FORUM

Comments and Replies on "Darwin's three mistakes"

COMMENT

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Hsü's (1986) provocative commentary concerning the "Darwinian fallacy" touches on several complex issues in evolutionary theory, some of which require clarification and elaboration.

The theory of biological evolution is not a single hypothesis but a series of more or less interrelated hypotheses (Riddiford and Penny, 1984). We can ask the questions: Has evolution occurred? Does all life share a common ancestry? If it does, then we should be able to reconstruct the phylogeny (relative degrees of relatedness) of different species. Reconstructing phylogeny depends upon a record, but the primary record of phylogeny is not the geologic record. Rather, the evidence of greatest utility in reconstructing phylogeny is the intrinsic morphology (sensu lato, including biochemical characters, behavioral characteristics, etc.) of the organisms under consideration (be they fossil or living species). Evolution as a theory of common descent was Darwin's (1859) foremost contribution. This theory is corroborated by the hierarchy of characters seen among various species (e.g., Hennig, 1966; Eldredge and Cracraft, 1980; Patterson, 1980; Rosen, 1984) and is independent of Darwin's interpretation of the geologic record and his proposed mechanisms of evolution. Perhaps Darwin was not the first to suggest some form of evolution, but his concept of common ancestry was novel. "The Great Chain of Being" (Lovejoy, 1936) was either considered static or as a series of evolutionary grades, and Lamarck's evolutionary theory included the concept of multiple instances of spontaneous generation followed by adaptation (Lamarck, 1984; Riddiford and Penny, 1984).

If we accept that all life has a common ancestry, we can hypothesize as to the tempo or rate of evolutionary divergence, change, speciation, and extinction; that is, we can address the problem of the history (as opposed to phylogeny) of life. In order to do so we need reconstructed phylogenies that we can subsequently relate to the geologic (fossil) record. As Hsü (1986) pointed out, Darwin (1859) was unsuccessful in reconstructing the actual history of life. But Darwin had neither accurate phylogenies nor a detailed and accurate knowledge of the geologic record at his disposal. However, we should not let Darwin's failure in this area detract from the importance of his successes otherwise.

If life shares a common ancestry we may also hypothesize mechanisms to account for the diversity of species seen in the world today. Darwin (1859) accounted for the diversity of species by means of both natural selection and chance events. By natural selection Darwin referred to the preservation under natural conditions of favorable individual variations within a species assuming that a typical population has the potential to produce more descendants than can survive given the available environmental resources (following the concept of Malthus). Natural selection may come about through biotic-biotic or biotic-abiotic interactions. Furthermore, according to Darwin's concepts of variations within a species and the common ancestry of all species, the descendants of a particular species need not belong to the same species as their ancestors (contra Hsü, 1986, p. 532). Darwin (1859) may have overemphasized biotic competition in discussing natural selection, but he never claimed that such direct competition is the exclusive mechanism of evolution.

Hsü suggested that a major causal mechanism of evolution is mass extinctions imposed upon species by external environmental factors; he termed this "survival of the luckiest" (Hsü, 1986, p. 534). But extinction alone serves only to decrease species diversity and put an end to lineages; extinction per se does not increase species diversity or form new lineages.

Recently some biogeographers have suggested that for many groups of organisms there is a strong correlation between the biogeographic distributions of various species and the phylogenetic relationships of the species (e.g., Croizat, 1964; Nelson and Platnick, 1981; Janvier, 1984). This suggests that there may be a causal relationship between phylogeny and physical factors that arise and separate geographic areas and their included biotic populations. If the individuals of a single species are divided by external factors into two or more populations, each population may differ slightly from the others. With time any slight differences may be magnified such that each population eventually forms a species distinct from the others. The diversity and history of life may in large measure be a function of vicariance events (physical events that subdivide an ancestral population) with natural selection and adaptation being of secondary importance. Of course, some potential vicariance events may result in catastrophic or mass extinctions.

Our understanding of evolution is better now than it was in Darwin's time, but it is still far from perfect. It is unfortunate that too many people continue to apply evolutionary concepts, many of which are falsified or outmoded, inappropriately to social situations. For this we should not blame Darwin; a better scapegoat is Herbert Spencer (Mayr, 1982, p. 386). Despite any shortcomings, Darwin and his ideas continue to deserve a place of honor in the history of science.

REPLY

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Schoch advances three points: (1) Darwin's main contribution was to have presented a "novel" concept of common ancestry; (2) Darwin did not claim that biotic competition is the exclusive mechanism of evolution; and (3) Darwin should not be blamed for social Darwinism. I cannot agree completely with the essence of his arguments.

The idea of evolution from common descent was very much in the air when the book *Vestiges of the Natural History of Creation,* published anonymously by Robert Chambers, appeared in 1855 (Mayr, 1982, p. 381). The idea had, in fact, taken root in biology when a Viennese biologist, Franz Unger (cited in Mayr, 1982) wrote in 1852: "It is in a marine vegetation consisting of thallophytes, particularly algae, that one must look for the germ of all kinds of plants that have successively originated." In fact, he then went a step further to designate the algae as "the origin of all organic life." As Mayr (1982, p. 391) pointed out, "Gregor Mendel was Unger's student, and has reported that it was Unger's pondering over the nature and source of variation leading to the origin of new species which had started him off on his genetic experiment." Darwin did make a novel contribution to the theory of common descent, and this is stated by the title of his book: *The Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life.* The essence of my article (Hsü, 1986) was to state that Darwin erred when he postulated natural selection as the means of evolution.

Darwin did consider the possibility of physical causes influencing the history of life, but he wrote, and I quoted him (Hsü, 1986, p. 583), "The most important of all causes of organic change is one which is almost independent of altered and perhaps suddenly altered physical condition, namely, the mutual relation of organism to organism—the improvement of one organism entailing the improvement or extermination of others." This statement is one of his three mistakes discussed in my paper; I particularly objected to his pronouncement that the improvement of one species entails the extermination of another, because he presented no scientific evidence.

Finally, I do not think the question is whether to blame Darwin for social Darwinism. The fact is, as Young wrote (1985, p. 609), Darwinism is social: "Extrapolations from Darwinism (of evolution) to either humanity or society are not separable from Darwin's own views." Frederick Engels, the philosopher and cofounder of communism, saw through the veil of pseudoscience in Darwin's history of life. He wrote the following:

"The whole Darwinist teaching of the struggle for existence is simply a transference from society to living nature, of Hobbes' doctrine *bellum omnium contra omnes* (that is, the war of all against all) together with Malthus' theory of population. When this conjuror's trick has been performed, . . . the same theories are transferred back again from organic nature into history and it is now claimed that their validity as eternal laws of human society has been proved. The puerility of this procedure is so obvious that not a word need be said about it."

Indeed too little has been said until one had the temerity to point out again, after more than a century, the "puerility of this procedure," after all the harm that has been done to humanity.

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COMMENT

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Hsü (1986) proposed that Darwin has been superseded, that three "mistakes led to the theory propounded in his book *On the Origin of Species.*" While there have of course been major advances since 1859, they have built on and to some extent modified Darwin's foundation rather than undermining it. Darwin's work is astonishingly modern, more so than much that has appeared in the interim. Neither natural selection nor Darwin is in trouble.

One of Hsü's "mistakes" is a result of an inaccurate quotation from Darwin. What Darwin said on p. 66 was, "Lighten any check, mitigate the destruction ever so little, and the number of the species will almost instantaneously increase to any amount." Hsü omitted "the" before "species" and inferred that Darwin was discussing an increase in number of species. He was not. As with the preceding (and also misinterpreted) quotation given by Hsü, Darwin was providing an informal deductive derivation of the importance of competition within a population as part of his deductive argument for natural selection. (Darwin used other kinds of evidence also.)

The same argument does, however, apply at the level of species, and Stanley (1979) and others before him have made this explicit. Lyell used it implicitly in 1832. It applies at higher levels also. For instance, the logistic-like increases in the number of marine families early in the Phanerozoic (Sepkoski, 1979) is easily explained, at one level, by the derivation of the logistic itself: potential exponential increase checked by a diversity-dependent damping. (Hoffman [1985] failed to find much evidence of diversity dependence, unlike most other studies, but his work does not take adequately into account the strong dependence of diversity, extinction rate, and origination rate on geologic time. These dependences are not in the same direction and are complex in different ways. Correlations with time can produce interesting results, like Yule's famous correlation of the number of storks in Denmark with the birth rate there. Hoffman also used total rates rather than probabilities, which are what is relevant for regulation.)

That less than a thousandth of the carbon reduced in photosynthesis becomes irretrievably buried is strong evidence that, considered in terms of energy, the world is as full of life as it can reasonably be now. For it to hold more, the only possibility is for more primary production. Because energy sources other than photosynthesis are negligible, either (1) total energy use is regulated by the amount of free energy (reduced carbon in an oxidizing environment) available or (2) the closeness of the total use to the total amount available is a coincidence. The closeness is local and nearly ubiquitous as well as global, so the unlikely possibility of a coincidence is ruled out. Free energy is a necessary resource, we see that it is commonly in short supply, and organisms increase until checked; therefore competition for energy, directly or indirectly, is common.

Note that this argument says nothing about how the free energy is partitioned; there might be a great increase of nematodes or mammals at the expense of bacteria (or vice versa). More species can be accommodated at the expense of less average energy, and probably narrower average niches, per species. Organisms do not cooperate unless they are selected to do so as a result of an advantage to themselves of a sort on which natural selection can operate. Such sorts are now known to be broader than used to be thought and can be important in geochemical cycling in a way that has not yet, I think, become known to geochemists, but our approach should emphasize mechanisms rather than optima. That something would be good for a species or the world's biota is no guarantee that there is a way for it to occur. Kropotkin's mutual aid and Lovelock's Gaia are merely desiderata, and they lack any reasonable mechanism (Van Valen, 1983).

It rather appears that Hsü's distaste for social Darwinism, which Darwin himself did not espouse, makes him antagonistic to competition. Yet the geochemical argument in the preceding paragraphs, Darwin's (and Lyell's and Malthus's) argument of the enormous potential for organisms to increase, and much specific evidence (Schoener, 1983, 1985; Connell, 1984) indicate its real importance. Possibly, Darwin and Wallace discovered the importance of natural selection in part because they lived in a culture where the importance of competition was not concealed. (I know of no culture, and cannot realistically imagine any, despite Marx and other utopians, where all desiderata are equally available to all.) It is nevertheless useful to take the natural world on its own footing and actively try to eliminate our own biases when we deal with it. Maybe we don't like it; maybe we shouldn't like it; but it is there.

The importance of physical catastrophes in the history of life remains controversial, as Hsü noted, although he did not note that his favorite catastrophe, that of the Cretaceous/Paleogene boundary, also remains controversial in its causation (Van Valen, 1984, provides a somewhat outdated review). He even seems to think that more or less all prehuman extinctions were caused by the physical environment. Undoubtedly many were, although it is commonly difficult to establish causes for extinctions, and association with a change in the physical environment is fully compatible with a change in biotic interactions that was caused by the physical change. The latter effect is also how competition can be involved in mass extinctions. There are nevertheless various general and specific examples (e.g., the mammalian order Multituberculata; Van Valen and Sloan, 1966) where physical causes are unreasonable. My bias (as a "specialist" in extinction if anyone is) is that most nonlocal extinctions have been caused by biotic interactions, although I confess a fascination with catastrophes at all scales.

Hsü (1986) also castigated Darwin for believing that stratigraphic gaps have created a false impression of discontinuities in the fossil record. He said that Darwin "must have envisioned a time gap of about 100 m.y." between the Cretaceous and Paleogene, during which ammonites gradually went extinct, by artfully conjoining views of Lyell with those of Darwin. Four points: Cretaceous ammonites did indeed go extinct gradually, although perhaps not the final few genera (and certainly not the plankton). There are indeed stratigraphic gaps, and Darwin was correct to emphasize them (although, I agree, too much) and also cautious enough *not* to try to quantify them. Darwin explicitly did not believe in a constant rate of evolution. For instance, he said in the summary of Chapter 11 in the sixth edition of the *Origin of Species,* "it is probable that the periods, during which each [species] underwent modification, though many and long as measured by years, have been short in comparison with the periods during which each remained in an unchanged state." Suppose Hsü were correct anyway. So what? Huxley, "Darwin's bulldog" in the acceptance of his theory, disagreed with Darwin on the prevalence of gradual changes and instead advocated saltational changes. He certainly did not consider the disagreement to be a central one. This is not the same as advocating the importance of mass extinction, where Hsü and I do agree, but a more fundamental disagreement with Darwin and one that is still active within the overall Darwinian framework.

Why should Hsü think that militarism has any bearing whatever on the processes of the natural world, unless it represents (as Hsü denied) a perverted extension of those processes? We regard it as evil and oppose it, but it is only now that the Catholic Church is thinking about rehabilitating Galileo. One should try to understand the world before condemning it or its students. We today know more than Darwin did, but in some things Darwin knew more than Hsü does.

ACKNOWLEDGMENT

I thank K. J. Hsü for comments on the manuscript.

REPLY

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Biologists have indeed built their advances in evolutionary theory on the Darwinian foundation, not realizing that the foundation is about to topple because of Darwin's three mistakes. Maynard Smith (1984), for example, expressed his surprise when he found out that paleontologists "read the fossil record differently. The dinosaurs, they believe, became extinct for reasons that had little to do with competition from the mammals." Honoring them at "the high table," this illustrious biologist took the paleontological data seriously. Van Valen, on the other hand, thought (Van Valen and Sloan, 1977), and apparently still thinks, that the dinosaurs lost out while the mammals survived because of what Darwin called the "preservation of favoured races in the struggle for life."

A century has elapsed before Darwin's mistakes were discussed, but my inaccurate quotation was pointed out by three persons within a week after my commentary was published; Darwin's "bible" is still being read. Yes, there was a misquotation, and I apologize. I may perhaps be excused for this faux pas on account of my Chinese origin, having grown up with a mother tongue devoid of the use of articles. After having used English as a means of communication for some 40 years, I still fail frequently to appreciate the importance of *the* in this foreign language. What Darwin said was indeed to repeat what Malthus said, that the number of individuals of a species may "increase to any amount." However, my point that Darwin was guilty of category confusion (Hsü, 1986) is still valid, with or without that particular quotation. When Van Valen (Comment above) says that "the same (Malthusian) argument does . . . apply at the level of species," this is only a reiteration of Darwin's mistake. The argument does not apply because individuals of a species multiply through reproduction, but diversification of species is not an inevitability. Malthus is correct to state that the world cannot support an unlimited growth of population, but there is no reason why the number of species on Earth would become infinite even if their increase is not checked by natural selection.

Whether the time gap between the Cretaceous and Tertiary envisioned by Darwin is 100 m.y. or somewhat less is debatable. The indisputable fact is that Darwin dismissed the record of terminal Cretaceous extinction as an artifact of an imperfect geologic record. This was his mistake. Whether the ammonites were on their decline before their final extinction has been debated, especially after the discovery of increasing ammonite diversity in a transgressive Maastrichtian sequence of the Antarctic (Macellari and Zinsmeister, 1985). The relevant issue is, however, whether there was then an unusually accelerated extinction rate for many groups of organisms. The fact, summarized by Jablonski (1986), speaks for itself; there is no need for repetition.

Van Valen has a "bias . . . that most nonlocal extinctions have been caused by biotic interactions" and thus denies that Darwin was mistaken. There are biotic interactions in the history of life, but there is little evidence that mass extinctions had much to do with such interactions. As Emiliani (1982, p. 13) pointed out, "extinctions of abundant and widespread species of marine Protista are abrupt and precede the appearance of new species. Extinctive evolution, . . . with its processes of sudden extinctions and sudden appearances, absence of competition, absence of "missing links," and frequent survival of the misfit or the indifferently fit is . . . more applicable to the paleontological record." The pattern of the terminal Cretaceous extinction of other marine invertebrates is similar (Surlyk and Johansen, 1984). Whereas biotic interactions may help determine winners among the survivors in a rush for the reoccupation of the vacated ecologic niches, there is no indication for selective survival of the "favoured races."

Van Valen denies that Darwin espoused social Darwinism. I disagree, but I shall not quibble over the issue. The fact remains that Darwinism has a social origin. As a recent student of Darwinism has pointed out, "the extrapolation from Darwinism to either humanity or society are not separable from Darwin's own views, nor are they chronologically subsequent. They are integral" (Young, 1985).

Darwin did not fool all of his contemporaries. I cite Frederick Engels in my Reply to Schoch's Comment (above) that the whole Darwinist teaching of the struggle for existence is but a "conjurer's trick." Arguments on the basis of the social philosophy of his time were the basis for Darwin's interpretation of the history of life, and his theory was then transferred back to provide the scientific basis in support of social Darwinism as the eternal law of human society. "The puerility of this procedure is so obvious," Engels wrote in 1875, "that not a word need be said about it."

I wish Engels had dwelt more on the "puerility of this procedure," so as to show the true color of "the argument of noise and sneers with which (Darwinists) tried to put down . . . everyone . . . who did not subscribe to the infallibility of the God Darwin and his prophet Huxley," as Tristram said in 1860. Darwin has made mistakes, and his mistakes have brought misery to humanity. Facing the bias and obstinacy of Darwinists, I feel almost tempted to join my colleague, Paul Feyerabend (1975, p. 7), who proposed to lead "three cheers to the fundamentalists in California who succeeded in having a dogmatic formulation of the theory of evolution removed from the text books and an account of Genesis included."

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Comments and Replies on "Early Archean silicate spherules of probable impact origin, South Africa and Western Australia"

COMMENT

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Lowe and Byerly (1986) presented detailed petrographic and chemical data to support the conclusion that layers of sand-sized silicate spherules found in Early Archean rocks of South Africa and Australia are deposits of melt droplets from ancient impact events. This is an intriguing theory that deserves further study. This Comment makes two points: (1) similar spherules have been produced in established impact events, particularly at the Precambrian impact structure at Sudbury, Canada; and (2) at all previously studied impact structures, such spherules make up a negligible fraction of the ejecta, which consist chiefly of shattered and shock-metamorphosed rock and mineral fragments and irregular bodies of glass.

Spherules of devitrified glass, apparently formed as a primary impact product, were found at Sudbury in one inclusion in the Onaping Formation, the unit of fallback breccia associated with the Sudbury impact event. Despite post-impact recrystallization, original glassy textures, flow banding, and compositional variations between different types of glass are still preserved in the Onaping Formation (French, 1968, 1972; Peredery, 1972). The spherules occur in a rim, 1–2 cm thick, composed of green heterogeneous glass and rock fragments that surrounds a core of shock-metamorphosed granitic rock of approximately 10 cm (French, 1968, p. 391–393).

The spherules are generally 0.1–0.5 mm, spherical to elliptical in shape, and composed of pale- to dark-yellow-brown devitrified glass (Fig. 1). The spherules commonly have a darker concentric rim and many contain one or more fragments of angular quartz in the center. A few of these quartz grains contain *shock lamellae,* planar features produced by intense shock waves (Fig. 2). These lamellae appear identical to those observed in shock-metamorphosed rocks at Sudbury (French, 1968, 1972) and in the granitic core of the inclusion.

The spherules are thought to have formed immediately after impact in the cloud of material ejected from the Sudbury crater. They apparently formed by deposition of melted or vaporized target rock onto small fragments of quartz that served as nuclei. The individual spherules were then accreted onto the central granitic block, together with other heterogenous glass fragments and rock fragments, as the block passed through the ejecta cloud.

Several features suggest that the spherules are primary and are not the result of subsequent devitrification. The spherules are not widespread; they

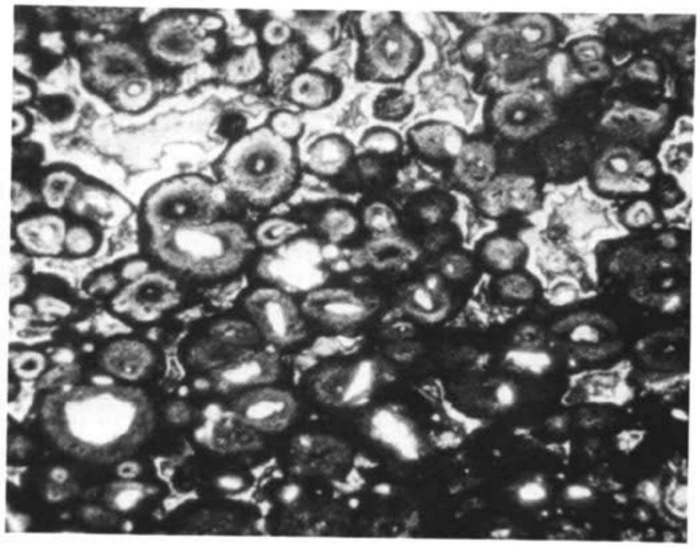

Figure 1. Silicate spherules, 0.1–0.5 mm, from glassy rim around shock granitic inclusion in Onaping Formation, Sudbury, Canada. Area of isolated individual spherules composed of yellow-brown devitrified glass. Note spherules with angular quartz fragments in center. Long dimension of field about 2 mm.

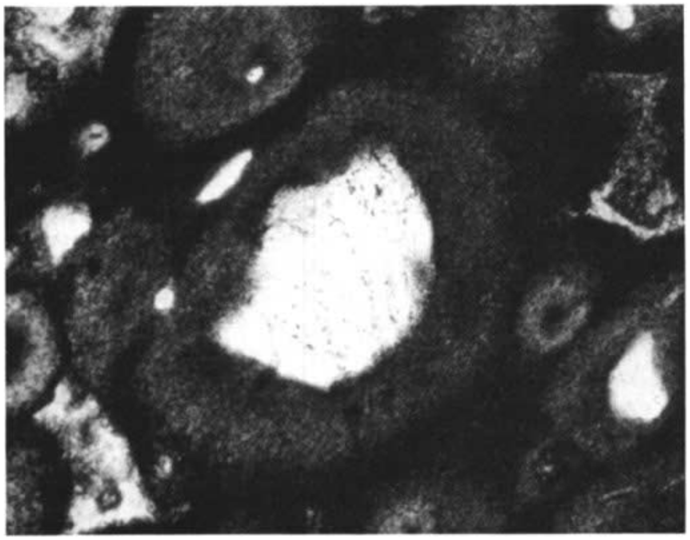

Figure 2. Single silicate spherule about 0.3 mm long, with central inclusion of quartz showing two poorly developed sets of shock lamellae. Long dimension of field about 0.55 mm.

occur only in specific regions within the glassy rim and are not found in adjacent glassy areas or in the surrounding Onaping Formation. Within the glassy rim, contacts between areas of spherule-rich and spherule-free glass are sharp. Contacts between the spherules themselves are smooth and not polygonal (mosaic). Apparent plastic deformation of the spherules is evident adjacent to associated rock fragments, where the spherules are deformed and are elongate parallel to the apparent direction of compression and flow around the fragment.

These observations indicate that spherules like those described by Lowe and Byerly (1986) can be formed by meteorite impact. However, such spherules are extremely rare in the Sudbury ejecta deposit; they have been observed only in the one inclusion described here. Spherules have not been found in the more common fragments of vesicular, flow-banded glass in the Onaping Formation, nor as individual fragments in the Onaping Formation itself (French, 1968, 1972; Peredery, 1972; Stevenson, 1972). This observation is consistent with the apparent rarity of spherule-like objects in the ejecta from the younger Ries Crater, Germany, and in lunar samples (Graup, 1981).

The rarity of spherules in ejecta from Precambrian and younger impact craters indicates that caution should be used in designating the spherule-rich layers described by Lowe and Byerly as impact deposits. It would be desirable to search these layers for associated shock-metamorphic effects, especially for shock lamellae in the quartz and feldspar that occur as lithic inclusions in the spherules and as associated lithic material. The layers should also be analyzed for iridium and other siderophile elements whose anomalously high content, compared to adjacent layers in the sequence, would indicate an extraterrestrial signature (Alvarez et al., 1980).

REPLY

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French provides some extremely interesting details on quench spherules associated with the Sudbury impact structure that resemble particles we described from the Archean of South Africa and Western Australia (Lowe and Byerly, 1986). In addition, he offers two comments concerning interpretation of the Archean particles: (1) caution must be exercised in interpreting the Archean spherule layers as impact deposits because of "the rarity of spherules in ejecta from Precambrian and younger impact craters," and (2) the layers should be searched for associated evidence of impacts such as shock lamellae and anomalous concentrations of siderophile elements, especially iridium.

French is correct in pointing out the low abundances of silicate spherules associated with younger impact structures; they represent trace constituents in most proximal ejecta blankets. The sample of known impact sites, however, is strongly biased toward relatively small craters located on cratons. There is both theoretical and empirical evidence that ejecta deposits developed around these craters may not adequately represent the range of possible impact processes and products.

Recently, Stevenson (1985) has argued that, during accretion, very large terrestrial impacts may have produced long-lived ($\sim 10^2$ yr), globe-encircling clouds of supercritical silicate fluids. The formation of fluid clouds of shock-melted and vaporized rock during impacts would clearly provide a mechanism for the subsequent condensation and/or crystallization of silicate spherules and the formation of spherule layers lacking other types of ejecta.

Such reasoning derives largely from a theoretical base; however, it is clear that there are also empirical reasons for suspecting that some terrestrial impacts have produced immense volumes of melt or vaporized material. Tectite fields are characterized by particles representing solidified liquid-silicate masses considered by most workers to have formed during terrestrial impact melting (Alvarez, 1986). Although larger particles commonly show aerodynamic shaping, smaller sand-sized grains or microtectites are commonly spherical. Eocene North American tectite deposits (Glass and Zwart, 1979) probably originally consisted of thin laminae composed of virtually pure spherules. Locally, these grains contain quench-textured crystallites (John and Glass, 1974). Similar spherule layers are widespread at the Cretaceous/Tertiary boundary (Montanari et al., 1983). Although the origin of these particles is still being debated, it is clear that their formation through impact melting remains a strong possibility. Although the thickness of these spherule layers is much less than those we describe from the Archean, they suggest that mechanisms exist for forming nearly pure spherule layers through impact processes.

These observations also suggest that some meteorite impacts may produce large amounts of impact melt and spherules whereas others do not. The control may include the size of the impacting bolide; the nature of the target material, possibly including the presence or absence of a thick column of surface water; or as yet unknown factors. It is also possible that spherules are produced mainly by processes acting away from impact sites, such as melt-vapor condensation and crystallization. If spherules are produced by condensation from impact melt-vapor clouds or represent long-traveled ballistic particles, it would be reasonable to hypothesize the common development of an extensive distal layer of fine-grained ejecta, as seen in volcanic eruptions, perhaps including both spherules and lithic or vitric grains, or an even more extensive deposit made up largely of condensate, perhaps spherules. Both the characteristics of known, probably impact-produced spherule deposits and the theoretical mechanisms developed to explain their formation are consistent with the conclusion that abundant spherules should not be found associated with coarse proximal ejecta blankets around impact craters. Therefore, it would seem premature to suggest that the abundance of spherules around younger impact sites constitutes evidence relevant to the impact or nonimpact origin of Archean spherule layers.

We certainly agree that additional studies are necessary. Toward that end, we have obtained funding from the NASA Planetary Materials and Geochemistry Program to complete a more thorough petrographic, major- and trace-element (including platinum group elements), and geochronological study of these rocks.

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COMMENT

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Is there any Earthly (as opposed to lunar or other planetary) evidence that extraterrestrial bodies once struck this planet much more often than they do now? Lowe and Byerly (1986) think that there is, in the shape of some peculiar spheroidal structures that are common in two Early Archean chert beds, one in South Africa and one in Australia. They consider these spherules to have formed when meteoritic impacts created droplets of silicate melt that were then solidified by quenching, thereby producing their unusual textures. It is thus significant that similar spherules occur in another chert bed (the North Pole chert-barite unit) located in the same stratigraphic sequence (the Warrawoona Group) as the Australian chert horizon sampled by Lowe and Byerly. These North Pole objects, however, had a commonplace, and entirely terrestrial, history: they were sand grains detritally derived from underlying silicified basalts. I here suggest that the same process produced the spherules found by Lowe and Byerly (1986).

The most abundant North Pole spherules, first described and illustrated by Dunlop and Buick (1981), appear identical to the type 1 spherules of Lowe and Byerly. They are spherical, ellipsoidal, or dumbbell-shaped grains, 0.3–2.5 mm in diameter, that have an exterior iron-oxide crust, an outer irregular layer of pale-green sericite, and a microquartz core that may contain eccentric internal spherules (cf. Figs. 2A, 2C in Lowe and Byerly, 1986). Under crossed nicols, most core microquartz extinguishes radially in a style manifestly pseudomorphing fans of fibrous chalcedony. As such, chalcedony is almost always precipitated from cool aqueous solutions as a secondary void-filling phase (Folk and Pittman, 1971); these spherules cannot be quenched droplets of highly siliceous melt. Instead, they must be sandy detritus derived from older rocks. An obvious source exists in the flows of high-magnesian and tholeiitic basalt immediately beneath the chert-barite unit. There, vesicles of appropriate size and shape once abounded but most now contain amygdules of identical composition, structure, and texture to the spherules. These were evidently precipitated soon after eruption when the basalts were intensely silicified during seafloor alteration (Barley, 1984). Initial reactions between hot rock and infiltrating seawater probably formed the outer layers of the amygdules (and spherules), whereas their core chalcedony precipitated slightly later when hydrothermal brines boiled or were chilled near the water-rock interface. The amygdaloidal basalts were then exposed to erosion, producing cross-bedded sandstones up to 5 m thick composed of lithic fragments and spherules or, rarely, massive sandstones 0.5 m thick containing nothing but spherules All arenites were later totally silicified.

Sand grains apparently identical to the type 2 and 3 spherules of Lowe and Byerly (with cores of sericite and microquartz-sericite, respectively) are also present in the North Pole chert-barite unit, though in lesser numbers than the microquartz-rich grains. Amygdules of similar composition, structure, and texture also exist in the underlying basalts. Spheroidal structures of a type not noted by Lowe and Byerly (with cores of drusy megaquartz) also occur both as sand grains in the chert and as amygdules in the basalt. Only the type 4 spherules of Lowe and Byerly (with "microcrystalline quench textures") are not present either as spheroidal sand-sized clasts or as amygdules. However, subrounded, subspherical sand grains with textures identical to those of many Lowe and Byerly type 4 spherules abound in the North Pole chert-barite unit. Specifically, they show in pseudomorphous form the "spinifex" quench textures of skeletal clinopyroxene, the dendritic quench textures of skeletal ilmenite, the subophitic igneous textures of lath-shaped plagioclase, and the fibroradial devitrification textures of chlorite and albite that are typical of the matrix in the underlying basalts. Thus, these subspherical sand grains are also detritus derived from older basaltic rocks. As they were already silicified when fragmented, their response to abrasion probably resembled that of quartz sand, well known for its tendency toward sphericity when subjected to prolonged transportation. Hence, if silicified lithic sand accumulated slowly in a littoral environment where the clasts were agitated by joint wave and tide action (rather than accreting rapidly in a sublittoral situation as in the North Pole chert-barite unit), most grains should have been abraded until spherical. It is thus significant that the rocks at both of the sites sampled by Lowe and Byerly were evidently deposited in very shallow water (Lowe, 1982; Buick and Barnes, 1984; Lowe et al., 1985).

Apart from their great similarity to detrital sand grains derived from silicified basalt, there are several other reasons for supposing that the Lowe and Byerly spherules did not form from droplets of silicate melt generated in impact events. First, microcrystalline spherules are common at both of their localities, but such objects are only rarely produced by impacts (Montanari et al., 1983). Even when they are, they are mainly found as minor components of thick terrestrial impact breccias in which the debris stayed hot for so long that crystals nucleated in melt droplets (John and Glass, 1974). Montanari et al. (1983) argued that similar spherules could form in abnormally hot ejecta clouds created by giant impacts, but fallout beds so derived should be graded (if subaqueous) or breccioid (if subaerial). Such host rocks were not noted by Lowe and Byerly. Second, their spherules are not associated with any angular ejecta (e.g., glass shards, lithic fragments); indeed, their samples are often remarkably free of nonspherical grains. However, in all undoubted impactites as yet known, fragmentary ejecta are overwhelmingly abundant. Some should be present even in deposits from giant impacts. Third, neither spherules nor associated sediments show any evidence of shock metamorphism, but even small impacts generate shocked material in abundance, within and without spherules (Fredriksson et al., 1973; Graup, 1981). Because shock features can survive mild metamorphism and deformation, they are often obvious in Precambrian impact deposits but are not obvious at the Archean sites of Lowe and Byerly. So, to account for the unusual abundance of spherules, especially microcrystalline ones, and the abnormal absence of angular ejecta, shocked or otherwise, at those sites, reworking must be invoked. But if this happened, why were spherules selectively concentrated?

It thus seems probable that the spherules found by Lowe and Byerly were not produced in ancient impact events but were instead rather ordinary detrital sand grains. So their exceptional abundance in Early Archean rocks does not necessarily show that the primordial cosmic flux to Earth was greater than can be extrapolated from other planetary surfaces. Lowe and Byerly (1986) implied the opposite, a conclusion that, if correct, requires the revision of current ideas about the evolution of the solar system. This need may now be illusory.

ACKNOWLEDGMENTS

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REPLY

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The main point offered by Buick against an impact origin for the Archean spherules discussed by Lowe and Byerly (1986) is that they are similar to detrital particles described by Dunlop and Buick (1981) from sandstone in the Archean North Pole chert-barite unit, Western Australia, and interpreted to have been derived by erosion of basaltic volcanic rocks. The only particles described by Dunlop and Buick (1981) that are similar to our type 1 spherules are their type 9 particles, "circular clasts (0.5–1 mm diameter) with a thin concentric surface crust of opaque minerals and internal coarse-grained and cryptocrystalline silica, carbonate and minor chlorite . . ." (Dunlop and Buick, 1981, p. 230). Although Dunlop and Buick concluded that most grains in the sandstone were derived by erosion of underlying basalts, the caption for their Figure 3 notes that "other grains in the arenites [including specific reference to the only figured grain of type 9] are not directly comparable to any textures observed in the metavolcanics. . . ." Buick (1985), however, indicated that these particles "resemble . . . amygdales in silicified basalt," and in his Comment he states that the underlying basalts "contain amygdules . . . of identical composition, structure, and texture to the spherules." The resemblance between type 9 particles and amygdules appears to have improved considerably with time. The scarcity of figures and contradictory observations make it difficult to assess the origin of these detrital grains and their relevance, if any, to the origin of the spherules we described.

Buick also notes the similarity between the quench textures in our type 4 spherules and those of still other basaltic detrital grains in the North Pole chert unit. This textural similarity leads him to conclude that the spherules we described must also have been eroded from volcanic rocks. The resemblance between quench textures in grains eroded from basalts and those in spherules that may have formed by impact melting of basalts should not seem surprising. Both have formed by the quenching of basaltic liquids. Quench textures in both types of grains reflect the mode of crystallization of the basaltic melts, not the mode of origin of the grains. The Archean spherules of Lowe and Byerly (1986), especially those from Australia, display a much greater variety of quench features than associated volcanic units, and they lack blocky olivine grains, microlites, and subophitic textures of nonquenched volcanic rocks. Such textures are abundant, however, in detrital grains in the North Pole chert unit (Dunlop and Buick, 1981).

What Buick and others who advocate a detrital origin for the spherules ignore are the many features of these particles and of the layers they form that are inconsistent with an origin by the erosion of volcanic rocks. In many areas, the spherule beds consist of nearly 100% spherules. There is simply no known natural mechanical sedimentary means of concentrating spherical quenched grains to the virtual exclusion of associated, irregular to nearly spherical quartz, feldspar, and lithic grains having the same overall size and density. Even Buick, while advocating a detrital origin for the spherules, queries ". . . why were the spherules concentrated selectively?" He paints an attractive picture of waves lapping onto an Archean beach, shaping and sorting the grains, but it does not accurately depict the actual depositional setting. In most sections where the Barberton spherule bed consists of nearly pure spherules, it forms a single unit of sand-sized material deposited in a quiet, low-energy environment. Underlying and overlying layers consisted mainly of fine-grained, organic-rich sediments or silt- and clay-sized debris lacking current structures. Normal size grading of spherules is commonly well developed in such sections; Buick's presumption to the contrary is incorrect.

It is especially telling that, where current structures are present in the spherule beds, the concentration of spherules is generally less than 10%, reflecting dilution, not enrichment, of the spherules by mechanical processes. Not only would an unknown mechanism have to be discovered to concentrate the spherules in the spherule beds, an equally enigmatic process would be required to exclude spherules from adjacent clastic layers. In both Australia and South Africa, interbedded clastic units lack spherules, even though, where current worked, the spherule layers contain abundant detrital particles. If derived by erosion of underlying basalts and distributed by surface currents over hundreds of square kilometres, as in the case of the Barberton unit, it is inconceivable that the spherules would be restricted to a single thin bed a few centimetres thick and be absent in underlying and overlying layers. The distribution, composition, and structuring of these beds argue strongly for a fall origin.

Buick should note that fans of fibrous chalcedony are not indicative of silica precipitation in void spaces. White and Corwin (1961) demonstrated their formation during the devitrification and alteration of silicate glass.

Buick also comments that (1) the "spherules are not associated with any angular ejecta (e.g., glass shards, lithic fragments) . . ." and (2) "neither spherules nor associated sediments show any evidence of shock metamorphism . . ."; however, we (Lowe and Byerly, 1986, p. 83) clearly stated that both the Barberton and Australian spherule beds in many sections consist of coarse-grained, silicified, current-deposited detritus containing less than 10% spherules. The bulk of this detritus in both units is lithic debris, but not juvenile volcaniclastic material, and much is angular to subangular and nonspherical. We are not yet certain that any of the debris represents ejecta; it may all be ejecta reworked by currents, or conversely, all may represent clastic detritus present prior to and/or introduced following spherule deposition.

We are studying these units in search of shock features, but have not yet recognized any. However, not only are these probably distal deposits, but shock lamellae and similar features are best seen within individual mineral grains (e.g., Fredriksson et al., 1973). Both spherule beds described by us contain less than 10% and generally less than 5% primary minerals, mainly coarse quartz. It is not clear that this is impact ejecta because similar debris is present as loose clastic material below and/or above the spherule layers in both areas. Because all lithic, ferromagnesian silicate, and feldspar grains in these sequences have been metasomatically altered, including obliteration of many fine primary internal features such as compositional zoning and feldspar twins, it seems doubtful that fine shock structures would have survived, as asserted by Buick.

We recognize that considerably more evidence must be developed if these Archean spherule layers are to be seriously regarded as impact deposits. However, the hypothesis offered by Buick that the spherules were derived by erosion of volcanic rocks is based on inconsistent, incorrect, and selective observations and fails to account for many of the important features of these beds. As presented, it cannot be considered as a viable alternative to the hypothesis of an impact origin. The only reasonable alternative would seem to be some unusual form of explosive volcanism, but it is very difficult to account for the widespread distribution and locally bimodal composition of the spherules by volcanic processes.

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Comment and Reply on "Deep seismic reflection profile across the northern Appalachians"

COMMENT

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Keen et al. (1986), in describing the deep crustal seismic structure of the Newfoundland Appalachians, stated that the surface locations of the geologic terranes shown in their interpretive section (Keen et al., 1986, Fig. 2) are known from the onshore-offshore correlation of magnetic and gravity anomalies (Haworth and Jacobi, 1983). I think this is a questionable assumption.

It has been known for a long time that the rocks of central Newfoundland are disposed in a major Z-shaped structure, initially referred to as the Notre Dame Bay orocline, but later renamed the Hermitage Bay flexure. In the Burlington Peninsula, the nature of the flexure—particularly if one adopts the views of Hibbard (1982) concerning geologic relationships in the vicinity of Mings Bight—suggests that the Baie Verte line swings to the east rather than continuing on a northeasterly, more or less straight-line trajectory to the west of the Horse Islands as indicated in Figure 1 of Keen et al. (1986). The same deviation is even more evident in the case of other potential major paleogeographic boundaries (Lobster Cove, Crescent Lake–Tommy's Arm, Sops Arm, Lukes Arm, Reach faults) in the Notre Dame Bay region. The location of the Gander River ultrabasic belt (GRUB) line in the interpretive section (Fig. 2) also does not match its position as shown on the terrane map (Fig. 1): to meet the seismic line of section at 265 km the GRUB line should bend to the east to match the offshore change in trend of the Dover fault. It would appear, therefore, that major geologic boundaries mapped on the Newfoundland mainland cannot be projected unambiguously onto the seismic line of section, and it is perhaps revealing in this respect that even Keen et al. recognized that "the boundaries between these five (seismically defined) regions do not directly correspond to the major terrane boundaries defined by surface geology (projected onto the seismic line)." It should also be noted that the existence of the flexure may cause the offshore ground width of geologic units mapped on the Burlington Peninsula to be substantially increased, and it is not impossible, therefore, that the Baie Verte line appears on the line of section in Figure 2 at 180 km, the supposed location of the collisional suture. Consequently, even if it is assumed that Keen et al. have really identified continental crust out to 180 km in the offshore seismic section, the common view of the onshore Dunnage zone as an in situ oceanic domain is not disproven—although the model has long been suspect on the basis of onshore gravity and geologic data.

Keen et al. commented on the variation in magnitude of tectonic transport along the length of the Appalachian system. It is perhaps worth noting in this respect that inasmuch as closure along the length of the Appalachian-Caledonian orogen is a minimum in its central part (Britain, northwest Newfoundland, and Quebec-Maine) and a maximum, but with opposite thrust polarities, in Scandinavia (Laurentia overthrusting Baltica) and the southern Appalachians (Laurentia underthrusting Africa), the Appalachian system mimics the Alpine belt between western Europe (Africa thrust over Europe), Turkey-Iran (minimum closure), and India (India underthrusting Asia), as well as perhaps the Pan-African of East Africa (east thrust over west) and the Arabian–Nubian Shield (minimum closure). Consequently, the major rethinking of Appalachian collisional tectonics that Keen et al. now feel obliged to undertake on the basis of their seismic model should perhaps first of all involve an evaluation of the results of recent investigations carried out in other orogenic systems, particularly those studies involving trace and isotopic geochemistry (e.g., Harris et al., 1984). The seismic data set alone does not provide the means to determine unambiguously the present-day nature of the crust beneath the Newfoundland Appalachians.

REPLY

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Church's main criticism of our paper (Keen et al., 1986) concerns the surface location of the Appalachian tectonostratigraphic zone boundaries, which were positioned on the basis of onshore-offshore correlation of gravity and magnetic anomalies. It is certainly true, as Church points out, that "the major geologic boundaries mapped on the Newfoundland mainland cannot be projected unambiguously onto the seismic line." This is a perennial problem that we face in the geologic interpretation of marine seismic data, and it underscores the importance of complimentary terrestrial deep seismic reflection studies. However, the gravity and magnetic anomalies are well defined both onshore and offshore, and the approximate position (±10 km) of the zone boundaries can be traced with confidence from land to sea.

As noted by Church, terrane boundaries shown in Figure 1 of Keen et al. (1986) do not include the flexure of the Baie Verte–Brompton (BV-B) line as proposed by Hibbard (1982). Incorporation of this flexure would place the BV-B line at km 140 in Figure 2 of Keen et al. (1986), thereby requiring about 40 km of overthrusting by the Dunnage zone onto the Grenville craton, as opposed to the 70 to 80 km suggested by the more conventional placement of the BV-V line (Haworth et al., 1976; Haworth and Jacobi, 1983; Keen et al., 1986). Hibbard (1982) extended the flexure northward from the Baie Verte Peninsula to within 15 km of our present seismic line (cf. Fig. 3 of Hibbard, 1982, and Fig. 1 of Keen et al., 1986) by using the geophysical trends of Haworth et al. (1976; see also Haworth and Jacobi, 1983) which continue to the north-northwest across our seismic line. This precludes an in situ interpretation for the BV-V line and the western Dunnage zone as suggested by Church.

Church also implies that our interpretation of Grenville crust underlying the Humber and western Dunnage zone to km 180 (Keen et al., 1986, Fig. 2) is questionable. This lower crustal block has a remarkably regular and characteristic seismic signature and is continuous to km 180, where a distinct break separates it from the eastward adjacent block. Deep seismic interpretation, in the absence of detailed refraction data and/or drillhole data, is always subjective. However, our ability to trace this lower crustal block from exposed Grenville craton in Labrador greatly increases our confidence in our interpretation.

Although changes in the positions of the zone boundaries as show in Figures 1 and 2 of Keen et al. (1986) by more than about 25 km are unlikely, there are minor discrepancies between some positions as plotted in these figures. In Figure 2, the BV-B line and the Gander River ultrabasic belt (GRUB) line have been plotted 17 and 15 km east of their correct positions, respectively. The correct positions are shown in Figure 1. We apologize for these errors and any confusion arising from them. However, they change none of the substantive conclusions we presented (Keen et al., 1986).

We thank Church for pointing out the amounts of closure across other orogenic belts. As more deep seismic data become available, it will be necessary to reevaluate other geological and geochemical data, as he suggests. However, this task was far beyond the scope of our paper (Keen et al., 1986).

Finally, we agree with Church that "the seismic data set alone does not provide the means to determine unambiguously the present-day nature of the crust." However, deep seismic data are among the most powerful tools available for constraining large-scale and deep-crustal tectonic processes in the Canadian Appalachians and other orogenic belts.

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Comment and Reply on "Distribution of maximum burial temperatures across northern Appalachian Basin and implications for Carboniferous sedimentation patterns"

COMMENT

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Johnsson (1986) presented evidence that clay-mineral diagenetic and other data indicate a west-to-east increase in paleotemperature in the Middle Devonian Tioga ash bed (metabentonite) across part of central New York State. However, his conclusion that a paleotemperature increase implies an easterly increase in depth of burial by inferred Carboniferous sediments is unconvincing.

Johnsson's (1986, Fig. 2, p. 385) composite graph of illite crystallinity, apparent apatite fission-track ages, and Upper Devonian paleothickness data indicates some relationship among the three variables. The easternmost point (WT1) shown in the graph, however, has an illite crystallinity of 90%, which is the same value as point WT8 near Syracuse. Point WT8 is about 170 km to the west and is overlain by about 610 m (2000 ft) more of Upper Devonian strata. This value indicates a negative correlation between illite crystallinity and Upper Devonian paleothickness for the easternmost 100 km of Johnsson's 375-km transect across part of central New York State. Thus, the illite crystallinity data imply higher temperatures to the west between points WT1 and WT8, and these temperatures are not related to Upper Devonian paleothickness. In addition, the apparent fission-track ages for the ash bed (metabentonite) decrease westward from WT1 to WT8. According to Johnsson, this decrease indicates that the ash bed for the western points, up to and including WT8, were the last to cool to 100 °C. However, Johnsson's (1986) error bars indicate no significant differences for these points. According to him, the data imply a greater paleothickness of Carboniferous sediments near Syracuse, in spite of an eastward-thickening Upper Devonian sequence. This increased paleothickness implies a shift in the depocenter during Carboniferous time. Unfortunately, Carboniferous strata in this area of New York are absent (Edmonds et al., 1979), so the causes of paleotemperatures are uncertain.

Diagenesis and metamorphism are related to paleothickness of sediments (Epstein et al., 1977). However, other variables exist, including structure and tectonism, that must be considered in an analysis of paleotemperatures.

Southwest of New York State, in Pennsylvania, West Virginia, and Maryland, a west-to-east increase occurs in the rank of the coal from high-volatile A bituminous coal near the West Virginia–Maryland state line to anthracite in the Valley and Ridge province. The Devonian and

Pennsylvanian strata were affected by the same event that caused the eastward progression in degree of metamorphism. This progression implies that the diagenesis or metamorphism is post-Pennsylvanian in age.

In the Anthracite region of eastern Pennsylvania, disagreement exists about whether the west-to-east increase in rank from semianthracite to anthracite in Pennsylvanian strata is due to depth of burial by sediments, increasing structural complexity, or tectonic burial. White (1925) and Wood et al. (1969) presented data indicating that the fixed carbon of coal increases eastward toward the cores of the more deformed synclines and toward thrust faults. This increase is in the direction of the regional plunge and in the direction of greater depth of burial. However, Levine and Davis (1983) used coal reflectance anisotropy and concluded that the eastward increase of metamorphism of the coal is due to increasing depth of burial by sediments. Levine (1986) suggested that tectonic burial probably is necessary to explain the 200–250 °C paleotemperatures in the western part of the Anthracite region. Clearly, the parallelism of isocarb lines to structure in the Anthracite region indicates that the coalification patterns were affected by structural deformation (Wood et al., 1969).

Compared to Pennsylvanian coal, burial by overlying sediments in some parts of the Valley and Ridge province of eastern Pennsylvania, northern West Virginia, and western Maryland does not indicate an increased degree of coalification of Lower Mississippian coal. In addition, in the Pennsylvanian of Maryland (Swartz and Baker, 1922; Lyons and Jacobsen, 1981; Lyons et al., 1985), a west-to-east increase in coal metamorphism (diagenesis) is in the direction of increasing structural deformation over a distance of about 50 km. The rank increases from high-volatile A bituminous coal near the West Virginia–Maryland state line in the Appalachian Plateau province to low-volatile bituminous coal in the Georges Creek basin at Frostburg, Maryland, just west of the Allegheny front. East of the Allegheny front and in the more structurally deformed Valley and Ridge province, the coal is anthracite.

In the Castleman coal field, the next synclinal basin west of the Georges Creek basin, whole-coal chemical data indicate that fixed carbon increases (that is, increasing degree of coalification) toward the synclinal axis and in the direction of plunge. This direction of increase appears to be true for the Georges Creek basin also. Thus, the regional west-to-east progression in degree of coalification is interrupted locally by geologic structure. Whether this increase is due to increased fracturing and devolatization along the synclinal axis, increased heat, or both is not clear.

In summary, no comprehensive explanation exists for the west-to-east increase in metamorphism in the northern Appalachian basin. Clearly, depth of burial alone cannot explain the coalification patterns. The data in his report do not support Johnsson's (1986) conclusion that heating due to burial by Carboniferous sediments is an adequate explanation for the Middle Devonian paleotemperatures across part of central New York State. His samples near Syracuse are within a zone of cleavage, jointing, and decollement faulting related to Alleghenian orogenesis (Geiser and Engelder, 1983). Structure and tectonism should be considered in an analysis of paleotemperatures as well as plutonism and depth of burial by sediments.

REPLY

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The Comment of Lyons provides an opportunity to clarify several points that were perhaps not sufficiently stressed in my original paper. I also thank him for summarizing a literature that is quite germane to the problem of Appalachian Basin thermal history.

Several misconceptions in the Comment should be corrected. First, I evaluated the thermal maturity of the Tioga metabentonite samples by determining the level of illitization of a mixed-layer illite/smectite phase and not, as Lyons states, by observing variations in illite crystallinity. Indeed, illite was absent or rare in the Tioga samples (Johnsson, 1986, Table 1). Measurements of illite crystallinity are taken to represent the thickness of illite crystallites perpendicular to *c*, and they involve the determination of the width, on an X-ray diffraction profile, of a particular illite reflection (usually the 002 or 003) at half-height. In contrast, the illitization of a mixed-layer illite/smectite (a distinct mineral species) represents the progressive replacement of smectite-like interlayers for illite-like interlayers, and may be measured by qualitative changes in the topology of the X-ray diffraction pattern (see Reynolds and Hower, 1970). Although both illite crystallinity and illitization of the mixed-layer phase tend to increase with diagenesis and metamorphism, the two phenomena are quite distinct. Second, Lyons has missed the main point to be drawn from my Figure 2 (Johnsson, 1986, p. 585). Although Upper Devonian sediment thickness increases fairly steadily from west to east, thermal maturity and apatite fission-track cooling age do not. Whereas samples WT8 and WT1 exhibit very similar cooling ages and levels of thermal maturity, sample WT8 was buried by Upper Devonian sediments to a much shallower (not deeper, as Lyons states) depth than was WT1. This indicates a thermal anomaly, roughly centered on Syracuse, which cannot be explained by a uniform geothermal gradient and burial of the Middle Devonian Tioga metabentonite beneath Upper Devonian sediments alone. It should be noted that this anomaly is superimposed on the unexpectedly high burial temperatures already noted from eastern New York (Friedman and Sanders, 1982; Lakatos and Miller, 1983). In my paper, I argued that these high background temperatures are the results of burial beneath thick Carboniferous sequences and that the thermal anomaly near Syracuse was caused either by a basinward shift in the local depocenter with the onset of Carboniferous sedimentation or by the presence of a locally elevated geothermal gradient near Syracuse during the time of maximum burial.

Lyons, by drawing an analogy with the coal belts of Pennsylvania, Maryland, and West Virginia, suggests that tectonic burial may have been an important process in shaping the thermal history of the sedimentary sequence I examined. As Lyons points out, areas of high-grade coal—particularly the Anthracite region of Pennsylvania—are regions characterized not only by great sediment thicknesses, but also by great structural complexity. In regions of high-amplitude folds and abundant thrusting, sedimentary sequences may have been significantly thickened by tectonic processes, and the question of the relative importance of sedimentation vs. tectonic burial in producing the indicated high burial temperatures is an important one.

It is puzzling, however, that Lyons should draw an analogy between these regions and the Allegheny Plateau in New York. Western New York State, unlike the Valley and Ridge province of Pennsylvania and Maryland, contains neither high-amplitude folds nor large thrust faults. The only large-scale folds in the region are the broad open warps, first described by Wedel (1932), in the southern part of the region. These folds all have amplitudes of less than 300 m (Rodgers, 1970) and could not have significantly increased paleotemperatures above those based on sedimentation alone. Large-scale faulting in western New York State is apparently confined to the Clarendon-Linden fault system between Buffalo and Rochester (Chadwick, 1920; Hutchinson et al., 1979), a high-angle system of modest throw. No evidence of large-scale thrusting is observed anywhere in the region (Rodgers, 1970).

Large parts of the Allegheny Plateau in New York and Pennsylvania are, however, characterized by 10% or more layer-parallel shortening (LPS) (Engelder and Engelder, 1977; Engelder, 1979; Geiser and Engelder, 1983). Contrary to what Lyons implies in his concluding paragraph, jointing, cleavage, and decollement faulting associated with LPS are not restricted to the region of the thermal anomaly near Syracuse. Within the Onondaga Limestone, LPS fabrics are developed everywhere between Syracuse and Buffalo (Engelder, 1979), and the modest westward decrease in maximum compressive strain (Engelder, 1979) is insufficient to account for the thermal anomaly.

Tectonic burial is undeniably an important process during diagenesis and metamorphism in structurally dynamic regions, and Lyons is quite correct in demanding that structure and tectonics be considered in an analysis of paleotemperatures in sedimentary basins. The Allegheny Plateau in western New York State was, however, too far removed from the Allegheny orogen for the development of the structure necessary to produce such burial.

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Comment and Reply on "Geometric test for Late Cretaceous-Paleogene intracontinental transform faulting in the Canadian Cordillera"

COMMENT

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Price and Carmichael (1986) addressed a question that we considered in detail in Chamberlain and Lambert (1985). In their contribution, however, Price and Carmichael appear to have misunderstood some aspects of our model. Their comments are similar to those we have received verbally from others, suggesting, perhaps, a general misapprehension. In our model, Cordilleria first amalgamates, then travels toward North America by the oblique subduction of an intervening portion of oceanic lithosphere and collides with North America in the Yukon area only. It is then constrained to move along a transcurrent fault in the Yukon, while at the same time the westward movement in the south is taken up by continuing oblique subduction. Finally, oblique collision occurs in southern Canada as the North American plate continues to move westward, combined with the counterclockwise rotation and slowing northward movement of Cordilleria. This causes the successive underthrusting of the more easterly parts of the North American miogeoclinal supercrustals, thus forming the Canadian Rockies. At the same time the Cordilleria supercrustals become detached from their basement and are thrust over the suture.

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It seems to us that Price and Carmichael (1986) have essentially restated a part of our model (Chamberlain and Lambert, 1985) for the origin of the Canadian Rockies. Their "transformation of strike slip into oblique convergence" is, in effect, a reiteration of part of our caption for Figure 5 which "shows that the dextral transcurrent movement (on the Tintina Fault) would have been accompanied by an anticlockwise rotation of Cordilleria, thereby causing convergence in the Canadian Rocky Mountain region." We further expanded on this concept in the sixth paragraph of our section entitled Cretaceous Subduction, where we stated that "Cordilleria was constrained to move northwestward and rotate anticlockwise . . . (which) would rotate the southern end of the microcontinent over the craton, pushing . . . thrust sheets in front of it (Fig. 6b) . . . produc[ing] maximum (~200 km) overthrusting at the southern end of the Canadian Rockies and minimum (~50 km) overthrusting at the northern end." Hence, their "elegantly simple" solution has many similarities to ours. However, our model explains many of the other puzzling features related to the formation of the Canadian Rockies, as we stated in 1985: "Subduction, besides taking up much of the palaeomagnetically required motion, would also provide a mechanism for Cretaceous (as opposed to Jurassic) thrusting and an accommodation at depth for the ~200 km of crustal shortening apparent in the southern Canadian Rocky Mountain thrust and fold belt." Our model also has the advantage of explaining numerous other features, notably the Mackenzie Mountains, the Lewis and Clark line, and the Idaho batholith, whereas the Price and Carmichael model seems to be oversimplified. Their proposed rotation movement along the Tintina–northern Rocky Mountain trench (NRMT) faults, by definition, must be tangential to this fault line at any point along it. Thus, it is essentially parallel to the Rocky Mountain thrust faults and, having no component toward the craton, cannot be invoked to have generated those thrusts, especially as along-strike movement in Rocky Mountain thrusts is insignificant, as noted by Price and Carmichael (1986).

Price and Carmichael's critical comments related to strike-slip movement on the southern Rocky Mountain trench (SMRT) appear to be due to a misreading of our paper (Chamberlain and Lambert, 1985). We did not, as they stated, "tacitly ignore the many published arguments supporting the conclusion that there has been no significant right-hand strike slip along the southern Rocky Mountain trench or in the adjacent Rocky Mountains." We did *not* advocate strike-slip movement along the SRMT. We showed that the Malton Gneiss region was no longer to be regarded as a pin across SRMT but nevertheless explained in detail how the emplacement of Cordilleria and the Belt-Purcell Supergroup may be achieved without recourse to any transcurrent movement along SRMT. Our published comments concerning SRMT include the following: "Its surface expression seems to result from thrust and dip-slip faulting"; "researchers who have studied the SRMT . . . have failed to uncover . . . significant differences across it"; "there is little evidence for transcurrent movement on the SRMT and no evidence of transcurrent faulting to the south"; "the relative motion . . . is taken up . . . by oblique subduction"; "the remaining northward movement . . . taken up by transcurrent movement in the north along the Tintina fault, and by oblique subduction and thrusting in the south"; and "SRMT . . . marks a line of weakness above a Cretaceous suture, now largely covered by the thrust sheets of the Rockies."

Price and Carmichael's other objection, which concerned geophysical boundaries, also seems to be based on a misconception. In 1985 we described the lithospheric suture as dipping at a shallow angle to the west, and we stated that the geophysical boundary may be as much as 100 km from the suture in the cover. Hence, if the gravity and magnetic anomalies of Kanasewich et al. (1969) are truly in the basement, as suggested by Price and Carmichael (1986), then it certainly fits our model to have North American basement below the thrust sheets at Kimberley. However, it is equally possible that the Kimberley magnetic anomalies (Kanasewich et al. showed no continuous gravity anomalies) are due to concentrations of Purcell diabase intrusives or an unexposed pluton in the cover rocks and are not basement anomalies at all. The most recent magnetic anomaly maps of the Earth Physics Branch (7684G and 7685G, 1973) show a definite break in the anomalies between Cordilleria and North America from lat 49° to 50°N, and this geophysical boundary was emphasized by Monger and Price (1979).

REPLY

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The Comment by Chamberlain and Lambert provides a welcome opportunity to elaborate on the nature and tectonic significance of the results of our geometric test for Late Cretaceous–Paleocene right-hand strike slip on the Tintina–northern Rocky Mountain trench (TT-NRMT) and Fraser River–Straight Creek fault zones.

Chamberlain and Lambert appear to have missed the main point made in our paper—that the 450 km of right-hand strike separation of mid-Cretaceous and older rocks along the small-circle trace of the TT-NRMT fault zone establishes the upper limit for the amount of displacement, relative to cratonic North America, of the intermontane terrane (Terrane I). This conclusion is incompatible with the tectonic interpretations of paleomagnetic data presented by Monger and Irving (1980), Irving et al. (1985), and Armstrong et al. (1985), according to which there has been more than 1000 km, and up to 2400 km, of northward displacement since mid-Cretaceous time of the intermontane terrane relative to cratonic North America. Chamberlain and Lambert have not challenged our main conclusion in their Comment on our paper, although it contradicts the basic premise upon which their model (Chamberlain and Lambert, 1985) is based—that the interpretation of the paleomagnetic measurements documents large (1000–2400 km) displacements of the intermontane terrane relative to cratonic North America since mid-Cretaceous time.

Chamberlain and Lambert (1985), having adopted the premise that the intermontane terrane has been displaced 1000–2400 km northward since mid-Cretaceous time relative to cratonic North America, speculated on the existence of another far-traveled terrane (their "Terrane III") lying between the intermontane terrane and North America and consisting of rocks that most, if not all, previous workers had considered as composing part of the parautochthonous supracrustal sedimentary cover of the North American craton. Although they do not address the topic directly, implicit in their speculation on the existence of Terrane III is the question of the location of the suture between Terrane III and the adjacent parauthochthonous North American rocks. In their (Chamberlain and Lambert, 1985) Figure 1, the eastern limit of Terrane III (and therefore the locus of the suture separating the far-traveled rocks from the North American rocks) follows the TT-NRMT (except for one small northeastward excursion in the Yukon) and the southern Rocky Mountain trench (SRMT), except in southeastern British Columbia where it appears to lie in the Rocky Mountains, east of the trench. This is the basis for our statement (Price and Carmichael, 1986, p. 470) that "speculation by Chamberlain and Lambert (1985) . . . tacitly ignores the many published arguments supporting the conclusion that there has been no significant right-hand strike slip along the southern Rocky Mountain trench or in the adjacent Rocky Mountains." Although Chamberlain and Lambert (1985) may have been unaware of the implications of their speculations, including what they portrayed in their Figure 1, we have no choice but to stand behind the one statement we made concerning their paper. Transverse, northeast-trending faults and thickness and facies changes in both Proterozoic and Paleozoic rocks have been correlated in detail from the Purcell Mountains, across the southern Rocky Mountains and into the Front Ranges of the Rockies (Price and Carmichael, 1986, and references cited therein). These detailed correlations, based on systematic regional geologic mapping in southeastern British Columbia and southwestern Alberta, preclude any large northerly translation, relative to cratonic North America, of the rocks in southern Canada and the northern United States that Chamberlain and Lambert (1985, Figs. 1 and 3) have identified as composing the main mass of their hypothetical far-traveled Terrane III. Accordingly, they negated the basic premise upon which the existence of the far-traveled composite terrane "Cordilleria" (Chamberlain and Lambert, 1985) is based, and we can only conclude that "Cordilleria" does not exist.

Contrary to what is implied by Chamberlain and Lambert in their Comment, the idea that strike slip on the TT-NRMT was transformed southward into oblique convergence in the southern Rocky Mountains did not originate in their (Chamberlain and Lambert, 1985) paper. It was introduced by Monger and Price (1979, p. 777) and has been developed in some detail by Price (1982a, 1982b, 1983), notably as an explanation for the northward decrease in Late Cretaceous–Paleocene subsidence of the Rocky Mountain foreland basin. Thus, one apparent similarity between our interpretation and theirs is rooted in an idea published previously by one of us; moreover, the assertion in their Comment that the "rotation movement along the Tintina-NRMT faults . . . is essentially parallel to the Rocky Mountain thrust faults and, having no component toward the craton, cannot be invoked to have generated those thrusts . . ." completely misses the essence of our geometric analysis, which is that south of lat 56°N, where the TT-NRMT fault system diverges southeastward from the southwesterly concave circular arc that characterizes the rest of its trace, there is a component of compression (horizontal shortening) normal to the fault as well as the component of right-hand strike slip parallel with the fault, and it is this compression that is expressed as thrust faulting in the Rocky Mountains.

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Comment and Reply on "Extension across the Eocene Okanagan crustal shear in southern British Columbia"

COMMENT

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Tempelman-Kluit and Parkinson (1986) have added to the understanding of the geology of the Okanagan Valley region by determining the sense and timing of displacement along a mylonite zone previously identified by Ross (1981). In my opinion, however, it is a pity that they chose to focus on one poorly supported estimate of the magnitude of displacement on this zone instead of discussing a range of models with displacements less than but as probable as the 80–100 km suggested. Acknowledgment of relevant previous and current research would also have been appropriate.

Most figures have minor missing, inaccurate, and illegible parts, but Figure 5 is geologically misleading. Numerous features affecting the interpretations are not shown or lie beyond the area illustrated. At the north end of Okanagan Lake, a belt of Carboniferous–Triassic strata (Fig. 1; brick pattern) of identical metamorphic grade and structural style east and west of the lake lies across possible northward extensions of the "Okanagan crustal shear" (Jones, 1959; Okulitch, 1979). Between the south end of Okanagan Lake and the 49th parallel, Permian–Carboniferous strata of the Anarchist and Kobau groups (Fig. 1; brick pattern), which crop out for over 50 km to the west and east of the Okanagan crustal shear (OCS), are of similar grade throughout the entire region (except within contact aureoles; Okulitch, 1973). In the hanging wall of the OCS, at least some of this metamorphism is Jurassic, and some possibly Permian–Triassic (Read and Okulitch, 1977). Restoration of the hanging wall over the footwall implies metamorphism of like ages in the latter. At these latitudes there is no demonstrable major omission of metamorphic zones, and dip slip in excess of 20 km is unlikely.

None of the small Coryell syenite plugs (Fig. 1; black) which crop out in the area near Penticton and Kelowna were shown. Several lie within 30 km to the east and *west* of the Marron volcanics (Fig. 1; "pillow" pattern) (Little, 1961). These are as probable subvolcanic roots as any of the more distant plugs illustrated by Tempelman-Kluit and Parkinson (1986). No estimate of dip slip can be made at present using these features.

Correlation of largely undated granitoid suites intruded over a 100-m.y. interval and with vaguely defined centers is entirely without support by available data. No detailed comparative studies of the petrology, geneses, or ages of these numerous and complex plutons have been made.

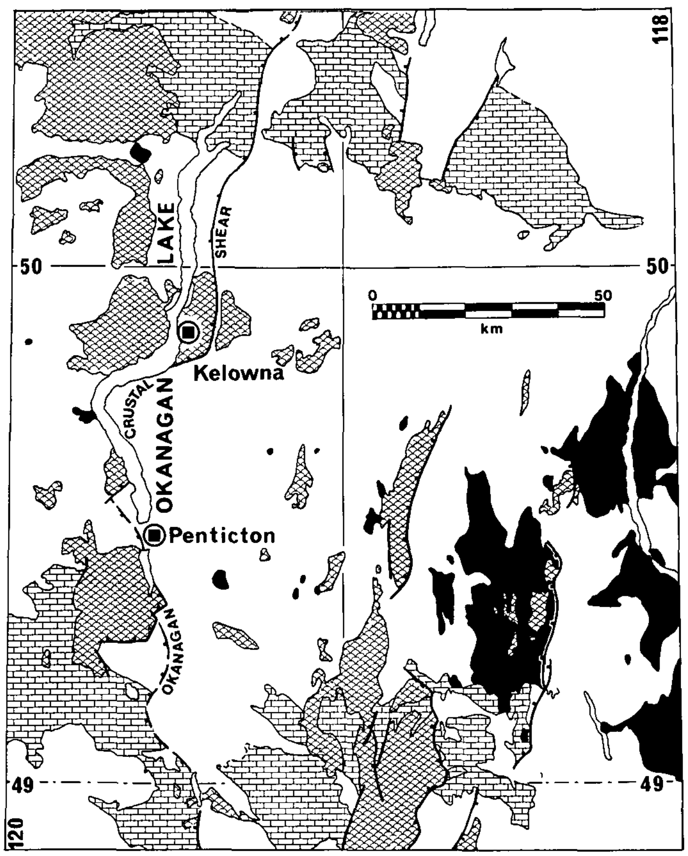

Figure 1. Paleozoic and Eocene geologic units near and east of the Okanagan Valley.

None of the Marron volcanics cropping out in the vicinity of the plugs illustrated were shown. Did these volcanics slide from some other still more distant plugs, or is it more likely that they merely lie close to their subvolcanic roots? In any case, a match of specific volcanics and plugs cannot be made.

Tempelman-Kluit and Parkinson (1986, Fig. 3) equated Monashee "Group" gneisses with the OCS, a serious error that ignores a polyphase deformational, metamorphic, and plutonic history (Christie, 1973; Ryan, 1973; Medford, 1975; Ross and Christie, 1979; Ross, 1981) that may have extended from the Permian–Triassic to the Tertiary. At least some mylonite zones near the Okanagan Valley are likely coeval with phase 2 folding (Ross, 1981). Tracing of continuous mappable units and mylonite zones deformed by recumbent phase-3 folds throughout much of the structural thickness of the gneisses argues against them having formed only within a thick Eocene crustal shear. The presence of gneissic cobbles in conglomerate basal to Eocene flows (P. B. Read, 1985, personal commun.; Bardoux, 1986) supports this contention. In the south-central part of the area in Figure 1, gneissic cobbles also occur in Triassic units, testifying to a prolonged tectonic history. The OCS may be but one of several discrete crustal shears formed at various structural levels and at different times.

If the earlier history of the gneisses is not distinguished from Tertiary extension, the significance of laterally varying grades of polyphase regional and contact metamorphism is missed. The complexity of the metamorphic pattern means that the observed grade cannot be simplistically used as an estimate of Eocene tectonic unroofing. Erosional unroofing prior to and after the Eocene must be included.

The above facts strongly suggest that significant displacement on the OCS is restricted to the 120-km segment along Okanagan Lake and is substantially less than 80–100 km. The amount of displacement remains to be documented by reliable criteria. Careful mapping, petrographic studies, isotopic age determinations, and integration of all data into closely constrained restored cross sections will be necessary to provide such criteria. Recent studies in the extensional terranes of the Great Basin are excellent examples of the work needed.

REPLY

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Our paper (Tempelman-Kluit and Parkinson [1986]) aimed to show how a broad range of existing data can be interpreted in a new way, namely in the light of crustal shear models (Wernicke, 1985). We agree that the story of the Okanagan shear zone is probably more complicated than we portray it—ours is a first pass—but we are heartened that Okulitch agrees with the elements of our interpretation and only questions specifics, such as the amount of displacement.

Contrary to Okulitch's Comment, we did not "focus on one poorly supported estimate of magnitude" but gave three lines of reasoning which suggest displacements of 60, 80, and 90 km of slip, and we specifically stated the assumptions for each. We agree that these estimates await tighter control, but we disagree that "closely constrained restored cross sections" are likely to answer this problem—rocks in the region are largely granitic and lack the stratigraphic detail needed to construct such sections.

We apologize for the mislabeled units in the legend of Figure 1; specifically, the patterns for the Middle Jurassic and Eocene granodiorite of the lower plate are interchanged. We disagree that Figure 5 is geologically misleading—it focuses on two of our three displacement criteria.

We equate only the rocks along the eastern Okanagan Valley, which Little (1961) mapped as Monashee Group, with the Okanagan crustal shear (OCS); we do not imply, and did not state, that the Monashee Group elsewhere is part of the OCS.

We agree that the Great Basin has some analogues to the OCS, but the former exposes an extended upper plate—the Okanagan shows us a shear zone in which displacement on upper-plate faults is gathered. Nevertheless, some Great Basin studies are models of the kind of work required as a next step; we feel such work is already well underway (Bardoux, 1985, 1986; Carr, 1985; Parrish, 1985; Parrish et al., 1985a, 1985b; Parrish, 1986a, 1986b), and we are encouraged that it supports our basic premise of large extension across a fault along the Okanagan Valley during the Eocene.

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