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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.

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Anonymous Referee #1

General Comments

The paper presents a time series analysis of comprehensive CO2-flux measurements. It addresses the question of whether ecosystem dynamics can explain variability in the biosphere-atmosphere exchange of CO2 at the global level. Three hypotheses are tested, namely that: (1) a significant gap emerges in the power spectrum of the CO2-flux at inter-annual time scales; (2) plant functional types (PFT) can explain differences in flux variability and co-variability with climate at the seasonal scale; and (3) discrim-

ination in terms of PFTs is a logical way to categorize responses to climate forcing. Spectra and co-spectra are obtained mainly through application of orthonormal wavelet transformation, and the ecosystem response to climate forcing is examined from the point of view of information transfer theory.

While the analysis is carried out on the background of a very rich data set – version 2 of the La Thuille FLUXNET database – and is probably unique in this sense, the results do not really disclose new aspects of the terrestrial carbon dynamics. Overall the discussion comes short in explaining how different mechanisms at the ecosystem levels act to mediate the climate signal at the various time scales. I am thinking here in particular at differences between plant and soil microbial processes – which are key to understand the specific behaviour of gross ecosystem productivity (GEP) and ecosystem respiration (RE) – as well as long-term (seasonal and longer) changes in the ecosystem composition (and possibly PFTs). For instance, given the contribution of soil respiration to RE, it is not surprising that the hypothesis of a characterization of RE variability in terms of PFTs at the seasonal and longer time scales is not supported by the results.

We concur that most findings are not novel. Rather than being a limitation we hold that this serves as a valuable proof-of-concept that results derived from OWT agree with accepted knowledge in most cases. This lends validity to the more novel findings, including the interesting results from the long term measurement records and the CANOAK modelling analysis that multi-annual spectral peaks may be a feature of flux records and the large spectral energy of RE time series at long time scales; These findings are novel for direct flux measurements, and we hope that these analyses help justify more long-term flux studies.

We also agree that a more mechanistic framework would improve the analysis, and we included CANOAK modelling to add mechanistic rigor. There is an inherent challenge with modelling the global FLUXNET dataset at the present, namely the lack of ecosystem level ancillary data and temporal changes therein that defines the state

of the vegetation and soils. These concerns are addressed in more detail elsewhere (e.g. (Williams et al. 2009)). To investigate a mechanistic framework more proactively, and to emphasize that we are seeking simplicity in a complex dataset, we test a revised hypothesis that follows from the results of Richardson (2007) (see also Stoy *et al.* (2005) and Law *et al.* (2002)), who found that the relationship between climate and flux diminishes at longer time scales.

The working hypothesis that ecosystem spectral transfer is a useful concept for analysis the spectral response also requires some additional explanations. As mentioned by the authors in the footnote on p. 4105, in information theory the concept is primarily applied to linear, time-invariant systems. In view of the complexity of ecosystem dynamics, the assumption that terrestrial ecosystems can be considered as linear and time-invariant should be motivated and not taken for granted. From a formal point of view, the paper is well written; in this sense I have no specific comments. Nonetheless, I believe that the main message can be conveyed more concisely. I therefore recommend shortening the manuscript.

The comment on the usefulness of the EST analysis is well taken, and it is apparent that we did not sufficiently explain the concept and its implications noting also the comments of Referee #2.

In brief, ecosystems are not LTI systems across all scales in time and we would not argue otherwise. But certain concepts from systems theory hold when discussing ecosystem behaviour. LTI systems are arguably the simplest analogy for ecosystems and a primary goal of science is to find the simplest possible description of natural systems. If elements of ecosystem function conform to linearity and time-invariance, then ordinary differential equations can be used to describe their behaviour. ODE's are, of course, fundamental for ecosystem modelling (O'Neill 2000), but a full discussion of systems theory in ecosystems science is not our goal.

The comment to shorten the manuscript is well-taken, and we combine this require-

ment with the suggestions of Referee 2 to create a more succinct analysis.

Technical comments

Relevant results (significance of differences between among climate drivers or PFT) are displayed with horizontal bars in the Figs. 3, 5 and 6. This is a very effective way to pack the information, but in practice it is not always easy to discern the details – unless the figures are greatly magnified. (This applies in particular to colour-blind persons.) In addition, while this approach does indeed show whether there is or not a difference, it does not indicate which is the difference. As it is difficult to propose an alternative that is as concise as this one but less difficult to read, in view of my second comment I was wondering whether the horizontal lines could be skipped altogether, and the relevant information simply provided in the text.

Previous versions of the manuscript included large (17 by 11 cell) tables of statistics for the EST and cospectral analysis. Interpreting the tables was cumbersome. It was decided that this information could be presented more concisely, and the strategy of including bars in the figures was introduced. The information in Table 2 corresponds to the bars in Figure 3 to help guide the reader. Table 2 is itself a simplified version of a full table of p-values, which is extraneous because of the significance threshold (0.05). All figures were created with both red-green and blue-yellow colorblindness in mind. All data figures are very high resolution, and magnifying the figures on the computer screen reveals all important details. Unfortunately adding this information to the text would lengthen the manuscript substantially and would conflict with the well-taken suggestion to produce a shorter manuscript.

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