We would like to thank this reviewer for their constructive comments, which have helped us to improve our manuscript. Here we address each question the reviewer raised, one by one.

> 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.
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> Overall I like the simplicity of the approach and analysis. The application of successive constraints on primary production from $GPP=\phi_0 \cdot a \cdot PARtoa$ to $GPP=\phi \cdot a \cdot PAR \cdot fAPAR \cdot c_i-\Gamma/c_i+2\Gamma^*$ 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. 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.
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> 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.

This suggestion to improve the Discussion is much appreciated. In response, we have extended the sub-section "Implications for modelling strategy" by three paragraphs. After a brief retrospective on the history of LUE modelling, where we cite a number of additional references, we point out the key difference distinguishing our conceptual model from other LUE models: namely that our model is built on an explicit theoretical basis (the co-limitation hypothesis and the least-cost hypothesis, both independently supported) and testable assumptions (no differences in the controls of LUE among C3 plants, and soil effects on GPP acting via fAPAR rather than LUE). This provides at least two substantial benefits. First, it leads to a simple, traceable model in which *no* parameter is tuned to the observations that the model is supposed to predict. Second, it incorporates the CO_2 effect into a LUE model in a natural way, in contrast with other LUE models where the CO_2 effect either has to be imposed using supplementary equations, or is lacking.

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?

This comment also has been addressed by means of a more thorough discussion under "Implications for modelling strategy". We indicate that the conceptual model proposed here provides a first step along the way to achieving a 'next-generation model' with a stronger theoretical and empirical basis. Moreover, we have added (a) a discussion in some depth (citing some highly relevant papers published during the last two years) of how nutrient limitation might be better represented in models and (b) a simple exploratory data analysis on the controls of fAPAR. We have also emphasized the importance of predicting fAPAR as a research goal for next-generation DGVMs.

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 CO2]. 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?

We thank the reviewer for pointing out this weakness in the manuscript. We have been able to sharpen our thinking on nutrient limitation significantly, while improving the way it is treated in the text. A new sub-section, entitled "[CO_2] and nutrient supply effects", has now been included in the Discussion. Based on a number of recent, independent analyses of observations, we argue that the way forward for the inclusion of nutrient limitations (and potential interactions between nutrient supplies and CO_2 fertilization) in primary production modelling involves making a clear distinction between effects on GPP, NPP and biomass growth, with probably the most important effects of site fertility being manifested through the allocation of NPP – to different plant compartments, and to export to the rhizosphere *versus* biomass growth. We note that this approach is different from the current paradigm of nutrient limitation in DGVMs.

Within this context, three points for your consideration:

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

Indeed, Huston and Wolverton were focusing on NPP, and moreover we now consider that this distinction is very important – a point that we had not fully appreciated earlier. Therefore, we deleted this paragraph as the reviewer suggested.

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. Granted there is a 1-sentence nod to nutrient limitation of CO2 fertilization in the following section. This however does not satisfactorily describe a concept nor its relevance to the interpretation of the data presented here.

We appreciate these detailed comments. The interactions of nutrient availability and the CO_2 response of NPP, or more precisely of biomass growth, constitute a complex subject with a large (and frequently confusing) literature. It would be far beyond the scope of the present manuscript to attempt a review of the subject. (Such a review is planned, perhaps a year from now.) Nonetheless, to avoid the impression of oversimplification, we have included in our revised Discussion reference to the influential paper by Norby et al. (2011) and also to some other papers (Lee et al., 2011, Vicca et al. 2012, Aoki et al. 2012, and Fernández-Martínez et al. 2014) that deal in different ways with the nutrient dependence of primary production and/or the CO_2 fertilization effect.

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. Perhaps the authors are correct, but again this point would benefit tremendously from deeper analysis.

On reflection, we agree with the reviewer that our argument was not watertight. On the other hand we are unconvinced of the logic behind Nemani et al.'s estimation of limiting factors (we already hinted at this in the Introduction). There is evidence for light limitation of primary production in tropical forests, and for N limitation in particular in temperate and boreal forests. But the suggestion has also been made repeatedly that tropical forest growth is limited by P and/or base cation availability, and more generally, there is ample experimental evidence that growth limitation by one factor does not (in defiance of Liebig's so-called 'law') preclude a response to others. This is another large subject, which we have opted to avoid, because at the moment we have not marshalled enough evidence for the deeper analysis that we would like to carry out.

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

See above for a summary of the revisions we have made to the Discussion, which mean that it is now more balanced, covering a broader range of topics in comparable depth.