were not uniquely decisive: "Such a large number of surprising simplifications and interrelationships become visible after only a preliminary scanning of the chief geological and geophysical results, that for that reason alone I consider it justified, even necessary, to replace the old hypothesis . . . by the new one" of continental drift (Wegener, 1912a, p. 185). This fascination guided him until the end; I repeat: "only by combining all the earth sciences may we hope to find the 'truth', i.e. the *picture* that describes *all* the *known* facts in their best order" (Wegener, 1929, Preface; not my definition, but my italics). I believe that a negative geodetic result (no large displacement found) would not have made Wegener give up the concept of continental drift but would have merely caused him to revise his estimates of drift rates.

Lehmann and Haller argue that the "crucial criterion would be to establish what is, indeed, a fact and where begins . . . speculation."

Speculation and (false) "fact" are not the same. There are facts and "facts," but more precisely, there are fact, "fact," and speculation [and they all can advance or impede science; this has been beautifully described for the history of astronomy by Koestler (1959); Jeffreys (for example, 1959, p. 364-371) has provided an example of how failure to recognize a model as a model can impede the advance of science when he refuted the incorrect model of sailing continents and, with it, continental drift].

Although I said that Wegener distinguished between fact and speculation, I stress that the borderline between them is difficult to define. This is so because facts have no meaning by themselves. Anything we recognize as fact we recognize because we include it in our "picture," harmoniously or disharmoniously. There are innumerable facts we do not include because we do not see them, or we do not see them because they are not included in our "picture."

The geodetic data alone would not have shaken the fixist view at the time, but that view was no longer in harmony with the many facts that had by then been recognized. These facts prepared the ground for someone like Wegener to present a more harmonious picture of the world.

COMBINED REFERENCES CITED

Haller, J., 1969, Tectonics and neotectonics in East Greenland; review bearing on the drift concept, *in* North Atlantic; geology and continental drift: American Association of Petroleum Geologists Memoir 12, p. 852-858.

Jacoby, W. R., 1981, Modern concepts of Earth dynamics anticipated by Alfred Wegener in 1912: Geology, v. 9, p. 25-27.

Jeffreys, H., 1959, The Earth: Cambridge, Cambridge University Press.

Jensen, P. F., 1923, Ekspeditionen til Vestgrønland, Sommeren 1922: Meddelelser om Grønland, v. 63, p. 209-283.

Koch, I. P., 1917, Survey of northeast Greenland: Meddelelser om Grønland, v. 46, p. 79-468.

Koch, I. P., and Wegener, A., 1917, Die glaciologischen Beobachtungen der Danmark-Expedition: Meddelelser om Grønland, v. 46, p. 1-77.

Koestler, A., 1959, The sleepwalkers: New York, McMillan.

Koldewey, K., 1874, Astronomische Ortsbestimmungen während der Schlittenreise nach Norden, *in* Verein Deutsche Nordpolarfahrt, Bremen: Die Zweite Deutsche Nordpolarfahrt in den Jahren 1869 und 1870 unter Führung des Kapitän Karl Koldewey: Wissenschaftliche Ergebnisse, v. 2 (2), V, Astronomie, p. 754-760.

Sinding, E., 1955, Astronomical determinations in Greenland 1927-1952: [Denmark] Geodaetisk Institute Skrifter, v. 19, p. 1-107.

Taylor, F. B., 1910, Bearing of the Tertiary mountain belt on the origin of the Earth's plan: Geological Society of America Bulletin, v. 21, p. 179-226.

Wegener, A., 1912a, Die Entstehung der Kontinente: Petermanns Geographische Mitteilungen, v. 58, p. 185-195, 253-257, 305-308.

——1912b, Die Entstehung der Kontinente: Geologische Rundschau, v. 3, p. 276-292.

——1915, 1920, 1922, 1929, Die Entstehung der Kontinente und Ozeane [first through fourth editions]: Braunschweig, Germany, Vieweg.

Wegmann, C. E., 1948, Geological tests of the hypothesis of continental drift in the Arctic regions: Meddelelser om Grønland, v. 144, p. 1-48.

Comment and Reply on 'Plate tectonics of the Ancestral Rocky Mountains'

COMMENT

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Kluth and Coney (1981) presented a long-needed synthesis of the timing of basin infilling in the Ancestral Rockies and compared this to the timing of deformation in the Ouachita-Marathon fold and thrust belt. They concluded that (1) basement deformation in the foreland is synchronous with thin-skinned deformation in the orogen; (2) motions on uplift-bounding faults are predominantly vertical; (3) the Ancestral Rockies were a result of continental collision acting on a weak peninsula of the craton; and (4) a good tectonic analogy to the Ancestral Rockies is the intraplate deformation of Asia produced as a result of its collision with India. Although I agree with their first two conclusions, there are several observations that negate the last two.

Kluth and Coney (1981) suggested that the Texas Recess (usage of Thomas, 1977) was a weak peninsula of the craton and was deformed during a continental collision, thus producing the Ancestral Rockies. Two observations argue against this process. First, basement foreland deformation is not associated with other Appalachian recesses, notably the Alabama and St. Lawrence recesses. Although the style of foreland deformation observed in the Ancestral Rockies is unique in the Appalachian-Caledonian orogen, the presence of isolated recesses is not. Second, deformation is localized at the apices of the Marathon and Ouachita salients rather than the spearhead of the Texas Recess. The Texas Recess is not viewed as a "dead zone" (Molnar and Tapponier, 1978), because the hypothesized presence of that area of mild deformation (but high stress) is based on analysis of the indentation of a rectangular die into the linear margin of a rigid-plastic substance. Granted, much of the localization of deformation in the Ancestral Rockies is due to reactivation of pre-existing weaknesses, such as the Wichita-Anadarko aulacogen and, possibly, another aulacogen at the site of the Delaware Basin. The Wichita-Anadarko aulacogen may well have, in the Appalachians (Rankin, 1976), counterparts that were not reactivated during collision. Thus, the contention that the Texas Recess served to focus collisional deformation in the basement of the foreland cannot account for the uniqueness of the Ancestral Rockies.

There are also major uncertainties regarding the geometry of late Paleozoic collision along the southern margin of North America. Kluth and Coney (1981) argued that the Ancestral Rockies were a response to collision, but they did not show that a collision ever occurred. Continental reconstructions have never fit well in the Gulf of Mexico region. Keller and Cebull (1973) argued that the Ouachitas were a result of noncollisional deformation, and other authors have presented tectonic models (Hatcher, 1974) or continental reconstructions (Morel and Irving, 1980) that do not require collision in that region. Although the present Gulf of Mexico is largely a Mesozoic feature, the pre-Mesozoic history of the area is a major unresolved problem. The presence of the Ancestral Rockies cannot be considered a priori evidence for a continental collision.

Kluth and Coney (1981) also proposed that the deformation of Tibet and southern Asia is analogous "in some respects" to Ancestral Rockies deformation. There are, however, almost no similarities between the two areas. The Himalayan foreland is dominated by strike-slip and normal faulting, with attendant volcanism (Molnar and Tapponier, 1978). In contrast, the Ancestral Rockies, as correctly noted by Kluth and Coney (1981), were dominated by vertical displacements, largely on high-angle reverse faults (Ham and others, 1964; Elam, 1967) and are amagmatic. There is a more profound reason for rejecting the analogy proposed by Kluth and Coney (1981). As a result of collision, the crust of Tibet and parts of southern Asia have been greatly thickened, and the resulting isostatic rise accounts for the great elevation of the area. Similar processes did not occur in the Ouachita-Marathon foreland; many basins in that area contain Pennsylvanian and Permian marine deposits. During and after foreland deformation, the region as a whole continued to subside, with the result that basement uplifts were completely buried by Permian marine and terrestrial deposits. This is not the response of tectonically thickened crust.

A much better analogy is made with the middle and southern Rocky Mountains (some of which are reactivated Ancestral Rockies elements). The following characteristics are shared by both provinces: isolated, elongate basement uplifts are separated by asymmetric sedimentary basins and have multimodal orientations; many uplifts are bounded by high-angle reverse faults; absolute motions of adjacent crustal blocks were both up and down relative to sea level; both provinces were bounded on the orogenic side by an active fold and thrust belt; and both provinces lack extensive contemporaneous magmatism (although the southern Rockies display local Laramide magmatism). The similarity between these two provinces is quite striking, but it is not necessarily evidence for identical plate-tectonics settings. It is equally possible that the basement foreland deformation resulting in the Ancestral Rocky Mountains (and, possibly more significantly, the intermontane basins) was a result of either collisional or noncollisional deformation. The factors controlling this deformation must be deduced before an accurate plate-tectonics interpretation can be accepted.

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REPLY

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Goldstein has apparently misinterpreted our comments (Klu and Coney, 1981, p. 13) about the role of an irregular souther margin of North America during the late Paleozoic orogenic event. We do not envision the salient that includes the Llano region (Texas Recess of Thomas, 1977), as the weak peninsula a craton that exerted primary control on the development of th Ancestral Rocky Mountains. That salient is part of the irregula margin of a much larger, southwest-trending peninsula that extended in width from the Ouachita-Marathon orogen to the Cordilleran miogeocline in Utah and Nevada. This southwestwa projection of North American craton includes the Transcontinental Arch. Presumably, this peninsula narrowed southwestward in Paleozoic time, but the geometry of its terminus in Mexico is speculative (Dickinson and Coney, 1980).

Goldstein re-emphasizes our point (Kluth and Coney, 1981 p. 13) of the uniqueness of the intracratonic deformation that included the Ancestral Rocky Mountains during the Appalachia Caledonian orogeny. He notes that irregular margins were involved in the collision elsewhere, but without resulting in wid spread foreland of the type seen in the Ancestral Rocky Mountains. We contend that the difference in the Ancestral Rocky Mountain region from other intracratonic areas along the oroge is its position on the large, southwest-trending peninsula of the craton. This peninsula was relatively narrow compared to the cratonic mass elsewhere along the orogen. Properly oriented, pre existing weaknesses were reactivated in the peninsula during the Appalachian-Caledonian orogeny and resulted in large-scale deformation far inboard of the margin. During Middle Pennsylvanian time, the peninsula was deforming across its entire widt from the Ouachita-Marathon margin on the southeast to the Cordilleran margin on the west, as features within the Antler belt were rejuvenated.

We do not interpret the Llano region as a "dead zone" bu rather as a buttress-like salient of the craton that exerted a control of strain patterns during the orogenic event along the margin. The controlling effect of an irregular plate margin on strain patterns during an orogenic event has been suggested by Tapponier and Molnar (1976), and Dewey and Burke (1974). We envision the effect of the Texas Recess to have varied as the orogenic event along the margin progressed southwestward through time, thus changing the directions of stresses applied to that salient of the craton.

We agree with Goldstein that the presence of the Ancestral Rocky Mountains is not a priori evidence for a continental collision, and we noted (Kluth and Coney, 1981, p. 12) one of the other interpretations of late Paleozoic plate geometries. The collision between South America–Africa and North America tha produced the Ouachita-Marathon orogenic zone was suggested and has been supported by others (Dewey and Bird, 1970; Graham and others, 1975; Ross, 1979). We concluded that the present evidence favors the collision model, and we proceeded with that model as an assumption (Kluth and Coney, 1981, p. 12).

We never intended to show that a collision occurred or to deal with details of the collision, because our work really did not address that problem. One of our principal points was that whatever caused the Ouachita-Marathon orogenic belt, the Ancestral Rocky Mountains appear to have resulted from that same event.

Finally, Goldstein objects to our analogy between the late Paleozoic deformation of the western United States and that of Asia during Cenozoic time. We noted that the analogy was not rigorous, and we pointed out (Kluth and Coney, 1981, p. 13–14) some of the differences that were emphasized by Goldstein. The point that we were attempting to make was that, in both cases, large areas of the craton, extending thousands of kilometres from the plate boundary, were being deformed during a collision along the margin. The perception of the cratonic interiors as rigid, undeformable masses during continental collisions may not be correct in certain cases. Goldstein's suggested analogy between the Paleozoic deformation and the Laramide deformation of the western United States is one that we considered in developing our model. We were trying to emphasize the large-scale response of both Asia and the late Paleozoic of the United States to continental collisions. Evidence does not appear to support a continent-continent collision as the cause of Laramide deformation in the western United States (Coney, 1972, 1973, 1978; Burchfiel and Davis, 1972, 1975). We assumed that collision was responsible for the late Paleozoic deformation; thus, we chose not to emphasize the analogy with the Laramide deformation. In this respect, Goldstein's suggested analogy may not be as helpful, despite the similarities that he notes.

COMBINED REFERENCES CITED

Burchfiel, B. C., and Davis, G. A., 1972, Structural framework and evolution of the southern part of the Cordilleran orogen, western United States: American Journal of Science, v. 272, p. 97–118.

——1975, Nature and controls of Cordilleran orogenesis, western United States: Extension of an earlier synthesis: American Journal of Science, v. 275-A, p. 363–396.

Coney, P. J., 1972, Cordilleran tectonics and North American plate motion: American Journal of Science, v. 272, p. 603–628.

——1973, Non-collision tectonogenesis in western North America, *in* Tarling, D., and Runcorn, S., eds., Implications of continental drift to the earth sciences: New York, Academic Press, p. 713–727.

——1978, Mesozoic-Cenozoic Cordilleran plate tectonics, *in* Smith, R. B., and Eaton, G., Cenozoic tectonics and regional geophysics of the western Cordillera: Geological Society of America Memoir 152, p. 33–49.

Dewey, J. F., and Bird, J. M., 1970, Mountain belts and the new global tectonics: Journal of Geophysical Research, v. 75, p. 2625–2647.

Dewey, J. F., and Burke, K., 1974, Hot spots and continental breakup: Implications for collisional orogeny: Geology, v. 2, p. 57–60.

Dickinson, W. R., and Coney, P. J., 1980, Plate tectonic constraints on the origin of the Gulf of Mexico, *in* The origin of the Gulf of Mexico and the early opening of the North Atlantic Ocean (Proceedings of a Symposium): Baton Rouge, Louisiana State University, p. 27–36.

Elam, J. G., 1967, The tectonic style in the Permian basin and its relationship to cyclicity, *in* Elam, J. G., and Chuber, S., eds., Cyclic sedimentation in the Permian Basin: Midland, Texas, West Texas Geological Society, p. 55–79.

Graham, S. A., Dickinson, W. R., and Ingersoll, R. V., 1975, Himalayan-Bengal model for flysch dispersal in Appalachian-Ouachita system: Geological Society of America Bulletin, v. 86, p. 273–286.

Ham, W. E., Denison, R. E., and Merritt, C. A., 1964, Basement rocks and structural evolution of southern Oklahoma: Oklahoma Geological Survey Bulletin 95, 165 p.

Hatcher, R. D., 1974, North American Paleozoic foldbelts and deformational histories: A plate tectonics anomaly: American Journal of Science, v. 27, p. 135–147.

Keller, R. G., and Cebull, S. E., 1973, Plate tectonics and the Ouachita system in Texas, Oklahoma, and Arkansas: Geological Society of America Bulletin, v. 84, p. 1659–1666.

Kluth, C. F., and Coney, P. J., 1981, Plate tectonics of the Ancestral Rocky Mountains: Geology, v. 9, p. 10–15.

Molnar, P., and Tapponier, P., 1978, Active tectonics of Tibet: Journal of Geophysical Research, v. 93, p. 5361–5375.

Morel, P., and Irving, E., 1980, Late Paleozoic reconstruction of the continents based on paleomagnetism, *in* The origin of the Gulf of Mexico and the early opening of the North Atlantic Ocean, (Proceedings of a Symposium): Baton Rouge, Louisiana State University, p. 75–78.

Rankin, D. W., 1976, Appalachian salients and recesses: Late Precambrian continental breakup and opening of the Iapetus Ocean: Journal of Geophysical Research, v. 81, p. 5605–5619.

Ross, C., 1979, Late Paleozoic collision of North and South America: Geology, v. 7, p. 41–44.

Tapponier, P., and Molnar, P., 1976, Slip line field theory and large scale continental tectonics: Nature, v. 264, p. 319–324.

Thomas, W. A., 1977, Evolution of Appalachian-Ouachita salients and recesses from recesses and promontories in the continental margins: American Journal of Science, v. 277, p. 1233–1278.

Comment and Reply on 'Episodic growth of Holocene tidal marshes in the northeastern United States: A possible indicator of eustatic sea-level fluctuations'

COMMENT

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Rampino and Sanders (1981a) have suggested that tidal marshes are rare in the Holocene transgressive record of the inner continental shelf off the northeastern United States. From this, they concluded that (1) tidal-marsh growth took place only when local sea-level rise slowed; (2) a decrease of local sea-level rise was caused by a reduction or reversal of eustatic sea-level rise; and (3) changes in salt-marsh growth are cyclic and can be correlated with indicators of climate change such as mountain-glacier advance. We contend that none of these points can be supported by the data presented.

The paucity of tidal-marsh peat in cores from the inner continental shelf indicates to Rampino and Sanders (1981a) the rarity of marshes on this part of the shelf. This rarity may be illusionary, because the cores used to establish this point were taken for purposes other than to sample peat. For example, Field and others (1979) pointed out that their cores, intended to sample sand, were taken at sites unfavorable for peat. Nevertheless, peat was found in about 3% of the cores. The purpose of the cores described by Oldale and O'Hara (1980) was to deterthe lack of carbonate sequences and the paucity of the Paleogene record. Elizabeth Truswell (Bureau of Mineral Resources, Canberra) reviewed the terrestrial floras and pointed out the particular problems associated with the existence of abundant and diverse floras at high latitudes.

PROBLEMS AND OPPORTUNITIES

A wide-ranging discussion of the main outstanding problems and opportunities in Antarctic earth science ended the conference. There was a broad consensus, given that only 2% of the bedrock of the continent is exposed, that the great variety of problems associated with the structure of West Antarctica, its relation to East Antarctica, the tectonic significance of the Transantarctic Mountains, and reconstruction could be solved only with modern geophysical study of the ice-covered areas. Undoubtedly, a major international program should be mounted to obtain the necessary data.

The development of the margins of the continent was also seen as an area for fruitful investigation. In this context Dennis Hayes (Lamont-Doherty Geological Observatory) pointed out that the passive margin of East Antarctica provides a unique opportunity to look at a continental margin that developed at different times and therefore could be studied at various stages of evolution.

Finally, two areas were identified in which research in Antarctica could make a substantial contribution to our overall understanding of "how the Earth works," as Ron Oxburgh put it. First, the Ferrar tholeiitic rocks may yield extremely important clues about major mantle processes associated with breakup. There is need to consider the petrogenesis of the Gondwana Mesozoic igneous province as an entity and in relation to contemporaneous igneous rocks along the Pacific margin. Second, the one unique aspect of Antarctic earth science is, of course, the opportunity to study the glacioclimatic history. The Antarctic continent has been a polar or near-polar continent for more than 200 m.y.; therefore, glaciation was not just the result of the shift with respect to the pole, and the search for understanding of this major event in Earth's climatic history can best be pursued in the Antarctic and sub-Antarctic regions. It is to be hoped that the next decade will see a renewal of the deep-sea drilling around the continent that was curtailed after the 1973–1974 season.

The conference has led us to view Antarctic earth science as being in much the same state now as geological knowledge of the ocean basins was 35 years ago. Continental geology provided the background for investigating the ocean floor. Extensive geophysical surveys and dredging led to development of hypotheses subsequently tested by the highly successful Deep Sea Drilling Project. In Antarctica, the 2% of exposed rock provides the background for modern geophysical studies of the continental margins and ice-covered areas. Such surveys will surely lead to new hypotheses that will be tested only by further drilling at sea and eventually by drilling through the ice.

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FORUM

Comment and Reply on 'Plate tectonics of the Ancestral Rocky Mountains'

COMMENT

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Kluth and Coney (1981a) made a good case for time correlations of Permian-Pennsylvanian events in the Ouachita-Marathon region and the Ancestral Rockies. It is tempting, but not mandatory, to assume that the events were related to a common cause; nor is there a compulsion to conclude that the cause was a continent-continent collision, the model adopted by Kluth and Coney.

Two major problems are involved in continent-continent collision tectonics. The one concerns criteria by which such collisions may be recognized; the other concerns the spectrum of diastrophic events that might result from such a collision. The former has been treated partly in an earlier discussion by Goldstein (1981), who cited noncollisional models for the Ouachita orogeny. However, a late Paleozoic collision between North and South America is not implausible. In order to simplify debate, we may assume that a collision took place and go on to the latter point.

The mechanical behavior of rock materials in a continental collision may be expected to depend to a large degree upon the velocity at which the continents collide, since strain rate plays a major role in rock deformation (Heard, 1963; Donath and Fruth, 1971). Assuming average plate motion of a few centimetres per year (Solomon and others, 1975) and a zone of deformation 100 km or so wide, the average strain rate would be of the order of 10^{-14}, about one-millionth of that obtainable in experiments. However, a considerable body of data (including Handin and Hager, 1957, 1958; Heard, 1960; Byerlee, 1968; Heard and Raleigh, 1972; Olsson, 1974; Narasimhan et al., 1980) suggests that the major factors controlling rock behavior at geologic strain rates are grain size, confining pressure, temperature, and pore water. Grain size in the continental basement may be taken as uniform, and lithostatic pressure increases rather uniformly with depth. The chief variables are temperature and pore water, which, assuming active subduction, would be greatest at the continental margin.

Available data combine to predict that during a collision, rock materials at the continental margin are weak, causing the marginal zone to behave as an effective "bumper" in shielding the craton from significant damage. For a boundary force, the maximum deviatoric stress that could be transmitted across the zone would not exceed that required to deform the weak marginal rocks. The marginal zone should have no difficulty in accommodating a crustal shortening of a few centimetres per year for the duration of a collision.

Assuming that the "bumper effect" may somehow be set aside, a related problem arises in connection with transmitting a boundary stress applied at the edge of a cratonic segment into the deep interior of the continent, as would be required in the case of the Ancestral Rockies. The problem is similar to that of moving a large thrust block by pushing from the rear. Assuming resistance due to basal drag on a sole fault, Hubbert and Rubey (1959, p. 122–127) demonstrated that the maximum length of a block that can be pushed by a boundary force is proportional to the block thickness times a strength factor for the rock. Their values for block length are reduced considerably when the state of stress within the block is considered (Forristall, 1972) and are increased when a decollement zone of substantial thickness is assumed (Kehle, 1970).

The concept of rigid lithospheric plates, initially assumed for mathematical convenience, needs to be modified as suggested by Roper (1974). The constraints on collision tectonics with regard to intraplate deformation apply as well to the model of Tapponier and Molnar (1976) cited by Kluth and Coney (1981a) in support of their hypothesis.

A final point concerns the mechanics and kinematics of Ancestral Rockies deformation. Kluth and Coney (1981a) stated that the major component of movement on faults bounding the uplifted blocks was vertical. However, they concluded that the regional stress system resulted from wrenching by distributive shear when a southwesterly peninsular projection of the craton was pushed northwestward as the collision progressed. The stress system would then have been one of dextral rotation, with the greatest and least principal stresses horizontal, and the direction of major transport would have been subparallel to the axes of the uplifts. One is at some loss to understand how uplift of northwest-trending basement blocks by dominantly vertical movement along marginal faults could have resulted from such a stress system.

The Ancestral Rockies are an example of intracratonic germanotype deformation, the mechanism for which has long been a subject of debate. Fragmentary structural data and the sedimentary record suggest a similarity in tectonic style and mode of origin to the Laramide Rocky Mountains that occupy essentially the same region. A variety of mechanisms have been proposed to account for the Laramide deformation, including vertical uplift and lateral compression. Results of COCORP deep seismic reflection profiling across the Wind River Range, Wyoming (Oliver, 1982, p. 693), indicate that the basement core was upthrust along a crustal shear of moderate dip, consistent with an eastward-directed lateral compression. A similar mechanism for Ancestral Rockies deformation seems preferable to that proposed by Kluth and Coney.

REPLY

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Warner appears to be unconvinced of the relationship between orogenic events in the Ouachita-Marathon region and the development of the Ancestral Rocky Mountains (Kluth and Coney, 1981a). We continue to believe that the deformation of both regions is associated too closely in time and space to be coincidental (Kluth and Coney, 1981a, p. 12). We also continue to believe that the Pennsylvanian tectonic events in the western and southern U.S. are best explained by a continent-continent collision model (Ross, 1979; Pendell and Dewey, 1982).

We are aware of the volume of data on the mechanical behavior of rocks, some of which is cited by Warner (Comment above). A variable that we think is important, but that Warner failed to cite, is the pre-existing anisotropy in the rocks. Donath (1964) has reviewed the effect of pre-existing anisotropies on directions and stress levels required for failure. The pre-existing zones of weakness in the craton with their complex earlier histories may have been so weak as to fail at stress levels different from those predicted by assuming an isotropic, homogeneous crust. If the effect of pre-existing weakness is large, then Warner's conclusion that a "bumper effect" at the plate margin will prevent intraplate deformation may not be valid in all cases. Nevertheless, intraplate deformation closely associated with a continental collision has occurred in the Pennsylvanian in the western United States, in the Tertiary in Europe (Celal Sengör, 1976), and from the late Tertiary to the present in Asia (Molnar and Tapponier, 1975). Deformation far inboard from the plate boundary (in a different plate-tectonic setting) is also a characteristic of Laramide deformation of the western U.S. In this regard, the constraints suggested by Warner apply to his deformational analogy with the Laramide uplifts as well as to ours. The influence of pre-existing zones of crustal weakness may have been the chief reason that the Laramide features in Colorado formed in the vicinity of the Paleozoic features.

The pattern of stress within the Pennsylvanian foreland of the western United States was almost certainly more complex than that suggested by Warner. We envision a complex pattern of stress trajectories which changed orientation, and probably shape, as collision suturing progressed southwestward along the irregular plate margin. We speculate that transpression and transtension across zones of weakness at the margin of the blocks was efficiently translated into vertical motion as those zones achieved a proper orientation relative to the changing stress field. Much more data must be collected on the geometry and timing of the faults before the stress patterns and fault kinematics can be accurately interpreted.

We considered the analogy with the Laramide structures during the course of our work but ultimately rejected it. As we have stated previously (Kluth and Coney, 1981b; and above), we interpret the plate-tectonic setting of the two episodes to have been different. The structural data referred to by Warner on Pennsylvanian-age faulting is indeed fragmentary. Where preserved and exposed, the faults bounding the Pennsylvanian uplifts in Colorado are large normal faults (Baars, 1966), or they have high-angle geometry (De Voto, 1972; Miller et al., 1963). While we feel that our model can accommodate crustal shortening, we felt constrained by these data, which do not resemble interpretations of the Wind River COCORP data. We are not aware of other structural data referred to, but not cited, by Warner that would justify the analogy between the Ancestral Rocky Mountains and Laramide structures on the basis of geometry. We are also uncertain what inferences Warner has made about Pennsylvanian-age fault geometry from the sedimentary record.

In addition, we found no evidence that a major tectonic event of sufficient magnitude to result in extensive foreland deformation took place in Pennsylvanian time along the western margin of the North American plate. Rocks deposited to the west are not characteristic of proximal synorogenic deposits (Brill, 1963; Roberts et al., 1965). The conglomeratic facies of those rocks can better be explained, in our opinion, as shed from older tectonic blocks reactivated in Pennsylvanian time rather than from a major orogenic highlands to the west. Pennsylvanian-age volcanic rocks in western Nevada are allochthonous with respect to North America and were emplaced later (Speed, 1979). Thus, we concluded that no major orogenic event took place along the western plate boundary to serve as a driving mechanism for the Ancestral Rocky Mountains. Because of the differences in setting and geometry, we concluded that the analogy with Laramide tectonics was not as helpful as the one (Asia) that we suggested, even though there are differences between the Cenozoic deformation of Asia and the Pennsylvanian deformation of the western United States.

COMBINED REFERENCES CITED

Baars, D. L., 1966, Pre-Pennsylvanian paleotectonics—Key to basin evolution and petroleum occurrences in the Paradox Basin, Utah and Colorado: American Association of Petroleum Geologists Bulletin, v. 50, p. 2082–2111.

Brill, K. G., Jr., 1963, Permo-Pennsylvanian stratigraphy of western Colorado Plateau and eastern Great Basin regions: Geological Society of America Bulletin, v. 74, p. 307–330.

Byerlee, J. D., 1968, Brittle-ductile transition in rocks: Journal of Geophysical Research, v. 73, p. 4741–4750.

Celal Sengör, A. M., 1976, Collision of irregular continental margins: Implications for foreland deformation of Alpine-type orogens: Geology, v. 4, p. 779–782.

De Voto, R. H., 1972, Pennsylvanian and Permian stratigraphy and tectonism in central Colorado: Colorado School of Mines Quarterly, v. 67, p. 139–185.

Donath, F. A., 1964, Strength variation and deformational behavior in anisotropic rocks, *in* Judd, W., ed., State of stress in the earth's crust: New York, Elsevier, p. 281–298.

Donath, F. A., and Fruth, L. S., Jr., 1971, Dependence of strain-rate effects on deformation mechanism and rock type: Journal of Geology, v. 79, p. 347–371.

Forristall, G. Z., 1972, Stress distribution and overthrust faulting: Geological Society of America Bulletin, v. 83, p. 3073–3082.

Goldstein, A. G., 1981, Comment *on* 'Plate tectonics of the Ancestral Rocky Mountains': Geology, v. 9, p. 387–388.

Handin, J., and Hager, R. V., 1957, Experimental deformation of sedimentary rocks under confining pressure: Tests at room temperature on dry samples: American Association of Petroleum Geologists Bulletin, v. 41, p. 1–50.

——1958, Experimental deformation of sedimentary rocks under confining pressure: Tests at high temperature: American Association of Petroleum Geologists Bulletin, v. 42, p. 2892–2934.

Heard, H. C., 1960, Transition from brittle rupture to ductile flow in Solenhofen limestone as a function of temperature, confining pressure, and interstitial fluid pressure, *in* Griggs, D., and Handin, J., eds., Rock deformation: Geological Society of America Memoir 79, p. 193–226.

——1963, Effect of large changes in strain rate on experimental deformation of Yule marble: Journal of Geology, v. 71, p. 162–195.

Heard, H. C., and Raleigh, C. B., 1972, Steady state flow in marble at 500°C and 800°C: Geological Society of America Bulletin, v. 83, p. 935–956.

Hubbert, M. K., and Rubey, W. W., 1959, Role of fluid pressure on mechanics of overthrust faulting: Geological Society of America Bulletin, v. 70, p. 115–166.

Kehle, R. O., 1970, Analysis of gravity gliding and orogenic translation: Geological Society of America Bulletin, v. 81, p. 1641–1644.

Kluth, C. F., and Coney, P. J., 1981a, Plate tectonics of the Ancestral Rocky Mountains: Geology, v. 9, p. 10–15.

——1981b, Reply *to* Comment *on* 'Plate tectonics of the Ancestral Rocky Mountains': Geology, v. 9, p. 388–389.

Miller, J. P., Montgomery, A., and Sutherland, P. K., 1963, Geology of part of the southern Sangre de Cristo Mountains, New Mexico: New Mexico Bureau of Mines and Mineral Resources Memoir 11, 106 p.

Molnar, P., and Tapponier, P., 1975, Cenozoic tectonics of Asia: Effects of a continental collision: Science, v. 189, p. 419–426.

Narasimhan, T. N., Houston, W. N., and Nur, A. M., 1980, Role of pore pressure in deformation in geologic processes: Geology, v. 8, p. 349–351.

Oliver, J., 1982, Probing the structure of the deep continental crust: Science, v. 216, p. 689–695.

Olsson, W. A., 1974, Grain size dependence of yield stress in marble: Journal of Geophysical Research, v. 79, p.4859–4862.

Pendell, James, and Dewey, J. F., 1982, Permo-Triassic reconstruction of western Pangea and the evolution of the Gulf of Mexico/Caribbean region: Tectonics, v. 1, p. 179–211.

Roberts, R. J., Crittenden, M. D., Jr., Tooker, E. W., Morris, H. T., Hose, R. K., and Cheney, T. M., 1965, Pennsylvanian and Permian Basins in northwestern Utah, northeastern Nevada and south-central Idaho: American Association of Petroleum Geologists Bulletin, v. 49, p. 1926–1956.

Roper, P. J., 1974, Plate tectonics: A plastic as opposed to a rigid body model: Geology, v. 2, p. 247–250.

Ross, C., 1979, Late Paleozoic collision of North and South America: Geology, v. 7, p. 41–44.

Solomon, S. C., Sleep, N. H., and Richardson, R. M., 1975, On the forces driving plate tectonics; inferences from absolute plate velocities and intraplate stress: Royal Astronomical Society Geophysical Journal, v. 42, p. 769–801.

Speed, R. C., 1979, Collided Paleozoic microplate in the western United States: Journal of Geology, v. 87, p. 279–292.

Tapponier, P., and Molnar, P., 1976, Slip line field theory and large scale continental tectonics: Nature, v. 264, p. 319–324.

Comment and Reply on 'Time-temperature-burial significance of Devonian anthracite implies former great (~6.5 km) depth of burial of Catskill Mountains, New York'

COMMENT

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Friedman and Sanders (1982) attempted to estimate the former depth of burial by some Devonian sediments in eastern New York state on the basis of the level of organic metamorphism (LOM), determined largely by vitrinite reflectance. They derived their conclusions by making assumptions as to the thermal history and paleogeothermal gradients of the strata. The data are misleading, however, and their conclusions regarding the former depth of burial, whether correct or not, should not be based upon the information presented.

The entire approach hinges upon the use of a diagram by Hood et al. (1975) that correlates LOM with temperature and time. As a consequence of the many assumptions and measurement errors inherent in the construction of their diagram, however, Hood et al. (1975) recommended that it be used only for "relative" time and temperature values.

Interpretation of the LOM involves a number of difficulties at the East Windham site sampled by Friedman and Sanders. Vitrinite reflectance values there range from 2.2% to 2.5%, but only the larger value was used in the calculation. Vitrinite reflectance can vary as a function of source material and rock type even within a single locality. Thus, the values from one sampling site should not be used without establishing local or regional trends. The color of carbonized "kerogen" provides little additional information on the organic rank, given the semiquantitative nature of the measurement, the possibility of recycling, and uncertainty about the identity of the material. Black, carbonized spore material, for example, corresponds to a range of LOM from 12 to 20 (Hood et al., 1975). The conodont alteration index (CAI) data generally correspond to