

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

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The Guo et al. paper provides the paleoclimate community with an important summary of ways in which evolving regional tectonics in Asia during the Cenozoic altered patterns of climate and vegetation across the eastern part of that continent. The paper is well organized and clearly written. With minor additions, it should be of great value to many scientists.

Guo's previous work showed that a monsoonal pattern had been established in the loess plateau region of eastern Asia by 22 Myr ago. Correlative soil sequences with excellent magnetostratigraphic time control show layered sequences of: (1) fully developed soils indicative of south-to-north inflow typical of wet summer monsoons and intense chemical weathering, and (2) loessic soils indicative of stronger north-to-south

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outflow in dry winter monsoons. Changes between these soil types occurred at the orbital scale (41,000 years).

Guo interpreted these sections as eolian in origin. Silt-sized particles in the loessic soils share strong sedimentological and geochemical similarities with late Pleistocene loesses, Although Guo's interpretation was challenged (deposition was ascribed to riverine processes), follow-up studies have confirmed his initial interpretation.

Sensitivity tests with general circulation models suggest that the onset of these contrasting wet/dry responses, along with the accompanying longer-term change from the 'planetary' (subtropical) aridity pattern to the 'inland' aridity pattern, resulted from the combined effects of Tibetan Plateau uplift (which intensified both summer and winter monsoons) and withdrawal of the central Asian seaway.

This paper adds a large database (nearly 400 new geologic sections with paleoclimatic indicators) to this developing understanding of the climatic evolution of eastern Asian. The mapped data (Figures 3 and 4) constrain the change from the 'planetary/subtropical' aridity pattern to the 'inland' pattern to the interval 25-20 Myr ago. The authors are well aware of the limits of their data in determining the exact time of this transition (because of imprecise dating and aliasing of shorter-term trends).

I see some room for a few improvements in this paper:

The typical 'planetary' pattern of subsidence of dry air in the subtropics over land masses is not well explained. The authors should describe typical subtropical subsidence in the downward limb of the Hadley Cell, etc.

The North American pattern of desert distribution is not an ideal example of the planetary (subtropical) pattern. Deserts in North America are indeed more prevalent toward the subtropics, but the more obvious pattern is longitudinal — rain-shadow development in the lee of mountains. Africa is the best example of dryness based on latitude independent of topography.

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This paper would benefit greatly by a figure that shows (probably in cartoon style) the effects of tectonic uplift and seaway withdrawal in changing Asian climate toward wetter or drier. These changes could be addressed by a single four-part figure showing simulated summer and winter changes for each of the two types of tectonic changes. This figure would be convenient for comparing with the paleoclimatic indicators mapped in Figures 3 and 4.

Smaller issues:

Couldn't the relative duration of the summer and winter monsoons also be an important factor in determining soil type, rather than simply monsoon intensity?

I think that the phrase on p. 545 "permanent ice sheets in the northern Hemisphere only appeared since the late Miocene, 10-6 Ma" is misleading. I would agree that im-permanent ice of a modest size (mountain and piedmont glaciers) was present during and after that time, but not permanent ice sheets (and especially not large ones).

Figure 5: Are the long time series shown here the only ones relevant to the development of Asian climate? Should others be added?

Figure 8: The missing parts of the ML-V section in figure 8 should be listed in the captions as equivalent depths in QA-1.

The English is very good, and the meaning comes through clearly, but there is room for some slight improvement. For example, on p. 560, the meaning of "sharpen" was unclear. I would be willing to help the authors smooth the text prior to final submission.

Interactive comment on Clim. Past Discuss., 4, 535, 2008.

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