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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.

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This MS represents a major methodological advance and 'proof of concept' for a next-generation terrestrial carbon cycle model. 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. 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. 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. 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. 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... 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.

A few additional, minor points: Denham et al. (2007) should be Denman et al. 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 Some of the data choices are not the best: in particular, Mouillot and Field has been superseded by GFED The lack of inclusion of flux measurements is a missed opportunity 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!) On p 13442 it is said that CO2 fertilization is likely to "stimulate the terrestial carbon sink .... in future": This is a major understatement – CO2 fertilization is the principal contender to be the cause of the terrestrial carbon sink today!

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