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Climate of the Past Discussions

Interactive comment on "A major reorganization of Asian climate regime by the early Miocene" *by* Z. T. Guo et al.

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Review on the paper “A major reorganization of Asian climate regime by the early Miocene” by Z. T. Guo, B. Sun, Z. S. Zhang, S. Z. Peng, G. Q. Xiao, J. Y. Ge, Q. Z. Hao, Y. S. Qiao, M. Y. Liang, J. F. Liu, Q. Z. Yin, and J. J. Wei

The major aim of this paper is to document and to understand the main climatic changes occurring in Asia during Cenozoic and more especially at the Early Miocene. Indeed, there are only a few studies that are devoted to the causes of the transition of Chinese climate from a zonal pattern to a monsoon-dominant pattern. To address this issue the authors argue following two lines of evidence. 1. production of to accurate paleoenvironmental maps through the whole Cenozoic 2. Analyse of loess sequences. These lines of evidence indicate a common feature: a change in circulation and inland

aridity by the early Miocene. Therefore the major questions here investigated are: 1. re-examine the paleoenvironmental data to assess the timing of the transition (Early or Late Miocene) 2. Inferred, from Miocene loess deposits, the atmospheric circulation and moisture advection.

This paper interestingly discusses both issues. Bringing new evidences for timing (sec2) and atmospheric circulation at Miocene (sec3), arguing convincingly the validity of such a scenario (Sec 4 and 5). Through these results, the authors depict a new framework for climate evolution in Asia during Cenozoic.

This paper is bringing new data and original view on this climate transition. They convincingly argue on their both lines of evidence and the discussion is indeed very useful for the community. Nevertheless, I have some comments described below in detail.

Paleogene Bottom of page 543. All the arguments on a climate shift occurring at last Oligocene is based on data that, in fact, don’t show a clear pattern of monsoon dominant climate. Because it is a very important point for the paper this should be clarified. Page 545. Is not it possible to check this hypothesis with simulations that already exist and that the authors could use to reinforce their point [Zhang et al.]? This point should be address here so that a state of the art in terms of modelling support does or not exist. -In section 3 The authors bring new pieces of evidence, in terms of compiling 5 sequences, of the onset of inland desert at 22Ma, but also more interestingly that there is a continuity of these deserts to Present Day. They also explain quite convincingly the winter monsoon transport of dust and the summer monsoon dominant wind to carry humidity to build paleosols. Page 553 bottom / top page 556 1. Indeed there are certainly possibilities to evaluate the contribution of south-western and south-eastern monsoons depending on the Tibet plateau elevation through modelling experiment. I think the authors should take more attention to use when they exist simulations and modelling studies. 2. Is it possible also to constrain this different contribution by measuring δO18? -Section 5 This section is indeed very interesting. It is here only (and a bit too late) that the authors referred to Zhang

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papers that are the most recent series of simulations that account for geologic changes. Indeed, to understand the transition from zonal to monsoon dominant climates, it is necessary to put forward causes that will irreversibly modify the climate. Shrinkage and Uplift and certainly both of them are good candidates. Another important feature is the CO2 decrease this should be discusses. But certainly we need regional causes, because as far as I know 22 Ma is not a transition elsewhere. My main comment is that the authors summarize Zhang’s paper but they should take more attention to the wind pattern. Because, one of their major finding is thanks to the loess record, to infer seasonal wind pattern and its stability since 22 Ma. Therefore, they should conduct a deeper comparison with the changes in the seasonal wind pattern (and heat transport) in Zhang’s experiments. This would reinforce their conclusion.

Page 561 The discussion concerning the co effects of Tibetan plateau uplift and Paratethys is interesting, but the authors should better discuss: 1. The possibility of using δC47 to date the altitude of the plateau (new data from Ghosh, P., J. Adkins, H. Affek, B. Balta, W. Guo, E. Schauble, D. Schrag, and J. Eiler (2006a), 13C-18O bonds in carbonate minerals : a new kind of paleothermometer, Geochimica et Cosmochimica Acta, 70, 1439–1456, doi :10.1016/j.gca.2005.11.014.) 2. From modelling: a series of simulations have been done and recently published that enable to test the different impact [Zhang et al.].

Conclusion: The remarks written above are also pertinent for the conclusion. In particular, the threshold could be tested by model simulations. Moreover, it is of course important to bring some constrain on the date of the transition (22Ma), but it is also important to understand why despite important t changes the desert has maintain during 22 Ma and this can also be investigated by modelling studies. Despite these comments which may be considered as minor, this paper can be published in Climate of the Past, because it provides a ex framework for Climate evolution of Asia through Cenozoic which will be very stimulating for a large community of scientists.

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