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Climate of the Past Discussions

Interactive comment on "Strong asymmetry of hemispheric climates during MIS-13 inferred from correlating China loess and Antarctica ice records" *by* Z. T. Guo et al.

Z. T. Guo et al.

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First of all, we would like to express our sincerest thanks to Drs. J.R. Petit and W.F. Ruddiman for the highly constructive reviews and the valuable suggestions, which have been fully considered in the revised manuscript as follows. We hope that these revisions have successfully addressed the main points of the Referees. Any further comments and suggestions will be highly appreciated.

Zhengtang GUO, On behalf of the co-authors

1. Revisions following the comments of Dr. J. Petit

Comments: The paper presents the peculiar character of MIS 13 as found in loess

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Interactive Discussion

Discussion Paper

that is discussed within the context of global ice volume, Antarctic temperature and dust, CO2 and CH4. The unusual wet and warm MIS13 in China seems coincident with other evidences suggesting mild conditions over Baikal region, Tibetan plateau, Mediterranean region, Indian monsoon regions and South Greenland. There is speculation about the importance of the Asian dust for the glacial interglacial carbon cycle, with (however unclear) implications for future climate.

Revisions: In the revised version, we indicated that the covariance between Asian dust flux and CO2 does not necessarily imply a strong causal link (P6, lines 160-162; P18, lines 474-476). The discussions about the implications for future climate have been dismissed.

Comments: By comparison the Antarctic and marine records, the authors wish to convince us that the global climate possesses a natural variability that is not predictable from the simple response of seasonal change of the Northern Hemisphere summer insolation and coupled CO2 changes. During MIS 13 both hemispheres would respond in a different way (or asymmetrically). The paper is worth for publication while there are several over interpretation by temptation to attribute to certain paleoclimate proxies a large geographical even global significance, while when we look at the details, cautious should be exerted. The paper presents two new series of measurements of from the Xifeng (coarseness and pedological index) and from Changwu (pedological index). The authors describe the peculiarity of MIS13 climate as recorded in Chinese loess, and remind us prior observations (e.g. An et al, 1990 among others) of a thick soil formation associated to a wet and mild climate over this region. This paper is an extension of a previous paper on MIS13 (Yin and Guo, CPD 2007, that would be worth to cited instead the Chinese version), with in addition two sets of data and comparisons with marine and Antarctic records. The new data from Xiefeng and Chigwan loess seems consistent with the previous data and suggestions.

Revisions: A reference (Yin and Guo, 2008, CP) is added but the reference of Yin and Guo (2006, also in English) is kept because it is the original reference that addresses

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the soil properties in southern China.

Comments: What are new are the possible implications which are made by the comparison with marine and continental records. There are however several concerns on the interpretation for both the data from China as well as from the other records. In general it seems the authors over interpret data from China. Those are certainly of importance but similarly to most of the proxies used in paleoclimatology the loess data have a limited geographical significance. This may hold for the coarse grains which cannot be transported over long distance, or the weathering of soils which results from local climate and precipitations. One other detail is the fact that dust and iron flux from Asia appear coincident with the lower global CO2 does not implies a direct link! The authors omitted about the role of the southern ocean and among other details, that makes some confusion.

Revisions: We clarified in the revision version that dust deposition and rainfall in the Loess Plateau in China reflect large regional, even continental scale changes of climate and atmospheric conditions (P3, lines 61-68). The dust-CO2 issue has been revised as mentioned above. The role of the Southern Ocean in modulating the CO2 changes is emphasized in the discussion (P6, line 162; P14, lines 366-368).

Comments: About the comparison with other climate records there are also several concerns: -For Tibetan record, the chronology is an issue. Chen et al, wrote in their conclusions: p 319 in their paper:”We tentatively correlate a high carbonate in the RH core (their series) with marine oxygen isotope stage 13. If this correlation is correct, it indicates;a strong interglacial. Therefore, caution must be exerted for presentation of this RH record as strong evidence.

Revision: We removed several statements about the Tibetan Plateau (including the Abstract section) to avoid using it as strong evidence. In contrast, a new reference was added about central Europe (e.x. Marcovic et al., 2008) that supports pervasive warm conditions in the Northern Hemisphere during MIS-13.

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Comments: DeVernal et al. studied a marine core from south Greenland suggesting warm and mild climate for stage 11 and 13, and possibly ice-free Greenland. In absence of IRD in marine cores, we may wonder continental ice elsewhere in the Northern hemisphere than over Greenland, significant in term of ice volume and sea level (specially after MIS 14) but too small to produce massive ice discharges? Also, we may wonder how one marine sequence in south Greenland is representative of the whole Northern hemisphere ice volume during stage 13?

Revisions: We added a few sentences to clarify that the ice forcing from the Eurasia continent was also weakened as evidenced by the largely weakened Asian winter monsoon and reduced inland aridity (P12, lines 315-316). These suggest a commonly reduced ice-forcing from the northern hemisphere.

Comments: There is also a misinterpretation of the Antarctic data. About dust size, Lambert et al (2008) pointed out the "regional character"; of the size change at Epica Dome C as opposite changes have been observed at Vostok during the last glacial maximum and the last deglaciation. The smaller dust size at Dome C that is observed during full glacial periods was suggested to be representative of a stronger subsidence but over Dome C region only! The links with the Asian dust coarseness is fortuitous and gives a miss connotation.

Revisions: These have been revised in the revised version. We just state that the coincident changes may imply some kinds of link (P6, lines 153-154), instead of a close link of the atmospheric circulations.

Comments: About Epica Dome C dust flux for MIS 13, data fall within interglacial values and one cannot say anything about the source strength or aridity over continents. Lambert et al. (2008) pointed out the climate decoupling between the mid and high latitudes of the southern hemisphere. During Interglacial periods, the climate over the dust sources seems not influenced by the (cold dry) Antarctic climate, and this holds also for MIS 13. Indeed, Lambert et al data for the MIS 13 period are included in their

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Fig 3 and values fall still above the isotope threshold which is suggested to characterize interglacial climate. Nothing from dust flux from Epica Dome C could be deduced about climate over the main dust sources (South America), or about continental aridity over the southern hemisphere.

Revisions: We dismissed the discussion in the revised version about the implication of the EDC dust flux on the source aridity, but just mentioned the observed co-variance of dust flux between the Asian and Antarctic records (P5 &6, lines 134-144).

Comments: About the larger ice volume over the southern hemisphere, there is confusion at two levels. The authors first mention the possible larger extent of sea ice as suggested by Wolff et al (2008) based on sodium flux. Unfortunately, the link between sea ice and sodium is no so obvious and still to be demonstrated (e.g. Curran et al, EGS Vienna 2008). Second, the authors extrapolate some consideration on sea ice to the ice volume in southern hemisphere, then use the term of polar ice.

Revisions: The discussion about the sea ice extent in the Southern Hemisphere has been removed in just mentioning that the lower Antarctic temperature and summer sea surface temperature in the surrounding oceans would favor more extended sea ice (P13, lines 336-338).

Comments: The paper presents a long discussion and conclusion. In a way the conclusion appears a little bit emphatic especially about the importance of the Asian loess for climate and CO2 for which there are several repetitions not justified. While advertised in introduction, one can hardly make the comparison between MIS13 and the trend of present climate. On overall the authors point out the importance of the ice volume and suggest an asymmetry between polar ice that may affect the atmospheric circulation and summer Asian monsoon. Indeed, sea ice extent, ice volume, ocean temperature for present time (end of a rather quiet Holocene period) are likely very different than conditions at the end or during MIS 13.

Revisions: In the revised version, we indicated that the covariance between Asian dust

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flux and CO2 does not necessarily imply a strong causal link, as mentioned above. The discussions about the implications for future climate have been dismissed, including the abstract, discussion and conclusion sections.

Comments: Indeed we may wonder why the authors did not discuss ocean surface temperature, especially SST over tropical Indian Ocean as well as over the nearby warm pool. Those regions may be important for atmospheric vapour as well as for fuelling the atmospheric circulation. Similarly the deep ocean which is likely an important player in climate oscillations (the internal oscillations of the climate system) is also not envisaged.

Revisions: A paragraph in Section 3.2 was added (P10, lines 252-258) to discuss the anomalous ocean conditions in the low-latitude Pacific Ocean and South China Sea where the East Asian monsoon originates. Their possible impacts on the Asian monsoon are also discussed in section 3.3 (P13, lines 346-351; P16, lines 376-379).

Comments: Other remarks and suggestions from text: P 1060: Summary I suggest removing the last sentence. Introduction Lin 22: ref inappropriate for the biological uptake: Lambert et al, 2008, Winckler et al, 2008-10-30 P 1064 Line 10: climate conditions over the dust source regions in the southern Hemisphere during glacial periods. P 1066: line 16: EDC dust flux would largely reflect the source aridity. This holds only for full glacial period and not valid for interglacial period and MIS 13 P 1067 line 3 to 5: there is a broad correspondence between grain size changes but the causes are different. This does not support a close coupling between the two hemispheres. P 1067 line 10 to 13: remove the sentence on dust CO2 link P 1067: line 29 remove the end of sentence:and suggest a potentially important role on greenhouse gases. P 1069: line 9: remove: This is the main focus of this study. P 1069 line 11 to 13: wrong interpretation about the dust flux in term of greater aridity. MIS 13 appear normal with respect to interglacial values. P1069: line 23: Yin and Guo : refer to CPD paper in 2007. P 1070 line 23: .the sea ice extend from sodium (Wolff et al, 2008) is highly disputable (e.g. Curran et al, 2008). P 1070: line 26 and followings: EDC dust size represent a regional

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phenomenon only (local subsidence). For MIS 13, the dust influence on marine productivity is of second order and should be similar than that other Interglacials. P 1071 line 13, I suggest to stop the sentence after hydrological cycle. The rest is disputable. P 1071 line 17. Cox et al, suggested temperature difference with the north Atlantic may cause aridity over Amazonian regions. The aerosol pollution is has no link with MIS 13! P 1071: line 22: replace global anomalies by anomalies in several locations P1071: line 25: remove drier;in the southern hemisphere P 1072: line 1: what means polar ice? P 1074 line 4: Lambert et al. (2008) reference is inappropriate P 1076: line 19-20: Quaternary loess may be taken as a first attempts at an indication of the average ice conditions in the Northern Hemisphere(!) P 1077 line 7: I cannot understand the link between MIS 13 and future climate! P 1077 line 17: again the potential role of the Asian dust.

Revisions: All these suggested revisions have been done in the revised version.

2. Revisions following the comments of Dr. W.F. Ruddiman

Comments: Guo et al. have done a nice job of assembling a wide range of data that show that marine isotopic stage 13 is unusual in several respects: warm in the northern hemisphere, cold in the southern hemisphere, and thus imbalanced between the two. The paper merits publication after minor revision. Most of the trends shown in figures 2 and 3 are interpreted in a sensible, straightforward manner, but I detected two areas that I feel deserve more thought and explanation. The evidence for warmth across large parts of the northern hemisphere is pervasive and convincing: in Greenland ice, in North Atlantic plankton, in European pollen, and in Asian pollen and winter-monsoon dust fluxes. Yet the benthic oxygen isotope stack has values that are unusually positive for an interglaciation. The authors (correctly) infer that this anomalous signal did not come from ice in a warm Northern Hemisphere. They suggest that it must come from extra ice on Antarctica. But ice volume on Antarctica is constrained by sea level, which is largely determined by ice in the Northern Hemisphere. With a northern ice volume that was if anything small for a Northern Hemisphere interglaciation, the high sea level

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in stage 13 would have kept Antarctic ice volume low. For these reasons, I doubt that changes in Antarctic ice volume account for the anomalously heavy oxygen-isotope values. The authors briefly mention and then dismiss, changes in deep-water temperature, but this explanation deserves another look. Because the Antarctic deuterium record shows that air temperatures were some 4C colder than in the Holocene, that cold anomaly would almost certainly have chilled the bottom waters sinking around Antarctica and thereby shifted the benthic O18 signal toward heavier values.

Revisions: We indicated in the revised version that the most positive marine d18O values during MIS-13 would have resulted from two effects: probably thicker ice sheets on Antarctica, and an imprint of deep ocean temperature. The possible control of sea level on the extent of southern ice-sheets is also indicated (P12, lines 317-332).

Comments: A second issue is the apparent inconsistency between the low methane values (compared to typical interglacials) and various proxies that indicate a strong stage 13 summer monsoon (magnetic susceptibility, pollen on Tibet, etc). Because summer monsoons are thought to be the strongest control on the methane signal via filling of tropical wetlands, this is baffling. More confusing still is the evidence that high-latitude temperatures in the northern Hemisphere were very warm. This would appear to indicate that the second most likely source of methane — boreal wetlands — would have been omitting large amounts during warm summers. The authors (reasonably) invoke suppression of southern hemisphere methane sources (such as the Amazon) by cold temperatures to explain the reduced methane values. But the methane signal at orbital time scales has generally had a phase consistent with the forcing of northern wetlands, so it seems a bit odd that the southern hemisphere wetlands would have taken control. One possibility the authors might consider is methane emitted from sources very near the equator but offshore of the Asian continent. The northward movement of the ITCZ caused by the hemispheric asymmetry they have so well demonstrated might have brought unusually cool temperatures to those regions and suppressed methane emissions.

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Revisions: We dismissed the discussions about a strong role of the southern hemispheric wetlands on the atmospheric CH4 concentrations. Meanwhile, all the other potential CH4 sources mentioned by the reviewer have been indicated in the revised version (P14, lines 369-379).

Comments: Other (minor) points: p. 1065: Clarify the phrase ’of dust intensity’ (or eliminate); p. 1067: As far as I know, chemical weathering is too slow a process to have large effect on glacial/interglacial cycles and the CO2 values in stage 13

Revisions: Dust intensity has been expressed as dust flux. The too slow chemical weathering is indicated in the revised version (P7, lines 175-176).

Interactive comment on Clim. Past Discuss., 4, 1061, 2008.