure of how carbon and water flows through compartments and make use of the plant functional type concept for generalizing parameters. However, the models differ in a number of ways, such as the representation of certain processes (e.g. functioning of canopy conductance, soil water dynamics, and big leaf vs. gap model) and parameters, which results in substantially different behavior as illustrated in the manuscript. Although addressing the challenge of estimating the structural independence of different models is highly interesting and relevant, we feel that this is clearly outside the scope of objectives.

P4042 ˜line 20 we do not know global GPP or NPP, modify the sentences accordingly to say “assuming a global GPP of 120 PgC/yr” plus add a reference for the figure. Likewise I do not think one can say based on the Cramer et al., 1999 results that two models may overestimate NPP. Reviewer 3 is right – we don’t know global GPP and NPP exactly. We have included the African contribution to estimated global productivity for illustrative reasons and stated the numbers that we assumed in brackets which are based on IPCC AR4. We agree with the second point of reviewer 3 that we cannot judge if a model is over- or underestimating by comparing to previously published results. However, we have good indication that a too large forest area simulated by the models is responsible for too large productivity accordingly. We have changed this section accordingly: “Possibly, the too extensive forest cover simulated by LPJ-DGVM (17.28 PgC y-1) and ORCHIDEE (15.38 PgC y-1) may have caused an overestimation of continental scale NPP. A realistic representation of savannah ecosystems is still a challenge for dynamic vegetation models.”

P 4041 & P 4045 GPP and TER do not appear to be defined when first used. We appreciate this comment. We have now defined the acronyms the first time they are used on page 4041 and 4045.

P 4045 line 27, JULES in capitals. Changes are arranged for the final manuscript.

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P 4047 line 10 – reference Morales et al., 2005 The article from Morales et al. (2005) is relevant at for our manuscript and is now cited in section 1. In section 3.4 page 4047 line 10 particular emphasis is given to the interannual variability of GPP, which is not subject of the Morales article. Hence we decide not to reference this paper here.

P 4047 line 17 – radiation limiting inner tropical productivity is not a new finding. Please reference Nemani et al., Science, 2003. We do not propose radiation limitation in the inner tropics as a new finding referring to Saleska et al. (2003) and other studies. The radiation limitation in the inner tropics as found by Nemani et al. (2003) certainly needs to be mentioned in this context, and will be included into the final manuscript. We discuss our findings also in the context of the study of Nemani et al (section 3.4, page 4047): “Using a production efficiency model Nemani et al. 2003 find that tropical Africa is primarily radiation limited, while both Churkina and Running 1998 as well as Jolly et al 2005 masked the inner tropical regions and concluded that no climatic constrain limits productivity here.” And on page 4048: “The overriding effect of nutrient availability may explain why we find only small areas with light limitation from the analysis using the satellite data, in contrast to the extensive areas of light limitation indicated by LPJ-DGVM, JULES, and Nemani et al. 2003 because none of the latter considers explicitly nutrient cycles.”

P 4048 “we find only small areas with light limitation from the analysis using satellite data” Is this not opposite to Nemani et al. ?

Reviewer 3 makes a good comment. Indeed, Nemani et al. found a spatially coherent pattern of radiation limitation in the inner tropics based on a simple conceptual model. The reason why we find only small areas is not fully clear – it may be related to data issues or it may be real and related to the role of nutrients that have a stronger control than climate in the African tropics. We have revised the entire section and provide a more detailed discussion of possible explanations:

“From the correlation maps between the remote sensing based FAPAR and PCA2 we also find patches in the inner tropics of Africa where productivity increases with increasing radiation but to a much smaller extent than suggested by some of the models. Thus, the role of light availability in the African tropics remains controversial. There are several possible reasons why we find only small areas with significant correlation between radiation and productivity from the remote sensing data: (1) the short time series (8 years) in combination with small variances make it difficult to achieve large correlations, (2) the uncertainty of the meteorological data that originate from global reanalysis, (3) possible errors in the satellite FAPAR retrievals due to subpixel cloud contamination, (4) a strong role of non-climatic limitations of productivity such as nutrients. If nutrients are not most limiting, ecosystems are more sensitive to climatic variations. Possibly, light limitation in the African tropics occurs where less phosphorous depleted soils are present since phosphorous is believed to be generally the main limiting factor for productivity in the tropics (Vitousek 1984). The overriding effect of nutrient availability may explain why we find only small areas with light limitation from the analysis using the satellite data, in contrast to the extensive areas of light limitation indicated by LPJ-DGVM, JULES, and Nemani et al. 2003 because none of the latter considers explicitly nutrient cycles.”

Figure 2 The atmospheric inversions exclude fire emissions. However LPJ includes fire – is these therefore directly comparable? This comment as well as similar comments raised by the other reviewer shows that we had explained this section insufficiently. Our analysis was consistent. In the case of LPJ (and all other models), NEP was calculated as the difference between GPP and TER. We had removed the contribution of the fires in the atmospheric inversions to also gain NEP (not NBP) from the inversions, so that the analysis was consistent. However, given that several comments targeted the role of fire on the interannual variability of the African carbon balance we have conducted additional analysis and used a new version of the atmospheric inversions to also extend the period of comparison. Accordingly, figure 2a (NEP IAV) has been updated and an additional figure 2b (NBP IAV) has been inserted in order to illustrate the effect of fire emissions on the interannual variability of African carbon balance. On page 4043 we clarify what exactly we are comparing: “We compare the simulated variations of the African carbon balance in terms of Net Ecosystem Productivity (NEP), and Net Biome Production (NBP) with results from global atmospheric inversions from Rödenbeck 2005 (version s96_v3.1) covering the period of 1997 to 2006 based on globally 51 stations of atmospheric CO2 records. This way we also determine the role of fire on the interannual variability of the carbon balance. Simulated NEP is calculated as GPP minus Terrestrial Ecosystem Respiration (TER). Modeled NBP is based on the difference of NEP and model specific fire emissions, except JULES which does not include fire. The inversions detect carbon emissions from fire. To facilitate comparability between NEP simulations and inversions, we used the Global Fire Emission Database (GFED version 2.1, available at ftp://daac.ornl.gov/data/global_vegetation/fire_emissions_v2.1) from Randerson et al. 2007 and Van der Werf et al. 2006 to correct for carbon fire emissions in the atmospheric inversions. ” The brief discussion of these two figures is given in section 3.1 which identifies fire emissions to be a minor source of African carbon balance IAV: “The comparison of simulated interannual variations of the African carbon balance with atmospheric inversions reveals a consistent pattern but some discrepancies remain (Figure 2). There is agreement of above average net uptake for 1997, 2000, 2001, and 2006 and a below average carbon balance for 1998, 2002, 2003 and 2005. The general pattern of continental scale IAV is consistent between NEP and NBP, which indicates a small contribution of fire emissions on the variability of the African carbon balance in the considered period. Relatively low interannual variability of fire emissions in Africa is consistent with van der Werf et al. 2006. Some of the deviations between the models and the inversions is certainly also related to uncertainties of the latter given that the density of atmospheric CO2 measurement stations is low.”

Interactive comment on Biogeosciences Discuss., 5, 4035, 2008.

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