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Interactive comment on "Causal networks of biosphere–atmosphere interactions" *by* Christopher Krich et al.

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General Comments

This is Ben Ruddell writing; I waive anonymity for this review. When I saw this paper come across my desk it caught my attention, because I have been working on similar topics and also following the authors' work for several years. In general, I like this paper and after reading it I would like to see it published in this journal, with some changes. This general area of work needs a lot more attention because of the promise of the general approach and the urgency of getting our inference and modeling right for this kind of complex and coupled system. Thank you for this effort! After working on this kind of paper for more than a decade (and contemplating many reviews of my

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Discussion paper

own work) I've come to the opinion that we need to move past a focus on innovating methods and toward the challenge of showing how the methods can be used to produce actionable and fundamentally novel insights- or to test process theories in science. If we cannot use advanced inference techniques to learn about these systems or critique previously inaccessible scientific ideas, these methods will continue to fall on deaf ears, so to speak. So, I challenge the authors and anyone else listening to move forward aggressively with the intent to apply causal networks (Process Networks) and advanced inference techniques to interrogate scientific hypothesis and learn about systems. The current paper could do more along these lines, with added investment, by (for instance) comparing its statistical results with expectations from climate or ecological models, etc. Before beginning the review, based purely on the expectations raised by the very broad title of the paper, I already had a few questions about the paper. I will pose and then evaluate those questions before moving on to line by line comments.

1. Is the now-substantial body of literature on this topic adequately summarized and cited, giving credit where credit is due?

Papagiannopoulou et al is cited twice, but the similar paper Seddon et al. 2016 is not cited; please cite appropriately. Please review GeoInfoTheory.org, which has a nice list of publications on related topics (https://geoinfotheory.org/reference-list/). In particular, there are a few recent papers that should really be cited appropriately in your paper, because they are recent and narrowly within the scope of your literature review; these treat global land-atmosphere interactions and feedbacks using similar methods to your own. Please describe in your introduction, methods, and/or results as relevant, how does your work relate to these? Yu et al. 2019 in GCB is especially important. A list of references that would seem to be highly relevant follows.

Brunsell, N. A., and Anderson, M. C. (2011). "Characterizing the multi-scale spatial structure of land-atmosphere interactions with information theory." Biogeosciences Discussions 8.2.

Seddon, A. W., MaciasâĂŘFauria, M., Long, P. R., Benz, D., & Willis, K. J. (2016). Sensitivity of global terrestrial ecosystems to climate variability. Nature, 531, 229–232. https://doi.org/10.1038/nature16986

Gerken et al. (in press) Robust observations of land-to-atmosphere feedbacks using the information flows of FLUXNET, NPJ Climate and Atmospheric Science

Garland, J. and E. Bradley (2018), Information Theory in Earth and Space Science, SIAM News, October 1st, 2018. Full Garland reference.

Gerken, T., Ruddell, B.L., Fuentes, J.D., Araujo, A., Brunsell, N.A., Maia, J., Manzi, A., Mercer, J., dos Santos, R.N., von Randow, C., and Stoy, P.C. (2017). Investigating the mechanisms responsible for the lack of surface energy balance closure in a central Amazonian tropical rainforest. Full Gerken reference.

Goodwell, Allison E., et al. "Dynamic process connectivity explains ecohydrologic responses to rainfall pulses and drought." Proceedings of the National Academy of Sciences (2018): 201800236.

Hlaváčková-Schindler, K.; Paluš, M.; Vejmelka, M.; Bhattacharya, J. (2007). Causality detection based on information-theoretic approaches in time series analysis. Phys. Rep. 441, 1–46.

James, R. G., Barnett, N. and Crutchfield, J. P. (2016) 'Information Flows? A Critique of Transfer Entropies', Physical Review Letters, 116(23).

Jiang, P. and Kumar, P. (2018). "Interactions of information transfer along separable causal paths," Phys. Rev. E 97, 042310.

Jiang, Peishi, and Praveen Kumar. "Information transfer from causal history in complex system dynamics." Physical Review E 99.1 (2019): 012306. Full Jiang and Kumar reference.

Knuth, Kevin H., et al. "Revealing relationships among relevant climate variables with information theory." arXiv preprint arXiv:1311.4632 (2013).

Kumar, P. and Ruddell, B.L. (2010). Information Driven Ecohydrologic Self-Organization. Entropy 2010, 12, 2085–2096.

Ruddell, B. L., Yu, R., Kang, M. and Childers, D. L. (2015). 'Seasonally varied controls of climate and phenophase on terrestrial carbon dynamics: modeling eco-climate system state using Dynamical Process Networks', Landscape Ecology, pp. 1-16.

Ruddell, B.L., N.A. Brunsell and P. Stoy (2013). Applying information theory to quantify process uncertainty, feedback, and scale in the Earth system. EoS, 94, 56. Full Ruddell reference.

Smirnov, D.A. (2013). Spurious causalities with transfer entropy. Phys. Rev. E 87.

Yu, R., Ruddell, B. L., Kang, M., Kim, J., & Childers, D. (2019). Anticipating global terrestrial ecosystem state change using FLUXNET. Global change biology. https://doi.org/10.1111/gcb.14602 Full Yu reference.

2. Is the concept and any methods used for "causality" adequately posed and defended?

You can expect such a strong claim and wording as "causal graph" to be aggressively challenged by readers and reviewers in this paper and any others using the term, for a long time to come. Renaming "correlation" or "information flow" to "causation" is a major and very aggressive departure from our disciplines' wording and conceptualization during the long and mature history of statistical inference, and requires very strong justification. Granger causality has never really been "causality"; it's a type of conditional time-lagged cross correlation. Please understand my point here; I'm not asking for you to give up on the use of "causal" language, but I am strongly requesting that you spend at least a paragraph in the introduction or methods section of this paper (and others, for the foreseeable future) to argue and explain to the reader exactly what is, and is not, meant by "causal" in this context. It is otherwise too strong a term to be using. As a more general comment, it's extremely important for us to reach a consensus about what to call things. This is an iterative community process of communication that works through conversation and engagement, and through clarification about what is the same and what is different. It's not my place to decide whether we should be calling something a "causal network" or a "Process Network", but I do insist that we have the conversation. For the purposes of this paper, this means citing my recent & prior work, and that of others, and trying to explain exactly how your terms relate to our terms for similar things, and proposing what you understand the similarities and differences to be; this is particularly important when writing a methods paper such as this one under review here.

3. Is there anything new here, and is that made clear?

Yes! PCMCI is put through some rigorious tests for both satellite and 30m flux data and appears to hold up well; this is novel and interesting as a methodological development. However, in my opinion, it is important when describing this method in the methods section that you distinguish it precisely and detail from other similar methods, explaining its relative advantages and disadvantages. There are lots of other methods out there that have used Granger-adjacent directed coupling statistics in various application contexts. In this precise context, my 2009 papers you cited (and several since) used 30 minute flux tower timeseries data to determine atmo-bio networks, identifying ranges of statistically significant time lagged couplings, and also studies periodicity and noise in the method, calling these resulting patterns "Process Networks", and distinguishing the most "causally" relevant couplings using a Tz metric that compares directed vs correlative information flows. This is a well worn topic in 2019, so it's not sufficient in a methods paper to contrast your method with correlations anymore. Contrast your method precisely against others that claim similar goals and results, please. Why should we use PCMCI instead of one of several other existing similar methods? How would the results differ in theory and in practice? Should we use PCMCI in this case, and use Ruddell et al. 2009a "Tz" in another case? What are the pros and cons?

Because this paper focuses on methods, it needs to be much more specific about how these methods relate to other adjacent methods and conflicting/overlapping terminologies already in use; this engagement is how we will build our community's knowledge and practice. (your treatment of the underlying assumptions is a strength of the paper and should help make these distinctions clear; thank you for this attention to detail here.)

4. Is the very broad title justified, or is the paper actually about something much more narrow and specific?

By the end of the abstract, I decided "negative" on #4 because this paper appears to be not a review or synthesis of the broad topic of causal networks in the bio-atmo-geo-sphere as implied by the title, but instead a methods case study establishing the robustness of a proposed method MCMCI in two land-atmosphere contexts. I suggest a much narrower title, like "PCMCI robustly identifies biosphere-atmosphere interdependencies", or some such. It is very important to use an accurate title that is not over-broad. The title directly summarizes the question and/or findings, in a nutshell. An overbroad or inaccurate title is grounds for rejection in my view.

Line by Line Comments

Sec. 2.1 I've followed the derivations in Runge et al. (various, 2014-2018) and I don't have a problem with the methods. However, I have not seen here or in Runge et al. (various) an explicit comparison of the MCI approach with Ruddell and Kumar's (2009a) "Tz" or zero-lag ratio method for the disambiguation of "strongly causal" versus "common-source causal" indicated couplings. There appears to be a lot of shared intent and intuition here, and possibly some very similar (but differently named) mathematics and assumptions. Please explain what is similar or different.

Pg.20-10 This discussion on "causal stationarity" and limitation of study to one season or system state appears to be treated in Ruddell and Kumar 2009(b) (second half of the paper you cited) under the terms "local" and "global" stationarity. What's the relationship here, please?

Pg.21-20 Although it isn't the focus of your paper, Kumar and Ruddell (2010, Entropy) and some of my more recent papers (Yu et al., Gerken et al.) have shown very strong changes in coupling strength across space, as well as across time. I wouldn't make the claim that "the interaction between biosphere and atmosphere is expected to change only marginally across space" in the absence of strong arguments supporting this. I've argued the opposite in several recent papers- I've argued that the Process Network characterizing these systems and their states changes dramatically between places and times, and that this represents a qualitative shift in how the systems are functioning. (note that I'm not arguing that physics changes, only that its structure and expression in a complex system changes dramatically) . . . please engage with this argument, or remove the claim.

Pg.21-25 Most of my papers have focused their analysis and presentation of results on a single "most significant" time lag (usually chosen as the first/shortest peak lag in my papers, called the "characteristic time lag" in my papers), or an average across a range of time lags (usually subdaily <18hrs) because of the extreme challenge of interpretation posed by a large number of statistically significant coupling links. Separating out every conditionally "momentary" coupling is not hard to do mechanically, but interpretation and communication is devilish. I think you're running into this problem here. Once we move past conditioning couplings on zero-lag correlations, it's not clear where to stop or how to interpret the results. I'd hope that PCMCI could add some clarity, but I'm not convinced based on this discussion that it is helping. Please comment and clarify if possible, or at least explain how what you're doing is different here from what Ruddell and Kumar 2009 did with T, I, Tz, canonical coupling types, and characteristic time lags. If possible, also engage with Goodwell and Kumar (recent) who have attempted to split out redundant, synergistic, and independent couplings in the land-atmosphere coupling context.

Pg.22-15 I am not convinced by biweekly or monthly scale correlation analysis in satellite or climate data represents causation in any real or approximate sense. There are several problems here. First, these data are modeled and abstracted several levels beyond primary observations, so patterns cannot be relied upon to strongly represent causal realities as well as in-situ flux data. Second, once we move past subdaily time lags, we are well into the scales dominated by diurnal cycles, synoptic weather cycles and by seasonal rhythms, so it is hard to distinguish signal from noise when the "noise" is an overwhelmingly energetic diurnal, seasonal, or synoptic cycle. Third, we already have strong reason to believe that the main process timescales are subdaily, due to e.g. our flux tower analyses, so we must presume that superdaily or monthly timescales indicated by the methods are merely echoes and confounding correlates of shorter timescale processes unless we can prove otherwise (e.g. through robust conditioning against shorter lags)- and that proof is not possible using coarse time resolution data. This is a basic problem with attempts to use satellite and coarse time resolution gridded data to establish "causal" relationships, and I haven't seen it adequately addressed in this paper or prior papers attempting similar. What am I missing here, please? Please explain how your method addresses these three problems. This gridded/monthly analysis may be a "bridge too far", so to speak, for this paper; it's different from and a weaker argument than your eddy covariance analysis, with several layers of practical problems weakening the conclusions.

Fig. 6,7 These results are begging for a detailed comparison with Yu and Ruddell et al., published earlier this year in Global Change Biology, which attempts a very similar analysis but uses an extrapolation of 30m flux data derived couplings to the global terrasphere rather than monthly gridded data. Please provide this comparison.

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