We thank Reviewer 2 for his/her comments. In this reply we address the suggestions for revisions of the manuscript point by point.

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?

Due to the limited availability of relevant data from China, we could not provide specific supporting studies. In particular, GPP estimates derived from eddy-covariance flux measurements would enable us to make specific comparisons with NPP but these are currently not publicly available. We hope that the current worldwide trend towards more open access to data will progress, eventually allowing flux measurements to be used to answer a wider range of scientific questions. For the time being, we are reliant for some kinds of data on syntheses from other regions.

The ratio of NPP to GPP *is* assumed to be approximately constant (as part of the theoretical justification for the LUE and WUE models), but it is *not* pre-assigned any particular value. Instead, when developing the semi-empirical models, we estimated slopes for the relationships between NPP and two composite climatic predictors. Then we used the global gridded GPP product of Beer *et al*. to estimate slopes for the relationships between GPP and the *same* predictors. By comparing the slopes for NPP and GPP, we were able to infer values for the ratio of NPP to GPP. This point has been made clearer in the revision.

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 consistent means of referring to this part of the methodology.

The concepts of WUE and LUE embody the idea that annual NPP is an approximately constant proportion of annual vegetation water use or light use, respectively. Thus, the simplest possible WUE model would be obtained by fitting a proportional relationship between NPP and actual evapotranspiration. The simplest possible LUE model would be obtained, similarly, by fitting a proportional relationship between NPP and annual absorbed PAR. This was our starting point, referred to in the revised MS as "Level 0" models.

However, we needed a way to represent the possible influence of other environmental variables on WUE or LUE. In order to do this, we first showed how both WUE and LUE models can be predicted (as approximations) from first principles of photosynthesis and transpiration. Then we built in the specific (and contrasting) environmental dependencies that are implied by these predictions, resulting in what we now call the "Level 1" models.

This logic is presented in the "model" section, now rewritten in a clearer way. We have also provided a table to define all the models used in this paper, as the reviewer suggested.

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

Total NPP. We now mention this.

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.*

Both equilibrium evapotranspiration and the Zhang curve (which is one version of the 'Budyko framework' for catchment water balance) are well-established concepts in hydrology. Air passing over a hypothetical homogeneous, well-watered surface adjusts its saturation deficit until an equilibrium evapotranspiration rate is reached, which depends only on net radiation and temperature. The well-known Priestley-Taylor expression for potential evaporation is 1.26 times equilibrium evaporation (see e.g. Eichinger WE, Parlange MB, Stricker H, 1996) and this has considerable empirical support. We recognize that these concepts are not as well known in biogeosciences as they are in hydrology, and therefore we have added some text to explain them in more detail.

The soil moisture accounting approach of Prentice *et al.* (1993) is widely used in the calculation of the bioclimatic "alpha" moisture index. But it is true that many users are not aware of the mechanics of the calculation, and it is only one of several possible algorithms for the purpose. So we have added more information about this as well.

The purpose of this section is to illustrate all of the environmental variables involved in the Level 0 and Level 1 models, before launching into Results. In doing so we also give the reader a general idea about the spatial pattern of the environmental variables (Fig. 1), and how they compare with NPP. By presenting this material first, we avoid interrupting the flow of the theoretical analysis in the "model" section.

The original Zhang equation can be written:

$$E_a = E_p [1 + MI - (1 + MI^w)^{1/w}]$$

(Zhang *et al.*, 2004) where E_a is actual evapotranspiration, E_p is potential evapotranspiration and MI is the moisture index. We set the empirical parameter w at a single generic the value of 3, appropriate to forests, and $E_p = 1.26E_q$ where E_q is the equilibrium evapotranspiration. These points are spelled out in the revision.

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.

We agree that the introduction of too many concepts in this section could bring some confusion. Therefore, we have rearranged the text as the reviewer suggested.

P4291 L15: Why would you assume that fAPAR is controlled only by water availability? This needs more justification.

We assume that the continental-scale pattern of annual fAPAR is *primarily* controlled by water availability. Annual fAPAR is the remotely sensed equivalent of foliage projective cover (FPC), and is related to leaf area index (LAI) through Beer's law such that fAPAR is approximately proportional to LAI at low values of LAI (< 1), approaching 1 at high values of LAI. It has been known for at least 40 years (Specht 1972) that water availability is the dominant control of evergreen (or growing-season) FPC, and repeatedly shown (e.g. Nemani et al. 1989, Kergoat 1998) that large-scale patterns of growing-season LAI are approximately in equilibrium with water supply. We confirmed this hypothesis for China by showing that there is an empirical relationship between fAPAR and MI.

We have added the key elements of this justification in the revised text.

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.

The classic LUE model states that NPP is proportional to *absorbed* PAR, which is the product of incident PAR and fAPAR. Thus, the inclusion of fAPAR is an essential part of the LUE model. But if we wish to predict NPP under climate-change scenarios, where fAPAR is not observable, then we need a method to predict fAPAR.

As mentioned above, fAPAR depends strongly on water availability; so it makes sense to use water availability to predict fAPAR.

We are not trying to 'disentagle' whether productivity actually depends on water or on light. Beyond question, productivity requires *both* water *and* light! Instead, we are trying to contrast two different simple approaches to modelling productivity. In the WUE approach, light enters the equation implicitly through its close relationship to solar radiation, the driving force of evapotranspiration. In the LUE approach, water enters the equation implicitly through its influence on fAPAR and the proportionality of fAPAR and photosynthesis. In either model, drought reduces productivity – through reduced water supply in the case of the WUE model and through reduced fAPAR in the case of the LUE model.

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?

At present we do not have a complete theory of how SWP and D interact to affect stomatal conductance and photosynthesis (work on this topic is ongoing in our laboratory). However, equation (2) is consistent with field measurements of stable carbon isotope composition, wthin and between species, which show a steady progression in values from wet to dry environments. Any variation in ξ with species and soil moisture is already implicitly included in the estimation of c_i/c_a from the carbon isotope data.

This point is now more clearly stated.

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.

The Zhang equation is a well-established equation in hydrology, as we have now explained in the revised text. Its theoretical justification rests on the Budyko framework, the subject of a large literature. The equation tells us how actual evapotranspiration is determined by precipitation and potential evapotranspiration. The way in which vegetation properties adjust to environmental conditions is *implicit* in this equation, although this aspect has not been much studied and remains as a research topic.

We agree that using c_i/c_a to estimate D is a simplification, but we do not agree that is 'not robust'. There are good reasons to expect a correlation between long-term values of c_i/c_a and D, acting through the two controls of c_i/c_a i.e. SWP and D, which must themselves be mutually correlated because of the dependence of atmospheric water vapour content on transpiration (see e.g. Monteith 1995). The R^2 value we obtained using this predictor shows that this simplification did not introduce any major error. We have explained this in the revised text.

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

The "NPP data" here are the observed total NPP data from forests. The revised text makes clear what parameters have been 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.

This too is clarified in the revised text.

The '*I*' in the 'fAPAR • *I*' term is not a typo. It is the letter 'I', representing the incident photosynthetically active radiation (PAR) integrated over the growing season. 'fAPAR • *I*', therefore, is the absorbed PAR by vegetation. Now we realize that the italicized symbol '*I*' brings confusion because it resembles a division sign. We have therefore replaced it with 'IPAR'.

As we explained in our response to the reviewer's first comment, the purpose of fitting a first simple model and then a theoretically derived, slightly more complex model is to allow us to predict the response of NPP to changes in temperature, rainfall and CO_2 while not losing the good correlation obtained with the simple model. We have revised this section and provided a fuller explanation of the goals of the fitting process.

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? 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?

Piao et al. (2010) used the Luyssaert data set of co-located measurements of GPP and NPP. If we fit the models (equation 8 and equation 9) with GPP data, rather than NPP data, then the regression slopes are estimates of $0.63q\xi^2c_a$ and Φ_0, respectively. In other words, the ratio of NPP/GPP would not be included in the estimated slopes. If we now compare those slopes with the ones that we obtained from forest NPP data, we can estimate the ratio NPP/GPP (Table 1). Similarly, the comparison between the slopes estimated by grassland aboveground NPP data and by forest NPP data is supposed to provide an estimate of the ratio ANPP/NPP (Table 1).

We like the suggestion to provide a table to show all of these different model instances, what was fitted to what, and how the performances varied. We have added such a table in our revised manuscript.

However we did not fit all models to all data simultaneously because there are much larger uncertainties in the grassland data, mainly associated with the larger (and hard to estimate) below-ground fraction of NPP.

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.

In the revision we have provided clearly defined names for all the models and used them consistently 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.

As we explained before, incorporating the response of absorbed light by vegetation to water availability in the LUE model is necessary, to assure the model's ability to capture the physiological process of drought-induced decline in vegetation cover, acting through a decrease in absorbed light. The relationship of fAPAR to MI applied in the LUE model is empirical, certainly, but it is not arbitrary. It is simply a statistical estimate of the relationship that applies today. And as mentioned earlier, we can learn from this comparison about the *consequences of contrasting approaches to modelling productivity*, recognizing that both light and water are controls on NPP in the real world.

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

Beer et al.'s GPP data were regressed on $E_a(c_i/c_a)^2/(1-c_i/c_a)$ in the WUE model and $\text{fAPAR}\cdot I(c_i-\Gamma^*)/(c_i+2\Gamma^*)$ in the LUE model. We have rewritten this and explained it more clearly in our revision.

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.

The range is large, showing that our method cannot estimate a precise value for CUE. Indeed, this probably reflects the fact that there is substantial variation in CUE. But the range we obtain is not larger than the range of values estimated by Piao *et al.* (2010) based on observations.

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

Even though based on modelled products, this published paper does claim to provide information about the relationship between NPP and GPP and therefore we felt it necessary to cite it – and to add a caveat.

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)?

We tested the possible dependence of NPP on stand age by performing separate regressions with forest NPP data for three stand age classes (<50 yr, 50-100 yr, >100 yr). The old forest (>100 yr) showed statistically significant differences from the other two classes (Fig 3). There is nothing unusual in this: with a large sample size, it is often possible to show significant effects, even when the residual (unexplained) variation is large.

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.

The study of Zhu et al. (2011) used the Integrated Biosphere Simulator (IBIS) to simulate GPP in China for 2009-2099 with climate scenario data generated from the Thrid Generation Coupled Global Climate Model (CGCM3). WUE was then calculated as the ratio of GPP to evapotranspiration for different vegetation types.

The study of Beer et al. (2009) used flux-tower data to calculate both WUE and 'inherent WUE' for different vegetation types. Inherent WUE was defined as $GPP \cdot D/E_a$. We used the same calculations as in Beer *et al.* (2009) to estimate forest WUE and inherent IWUE with our WUE model at the forest NPP sampling sites.

This information has all been provided in the revised text.

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

We have rewritten this paragraph to explain these comparisons better. The calculations are approximate, but they do allow studies to be compared that are based on quite different sources of information about plant productivity.

Our estimated value of LUE for **NPP** in 0.2196 gC/mol photon, which is equivalent to 0.0183 in dimensionless form (because 1 molC = 12 gC). If we assume a CUE of 0.5 (the average of the values we estimated based on the two different models), we obtain a rough estimate of the LUE for **GPP** of 0.0366. We can now compare these approximate values with:

- 0.06, the theoretical value given for the LUE of **GPP** in C_3 plants by Farquhar, Caemmerer and Berry (1980) based on plant physiological principles;
- 0.02, the empirical value given for the LUE of **NPP** by Knorr and Heimann (1995) based on analysis of the seasonal cycle of CO_2 concentration.

Thus, our value of 0.0366 for GPP is rather low compared with the theoretical maximum value based on plant physiology, but our value of 0.0183 for NPP is close to the empirical value obtained from the seasonal cycle of CO_2.

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

Because the data available for this purpose are very limited, we can only make a very rough comparison here. We agree with the reviewer that we cannot make a particularly strong case for the extension of the models into non-forest vegetation based on this rough comparison alone. In our revision, we have added a caveat about the limits of this extrapolation to sparse vegetation.

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?

See the revised text (and comments above) for the definition of equilibrium evapotranspiration (E_q). This makes clear, among other things, how temperature affects E_q. Warming means higher E_q, and therefore higher E_a under well-watered conditions.

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?

The growing-season temperature and length are only employed in the LUE model, and not in the WUE model.

Growing-season temperature is used in the estimation of the CO_2 compensation point. Warming will increase growing-season temperature, and therefore increase the CO_2 compensation point, which has a negative effect on vegetation production due to increased carbon loss in photorespiration.

Growing-season length affects the term '*I*' or IPAR, which is the total incident PAR integrated over the growing season – defined as the period when the daily mean temperature is above 0˚C, termed PAR_0. A longer growing season allows more PAR to be used by plants, especially in low to mid-latitudes where there is substantial PAR in the winter months.

If the daily mean temperature is all above 0˚C all the year round, however, increasing temperature cannot lead to any extension of growing season length. Therefore there will be no positive effect on vegetation production through PAR_0, but there will be a small negative effect of warming due to the higher CO_2 compensation point. By contrast, in colder regions, warming will lead to an extension of the growing season, and this positive effect on production can easily outweigh the small negative effect of warming due to the higher CO_2 compensation point.

The LUE model thus allows us to analyze the possible competing effects of different environmental factors on vegetation production under different climatic regimes. We have rewritten the paragraph to make this analysis clearer.

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.

We hope that we have made all these things clear in our extensively revised text.

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