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Biogeosciences Discussions

Interactive comment on "Biosphere-atmosphere exchange of CO_2 in relation to climate: a cross-biome analysis across multiple time scales" *by* P. C. Stoy et al.

Anonymous Referee #2

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General Comments:

This work investigates the degree to which ecosystem-atmosphere CO2 exchange, and component productivity and respiration processes, are resonant with forcing by variation in environmental conditions at different time scales. Multi-year time series of half-hourly carbon fluxes (from eddy covariance) and environmental conditions measured at multiple sites are transformed with orthonormal wavelets to represent the spectra and cospectra needed for such an analysis. While technically sound and unique, the motivation behind this work and the understanding it offers are both significantly lacking. The hypotheses are weak and the conclusions are not well supported by the results. Taken together, the paper needs major revision. These criticisms are developed further below.

The manuscript states that quantifying the strength of the interaction between flux and climate variables at multiple time scales is necessary to begin to understand climatic controls on ecosystem dynamics. This is arguably not necessary. In fact, one interpretation of the final sentence of the abstract is that this analysis does not offer the mechanistic insights needed to understand climatic controls on ecosystem dynamics. Instead we are left only with ambiguity, absent of information about biophysical / ecological processes and mechanisms that give rise to the observed dynamics. The low-dimensional view obtained with the wavelet decomposition is proferred here to be an advantage, however it may not be so advantageous given its abstract nature.

Furthermore, the hypotheses are weak and not well motivated. Perhaps this is not hypothesis driven research and instead descriptive, which would be fine and in my opinion, certainly better than weak hypotheses. If hypotheses are deemed as necessary, the authors should hazard well-reasoned expectations. For example, it may be that temperate and mediterranean settings will have a higher peak at seasonal scale than wet tropical (e.g. EBF in Brazil). You might also hypothesize that places with high interannual variability in rainfall will have proportionally higher variability in GEP and Reco, but not in NEE because the process terms are offsetting. As it stands, the hypotheses strike me as rather useless.

Another concern is the inability to soundly address across-PFT differences in interannual variability. Section 2.4 describes how it was dropped from the wavelet-based analysis given inadequate sampling, and it was retained for the Fourier analysis despite dissimilar frequency bins depending on site-specific record lengths. Given these data limitations, it is an overstatement to claim that spectra diverge according to PFT at long time scales. It does not emerge from Fig 3 that PFT is a 'logical' or even predictively powerful explanatory variable for GEP or Reco. Statements to this effect should be removed.

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Finally, the spectral transfer and co-spectra analyses (Fig 5, 6) are misleading by being overly simplistic as a representation of system dynamics. Fluxes do not respond to only one of the meteorological variables but rather all of them in concert in some mechanistic way. For example, it is incorrect to suggest that Reco amplifies precipitation variability, when in fact Reco may be responding to something else entirely.

Specific Comments:

Abstract: Recommend the following change: "...significant divergence appeared among PFTs at the biweekly and longer time scales [suggesting what?]. At these long time scales, NEE and GEP are relatively less variable than climate, indicating some dampening through biophysical processes."

Introduction: 4098, Line 2, "alterations to their structure" to "structural alterations"

4098, Line 10, I'm not convinced that understanding the time scales of activity is really a major challenge, but surely the second point is, regarding the need to understand and represent the processes.

4099, Line 7: What is meant by "canonical frequencies", this sentence is full of unhelpful jargon.

4099, The discussion of deterministic versus stochastic drivers is off topic and does not really help organize thoughts about ecosystem responses to climate.

4100, Hypothesis 1 should be motivated by a process-specific expectation. Why should vegetation response to climate be less variable than climate itself? Of course the idea makes sense but it should be connected to a mechanisms that describes the dampening.

4100, In what way does hypothesis 2 follow from hypothesis 1? These are not well connected logically. Again, of course, it would be no surprise that some ecosystems will be more variable than others and at different time scales (highly seasonal, or large interannual variability in water).

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4100, H3 is not really an hypothesis. "... will be a logical way..."??

4100-4101: I find Analysis (3) to be unclear, primarily "...the low-frequency climate-flux relationship...". How does this differ from the cospectra or transfer functions at low-frequnecies?

4106, Statistical Analysis did not include 3.74 and 7.48 year time scales, but isn't this the time scale needed to evaluate the low-frequency climate-flux relationship(s), namely goal 3 and H3? Furthermore, using the Fourier coefficients seems bunk because the time scales are not aligned across sites, given the differing lengths of data records. Doesn't this invalidate the statistical analysis and the strong claim that wavelet spectra are dissimilar across PFTs at long time scales such as interannual?

4107, top, Is it correct to refer to a 'spectral gap' in the absence of a phenomenological expectation for variability at a particular time scale? It is not at all surprising to have lots of variability at the annual timescale relative to longer timescales. If we were talking about an energy cascade (i.e. Kolmogorov), for which energy is handed down from larger to smaller scales by a physical process, then sure, but in this case we do not have such an expectation so the expectation of always have more energy at longer time scales seems misplaced.

4107, and 4113 line 20: I found a particular point very intriguing and feel that it could be discussed futher. Across site variation in Reco variability continues to grow toward longer time scales, unlike for GEP or NEE. Why? Does Reco have a longer memory of historical disturbance and climate induced perturbations than does GEP? There are plenty of reasons to think this might be true (e.g. soils far from equilibrium).

Section 3.2: Most of the PFT stratification appears to be due to EBF. This should be mentioned. Furthermore, it suggests that the sizeable claim about PFT as a predictor. In fact, climate seems to be much better at separating OWT_flux at monthly to interannual time scales.

4108, Line 17-20: Table 2 reports only the interaction effects that are significant, however this is almost impossible to interpret w.r.t. mechanisms and driving variables. The text suggests that the results of the multiple comparisons tests are presented, but they are only shown with the lines on Fig 2.

4109, The precipitation spectrum is whiter than I expected but okay. The problem is that this result is not consistent with the explanation that there are multiple scaling laws across various frequencies, and rather suggests that there are _NO_ scaling laws to speak of.

4109, line 13: Cut the text about 3.74 y variability exceeding that at 1.87 y. It is not even true for GEP and NEE!

4110: The EST analysis is intriguing but offers an overly simplistic representation of system dynamics. Fluxes do not respond to only one of the meteorological variables but rather all of them in concert in some complicated, mechanistic way. In other words, it is misleading to suggest that Reco amplifies precipitation variability, when in fact Reco may be responding to something else entirely.

Figure 6. The Figure Label is incorrect. The three main subplots show not just NEE but also GEP and RE. Furthemore, the y-axis labels should reflect, not just the test of relations to MET variables, but also among the carbon fluxes (NEE,GEP; GEP,RE; NEE;RE). Maybe OWT_{NEE,X}, where X is MET or Flux.

Section 3.5, Analysis III is flawed in that the 'second-lowest' frequency differs among sites. If you are not comparing the same scales, how can you analyze differences across sites? This should probably be dropped from the manuscript.

Conclusions: The idea that "PFT is a scale-dependent concept" is presented in an ambiguous way and is not well supported or explained in the analysis. More importantly, it does not emerge from Fig 3 or the analysis that PFT is a 'logical' or even predictively powerful explanatory variable. This statement should be removed. Not only was Reconot clearly separated by PFT across time scales, but the same also holds for GEP and NEE.

Many aspects of the conclusions, mainly 4120 Lines2 - 20, are grandiose and do not follow from the analysis presented here, so should be moved to the Discussion.

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