Interactive comment on "Terrestrial mollusc records from Xifeng and Luochuan L9 loess strata and their implications for paleoclimatic evolution in the Chinese Loess Plateau during marine Oxygen Isotope Stages 24-22" by B. Wu and N. Q. Wu

B. Wu and N. Q. Wu

zealot@ustc.edu

We thank Y. Sun, H. Lu and R. Oches for helpful and constructive comments on our manuscript, which will lead to substantial improvements in our revised version. Below we reply all questions raised detailedly.

Response to Y. Sun:

1. Carbonate dissolution (see second paragraph of Page 2773): Since carbonate dissolution is a key factor affecting the total number of mollusk shells and the abundance of difference species as well. First, Can the authors provide a rough estimation on the depth of carbonate dissolution either based on the development of carbonate nodule layer below the S8 or variation of the CaCO3 content? Second, if strong carbonate dissolution happened, how to exclude the dissolution effect and obtain the real paleoclimate information from the mollusc data?

Based on the development of carbonate nodule layer below the S8, we estimate the depths of carbonate dissolution are ~1.4 m in the Xifeng section and ~1.6 m in the Luochuan section. Hence the mollusc data are affected by carbonate dissolution at the depths of ~73.2-74.6 m in Xifeng and ~54.2-55.8 m in Luochuan (we have added this interpretation in revised version, on p.2773, line 10). In order to reduce the dissolution effect we used the percentages variations of mollusc fauna instead of absolute abundance. If strong carbonate dissolution happened such as S5 and S1 layers, it is hard to exclude the dissolution effect completely. We can only get parts of information based on the mollusk faunal compositions. Some abrupt peaks in the affected layers are meaningful for paleoenvironmental reconstruction.

2. Implication of *P. aeoli* abundance (see third paragraph of Page 2773): The mollusk assemblages can be divided into three ecological groups, and the cold-aridiphilous group consists of *V. tenera, P. aeoli*, and *P. cupa*. However, even based on visual inspection on Figure 4, down-section variation of the *P. aeoli* abundance is different from other two cold-dry species, but rather similar to that of the *G. armigerell*a abundance (a thermo-humidiphilous group), why? Thus, a detailed discussion on different responses of these species to temperature and moisture will help the readers to understand the paleoclimatic implications of different mollusc fossils.

It is correct that every species has a suitable growing range for temperature and moisture conditions. In our study, though both *P. aeoli* and *V. tenera* are the cold-aridiphilous species, but *P. aeoli* adapts wider ecological conditions with MAT ranging from about 5-10°C and MAP ~200-500mm (Chen and Gao, 1987), while *V. tenera* prefers drier conditions, generally with a moisture range of ~200-350mm (MAP) (Wu et al., 2002). Among the thermo-humidiphilous group, the species of *G. armigerella* is eurytopic species which likes to live in relatively cooler habitat with MAT and MAP ranges being 5.8-10°C and 450-550 mm. Therefore there is a range of coexistence between the ecological conditions of *P. aeoli* and *G. armigerella*. Their high abundances in L9SS1 and L9LL1 indicate relatively cool-wet environment. Modern populations, most similar to this assemblage, are associated with MAT of about 6-8°C and MAP of about 300-500mm (Wu et al., 2007). However, these two species do not always vary synchronously because *G. armigerella* adapts to narrower ecological conditions than *P. aeoli*. That's why *P. aeoli* shows high abundance but the amount of *G. armigerella* is very low in L1, which can provide evidence to the comparison of climatic conditions of L9 and L1. According to the reviewer's suggestion, we have added above interpretation in our revised version, on p.2775, line 9.

3. Climatic implications of L9 loess (see Discussion section 4.1): By comparison of mollusc results from L9 and L1, the authors argue that the last glacial climatic

condition was colder and drier than the MIS 24–22, because of the relatively high mollusk biomass in L9 compared to L1. Firstly, this finding is different from conventional view derived from the magnetic susceptibility record, i.e., much higher susceptibility in L1 indicating a relatively warm and humid climate in last glacial than that of MIS 24-22. Can the authors provide a reasonable explanation for such a contradiction? Secondly, as mentioned in the manuscript, carbonate dissolution may dissolve a large number of mollusc shells. Why the low mollusc biomass in L1 is not due to strong dissolution under warm-humid climate conditions (if magnetic susceptibility record is a reliable indicator of the summer monsoon intensity)?

The reviewer mentioned an important issue that if MS could be used as a reliable proxy of summer monsoon. Although many previous studies considered that it can be used as an indication of summer monsoon variations, more recent works found it with apparent constraints especially under extreme conditions. Lu et al. (2000) suggested that the MS signal is not a good proxy indicator of summer monsoon in periods of extremely cold climate. In the strongly developed paleosol layer S5-3 the MS values are also exceptionally low and could not reflect the strong summer monsoon variations. Till now, the MS is only used as a relative indication of summer monsoon variations, its value could not simply be equaled to summer monsoon intensity as it suffering the effects of depositional, pedogenic and vegetation changes etc. (Heller et al., 1993; Guo et al., 1998; Han et al., 1998). It is hard to tell if L1 had stronger summer monsoon than L9 according to the small differences (20-30 $10^{-8}m^3kg^{-1}$) in MS values of L9 and L1.

As for carbonate dissolution of L1 stratigraphy, we consider that its action was weak in L1 loess based on the relatively high $CaCO_3$ content and weakly development of carbonate nodule (Liu, 1985). Thus, we can conclude that the low biomass in L1 is not due to strong carbonate dissolution but colder and drier climatic conditions. Besides biomass, mollusc species composition together with sedimentation rates, FeD/FeT ratio and pollen records also indicates much more cold-dry conditions during last glacial period than the MIS 24-22.

4. 900-ka cooling event in the Chinese Loess (see Discussion section 4.2): The 900-ka cooling event refers to the exceptionally cooling during MIS 22 compared to previous glacials (MIS 28 to 24). In Chinese loess, this cooling event is evidenced by remarkable coarsening of the grain size data (See Lu et al., 2000; Ding et al., 2002; Sun et al., 2006). In this paper (lines 16-18 in Page 2278), the authors suggest that the one at 905–895 ka (the boundary of MIS 23 and 22) was the toughest in both intensity and extent (Fig. 4). Actually, as shown in Fig.4a, I would believe that the last one around 880 ka (relatively high percentage of *V. tenera* and *P. cupa*) is the toughest cooling event; this inference is consistent with other loess-based proxies (e.g., grain size). However, the high peak of *V. tenera* and *P. cupa* abundances at the Luochuan section occurred around 900 ka (Fig. 4b). What's the major cause for such a spatial difference? Strong carbonate dissolution at the Luochuan section? To better address the feature of 900-ka cooling event recorded in Chinese loess, I would suggest that the authors should try to synthesize all extant proxies of L9, such as grain size and FeD/FeT ratio.

It is a good suggestion. In the supplement, we add a new figure of synthesized proxies including the ratios of cord-aridiphilous (CA) and thermo-humidiphilous (TH) mollusc groups, MS, grain size and FeD/FeT ratio (Fig. 1). As shown in the figure, it is no doubt that these proxies are consistent in general trend. However, the relative amplitudes are different because of each proxy with different climatic implications. Grain size coarsening in L9 may be related to the tectonic uplift of Tibetan Plateau as inferred by Sun and Liu (2000). Since mollusc fauna are sensitive to temperature and precipitation, they can be regarded as a reliable index of paleo-ecological conditions in Loess Plateau. The CA/TH ratios reflect cold-dry degree of the environment, and its variations in Xifeng and Luochuan indicate that the cooling fluctuation at ~900 ka was rather tough in both sections. The one at ~880 ka was also prominent in Xifeng, but not apparent in Luochuan. We suggest that such a spatial difference might be caused by the variations in gradients of summer monsoon intensity as discussed in our manuscript (please see the last paragraph in section 3.2). Since the cooling fluctuation at ~880 ka was not as prominent as the one at ~900 ka in Luochuan, we considered that the ~900 ka one was the toughest in both intensity and extent.

5. Cause of formation of the L9 (see Discussion section 4.3): I agree that the L9 problem should be investigated more intensively, particularly regarding its regional and global significances. In the discussion section 4.3, the authors present a general review on the possible cause of the formation of L9. Based on the mollusc data, however, we cannot obtain new clue to understand why the L9 is exceptionally thick and coarse. Thus, I wonder whether the authors can remove this part from the main text.

It is true that mollusc data itself can't reveal why the L9 is exceptionally thick and coarse. However, it is an important issue needing more new works to validate in the future. Our mollusk study tells that the L9 loess was not deposited under the coldest glacial conditions as suggested by previous studies. Thus, the formation of exceptionally thick and coarse L9 loess could not be attributed to climatic effect completely. In order to let readers understand the possible causes, we think that the discussion section 4.3 is still necessary to make the manuscript logical and complete. In this part, we shortened our discussion in the revised version according to reviewer's suggestion.

Response to H. Lu:

1. There is a great change in mollusc numbers in the samples. For example "in Xifeng section, the maximum count (2151/15 kg shells) is at the bottom of L9SS1 (80.6m at the depth) whereas the minimum (only 1 individual) is at the top of L9LL1 (74.5m at the depth). In Luochuan section, the highest value of mollusc shell individuals reaches 4156/15 kg in L9SS1 (59.3m 10 at the depth), and the top of L9LL1 (55.3m at the depth) just contains one individual (the minimum)." The authors simply explain that "It should be pointed out that a large number of mollusc shells were dissolved at the upper part of L9LL1 due to strong carbonate dissolution as the upper S8 soil formed." Since the "dissolved" process is very

important in interpreting the data of this research, I would like to see more evidence concerning how the dissolution influenced the preservation of the fossil mollusc shell. Has the great diversity of numbers of the fossil mollusc individuals in the loess/paleosol units only been influenced by dissolution or also influenced by paleoclimate and depositional environment?

This question is similar to the first and third ones of Sun's comments. Please see our answers to them. Here, we would like to emphasize that the preservation of fossil mollusc shells might be influenced by different pedogenic intensity. For example, strong carbonate dissolution occurred in the formation of S5 paleosol (MIS 15) caused almost all mollusk shells dissolved, and weak dissolution happened during interstadial of L1SS1 (MIS 3) can only cause small dissolution since most mollusk shells occurred well and abundantly. Paleoclimate and depositional environment may also influence the preservation of fossil mollusc individuals in the loess/paleosol units. However, it is hard to distinguish the dissolution effect caused by them. This is an important issue needing more works to testify in the future.

2. Much new evidence points to the possibility that the Tibetan Plateau and the Himalaya uplifted much earlier than the previously suggested (please see the review Molnar et al., 2010 and many other references). However, the authors have ignored these new results and used outdated references (p.2780, line 4-9) to support their interpretations. I suggest that the authors should mention the new results, or alternatively explain their results in more detail in light of this, rather than evoking "a tectonic movement of Tibetan Plateau named "Kunlun-Huanghe" happened during the depositing time of L9 loess (Li, 1991; Cui et al., 1998)" to interpret their result. In addition, because this part is not well evidenced by the authors' work, I suggest weakening these statements in the Conclusion section.

This is a good suggestion. In the revised version, we reduced our discussion in this part and weakened relative statements in the conclusion section according to reviewer's comments. Though new evidence points to the possibility that the Tibetan

Plateau and the Himalaya uplifted much earlier than the previously suggested, this can not exclude the uplift between 1.1-0.9Ma, which might still play a role in the increase of dust source of loess.

3. The general cooling experienced by the Earth during the Middle Pleistocene Transition (MPT) may also strengthen high-mountain weathering and the Asian winter monsoon intensity, which in turn may increase general dust transport capability and therefore rapid loess accumulation. As such, it is possible that global cooling can be a cause of the higher rates of loess accumulation. This may be an alternative interpretation of the formation of L9 and has been discussed in a recent paper published in the Geological Society, London, Special Publications (Lu et al., 2010).

The reviewer provides a new possibility of explanation to the formation of L9. We added the part of discussion in the revised text, on p.2780.

4. The loess grain size distribution, which is suggested as a direct proxy index of the winter monsoon strength, indicates that L9 is one of the coldest and driest periods during the Pleistocene in north China, as the authors mention on p.2779 line 25-28. However, the question over how to reconcile the grain-size and the mollusc records needs more focus. Although I believe the mollusc record is a more reliable method for revealing past climatic changes than grain size distribution, the interpretation of the coarsening of the L9 is still rather weak in this paper. In addition, the Tibetan Uplift forcing has been reported in the previous paper (Sun and Liu, 2000), it is not an important part of this paper and the descriptions could be significantly shortened.

The reviewer mentioned an important question how to use different proxy to accurately interpret past environment change. Grain size has been commonly regarded as a good proxy of wind intensity. But recently, some works considered that it is a proxy representing the source-to-sink distance (Ding et al., 2005; Yang and Ding, 2008), and even the post-depositional weathering. For the L9 loess deposition, the coarsening grain size might indicate other influential factors not only the winter monsoon intensity. Mollusc fauna are sensitive to environmental changes especially temperature and precipitation, they have been considered as a more reliable index for ecological conditions in Loess Plateau. Thus, we prefer to use it in our study as a more reliable indicator to reveal past environmental changes.

According to the reviewer's suggestion, we shortened the descriptions of Tibetan uplift forcing in our revised version.

Technical corrections (suggestions)

All the technical corrections have been done in our revised version.

Response to R. Oches:

1. Although it is possible to sample loess strata in the field at closely spaced intervals, deposition is a non-linear, episodic accumulation of silt, alternating with periods of reworking, insitu weathering, and pedogenesis, reflecting local, regional, and global climatic conditions. The age model for the Luochuan and Xifeng loess sections is based on the correlation of local magnetic susceptibility profiles – a measure of primary (depositional) + secondary (pedogenic) magnetic mineral content – with the globally averaged marine oxygen-isotope stratigraphy. While the general applicability of this model has been demonstrated in the literature, it is commonly over-interpreted in terms of the temporal resolution and the precision and accuracy of ages applied to the loess stratigraphy.

The authors briefly reference the chronology and acknowledge dating limitations (p. 2771, lines 10-18), but then apply a high-precision, high-resolution timescale to their interpretations. Beginning on p. 2775, the authors state ages (e.g., 940-923 ka) and sedimentation rates (e.g., 17.12 cm/kyr; 9.62 cm/kyr) that exceed the resolution supported by their magnetic susceptibility – $\delta^{18}O$-correlated age model. Attention to significant figures will help resolve this problem.

The reviewer mentioned an important issue concerning the loess chronology. It is true that the age model based on the MS included some assumptions such as averaged deposition rates and no pedogenic effects. Though it was not of so high temporal resolution and precision and accuracy of ages, it is still used as a working model for establishing an independent timescale of the long term loess stratigraphy. We checked and corrected significant figures of all data in our revised text according to reviewer's suggestion.

2. Carbonate dissolution is noted in the upper part of L9 (corresponding to late MIS 22) as impacting the preservation and therefore number of mollusk shells present. It would be helpful to include a plot of $CaCO_3$ vs. depth, if available, to demonstrate whether dissolution affects the preservation or abundance of shells elsewhere within L9.

This question is similar to the first one of Sun's comments. In fact, the carbonate dissolution only happened in the upper part of L9 as reviewers noted. This was because the upper soil layer of S8 experienced strong pedogenic process which caused large amount of $CaCO_3$ dissolution and even penetrated down to the upper part of L9, which has been attributed to secondary carbonate dissolution. We estimated the depths of carbonate dissolution based on the development of carbonate nodule layer below the S8, please see our answers to Sun's comment (question 1). High $CaCO_3$ contents (unpublished data of our colleagues) below this dissolution depth indicate that the dissolution did not affect the preservation or abundance of shells elsewhere within L9. Anyway, the preservation condition of our mollusk shells within L9 also proved that the dissolution was very weak.

3. Plots of percentage variations (figures 4, 5) have different scales for each taxon, sometimes with breaks in the axes, making it very difficult to interpret the plots at the figure size presented. The axis breaks should be made more clear, and ideally the individual taxon percentages should all be plotted at similar scales.

This is a good suggestion. We corrected the axes breaks in figures 4 and 5 in our revised version to make them clearer. However, the individual taxon percentages was hard to plot at similar scales because of the values with very large difference, resulting in the variations of relatively minor taxon percentages hardly being seen in that case.

4. The authors define three ecological assemblages based on a subset of the mollusk fauna. *P. aeoli* is listed as a cold-arid indicator (blue, in figs. 4 & 5), but it appears to vary inversely with the other cold-arid taxa (*V. tenera* and *P. cupa*) and parallels more closely the warm-moist indicator *G. armigerella* in fig. 4. Why are those seemingly incongruent taxa so commonly present together in L9, and how does that affect the paleoclimatic interpretations?

This question is similar to the second one of Sun's comments. Please see our answers to them.

5. Authors argue based on high values of *G. armigerella* and *P. orphana* that warm-humid conditions prevailed during formation of L9SS1 (MIS 23). However, this interval is dominated by *P. aeoli* (cold-arid), especially at Luochuan. *P. orphana* appears to be a relatively minor percentage of the total. The argument for warm-humid conditions during MIS 23 is unconvincing, based on the presented taxa.

We interpreted the conditions during formation of L9SS1 (MIS 23) as "mild" (please see on p. 2775, line 10) instead of "warm-humid" just because of the high values of *G. armigerella* and *P. aeoli*. Though *P. orphana* (a thermo-humidiphilous species) appears to be a relatively low percentage of the total, its appearance in L9SS1 still indicates a distinct amelioration of thermal-hydrologic conditions in the Loess Plateau during the period of MIS 23. In our answers to the second question of Sun's comment we interpreted the different responses of these species to temperature and moisture.

6. The comparison of L9 and L1 faunal data is seemingly less similar than authors suggest on p. 2777, lines 9-10. While thickness and sedimentation rates

may be somewhat comparable, different taxa are included in two ecological groups (figs. 4 & 5), and the warm-moist group is essentially absent in loess corresponding with MIS-3. Within the paper the authors appear to argue that L1 and L9 are similar early on, while later (fig. 6) they suggest that L1 is representative of much colder, stronger glacial conditions than L9. The comparison between L9 and L1, and the purpose of that comparison, should be clarified.

MIS 24-22 (corresponding to L9 loess) has been considered with similar structure as MIS 4-2 (L1 loess), as suggested by Clark et al. (2006), both being regarded as severe glacial climate. Based on this assumption, we compared L9 and L1 in term of different proxies. As the reviewer mentioned, both thickness and sedimentation rates can be comparable, but both faunal data are quite different. The warm-moist group is essentially absent in loess corresponding with MIS-3. That was the way we would like to show that these two glacials had different climate conditions, and L9 loess was not deposited under extreme cold and dry conditions.

7. The authors present a possible alternative explanation for the thick, coarse L9 loess on pages 2779-2780. This is a useful model that may explain the seemingly conflicting interpretations between the current faunal analysis and sedimentological data. Further exploration in a separate study can test this hypothesis, but it is appropriate to present this model as a potential explanation here.

Good suggestion. We presented this model as a potential explanation in the revised version.

Overall the paper is well written with few technical problems. A thorough English language editing is recommended to correct a few stylistic problems. Throughout the paper the authors should correct significant figures on reported values for ages, sedimentation rates, percentages of taxa, and sample depths, and they should adjust interpretations accordingly.

We corrected significant figures and adjusted interpretations according to reviewer’s suggestion in the revised version.

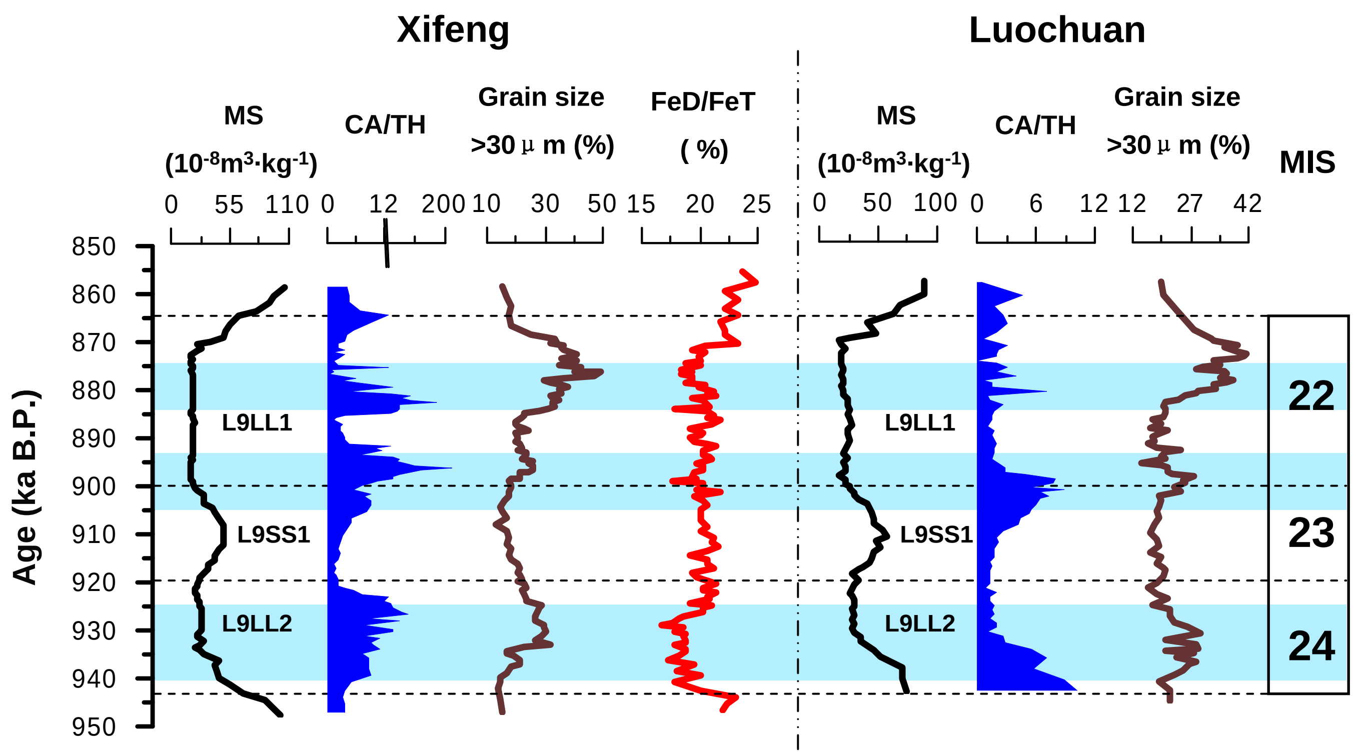

Fig.1. Variations of cord-aridiphilous (CA) mollusc group/thermo-humidiphilous (TH) group ratio, MS, grain size and FeD/FeT ratio in L9 loess. The grain size data are from Lu et al. (2000), and the FeD/FeT ratio data are provided by Z. T. Guo. Ages of all the proxies are modified according to their correlations to MS data. Light blue horizontal bars show three major cooling fluctuations.