Biogeosciences Discuss., 4, S1238–S1246, 2007
www.biogeosciences-discuss.net/4/S1238/2007/

Biogeosciences
Discussions

Interactive comment on "Characterizing ecosystem-atmosphere interactions from short to interannual time scales" *by* M. D. Mahecha et al.

M. D. Mahecha et al.

Received and published: 23 August 2007

The authors are sincerely grateful to the in-depth reviews provided by Dr. Milan Paluš and two anonymous reviewers. Although all reviews advocate the publication of our manuscript in Biogeosciences (BG), some include major comments which (in part) make reference to very different aspects of the work. We have attempted to incorporate all of these concerns into the revised manuscript. Reviewer #1 also asked for a language revision of the manuscript, which has now been performed by a native speaker. In the present responses, letter reviewer concerns are briefly summarized or cited in **bold** type.

Detailed Responses to Reviewer #1

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Discussion Paper

1. **Missing discussion of SSA versus wavelet analysis: "A more thorough discussion of the benefits and drawbacks of SSA versus wavelets is necessary."** We agree that wavelets were not adequately discussed in the BG discussion paper, given their prominence in the literature on eddy covariance (EC) data. The revised manuscript now includes a section ("Comparison of SSA with wavelet analysis") that takes this criticism into account and clarifies the complementary character of SSA and wavelet analysis. We also show that both approaches can be understood and applied in a common framework.

2. **Critiques on analysis and discussion of gap filling: "I would prefer that the gap filling discussion be removed entirely as it is tangential to the present manuscript. The use of SSA to fill gaps would be more appropriate for a different manuscript."** We agree that an in-depth analysis of SSA gap filling for eddy data requires a new study. However, to the best of our knowledge, this is the first paper to apply SSA to eddy covariance data. Since an analysis of EC data is always influenced by the occurrence of notorious gaps, and because a proper SSA gap filling technique had just been published, we took the opportunity to implement it. This can be regarded as part of data preprocessing and thus had to be included. We argue that the gap filling part of the manuscript is not "tangential" for the data at hand since the original SSA simply does not work with gappy data. However, we reduced the bulk of SSA gap filling in the final manuscript. We pointed outthat while the SSA gap filling procedure was necessary, it was not the actual focus of research. The revised manuscript now places less emphasis on gap filling, since this was not a major objective of the paper.

   Assuming our presentation of data processing was not clear in the BGD-MS, we therefore restructured and reworded this part completely (see Section 2.3).

3. **What are the significant modes of the R_{eco} model without measured night-time values?** Flux partitioning was carried out based on the standard Arrhenius model as formulated by Lloyd & Taylor. The point is that we worked with daily

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aggregates, which meant that a separate SSA of daytime and nighttime fluxes was unfeasible.

4. **Most passages were poorly worded.** The new version of the paper has been edited by a native speaker.

5. **Setting of P (the “embedding dimension”) and sensitivity of results to its size?** The “embedding dimension” P is usually based on heuristics. We have now included a paragraph in the methods section (2.1) that provides details on the setting of P, and we discuss this further in Section 3.4.

6. **Description of the principal components A.** The description has been enhanced in order to make this equation more comprehensible for those not familiar with PCA.

7. **Doubts on the definitions of L_t and U_t.** The definitions have now been summarized in detail in Table 1. We agree that here, too, the definitions are still compact. We therefore refer the reader to Ghil et al. (2002). However, we believe that there is some degree of misunderstanding: the “correction” is not an approximation or a necessary simplification, but rather the correct handling of the boundaries. The results are not influenced by these “corrections” since this is the only accurate way to do it.

8. **Page 1415, line 2: “I do not doubt that the flux time series contain modes of variability beyond the frequency of what the measurements and SSA technique can determine, but do these significant modes represent a real signal or edge effects in the analytic procedure?”** To our understanding, these modes represent real signals but at the edge of the frequency detection. Their proper interpretation is that modes with a period longer than the window length (in this case, 3.4 years), have been lumped together. Their total contribution, however, is correctly summarized through the two corresponding eigenvalues. It can

be seen as an advantage of SSA that even modes which are not reconstructible in the time domain are at least detected.

9. **"[A] brief appendix with pseudocode may make the algorithm easier for others to understand and apply."** We have added an Appendix with a pseudocode, which should facilitate future applications.

10. **"I, too, am confused as to why T does not vary at low frequencies. Could the authors test this against a T time series that is known to have interannual oscillations to demonstrate the fidelity of the SSA approach?"** We do have (plenty of) T series with long-term structures, and several of the papers cited indicate this as well. However, this is a more or less trivial exercise–SSA will find the modes up to the window length. In the present case, the review by Dr. Milan Paluš provided several possible explanations, which we have included in the manuscript.

11. **1415, 27: Please describe the multiscaling behavior in P in more detail; can SSA quantify this behavior?** We have now provided more details on the study by Peters et al. (2002). The multiscaling behavior is not, however, detectable in the present study, since we work with daily aggregated data.

12. **"I question the exploration of R_{eco} and *GPP* time series given that these are (usually) not directly measured products (especially the latter) and are instead the output of a model that is driven by a climatic variable which, in the case of GPP, is again filtered through the NEE time series...."** We are aware of the fact that these variables are not "measurements" but modeled fluxes. However, both represent a fraction of the *NEE* time series and are frequently used to characterize ecosystem-atmosphere interactions. There is no reason for not analyzing derived time series from a real measurement.

13. **How was it determined that high-frequency components are essential to to-**

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tal variance for *GPP*, *H*, etc., and what is meant by "essential"? Determination means quantification through the (sum of) eigenvalues for the high-frequency components. We reworded the paragraph to provide separate discussions of the significant high-frequency components and the high-frequency components that are compatible with the red noise assumptions. The word "essential" has been avoided in order to clarify the section.

14. **"I like the *NEE-T* analysis. Would a residual analysis between these terms (and others) display complimentary or new information?"** We have shown that the signals leading to the *NEE-T* analysis are subsignals of the original series. The residuals would be the respective time series where we remove these components. The red noise components are fractions of the time series which were not included in the analysis of the hysteretic behavior and represent what one could here call a residual time series.

15. **"Pronounced hysteresis in the *NEE-u* relationship may not be causative, and may simply reflect the fact that both show some seasonal patter. I doubt this has anything to do with ecosystem 'memory'."** We agree and have therefore removed all speculation regarding the *NEE-u* hysteresis. But we would like to stress that throughout the whole study, an entirely data-driven approach was used to reveal unexpected patterns previously hidden in the raw data. We made no claim that any of the scale-dependent relationships shown were causative!

16. **Minor.** All minor comments have been incorporated into the revised MS.

Detailed Responses to Reviewer #2: Dr. Milan Paluš

1. **"Just a remark to already-mentioned confusion with respect to T and N for the total length. At the top of p. 1410 (l. 5) N is used, while in eq. (6) T**

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is used." The confusion generated by misrepresentation between T and N has now been eliminated; T is now just temperature, and N is the time series length.

2. **"Another small remark: p. 1420, l. 21-24: Paluš and Dvorak (1992) showed that SSA cannot be reliably used for estimating the dimension of nonlinear systems."** We think there has been a misunderstanding: it was not our aim to use SSA for estimating the dimension of a nonlinear system. We discussed alternative extensions of SSA to achieve nonlinear generalizations of the extracted modes, all of which are tied to the problem of linear versus nonlinear dimensionality reduction.

3. **P. 1415, l. 15-19. "Not only the length of the record plays its role, but also the position in both time and space–the presence of some oscillatory modes in the *T* record changes with the geographical location (Paluš & Novotná, 1998) and the relative variance of a particular mode changes in time...."** This concern is now mentioned in the respective section.

4. **Relations of oscillatory modes: nonlinear?** This issue was one of the main concerns in the review by Dr. Milan Paluš. He showed that the detected patterns do not necessarily have to be generated by a nonlinear relationship. He furthermore demonstrated ways of generating similar patterns using entirely linear transformations. We agree that the statements in our paper were not well-founded in terms of theoretical accuracy. The original idea behind that was that we had observed nonlinearities that were obviously "time-instantaneous." Our intention was to show that we are dealing with a problem that cannot be described by means of simple linear regression models, as is the standard case. In the revised manuscript we have removed all of the statements under criticism. The proposed tests on nonlinearities are not essential for the present paper since they do not infer our results. However, we agree that such advanced test statistics might enhance further studies.

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5. **Synchronization analysis** The review points out the potential of analyzing the phase synchronization between the available time series (and extracted RCs). We are aware of these approaches and the respective literature. Indeed, before submitting the manuscript, the first four authors submitted a research proposal focusing on phase synchronizations in eddy covariance data. Our next paper will hopefully include this analysis. We, too, consider this highly promising for the characterization of ecosystem-atmosphere interactions; however, we do not see any merit in including it in the present study, where the focus is the exploration of SSA for extracting intermediate to low-frequency components of short EC time series.

6. **"In summary, in order to characterize the ecosystem behavior as highly nonlinear (p. 1417, l. 22), the authors should provide results of more detailed analysis and quantitative/statistical testing. I tried to point out some possibilities using the above examples. Of course, the authors are not obliged to use any of the above-mentioned approaches, but can find inspiration in extended literature on nonlinear dynamics and nonlinearity detection in physical or statistical literature."** We appreciate the comments very much, and they have certainly widened our horizon. Again, we would like to justify not having used the proposed methods here, as our goal was not to characterize a relationship as "linear" or "nonlinear." Rather, our aim was more to show the potential of SSA, providing the basis for the investigation of time-scale-dependent fluxes, which in a next step could involve much more process-based analysis on the one hand, and more statistical sophistication on the other.

Detailed Responses to Reviewer #3

1. **Introduction of Eddy Covariance Term.** We modified the first sentence in order to clarify that Eddy covariance is a measurement technique described in the references provided.

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2. **Convergence of gap filling.** Indeed, in the original paper no criterion is supplied in the formulation of the algorithm. One can only guess that it might be the RMSE. However, we asked one of the authors of original paper (Dr. Kondrashov) and he agreed that the use of R^2 should yield almost similar results.

3. **Sec. 2.3: "Here more details are needed; would be very instructive to plot one or more raw time series before gap filling and the end result also showing the SSA parameters that have been used."** We completely reworded the section in order to clarify our two-step gap-filling strategy. The two-step data filling at two levels of aggregation furnishes a plot of the time series that is not very intuitive. The problem is also that the missing data are quite random, thus the representation did not provide any insight. The plot in which we showed the performance of the gap filling with an artificial gap (Fig. 5, in the discussion MS) was removed due to severe criticism by reviewer #1 of our presentation of the gap filling.

4. **"It should also be made clear that univariate gap filling has been used"** This has now been clarified.

5. **p. 1414 l.11: "Note that Ghil and Vautard (1991) deal with the hemisphere mean temperatures, while here it is a local record**" This has been clarified.

6. **p. 1416. l 24: "Fig. 6 can help to explain Fig. 3 in some way, so perhaps these figures should be combined. Caption of Fig. 3 is not consistent with its description."** We see the point, but Fig. 6 was meant to serve as an outlook figure showing further advantages of SSA. We have, however, restructured this part of the manuscript. In the revised manuscript, the discussion of the deviations immediately follows the hysteresis plots, and both sections have been linked by a number of discussion points.

7. **Sec. 3.5. "Should mention how much missing data is missing (see also**

my comments to Sec. 2.3).” Again, this is not particularly relevant since we have two levels of missing data that vary for each time series investigated. Furthermore, missing data is not necessarily missing, but can simply be a highly uncertain point due to advection.

8. **Minor.** All other points have been incorporated.

References

[Ghil et al.(2002)] Ghil, M., Allen, M. R., Dettinger, M. D., Ide, K., Kondrashov, D., Mann, M. E., Robertson, A. W., Saunders, A., Tian, Y., Varadi, F., and Yiou, P.: Advanced spectral methods for climatic time series, Rev. Geophys., 40, 1–25, 2002.

[Peters et al.(2002)] Peters, O., Hertlein, C., and Christensen, K.: A Complexity View of Rainfall, Physical Review Letters, 88, 018 701–1–018 701–4, 2002.

Interactive comment on Biogeosciences Discuss., 4, 1405, 2007.

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