

Interactive comment on “Primary production in forests and grasslands of China: contrasting environmental responses of light- and water-use efficiency models” by H. Wang et al.

Anonymous Referee #1

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Manuscript: Primary production in forests and grasslands of China: contrasting environmental responses of light- and water-use efficiency models

Authors: Wang, Prentice, and Ni

Dear Editor,

General comments:

Wang et al. use simple stand-alone primary productivity models to attempt to improve understanding of the processes responsible for the widely ranging DGVM model re-

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sponses to both CO₂ fertilization and the combined effects of climate change. Their idea is that the use of these simple stand-alone models will allow for a reduction in the uncertainty associated with the differing responses of the more complex models. A similar approach has seen great success with high complexity climate models, so it seems to be have good possibility of success. I generally find their analysis to be sound, however I have a few points I would like to see addressed.

First, the language used is often overly vague. For example, the authors often refer to ‘the data’ or ‘forest NPP data’, but it can be difficult to determine if they are referring to ‘observed data’ or ‘model output data’ or even regressions on observed data rather than their simple LUE/WUE models. I believe I generally understood which dataset was referred to in each instance, but I had to re-read sections repeatedly due to vague language. For clarity the authors should amend their language to be more specific. Second, two of the conclusions of the study relate to the likely-incorrect CO₂ response of the LUE and WUE models, which the authors attribute to an ability to capture an increase in runoff (for WUE) and an increase in vegetation cover (for LUE). Both of these proposed reasons for the models’ likely-incorrect response are plausible, but they are just educated guesses that do not come directly from the model results. I don’t feel then that these conclusions merit such a prominent role in the study’s conclusion (they are mentioned in the abstract). I would recommend leaving these interpretations in the main text, or at least provide further support for the interpretation. Lastly, I find the discussion on ratios of ANPP to total NPP for grassland to be unconvincing. Given the very large range from the Hui and Jackson (2006) paper, it is actually surprising that the value from the WUE model falls outside of it. I am not sure then if these model results can be used in sparse ecosystems as the authors contend. I would like to see further evidence supporting the validity of the models in grasslands.

Overall assessment:

This paper presents a worthwhile attempt to address a cause of perplexing uncertainty in DGVM results. The approach taken is generally valid and, while I have some con-

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cerns about the validation datasets, the results are of interest. I think this paper will be of interest to readers of Biogeoscience. I recommend publication after revisions (especially addressing clarity and precision of language).

Detailed review:

Throughout the manuscript, the authors occasionally neglect to list the units of variables. Please ensure units are consistently labelled.

p.4289 l.5-10: The assumed NPP/GPP ratio is never given that I could find. Please state the value used outright. This is especially confusing as the authors later derive a NPP/GPP ratio for their LUE and WUE models (section 4.1), but it is unclear if these values are used elsewhere.

l. 12: I think the authors should also examine/discuss the implications of their use of a managed forest as opposed to an old-growth forest.

p. 4290 l. 5: Were the grasslands also managed or pastured? What are the implications of this dataset's values if they were?

p. 4295 l. 15: Remove one instance of 'performed separate'. There are also other instances of typos and grammar problems that should be carefully checked for in other parts of the MS.

p. 4295 l. 20-25: Please better describe what was done here, it is difficult to understand at present how this performs the independent check that the authors describe. The Beer et al. (2009) dataset is also based upon a WUE model so perhaps it is not truly an independent check.

p. 4297 l.1-22. This whole section is very opaque on what
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model/regression/observation data the authors are using. Please re-write this section to enhance clarity.

p. 4297 l. 11-19: A maximal slope of 21% strikes me as a large value. In the discussion, a lot is made of the hypothesized influence of runoff and vegetation cover, but little to the nutrients. Even though the influence of the nutrients is likely not heavily important, more discussion of their influence should be given as the influence is not insignificant. While I agree that nutrient availability is not the primary control of forest NPP, I don't see much support for the authors' contention (p. 4300 l.20) that 'the data provide no support ... that nutrient availability is the primary control on forest NPP'(again, specify which data!). I think the authors need to back this statement up with further evidence.

p 4297 l.19-21: This difference for the oldest age class is one reason I would like to see more discussion on the implications of comparing the model results to managed as opposed to old-growth forests. I am also puzzled as why later on (p.4299 l. 25) the authors compare their NPP/GPP ratio against forest stands > 100 yrs old, but give no information about the proportion of their modelled forests that are of that age.

p. 4299 l.18: The Zhang et al. (2009) value noted is the global average, not really comparable to a China only value. The authors should get a China specific value or at least give the range for China that are shown in the Zhang et al. (2009) paper.

p. 4300 l.19: Besides my earlier objection to this statement around the nutrient availability, I also don't understand the statement regarding NPP in the tropics than in temperate regions. This whole paragraph needs to be rewritten as it does not presently make sense on which data are apparently contradicting Huston and Wolverton (2009).

p. 4300 l.18-30: The range in Hui and Jackson (2006) is so large that it is surprising to be outside of it. Also since they are fractions, to **not** be of 'similar magnitude' would be exceptional! I find this to be weak proof that the models are application to sparse vegetation types. More evidence should be provided to justify the models use in sparse ecosystems.

p. 4303 l. 12-16: Where in the models does runoff appear? I can understand why the authors hypothesize about the influence of both runoff and vegetation cover, but I can't see how it is backed up by their model results. As a result I don't think the discussion surrounding runoff or vegetation cover should feature so prominently in their conclusions (e.g. abstract l. 11 -13).

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