Upper Cretaceous (Maastrichtian) paleosols in Trans-Pecos Texas: Discussion and reply

Discussion

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In a recent paper, Lehman (1989) has described a Cretaceous stratigraphic sequence in Texas containing numerous inferred paleosols. We wish to point out some difficulties in accepting these units solely as products of soil formation.

Lehman (1989) has largely excluded the possibility that the flood-plain strata, subsequent to burial, may have been affected by intrastratal diagenetic alteration over many tens of millions of years, which, coupled with initial depositional heterogeneities, has produced the presently observed chemical and lithological variations. The products of diagenesis and of pedogenesis are quite similar and are very difficult to differentiate by normally utilized field and laboratory methods. Below, we discuss some properties of Lehman's paleosols that are enigmatic.

1. Lehman (1989) has attributed the color variation (see p. 191 and 192) in the stratigraphic sequence entirely to pedogenesis. The red and purple colors he described, however, are uncommon in soils and may reflect a diagenetic overprint.

The presence of purple and gray rip-up clasts in channel lag deposits is not unequivocal proof, as concluded by Lehman, that the colors in the clasts could have been acquired only through pedogenesis prior to their erosion and deposition. In three other studies of strata containing red and green intraclasts, divergent conclusions about the origin of clast coloration, some involving diagenesis, were formulated (McBride,1974; Braunagel and Stanley, 1977; Patterson, 1981).

2. Lehman's Figure 3 depicts relatively high clay contents in the indicated B horizons, and his Figures 7 and 10 illustrate his interpretation of how this argillic zonation formed pedogenically through the progressive clay buildup in an originally uniform overbank deposit. Before down-profile clay increases can be ascribed to pedogenesis, however, it must be demonstrated that the parent material was lithologically uniform; accepted methods include analysis of resistant-mineral suites and diagnostic element ratios (Birkeland, 1984, p. 182; Muhs and others, 1987). Methods such as these were not applied by Lehman.

Moreover, flood-plain alluvium is characteristically nonuniform, varying markedly in both grain size and composition, as a result of variable energies at the time of deposition. Modern flood-plain sediments generally decrease in both grain size and bed thickness away from the main channel (Reineck and Singh, 1980). McKee (1966) noted that the individual flood deposits commonly consist of a lower sand or silt bed overlain by a clay-rich layer. Thus, lateral and vertical variation in clay content in a stack of flood-plain deposits would be a natural consequence of original deposition. Disruption of the sedimentary bedding by plant and animal activity would generally occur predominantly in the upper horizons. The combination of initial lithological heterogeneity and bioturbation would

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result in stratigraphic sequences that closely mimic presumed pedogenic argillic horizons overlying unaltered alluvia.

Initial variation in lithology could substantially influence later diagenetic pathways (Walker and others, 1981; Larson and others, 1982). In the vari-colored flood-plain sediments of the Eocene Wasatch and Wind River Formations, Wyoming, which are closely similar to many reported paleosol sequences, Patterson and others (1988) showed that (a) the light green-colored beds are coarser grained than are the red, hematite-stained beds; (b) the coarse and fine sediments of both formations became reddened through intrastratal diagenesis; and (c) later preferential leaching of the hematite, with progressive color bleaching, primarily affected only the coarser-grained, more-permeable layers, resulting in their light green coloration. This same diagenetic sequence could result in vari-colored rip-up clasts (Patterson, 1981).

3. Lehman has documented clay films in the sediments and attributed their formation to pedogenesis. Clay films, however, are not unique to soils. Walker and others (1978) have shown that pellicular clay coatings commonly form through infiltration well below the soil-forming zone, prior to major soil formation.

4. Lehman has concluded that chemical data (his Table 1) are indicative of pedogenesis; however, this conclusion is predicated on the assumption that the parent material was uniform. In flood-plain deposits, chemical uniformity is not to be expected (see point 2).

There are some specific points about the chemical data of Lehman that need clarification. The first concerns the reporting of chemical analyses (his Table 1). Inasmuch as there is only a partial listing of analyses, it is impossible to determine if they are internally consistent. Moreover, the contents of several species (total Fe, Mn, Al, and Ca) are given as element weight percents but actually seem to be oxide weight percents. Second, Lehman has stated that much of the ferric iron in the red and purple beds occurs in amorphous clots and small crystalline aggregates of hematite in amounts from 3.01 to 7.69 wt %. These contents seem high, inasmuch as only 1 wt % of extractable Fe_2O_3 (pigment) is adequate to produce colors redder than those reported by Lehman (Walker and Honea, 1969). Third, Lehman has reported very high values of Ca (as much as 39.8 wt %) in the strata. If this represents Ca in secondary $CaCO_3$, it must be removed prior to interpretation of Fe and Al trends (note the extremely low value for Al in the sample with 39.8 wt % Ca). Finally, several samples have total organic carbon in excess of 2 wt %, and one of his B2 horizons possesses 8.53 wt %, a value above that reasonably attributed to pedogenesis. It seems unusual that organic contents attributed to soil formation are so consistently high in strata that have been buried for 60 m.y. Most upper Quaternary buried soils lose much of their original carbon soon after burial (Holliday, 1988).

5. Lehman, to explain the common occurrence of carbonate in the A2 horizons, has concluded that each inferred paleosol is composite, having begun development during a humid climate and having ended formation under arid conditions. Given the proposed cyclicity of the climate and repeated soil development during intermittent flood-plain sedimentation, it would be expected that in addition to the above composite soil, the sequence should also contain single humid-climate soils, single arid-climate soils, and composite arid-to-humid soils. The absence of all but the composite humid-to-arid soil type casts doubt on his hypothesized relationship between deposition, climate, and soils.

CONCLUDING REMARKS

It is probable that during the deposition of continental sediments, significant intervals of soil formation may leave discernible records. Undoubtedly, during times of extended exposure, burrowing and growth of vegetation may have occurred. It does not follow, however, that bioturbation and root traces are proof that a well-differentiated soil formed. In many cases, the evidence (both field and laboratory) cited for recognition of paleosols has not been definitive, in part because of possible modification of the strata after burial. Future research should focus on ways of looking through the veil of diagenesis to detect those chemical and physical attributes that could be related to pedogenesis.

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Reply

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The features I have described and interpreted as paleosols are not unique but are found in similar fashion throughout the Upper Cretaceous and Tertiary strata of the Rocky Mountains. Patterson and others apparently do not dispute the fact that the deposits I have described represent subaerially exposed fluvial flood-plain deposits and that the carbonate horizons I have described are of pedogenic origin. The association of petrified logs, *in situ* stumps, root casts, and dinosaur bones with these horizons clearly indicates that the flood plains experienced at least some degree of soil formation. It is with the interpretation of the color banding as *well-differentiated* soil horizons that Patterson and others diagree. The deposits in question consist of alternating bands of generally coarser light gray mudstone and generally finer purple or red mudstone, within a sequence of predominantly olive or dark gray mudstone. I address the comments of Patterson and others in the same order they were presented.

1. I did not state that the present purple/red coloration of these horizons reflects their original color as soil horizons 70 m.y. ago. Diagenetic dehydration, crystallization, and crystal coarsening of original soil oxyhydrates have no doubt altered their original color. Patterson and others correctly state that this is probably a "diagenetic overprint." Nevertheless, I do believe that diagenesis has simply altered, and perhaps accentuated, color differences that originally existed between soil horizons. Importantly, and contrary to Patterson and others' statement, the purple/red coloration is not solely a function of grain size. I stated (p. 191) that purple/red coloration is also found in sandy parent material and, conversely, that gray coloration is found in clayey parent material. Hence, the observed coloration cannot have resulted simply from differential diagenesis of sediments that varied originally in grain size and mineralogy. Alone, the observation that reworked mudstone clasts of both colors are

present in adjacent channels merely suggests that the coloration must have formed very early after deposition.

2. Patterson and others suggest that depositional grain size and mineralogical segregation is responsible for later diagenetic coloration and that each coarse/fine (or gray/purple) couplet is the product of a single episode of overbank flooding. Most or all such couplets resulting from overbank flooding on modern rivers, however, are relatively thin (less than 1 m). The maximum thickness of the modern couplets described by McKee (1966) is about 1.2 m, and these were deposited by the Indus River in Pakistan, a river much larger than those represented by the Cretaceous deposits in Texas. Yet, the coarse/fine (gray/purple) couplets observed in the Texas deposits average 3 m in thickness (p. 193). It is unlikely that such thick couplets were formed by the mechanism suggested by Patterson and others. In any case, this is a moot point because, as stated above, the purple/red coloration is not grain size–dependent.

Moreover, their model does not explain why great thicknesses of overbank strata (as much as 20–30 m) are completely unaffected by the color banding (see Fig. 1B and p. 202). If the color banding is of diagenetic origin, why did this not affect the entire sequence of flood-plain alluvium, all of similar origin and of comparable texture and mineralogy? X-ray diffractometry (p. 192) indicates that the mineralogy of all units is similar.

Also, if the coloration is solely of diagenetic origin, why does the coloration change so dramatically over short stratigraphic intervals? For example, the color banding observed in Paleocene flood-plain alluvium is strikingly different from that observed in Cretaceous alluvium (Lehman, 1988). Eocene flood-plain alluvium is likewise different in coloration. Even more compelling is the observation that correlative Upper Cretaceous, Paleocene, and Eocene strata of northwestern New Mexico, 1,000 km removed from west Texas, exhibit nearly identical color banding, likewise differing between the series (for example, Lehman, 1985). This is impossible to explain in terms of diagenesis but is readily interpreted in terms of similar regional climates and paleosols.

Patterson and others object to the lack of diagnostic element ratio analysis. Although it is inappropriate to include new information in this Reply, trace-element data are available for one of the paleosols I described. Concentration ratios of relatively immobile elements (Ti, Th, Zr, Y) are used to test the origin and uniformity of soil parent material. These new data (to be published elsewhere) are compatible with the origin of these soils from uniform parent material and are comparable to those described by Muhs and others (1987), for example.

3. I agree that the presence of clay films alone does not make a soil but, in concert with the other data given, adds weight to their interpretation as soils. For example, Birkeland (1984, p. 30–32) gave the following criteria for the recognition of paleosol B horizons: (a) greater clay content, (b) redder color, (c) pedogenic structure, (d) presence of a carbonate horizon, (e) sharp upper and gradational lower boundaries, and (f) darker and redder at top with chroma diminishing with depth. The purple/red beds I have described meet all of these criteria.

4. Patterson and others correctly state that the chemical data I gave are a partial listing and are given in weight percent as oxides. I did not state, however, that hematite occurs in amounts from 3 to 7 wt %, but instead that the *total iron content* of red/purple beds varies from 3 to 7 wt % (as Fe_2O_3). This presumably reflects some hematite, some detrital Fe-bearing minerals, and some iron in the clay minerals. Because I was interested in the distribution of calcium, I did not remove it; however, carbonate nodules were separated from the predominantly mudstone matrix prior to analysis. The single anomalously high calcium value noted by Patterson and others likely reflects impregnation of the mudstone matrix itself with finely disseminated calcium carbonate. This atypical data point is unimportant because, regardless of the carbonate content or position of the carbonate in the sequence, the relationships between Fe and Al contents in gray and purple beds remain unchanged. Patterson and others do not dispute the fact that Fe and Al content varies regularly between gray and purple beds. Even their diagenetic model would allow for this.

Patterson and others incorrectly state that I attributed the total organic carbon content of these beds solely to pedogenesis. In fact, I clearly stated (p. 193) that some, perhaps most, of the organic carbon is allochthonous, having been deposited as organic material with the original parent alluvium. It may seem unusual to Patterson and others that the organic carbon content of these strata is "high"; however, they have again singled out one high value from the data. All of the other samples contain 2 wt % organic carbon or much less. Regardless, these are the data, and they offer no alternative explanation.

5. Patterson and others mistakenly state that all the paleosols I described are composite humid-to-arid soils. In fact, I stated (p. 196) that *most* of the carbonate horizons are found in olive beds (presumed C horizons) and purple beds (presumed B horizons), where they would be expected in a "single climate" soil. Some carbonate horizons are unassociated with purple beds (single arid-climate soils), and some paleosols with pronounced color banding are devoid of carbonate accumulation (single humid-climate soils). This is clearly stated on page 201 and shown in Figure 1B (see particularly section 2). Therefore, this sequence of paleosols actually exhibits the very criteria they predict should be present, and which the alternating-climate hypothesis explains.

The Alternative Interpretation presented by Patterson and others, although possibly applicable to other sequences, fails to explain the features observed in the section I described. I remain convinced that these features are indeed pedogenic in origin. The debate should focus on what kind of soils these represent, and what their environmental implications may be.

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