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Discussions

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Interactive comment on "Spatially asynchronous changes in strength and stability of terrestrial net ecosystem productivity" *by* Erqian Cui et al.

Anonymous Referee #2

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1. General comments

Erqian Cui et al. studied the annual NEP and the inter-annual variability of NEP and intended to provide local indicators to better understand their spatial patterns at the FLUXNET site level. I find this study relevant as it is important to have a better understanding of the factors controlling the spatial and inter-annual variability of NEP. However, I have some concerns about some aspects of the method and how the results are presented (see More specific comments section). In addition, there are some results presented in this study that do not provide ay significant new information compared to the available literature (e.g. spatial patterns of annual NEP and IAV of NEP at the global scale). Plus, most of the analysis is done at FLUXNET site level, therefore I do not really the point of using the FLUXCOM and CLM4.5 for the presented study. In

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Discussion paper

short, although I find the presented study suitable for the scope of Biogeosciences, the manuscript is still in its early stage to be accepted as it is, therefore I suggest to make major revisions before potential acceptance.

2. Specific comments

L. 3-4 The title is very confusing and does not really reflect the findings of the analysis. Please try to rephrase the title so that it matches the message the analysis is trying to convey.

L. 38 "machine-learning-derived database." This concept seems odd and confusing. What about something like "based on a compiled global dataset and a machine learning method". The use "'machine-learning-derived database' is also not entirely true because, as far as I understood, only the FLUXCOM dataset is based on machine-learning approaches. FLUXNET in-situ data and the CLM4.5 product are not using any machine-learning methods.

L. 65 "is related to the strength of carbon sink". It can also relate to the strength of the carbon source. Consider rephrasing to be more generic.

L. 68 Not convinced by the use of 'asynchronously' all over the manuscript, particularly because the results presented in the manuscript do not provide evidence that the spatial patterns of annual NEP or IAV_NEP are not simultaneous or concurrent in time.

L. 76-77 'environmental fluctuations among years'. Musavi et al., 2017 attributed the year-to-year variation to species richness and stand age. In the same line, Besnard et al. 2018 attributed most of the annual NEP variation to forest age.

L. 82-84 Can this sentence be merged with the 1st sentence of the paragraph (L. 71-72)? They seem quite redundant.

L. 84-86 The last sentence of this paragraph seems a bit out of the context of the whole paragraph. Consider improving the transition between the last sentence of the paragraph and the entire paragraph.

L. 85 "could be integrated into some simple indicators". I would use the term 'decompose' instead of 'integrated'. After all, the authors want to decompose the contribution of a series of carbon uptake and carbon release metrics to annual NEP and IAV_NEP.

L. 98-99 Not sure that FLUXCOM products are the best to assess IAV_NEP. Please check Jung et al. 2020 to understand the issues of such products when looking at IAV_NEP. Why not using NEE derived from atmospheric inversions though (e.g. Jena CarboScope (Rödenbeck et al., 2018), CAMSv17r1 (Chevallier et al., 2005, 2019) and CarbonTracker-EU (Peters et al., 2010)). At least, we know that this data capture some processes that contribute to IAV_NEP, which are not being captured with eddy-covariance data (e.g. fire, CO2 fertilization).

L. 122-129 It might be relevant to specify that you use the FLUXCOM RS-meteo products for which the inter-annual variability is only driven by climatic conditions as they used the mean seasonal cycle of remote sensing products. This basically means that there is no inter-annual variability directly related to the state of vegetation.

L. 124 why only using the CRUNCEPv6 product. In my understanding, FLUXCOM uses more than one meteorological forcing as well as different machine-learning methods. Using all the FLUXCOM RS-meteo products could additionally provide uncertainty estimates for the presented indicators.

L. 122-136 If one of the aims is to compare FLUXCOM and CLM4.5, I would suggest comparing the two products during the same time period (i.e. 1990-2010).

L. 133 'match the available FLUXCOM dataset.' Spatially or temporally? As far as I know, the FLUXCOM products have a spatial resolution of either 0.5 or 0.0833 degrees (http://www.fluxcom.org/CF-Products/).

L. 140 equation 1: So U is conceptually GPP and R ecosystem respiration, right? I would be curious to see how GPP compared to U when U is computed as in equation 4 for a sanity check. Are they the same? In principle yes, right? Same for ER and R.

L. 143 I am not sure if this equation is written correctly. Assuming that U is supposed to be expressed in gC m-2 d-1, the way the equation is written suggests that the U would be expressed in gC m-2 (assuming that CUP is a length expressed in the number of days), which is then inconsistent with equation 4. Or did I misunderstand how CUP is calculated?

L. 144 The same applies to this equation.

L. 148-149 I think these equations are correct and good enough to explain how U and R are calculated, therefore I would discard equation (2) and (3) to avoid confusion. Again, U and R derived from equations 2 and 3 do not seem to match how U and R are calculated from eq 4 and 5.

L. 150-153 "Because many studies have [..] are tightly correlated" I would move this sentence to the introduction. I am also not sure that this is enough to justify the need to look at the relationship between annual NEP and the ratio U/R.

L. 160 This equation is correct if one assumes that equations 2 and 3 correct, and if I understood correctly their formulation, equations 2 and 3 are not (see comment above). Therefore, I do not believe that the ratio U/R can be partitioned as presented in equation 7. It seems that part of the paper is based on assuming that equations 2 and 3 are correct, therefore I have concerned related to the analysis relying on equations 2 and 3.

L. 171 I think the analysis presented in section 4 is not correct for the issues I have raised related to equations 2 and 3 at least the way equation 8 is expressed. One could express U/R = f(U/R, CUP/CUR) though and run the variable importance analysis. Why not just do the variable importance analysis as NEP = f(U/R, CUP/CUR)? I find it cleaner although it might be a bit circular and spurious as U and R are derived from NEP.

L. 186 I do not find this section relevant in the context of the study. Besides, most of the

presented results are already well documented in the literature (e.g. Jung at al. 2020).

L. 188 Be aware that the 'large carbon sinks' are very likely related to an artifact in the eddy-covariance datasets due to advection and storage issues. It might be relevant to discuss eddy-covariance data quality issues.

L. 204 Would that make sense to discard the sites for which the logarithmic function does not provide a correlation >0.9 for robustness?

L. 207-208 "This finding suggests that the mean annual ratio ln(U/R) is a good indicator for NEP and its spatial variation." Isn't it expected? I mean U and R are derived from NEP so you might expect that their ratio explains the annual variability of NEP, right?

L. 218 Again, is this analysis being done on the extracted time series for each Fluxnet sites or globally? If the former, I do not really see the point of included results based on FLUXCOM or CLM4.5 for the purpose of the study. It would be interesting to run this analysis both at the global scale and at the Fluxnet level.

L. 219 I do not think that one can directly compare the results from FLUXNET data and the two global products (i.e. FLUXCOM and CLM4.5) simply because of the strong bias in representativeness in the FLUXNET datasets. For instance, there are very few semi-arid ecosystems (e.g. 2 shrublands and 5 savannas in the presented study) in the FLUXNET dataset, while they represent a large portion of land at the global scale and have been shown to substantially control the interannual variability of NEP (Ahlström et al., 2015). Or do you extract FLUXCOM and CLM4.5 time series for each FLUXNET site location? If so, it is anyway not a fair comparison due to spatial mismatch as the footprint of a tower is definitely lower than 1 degree (CLM4.5) or 0.5 degree (FLUXCOM) spatial resolution. As previously mentioned, I would rather run this analysis globally and not only at FLUXNET sites to have a real added value by using global products such as FLUXCOM and CLM4.5.

3. Technical corrections

L.57 'However' does not sound appropriate. Maybe 'furthermore' or 'in addition'.

L. 62 'dramatic'. Try to avoid emotional semantic in a scientific paper. Maybe 'substantial' instead?

L. 77. replace Musavi, 2017 by Musavi et al., 2017

L. 104 'database' Replace database by product.

L. 119-121 Stand age information is mentioned here but is they even being used further in the analysis? If not, please remove it.

L. 154-155 'Then we found that annual NEP [...] (Figure S2).' To me, this already belongs to the results section.

L. 154 'the ratio U/R'. It might be relevant for the reader to see a sentence explaining the meaning of the ratio U/R. This explanation in L. 162-163 comes a bit too late.

L. 151-152 'the non-growing soil respiration' Is that what you mean here? Maybe rephrase.

L. 208 I would not say 'was well explained' but rather that the correlation was moderate (i.e. $0.3 > r > 0.7$)

L. 347 In Fig. 1, it is not clear to me what products are we looking at. FLUXCOM, CLM 4.5 or both? It seems to be FLUXCOM (L. 99) but please specify in the figure's caption.

References:

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