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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Reliable, robust and realistic: the three R's of next-generation land surface modelling" *by* I. C. Prentice et al.

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The three reviewers' comments were gratifying: all made strong positive statements about the value and quality of this manuscript, and were evidently in sympathy with the main thrust of our argument. Here we address all of the specific criticisms and suggestions for improvement, one by one. We take the reviews in the order in which they were published:

Anonymous Referee #1 The author may want to add a brief discussion on this and how the three proposed 'tools' can increase the reliability of simulated NPP. There is a natural place to do this, namely at the end of Section 5. We propose to add the following new text starting on p 24830, line 3: "This idea also has the potential to simplify the

modelling of GPP and eventually NPP, which is a key quantity for the terrestrial carbon cycle. For example, Wang et al. (2014) have shown that a model explicitly derived from optimality considerations – the least-cost hypothesis of Wright et al. (2003) and Prentice et al. (2013), and the co-limitation or co-ordination hypothesis (e.g. Maire et al. 2012) – can predict global patterns of forest GPP without no need for PFT-specific parameters. The same has not yet been done for NPP and biomass growth. But the least-cost hypothesis also makes explicit predictions about respiration costs; together with recent findings of general relationships between carbon use efficiency and soil nutrient status (Vicca et al., 2012; Fernandez-Martínez, 2014), these predictions are likely to provide the basis for an equally general model of NPP." References to be added: "Fernández-Martínez, M., Vicca, S., Janssens, I. A., Sardans, J., Luyssaert, S., Campiolo, M., Chapin, F. S. III, Ciais, P., Malhi, Y., Obersteiner, M., Paple, D., Piao, S. L., Reichstein, M., Rodà, F. and Peñuelas, J.: Nutrient availability as the key regulator of global forest carbon balance, Nature Clim. Change., 4, 471-476, 2014. Vicca, S., Luyssaert, S., Peñuelas, J., Campiolo, M.„ Chapin, F. S. III, Ciais, P., Heinemeyer, A, Högberg, P., Kutsch, W. L., Law, B. E., Malhi, Y., Papel, D., Piao, S. L., Reichstein, M., Schulze, E. D. and Janssens, I. A.: Fertile forests produce biomass more efficiently, Ecol. Lett., 15, 520-526, 2012." The authors could add recent findings on the importance of mesophyll diffusion on carbon fluxes, for example Sun et al. (2014) and references therein... We propose to add some words on this topic in the last paragraph of section 6.1, before the final sentence on p 24832, line 23: "The resistance to diffusion of CO2 in the mesophyll, between the intercellular spaces and the chloroplasts where photosynthesis is carried out, is often ignored but can be substantial, and has implications for the strength of CO2 fertilization (Sun et al., 2014). Again there is an over-riding physical constraint, i.e. the flux of CO2 to the chloroplasts must match the net flux of CO2 into the leaves." Reference to be added: "Sun, Y., Gu, L., Dickinson, R. E., Norby, R. J., Pallardy, S. G. and Hoffman, F. M.: Impact of mesophyll diffusion on estimated global land CO2 fertilization. Proc. Natl Acad. Sci. U.S.A., 111, 15774-15779, 2014." P24812 L5: LSMs are also applied to assess the response to land use and land use change. This should be added. We propose to replace "climate and atmospheric environment" with: "climate, atmospheric environment, land use and land-use change" P24832 L22: The co-ordination theory allows also to derive Vcmax as a function of leaf nitrogen. Thereby Vcmax can be derived dynamically from the state of the N cycle, rather than being a PFT-specific parameters (P 24829 L22). We agree about the linkage between Vcmax and the N cycle, although the direction of cause and effect is open to discussion. Many current models with interactive C and N cycling predict Vcmax from N supply. On the other hand, optimality considerations suggest that N supply should primarily affect allocation to foliage versus fine roots, and there is plenty of experimental evidence to support this; also that Vcmax in wild plants, at the leaf level, should be treated as a regulator of N demand rather than a response to N supply. In any case, we propose to add after the new sentence beginning "The resistance to diffusion...": "Vcmax no longer needs to be a PFT-specific parameter but can be predicted dynamically from environmental variations. Moreover the strong relationship between leaf nitrogen and Vcmax provides a natural way to predict plant nitrogen demand, a key quantity in determining how plants allocate carbon to different functions." Figures 1, 2, 3, and 6 would benefit from more comprehensive captions. This point is well taken, as there is rather a lot of information in these pictures. For the revised version, we propose to write much more informative captions to all four, drawing attention to their most salient features. The layout of the figures is not fully consistent, for example the "atmosphere – land surface label" is not always present. This was an oversight. We will check the Figures and provide revised versions where necessary. The atmosphere – land surface label may be redundant; we will either apply it consistently or remove it in revised versions of the Figures. Several aspects of Figure 6 are not easy to interpret. Brief captions with a list of changes from the previous version would be helpful in this respect. Our revised captions will take care of this. Figure 7 would benefit too from a more comprehensive caption. We will do the same for Figure 7.

B. F. Zaitchik (Referee) 1. The Three R's of the title are never formally defined. This is a very good point! We propose to add the following new paragraph at the end of

Section 1. "The three R's of the title are all generally recognized as important characteristics of a numerical model, but models often do not possess all three. Possession of one feature does not by any means guarantee the rest. By reliable, we mean a model that gives approximately correct predictions under most circumstances. By robust, we mean a model whose results do not depend sensitively on the specification of quantities that are poorly known. By realistic, we mean a model that includes sufficient processes, represented in adequate detail, to allow simulation of the system's response to a changes in all of the external variables of interest. We will argue that the dominant paradigm in land-surface modelling focuses too heavily on realism at the expense of the other two R's." 2. The authors might also provide guidance on how the modelling community would know when any of these "R's" has been achieved. We propose to add new text just before the penultimate sentence ("Moreover, the widening field...") of the manuscript, p 24835, line 7: "Observational data sets originating in different disciplines, including remote sensing, atmospheric chemistry, ecophysiology and hydrology, will need to be brought to bear routinely to benchmark models and thereby establish their reliability. Robustness will be achieved through the discovery of general regularities that obviate the need to specify large numbers of poorly known or ill-conditioned parameters, such as (non-existent) universal Vcmax values for PFTs, and evaluated over time as a community enterprise facilitated by the open publication and sharing of code. Realism will be assessed not as an over-riding requirement to include every known process, but rather by models' ability to give consistent answers to scientific questions, such as the influence of different aspects of climate, environment and land use on global NPP." 3. ...can the authors say anything more concrete? ... [They should] use their pulpit to conclude with some more specific and potentially controversial recommendations for the community. The new text proposed above takes a big stride in this direction. As a further response to this encouragement, we propose to add before the last sentence of the text ("It will be challenging..."), p 24835, line 12: "A new level of reliability is unlikely to be achieved through 'business-as-usual' model development. More robust ways to model key processes are within reach, but will require both further scientific development and new code to be written. Several proposals now exist in the literature for possible community-wide benchmark standards, but progress on this front will require community adoption of such standards. A technical facility will be required to help make comprehensive LSM benchmarking and data assimilation a routine process." 4. Figure 6 and/or the header paragraph for Section 6 need to be expanded. ... I encourage the authors to rethink this figure and how it relates to the text. We agree, this is a deficiency. We propose to address it by providing an informative caption, as mentioned in the response to Anonymous Referee #1 above.

Anonymous Referee #3 ... the brevity of this part of the paper should be compensated by more detail in the discussion that follows ... The discussion of how model development should proceed ... is ultimately lacking in any solid advice ... We have avoided lengthy prescriptions, because much of what we propose has not yet been demonstrated in the literature – even though we are actively engaged in work along these lines. However, the additions we have proposed above all go strongly in the direction of providing "more detail" and "solid advice" as requested here. The impact of the manuscript could have been increased by suggesting areas for coordinated activity in [the field of Data Assimilation]: what are the problems we need to solve? We did mention some areas where work is needed, particularly on generic schemes, as currently the barrier to implementing data assimilation methods is rather high. We also stated why data assimilation can be a valuable aid to model development (p 24833, lines 21-27), and we even listed some of the key problems that need to be solved (p 24833, lines 28-29 and p 24834, lines 1-13), including difficulties in the application of multiple-constraint approaches. Nonetheless, for greater clarity, in our proposed revision we have numbered these key problems and provided some more explanation of each. A more detailed analysis can be found in several recently published papers – see revised text below. Suggested revision to the final paragraph on data assimilation, p 24833 line 28 – p 24834 line 13: "Data assimilation confronts a number of practical difficulties. Here we identify three issues that require further research for their satisfactory resolution. (1) High computational demand. Investigators have to choose

between gradient-based methods and 'brute-force' ensemble simulation (Wang et al., 2009). Ensemble simulations are computationally extremely intensive, and can easily become infeasible for global LSMs with several hundred parameters. Gradient-based methods use adjoint codes or finite-difference methods to compute the gradients that are required for optimization (Rayner et al., 2005). The gradient-based approach is many times more efficient than ensembles whenever a large number of parameters are to be optimized. However, adjoint code needs to be generated afresh whenever the model code is modified (Kaminski et al., 2013). (2) Maintaining mass and energy conservation in state assimilation. Compared to empirical ecosystem models, one of the advantages of global LSMs is that they enforce the conservation of mass and energy. However many state assimilation techniques do not automatically conserve mass and energy, and therefore need to be modified to include conservation constraints. It has yet to be fully explored how this modification affects the parameter estimation process. (3) Quantifying uncertainties in multiple datasets for parameter estimation. Because state-of-the-art LSMs typically include processes with time constants ranging from hours to decades or beyond, multiple datasets with different characteristic temporal and spatial scales are needed to constrain all the model parameters. However the uncertainties of multiple datasets and how those uncertainties vary in space and time are poorly quantified in many cases – introducing an element of subjectivity into the analysis. This problem has been discussed by Raupach et al. (2005) and Wang et al. (2009). A general solution has yet to be found."

Reference to be added: "Raupach, M. R., Rayner, P. J., Barrett, D. J., DeFries, R. S., Heimann, M., Ojima, D. S., Quegan, S. and Schmullius, C. C.: Model-data synthesis in terrestrial carbon observation: methods, data requirements and data uncertainty specifications, Global Change Biol., 11, 378-397, 2005."

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