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Biogeosciences Discussions

Interactive comment on "Effects of climate variability and functional changes on the interannual variation of the carbon balance in a temperate deciduous forest" *by* J. Wu et al.

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1 Declaration

I was contacted by the first author after he received the first (or second?) reviewer's very negative review on his manuscript. I have met the first author a few weeks earlier in the ABBA summer school at Tuczno, Poland, where I was invited to hold the lecture on models and modeling in a summer school that focussed on eddy covariance flux measurements. Since a couple of the statements made by the first (or second?) anonymous reviewer are in stark contrast to what I tell students, I feel obliged to clarify a few things and then provide my assessment of the manuscript under discussion.

Please note that there is a mess on the BGD website with respect to numbering the reviewers. http://www.biogeosciences-discuss.net/8/C3220/2011/bgd-8-C3220-2011.pdf states that this is reviewer #1, but on the web site he/she is referenced as #2, and vice-versa for referee #2/#1 (http://www.biogeosciences-discuss.net/8/C3729/2011/bgd-8-C3729-2011.pdf). So, in what follows I will refer to the reviewer who produced bgd-8-C3220-2011-suplement.pdf as "Reviewer #2".

2 What is a model and what is modeling?

Reviewer #2 makes a few statements that I'd like to comment on in the hope they help the discussion of the manuscript that Wu et al. submitted.

"Unfortunately, this is just curve fitting not modelling" – I tend to refer to Legendre & Legendre (1998) who have a very good overview over what a model actually is, and hence if this reviewer does not consider curve fitting to be part of modelling, then he/she seems to be from a different school and must explain this first to the readership.

Legendre & Legendre (1998) name three types of simulation models: logical, theoretical and "predictive" (or numerical). The "predictive" models are further divided into two types: application models and calculation tools. Legendre & Legendre (1998) call application models those which "are based on well-established laws and theories, the laws being applied to resolve a particular problem" (page xiii of the Preface in Legendre & Legendre (1998)). The calculation tools (also called forecasting or correlative models) "do not have to be based on any law of nature and may thus be ecologically meaningless, but they may still be useful for forecasting. In forecasting models, most components are subject to adjustments, wheraeas, in ideal predictive models, only the boundary conditions may be adjusted" (also page xiii).

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My best guess is that Reviewer #2 confused the various types of modeling.

"True model parameters should be constants" – In a static model I would agree, but not in a dynamic model. Now comes the problem: often we approach a dynamic system by using statical models (e.g. statistical regressions) that have a limited validity. So, within the scope of the application of such a model, the parameters are pseudo-constants. Now in my view there are two modes how to work with such a model: either provide a prediction using parameters that were obtained otherwise, or perform a parameter extraction via a stastical fit (e.g. least squares fit or another suitable method to search for the local minimum of residual variance). If the scope of the parameter extraction is specified (number of days for which the parameter set in the model is considered to be constant) as in the approach by Wu et al., then one can start to investigate the temporal evolution of these parameters.

There is nothing wrong with this, we do this all the time with our modeling. E.g. when looking at flux data we can treat evolutionary adaptation of our species under investigation to be so slow that we can neglect it. Or a comparison with our colleagues in geology: we consider space to be a constant (mountains and hills stay in place, they do not move, so the are constants in our models), whereas geologists working with models covering millions of years need to get the variability of the topography (and tectonical drifts) explicitely included as variables in their model. In summary: models are always simplifications of reality, and we should be debating about how simple models should be so that they are still a helpful tool to increase our understanding of ecosystem processes.

"This cannot be correct..." – I tend to think of models to be simplifications of reality, so there is no such thing as a correct model. A model may be more sophisticated or overly simplistic. Here again my best guess is that since Reviewer #2 uses a different philosophical concept he does not consider empirical simplifications to be valid, but I would strongly contradict here. Empirical approaches fall under the classification of "calculation tools" in Legendre & Legendre (1998), but as they state such models can still be useful, and I expect the authors to show the reader that they actually are. Or at least, that it is a suitable approach for a focused question, although it may not be the universally valid approach for all types of ecosystems and all climate zones.

3 Recommendations for revisions

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.

For the revisions hence my recommendations are:

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

2. I also think that there is always a 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.

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 woring 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 non-neutral 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.

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

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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.

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 interoduction (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.

Werner Eugster

4 References

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