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Biogeosciences Discussions

Interactive comment on "A model-based constraint of CO_2 fertilisation" *by* P. B. Holden et al.

Anonymous Referee #2

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Main comments: The paper uses a top down approach to explore past changes in GPP using Baysian approaches to constrain model parameters by observed CO2 changes. The approach is complementary to bottom up DGVM approaches (global) and site based data. There is certainly a contribution that this paper makes to the wider field. I have a number of specific comments below that it would be good to see addressed by the authors. In general, the authors could do a bit more in terms of pulling out the real world implications of the parameters explored (too much discussion of "KC", for example, rather than the process it represents). I also feel that the paper misses a few big results. There is an opportunity for example, to pull out global GPP estimates from this top down approach (something which would complement current bottom up estimates. However, these points aside, the paper would fit well within BG.

Specific points: Only a subset of parameters are constrained against an observed timeseries. How important are KC and VPC relative to the other parameters that are also sampled? Some brief discussion on this would help provide the context.

The GPP is expressed as a percentage change. While this is interesting it would be good to see what absolute values are inferred for GPP. Observations give sparse sample points for the real world and there is a tendency for current DGVMs to reproduced global values in the ball park of earlier studies. The paper motivates looking at CO2 change rather than CO2 absolute. Is this the same motivation for showing GPP changes but not GPP?

The marginal probability distributions ('marginal' needs to be added in the text to clarify that the pdfs are conditional on the KC and VPC constraints in this study) show a very spikey surface. This I find highly surprising. Is a land-atmos flux of -1.5 and -1.0 really more likely than -1.8 and -1.2? This seems to suggest that some aspect of the resultant pdf is over constrained. Is this due to an under-estimate of the structural uncertainty that leads to this?

Does the addition of 4 new parameters (VPC, ALUA, etc) impact spin up states, or are they only influential in the response? Has any control drift in the spinup states been accounted for?

A table summarising the 4 experiments would be useful (currently described in 9433, paragraph starting on line 6). Reading linearly through the manuscript these 4 experiments are not well motivated. what do they do? How do they fit into the flow chart in figure 1?

Page 9434, line 10: Which comparison suggests that parametric is greater than scenario? Clarification needed in text. (which study?)

Language: The authors need to pull out the inferences more fully. For example discussion p9434, line 23+ discusses uncertain parameters VPC and KC. Translating this into language the more casual reader would understand – they are suggesting that representation of CO_2 fertilisation and some aspect of LUC implementation (the authors do not specify). Relating these parameters to the processes they represent is important if the import of this work is to be appreciated by a wider audience. What KC uncertainty represents should certainly be discussed. Later the authors note that very high KC values are not physically reasonable – again earlier discussion of what KC is would help the reader understand what is being discussed. And linking back to the process, rather than an obscure parameter name with the important discussions would help.

How sensitivity are the results to assumptions of the prior? The authors use a normally distribution. What is the justification for this? And are the results dependent on this assumption (i.e. are the results strongly observationally constrained or not?).

The inclusion of structural error differs from that taken in Murphy et al, in that the C4MIP spread is used (scaled). Murphy et al, estimate a distribution of structural errors by quantifying the distance between each Multi-model simulations and the best emulated response from the perturbed parameter ensemble. The current manuscript notes that the EMIC showed no systematic difference with AOGCMs – would the structural error diagnosed in the Murphy method lead to a different estimate? A related question, would differences in the structural error term influence the results?

Interactive comment on Biogeosciences Discuss., 9, 9425, 2012.