**Climate
of the Past
Discussions**

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

Anonymous Referee #1

Received and published: 13 May 2011

The manuscript "Holocene vegetation and biomass changes on the Tibetan Plateau - a model-pollen data comparison" submitted to the journal Climate of the Past by A. Dallmeyer et al. contains results of a modelling experiment, performed with a coupled atmosphere-ocean-vegetation model, which are further compared to a pollen-based vegetation reconstruction performed at four locations at the Tibetan Plateau. Results are a scientific contribution to the German Science Foundation research program IN-TERDYNAMIC and to several other German-financed programs. I believe that presented results may be interesting to the international readership of CP. I would be glad to see the manuscript published, assuming the authors would agree to perform a moderate revision, taking in to account my comments below.

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Interactive Discussion

Discussion Paper

p. 1075, lines 8-12. North Africa and Middle East can not be called "surroundings". I suggest reformulating this sentence. The Taklamakan Desert and Dzungar Desert are conventionally used names

line 20. What is the source of snow?

lines 25-30. Introduction is mainly concerned about how the TP influences climate of the surrounding regions. However, the area of the plateau itself is large, settled by people and gives the origin of several great rivers. It would be fair to say a couple of sentences on what is influencing the TP climate and environments. For example, what causes above/below-normal rain or snow.

p. 1076 line 19. I suggest to mention Kleinen et al. 2011 paper, which is available online in The Holocene.

p. 1077 lines 1-4. Do you mean changes in natural vegetation/carbon storage here? If so, you need to say this more clear.

Lines 6-10. You already mentioned part of this earlier.

I recommend to keep the structure of the 2.1 section more clear, i.e. starting with monsoon circulation mention its summer and winter features, total, summer and winter precipitation, and temperatures. In your case, precipitation story is interrupted by the temperature. Summer monsoon provides 60% of the annual precipitation... I thought it should be more. What is the source/amount of winter precipitation?

p. 1078 line 1. 700 mm/year is not a small amount

line 10. intensive logging. You need to specify when. In the past 20 years large areas were reforested.

Lines 23-28. This jump to the global circulation is confusing. I recommend to reserve chapter 2.1 for the general climate, providing more precise (uniform) climate information for the four pollen sites. In the current version climate and environments descrip-

tions are taken from former publications, therefore not easy to compare. In your case, I would use advantage of having modern climate data in hands (I assume, grids are better than meteostation data) and summarize sites names, coordinates, and climate variables used in this study in a separate table. Satellite MODIS information would be helpful to present for the selected grids, as it will help to understand structure of modern vegetation cover. This will also save space, as you do need to repeat all numbers in the text.

p. 1079 MAT is common in the Chinese publications, but I rather would like to see mean July and January temperatures which are more relevant for plant growth/biome simulation.

p. 1080 3.1 Reconstruction section needs to be clarified. "Standard" is not correct expression here. There were several publications on pollen-based biome reconstruction in China (after Yu et al. 2000), which all present different biome-taxa matrixes. Moreover, biomes which are presented in the current study are different from Yu et al. and classical biome scheme of Prentice et al. I would suggest to present here a table, attributing all pollen taxa from the four pollen records and 112 surface pollen sites to the respective pfts and biomes. I would also name this section 3.2 Vegetation reconstruction, and put it after the General model section.

line 23. instead of writing "reasonable" I would suggest to express results of this study (for the relevant biomes) in a quantitative way.

p. 1081. 3.2. should be 3.1 (see my previous comment). I would name this section 3.1. Model and experimental design

Line 24 ". . . have no influence." On what? Please, edit this sentence.

Lines 25-30. This part is very important, but is not easy to understand from a very scarce description provided here. Could you explain it more clear? It would be helpful to present a table with the average climate/vegetation characteristics for the grids

attributed to each study site. In this case the readers themselves would be able to compare modern actual and experimental characteristics at the study sites.

Line 26 "may differ". In your case I would write just "differ".

p. 1082. 3.3 section. Does it need to be a separate or can be merged with the 3.1 model setup? I have one major criticism to this section. It is extremely poorly referenced. Could you add some references where appropriate.

p. 1083. Lines 16-18. This paragraph citing Brovkin et al must be in the beginning.

Lines 19-21. Be aware that you must apply the same (or similar) treatment to the pollen-derived vegetation units. Therefore, biome-pft-taxa table suggested earlier is absolutely necessary.

4. Results. Again, I like to stress that this biome classification is different from anything published by Yu et al. and Prentice et al. Differences between the modelling and pollen-based techniques should be mentioned in the method section. For example, model takes desert as non-vegetated area, while pollen-based reconstruction definitively uses pollen/plant taxa for this land cover category.

Line 25. Explain, why 20 year average is taken. Is this comparable with pollen averages?

p. 1084 lines 3-8. Again, reference to the table where MODIS values of vegetation cover for the chosen areas would be very helpful for evaluating the results.

line 13. How to explain this? I thought that simulated trends, which favor decrease in tree cover would also cause increase in desert fraction.

Line 26. Why? Is this also supported by any other data?

p. 1085 line 12. vegetation decline. What does it mean? Please, edit this sentence.

Differences between Fig. 3 and Fig. 4 must be stated in this section. For example

results for Qinghai show good correlation, for Naleng – almost opposite (worse of all), and are moderately good for the other two sites.

p. 1086 lines 1-10. I am surprised why Qinghai is taken as example. It shows best correlation between model and data. Could this 1. factor be tested with the modern data? I would understand reference to the general studies, if nothing else is available from the region. It is not the case here. Why not to use top core samples from the four pollen records discussed in the current study and to see whether it is true and in which way results are biased?

Lines 13-16. Again it is much more convenient to use examples from the analyzed areas/records in addition to the more general references.

Lines 18-28. I do not see your reason clear. Biome is also a great simplification. What is 'biome flickering'? Could you explain this or provide a reference?

p. 1087 Line 9. What is 'the strong relief'? Edit, please. In general the TP is rather flat and the relief is less complex than that of the Alps, for example.

Lines 12-17. I would suggest to present simulated values in the table (not only in figures) for comparison with modern data and for facilitating discussion. '... is too warm (up to????)' Please add value.

p. 1088 lines 1-5. This paragraph needs more attention. Nomads are living at high elevations, whilst low elevations are shared between nomads and pastoralists, each group may influence vegetation in different way. Please, make it clear in the paragraph and extend this paragraph by adding more references to the appropriate environmental/archaeological/historical studies of the region.

Lines 17-21. If this values are simulated for the study region, then they must be in the table in order to facilitate comparison with the observed climate.

p. 1089 lines 9-10. How this conclusion was obtained. What are the taxa-indicators for reconstructing human activity in this area?

Line 27. What are raingreen shrubs? This pft should be clarified in the biome-pft-taxa table.

p. 1090 lines 5-9. This statement is not clear to me. Why modern vegetation must be forest? Does this statement has support from MODIS or botanical observations. Please, explain.

p. 1091 line 14. 'low pollen assemblages' What do you mean by this?

Line 16 'pollens' should be pollen grains or pollen types representing low-elevated vegetation! The whole paragraph needs editing. By the way extensive pollen literature from China provides examples of arboreal pollen being transported to the low elevations by the streams (i.e. Herzschuk et al. 2004). This feature deserves to be mentioned.

p. 1092 line 18. I do not understand this technique. Since it is not a strait forward to the readers, I would suggest to explain the method in the method section above. It also seems to me that the 'cut' area of 3.43 million km2 is by 1 million km2 larger than original area of the Tibetan Plateau mentioned at page 1074. How this is possible?

28.3% is covered by forest in the model. Could this value (and other simulated values) be compared with the MODIS or any other observational data from the same region? This will give more credit to your modelling results. Again, it surprises me that there are no other references in this section? Does it mean that nobody performed such simulations before? If so this must be clearly stated as the advantage of the current work? If not, the results must be discussed against the other simulations and differences are mentioned and explained, if possible. I believe that many readers would like to see not only the absolute simulated values, but also how much the TP changes in biomass contribute to the global values during the Holocene.

p. 1093 lines 25-28. Here I see a contradiction to the figures 3 and 4 in the results section (see my comment above).

p. 1094 line 10. You state that there are discrepancies between simulation and ob-

servation at 0ka, but what can you say about the past. How large these discrepancies should be expected? What is general credibility of your results?

Line 20. Surprisingly reconstructed temperatures appear here? Where do they come from? The paper presents only pollen-based vegetation reconstructions. Please, insert climate reconstruction to the results or change your text.

References. I mentioned that several parts of the manuscript would benefit from better referencing. On the first quick view several important papers are missing, e.g. Kleinen et al., 2010, 2011; Gaillard et al., 2010, Ren and Beug, 2002; Wanner et al., 2008.

Table 2. Please, check that the number size is not too small.

Figure 1. Please, check that all references are provided for the figure.

Figure 2. Actually, why the topography in the model is so different from the reality? As I mentioned before, the topography is flat in the central regions of the TP. Could be topography corrected?

Figures 6-9. I do not see a reason, why not to present complete set of the key climatic variables for the four regions discussed in the article, and not only one-two selected parameters from each region?

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