

Interactive comment on “Functional convergence of biosphere–atmosphere interactions in response to meteorology” by Christopher Krich et al.

Anonymous Referee #2

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This is a very interesting paper on biosphere-atmosphere interaction, but it is also a bit difficult for me to understand. I have some background in causality detection (Granger Causality, CCM), but still find the paper hard to follow when I read it for the first time. This is mostly due to the incomplete description of the methods the authors used. When I went back and read the paper the same lead author published earlier this year (Kirch et al 2020, BG), this paper becomes clearer.

The author used a causal relationship detection method, PCMCI, and quantify the interactions between biosphere and atmosphere (represented by the energy, water and carbon fluxes measured by eddy covariance flux towers). With the resultant 10038 networks obtained from PCMCI, the authors applied a dimension reduction algorithm and visualize and analyze these networks along two dimensions. By quantifying the

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2-dimension space into four regions, the authors can show the trajectory of biosphere atmosphere interactions changes through time and across different sites. The authors lastly claim that environment are the major factors that regulates the biosphere-atmosphere interactions, effect from the vegetation type is small.

This paper tackles a very important question, and use rather novel method. And also because of this, the presentation is not very clear. The final conclusion is drawn based on qualitative evidence which weakens the importance of this study. I have several comments below for the authors to consider.

1. From a reader perspective, I find this paper difficult to read. Clearly, there is a large gap between “what the readers know” and “what the authors assume the readers know”. For example, in the introduction (P2 L33) when the author first mention PCMCI, I would expect further explanation on this new method because it is clearly not known by most or at least some readers. Instead, the author did not give any explanation on this but just mentioned one paper. I think this is a good place where the authors can briefly explain what is the basic ideas behind this method and what kind of information it can tell us. With this in mind, the readers can better follow the research question. Another example is in section 2.5, the author mentioned a method called OPTICS, but also did not provide enough explanations. There are also some good practices to improve the readability. For example, in the last paragraph of the introduction where the authors describe the structure of the paper. The authors can define the aim of each section first before directly stating what they did in each section. This also apply for the method description which is very dense and filled with lots of jargons and acronyms.

2. The major conclusion of this study, to me, is not well supported. The authors claim that the biosphere-atmosphere interactions are determined mostly by the environment, with limited effect from vegetation types, etc. This is supported by the similar causal network shown in the 2-D space of the t-SNE. However, there are two problems with this. First, when looking at this network, much of the linkage are physically based, with limited effect of vegetation, for example, the relationship between T-VPD, T-H, Rg-

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T, etc. Vegetation would have limited effect on these relationships and for the other carbon fluxes related relationships, different biome types may have different linkage strength. For example, in Figure 1, when looking at link strength related to NEE and other variable, the patterns become more sporadic, especially at bottom left corner, these differences in responses may be caused by the vegetation types or differences in months, but may have limited contribution to the overall network. That is to say, the vegetation types can have effect on the biosphere-atmosphere interaction, but only contribute limited to the network evaluated, therefore, their effects are ignored. Another problem related to this is that the authors showed several cases of the change in climate can cause a shift in interaction in the t-SNE space. However, these effects are not quantitatively analyzed, how much of this change in interaction strength are caused by climate and how much is caused by differences in ecosystems. are they both significant enough? Without these information, the conclusion is draw without solid support.

Some detailed comments: P2L49, “one high dimension observation” is not clear. Is it better to say “one facet in the high dimension space”

P4L92, based on the information theory, the causal relationship that happens within the smallest time step of observation cannot be detected. For example, although we know that R_g has a causal relationship with NEE, but this happens in seconds or minutes considering the lags in measurements, this causal relationship cannot be detected by the algorithm and will be regarded as unidirectional. This need to be mentioned or discussed as a limitation for interpretation of the results.

P5L129, This is not clear enough, is computation efficiency the only difference? Why would it generate different results as compared to t-SNE?

P7 Fig 1, R_g can also be affected by the cloud which can be affected by ET, H and other factors. See Green et al. 2017 Nature Geoscience.

P11 Fig 4, Maj->May

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