



## **MIO-PLIOCENE GROWTH OF THE TIBETAN PLATEAU AND EVOLUTION OF EAST ASIAN CLIMATE**

**Peter Molnar**

### **ABSTRACT**

During the past 15 years, numerous studies have reported not only ecological and climatic changes at 6-8 Ma, but also the roughly concurrent onsets of tectonic processes in regions surrounding the Tibetan Plateau. The ecological changes corroborate suggestions of a switch to a more arid climate on the northern margin of the Indian subcontinent, which were first made by vertebrate paleontologists working in Pakistan. Some of the more recent observations extend the region of increased aridity north and northeast of Tibet. Moreover, some evidence suggests that near 8 Ma, the wind fields over the Arabian and South China Seas simultaneously became more like those of the present day, which are dominated by the Indian and East Asian monsoons, respectively. The onsets of tectonic processes, which in nearly all cases are less precisely dated than the ecological and climatic changes, could have occurred as responses to a ~1000-2000-m increase in the mean elevation of the Tibetan plateau; a greater mean elevation of this amount should raise deviatoric compressive stresses on the margins of the plateau sufficiently to initiate deformation of these regions. The link between deep-seated tectonic processes and regional, if not global, environmental change is too tantalizing to ignore, but inconsistencies in timing and in mutual implications of the various observations by no means require such a link, or that such a change in Tibet's mean elevation occurred. I review the various observations objectively both to show the potential correlations and to expose these various inconsistencies.

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## INTRODUCTION

The approximate simultaneity of environmental changes within a few million years of 8 Ma in and near Tibet with tectonic events in the same area has been interpreted to suggest that the Tibetan Plateau grew rapidly at or just before ~8 Ma, and that the rise of the plateau affected regional climate changes, including a strengthening of the Indian monsoon (e.g., Harrison et al. 1992, 1995; Molnar et al. 1993). Although some of us have dedicated much of our research effort to addressing both the mechanics of Tibet's apparently rapid growth and the impact of its growth on regional climates, it might be appropriate to ask whether the facts motivating this effort merit consideration of such interconnected geodynamic and meteorological processes. In fact, new evidence gathered in the past 10 years has both added and removed support for such simultaneity and for an environmental response to a tectonic change. Hence, a review of such data is timely.

Will Downs and I became acquainted and then friends as I became interested in how tectonics, erosion, and climate change interact, and our interaction culminated in a collaborative paper that related results of erosion to climate change rather than tectonics (Zhang et al. 2001). Thus, a volume dedicated to him seems to be an appropriate place to discuss other data that bear on these interactions.

My main goal is to ask two specific questions. First, to what extent do paleoenvironmental data imply widespread environmental change in the Tibetan region near 8 Ma? Second, do tectonic and other observations imply a rapid change in Tibet's growth at ~8 Ma? Before reviewing the relevant observations, however, let me summarize the basic scientific hypotheses derived from observations that motivate a review and depend on them.

If crust shortens horizontally and thickens, so that isostasy supports a high mountain belt or plateau, the underlying mantle lithosphere must also shorten. Suppose that shortening occurs by pure shear (at least approximately), and not by "intracontinental subduction," the detachment of mantle lithosphere from the overlying crust (e.g., Mattauer 1986; Wellman 1979). Then, the shortening and thickening of cold, dense mantle lithosphere adds excess mass to the lithospheric column, with the likely result that its isostatic compensation supports a lower mountain belt or plateau (by ~1000 m) than if only crust thickened (England and Houseman 1989). Thickening of mantle lithosphere also requires downward advection of isotherms, so that

cold material beneath the region of thickening is juxtaposed next to hotter material, a thermal state that facilitates convective instability, which ultimately can remove part, if not all, of the thickened mantle lithosphere (e.g., Houseman et al. 1981). Many studies of such instability suggest that at least the lower half of thickened mantle lithosphere should be removed in less than 10-20 Myr after it has been thickened roughly two times (e.g., Conrad and Molnar 1999; Houseman and Molnar 1997; Houseman et al. 1981; Molnar and Houseman 2004; Molnar and Jones 2004), although some (e.g., Buck and Toksöz 1983) disagree. Removal of the thickened mantle lithosphere would lighten the load on the base of the lithosphere beneath a mountain belt or high plateau, and that already-high terrain should rise somewhat higher (~1000-2000 m; England and Houseman 1989). This process not only does not, but cannot imply that the entire ~5000-m mean elevation of the plateau would occur as a result of removal of mantle lithosphere; most of the high terrain must be supported by thick crust, as Airy (1855) envisaged 150 years ago. Moreover, whether Tibet has grown upward *en masse* or outward over time remains another open question.

Steady winds blowing northeasterly along the coast of east Africa and Arabia toward India characterize the Indian monsoon. Warm rising air over northern India and Tibet, southward flow in the upper troposphere, and subsiding air south of the equator over the Indian Ocean complete return flow that feeds the monsoonal winds. In summer, the hottest part of earth's upper troposphere lies above Tibet (e.g., Li and Yanai 1996; Webster et al. 1998). A high plateau contributes to this circulation because of the transparency of the atmosphere to both solar energy and infrared radiation. The air over the plateau warms and becomes warmer than air at the same pressure (and hence same altitude) over India, but the transparency of the atmosphere to infrared radiation prevents it from warming so much as to create thermal runaway (e.g., Molnar and Emanuel 1999). Most calculations of global climate using general circulation models of the atmosphere associate a higher Tibetan Plateau with a stronger Indian monsoon (e.g., Hahn and Manabe 1975; Kutzbach et al. 1989, 1993), and those that consider many possible mean heights show a monotonic increase in monsoon strength with height (e.g., Abe et al. 2003; Kitoh 2004).

For various reasons, calculations using general circulation models of the atmosphere do not demonstrate that a small change in height (of only 1000 m) should strengthen the monsoon by a large amount. Hence, perhaps more important than

these studies of a realistic atmosphere are theoretical arguments for a threshold in the north-south temperature gradient necessary for meridional (monsoon-like) circulation (e.g., Emanuel 1995; Plumb and Hou 1992). These arguments allow for the possibility that a small change in Tibet's height might be sufficient for the meridional temperature gradient to exceed the threshold needed for meridional circulation (e.g., Molnar et al. 1993).

## EVIDENCE OF LATE MIOCENE ENVIRONMENTAL CHANGE

### Stable Isotopes and Paleobotanical Evidence from the Indian Subcontinent

Vertebrate paleontologists working in northern Pakistan with Will Downs seem to have been the first to recognize a significant Mio-Pliocene climate change in that area. Flynn and Jacobs (1982) inferred a change near 7 Ma from forest to grassland from changes in fossil rodents, and Barry et al. (1985, 1995; Barry 1986, 1995) noted the same change from largely browsers before ~7 Ma to grazers afterward. The remarkable fossil record had been the stimulus for applying magnetostratigraphy to continental sediment, and dating of this material in the late 1970s and early 1980s provided an unusually good chronology.

To my knowledge, Quade et al. (1989) presented the first detailed record of apparent climate change in this region (Fig. 1). They showed pronounced changes in carbon and oxygen isotopes in pedogenic carbonates from northern Pakistan, just south of the Himalaya. Quade et al. (1989, 1995; Quade and Cerling 1995) showed an abrupt increase of more than 8-12‰ in  $\delta^{13}\text{C}$  beginning between 7.5 and 8 Ma (with adjustment to Cande and Kent's (1995) most recent time scale for geomagnetic polarity reversals) and finishing by 6 Ma. Analyses of enamel from fossil teeth showed the same pattern, if with less time resolution (Quade et al. 1992; Stern et al. 1994). Oxygen isotopes,  $\delta^{18}\text{O}$ , from pedogenic carbonates also show a clear change, of  $3 \pm 1\text{O}$ , from pedogenic carbonates also show a clear change, of  $3 \pm 1\text{O}$ , from pedogenic carbonates also show a clear change, of  $3 \pm 1\text{O}$ , from pedogenic carbonates also show a clear change, of  $3 \pm 12003$ ) who used Cande and Kent's (1995) time scale. Stern et al. (1997) inferred a similar change in  $\delta^{18}\text{O}$  values from the clay mineral smectite within the paleosol units. Analyses of  $\delta^{13}\text{C}$  and  $\delta^{18}\text{O}$  values from paleosols in Nepal also showed shifts, if smaller and less well resolved, between 6 and 8 Ma (Harrison et al. 1993; Quade et al. 1995). Moreover, France-Lan-

ord and Derry (1994) found that  $\delta^{13}\text{C}$  in organic carbon within sediment in the Bay of Bengal also showed this shift at  $7 \pm 1$  Ma. The changes in isotopic composition first seen by Quade et al. (1989) seem to characterize much of the northern Indian subcontinent.

Paleobotanical evidence corroborates some kind of environmental change. A change from forest, indicated by fossil leaves, to grassland, shown by fossil pollen, occurred at ~8 to 6.5 Ma in Nepal (Hooen et al. 2000). Although less precisely dated, Prasad (1993) noted a similar change farther west in the Indian Himalaya. Using both pollen and increasing pedogenic carbonates in the Thakkhola graben of Nepal, Garzzone et al. (2003) reported greater aridity since ~11 Ma, and especially near ~7 to 8 Ma,

These various observations both led directly to inferences of climate change and expose their non-unique implications. Quade et al. (1989) suggested that the isotopic shifts toward present-day conditions marked an onset or pronounced strengthening of the Indian monsoon. Later, however, it became clear that the shift in  $\delta^{13}\text{C}$  occurred elsewhere, if not everywhere at the same time (Cerling et al. 1993, 1997; Wang et al. 1994). Cerling et al. (1993, 1997) argued that the shift in  $\delta^{13}\text{C}$  implied global ecological change that enabled C4 plants to become dominant in some settings after a long interval of almost entirely C3 plants. As C4 grasses gain an advantage over C3 grasses when climates become seasonably arid, with pronounced dry seasons but not necessarily less rain (e.g., Ehleringer and Monson 1993; Ehleringer et al. 1997), the shift in Pakistan could reflect a change toward more monsoonal conditions. C4 plants also thrive when atmospheric  $\text{CO}_2$  becomes low (e.g., Ehleringer and Monson 1993; Ehleringer et al. 1997), but (as discussed below) inferences of global paleo- $\text{CO}_2$  concentrations cast some doubt on this explanation for the emergence of C4 plants. More importantly, the change in  $\delta^{18}\text{O}$  values seems to have occurred earlier than that in  $\delta^{13}\text{C}$  values, suggesting that whatever caused one might not be responsible for the other.

To examine seasonal variations in rainfall, Dettman et al. (2001) examined  $\delta^{18}\text{O}$  variation across freshwater bivalve shells from Nepal. In monsoon climates, such shells should record pronounced seasonal variations in  $\delta^{18}\text{O}$  values throughout the period of their growth, with wet-season values much more negative than dry-season values. The range of  $\delta^{18}\text{O}$  values, however, shows no obvious changes since 10.7 Ma that might suggest stronger wet and dry seasonality since that

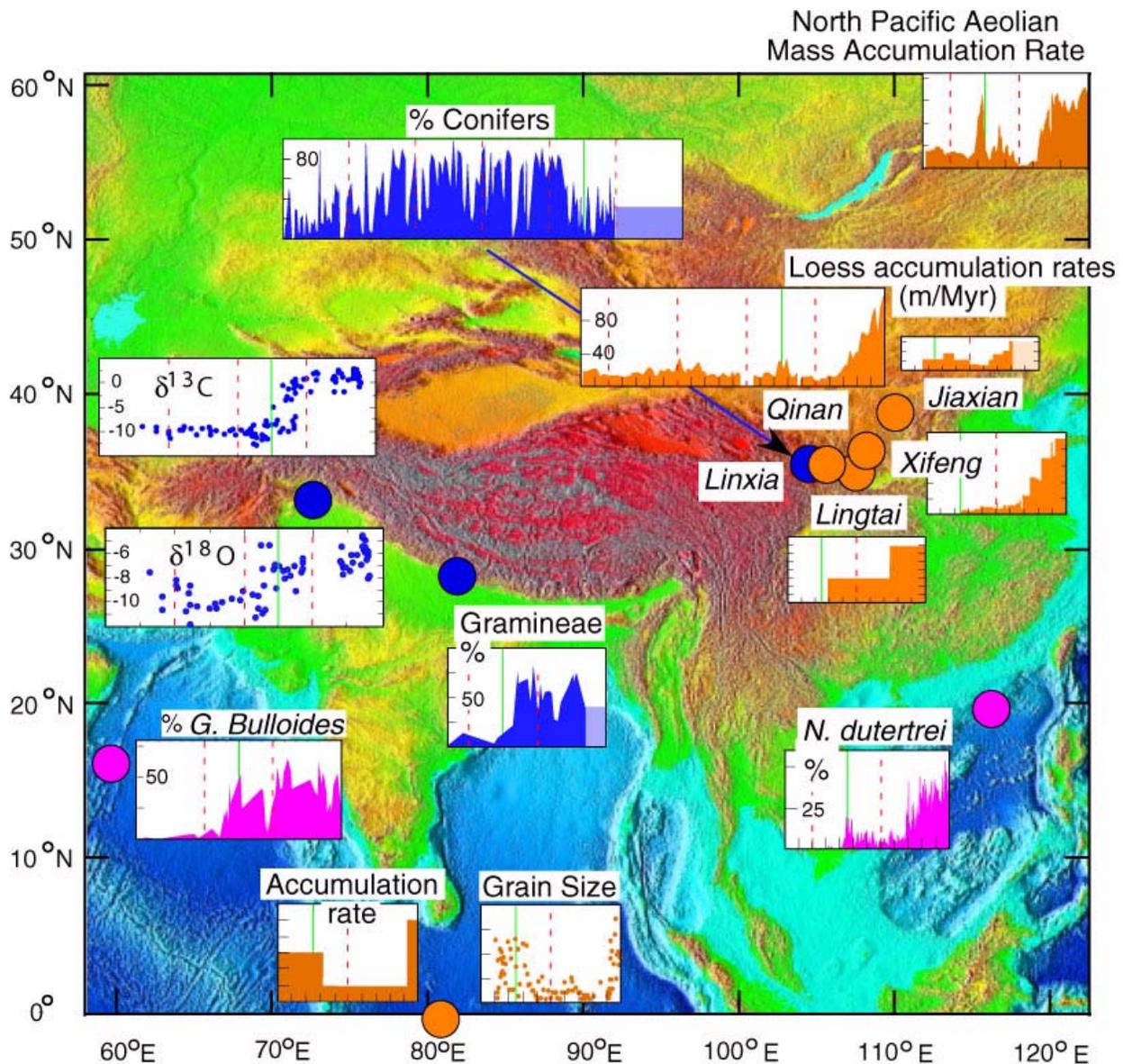


Figure 1. Map of eastern Asia showing time series of environmental changes. In all plots, time increases from the past on the left to present day on the right; red lines show 5-Myr intervals, the green line shows 7.5 Ma, and light shading for young parts of some plots show periods for which there are no data (no sediment, in most cases). Blue symbols indicate measurements of carbon ( $\delta^{13}\text{C}$ ) and oxygen ( $\delta^{18}\text{O}$ ) isotopes from pedogenic carbonates in Pakistan (Quade and Cerling 1995; Quade et al. 1989, 1992, 1995), percentages of grass (Gramineae) pollen from the Siwalik sediment of Nepal (Horn et al. 2000), and percentages of conifer pollen from the Linxia Basin, Gansu Province, China (Ma et al. 1998). Magenta shows proportions of microorganisms in Ocean Drilling Project cores: percentages of *Globigerina bulloides* in the Arabian Sea (Kroon et al. 1991) and of *Neogloboquadrina dutertrei* in the South China Sea (Wang et al. 2003b). Orange indicates loess accumulation and terrigenous sediment at the southern edge of the Bengal Fan, both the accumulation rate, with tick marks at intervals of 50 m/Myr, and mean grain sizes, with tick marks at 200-micron intervals (France-Lanord et al. 1993). Loess accumulation rates, plotted at the same scale, are shown for four regions: Qinan (Guo et al. 2002), Lingtai (Ding et al. 1999), Xifeng (Sun et al. 1998a,b), and Jiaxian (Qiang et al. 2001, who did not sample the top 3 Ma). Finally, aeolian deposition in the North Pacific (Rea et al. 1998) is plotted with tick marks at 0.2 kg/m<sup>2</sup>/kyr.

period. Thus, Dettman et al. (2001) concluded that insofar as the shells recorded seasonal variations in precipitation associated with the monsoon, the monsoon had changed little since 10.7 Ma. In fact, if their data did indicate a change at ~8 Ma, the change would be toward less seasonality, and hence perhaps a weaker monsoon in a more arid climate. Four of five of the pre-7.5 Ma wet season  $\delta^{18}\text{O}$  values presented by Dettman et al. (2001) are more negative (-9.5‰) than the six post-7.5 Ma wet season values (-6.5‰). They might suggest that the wet season was wetter prior to 7.5 Ma than after that time, which is consistent with aridification at ~7-8 Ma, but not with an increased seasonal (monsoonal) rainfall at that time. Thus, stable isotopes from the Indian subcontinent do not provide a firm footing for the inference of a stronger monsoon since 6-8 Ma than before that time.

In summary, most of the observations can be interpreted as a shift toward more arid conditions at 6-8 Ma. Moreover, the various factors that can contribute to a shift in  $\delta^{18}\text{O}$  values, which is one of the more precisely dated and clearer signals, do not allow them to resolve what climate change actually occurred or, more precisely, whether or not the Indian monsoon changed significantly.

#### **Winds over the Arabian Sea**

Results from the Ocean Drilling Project's Leg 116 off the southwest coast of Arabia gave impetus to the inference that the Indian monsoon strengthened near 8 Ma (Fig. 1). Seasonally changing winds, from the southwest in summer and from the northeast in winter, define the monsoon, and one manifestation of these winds is an upwelling of cold, nutrient-rich, deep water, especially during summer. The steady wind causes a transfer of the upper waters to the right of the wind direction (Ekman transport), and with the Arabian coast on the northwest side, cold water must upwell to replace the water that has been transported south-eastward during summer. In the present-day ocean, one foraminifer, *Globigerina bulloides*, dominates plankton in the northwest Arabian Sea (Curry et al. 1992; Kroon 1988; Prell and Curry 1981). Although most *G. bulloides* plankton live at high latitudes, this foraminifer reproduces abundantly during summer monsoons, and then virtually disappears between the summer and winter monsoons. *G. bulloides* currently comprises roughly half of the planktonic foraminifera in the northwest Arabian Sea, and it has done so since ~8 Ma (An et al. 2001; Kroon et al. 1991; Prell et al. 1992). It evolved before 14 Ma, but in the northwest Arabian Sea, it contributed only a few percent of the planktonic foraminifera until 8 Ma (Fig. 1). Thus, the

obvious inference, drawn by An et al. (2001), Kroon et al. (1991), and Prell et al. (1992) and exploited by others, is that the monsoon strengthened at ~8 Ma. Prell et al. (1992) supported this inference further using approximately simultaneous, qualitative changes in abundances of radiolaria that also seem to thrive during upwelling.

#### **Sedimentation in the Bay of Bengal**

Roughly 40% of the sediment derived from erosion of eastern Asia accumulates in the Bay of Bengal (Métivier et al. 1999). As a result of its exceptional thickness, the sediment volume can be constrained well (e.g., Curray 1994), but studying most of it is impossible. Ocean Drilling Project cores on the distal edge, however, recovered sediment spanning the last 16 Myr. This record revealed two surprises, at least to those of us who infer a strengthening of the monsoon from the increase in *G. bulloides* at ~8 Ma. At ~7 Ma, both grain sizes and sediment accumulation rates decreased at the distal edge of the Bengal Fan (Fig. 1; France-Lanord et al. 1993). Most would expect a stronger monsoon to convert rivers into torrents capable of carrying abundant large grains toward the plains of northern India and onward to the Bengal fan. Dating of this material is less precise than that in the Arabian Sea, and the decreases in both accumulation rates and grain sizes at 7 Ma could have begun at 8 Ma. Similarly, increases in accumulation rates and grain sizes at ~1 Ma might have begun earlier.

Aalto (1999; personal commun. 1999) suggested that the logic of associating increased grain sizes and accumulation rates with stronger monsoons may overlook an important internal shift within the fluvial dispersal system. The change from forests to grasslands at ~7-8 Ma could have altered the geomorphic regime and the locus of storage for fine sediment. Streams through forests are commonly wide, dynamic, and anastomosing, but those through grasslands are confined to deep, narrow, single-threaded channels until their discharge exceeds a sharply defined bank-full level. The anastomosing forested channels are inefficient at transporting gravel but efficient at transporting suspended fine sediment. Conversely, deep grassland channels are efficient at sluicing gravel to the delta, but floodwater conveyed over bank diverges across wide, hydraulically rough grasslands, and sands and silts are trapped. This mechanism is corroborated by many observations that the size of sediment accumulated switched from coarse channel deposits to fine overbank deposits in the Siwalik foreland basins (with a minimal change in total volumetric rates) at the same time

substantially more coarse sediment began accumulating in the delta. Because of the large volume of fine sediment now stored in the foreland floodplains, the transition to grassland channels might have decreased the discharge of sands and silts to the Bay of Bengal and distal reaches of the Bengal fan. If Aalto's hypothesis is correct, the decrease in accumulation rates and grain sizes at ~7 Ma at the distal edge of the fan need not be surprising.

Alternatively, the sediment deposited at the distal edge of the fan might not be representative of what rivers bring to the head of the fan. Currently, only one narrow channel transports all sediment entering the head of the fan across it (e.g., Curray et al. 2003), and the evolution and migration of that channel could bias records of accumulation 2000 km from the mouth of the river that delivered the material. In fact, as J. Quade (personal commun., 2004) pointed out, if coarser material were preferentially deposited in the Ganga Basin between 7 and 1 Ma, we might expect to see a higher deposition rate there during that interval, but Burbank et al. (1993) report the opposite. Thus, the observations of decreased grain sizes and accumulation rates at the distal edge of the Bengal fan suggest some kind of environmental change at ~7 Ma, but what change occurred remains unresolved.

#### Upwelling in the South China Sea

East Asia undergoes seasonal climate changes that are not simultaneous with the Indian monsoon, but that nevertheless share patterns typical of monsoons. In summer, moisture from the western Pacific and South China Sea enters South China. Depending on the strength of this circulation, moist air penetrates to North China or stops farther south. In winter, winds from the northwest transport cold dry air into central China. Like the Indian Monsoon, winds reverse seasonally in phase with reversals in meridional temperature gradients between Tibet and equatorial regions (e.g., He et al. 1987; Hsu and Liu 2003).

*G. bulloides* does not thrive in the South China Sea, but Wang et al. (2003a) suggested that the relative abundance of *Neogloboquadrina dutertrei* can provide a measure of monsoon strength in this region. *N. dutertrei* lives above the thermocline but below the mixed layer. Hence, the depth of the thermocline affects its productivity; when too deep, light cannot reach the organisms on which *N. dutertrei* feeds, but when shallow, it thrives. Wang et al. (2003a) showed that the percentage of *N. dutertrei* increased at ~7.6 Ma, from which they inferred a strengthening of monsoonal winds that caused a shoaling of the thermocline. A greater

increase in the percentage of *N. dutertrei* at 3-4 Ma (Fig. 1) presumably reflects yet stronger winds associated with an ice-age world.

#### Loess Deposition in China and Aeolian Sediment in the Pacific Ocean

In spring, winds over the deserts of Mongolia and western China sweep up dust and spread it over eastern China, if not much farther east. This process has a long history, for loess deposition had begun just west of the Loess Plateau, in Qinan (Fig. 1), by 22 Ma (Guo et al. 2002). Moreover, isotopic fingerprinting of aeolian sediment in the North Pacific indicates a constant source, the Gobi Desert region, since at least 12 Ma (Pettke et al. 2000). Loess accumulation increased at ~7-8 Ma; this can be seen in peaks of accumulation rates both at Qinan and in the North Pacific (Rea et al. 1998), and by the onset of loess deposition at other sites: Lingtai, 7.05 Ma (Ding et al. 1999), Xifeng, 7.2 Ma (Sun et al. 1998a,b), and Jiaxian, 8.35 Ma (Qiang et al. 2001). At these three latter sites, the basal loess overlies much older rock of a different type. What this increase in aeolian sediment transport and deposition implies for climate change remains open, but obviously some kind of climate change must have occurred.

#### Palynological and Isotopic Evidence of Aridification of the Linxia Basin

Fossil pollen spectra from the Linxia Basin show a number of changes near 8.5 Ma, with perhaps the most important being a decrease in conifer pollen (Fig. 1) and a concurrent increase in grass pollen (not shown in Fig. 1; Ma et al. 1998). These changes suggest that the region became more arid at this time (e.g., An et al. 2001). Moreover, less precisely dated pollen from the Qaidam Basin, west of the Linxia Basin, also suggest aridification in late Miocene time (Wang et al. 1999).

Dettman et al. (2003) reported a relatively small change in  $\delta^{18}\text{O}$  values, from -12‰ to -9‰ from the Linxia Basin near 12 Ma, which they associate with a shift in atmospheric circulation and a more arid climate. Superimposed on this baseline shift are brief intervals with much less negative  $\delta^{18}\text{O}$  values with the least negative reaching -2‰ to -3‰ between ~9.6 and 8 Ma. These values, from lacustrine sediment, suggest that lakes were closed and that evaporation was high in this period, consistent with marked aridification. These observations, which show a larger environmental change at ~12 Ma than at 8 Ma do not offer much support for a change at 8 Ma, but they do permit one at that time.

### To What Extent do Paleoenvironmental Data Imply Synchronous Environmental Change Near 8 Ma?

If one treats phenomena occurring within the interval between ~9 and ~6 Ma as simultaneous, then nearly all of the changes discussed above occurred simultaneously. Uncertainties in dating most of the marine records are less than ~1 Myr, except perhaps that for sediment accumulation on the Bengal Fan, for which the uncertainty is surely less than 2 Myr. In particular, magnetostratigraphy of terrestrial material gained much of its credibility from applications to the Siwalik series in Pakistan, and the magnetostratigraphic records from the loess in China are among the most impressive. The least accurately dated change discussed above is that of pollen in the Linxia Basin. Even ignoring it, there seems little doubt that environmental changes occurred near 8 Ma in the region surrounding Tibet.

Ignoring suggested changes at 11 or 12 Ma (e.g., DeCelles et al. 1998; Dettman et al. 2003), a closer look at timing of the others requires that changes within the period between 9 and 6 Ma not be simultaneous. Changes in  $\delta^{18}\text{O}$  occurred ~1-2 Myr before those of  $\delta^{13}\text{C}$ , and because the same samples were used for analyses of both isotopic systems, this difference seems to be required.

Most agree that the change in  $\delta^{13}\text{C}$  implies a change from a dominance of plants that use the C3 pathway for photosynthesis to a rise in importance of plants, grasses in nearly all cases, that exploit the C4 photosynthetic pathway. Environmental changes that could facilitate a shift from C3 to C4 plants include (1) a switch to more seasonally concentrated precipitation, perhaps associated with greater aridity, but not necessarily so, and (2) a reduced partial pressure of  $\text{CO}_2$  (e.g., Cerling et al. 1993, 1997; Ehleringer and Monson 1993; Ehleringer et al. 1997). Estimates of paleo- $p\text{CO}_2$  do not indicate much change since ~10 Ma (Demico et al. 2003; Pagani et al. 1999; Pearson and Palmer 2000; Royer et al. 2001; Van der Burgh et al. 1993), and thus ascribing the emergence of C4 plants at 6-7 Ma to changes in  $p\text{CO}_2$  requires ignoring this evidence. As noted above, variations of  $\delta^{18}\text{O}$  across freshwater bivalve shells, which measure amplitudes of seasonal differences in precipitation, show no indication of change near 8 Ma in Nepal and do not support the inference of increased seasonality (Dettman et al. 2001). Yet other observations, such as the shift from browsers to grazers (Barry 1986, 1995; Barry et al. 1985, 1995; Flynn and Jacobs 1982) and the shift from forest to grassland macrofossils and pollen (Garzi-

one et al. 2003; Hoorn et al. 2000; Ma et al. 1998; Prasad 1993), do corroborate a change consistent with some combination of greater aridity and (if one ignores the evidence of Dettman et al. (2001)) more seasonally concentrated precipitation. Thus, the association of the abrupt increase in C4 plants with such climate changes seems sensible.

The increase in  $\delta^{18}\text{O}$  values (wet season) near 8 Ma almost surely reflects a change in the water precipitated on the northern Indian subcontinent. Distinguishing the extent to which the increase, however, indicates a shift in the source of water, the degree to which  $^{18}\text{O}$  was rained out either where  $^{18}\text{O}$  was deposited or en route via the tendency for  $\delta^{18}\text{O}$  to become more negative with increasing rainfall (the "amount" and the "continental effects" of Dansgaard 1964), or the degree of evaporation of surface water (which is important because measurements exploit the carbonate record) remains open to debate. Accordingly, associating this increase with a strengthening of the monsoon must be considered speculative. In fact, the shift to monsoon rainfall should lead to a depletion of  $^{18}\text{O}$  in atmospheric water vapor that precipitates as far inland as northern Pakistan, not the increase in  $\delta^{18}\text{O}$  that has been observed. The simplest explanation for the increase in  $\delta^{18}\text{O}$  values is a change to a more arid environment, which is puzzling because it preceded the increases in  $\delta^{13}\text{C}$  values, which suggest the same change. Moreover, the evidence most suggestive of a strengthening of the monsoons, the increases in, especially, *G. bulloides* in the Arabian Sea and in *N. dutertrei* in the South China Sea, seem to have occurred before the change in  $\delta^{13}\text{C}$ , but apparently concurrently with that in  $\delta^{18}\text{O}$ .

These observations suggest a crude simultaneity of environmental change near 6-9 Ma throughout the region surrounding Tibet. Moreover, they can be tentatively associated with aridification at least north and northeast of Tibet, perhaps with more seasonally concentrated precipitation on the Indian subcontinent, and with stronger winds over the Arabian and South China Seas. Yet, the apparent difference in timing between changes within the period 9 to 6 Ma makes deducing cause and effect among possible physical processes speculative, if not premature. It is worth recalling that the features that we associate with the monsoons – seasonal winds of constant direction, seasonally concentrated rains, and very heavy rains – owe their origins to different aspects of the ocean-atmosphere system (e.g., Webster et al. 2002). Perhaps not all developed at the same time.

### TECTONIC EVENTS IN MIO-PLIOCENE TIME

As with environmental changes, a spectrum of observations suggests changes in the tectonic development of Tibet and its surroundings in late Miocene time. Some such events can be dated precisely at 7-8 Ma, and although dating of others remains less precise, many seem to have occurred near that time. If the growth of Tibet somehow affected regional climate, perhaps the most convincing test would be a demonstration that the mean elevation of Tibet changed concurrently with those climatic changes. So, let's begin with a discussion of paleoaltimetry and then address other observations.

#### Paleoaltimetry of Tibet

The best test of the suggestion that most, if not all, of Tibet rose 1000-2000 m around 8 Ma would be a demonstration of lower elevations before that time and higher after it. Asserted histories of Tibetan elevation changes abound, but in my opinion, those that might address the question posed here are indefensible, if not wrong, and those that are right do not address it. The weakest inferences are based on either one or more fossil plant organs and pollen, fossil mammals, deposition rates and facies of sediment deposited near Tibet, and qualitative geomorphic observations. Let me discuss them before considering more reliable inferences.

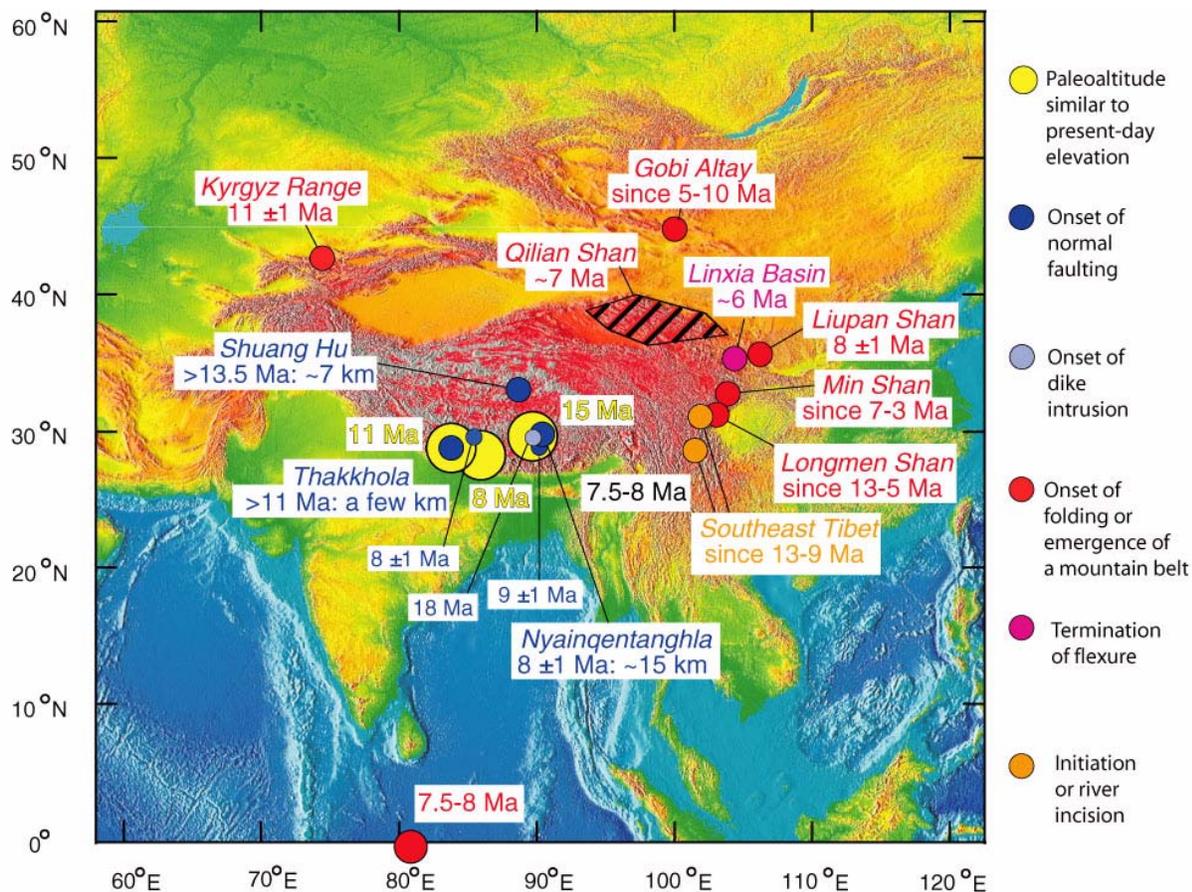
Axelrod (1981) and Xu Ren (1978, 1981, 1984) assigned nearest living relatives to fossil plants and pollen, and they then used the environments shared by the nearest living relatives to assign paleo-elevations to *southern* Tibet. Xu Ren (1981), in particular, reported a rise of 3000 m since late Pliocene time. Mercier et al. (1987) subdivided these and similar data regionally to suggest that different parts of the plateau rose at different times, but they too found a rapid late Cenozoic rise for most of the plateau. Although many have criticized this approach (e.g., Chaloner and Kreber 1990; Wolfe 1971, 1979), let me use one example to illustrate how far astray this approach can go. Axelrod (1966, p. 29) associated roughly 90% of the fossil plant taxa from the Eocene Copper River Basin in northeastern Nevada with nearest living relatives either from "the Coast redwood forest [that] extends from sea level up to near 400-500 feet," or with "the Spruce-Hemlock forest [that] has its lower margin near 4,500 feet." The gap of 4000 feet, more than 1000 m, in the elevation ranges separating roughly half of these modern taxa from the remaining half requires that roughly half of the taxa subsequently adapted to very different

present-day climates. Hence, the method not only ignores, but also is blind to evolutionary change.

Similar logic has been applied to fossil mammals, such as *Hipparion* (e.g., Li et al. 1981; Liu and Ding 1984). Proponents of a low Tibet in late Miocene and Pliocene time have associated such animals with warm environments that characterize regions outside of Tibet where fossil specimens have been found. Having excluded Tibet from those environments, these authors then deduced that for *Hipparion* and other animals to have lived on Tibet, Tibet must have been much warmer than it is today. This logic not only ignores late Cenozoic global cooling, but also denies the possibility that *Hipparion*, like modern horses, could have lived in a wide spectrum of environments, including Tibet.

Finally, many inferences that since 3-4 Ma parts, if not the whole, of Tibet rose from a low plateau only ~1000 m high to its present height derive from increases in sedimentation rates and in grain sizes of material deposited near Tibet (e.g., Li and Fang 1999; Li et al. 1997a, 1997b; Pares et al. 2003; Zheng et al. 2000) or from geomorphic inferences of recent down-cutting of rivers, like the Yellow River (e.g., Li et al. 1981, 1996). First, all such observations apply only to the edges of the plateau, not to the mean elevation of the large, internally drained, highest part of the plateau. Thus, even if these observations did relate to tectonic activity of Tibet, they would apply only to its outer edges. As did Will Downs, I think that most of these changes in erosion rates and grain sizes result from climate change (Molnar 2004; Molnar and England 1990; Zhang et al. 2001).

In the last few years, three studies have put bounds on middle to late Miocene paleo-elevations of three sites in southern Tibet, and all three imply that subsequent changes in height have been less than 1000-1400 m (Fig. 2). Garzzone et al. (2000b) measured values of  $\delta^{18}\text{O}$  in modern stream water to derive a calibration of its values with height; they then measured such values in pedogenic carbonates, shells, and lacustrine micrites dating from 11 to 7 Ma in the Thakkhola graben of Nepal and used their local calibration to infer paleo-altitude (Garzzone et al. 2000a). Rowley et al. (2001) used some data of Garzzone et al. (2000b) and Wang et al. (1996), among other samples, to calibrate a theoretical relationship based on Rayleigh fractionation; they applied it to sedimentary rock in grabens in southernmost Tibet. They reported no change in elevation since ~10 Ma, though their results allow systemically higher estimates of paleo- than present-day elevations. Finally, Spicer et al. (2003) used leaf physiognomy and Wolfe's (1993; Forest



**Figure 2.** Map of eastern Asia showing loci of inferred tectonic developments that began within a few million years of 8 Ma, with types of observations color-coded. Yellow dots show three locations where Garzione et al. (2000b), Rowley et al. (2001), and Spicer et al. (2003) reported elevations similar to those today at 11, ~10, and 15 Ma, respectively. Dark blue dots show places where northerly trending normal faults have been dated: Shuang Hu graben (Blisniuk et al. 2001), Thakkhola graben (Coleman and Hodges 1995; Garzione et al. 2000a, 2003), Nyainqentanghla graben (Harrison et al. 1995; Pan and Kidd 1992), and two other normal faults assigned dates of  $8 \pm 1$  and  $9 \pm 1$  Ma (Harrison et al. 1995); and the light blue dot shows where north-south trending dikes were dated as old as 18 Ma (Williams et al. 2001). Red dots show places where folding, thrust faulting, and crustal shortening seem to have begun in late Cenozoic time: Equatorial Indian Ocean (Cochran 1990; Curray and Munasinghe 1989; Krishna et al. 1998), Tien Shan (Abdrakhmatov et al., 2001; Bullen et al. 2001, 2003), Qilian Shan (Métivier et al. 1998), Gobi-Altay (Kurushin et al. 1997), Liupan Shan (Zheng et al. 2005), and the Min Shan and Longmen Shan (Kirby et al. 2002). The magenta dot shows the location of the Linxia Basin, where Fang et al. (2003) inferred that progradation of a foreland fold-and-thrust belt and flexure ceased in late Miocene time, and the locus of deformation moved far from this area. Orange dots show regions where late Cenozoic incision is inferred to result from a Late Miocene rise of the gentle surface of the southeastern Tibetan Plateau (Clark 2003; Clark et al. 2005).

et al. 1999; Wolfe et al. 1998) correlations of it with environmental parameters to infer a 15-Ma paleoelevation of the Namling basin in southernmost Tibet that is indistinguishable from that of today. These three papers rely on two different types of data ( $\delta^{18}\text{O}$  and fossil leaves) and three different methods. They imply that southernmost Tibet has undergone, at most, only small changes in elevation in late Cenozoic time.

As important as the studies by Garzione et al. (2000a,b), Rowley et al. (2001), and Spicer et al.

(2003) are in corroborating the methods used, they do not place tight bounds on the evolution of Tibet's *mean* elevation. Most students of Tibetan geology recognize that an Andean margin, characterized by high elevations, and not a low terrain punctuated by a chain of volcanoes, typified southern Tibet before its collision with India (e.g., England and Searle 1986; Murphy et al. 1997). Late Cretaceous and early Cenozoic volcanic rock overlying folded, late Cretaceous sedimentary and volcanic rock in southern Tibet demonstrates sig-

nificant pre-collisional crustal shortening (e.g., Chang and Cheng 1973). Thus, high paleo-elevations for *southern* Tibet offer no great surprise to those who imagine a rise of 1000-2000 m of the mean elevation of Tibet at ~8 Ma or a marked outward growth at that time.

### **Folding of the Indian Plate South of India**

Because quantitative paleoaltimetry has not constrained the elevation history of more than a fraction of the plateau, we must use surrogates for changes in both the mean elevation and the lateral extent of the plateau. The best dated, if also the most remote, tectonic change occurred south of India in lithosphere beneath the Indian Ocean (Fig. 2).

Excluding narrow plate boundaries, the earth's most seismic submarine region lies near the equator south of India (e.g., Stein and Okal 1978). Sykes (1970) seems to have been the first to notice this activity; he suggested that it marked the initiation of a new plate boundary. Shortly afterward, Eittreim and Ewing (1972) described young folding and faulting of this region. Bands of gravity and geoid anomalies with a characteristic wavelength of ~200 km trend approximately east-west across this region, and positive anomalies mark zones of localized deformation (e.g., Chamot-Rooke et al. 1993; Krishna et al. 1998; Van Orman et al. 1995; Weissel et al. 1980). Drilling of one such fold (Fig. 2) allowed an age of 7.5-8 Ma to be assigned to the onset of folding (e.g., Cochran 1990; Curray and Munasinghe 1989), and Krishna et al. (2001) later correlated and extrapolated sedimentary horizons to assign that age to folding over the entire region.

Using magnetic anomalies and fracture zones formed at the boundaries between the Somalia plate and the India or Capricorn plates, DeMets and Royer (2003) showed that convergence between the India and Capricorn plates was small until about 8 Ma. They could not rule out small relative motion at earlier times, and they reported small but resolvable movement between 20.1 and 17.4 Ma, but all of the folding seems to have occurred since 7.9 Ma. Less clear is a change in relative motion between the Somalia and India plates.

Although one cannot uniquely attribute the onset and continued deformation in this region to a single change in boundary conditions on the Indian lithosphere, an obvious possibility is that some aspect of India's convergence with Asia changed, and the force per unit length along their boundary also changed. As the "pressure gauge of Asia" (e.g., Molnar and Tapponnier 1978), Tibet applies a

force per unit length to India, and if Tibet rose to a sufficient height, the force (per unit length) resisting India's penetration into Eurasia could become large enough to deform the Indian lithosphere (e.g., Harrison et al. 1992; Molnar et al. 1993). A test of this idea can be made using simple estimates of the force per unit length needed to deform oceanic lithosphere (Martindale and Molnar 1995) and of the force per unit length necessary to maintain a high plateau at different mean elevations. Lateral support of a high plateau at a mean elevation of ~5000 m exceeds the force per unit length needed to deform oceanic lithosphere, but a plateau lower than ~4000 m can be supported by a force per unit length too small to deform oceanic lithosphere (Molnar et al. 1993). Thus, allowing for uncertainties in these estimates, an increase in elevation of ~1000-2000 m could have transformed the boundary condition on the northern edge of the Indian plate sufficiently to initiate deformation of it, and the ultimate separation of the Indo-Australian plate into the Indian and Australian plates.

My impression is that this folding is the most precisely dated large-scale tectonic event in the region that includes Tibet and its surroundings and that apparently occurred simultaneously with environmental changes in the region.

### **Deformation of Intracontinental Regions North and East of Tibet**

Although India collided with Tibet at ca. 45-50 Ma (e.g., Garzanti and Van Haver 1988; Najman et al. 2001, 2002; Rowley 1996, 1998; Searle et al. 1987), a variety of observations suggest that currently high terrain north and east of the high plateau developed long after that time.

**Tien Shan.** Magnetostratigraphy of sediment accumulating in intermontane basins suggests that they formed at ~10-13 Ma in much of the western Tien Shan (Abdrakhmatov et al. 2001; Fig. 2). The tightest constraints come from the Chu Basin on the northern edge of the Tien Shan in Kyrgyzstan and the adjacent Kyrgyz Range (Fig. 2). Using magnetostratigraphy, Bullen et al. (2001) showed that sediment accumulation had begun before ~9 Ma, though at a low rate. Cooling ages using both fission-track and (U-Th)/He dating suggest an abrupt onset of erosion of the Kyrgyz Range at  $11 \pm 1$  Ma, from which Bullen et al. (2001, 2003) inferred an emergence of this Range and the formation of the Chu Basin. Magnetostratigraphy from intermontane basins farther south suggests comparable ages, if slightly older (12-13 Ma) for the oldest sediment (Abdrakhmatov et al. 2001). Fission-track dates of ~20-25 Ma from other parts of the Tien Shan suggest that erosion was not neg-

ligible (e.g., Sobel and Dumitru 1997; Yin et al. 1998), and therefore that elevated terrain existed before late Miocene time. The filling of basins beginning at 10-13 Ma, however, suggests that mountain building became important at this later interval.

A late onset of mountain building gains some support from estimates of the total shortening across the Tien Shan and its current rate of shortening. Avouac et al. (1993) estimated ~200 km of north-south shortening across the western part of the belt between the Tarim Basin and the Kazakh Platform, although more recent work suggests that the amount may be less (e.g., Abdrakhmatov et al. 2001). The current rate across the same region as measured using GPS is ~15-20 mm/yr (Abdrakhmatov et al. 1996; Reigber et al. 2001), and in Kyrgyzstan, late Quaternary slip rates on the major faults match the GPS rates for that portion of a transect (Thompson et al. 2002). Thus, if the rate were constant, it would imply that all of the shortening occurred since ~10 Ma (Abdrakhmatov et al. 1996). In any case, the combination of present-day rates with the total shortening requires an acceleration of convergence since ~20 Ma, if not since 10 Ma.

**Gobi-Altay of Mongolia.** Evidence for timing of the rise of the Gobi-Altay (Fig. 2) is far less convincing than that for the Tien Shan. Using the ratio of the height of the broad, flat summit plateau on Ih Bogd, the highest mountain in the Gobi-Altay, above the adjacent basin to the vertical component of current rate of slip on the main oblique strike-slip fault north of Ih Bogd (Ritz et al. 1995), Kurushin et al. (1997) inferred that the summit plateau emerged since 5 to 10 Ma.

**Baikal Rift zone.** Although deposition in basins and volcanic activity suggest that rifting in the Baikal area began in Oligocene time but then accelerated in late Cenozoic time (e.g., Kaz'min et al. 1995; Kuzmin et al. 2000; Logatchev 1974; Mats 1993; Mats et al. 2000; Rasskazov et al. 2003). Kaz'min et al. (1995) argued that most sediment was deposited since Late Miocene time, and that faulting began at that time. Indeed, a rift seems to have been present at 5 Ma, for drill cores have penetrated sediment of that age (e.g. Kuzmin et al. 2000). Relying in part on the offset of a dike by a thrust fault dated at 12-14 Ma in the Baikal region (Ruzhich et al. 1972), Delvaux et al. (1997) inferred that thrust faulting and northeast-southwest shortening occurred until late Miocene time, when rifting began, if at a slow rate. Moreover, dating of volcanic rock in northern Mongolia suggests that grabens southwest of Lake Baikal itself began

to form at 10-8 Ma (Rasskazov et al. 2003). Thus, although a precise date for when rifting began cannot be given, much of the crustal extension associated with that process seems to have occurred since 10 Ma or so.

**Qilian Shan.** Similarly, although deformation seems to have occurred on the northeast margin of Tibet shortly after the collision between India and Eurasia, Métivier et al. (1998) inferred that the mountain ranges comprising the Qilian Shan and adjacent high terrain rose at ~5.3 Ma (Fig. 2). They relied on an apparently abrupt increase in sedimentation rate at that time in the Qaidam Basin, southwest of the high terrain. As others have emphasized from the distribution of Oligocene and Miocene sedimentary rock and differing grain sizes (e.g., Dupont-Nivet et al. 2004; Fang et al. 2003; Horton et al. 2004; Ritts et al. 2004; Wang et al. 2003b; Yin et al. 2002), eroding terrain must have lain near the various sedimentary basins in northeastern Tibet when that deposition occurred. Thus, the arguments of Métivier et al. (1998) do not imply that prior to ~5.3 Ma relief was absent, but rather that mountains of the present size seem unlikely. A weakness in the suggestion of a rapid rise at 5.3 Ma is that that age corresponds to the boundary between the Miocene and Pliocene Epochs; it requires that erosion rates, attributed to the emergence of adjacent terrain, accelerated precisely when marine microorganisms evolved, which seems causally impossible and therefore improbable. I do not doubt that sedimentation increased within a few million years of 5.3 Ma, but assigning a different date for the onset of rapid sedimentation also requires revising the sedimentation rates. Regardless of my doubts about the precise date when the Qilian Shan emerged, the observation that sedimentation increased rapidly at ~5 Ma in a basin dammed by high terrain to the north and northeast is consistent with a late Miocene emergence of that terrain then.

**Linxia Basin and adjacent edge of the Tibetan Plateau.** Using magnetostratigraphy, Fang et al. (2003) showed that sediment began accumulating in the Linxia Basin (Fig. 2) at ~29 Ma and slowly accelerated, as if within a foreland basin adjacent to a northeastward propagating fold-and-thrust belt. The accumulation rate then decreased at ~6 Ma. Fang et al. (2003) suggested that flexure ceased at that time, presumably because thrust slip on the fault creating the load flexing the basin slowed or stopped. Accordingly, they inferred that the locus of thrust faulting stepped northeastward.

In support of this timing, Zheng et al. (2003) measured detrital fission-track ages from the Linxia

Basin, and using Brandon's (1992) method for isolating populations of ages, they showed an abrupt change near 8 Ma, though possibly as early as 14 Ma, in the difference between the mean age of detritus and the depositional age (Fang et al. 2003). Zheng et al. (2003) interpret this change to document the emergence of the range just west of the basin, Laji Shan, where Proterozoic rock crops out widely.

**Emergence of the Liupan Shan.** The Liupan Shan, which lies ~200 km northeast of the Linxia Basin and is marked by a single ridge of high terrain, forms the northeasternmost zone of crustal shortening and thickening associated with the formation and outward growth of Tibet (Zhang et al. 1991). Using reset fission-track ages on detrital material deposited since Cretaceous time, Zheng et al. (2005) found that the older material had been heated sufficiently to anneal tracks and to reset ages, and then subsequently cooled since ~8 Ma. They inferred that thrust faulting, growth of the mountain range, erosion of it, and the resulting cooling of buried sediment began then.

**Min Shan and Longmen Shan.** Using both fission-track and (U-Th)/He dates from different elevations in these regions, Kirby et al. (2002) found slow cooling, at  $<1^{\circ}\text{C}/\text{Myr}$ , from Jurassic to mid-Miocene time, and then rapid cooling at  $30\text{-}50^{\circ}\text{C}/\text{Myr}$  (Fig. 2). For the Longmen Shan, rapid cooling began no earlier than 12-13 Ma and perhaps as recently as 5-6 Ma. For the Min Shan, it began no earlier than 6-7 Ma and perhaps as recently as 4-5 Ma. Kirby et al. (2002) suggested that mountain building began when cooling accelerated.

**Incision of southeast Tibet.** Also using fission-track and (U-Th)/He dates from elevation transects into deep gorges cut into southeastern Tibet, Clark et al. (2005; Clark 2003) inferred slow cooling at  $<1^{\circ}\text{C}/\text{Myr}$  in Cretaceous and early Cenozoic time, followed by rapid cooling beginning between 9 and 13 Ma (Fig. 2). They reported an average erosion rate during the period of slow cooling of  $<0.02\text{ mm}/\text{yr}$  ( $< 20\text{ m}/\text{Myr}$ ) on the interfluvies between deep gorges, and they concluded that the present-day gentle surface of low relief almost surely was flat and low when cooling was slow. Thus, since 9-13 Ma, the surface defined by this low-relief terrain rose ~2000 m. They buttressed this inference with a discussion of the present-day drainage and the possibility that rapidly rising terrain led to river capture (Clark et al. 2004).

**Summary.** The preceding discussion describes regions where tectonic activity seems to have begun long after India collided with Eurasia. In the context of tectonic processes that reflect the

growth of high terrain concurrent with regional climate change, the mention of a few caveats might help readers.

First, notice that the formation of basins and ranges within the Tien Shan began before 8 Ma. Thus, if that tectonic activity relates to growth of Tibet, it preceded most (but not all) of the environmental changes discussed above, if by no more than a few million years. Of course, a rise of Tibet by as much as 1 km surely requires a finite amount of time measured in millions of years. Conversely, if these tectonic developments in the Tien Shan reflect a change in the structure of Tibet, they imply that such changes began before the atmosphere responded to them, presumably because exceeding a threshold required a finite change in Tibet's mean elevation.

Second, with the uncertainties in ages, perhaps none of the inferred emergence of mountain ranges and higher elevations occurred simultaneously. Nor do any of these observations require a concurrent increase in elevation of the high part of Tibet. Rather, current uncertainties merely permit such a possibility.

Finally, all of these inferences depend on changes in erosion, incision, or sedimentation; none documents changes in mean elevations and none quantifies amounts of vertical movement. Tectonic activity is not the only trigger for increased erosion, and changes in erosion, incision, or sedimentation do not necessarily imply concurrent tectonically created relief (e.g., Molnar and England 1990; Zhang et al. 2001). From a converse view, however, if one hypothesizes that the surface of Tibet rose 1000-2000 m beginning a few million years before 8 Ma, and that the associated gain in potential energy per unit area induced deformation in surrounding regions, then these hypotheses pass tests set by the evidence presented above for late Miocene increases in erosion, incision, or sedimentation.

### Normal Faulting and East-West Extension of Tibet

When Harrison et al. (1992) suggested that Tibet had reached its maximum elevation at ~8 Ma, and regional climate changed at the same time, one fact that they used, and that Molnar et al. (1993) also exploited, was the date of ~8 Ma for the onset of slip on one major normal fault in Tibet. Dating along two transects of material in the fault zone on the southeast side of the Nyainqentanghla in Tibet (Fig. 2), supported by calculations of conductive heat transport, implies an initiation of faulting at  $8 \pm 1\text{ Ma}$  (Harrison et al. 1995; Pan and Kidd 1992). Moreover, from the cooling ages on the two

sides of the fault, Harrison et al. (1995) inferred 15–20 km of slip on the fault, which shows that this fault is not minor. Harrison et al. (1995) also reported, without giving details, that two other normal faults in southern Tibet formed at  $8 \pm 1$  Ma and  $9 \pm 1$  Ma (Fig. 2). Stockli et al. (2002) also obtained dates elsewhere in southern Tibet that corroborate this timing, but subsequent to Harrison's work, others have measured dates, from which they inferred an earlier onset of normal faulting.

Coleman and Hodges (1995) dated hydrothermally deposited mica in north-south extension fractures in the Greater Himalaya near the Thakkhola graben (Fig. 2). They used their date of  $\sim 14$  Ma to demonstrate normal faulting and east-west extension at that time. Later Garzzone et al. (2000a, 2003) showed that sediment had accumulated in the Thakkhola graben by 11 Ma, and like Coleman and Hodges (1995), they questioned an 8-Ma onset of east-west extension via normal faulting. I do not think that normal faulting in this setting necessarily bears on the question of when east-west extension of Tibet began; in some regions of oblique subduction, analogous normal faulting occurs within the overriding plate (e.g., Ekström and Engdahl 1989; McCaffrey 1992). If the faulting associated with the Thakkhola graben bears a similar relationship to oblique convergence, it need not reflect the deformation field farther north within Tibet.

Williams et al. (2001) dated north-south trending dikes in southern Tibet (Fig. 2) and obtained ages of  $13.8 \pm 0.8$  to  $18.3 \pm 2.7$  Ma. They argued that these dates imply that east-west extension of Tibet had begun, and thus Tibet had reached its maximum elevation by this time. As Nakamura (1977) showed for island-arc volcanoes, dikes tend to form orthogonal to arcs, presumably because the maximum compressive stress is aligned in that direction, and hence the least compressive horizontal stress is oriented parallel to the arc. Dike injection does not imply that significant strain occurs parallel to the arcs, but simply that horizontal compressive stresses resist intrusion of dikes oriented perpendicular to the arcs less than dikes oriented parallel to the arcs. I consider the existence of potassium-rich basaltic dikes in southern Tibet (see below) more of a surprise than their orientations, and that the orientations of dikes do not demonstrate the attainment of a maximum elevation or the onset of significant east-west extension.

Among normal faults dated in Tibet, that for the Shuang Hu graben in central Tibet (Fig. 2) poses the biggest problem for those of us who cling to the view that Tibet reached its maximum

elevation after, not before,  $\sim 10$  Ma. Using Rb-Sr and  $^{40}\text{Ar}/^{39}\text{Ar}$ , Blisniuk et al. (2001) determined ages of mineralization in a fault zone as 13.5 Ma, which they consider a minimum age for the onset of faulting. Noting that Yin et al. (1999) had inferred a vertical offset of  $\sim 7$  km for the faulting on the west side of the graben, Blisniuk et al. (2001) inferred that the faulting that they dated was significant. They argued further that this faulting called into question proposed relationships between normal faulting and tectonic processes on the margin of the plateau, relationships that motivate this paper. Yet, perhaps one should recall that normal faulting often occurs at discontinuities in strike-slip faults, where one strand steps to another parallel strand. A good example is along the Xianshuihe fault in eastern Tibet, where fault-plane solutions of earthquakes show normal faulting in the pull-apart zone between parallel strike-slip strands (e.g., Molnar and Lyon-Caen 1989; Zhou et al. 1983). Many of the grabens in central Tibet lie along strike-slip faults (e.g., Taylor et al. 2003), and thus the relationship of the Shuang Hu graben to strike-slip faulting may serve as a reminder that normal faulting can occur in local pull-apart basins along strike-slip faults and not be associated with regional extension. (Near Death Valley in the Basin and Range Province, thrust faulting in the Avawatz Mountains at the east end of the Garlock fault provides a spectacular exception to the rule that normal faulting dominates late Cenozoic deformation of that Province.)

The preceding paragraphs on normal faulting may read like a political argument trying to refute claims by opponents. When Pan and Kidd (1992) first presented evidence that the Nyainqentanghla faulting dated from approximately 8 Ma, and then Harrison et al. (1995) trimmed its uncertainty to only 1 Myr, the result seemed too good to be true. Perhaps those of us who used that single date to assign a major role to Tibet and its underlying mantle (e.g., Harrison et al. 1992; Molnar et al. 1993) strained its applicability. At the same time, it seems to me that the few reliable dates of faulting from the interior of the plateau leave open the timing of Tibet's attaining its maximum mean elevation.

### **Cenozoic Potassium-Rich Basaltic Volcanism**

Turner et al. (1993, 1996) interpreted late Cenozoic, potassium-rich volcanism in northern Tibet to reflect melting of mantle lithosphere. They assumed that potassium and other large-ion lithophile elements had seeped into the lithosphere from an underlying partially molten asthenosphere, and their presence in erupted lavas implied melting

of the lithosphere. Following England and Houseman (1989), they suggested that such lithosphere had thickened in response India's penetration into Eurasia, and then removal of the lower part of that thickened lithosphere exposed its upper parts to temperatures high enough to remelt it, as potassium-rich volcanism.

Although Arnaud et al. (1992) associated such volcanism with intracontinental subduction, Molnar et al. (1993) used both the logic given by Turner et al. (1993, 1996) for convective removal of mantle lithosphere and the relatively young ages of such volcanism, mostly <10 Ma, as further support for late Cenozoic removal of mantle lithosphere. Subsequently, others have reported older potassium-rich volcanism elsewhere in Tibet (e.g., Chung et al. 1998), and in particular in southern and southwestern Tibet as early as ~25 Ma (Chung et al. 2003; Miller et al. 1999; Nomade et al. 2004; Williams et al. 2001) to 42 Ma in southeastern Tibet (Wang et al. 2001). Thus, using young basaltic volcanism in northern Tibet as an argument for a late Cenozoic rise of Tibet seems to have been a mistake, and I omit it from Figure 2.

#### **Do Tectonic (and Other) Observations Imply a Rapid Change in Tibet's Growth at ~8 Ma?**

When Harrison et al. (1992) proposed that Tibet reached its maximum elevation near 8 Ma, they relied on the dating of one normal fault (Nyainqentanghla), one fold south of India, and some of the environmental changes discussed above. Molnar et al. (1993) both developed these arguments further and added late Cenozoic potassium-rich volcanism to them. Since that time, much new data have been added, with the 8-Ma onset of normal faulting and crustal thinning within the plateau becoming questionable, and volcanism becoming irrelevant to changes in Tibet's elevation. The initiation of folding south of India provides the only major tectonic event that can be both associated with Tibet's growth and shown to have occurred simultaneously with the environmental changes discussed above. This reduction in supporting evidence might seem to deny Tibet's growth a relationship to regional environmental change.

As summarized above, the dating of many features in the areas surrounding Tibet over the past 10 years can be interpreted to suggest that the high terrain north and northeast of Tibet and on the northeastern and eastern margins of Tibet developed in late Miocene time. Several aspects of these dated phenomena, however, make tying them all to a late Miocene pulse in the growth of

Tibet a bit like a house-of-cards. First, in most cases, dating is not sufficiently precise to require them to be simultaneous, and for one region, the Tien Shan, the onset of rapid erosion of mountainous terrain and deposition of adjacent sediment preceded 8 Ma by a few million years. Moreover, virtually all of the phenomena dated—erosion, incision, cooling of deeply buried rock, and accumulation of sediment—do not by themselves require a change in tectonic processes. The one clear exception is the ratio of total shortening across the western Tien Shan of at least 100 km (Abdrakhmatov et al. 2001) and perhaps 200 km (Avouac et al. 1993) to the current rate of shortening of 15–20 mm/yr (Abdrakhmatov et al. 1996; Reigber et al. 2001), which requires a marked acceleration of shortening in Late Cenozoic time. Skeptics, therefore, cannot be faulted if they reject the assertion that Tibet rose near 8 Ma and its rise effected regional climate change.

It seems to me that alternative views of the data presented here have comparable validity with that offered above to a skeptic. First, if Tibet did rise 1000–2000 m just before 8 Ma, it would have imposed a higher deviatoric compressive stress on its surrounding regions. Accordingly those surroundings might undergo accelerated horizontal shortening and mountain building. Such shortening and associated crustal thickening would in turn create additional potential energy of erosive agents. Thus, the evidence suggesting an abrupt change in erosion, or incision, of Tibet's surroundings allows an abrupt rise of Tibet to pass one test made of it.

From another perspective, suppose that the arguments used above do call for crustal shortening, and not accelerated erosion due to climate change. Then, recall that India collided with southern Asia at ~45–50 Ma and has penetrated into Eurasia at an essentially constant rate since that time. The apparently rapid onset of such deformation, in the period since 15 and before 5 Ma and across a wide region surrounding Tibet implies a change in processes within Tibet, even if the evidence is insufficient to define the specific processes that changed.

Thus, the answer to the question – do tectonic (and other) observations imply a rapid change in Tibet's growth at ~8 Ma? – lies somewhere between a cautious *no* and a risky *yes*. Like Will Downs, I like taking risks in science. Accordingly, the replacement of the word "imply" with "suggest" would elicit a stronger *yes*, but the word "require" would demand a *no*.

## CONCLUSIONS

During the period within a few million years of 8 Ma, a wide variety of changes took place in the region surrounding and including the Tibetan Plateau. These changes span a range of phenomena including the onset of folding and faulting of oceanic lithosphere and environmental changes north and south of Tibet. Together they motivate exploration of mechanisms by which the growth and development of Tibet might have affected regional environments, but because of various inconsistencies among the observations, it is perhaps no wonder that only a few of us pursue this topic.

Dating of several environmental changes suggest that in the period between ~9 and ~6 Ma, much of eastern Asia either became drier or precipitation became more seasonally concentrated. These changes, however, were not synchronous. Increases in abundances of microorganisms sensitive to winds over the Arabian and South China Seas, in rates of aeolian deposition over China and the Pacific Ocean, and in values of  $\delta^{18}\text{O}$  in pedogenic carbonates near 8 Ma occurred before changes in  $\delta^{13}\text{C}$  and apparently before other evidence for increased aridity and/or seasonal precipitation. Any understanding of these changes must account for the lack of simultaneity, to say nothing of evidence inconsistent with such environmental changes (e.g., Dettman et al. 2001).

Both the nature of tectonic events and their dating make their relationship to the growth of the Tibetan Plateau non-unique. First, dating is too imprecise to show that most occurred at, or just before, the environmental changes. Moreover, it appears that some tectonic developments definitely began before those changes. Thus, on the one hand, one can concoct explanations for differences in timing in various places, so that one can interpret the observations of tectonic development in terms of growth of Tibet, and then as the stimulus for resulting climate, or environmental, change. On the other hand, some, but not all, suggestions of abrupt onsets of tectonic development near or just before 8 Ma might be explained by some kind of climate change that accelerated erosion, river incision, and sedimentation. Suffice it to say that the rough simultaneity of these events will motivate us to pursue the link between these various phenomena, but that others should be justifiably skeptical about the speculative thinking that still must be employed to make such a link.

The most convincing test that a significant part of the Tibetan Plateau rose ~1000 m (or more) at ~ 8 Ma would be the demonstration of lower elevations before that time and confirmation that the

plateau was as high or higher (by ~500 m) at ~ 8 Ma. Present knowledge suggests that carrying out such a test will be difficult. First, fossil leaves that could be used to estimate paleo-elevations appear to be sparse on the Tibetan Plateau, and finding enough to yield convincing paleo-elevations appears, at present, to be impossible. Moreover, although oxygen isotopes can be measured from pedogenic carbonates and from clay minerals on Tibet, how they should be interpreted remains uncertain. As Garzzone et al. (2004) showed, diagenesis can destroy records of past local climate. More discouraging is the absence of a comprehensible dependence of  $\delta^{18}\text{O}$  on elevation, measured both from precipitation over northeastern Tibet (Araguas-Araguas et al. 1998) and from stream water that samples annual precipitation (Garzzone et al. 2004). Thus, extracting direct measures of paleo-elevations appears difficult, at least on the northern side of the plateau.

Despite the numerous phenomena that seem to have occurred near 8 Ma, dating of many remains sufficiently imprecise to demonstrate simultaneity or its absence. In a search for the ideal field area, the northeast margin of the plateau might allow precise timing of both tectonic and local climatic events using the same material.

Tests of both a rise of Tibet at or just before 8 Ma and resulting climate changes may require theoretical developments that yield predictions to be tested. For instance, if we understood how Tibet perturbs regional climates and we could confidently predict how different distributions of elevations affected climate, we could use the paleoclimatic record outside of the plateau, such as records of loess deposition, pollen and other paleobotanical fossil organs, or stable isotopes, to test possible past elevation distributions. My impression is that we can calculate possible paleoclimates with atmospheric general circulation models, but we lack the understanding of how the many interrelated phenomena affect calculations; hence we cannot yet use such calculations to draw unique inferences.

A rise of the high interior of Tibet at ~8 Ma requires increased horizontal normal deviatoric stress for its support and therefore should facilitate compressive deformation of the flanks of the plateau. Thus, a documentation of rapid growth of the margins of the plateau beginning near 8 Ma would allow the inference of such growth to pass a test. Conversely, a demonstration that the plateau has grown outward steadily for tens of millions of years would cast doubt on an abrupt rise of its interior. Perhaps most convincing would be the demonstration that normal faulting and crustal extension

became important at ~8 Ma. This demonstration requires the study of many normal faults.

If mantle lithosphere has been removed recently in geologic time (since ~10 Ma), cold material should lie beneath Tibet. Seismological confirmation of such material would allow the idea of such removal to pass a test, but a demonstration, for instance, of intact sub-lithospheric mantle beneath the majority of Tibet would cast doubt on this idea.

The variety of studies that can be used to test ideas of Tibet's growth and its impact on Tibet call for a multidisciplinary approach, and it seems safe to assume that no single study will settle the debate conclusively. Moreover, although the best approach seems to be to pose tests of ideas, the non-unique interpretations of most observations will enable such tests to falsify suggested interrelationships between Tibetan growth and regional climate change, but not confirm them. Most likely, someone will stumble onto some new measurement or new observation that we have not anticipated, and it will provide the most definitive test.

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