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Biogeosciences Discussions

Interactive comment on "Climate-driven shifts in continental net primary production implicated as a driver of a recent abrupt increase in the land carbon sink" *by* W. Buermann et al.

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Referee (Chris Jones):

This is an interesting manuscript that explores a potentially important feature of the land carbon cycle, namely an apparent shift in land carbon uptake in the late 1980s. This shift has been analysed before by some of the same authors and this paper seeks to bring new insights into the climate drivers of the shift. The results are that two distinct regions play the dominant role and for different process-reasons. The coincidence of these two regions (tropical northern Africa and Northern Eurasia) is enough to provide a global signal in NPP and by inference NEP.

In general the paper is carefully planned and the authors present a 3-fold attack based on data-driven model CASA, process based models from TRENDY, and a residual carbon budget analysis from GCP. For each they consider sources of uncertainty – for example my first reaction on reading the abstract was to question the precip dataset used, so I was pleased to see that multiple datasets were used to quantify the uncertainty from climate drivers.

The results and discussion are presented in a very convincing manner, although I was left wondering if the paper focused too much on the similarities in the different methods and neglected discussion of some of the differences. I've listed below a few areas I'd like reassurance that there isn't an underlying problem. For example, the change-point analysis on CASA output reveals quite clearly a shift in the 1980s, but to find the same shift in TRENDY runs, the long-term trend due to CO2 needs to be removed. This is very clear in table S2, but only gets a passing mention in the main text with no attempt to explain either WHY this is the case (maybe a slight hint that the authors don't believe the CO2 fertilisation response of the TRENDY models - but this is only speculation), or more importantly what this means for the analysis. What do we conclude about the 1980s shift if one approach finds it, but another approach needs more careful processing to reveal it?

Authors: We highly appreciate these thoughtful comments to increase the transparency in our analysis and also to discuss more about the differences in the modeling approaches. In our thoroughly revised version, we thus have extended the analyses to include all carbon component fluxes (NPP, Rh, and NEP) for both CASA and Trendy simulations. We added three additional figures (Fig. S2, S5-S6) and an additional table (Table S3) in the revised supplement that show corresponding results. In addition, we revised our estimate of a global late 1980s NEP shift using a combination of residual calculations (e.g. estimating Rh as residual of NPP from CASA and Trendy and NEP from GCP) and direct model estimates (see newly formed Section 3.4 and details are given in the Supplement as well). Following this referee's suggestion, we also elaborated more on the differences between CASA and TRENDY (for example see added material in Section 3.3 about 'seasonal NPP carry-over effects'). In regards to CO2 fertilisation, however, our interpretation of the results are somewhat conservative. On one hand, the fact that the late 1980s shift is not seen in the TRENDY S2 experiments (neither in the component fluxes nor in the corresponding NEP data which are now included; see Table S1-S3 in the revised Supplement) may suggest the possibility that the sensitivity of process-based models in regards to CO2 may be too high. A brand new study that solely focused on a comparison of satellite-constrained NPP sensitivity to CO2 concentrations with those based on process-based models came to the same conclusions (Smith et al. 2015; this reference is now also included in the revised ms). On the other hand, the TRENDY ensembles also show a lower climate sensitivity when compared to CASA (e.g. temperature sensitivity over Northern Eurasia), which could be another explanation for the lack of a 1980s shift in the S2 experiments. We realize that our results and interpretations on this CO2 fertilisation issue do perhaps create more questions than they provide answers at this stage, but we also believe these are important points for the community to engage in.

Other more direct comparisons that the authors might show are: - you compare and contrast 3 different solar and precip datasets for driving CASA. Can you also add to this comparison the radiation and precip datasets used in TRENDY. Is this the same data? and if not how does it differ? what happens if you drive CASA and TRENDY models with as close to possible all the same datasets (T, P, radiation etc)

Authors: In both the CASA and TRENDY simulations CRU monthly temperature and precipitation data play a primary role. For example, in TRENDY CRU climate data are exclusively used. Our analysis also shows that the differences between the 3 precip data sets that we investigated were actually rather small (see Fig. S11 in the Supplement). A substantial difference in the CASA and Trendy simulations, however, is that in CASA surface solar radiation is one of the driver variables, whereas in TRENDY it is not (see Sitch et al. 2015; ref. in ms). Yet, the two focus regions of our study

(Northern Eurasia and Northern Africa) are both generally not considered radiation-limited, and this may suggest that the differences in model driver protocols may not play a substantial factor in explaining possible differences between simulated carbon fluxes from CASA and TRENDY.

- you mention that TRENDY models simulate FAPAR whereas CASA takes it as an (uncertain) input. How do these compare? the input FAPAR to CASA has a very marked jump visible in figure 2. What does FAPAR from the TRENDY models look like? Does it also follow this shift?

Authors: We also feel that it would be really informative to compare the satellite-based FAPAR (used in CASA) and the prognostic TRENDY-based FAPAR specifically in light of the marked late 1980s jump. However FAPAR outputs for the TRENDY models are as of yet not available and this precludes as unfortunately from looking into that in more detail. It should be noted, however, that comparisons between satellite-based and TRENDY ensemble leaf area indices (a variable closely related to FAPAR) generally show some broad consistency (see Fig. 7 in Sitch et al. 2015; ref. in ms). On the other hand, observation-model comparisons do indicate that the phenology responses in satellite-driven models are generally more robust than in process-based models (e.g. Raczka et al., 2013; this reference is now included in the revised ms in Section 3.3.).

- you say there's no data-constrained RH product, so you use the output from CASA. But TRENDY will also produce RH of course, so what does this look like? Why neglect 9 models output to just show CASA?

Authors: In this revised version, we have included also an analysis on the Trendy Rh data and more (see also detailed response to the following comment as well as our first response above).

- in general, some direct comparison of the different methods would be nice - so a summary plot of all the different climate datasets in one place (those for driving CASA vs those for driving TRENDY), all the NPP outputs (CASA and TRENDY), all the RH outputs (CASA and TRENDY), all the NEP outputs (CASA, TRENDY and GCP). Figure 2 and 3 show there are similarities of course between CASA and TRENDY, but also some clear differences - how important are the differences? and how do would this comparison look at the seasonal scales (the TRENDY counterparts to fig 2 lower traces). I'm not suggesting there are any problems from the above comments nor that the authors have been selective in what they show. But some of the above comparisons seem obvious things to do, so it would be reassuring to know the authors have considered them and not found any issues. As long as we can be assured there are no hidden problems in any differences between the 3 datasets examined, then I recommend publication.

Authors: Again, we appreciate these comments to increase specifically the transparency of our analysis. In addition to our detailed response in regards to added elements in this revised version (see first response above), we also added the seasonal counterparts in the TRENDY NPP series for Northern Eurasia and Northern Africa as requested by this reviewers (Fig. 3 in revised ms). A discussion about the differences (in TRENDY and CASA) between seasonal responses is added in Section 3.3.

other minor points to address that may help improve the paper: - I wondered about your pinup process for CASA. (a) why do you pinup to equilibrium for a period that almost certainly isn't (does this maybe explain your difference wrt TRENDY trends?). (b) why repeat a rather small period of forcing (1982-86) which itself is rather anomalous (containing both a big El Nino and Volcano - El Chichon). Have you tested the sensitivity of your results to the CASA initialisation?

Authors: This point is well taken, and when exploring this issue we discovered an error in our CASA spinup description. We actually did use a driver climatology based on the whole study period (1982-2011) and not as stated based on the shorter 1982-1986 period. We corrected this in our revised ms. In response to this reviewers' comment we did nevertheless test the robustness of our results in regards to spinup climatology. Results show that only in the case of Rh fluxes (and consequently NEP) and during the first year there is a some influence of spinup initializations (e.g. dashed circle in Fig. 1

of this response), suggesting that the CASA model is indeed close to equilibrium. Our relatively conservative approach in our change point methodology (discounting the first and last 5 years in a series as possible change points) further ensures that spinup effects do not have any sizebale effects on our results.

- you mention land-use as another potential driver of a shift, but this is not included in the TRENDY S1 or S2 runs. Have you looked at the runs that DO include land-use? (I'm not up to date on whether these are published yet, but the last TRENDY protocol had land-use forcing)

Authors: Yes, the TRENDY S3 experiments DO include land use, but these simulations are unfortunately as of yet still under embargo from the TRENDY modeling team. But also recall that at least at global scale the land use is considered in calculations of the residual carbon sink from the GCB (e.g. see Table 1 in ms).

- you dismiss using the ISCCP solar radiation data as it is "biased high over the Amazon". But from figure S4a this doesn't jump out as being that different from the other datasets. And this looks like a region with very few obs. How subjective is this decision, and how important is a bias over the Amazon given your focus on other regions?

Authors: Yes, the comparison in Fig. S4 does not provide a clear picture, but a comparison of area-averaged radiation data representative of the Amazon basin shows a large positive 'bias' of roughly 15 W/m2 in the ISCCP data relative to the other 3 data sets that we evaluated (see Fig. 2 in this response). Including the ISCCP would increase the uncertainty in the CASA NPP simulations considerably, which would make identification of a potential shift even more challenging (but recall that we did not see evidence of robust prominent shifts in data-driven NPP over the Amazon basin).

- if the UMD data is based on ISSCP, how different is it and why is it acceptable?

Authors: The UMD data use the same ISCCP-DX cloud, water vapor and ozone data, but the radiative transfer model and supporting data (e.g. aerosols, land cover) differ.

Comparisons with surface observations indicate that UMD has indeed lower biases than the other products (Ma and Pinker, 2012; ref. in the Supplement) and does not suffer from discontinuities in the record (Vinokullo et al., 2011).

- you mention that some radiation datasets include the effects of aerosols - do you mean on the total radiation? or also on the diffuse fraction? if only on the total then aerosols lead to reduced light and productivity, if you include the diffuse effect productivity can be enhanced - so can you specify which is included?

Authors: Yes, some radiation data sets include the effects of aerosols on the total radiation. All solar radiation data sets included in our analysis do not separate diffuse and direct components. The CASA model has no explicit diffuse light response parameterizations, which may be considered a limitation. On the other hand, some diffuse effects on productivity may be captured through the satellite-based fAPAR. In general the inclusion of aerosol effects in available solar radiation data is somewhat limited. For example, the SRB (V3) radiation product uses an aerosol climatology that does not include the impact of biomass burning which is clearly important in the Amazon and other tropical regions (Stackhouse et al. 2011; ref. in the Supplement). The UMD also uses a aerosol climatology, but has a more detailed treatment of aerosol properties that includes variations in single scattering albedo and asymmetry factor (Ma and Pinker, 2012; ref. in supplement).

References: Vinukollu, R. K., R. Meynadier, J. Sheffield, and E. F. Wood (2011), Multi-model, multi-sensor estimates of global evapotranspiration: Climatology, uncertainties and trends. Hydrol. Processes, 25, 3993–4010, doi:10.1002/hyp.8393.

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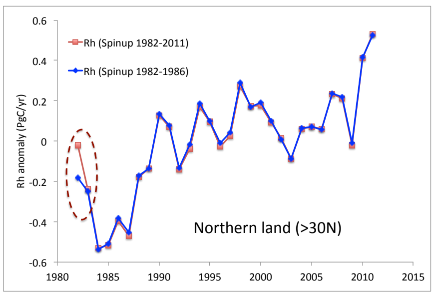

Figure 1. Anomalies in CASA-based Rh flux for northern land and for two sets of model spinup initialisations. In one scenario, the model is spinup for 250 years using a driver climatology based on 1982-2011, whereas in the second scenario a climatology based on 1982-1986 is used.

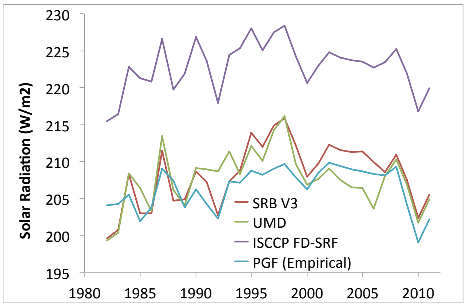

Figure 2. Time series of various total solar radiation data products representative of the Amazon basin. For details on individual data products see Table S4 in the Supplement of the ms.