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Interactive comment on “Paleovegetation reconstruction using $\delta^{13}\text{C}$ of Soil Organic Matter” by G. Wang et al.

Anonymous Referee #2

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The authors attempt to estimate the effect of several factors on carbon isotope ratios of soil organic matter used for reconstructions of C3 and C4 vegetation composition at a given locality on the Chinese Loess Plateau. They present the evidence for carbon isotope enrichment of about 1.8 permil caused by decomposition of predominantly C3 plant tissues (based on new data presented in the paper). They also examine the relation between carbon isotope compositions of C3 and C4 plant tissues along a precipitation gradient in China and report a negative relationship between carbon isotope compositions of C3 plants and precipitation (-0.40 permil per 100 mm of precipitation; based on their new data presented partially in the paper). They use these data to reinterpret the C3/C4 structure of vegetation on the Chinese loess plateau for Holocene and last glacial using published carbon isotope data. They are also discussing other

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corrections that are not related to their data.

I can not recommend the publication of the paper in its present form for reasons outlined below. However, I would recommend publication of this paper if the issues listed below were addressed satisfactorily.

There two fundamental problems with the paper:

1.No data are presented. All of their data are either plotted or averaged, so the data can not be examined and their models can not be checked or reproduced.

2.Carbon isotope correlations with annual precipitation are done on averaged data (Fig 2). Even then, very little variation is explained by the correlation(e.g., 36% for fig. 2a). I assume that the correlation becomes even weaker and less statistically significant for raw data. It follows that their interpretation based on such correlation are questionable.

There several other issues that the authors need to address:

1.Carbon concentrations in lower parts of soil profile are very low (I can not determine how low from the figure, but the author's themselves admit that they are close to 0). Carbon isotope values of such samples are very unreliable. Unless they can prove otherwise, they should not make conclusions that based on those data.

2. Their references are incomplete, they fail to refer to such key publications as Ahrens et al., 2000, who discuss the influence of different environmental factors on carbon isotope composition of C3 plants. There may be other key publications that have not been discussed.

3. Whereas their carbon isotope enrichment due to C3 plant tissue decomposition in soils is valid and an important contribution, they fail to discuss the opposite effect of the C4 plant tissues shown in a litter bag decomposition experiment by Wedin et al. 95. Therefore, in a mixed ecosystem the isotopic enrichment of C3 tissues during the decomposition may be cancelled by isotopic depletion of C4 tissues during the decomposition. A comparative analysis of soil profiles in mixed or C4 dominated soil

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profiles would be interesting, but not necessary to complete this paper.

The importance of presented data needs to be interpreted in a different light with less emphasis on devaluation of other people's work. The authors go to great lengths to show that there was little C4 vegetation present at the site during last glacial. What they fail to realize is that their corrections, while shifting the plant composition to the C3 range for both Holocene and last glacial, increase the magnitude of the vegetation shift from glacial to interglacial (the difference in C3/C4 composition increases), which is considerably more important than the exact C3/C4 composition at any given time (which is sort of arbitrary anyway because it depends on the values a researcher decides to use in the mixing model). Perhaps calculating C3/C4 ranges rather than the averages is the way to go, e.g., showing maximum, minimum and average values for C4 vegetation for Holocene and glacial.

Also, focusing the paper on the interpretations based on presented new data rather than trying to evaluate multiple factors that are not related to their data would streamline the paper. Language flow and organization of the paper could be improved.

Tables and figures: Table 1 should show some data on soil profiles including soil type (preferably in WRB classification), location, climate data, etc.

Figures should be bigger, with better resolution, perhaps in color, especially Fig. 2

I hope the authors find these comments useful and wish them good luck with paper revisions. I am looking forward to reading the revised version.

Interactive comment on Biogeosciences Discuss., 5, 1795, 2008.

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