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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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Review of "Climate-"driven shifts in continental net primary production implicated as a driver of a recent abrupt shift in the land carbon sink, by W. Buermann et al.

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?

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)

- 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?

- 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?

- 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.

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?

- 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)

- 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?

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

- 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?

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