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Biogeosciences Discussions

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 #2

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This paper concerns the simulation of (mostly) Net Primary Productivity for a sample of Chinese ecosystems. More importantly, however, it attempts to disentangle the impacts of changing light-use and water-use efficiency on the responses to environmental forcings. This is a promising idea, but the paper is unconvincing in it's attempt to analyze and communicate how and why the models differ in their responses, and the formulation of the models also confuses the boundaries between the effects of water and light stress on productivity. My greatest concern, however, it that there are numerous problems with the communication of the model structure, the fitting procedures, the meanings and uses of the underlying inputs, and some missing explanations of key principles. Because of this I actually cannot definitively tell what the authors have

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done, and therefore it is very difficult to comment extensively on whether it is scientifically interesting or otherwise.

Specific comments

p4289 L6: For tropical forests, it is widely established that CUE is ∼0.3. Are there any data from Chinese ecosystems to defend the assumption that it is constant? Also, you state the fraction is fixed, but not what the fraction actually is?

P4289 L15-20 : This section is confusing, as are many of the references to the different modeling approaches throughout the paper. In the abstract, the proportional models are not mentioned, but 'semi-empirical' models are. Here, another class of models is introduced that 'account for how VPD affects WUE... etc". Also, if another class of simple proportional model is to be introduced, it should have an equation to describe it, or at least some consistant means of referring to this part of the methodology.

P4289 L23: What data are used to construct these NPP estimates? Are they aboveground or total NPP?

P4290 L22-27: This section is also confusing. The equilibrium evaporation is never defined, and so the meaning of the remainder of the section cannot be deciphered. The Zhang equation is not introduced, nor referenced, and the purpose of the section is not defined. Also, it states that the "soil moisture accounting algorithm of Prentice 1993 was also tried" but it is not clear what it was tried for or what the aim of this exercise might have been.

P4291 L7: The ordering of this section is difficult to understand, as it introduces numerous concepts prior to their complete explanations in the modeling sections. I had to read the paper numerous times before I began to understand what was happening in this section. I would recommend putting the empirical sources of data next to where they are used by the model derivations.

P4291 L15: Why would you assume that fAPAR is controlled only by water availability?

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This needs more justification.

P4291 L19: The assumption of the LUE efficiency model is that light controls uptake, and that simplicity is significantly undermined by making fAPAR a function of water availability. Because of this, I find that the authors attempt to disentangle the two responses is not successful, in the sense that I no longer understand which features of the model are contributing to the outcomes.

P4294 L4: The authors assert that plants adapted to dry environments show less response to SWP than to D, and that therefore it can be assumed that the efficiency parameter is constant, based on unpublished data and in contrast to the actual conclusions of Medlyn 2011... The domain of the study, however, covers moist environments too, which might be expected to have less significant responses to D on account of the expected variations in the stomatal efficiency parameter?

P4294 L 10: The term for A seems to depend critically on the derivations of Ea, which is still an unexplained empirical function of annual precipitation (the 'Zhang Equation'). While the use of the correspondence between D and ci/ca is interesting, I am unconvinced that this is a robust means of predicting changes in assimilation with changes in environmental drivers.

p4924 L 12: I don't understand what the 'fitted NPP data' term here refers to. What parameters are being estimated?

p4295 L 10: Again, I really don't understand what the 'fitted NPP data' term here refers to. What parameters are being fitted here? What does the "fAPAR/" term mean? Is it a typo? What is the purpose of 'fitting' the two different terms? This section needs rewriting and expanding to include an explanation of the goals of the fitting process and the theoretical background.

p4295 L24: This section on 'fitting the WUE and LUE molds to these (GPP) data" is poorly explained. Which parameters of the models were fitted to the new GPP data?

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Are these new model fits referred to with a different naming convention to the existing NPP fitted data? There is a similar lack of explanation on the process of fitting to the grassland NPP in the next paragraph. At a minimum, a table is required showing all of these different model instances, what was fitted to what, and how the performances varied. Finally, why not fit the models to all the data simultaneously, as the model is designed to predict all of these things in an internally consistent manner?

p4297 L6: Again, there is no equation to reference the 'simple models' and the LUE and WUE are also referred to as 'simple' elsewhere in the text. 'Simple' is a relative term with apparently shifting reference points. These models need to have clearly defined names throughout the paper.

p4298 L1: Precipitation changes uptake mechanistically in the WUE model and using an arbitrary empirical relationship in the LUE model. It is not clear what we can really learn from this comparison.

p4299 L6: Slopes of what regressed on what? This is confusing.

p4299 L16: The range of CUE predicted (0.62 to 0.37) is huge, and very dissimilar to the Waring et al. estimate of 0.5, given the observed range of these values.

p4299 L18: I don't understand why you have even made a reference to the MODIS model products, even with the caveat given.

p4299 L25: How can these these results be consistent with reduced CUE in old forests, when they range from 0.37 to 0.62 (an enormous range)?

p4300 L1-10: This paragraph contains a discussion of comparisons between this and two other methods for WUE, but no information on what the other methods are and what data they derive from.

p4300 L11-23: What is the value of LUE from this analysis? ('ours' is not defined?)

p430 L 23-29: The range of values of NPP/ANPP predicted by the two models is huge,

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(0.31-0.59) and so is the range observed (0.40 -0.86). That the two models span the very large observed range does not indicate that there is a 'general consistency'. In fact, these values are implied as the mean for all ecosystems, so both are predicting either very high or very low values compared (presumably) to the observed mean value. Also, I don't understand the link to the discussion of sparse ecosystems and how this poor comparison means that they are well simulated?

p4301 L8: 'Equilibrium evapotranspiration' is still undefined, and is not discussed at all in the derivation of the WUE model. How is it predicted by that formulation and why is it different?

p4301 L18: This reads like the model cannot respond to increases in temperature at all, unless they are from <0 to >0? Also, I now realize I cannot decipher how the growing season temperature and growing season length are actually employed in either the LUE or WUE model? Maybe I have missed this explanation, but I cannot find it even searching for the terms?

p4303 L 24: Because of the many difficulties in interpreting the methods in this paper, I have not, despite several readings of the paper, been able to discern what exactly it can tell us about the possible causes of divergence in DGVM behavior.

Interactive comment on Biogeosciences Discuss., 9, 4285, 2012.