The Biogeosciences Discussion paper of Hang et al. reports biophysical constraints over terrestrial primary production. Using a simple model of light-use efficiency and photosynthesis along with latitudinally distributed PAR, fAPAR, elevation etc. they suggest the primary constraint on terrestrial productivity is sparse vegetation cover imposed by water limitation.

Overall I like the simplicity of the approach and analysis. The application of successive constraints on primary production from $\mathrm{GPP} = \phi_0 \cdot a \cdot \mathrm{PAR}_{\mathrm{toa}}$ to $\mathrm{GPP} = \phi \cdot a \cdot \mathrm{PAR} \cdot \mathrm{fAPAR} \cdot {}^{c_i - \Gamma}/_{c_i + 2\Gamma^*}$ helped elucidate how and why we observe its spatial distribution. Figures 1 and 3 are fascinating. To me this paper provides a quantitative framework for understanding what we've known for [possibly] decades regarding biophysical constraints over terrestrial productivity[1]. The addition of remotely sensed data and the conclusion that potential primary production is most limited by sparse vegetation cover due to water limitation appeared more novel to me. The discussion and implications of the paper fall far short of what I would hope for a paper published in Biogeosciences. Below are my thoughts on why I think the paper needs pretty substantial revision. I hope they are helpful to the authors.

1. I found the paper through the results section interesting and informative. The Discussion section was, however, remarkably uninformative. It did not deepen fundamental understanding of biophysical ecology or place the current results within the historical context of the field. This was disappointing because the authors seem to have interesting and unique results that should lead to substantial advancements.

   I. On a similar note, the introduction begins with the idea that climate-CO2 feedbacks in global models are poorly constrained, as indeed they are. Section 4.2 discusses some of the current limitations to predicting fAPAR within this context, but there is no meaningful discussion of how the results presented in this study enable better model constraint. Is this possible? Why and how?

2. The discussion section considers nutrient limitation on several occasions. It seems largely to argue nutrients are at best a second-order effect, yet the model itself has no nutrients in it [apart from CO_2]. Thus it does not seem that much can be made of nutrient limitation in the present study—it was never designed to do so. Rather on this point the discussion is framed around straw men. Would it not be far more interesting to discuss the present results in the context of where and how interactions with nutrients are likely to be manifested?

   Within this context, three points for your consideration:

   I. Huston and Wolverton focus on NPP and ANPP rather than GPP, which is

[1] **Gates**, DM (1980) Biophysical Ecology. Springer-Verlag. New York; **Iqbal**, M. 1983. An introduction to solar radiation. Academic Press Orlando FL.

the focus of the present study, so it is not entirely surprising that interpretations regarding [*any*] controls over productivity differ. I found the third paragraph of the discussion neither insightful nor informative [and borderline disrespectful], so I would recommend deleting it.

II. The discussion of forest FACE +23% NPP stimulation at eCO2 [Norby et al. 2005] ignores (a) follow on papers showing nutrients limit primary production at eCO2 in forest FACE [e.g., Norby et al. 2011] and (b) the broad diversity of long-term responses to eCO2 controlled by nutrients in other studies/ecosystem types.[2] Granted there is a 1-sentence nod to nutrient limitation of CO_2 fertilization in the following section. This however does not satisfactorily describe a concept nor its relevance to the interpretation of the data presented here.

III. The discussion of modeled vs flux-tower GPP in the 7th paragraph suggests the model's over-estimate cannot be related to nutrients on the basis there is no overestimate in the tropical biome—i.e., black symbols w/GPP>2500 g C m^{-2} a^{-1}, presumably [Figure 2]. Does this contention follow logically? There is good evidence to suggest tropical forest productivity is light limited whereas temperate and boreal forest productivity is nutrient limited[3]. Perhaps the authors are correct, but again this point would benefit tremendously from deeper analysis.

3. The discussion of elevation effects was very interesting. Why not a similar approach to the remainder of the discussion section?

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[2] **Norby et al.** 2010. CO2 enhancement of forest productivity constrained by limited nitrogen availability. Proceedings of the National Academy of Sciences of the United States of America **107**:19368-19373; **Reich**, and **Hobbie**. 2013. Decade-long soil nitrogen constraint on the CO2 fertilization of plant biomass. Nature Clim. Change **3**:278-282.; **McCarthy**, H. R. et al. 2010. Re-assessment of plant carbon dynamics at the Duke free-air CO(2) enrichment site: interactions of atmospheric CO(2) with nitrogen and water availability over stand development. New Phytologist **185**:514-528.

[3] **Nemani et al**. 2003. Climate-driven increases in global terrestrial net primary production from 1982 to 1999. Science **300**:1560-1563; **Graham et al**. 2003. Cloud cover limits net CO2 uptake and growth of a rainforest tree during tropical rainy seasons. PNAS **100**:572-576.; **Sigurdsson et al**. 2013. Growth of mature boreal Norway spruce was not affected by elevated CO2 and/or air temperature unless nutrient availability was improved. Tree Physiology **33**:1192-1205