We thank Reviewer 1 for his/her comments. In this reply we address the suggestions for revisions of the manuscript point by point.

> **general comments**
>
> This paper makes a solid contribution to the task of projecting and understanding the response of vegetation to climate change, through its application of empirical (statistical) approaches for predicting the distribution of vegetation types, but with predictor variables that incorporate the mechanistic controls of vegetation. This fusion of a statistical, and what might be called semi-mechanical approach (which in principle should be extendible to modelling the response of individual species), helps to satisfy the demand for making predictions about vegetation and agricultural land-use types in the interim while mechanistic or dynamic vegetation models are still evolved from the state of being able to predict only taxonomically coarse-grained descriptions of vegetation (i.e plant functional types and biomes) to more fine-grained descriptions.
>
> **Specific comments**
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> Fig. 3. It would be interesting to see a lowess curve on the figure.

A lowess curve would not represent the *partial* response of log NPP to α, which is what we are most interested in – because of the effects of variations in the other predictors that are to some extent correlated with variation in α. But we appreciate the need to show a fitted curve and not just a scatter of points. Instead of showing a lowess curve, therefore, we have shown a visualization of the partial relationship between log NPP and α obtained from the multiple regression of log NPP against α, GDD0 and mGDD5.

> Fig. 4. There are some interesting sharp edges and linear features in the background scatter of points on the figure. Presumably these are related to built-in relationships among the predictors.

Linear features arise in such plots because of strong relationships among variables that apply in some part – not necessarily all – of the region. Sharp edges occur because the environmental space is bounded, e.g. by coasts and by elevation limits.

> p. 53, line 24: "hypotheses" I think the list (a)-(d) would be better referred to as "underlying assumptions" because they are only indirectly tested here (by producing the paper). It would be useful to indeed test those assumptions, both individually and jointly, but that is not the focus of this paper.

Agree! The term "hypotheses" has been changed as "underlying assumptions" in the revised manuscript.

> p. 58, lines 9: I don't understand why this calibration step was necessary, i.e, why the vegetation type with the highest predicted value from the GLM models could not be assigned as the predicted vegetation type. Also, the regression described here apparently uses probabilities as the response, which, unless the data were very well behaved, would suggest using a GLM as opposed to linear regression. Then why invert the regression?

The simplest approach would be to assign to each grid cell the vegetation type with the highest fitted probability. This was tried first. The results were reasonably good but (in particular) relatively less common vegetation types tended to be under-predicted. The approach we adopted gave much better results. Note that our predictions are still based on a GLM – the linear regression step is, in effect, merely an adjustment of the fitted coefficients. The need for such adjustment is well established in the literature on multivariate calibration. The principal reason cited in the literature is that the coefficients in a GLM after initial selection of variables are inherently biased due to the selection process. See for example:

Gude, J.A., M.S. Mitchell, D.E. Ausband, C.A. Sime and E.E. Bangs (2009) Internal validation of predictive logistic regression models for decision-making in wildlife management. Wildlife Biology 15: 352-369.

p. 62, lines 20-25: I can see why the crop vegetation types might be under predicted, inasmuch as they could be considered to be imposed on the natural vegetation, but what explains the overprediction? (Oh, karst. p. 67, line 20)

The overprediction of crops in the Yungui Plateau region is attributed to the karst topography there, which makes the land difficult to cultivated. This is discussed in the last paragraph of section 4.2.

p. 64, line, 5: This estimation is just by map comparison, right?

Yes, by visual comparison between maps, we saw similar effects of [CO_2] doubling to increasing precipitation by 30%. The relevant figure (Fig. 5) is referred to in the revised text.

p. 66, line 7: “The empirical model makes no prediction. . .” I first thought this meant “no prediction of change” but apparently it means “no prediction at all.” How is that represented on the maps?

In the lower half panels of Fig. 5, the blank areas (in the same white colour as the background) in the north-western deserts and the North China Plain are areas where the empirical models made no prediction at all. We have modified Fig. 5 so that “no prediction” is distinguished using a dark grey color.

Technical corrections

p. 55, line 15: 0.1 as opposed to 0.01 degrees?

All the predictor variables are calculated first from climatologies interpolated at 0.01 degree resolution. They are then up-scaled to 0.1 degree to give mean values. We have revised the wording to make this clear.

p. 55, line 18: Gallego-Sala and Prentice (2013)?

This cited paper was first published as an online publication in 2012, as it is eventually published now, the citation has been corrected as Gallego-Sala and Prentice (2013), and the reference has been corrected accordingly.

p. 55. line 21: mGGD0 and mGGD5. Cite Prentice et al. (2012b) for this?

Most definitions and calculations for predictor variables were described in details in the cited papers (Prentice et al. 1993, Gallego-sala et al. 2011). Prentice et al. (2012b) was cited by mistake as a reference for precipitation timing and seasonality. This citation has been corrected to "Harrison et al. 2003" during revision.

p.55,line27:precipitation timing variablesâA ˘T˘not clear(and not discussed in the cited paper)

More words have been added to clarify precipitation timing variables during revision. The cited paper has been corrected to "Harrison et al. 2003".

p. 60, line 1: What does the subscript on c_i? (And for that matter, why use c to mean two different things?)

Indeed, the ratio of NPP at elevated and reference [CO_2] was denoted by "*c*" and this is quite different from the leaf-internal [CO_2] (c_i) at reference [CO_2]. c_i is the standard abbreviation for this quantity in ecophysiology. To avoid confusion, the NPP ratio is now called "*x*" instead of "*c*".