

***Interactive comment on “Biosphere-atmosphere  
exchange of CO<sub>2</sub> in relation to climate: a  
cross-biome analysis across multiple time scales”  
by P. C. Stoy et al.***

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**Anonymous Referee #2**

General Comments:

*This work investigates the degree to which ecosystem-atmosphere CO<sub>2</sub> 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*

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*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 proffered 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 inter-annual 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.*

We disagree that quantifying the time scales of ecosystem activity is not an important contemporary challenge. Models consistently demonstrate a lack of skill at longer time scales when confronted with measured flux data (Hanson et al. 2004; Siqueira et al. 2006; Urbanski et al. 2007). Part of the challenge is that biological response to climate forcing via model parameters, rather than direct ecosystem response to climate, has been found to dominate interannual flux variability (Richardson et al. 2007; Stoy et al.

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2008), and this notion motivates our revised hypothesis.

That being said, we agree that the hypotheses were too general and lead to a discussion section that was not succinct or fully informative. We adjusted the goals of the analysis and hypotheses as discussed.

We do not agree that wavelet techniques for time series analysis are abstract and that the statistical results are ambiguous. The mathematics of wavelet analysis is well-developed (Daubechies 1992) and a recent ISI Web of Science search revealed over 44,000 publications that employ or discuss wavelets.

The point about the role of precipitation is interesting and we note that the Referees' notion holds at the Harvard Forest and likely ecosystems with seasonal drought of varying magnitude, but not on average in the other long-term measurement records (Figure 5).

*Another concern is the inability to soundly address across-PFT differences in inter-annual 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.*

The analysis of Fourier spectra from the eight long-running sites has been dropped from the analysis for consistency with the rest of the manuscript. It was introduced originally to address the very concern that most data records are insufficiently long to compute statistical differences at low frequencies.

From Figure 3, the magnitudes of NEE and GEP spectra are statistically-significant different among PFT at bi-weekly to annual time scales. Significant differences deter-

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mined by the mixed model may be considered cautious or 'conservative' given that an autocorrelative covariance structure was employed to compute statistical differences among the wavelet coefficients, which show little to no autocorrelation (they are approximate Karhunen-Loeve transformations (Katul et al. 2001)). The care taken in the statistical analysis should assuage concerns that the results are not robust.

*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.*

This comment is well-taken and reflects the fact that we did not sufficiently describe that climatic inputs need not be related to flux outputs (see e.g. Page 4105 Line 10 of the original manuscript) nor does correlation imply causation. Fluxes respond to a combination of meteorological variables (and VPD is in practice a combination of measured quantities) and the most robust method of quantifying these effects is through an ecosystem model, hence the CANOAK analysis. A full modelling analysis is not our intent and would be difficult to combine with the most data-intensive flux data synthesis to date: the entire time series of flux and meteorological drivers from all 253 sites are used in this analysis without the limitations of site selection. We limit the revised discussion to causal relationships.

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"*

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We made changes to these passages and others to improve readability. The divergence of the spectra by PFT at biweekly and longer time scales indicates that the magnitude of variability in these quantities differs among PFT.

*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.*

Representing the processes that occur at multiple time scales, including disturbance, is central to quantifying land surface fluxes. We place stronger focus on processes in the revised manuscript.

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

Canonical is commonly used in the mathematical literature to represent a natural way of conceptualization. Writing a polynomial equation with the highest order first is an example of the canonical form of such expressions. To avoid combining standard phrases from multiple fields of science in the interest of the wide audience of BGD, we removed this usage.

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

Climatic variability is characterized by both predictable and unpredictable events. Vegetation has evolved to respond to the former via circadian rhythms, canopy seasonality, etc. The topic of the manuscript is the response of ecosystems to climate across time scales, but in the interest of simplicity we removed the discussion of these distinctions.

*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.*

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The mechanism is ecosystem homeostasis (Levin 1998; Richardson et al. 2007), but it was decided while writing the manuscript that this concept is insufficiently mechanistic despite its historical application in ecosystem ecology (Odum 1969).

*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).*

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

The hypotheses were changed as discussed. With respect to H3, the Referee is correct in noting that the specific wording is difficult to formally test because 'logical' is qualitative.

*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-frequencies?*

Analysis 3 adds Fourier analysis to the analytical tools.

*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?*

The Fourier analysis of the long-running sites, especially when coupled with the CANOAK modelling analysis, suggested that low-frequency spectral peaks may develop in flux time series and strengthens the argument for making long-term flux measurements. The lengths of the data records did not align, unfortunately, and the results were never intended to be more than indicative to motivate potential future studies with

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longer flux records.

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.*

Spectral gaps in flux time series at time scales of a few weeks to a month (20 to 40 days) were discussed first by Baldocchi and others (2001b). Flux time series (at least in the temperate zone) consistently have more energy at seasonal and annual time scales (Baldocchi et al. 2001a; Braswell et al. 2005; Katul et al. 2001; Richardson et al. 2007; Stoy et al. 2005).

4107, and 4113 line 20: *I found a particular point very intriguing and feel that it could be discussed further. 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).*

A figure was introduced in previous versions that investigated this feature (attached); it represents simply the variance of the coefficients that are displayed in the box and whisker plots of Figure 1. The simplest explanation that follows from our analysis is that air temperature likewise has relatively high spectral energy at long time scales (see e.g. Figure 4), but this interpretation cannot exclude other mechanisms including disturbance effects or equilibrium assumptions.

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*

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*interannual time scales.*

The mixed model analysis did not test for the effect of individual types. Delving into a PFT-by-PFT analysis would make the manuscript substantially longer and we do not pursue this analysis further given existing concerns about length. EBF is significantly different than the other ecosystems at the time scales noted in Figure 3 when performing a less-robust t-test on the OWT coefficients.

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. (Note: Does the Referee mean Fig. 3?)*

The bars in Figure 3 correspond to the significant interaction effects listed in Table 2. Subsequent figures maintain this convention to signify when significant climate or PFT-related differences emerge in the spectra and cospectra.

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.*

This comment is inconsistent with the research cited in the manuscript.

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!*

The statistical significance of these increases could not be computed and we removed the statement. Our statement that mean 3.74 y variability of NEE is greater than 1.87 y variability is incorrect and we thank the Referee for the careful review.

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,*

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*it is misleading to suggest that Reco amplifies precipitation variability, when in fact Reco may be responding to something else entirely.*

Testing the limitations of the simplest system possible has intrinsic scientific value, as discussed. We now limit our discussion to mechanistic relationships. Unfortunately soil moisture time series are only available for a small subset of the database at the present.

*Figure 6. The Figure Label is incorrect. The three main subplots show not just NEE but also GEP and RE. Furthermore, 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<sub>NEE,X</sub>, where X is MET or Flux.*

The figure label and axis labels were revised to provide a more complete description: 'OWT<sub>NEE,X</sub>' was used in previous versions and we use this expression in the revised manuscript where it helps.

*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.*

We removed the Fourier analysis of the sites with long, continuous flux records. Analyses of this nature are better suited for more detailed future analyses after the data records of more sites has been extended, hopefully following the recommendations of the present 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 Reco not clearly separated by PFT across time scales, but the same also holds for GEP and NEE.*

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We removed hypothesis 3 in response to the general comments. The statistical analysis demonstrates that flux spectra do, in fact, differ among PFT at biweekly and longer time scales (e.g. Table 2, Figure 3).

*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.*

Rather than listing these sentiments as statements in the discussion, we added a Future Work section to describe implications of our results in the context of other recent research findings. Some of the more speculative low frequency findings also make their way into this section.

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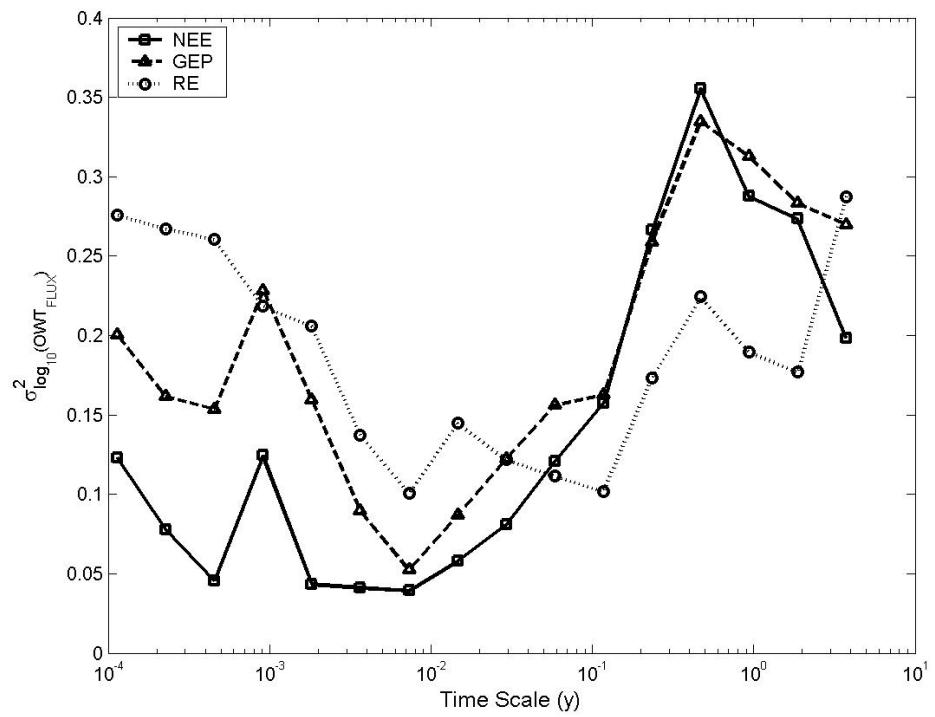


Fig. 1.

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