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Biogeosciences Discussions

Interactive comment on "The climate dependence of the terrestrial carbon cycle; including parameter and structural uncertainties" *by* M. J. Smith et al.

M. J. Smith et al.

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Response to I. C. Prentice comments

Thank you Colin for your valuable comments. We will respond to every one of your concerns by making changes to the manuscript and we will respond again in a detailed response letter specifying precisely how we have done this – provided the journal approves publication and invites us to submit a revision. However for the purposes of responding in a timely fashion we will provide preliminary responses to your comments here. Broadly, your comments about improvements in how we could better represent carbon fixation, on our lack of use of carbon flux data, and on our comments about CO2 fertilisation all underline to us that one of the most important next steps will be to extend our approach to assess the implications, benefits and costs, of including differ-

ent eco-physiological details in the carbon model (such as assessing more mechanistic representations of photosynthesis). We hope that by now establishing our methodology to move forward with carbon model development we can more rapidly and rigorously make such assessments.

——— Comment: "My principal concern is that the presentation does not make crystal clear what it is about the paper that is revolutionary, and what is merely a demonstration of what could be achieved in future. I would like to see the Introduction, especially, revised so as to make a number of points clearer than they are at present: - That a key problem with the present generation of models, whether offline (Sitch et al. 2008) or coupled into GCMs (Friedlingstein et al. 2006), suffers from a lack of data constraints. - That a further problem with these models is a general lack of transparency – they are typically complex codes that have accreted over time. - That available parameter estimation and uncertainty analysis methods allow models to be constrained by data and the degree of constraint assessed. - That we are now operating in a far more data-rich world, with extensive remotely sensed data sets being complemented by huge compilations of plant-level data e.g. in GLOPNET and TRY."

Response: We agree with your recommendations and will make these points much clearer in the introduction.

———— Comment: "I'm also concerned by the implication in the Introduction that "What to include" and "how best to include them" are implied to be issues treated by the paper, but they aren't really – the set of processes considered and their parameterizations are presented as a fait accompli just as in all other papers that present new models."

Response: We agree that we should ensure that we do not imply that we actually have conducted thorough analyses of "what to include" and "how best to include them" as part of our study; other than the comparative analysis with model components with no climate dependencies. However, we developed our methodology with the intention of enabling us and others to conduct such investigations in future (e.g. by systematically comparing soil models or photosynthesis models within the same framework). Our parameterisations and processes are presented as a fait accompli within the context of the paper but we certainly do not wish to imply that these are the best representations that can be achieved. We have no doubts that better representations will be found.

———— Comment "I would rather see the Introduction concentrate on the new model's real strength, as a demonstration of how better to construct and evaluate a model." Response: We will make sure this message is clearly communicated in the introduction.

————— Comment: "With this background it is not especially meaningful in a review to focus on what I think are the merits or otherwise of the particular representations that have ben adopted for each process. However, the paper really should emphasize when parameterizations new to modelling have been adopted (e.g. the one by Ommen et al.) – as a strength – and when old parameterizations have been pressed into service, such as the Miami model."

Response: Thank you, we will emphasise this more clearly in the methods and elaborate in the discussion.

——————- Comment: "The latter is a key example of what the new model has NOT achieved – because, as shown already by Bonan (1993) Tellus, the relationship between MAT and NPP at a global scale is to a large extent a surrogate for the relationship between growing-season integrated PAR and NPP. The Miami model is incapable of predicting a correct response of the carbon cycle to MAT for exactly this reason: when a place warms, it does not shift in latitude..."

Response: The MIAMI model is a convenient, well recognised placeholder for a much better representation in future. We were aware of a number of its limitations although were not aware of the Bonan (1993) paper. Thank you for the reference. Your advice here implies a nice way to explain to readers what we're trying to achieve and tempt them into trying alternative representations for themselves.

———————— Comment: "The paper refers to the absence of CO2 effects in the model. This points to another key issue for next generation modelling i.e. how to include effects that cannot be represented by observational data sets? The answer has to be to build in the results of key experiments, alongside passive observations, as part of the model design."

Response: This is a good recommendation that we will include in the discussion when we talk about future model development

——————— Comment: "Denham et al. (2007) should be Denman et al."

Response: Thanks, we will correct this.

——————— Comment: "The model is not similar to those by Melillo et al. (1993) and Friend et al. (1997) in the sense that these explicitly include N cycling"

Response: Good point. We will correct this.

——————— Comment: "Some of the data choices are not the best: in particular, Mouillot and Field has been superseded by GFED"

Response: We will note this in the discussion. However, one of our points is that even the old datasets contain useful information. If they have been superseded by better datasets it is often not clear precisely what additional benefit we get from using the better datasets. Out methodology will enable this to be investigated.

————————— Comment: "The lack of inclusion of flux measurements is a missed opportunity"

Response: We are exploiting that opportunity in our next study. It will become particularly useful when we data-constrain eco-physiological representations of photosynthesis. However for the purposes of this study we did not need to include the flux data.

——————— Comment: "Prentice et al. (1993)'s use of the lesser of supply and demand to predict AET is not, as stated, incompatible with the idea of switching PET formulations – it would be simple to do (having said that, it is likely that the description of the algorithm in that paper was less than transparent!)"

Response: We will look into this for the next study and will correct the incorrect implication in the manuscript.

———————— Comment: "On p 13442 it is said that CO_2 fertilization is likely to "stimulate the terrestrial carbon sink .... in future": This is a major understatement – CO_2 fertilization is the principal contender to be the cause of the terrestrial carbon sink today!

Response: We will change this statement to recognise the point that "CO_2 fertilization is the principal contender"

————— Sincerely, The authors.

Interactive comment on Biogeosciences Discuss., 9, 13439, 2012.