

## ***Interactive comment on “Holocene vegetation and biomass changes on the Tibetan Plateau – a model-pollen data comparison” by A. Dallmeyer et al.***

### **Anonymous Referee #2**

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Summary: Dallmeyer et al. present a model-data comparison of vegetation changes on the Tibetan Plateau since the middle Holocene. The goal of determining the causes of vegetation change in this climatically-important region is worthy. Yet, significant limitations in the model simulations, including climate biases and specification of bioclimatic limits (see major comments below), as well as in the limited review of pollen reconstructions (also in major comments below), should be discussed more completely in the manuscript.

Major comments:

1. Pg 1074, Line 11-14: These sentences state that the reconstructions primarily

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identify decreasing summer monsoon precip as the most important factor causing vegetation shifts and that the model changes in land cover all occur due to temperature changes. These statements greatly oversimplify what is presented in the text, as 2 out of the 4 reconstructions call upon temperature changes to explain vegetation shifts and 1 out of the 4 sites in the model is strongly influenced by precipitation. There seems to be no great difference between reconstructions and model with regard to the climatic factors causing vegetation change and this conclusion should be deleted.

2. Results: It would be possible (and preferable to the subjective approach given in the text) to statistically analyze the vegetation trends to determine whether they are significantly different from zero.

3. Table 1: It appears that there are no precipitation-related bioclimatic limits in the model, only temperature-related. In this case, how can you make any conclusions about whether vegetation shifts are due to temperature or to precipitation?

4. Section 5.3: Please place the total terrestrial carbon loss in perspective. Is this a large and important number, or a small and unimportant number? What is the significance of this finding?

5. Summary and conclusion: The methodology used in this paper (analyzing dynamic vegetation) has some drawbacks compared to other approaches (specifically, offline vegetation modeling using an anomaly approach, eg, Wohlfahrt et al. 2008 Climate Dynamics, Miller et al. 2008 Journal of Ecology), as demonstrated in the issues the authors have with climate biases. Discussion of these different approaches would make a useful addition to this section.

6. A reconstruction that is not discussed, but should be, is Co Ngion (Shen et al. 2008 Ecology). This site is very close to Lake Zigetang, but shows distinct meadow-steppe ecotone fluctuations quite different from either the Zigetang reconstruction or the model. The Co Ngion record, at the very least, is a good reminder that we might not get the entire picture from one reconstruction.

Minor comments:

1. Some spelling and grammar errors in abstract, eg, Line 16 change “is shrinking” to “shrunk” and Line 18 change “Gras” to “grass.”
2. Pg 1074, Line 18: Grass fraction 38.9% does not match number presented in text (38.1%).
3. Introduction pg 1075: Ideas not ordered into logical paragraphs. Difficult to read.
4. Pg 1077, line 24: I don't understand “(spring).” Do you mean to say “during spring?”
5. Pg 1079, line 13: Do you mean “approximately” rather than “presumably?” I don't understand why the mean annual temperature is presumed to be 1.6 degrees C.
6. Table 2: Too many numbers for a table (with too many significant digits). This would be better shown in a figure.
7. Pg 1091, line 7-8: “Therefore, the annual temperature sum is not high enough to fulfil [sic] the limit of growing degree days in the model.” What limit do you mean, the limit between trees and shrubs?

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Interactive comment on Clim. Past Discuss., 7, 1073, 2011.

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7, C609–C611, 2011

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