

# FORUM

## Diachrony of mammalian appearance events: Implications for biochronology: Comments and Reply

### COMMENT

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Alroy (1998) argued that there is significant diachrony in the first and last appearance events (FAE and LAE) of Cenozoic mammals in North America, and therefore individual events cannot be used for biochronology. A close examination of the data used in his paper, however, suggests that (1) most of the diachrony is largely a sampling artifact, and (2) once sampling is factored out, the remaining apparent diachrony would have little effect on the biochronological correlations that have been established for over a century.

These points are demonstrated by Alroy's (1998, Fig. 1A) plot of FAEs of the Midcontinent regressed against the FAEs of the West Coast. Almost all of the outliers are on the West Coast side of the regression line, and there are large gaps in the data for most of the Paleocene–early Eocene (Puercan–Wasatchian) and late Eocene–late Oligocene (Chadronian–Whitneyan). Both of these effects are due to well-known gaps in the West Coast mammalian record (see chapters in Woodburne, 1987), which has yielded only one sparse assemblage of Paleocene age, only two sparse Wasatchian assemblages, and no Chadronian, Orellan, or Whitneyan assemblages. (Alroy [personal commun.] includes the Chadronian Kishenehn faunas of eastern British Columbia as “West Coast,” but these faunas are really a part of the Rocky Mountain region, both geologically and faunally.)

Any comparison between two such unequal records will inevitably yield large diachrony values, simply because of the large stratigraphic gaps in the West Coast. Alroy (personal commun.) provided me with a list of the ten “worst offenders” among FAEs. Most of these taxa (*Thylacaelurus*, *Domnina*, *Pseudotrimylus*, *Mystipterus*, *Anchitheriomys*, *Nyctitherium*, *Plionictis*, *Mytonomys*, *Paramys*, *Leptodontomys*) are small, relatively rare mammals that rarely have been important in biochronology. In addition, anyone with extensive first-hand experience in identifying the Miocene faunas of the West Coast knows they are much scappier and less complete than those of the Midcontinent, with many erroneous or tentative identifications based on fragmentary specimens. If many of these uncertain identifications were thrown out, the apparent diachrony might diminish even further.

Instead of including all available taxa, most of which are rare and subject to sampling problems and historically have not been important in North American mammalian biochronology, Alroy's point would be better demonstrated if he were to focus on mammals (such as those given by Woodburne, 1987, Fig. 10.1) which were explicitly designated as index taxa for mammalian biochronology. If this list were to show significant diachrony (greater than the available chronologic resolution), then there might be serious concern about using fossil mammals as time indicators. But comparisons based on rarely sampled taxa that were not important to the original biochronologic framework are of dubious value.

I thank John Alroy for providing data, and S. L. Walsh and M. O. Woodburne for comments.

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### COMMENT

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Alroy (1998) analyzed the apparent diachrony of fossil mammal taxa between the West Coast and Midcontinent regions of North America, on the basis of methods and data in Alroy (1994) and his personal paleofaunal database. While I applaud Alroy for attempting to quantify this subject, several aspects of his paper require comment.

First, because the frequency distribution of the diachrony values for various classes of taxa is skewed by a relatively small number of large values, the high mean values given by Alroy are misleading and should be put into context by providing medians as well. For example, for the 407 genera analyzed by Alroy, the median FAE diachrony is 1.50 m.y. (i.e., 203 genera have diachrony values less than this, and 203 genera have values greater than this). This median value is much smaller than the mean overall FAE diachrony of 2.66 m.y. (Alroy, 1998, p. 25).

Alroy's diachrony calculations also include all genera known from at least one faunal list in each region. The inclusion of such rare (or regionally rare) genera would be expected to inflate the average apparent diachrony values. Table 1 displays the parameters for the FAEs of six common genera often used to define the beginning of the Uintan North American Land Mammal “Age” (NALMA; e.g., Woodburne, 1987). I have also separated the number of faunal lists in which a genus appears for each region, making it easier to discern regional collecting biases (data from <http://homebrew.si.edu/nampfd.html>).

These values are low, with one exception. *Ephippus* shows an apparent diachrony of 6.36 m.y. but is known from only one faunal list in the West Coast vs. 31 lists in the Midcontinent. Collecting bias may thus be a factor here, although the absence of *Ephippus* in the well-sampled early Uintan rocks of southern California suggests that some true diachrony may also be involved. In either case, average diachrony values for the common taxa that are generally used to define NALMA boundaries are likely to be less than the overall average values reported by Alroy (1998). Indeed, although Alroy's (1998, p. 24) conclusion that “correlations to the subage or ‘zone’ level cannot be based on individual FAEs” is often true, his data also show that 127 (31.2%) of the 407 analyzed genera have FAE diachrony values less than 0.5 m.y., while 167 genera (41.0%) have FAE diachrony values less than 1.0 m.y.

Alroy (1998) addressed the role of sampling bias by regressing “sampling probability” against diachrony of FAEs and LAEs on a logarithmic scale. Alroy (1998, p. 26) asked: “how can rarity be defined without already having temporal correlations to start with?” One answer to this question would be by counting the number of faunal lists in each region that

TABLE 1. PARAMETERS FOR FAEs OF SIX COMMON GENERA

Genus	No. of lists (w/m)	FAE-w (Ma)	FAE-m (Ma)	Diachrony (m.y.)
<i>Amyrnodon</i>	16/33	46.20	47.06	0.86
<i>Ephippus</i>	1/31	40.70	47.06	6.36
<i>Metarhinus</i>	10/3	46.27	46.95	0.68
<i>Leptoreodon</i>	33/13	46.07	46.07	0.00
<i>Protoreodon</i>	24/37	46.07	46.85	0.78
<i>Protylopus</i>	18/4	45.47	45.04	0.43

Note: w—West Coast; m—Midcontinent.

contain a given genus, taking the smaller of these values, and then plotting it against the apparent diachrony of that genus. As the number of minimum regional lists increases, average apparent diachrony would be expected to decrease. Although this method could not detect the difference between “lumped” and “split” fossil localities, it would allow easier visual identification of genera that appear on many lists in both regions, but still show a large amount of apparent diachrony. This situation could then be explained by stratigraphic incompleteness in one region (Alroy, 1998, p. 25), or by “true” diachrony in the dispersal or extinction of that genus.

Stratigraphic incompleteness is indeed a major cause of apparent diachrony. For example, the FAE diachrony of the long-lived genus *Peradectes* is 12.00 m.y. (FAE-w = 52.77 Ma; FAE-m = 64.77 Ma). This genus occurs in many lists in each region, but because Paleocene mammal-bearing rocks are virtually unknown from the West Coast, its oldest known record there is early Eocene. Given the inclusion of taxa such as *Peradectes* in the dataset, it is uncertain whether the results of Alroy’s analysis have any general paleobiological significance. Are the average diachrony values for each class of taxa (and the values of  $D_p$  obtained by Alroy, Table 1) applicable to all continents, or would the combination of stratigraphic incompleteness and other biases unique to each continent result in quite different values for these parameters, e.g., perhaps reversing his conclusion that LAEs are generally less diachronous than FAEs? If possible, paleontologists would like to eliminate obvious stratigraphic and collecting biases at the outset to better evaluate the existence of any true diachrony in the dispersal and extinction of taxa (cf. Springer, 1990, p. 512).

Alroy’s main conclusion (1998, p. 23) is that “joint analyses of all known appearance events are preferable to the a priori selection of particular appearance events as time indicators.” Although I partly agree, the phrase “time indicators” is ambiguous here, and a distinction must be made between the concepts of “defining taxon” (a taxon that conceptually defines a biochron boundary) and “index taxon” (a common taxon restricted to a particular biochron). For example, Rose (1980) defined the beginning of the Clarkforkian NALMA by the initial immigration of the rodent *Paramys*, but because this genus also ranges into younger NALMAs, it is not an index taxon for the Clarkforkian. Obviously, the FAE diachrony of a given taxon can potentially exceed the duration of a given biochron only if the temporal range of the taxon is greater than that of the biochron. But by definition, such taxa are not index taxa, so they are not used as “time indicators” in this sense. Finally, paleomammalogists are aware that the lowest stratigraphic datums (LSDs) of most boundary-defining taxa will seldom be synchronous in sections scattered throughout a continent, but will often be more or less diachronous depending on various biases (e.g., Walsh, 1998). Thus, the discovery of fossils of those defining taxa is a “time indicator” only in the sense that it demonstrates that a given biochron boundary has been crossed; it does not imply that the LSDs of these taxa must approximate a chronohorizon. I thank J. Alroy for providing data, and Alroy, S. Lucas, D. Prothero, and M. Woodburne for discussions; their agreement is not implied.

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#### REPLY

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I welcome this opportunity to clarify some contentious issues raised by my recent paper (Alroy, 1998). Walsh’s comments are consistent with my fundamental conclusions; their main purpose is to discuss related issues. By contrast, Prothero’s comments fail to address my main argument, err on several factual points, and hinge on a speculation that is not borne out by the data. I will begin by rebutting Prothero’s key arguments, and then explain how Walsh’s concerns relate to my own.

Prothero (like Walsh) agrees with me that observed interregional diachrony is substantial for genus-level mammalian appearance events, and that most of this diachrony originates from undersampling. His fundamental objection is that diachrony has never been recognized as a problem for the traditional, subjective time scale, which suggests to him not that I have made a useful observation, but that earlier workers have somehow factored out the effect. He therefore dismisses my evidence that the most coherent of all traditional schemes (i.e., the IFAD list of Woodburne and Swisher, 1995) depends on highly diachronous events. Instead, he suggests that my analysis is “of dubious value” because I chose not to analyze an earlier, and even more subjective, master list of biochronologically interesting taxa. This was proposed by the 41 authors of the last major time scale revision (Woodburne, 1987).

There are two basic problems with Prothero’s argument. First, even if it were true that these workers had successfully recognized and ignored diachronous taxa, there would be no simple way to show that this was an a priori judgment. Indeed, the evidence concerning Walsh’s rarity statistic that I give below tends to challenge the assertion that this type of independent judgment is even possible. As I suggested previously, it is therefore more likely that workers first construct a biochronological framework by taking all evidence into account, and only then identify diachronous taxa. All I have tried to do is make this procedure quantitative and explicit.

Second, the first appearances of the taxa listed in 1987 turn out to show even more diachrony than do the IFADs listed in 1995. I analyzed the 193 genera on the earlier list, excluding an additional handful of species-level appearances for the sake of consistency. Many of the genera are listed erroneously in more than one biochron, and even more disturbingly, 11 (6%) are based on unpublished occurrences, are nomina dubia, or are junior synonyms, and 84 (44%) are found only in the “West” or “Midcontinent.” For the remaining 98 genera, I found mean FAE diachrony of 3.34 m.y.—but mean FAE diachrony of merely 2.45 m.y. for the 309 other widespread genera that these 41 experts omitted. So instead of picking out relatively reliable events, the 1987 list emphasizes unusually diachronous first appearances. As with my original results, LAEs are far less diachronous for both subsets; the respective values are 1.67 m.y. and 2.14 m.y.

Prothero also raises a series of minor and essentially irrelevant concerns. He emphasizes that the “West” region is less well sampled than the “Midcontinent” region. But this obscures the fact that correlating from well to poorly sampled regions is precisely the purpose of a continental biochronology. No simple geographic split of the continent could attain a completely even partitioning of the lists, and even if such a split were possible, analyzing it would fail to address real-world concerns about correlating between geologically and biologically meaningful regions.

Prothero goes on to emphasize his disagreement with assigning the Kishenehn faunas to the “West.” However, these two lists include just five of the 28361 identifications in the database (0.02%).

Prothero then notes that the 10 “worst offenders” (i.e., most diachronous genera) are mostly small and relatively rare. This comes as no surprise because any set of several hundred genera must include some very rare ones, and because I did demonstrate that rarity and diachrony are correlated. More important, these 10 genera in fact are as “important in [tradi-

tional] biochronology” as any other genera. Four of them were listed by Woodburne and Swisher (1995) in their compilation of IFADs (*Domnina*, *Leptodontomys* = “*Eomys*,” *Plionictis*, *Thylacaelurus*), as well as by Woodburne (1987) in his summary of first-appearing taxa. This 4/10 ratio is actually higher than the ratio for all genera found in both geographic regions (89/407 = 22%).

Prothero ties his position to the status quo by claiming that “all available taxa . . . historically have not been important in North American mammalian biochronology.” This may seem to imply that North American workers make a virtue out of discarding useful evidence. A more charitable interpretation is that although workers use all the taxa they can when drawing interregional correlations, the fact that very rare taxa tend to appear in only one region means that rare taxa tend to not figure in their calculations. That explains why it is not true that all taxa, most taxa, or even a biased sample of rare taxa were included in the diachrony analysis. Instead, the 407 genera found in both geographical regions average 32.3 occurrences, whereas the 774 additional, single-region genera average just 14.3 occurrences.

Finally, Prothero claims that I have included uncertain identifications in my database. In fact, I consulted virtually every taxonomic or faunal account published during the 20th century in order to bring the faunal lists up to date. I modified 7748 identifications, and removed an additional 2826 that were rendered ambiguous by later taxonomic decisions. This does not represent a superficial effort. If Prothero’s real point is merely that West Coast identifications are less reliable than others, then it falls on him to support this with more than a mere invocation of “first-hand experience.” As for tentative identifications, these are impossible to remove because of the inconsistent application of terms like “aff.,” “cf.,” “nr.,” and “?” by taxonomists.

I am grateful that Walsh has brought up the issue of quantifying diachrony. He is correct that one could report median diachrony values instead of the mean and thereby minimize the effect even further. I also considered two other equally reasonable and informative measures. First, one could report the standard deviation around a line of perfect diachrony, which would intuitively summarize the pattern in Figure 1. These values are 4.32 m.y. for FAEs (62% greater than the mean) and 3.17 m.y. for LAEs (57% greater). Second, just as the median represents the 50% confidence limit of an interregional temporal correlation based on one single genus—the problem my paper was meant to address—the 95% tail represents the 95% confidence limit. These limits are 9.88 m.y. for FAEs (271% greater) and 6.96 m.y. for LAEs (245% greater). The mean is the simplest and most widely used of the four measures, and it biases the results against finding diachrony.

Walsh’s discussion of the Uintan and Prothero’s more general statements both imply that workers routinely focus on common taxa. In the general case, the above-mentioned analysis of the master list of Woodburne (1987) shows this implication to be false. As for the Uintan, Walsh’s list actually does not correspond with that of Woodburne (1987), which omitted *Leptoreodon* and *Protylopus*—the two least diachronous genera in Walsh’s list. Furthermore, Walsh omits *Triplopus*, whose FAE diachrony is large (2.71 m.y.).

Walsh’s statistics regarding the fraction of genera that have low diachrony values bolster my main argument: If one uses, say, three genera instead of just one to guide a correlation, one is likely to include at least one genus with an observed diachrony <0.5 m.y. The more genera employed, the less likely that interregional diachrony will be an important factor.

Walsh’s attempt to quantify the influence of sampling effects on diachrony is interesting, but not very fruitful. It is true that his proposed measure does not depend on already having a set of correlations. However, using Walsh’s statistic actually weakens the correlation between sampling and diachrony: Spearman’s  $r = -0.167$  (vs.  $-0.322$ ) for FAEs, and  $-0.252$  (vs.  $-0.377$ ) for LAEs. The reason is that any count of occurrences is a joint function of a taxon’s sampling probability and the length of its age range, and the two factors essentially cancel out. It would be unwise to discard or downweight appearances using a measure like Walsh’s that accounts for

<3% of the variation in FAE diachrony; the measure I originally proposed is far more informative, but cannot be computed a priori.

Walsh asks whether the diachrony values seen in this data set should pertain to all parts of the mammalian fossil record. I do not believe this, nor did I imply it. That said, I see no reason why mammalian LAEs should ever be found to be more, instead of less, diachronous than FAEs. As for “paleobiological significance,” my purpose was to test assertions regarding biochronological utility. If we define biochronology as being of paleobiological interest, then the results certainly are significant; if not, then paleobiology per se was not the point of my analysis. Given the methods at hand, it makes no difference to biochronology whether observed diachrony is due to the biological properties of extinct organisms, or to the inescapable shortcomings of the fossil record.

Walsh’s comments concerning the definitions of biochrons (and specifically NALMAs) are interesting, but not germane to the subject of my paper, which concerned correlations that are independent of biochronological nomenclature. I did not discuss formal definitions of biochrons, and in fact, the IFAD system that I attempted to quantify has not yet been formally implemented in this role (see Woodburne, 1987; Woodburne and Swisher, 1995). Likewise, I nowhere mentioned index taxa, for the simple reason that listing index taxa requires assuming the contents and identities of the biochrons already are known. Therefore, index taxa are a posteriori descriptions of biochrons, and should not be treated as a priori tools for correlation. Walsh’s (and Prothero’s) implication that index taxa are superior “time indicators” is irrelevant in the context of my paper, which assumed that workers are willing to put their a priori assumptions aside when making correlations.

Finally, I agree with Walsh that time indicators like FAEs and LAEs only indicate maximum or minimum ages, and I nowhere implied otherwise. My purpose was to show that in practice, regional FAEs may differ by an appreciable amount, an empirical point that is independent of the logical requirement for at least some miniscule difference to exist.

Despite the fact that none of Prothero’s and Walsh’s arguments truly challenges my basic conclusions, I do not agree with Prothero’s statement that if these results are valid, then the high degree of diachrony shown by individual genera raises a “serious concern about using fossil mammals as time indicators.” To the contrary, their remarkable utility has been reaffirmed by the robust performance of quantitative biochronological analyses (e.g., Alroy, 1996; Azanza et al., 1997). The problem is not that mammals are diachronous, but that certain workers advocate drawing correlations based on only a small number of taxa. This is exactly the general approach that both Prothero and Walsh wish to defend.

Ironically, neither Prothero nor Walsh specifically argue in favor of the IFAD method that my analysis originally targeted. Because evaluating this method is in fact important, I will add one last, and quite crucial, piece of evidence concerning it. Specifically, I used the IFAD compilation of Woodburne and Swisher (1995) to place faunal lists directly into the series of 41 biochrons given by those authors. Each list was given a biochron number based on the identity of the supposedly earliest-appearing of all immigrants the list includes. For example, a list including *Hyopsodus* and *Hyrachyus* would fall into the tenth biochron (starting from the oldest) because *Hyopsodus* first appears then, whereas *Hyrachyus* first appears in the twelfth biochron.

For the 101 lists in the database that include immigrants and are tied to geochronological age estimates, the age estimates and biochron number assignments do show a strong rank-order correlation of +0.9766. But these same age estimates and the concurrent range zones generated by appearance event ordination have a rank-order correlation of +0.9934 (Alroy, 1998). In other words, the ordination results have a maximal correlational error of about 1.3%, whereas the IFAD scheme yields an error of 4.6%—about 3.5 times as much. These results are no quirk: The 45-biochron first appearance list favored by Prothero (Woodburne, 1987) yields a rank-order correlation of +0.9768.

In this case at least, independent data show that “extensive first-hand experience” yields correlations that are far cruder than what may be obtained algorithmically. Based on this and the fact that individual, genus-level mammalian appearance events are now known to be highly diachronous, excluding most taxa on a priori grounds is not a guarantee of success, but of failure. Instead, biochronological analyses should include as many faunal and stratigraphic data as possible and should infer correlations from those data using objective criteria. I hope this exchange of views will encourage workers to reaffirm these traditional goals and move on to more profitable debates.

I thank G. Eble and P. Wilf for comments.

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## Did the Indo-Asian collision alone create the Tibetan plateau?: Comment

### COMMENT \*

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Mainly on the basis of a traverse through the central Lasha block, southern Tibet, China, Murphy et al. (1997) suggested that the southern Tibetan plateau had attained an elevation of 3–4 km by ca. 99 Ma and maintained significant topography until the onset of the Indo-Asian collision, and that this elevation was due to the collision between the Lasha and Qiangtang blocks during Early Cretaceous time.

The best way to test their conclusions perhaps is to examine the paleogeography of southern Tibet from Late Cretaceous to early Tertiary, because if southern Tibet had indeed elevated 3–4 km since 99 Ma, the sediments there should have been predominantly continental. However, unfortunately, voluminous geologic investigations in this region by our Chinese colleagues do not support their conclusions on the tectonic evolution of southern Tibet (e.g., Wan, 1987; Pan et al., 1990; Guo et al., 1991; Liu et al., 1992; Tang et al., 1992). These investigations show that over much of southern Tibet, in particular, the western border area between the Lasha and Qiangtang blocks, shallow marine sedimentation had not terminated until Eocene time (Pan et al., 1990; Liu et al., 1992) (Fig. 1). This sedimentation is marked by reef limestone, radiolarian chert, etc., and its timing is well constrained by rich fossils and radiometric dating on the interbedded volcanics. In Figure 1, we indicate several main locations where Late Cretaceous to Tertiary marine deposits and fossils have been discovered on both sides of Bangong Co–Nujiang suture.

#### Locations for Late Cretaceous Marine Deposits, Index Sediments, and Key Fossils (Numbered as in Figure 1)

1. Aqike, Tarim, E81°05′, N35°40′, bioclastic limestone, reef limestone. *Biradiolites boldjuanensis*, *Braarudosphaera bigelowii* (bivalve, Late Cretaceous) (Tang et al., 1992).

2. Ouli, Rutog, E80°20′, N33°45′, reef limestone, *Trigonioides (Diverstr.) bangongwensis* Gu, *Trigonioides (Diverstr.) xizangensis* Gu (bivalve, Late Cretaceous), interbedded with andesites dated at 77.8 Ma (K-Ar) (Guo et al., 1991), *Bournonia* sp. (bivalve, Late Cretaceous).

3. Geji, E81°05′, N32°01′, bioclastic limestone, *Nerinea parahicoriensis* (bivalve, Late Cretaceous) (Guo et al., 1991), *Orbitolina concava* (foraminifera, Late Cretaceous) (XBGMR, 1993).

4. Kasi, Coqin, E87°05′, N32°01′, bioclastic limestone, *Plicatula placunen*, *Plicatula cf. inflata* (bivalve, Late Cretaceous) (XGS, 1986;

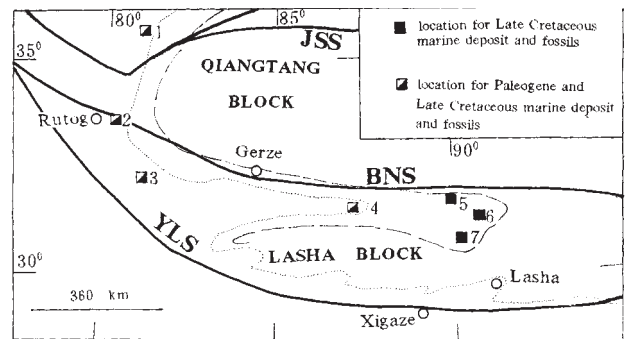


Figure 1. Sketch map of western Tibet showing distribution of Late Cretaceous to early Tertiary marine deposits (revised after Pan et al., 1990; Liu et al., 1992). BNS—Bangong Co–Nujiang suture, JSS—Jinsajiang suture, YLS—Yaluzanbu suture. Dotted line indicates eastern limit of early Tertiary marine deposit, dashed line, Late Cretaceous.

XBGMR, 1993), *Orbitolina concava* (foraminifera, Late Cretaceous) (XGS, 1986).

5. Jiangmuqu, Bange, E90°00′, N31°55′, bioclastic limestone, *Bournonia* sp., *Neithea sexcostatus*, *Plicatula placunen*, *Plicatula cf. inflata* (bivalve, Late Cretaceous) (XGS, 1986; XBGMR, 1993), *Orbitolina concava* (foraminifera, Late Cretaceous) (Han et al., 1983; Wang, 1983).

6. Xizashan, Bange, E90°20′, N31°15′, bioclastic limestone, *Orbitolina concava* (foraminifera, Late Cretaceous) (Wang, 1983; XBGMR, 1993), *Cymopolia* sp. (calcareous alga, Late Cretaceous–Paleogene), *Natica* (gastropod, Late Cretaceous–Quaternary) (Liang and Xia, 1983), interbedded with andesites dated at 77 Ma, 83 Ma (K-Ar) (XBGMR, 1993).

7. Lagouco, Bange, E90°05′, N30°50′, reef limestone, *Bournonia* sp., *Neithea sexcostatus*, *Plicatula placunen*, *Plicatula cf. inflata* (bivalve, Late Cretaceous) (XGS, 1986; XBGMR, 1993), *Orbitolina concava* (foraminifera, Late Cretaceous) (Wang, 1983).

#### Locations for Early Tertiary Marine Deposits, Index Sediments, and Key Fossils (Numbered as in Figure 1)

1. Aqike, Tarim, E81°05′, N35°40′, bioclastic limestone, reef limestone. *Cibicidina* sp. (foraminifera, Paleogene–Neogene), *Flemingostrea* (bivalve, Paleogene), *Miliola* sp. (foraminifera, Paleocene–Eocene), *Sokolovia* (bivalve, Paleogene) (Tang et al., 1992).

2. Ouli, Rutog, E80°20′, N33°45′, reef limestone, *Astrocoenia gibbosa* (coral, Eocene), *Dendrophyllia* (coral, Eocene), *Deplhelia papillosa* (coral,

\*There is currently no Reply to this Comment.

Eocene), *Oculina alabamensis* (coral, Eocene), *Stephanocoenia microtuberculata* (coral, Eocene), *Stylocoenia* (coral, Eocene) (Guo et al., 1991).

3. Geji, E81°05', N32°01', radiolarian chert, reef limestone, *Collonia* (gastropod, Paleocene-Pliocene) (Guo et al., 1991)

4. Kasi, Coqin, E87°05', N32°01', bioclastic limestone, *Nummulites rotularius*, *Deshayes* (foraminifera, Late Cretaceous-Paleogene) (Pan et al., 1990).

The marine sedimentation seems to have spread to most of western Tibet, including the Tarim basin (Tang et al., 1992) and both sides of the Yaluzhanbu suture (Wan, 1987). Therefore, we believe that southern Tibet, in particular, its western segment, could have been slightly elevated, but not up to 3–4 km, and that Tibet was intensively elevated only until the Late Tertiary. Our recent investigations in southern Tibet, which have revealed that Late Cretaceous to early Tertiary pyroclastic rocks have been involved in deformation (folding and thrusting), support this preliminary conclusion.

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