Reply to the Review of Dr Werner Eugster

Ref: bgd-8-C3770-2011.pdf

We thank Dr Werner Eugster for his comments, clarification of modeling concepts and constructive recommendations for manuscript revisions. In this reply we address the suggestions for revisions of the manuscript point by point. Reviewer comments are given in smaller italics; our responses follow in normal font. Because some remarks here were in line or related to the comments from other reviewers, we also refer to those comments during our response.

> *In the abstract the authors note: "The possible causes for the observed functional change could not be addressed with the available dataset. This demonstrates the need for more targeted experiments, such as long-term measurements of leaf nitrogen content." – This is probably the statement where the conflicting views of Reviewer #2 were initiated. In general, of course, one wants to find out these functional changes, and if you have to state that you failed to crack those, then of course one could question the model or the scientific approach. I however do not see this as a reason to reject the manuscript. As Karl Popper puts it, the falsification theory actually suggests that we make more progress if we can falsify our hypothesis or approach than if we have to keep our alternative hypothesis. The reality, however, is that we normally do not publish negative outcomes.*
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> ***Specific recommendation 1:*** *Rework the manuscript to clearly make the point that this manuscript shows the progress on a long way to finding the link between interannual variability and ecosystem response. It is a trial and error approach, for sure (we also found that mechanistic models such as the LPJ-GUESS completely fails to model our subalpine forest at Davos, 1690 m asl in Switzerland, although it was rather successful for boreal forests with similar plant species combiations). But you must make clear that you carefully designed your study and that it was not foreseeable that the tools you're using won't actually solve the question that you were asking*

The main critique on our manuscript (also from other reviewers) was that the study did not make substantial progress in finding the fundamental mechanisms between interannual variability and ecosystem functional change. This impression was initiated most likely because we made a very negative statement in the abstract, that we were not able to evaluate what caused functional change.

Indeed, we accept that we should avoid making such speculations, which was beyond the framework of the analysis and the specific study objective. This study aim to find out how important is the functional change rather than what caused it. From the feedbacks received from the reviewers, we realized that we did not describe this scope clearly enough in paper to inform the reader that analysis has limited capacity in exploring the mechanism of functional change. Meanwhile, we also see that we did not highlight the value of this study clear enough to show why our results makes an important contribution. This causes the impression from the reviewers that we were simply repeating previous analysis at a different site. The specialty of this study were briefly discussed in the response to Reviewer #2 (Wu et al. 2011, bgd-8-C4052-2011).

Therefore, we will clarify the scope, value and limitation of our study in the revised version and avoid making unnecessary speculation.

> ***2.*** *I also think that there is always confusion in concepts and terminology if modelers and experimentalists get in touch with each other. My recommendation is that you add a better theoretical description that clarifies the issue with the concept you are using. Although parameter extraction is widely known in the scientific community, it is not a quick deal to find the relevant references to build the conceptual construct that you need to address the critique of Reviewer #2 (and which also convinces him/her). But if you can show the conceptual framework (maybe with examples) and clearly address the issue of static vs. dynamic models, then I think this paper would be very helpful for others. We (Eugster & Zeeman 2006) once made an effort in a similar setting to show how dynamic model approaches can be of help to understand eddy covariance time series.*

This is a very good suggestion. We will explain the choice modeling approach with respect to the goals of the study and describe the meaning of the parameter values more accurately in a revised manuscript.

> ***3.*** *One problem we still face in general is that 13 years of data are still way too short to have a full picture of the life cycle of a forest, and for a good statistical characterization of interannual variability. So you may have to expand your introduction to better introduce this universal problem, before you focus on your specific problem and science question. Also be careful in writing choices. If you write "It is important to*

jointly consider. . . " (line 18, page 9128) but then have to admit that it did not work, then you lack some internal consistency. Try to reduce all nonneutral statements and claims that are not substantiated either by evidence or your results to a minimum. It is always safer to say "some authors claim that this and that is important" than to make an absolute statement that does not allow for exceptions.

This advice is greatly appreciated. It is true that 13 years is still a short period from the perspective of climate or forest development, although the data set we analyzed represents one of the longest eddy flux time series available. Our analysis represents the behavior of a deciduous forest at its mature stage (90 years old); of course the responses of young deciduous forests may be different. We will address this issue in the introduction during the revision. We will also be more careful with the writing choices.

4. *You introduce the relevant aspect of time lags (pages 9127–9128), but then you do not pick this up in the work you describe. You simply state in Section 4.4 "To better understand these indirect and lagged processes, more targeted observations and data are needed." – If you know this is relevant (and obviously you did lagged correlation analyses), why do you not elaborate on this? In a revised manuscript I would expect that you address the lag issue more clearly. As is, I am not that surprised that Reviewer #2 go the impression that you stopped too early with your analysis. Don't let you get under pressure to publish a paper every specific time unit, if there is still relevant work to be done that would help the case. And if you did all the analyses but they did not succeed, then you must solve this internal contradiction. Let the reader know what you did and why it did not work out in the best of your understanding. Be aware that most journals warn the authors that they are not accepting conclusions (in your case I am referring to the conclusions that you draw from Section 4.4) of the type "more data or more research on this is needed". With such statements you immediately will feel the breeze of the critical reviewers.*

In this study we considered that both the instantaneous and lagged indirect ecosystem responses as the aggregated biotic ecosystem response and was represented by the fitted model parameters. This position was stated in the introduction section (Page 9128, line 22), although we agree that we need to make this clearer. With this aggregation, we were able to distinguish the impact of climate variability and ecosystem functional property on the inter-annual variation of carbon balance, which was the key research question of the study.

We discussed lagged effects as a possible cause of functional change, very briefly in section 4.4. This section has initiated many critiques on our study. One thing we need to clarify again is that the goal of this study was to evaluate how important is functional change. It is true that after the analysis was finished, we thought it might be interesting to try lagged correlation between the parameters and climate time series to evaluate, e.g. whether the instantaneous maximum photosynthetic capacity during summer was strongly correlated with previous temperature anomalies. This was a natural reasoning during the scientific work but unfortunately, we cannot interpret the resulting correlation coefficients as cause effect relationship of ecosystem internal dynamics. For these reasons we did not include the lag correlation analysis in the present manuscript.

Nevertheless, we clearly aware that during revision, we need to more carefully address why it is difficult to evaluate the lagged response and cause of functional change, rather than making vague statement such as “more data or more research on this is needed”.

5. *In your conclusions the combination “process-oriented empirical analysis” sounds at the edge of a misnomer; what would be the inverse of a “process-oriented empirical analysis”? This would be a “non-process-oriented empirical analysis” or a “process-oriented non-empirical analysis”. I would have a hard time to define either of them! Maybe introduce this term with a reference in your introduction (if this is a sound term), or be careful with introducing such new terms or topics etc. in the conclusions. Best would be to only use terminology and results that were adequately covered in the text. Also sharpen your conclusions to be more quantitative, if possible.*

Thanks for the critique; we agree this term was somehow confusing. We will replace this with established term “The analysis based on semi-empirical modeling enabled….”. We will also add more quantitative results to the conclusion.