FORUM

Comments and Replies on "Model for the origin of the Yakutat block, an accreting terrane in the northern Gulf of Alaska"

COMMENT

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Bruns (1983a) has proposed a model that requires the Mesozoic part of the Yakutat block to have been plucked out of the central California margin during the early Eocene and moved 30° northward along the Pacific margin to its present position in the Gulf of Alaska. During its travels, the Mesozoic sequence and adjacent oceanic Eocene basement of the Yakutat block were covered by the thick, continuous sequence of bedded Cenozoic rocks that characterizes the block. The primary reason for generating Bruns's model is to account for marine seismic-reflection data acquired along the Transition fault zone that defines the southern margin of the Yakutat block. As interpreted, the reflection data show no evidence for major deformation, accretion, or subduction, leading to the conclusion that there was "little net convergence between the Pacific plate and the Yakutat block prior to the Pliocene" (Bruns, 1983a, p. 719) and none since the Pliocene. This leads to the need for a model that permits only pre-Pliocene strike-slip displacement between the Yakutat block and the adjacent ocean floor.

Major questions concerning the basic premises of Bruns's model are the following: (1) How can lack of deformation or an accretionary wedge be cited as evidence for absence of convergence along the Transition fault zone when such features are not present on seismic-reflection records in many known circum-Pacific convergent margins? An example is a recent publication on the convergent eastern end of the Aleutian Arc, coauthored by Bruns (Plafker et al., 1982), in which multichannel seismic data are interpreted as showing subduction of oceanic crust overlain by 3 km of absolutely undeformed sediment and without an accretionary wedge on the trench inner wall. (2) How can the seismic-reflection data be reconciled with recent earthquake mechanisms near the southern end of the Transition fault zone, indicating active oblique underthrusting? (3) Why aren't there any sea-floor magnetic anomalies on the western Yakutat block perpendicular to the Transition fault zone, as is required by the model? (4) The interpretation precluding Pliocene and younger displacement along the Transition fault zone hinges on the age of apparently undeformed slope-rise deposits across the southern part of the fault zone. However, the oldest material dredged from this sequence is Quaternary, and there is no direct evidence that pre-Quaternary strata are present.

Serious problems exist with virtually all the lines of published data used by Bruns to support his model. For example:

1. Bruns (1983a) cited paleomagnetic data for layered gabbro in the Chugach terrane as suggesting that the Yakutat block was originally part of a larger terrane, pieces of which preceded the block northward to Alaska. The paleomagnetic data indicate about 25° northward displacement, and the most likely age for the layered gabbro is 28 ± 8 Ma (Loney and Himmelberg, 1983). Thus, if the paleomagnetic and age data are both valid, the model requires astonishing plate movements whereby the extensive Chugach terrane was off central California when the Yakutat block was off Washington in the late Oligocene, yet the Chugach terrane somehow ended up north of the Yakutat block!

2. Bruns (1983a, p. 719) cited published data on sandstone petrology as indicating that "onshore Mesozoic sandstones of the Yakutat block have a very different source area from coeval sandstones north of the block and may have undergone substantial tectonic transport." These slightly volcanogenic arkosic sandstones are in a flysch and melange sequence that has gone through a Late Cretaceous to early Eocene low-P–high-T metamorphic and intrusive history similar to coeval rocks along the British Columbia and southeastern Alaska margin, but markedly different from the metamorphism that characterizes coeval rocks of California and Oregon. Furthermore, olistostromal blocks in the Mesozoic melange facies require that the sequence be adjacent to a Wrangellian source terrane during Late Jurassic to Early Cretaceous time, such as the terrane that underlies the continental margin of British Columbia and southeastern Alaska.

3. Bruns (1983a, p. 719) cited published data as determining that "the source area for the offshore Paleogene strata of the Yakutat block could not have been in the adjacent mountains." These Paleogene rocks were derived from a remarkably homogeneous and distinctive crystalline plutonic and high-grade metamorphic source terrane, whereas the proposed Paleogene trajectory of the block according to Bruns's model has it adjacent to a variety of source terranes—largely of the wrong lithology. A potential source terrane with the proper lithology, uplift history, and volume for the Paleogene sediments that has been identified in the Coast Crystalline Complex of British Columbia and southeastern Alaska (Hollister, 1979) was not cited in Bruns's paper. Among the more startling implications of Bruns's reconstruction are that during the Eocene, it would have been necessary for coal-forming swamps to have extended along the northern margin of the Yakutat block for a distance of at least 450 km west of the northern California and Oregon continental margin and that thick Paleogene subaerial and shallow marine conglomerate and sandstone somehow bypassed continental margin basins to be deposited over the entire Yakutat block as it worked its way northward.

A model for the evolution of the Yakutat block that explains all of the first-order geologic features and requires about 5°, rather than 30°, northward displacement has been proposed previously (Plafker, 1983). It postulates that the Yakutat block was sliced off the margin of southeastern Alaska and British Columbia and moved to its present position by about 550 km of dextral slip along the Queen Charlotte–Fairweather transform since about early Miocene time. Attempts to test the displacement history with paleomagnetic studies have thus far been unsuccessful due to a lack of suitable igneous rocks and absence of remanent magnetization in the sampled sedimentary rocks on the Yakutat block.

REPLY

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Plafker questions the Bruns (1983a) model that requires 30° of northward motion of the Yakutat block during the Cenozoic, and he proposes instead a model with about 5° of northward motion (Plafker, 1983). While both models leave some questions unanswered, Plafker's model is inconsistent with at least two first-order geologic features: (1) the observed lack of deformation within the Yakutat block or along the southern boundary at the Transition fault, and (2) the presence of low- and middle-latitude microfaunal assemblages from the Paleogene strata of the block. Plafker's model requires substantial subduction of Pacific plate beneath the Yakutat block and about 45° of rotation of the block during emplacement; presumably, such events would be accompanied by deformation of the block and of strata covering the Transition fault. The Yakutat block microfaunal assemblages correlate to coeval onshore sections now at 30° ± 5° for 50 Ma, to 40° ± 5° for 44–40 Ma, and to 45° ± 5° for 40–36 Ma (Keller et al., 1984). These data are consistent with my model, which allows the block to migrate 30° northward with the Pacific and Kula plates during the past 50 Ma.

Plafker has four principal objections to the Bruns (1983a) model:

1. He concludes that an absence of deformation or an accretionary wedge is not evidence for an absence of convergence. I conclude that there has not been convergence or subduction of the Pacific plate below the Yakutat block during at least Pliocene and more recent time. My conclusion is based on seismic stratigraphic mapping on a grid of multichannel seismic-reflection records. Ages of seismic units on the Yakutat block are based on dredge samples and exploratory wells, and on the Pacific plate they are based on a seismic tie to Deep Sea Drilling Project hole 178 near Kodiak. These studies indicate that deformation and historic seismicity along the Transition fault are related to Pliocene and more recent local uplift of Fairweather Ground. Both east and west of Fairweather Ground, the Transition fault is covered by 0.7 to 2 km of undeformed Pliocene and younger strata (Bruns, 1983b).

For pre-Pliocene time, subduction cannot be ruled out, but it seems unlikely given the lack of major deformation or accretion along the block. Along most convergent margins where an accretionary wedge is small or absent, either tectonic erosion is suspected or the subducting plate is sediment starved. In contrast, convergent margins with thick sediment on the subducting plate characteristically have a well-developed accretionary wedge. Sediment input into a Transition fault subduction zone would have been large, as the Pacific plate was adjacent to the North American continental margin during its northward travel. Therefore, the fault would probably be marked by a well-developed, seismically visible accretionary wedge. Even in failed subduction zones, the accretionary wedge is commonly preserved, as in the Palawan Trench, the eastern Luzon Trench, the Bering Sea margin, and the central California margin. The Pacific plate adjacent to the Transition fault is instead overlain by as much as 6 km of Miocene and younger, undeformed strata (Bruns, 1983b). Therefore, pre-Pliocene subduction at the fault seems unlikely.

Plafker cites a seismic line (line 425) at the east end of the Aleutian trench as showing subduction without deformation (Plafker et al., 1982). This line is markedly anomalous, as indicated by adjacent lines 30 km to the northeast and 60 km to the southwest which show considerable deformation and accretion. The structural variability within the 30- to 60-km spacing of the seismic lines suggests that line 425 is not representative of the Aleutian convergent margin, nor can structure observed on the line be extrapolated to infer that the 500-km-long Yakutat block margin was a nonaccretionary, nondeforming subduction zone for the Cenozoic.

Plafker asks why sea-floor magnetic anomalies are not observed on the Yakutat block. Detection of such anomalies would be difficult, because of masking by the high-amplitude Slope anomaly (Schwab et al., 1980), and attenuation by thick (at least 9 km) Cenozoic strata overlying the magnetic basement (Bruns, 1983b; Bruns and Schwab, 1983).

2. The paleomagnetic and age data cited by Plafker cannot both be correct, as the required northward movement rate would be almost double the Pacific plate rate. This problem is independent of the Yakutat block, because the rocks involved are not on the bloc. I cited these data only to indicate the potential for more general applicability of the model—rifting and incorporation into the oceanic plate of a continental-margin fragment with subduction of a spreading center.

3. I am not aware of publications on the cited Mesozoic sandstones which discuss either the low-P–high-T history of the rocks, or the origin of olistostromal blocks derived from a Wrangellian source terrane. I note, however, that subduction of a spreading center would include a low-P–high-T metamorphic phase. I also note that southern Alaska is composed of remarkably narrow terranes. Could not the Mesozoic rocks compose another such slice, with the suture between the Yakutat block and the Mesozoic rocks concealed beneath the thick late Miocene and younger strata that cover the block? The Yakutat block could have amalgamated with such a slice either during its northward travel or during collision with southern Alaska.

4. The source terrain for the Yakutat block Paleogene strata is indeed a problem; the sandstone lithologies reported by Plafker et al. (1980) for the Yakutat rocks are not well matched to lithologies of Oregon and Washington. However, Plafker's conclusion (Comment above) that the offshore Yakutat Paleogene strata were derived from "a remarkably homogeneous and distinctive . . . source terrane" is based on modal analysis of only ten dredge samples from a poorly controlled position within the Yakutat block stratigraphic section by Plafker et al. (1980); they also noted that "in several places, glauconitic tuffaceous sandstones . . . were dredged from the continental slope"; modal analyses were not done of these "volcaniclastic" sandstones. Thus, on the basis of the few samples studied, it is perhaps premature to restrict the source terrain for a section up to 5 km thick to a single area.

An alternative possibility is that during the late Eocene and Oligocene, the Yakutat block was extensive enough to receive sediment from Plafker's preferred source terrain. About 120 km of subducted Yakutat block may be present in a recently recognized Benioff zone that underlies southern Alaska (Stephens et al., 1983); additional parts of the block may lie in the Aleutian subducted slab. Drainages may have connected across now-subducted parts of the block to the Coast Crystalline Complex of British Columbia.

The principal difference between the models is that the Bruns (1983a) model accounts for the large northward displacement required by microfaunal assemblages from the Yakutat block, whereas the Plafker (1983) model, which suggests considerably less northward displacement, places the block adjacent to a possible source terrain for the Yakutat Paleogene strata. Perhaps, as suggested by Plafker, paleomagnetic data will eventually yield a paleolatitude, but until then the paleolatitudes indicated by the microfauna appear to be more compelling than a possible source location for the Paleogene strata.

COMMENT

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The Bruns (1983a) model for major northward movement of the Yakutat block since the early Eocene is not supported by foraminiferal or molluscan evidence and is strongly contradicted by paleobotanical data. The warmth indicated by the Yakutat floras and faunas is largely a reflection of generally high temperatures evidenced by analysis of other Eocene floras and faunas.

Foraminiferal faunas. Planktic foraminifers, although apparently lacking in onshore Yakutat sections because of extensive deformation, are present in dredge samples from offshore sites (Poore and Bukry, 1979; Keller et al., 1984). The late early Eocene samples have abundant *Acarinina primitiva* and *Subbotina linaperta.* In discussing subantarctic Deep Sea Drilling Project (DSDP) early to middle Eocene samples, Kennett (1978, p. 312) noted that "*Subbotina* and *Acarinina* are the dominant forms, with *Acarinina primitiva* perhaps the most dominant species at highest latitudes. . . ." *Acarinina primitiva* generally decreases in abundance equatorward; for example, the taxon is represented by only "numerous" specimens at middle-latitude sites, as in the Roseburg Formation of southwestern Oregon (Miles, 1981, p. 97, as *Truncorotaloides primitivus*), and although present at low-latitude DSDP Site 363, is not sufficiently abundant to be included in Keller's (1983b, p. 469) diagram of "dominant" species.

Comparison of the Yakutat late early Eocene planktic faunas to onshore faunas in California is hampered by the fact that the foraminifers in the onshore faunas "are poorly preserved and commonly deformed" (Poore et al., 1977, p. 738). Because of these preservational factors, many of the foraminifers could not be specifically determined, making comparisons to the Yakutat assemblages in terms of abundances and relative diversity of doubtful value.

Benthic foraminifers of late early Eocene age are also unknown in Yakutat onshore sections, but dredge samples of this age contain benthic taxa that "generally suggest cool water.... However ... temperatures were not extreme ..." (Rau, 1979, p. B140). Late middle Eocene benthics are known from both offshore dredge and onshore samples (Rau et al., 1977; Rau, 1979); whereas the onshore samples are of relatively low diversity, the dredge samples are diverse and contain taxa that also occur in middle Eocene benthic faunas from the Pacific Northwest and California (Rau, 1979). If diversity were used as a criterion for placement of the Yakutat block, we would have to draw the absurd conclusion that the Yakutat offshore terrane was at a lower latitude during the middle Eocene than was the Yakutat onshore terrane.

The Yakutat planktic and benthic foraminiferal faunas of early and middle Eocene age are indeed indicative of warm or at least equable conditions. Included in these faunas are generally megathermal planktics (e.g., *Morozovella broadermanni;* Poore and Bukry, 1979) and the possibly reef-dwelling benthic *Amphistegina* (Keller et al., 1984). Durham (1950) recorded the presence of many genera of reef-forming corals in the early and middle Eocene of northwestern Washington (paleolatitude 55°N), although no reefs were found, and he concluded that the 20 °C isotherm was certainly north of this area during the early Eocene. Durham's (1950) conclusion accords well with that of Wolfe and Poore (1982), who noted that the low-latitude planktic assemblages in the Atlantic expanded to 50–55°N during the early Eocene, and with that of Kennett (1978), who suggested a mean annual temperature of 19 °C for early Eocene surface waters at 70°S. Thus, rather than indicating a low latitude for the Yakutat block as inferred by Keller et al. (1984), the totality of planktic data clearly fit a pattern of maximal expansion of warm climates during the early Eocene.

Molluscan faunas. The early and middle Eocene molluscan faunas from the Yakutat block are poorly known. Available data, however, indicate more similarities to coeval faunas in the Pacific Northwest than to coeval faunas in California. In the late early Eocene, the turitellid *Cristispira pugetensis* is known on the Yakutat block at two localities; elsewhere, the taxon has been found in the Puget Sound region (Allison, 1965) but not farther south. Middle Eocene (i.e., "Cowlitz-Tejon") mollusks, although poorly preserved, appear to represent species otherwise present only in the Pacific Northwest (e.g., *Ficopsis cowlitzensis* and *Whitneyella coosensis;* see H. G. Hertlein, cited in Wolfe, 1977, p. 5) or species that occur both in California and the Pacific Northwest. Neither the early nor middle Eocene Yakutat molluscan faunas indicate significant northward movement of the Yakutat block; these faunas are best interpreted as northward extensions of the Eocene Pacific Northwest province.

Climatic inferences. The Yakutat block has plant assemblages of both late early and late middle Eocene age. An assemblage from near Berg Lake (loc. 11157-11160 of Wolfe, 1977) in the lower part of the Kulthieth Formation is associated with molluscan faunas of "Tyee-Umpqua" (or "Domengine") type, most of which are now placed in the late early Eocene (Armentrout, 1981). The Charlotte Ridge assemblage (Kushtaka loc. 9389 of Wolfe, 1977) is stratigraphically well above the Berg Lake but is still below mollusks of "Cowlitz-Tejon" type and is thus probably of late middle Eocene age.

The Berg Lake assemblage has an inferred mean annual temperature of 22 °C (Wolfe, 1980) based on foliar physiognomy. Early Eocene plant assemblages from paleolatitude 35° in the Mississippi embayment have an inferred mean annual temperature of 28 °C (Wolfe, 1978), indicating that Bruns's suggestion of a 30° ± 5°N position for the Yakutat block is invalid. Plafker's (1983) placement of the Yakutat block off southeastern Alaska and the Queen Charlotte Islands (ca. paleolatitude 60°–65°N; cf. Hillhouse and Grommé, 1983, for paleomagnetic data that place Alaska closer to the North Pole than at present), is consistent with paleobotanical and other paleoclimatic inferences.

The Charlotte Ridge assemblage has an inferred mean annual temperature of 18 °C (Wolfe, 1978), in contrast to 21–25 °C for coeval assemblages in the Puget Group at paleolatitude 50°N (Wolfe, 1978) and 27° for the coeval Susanville (California) assemblage at paleolatitude 43°N (Wolfe, 1980). In Bruns's model, the Susanville and Charlotte Ridge assemblages were supposedly at about the same latitude.

Both the Berg Lake and Charlotte Ridge assemblages are dominantly broadleaved evergreen, which could be considered anomalous for a high-latitude position. During the late Paleocene, a somewhat cooler interval than the early Eocene (Wolfe and Poore, 1982), the vegetation on Kupreanof Island in southeastern Alaska (paleolatitude 65°N and inboard of the Queen Charlotte-Fairweather transform fault) was dominantly broadleaved evergreen (Wolfe, 1980). Similarly, vegetation during the early and middle Eocene in southeastern Australia (paleolatitude 55°–60°S) was broadleaved evergreen (Christophel, 1981) and in general physiognomically similar to the early and middle Eocene assemblages of the Yakutat block.

Floristic relations. The Yakutat early Eocene flora comprises approximately 60 species (Wolfe, 1977), of which 15 (25%) occur in the Puget Group of western Washington. The Eocene floras of northern California contain 5 (8%) of the early Eocene Yakutat species. A parallel relation results from floristic analysis of middle Eocene assemblages; of the 28 Yakutat species, 8 (29%) are known in the Eocene of the Pacific Northwest, but only 1 (4%) is known in the Eocene of California. These data strongly indicate that (1) the Yakutat block was not close to California during the early and middle Eocene, and (2), because of the only moderate degree of floristic similarity between the Yakutat and Puget Group assemblages, the Yakutat block was geographically some distance (i.e., north) from the Puget Sound area.

REPLY

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Wolfe and McCoy assert that Bruns's (1983a) model for the northward migration of the Yakutat block is not supported by foraminiferal or molluscan evidence and is contradicted by paleobotanical evidence. Contrary to this claim, both planktonic and benthic foraminiferal data are consistent with Bruns's model and provide temporal constraints on the northward migration of the Yakutat block, Prince William terrane, and Pacific plate between late early Eocene (50 Ma) and late Eocene to early Oligocene (38–34 Ma; Keller et al., 1984). Our foraminiferal evidence is based on detailed study of material from DSDP cores (Sites 192, 183, 178), an exploratory drill hole (Middleton Island Well), dredged rocks from the Gulf of Alaska, and comparison with onshore coeval sediment sequences from California, Oregon, and Washington. We are not aware of a comparable geographic coverage of mollusca or paleobotanical data from well-dated rocks. Wolfe and McCoy have compared our microfossil assemblages with molluscan and floral assemblages that are not coeval, and they try to correlate continental paleoclimates with oceanic paleoenvironments. Even worse, however, is the uncertainty in their dating, which in some cases spans the time during which the Yakutat block would have traveled through one climatic zone and well into another.

Wolfe and McCoy interpret the presence of tropical and subtropical foraminiferal taxa in the Yakutat block samples not as indication of a more southern region of deposition, but rather as indication of "maximal expansion of warm climates during the early Eocene into high latitudes." Unfortunately, they base their conclusion on very imprecise dating. Our late early Eocene date is specific at 50 ± 1 Ma (Zone P9). The early Eocene prior to about 53 Ma had the warmest global marine temperatures in Tertiary time. An abrupt and drastic cooling started at about 53 Ma and continued in a stepwise manner into Oligocene time (Keller, 1983a). The high early Eocene mean annual temperature of Kennett (1978) for high latitudes quoted by Wolfe and McCoy refers to this warmest period prior to 53 Ma. The low-latitude planktonic assemblages in the Atlantic observed by Wolfe and Poore (1982) at 50–55°N during the "early Eocene" could have lived during this warm period also, but in any case they are not comparable to those of the northeast Pacific because of different oceanographic conditions. In the North Atlantic the gyral circulation brought warm low-latitude water north (Gulf Stream), whereas in the northeast Pacific, cold high-latitude water is transported south (California Current); thus, a comparison of faunal latitudes between the two oceans is inappropriate.

Specifically, Wolfe and McCoy cite the presence of *Globigerina (Acarinina) primitiva* and *Globigerina (Subbotina) linaperta* in the Yakutat block 50-Ma sample (Keller et al., 1984) and the presence of these species in the high-latitude South Pacific (Kennett, 1978) as evidence of deposition at high latitudes. First, these two species are cosmopolitan and are found at all latitudes, although most abundantly at high latitudes and decreasingly into low latitudes. Therefore, unless an assemblage consists almost entirely of these two species, as observed by Kennett (1978), it is not possible to interpret it as high latitude. Second, Wolfe and McCoy ignored the subtropical to warm-temperate species in the Yakutat samples which are more restricted geographically because they are less tolerant of temperature fluctuations. Keller et al. (1984) stated that the Yakutat block assemblage "contains common cool water *Globorotalia pseudoscitula, Globigerina primitiva, G. linaperta* and warm water *Globorotalia soldadoensis, Gl. bullbrooki, Gl. aragonensis* and *Gl. broedermanni.*" This assemblage is indicative of a subtropical to warm-temperate environment of about lat 30 ± 5°N, as compared with coeval assemblages of the Santa Lucia Range, Lodo, and La Jolla Formations of California, the Roseburg Formation of Oregon, and the Guyabal and Aragon Formations of Mexico (Keller et al., 1984). Our faunal correlations were also based on benthic foraminifers. Yakutat block assemblages were found to be most similar to assemblages from the Vacaville Shale correlative of the Lodo Formation and the Guyabal and Aragon Formations of Mexico.

Wolfe and McCoy cite the presence of *Amphistegina* in the Crescent Formation of southwest Washington and the Olympic Peninsula as evidence of tropical to subtropical conditions during late early and middle Eocene at high latitudes. The age of these benthic assemblages (Ulatisian; Rau, 1964, 1966) is questioned, however, because (1) the upper part of the Crescent Formation contains more late Ulatisian and Narizian species, suggesting a younger age, and the *Amphistegina*-bearing basal part appears to be of Penutian or late Paleocene to early Eocene age and hence appears to be coeval with the globally warmer conditions at this time.

Wolfe and McCoy's argument that molluscan and plant assemblages suggest deposition at high latitudes and indicate no northward movement is based on imprecisely dated molluscan assemblages—the early Eocene *Cristispira pugetensis* and the middle Eocene "Cowlitz-Tejon" mollusks. As pointed out above, our age and latitudinal constraints deal with specific intervals of time: at 50 Ma, 42–44 Ma, and 38–36 Ma (Keller et al., 1984). Correlation of molluscan and plant stages to the marine microplankton time scale is difficult at best. In fact, Wolfe (1981, p. 39) stated that "the occurrence of some plant megafossils in dominantly marine sections of Paleogene age provides only a general concept of relation to the marine geochronology." Molluscan and plant stages have undergone major revisions in recent years in an attempt to correlate them to the marine microplankton time scale. For instance, Wolfe and McCoy place the "Cowlitz-Tejon" mollusks in the middle Eocene (50–40 Ma). However, mollusks of "Tejon" age (Narizian) are found above the middle Ravenian floral stage which Wolfe (1981, p. 41) correlates to the planktonic foraminifer P15/P16 Zone boundary, or well within the late Eocene at about 38 Ma. Hence, Wolfe and McCoy's middle Eocene Cowlitz-Tejon" mollusks are actually late Eocene in age; we have no age comparable to their "early Eocene," but this age is likely to be younger also. On the basis of these two poorly dated faunas, they conclude that the early and middle Eocene Yakutat molluscan faunas indicate no northward migration of the Yakutat block. Their conclusion is correct for the wrong reason; with correct age correlations, their late Eocene "Cowlitz-Tejon" fauna is in place at high latitudes by 38 Ma according to the model of Bruns (1983) and Keller et al. (1984).

As evidence of floral warm paleoclimatic conditions, Wolfe and McCoy cite the "Tyee-Umpqua" or "Domengine" mollusk assemblages, which, however, are also imprecisely dated. Wolfe and McCoy place these faunas in the late early Eocene, after Armentrout (1981). However, in an Alaskan assemblage from Berg Lake, Wolfe (1981) found "Domengine"-age megafossils in the lower part of the Kushtaka and Kulthieth Formations (lower Ravenian, Wolfe, 1977), which he correlates to the upper middle Eocene planktonic foraminifer Zone P12 to lower part of Zone P14 (45–42 Ma). Hence, these assemblages are late middle Eocene in age and not late early Eocene. Above the Berg Lake but below the "Cowlitz-Tejon" mollusks is the Charlotte Ridge assemblage, which should be of very latest middle Eocene or late Eocene age. Wolfe and McCoy's comparisons of the Berg Lake assemblage with an early Eocene assemblage from Mississippi, and the Charlotte Ridge assemblage with his "coeval assemblages in the Puget Group" and the Susanville assemblage are thus not coeval with our assemblages. Our paleolatitude determinations for the Yakutat block at the 44–40 Ma interval is 40° ± 5°N and for the 40–36 Ma interval 45° ± 5°N. Unless molluscan and floral assemblages can be more precisely dated, we will not be able to compare marine microplankton data with molluscan and floral records. However, even if the precise age correlations could be made, it might be difficult to use plant assemblages to interpret the marine environment because extreme floral provincialism makes inter-regional comparisons difficult.

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Comment and Reply on "Abundant and diverse early Paleozoic infauna indicated by the stratigraphic record"

COMMENT

R. K. Pickerill, *Department of Geology, University of New Brunswick, Fredericton, New Brunswick, E3B 5A3, Canada*

Miller and Byers (1984) correctly pointed out that bulldozing, tiering and other such models related to benthic community evolution, particularly in the early Paleozoic, have been based largely on the body-fossil record. Nevertheless, I feel comment is necessary particularly with regard to ichnotaxonomy and the apparent unawareness by Miller and Byers of certain relevant literature and with regard to the occurrence of early Paleozoic infaunas in deep-water sediments, as revealed by their contained ichnotaxa.

At the ichnogeneric level, trace-fossil classification is based on essential features (sensu Fürsich, 1974) and in spite of the absence of a formalized code, most ichnologists adhere to this system. Trace-fossil literature contains a vast number of ill-defined ichnogenera, many of which will, provided detailed studies are undertaken, undoubtedly prove to be synonymous. This is particularly exemplified by vertically oriented dwelling burrows of suspension-feeding animals. Of these forms cited by Miller and Byers (1984, Table 2, p. 41), *Tigillites* is regarded by ichnologists as a junior synonym of *Skolithos*, because the only essential difference between these ichnogenera was, historically, based on burrow density, which is, of course, a paleoecological and not a morphological variable (Alpert, 1974). Similarly, following the exhaustive reasoning of Pemberton (1979), *Histioderma* should be included within *Monocraterion* and specimens previously assigned to *Sabellarifex* to *Monocraterion* or *Skolithos* (see also Crimes, 1981). *Corophioides* has been demonstrated by Fürsich (1974) to be a junior synonym of *Diplocraterion*, and the ichnogenus is generally no longer utilized. Thus, the list of suspension-feeding burrows is taxonomically misleading because at least four ichnogenera are generally no longer utilized. Indeed, the resurrection and perpetuation of the aforementioned forms should be discouraged. Furthermore, to the uninformed reader, to present the list as "early Paleozoic suspension-feeding burrows" is misleading, because the list is far from complete. Ichnotaxa such as *Bifungites* (see below), *Calycraterion*, and others produced by filter feeders are equally abundant and widespread. Additionally, although not detracting from the main purpose of the table, it is notable that Pickerill and Keppie (1981, Fig. 2) have reported *Arenicolites* burrows as deep as 28 cm from the early Paleozoic of Nova Scotia, considerably deeper than the supposed maximum depth of 4 cm.

Miller and Byers's (1984) list of early Paleozoic deposit-feeding burrows is more lengthy but still omits ichnogenera such as *Daedalus*, *Glockerichnus*, *Helminthoida*, *Protopaleodictyon* and other graphoglyptids and deposit-feeding burrows too numerous to include here. Such omissions could similarly prove misleading to the uninformed reader. More seriously, the listed ichnogenera (Miller and Byers, 1984, Table 3, p. 42) include *Bifungites* and *Astropolithon*, the former of which is undoubtedly a dwelling burrow of a filter feeder (Gutschick and Lamborn, 1975; Pickerill and Forbes, 1977) and the latter of which is unavailable as it is clearly a dewatering structure and not of biogenic origin (Pickerill and Harris, 1979).

The examples of highly burrowed early Paleozoic sequences are from shallow-marine environments, presumably because the authors were directly concerned with the supposed absence of deep burrowing in the early Paleozoic being used to advocate biotic controls on community structure, benthic species diversity, and substrate stability. Nevertheless, the occurrence of a deep-water infauna as reflected by preserved ichnotaxa in early Paleozoic, particularly pre–Middle Ordovician sequences, is equally important with regard to these questions. Paleontologists are now familiar with the Middle and Upper Ordovician community-restructuring events, which, among other effects, resulted in the establishment of benthic communities in deeper water regimes than their pre–Middle Ordovician counterparts, which were essentially restricted to shelf margins and shallower conditions (Boucot, 1983). It is notable, however, that in deep-water regimes the burrowing and infaunal habitat was clearly established prior to these radiation events. Pickerill and Keppie (1981), for example, recorded an assemblage of at least eleven distinctive ichnogenera from the deep-water Cambrian-Ordovician Meguma Group of Nova Scotia. Other pre–Middle Ordovician sequences have also yielded deposit- and suspension-feeding ichnogenera (e.g., Crimes and Crossley, 1968). Admittedly, pre–Middle Ordovician deep-water successions are never as diverse in terms of ichnotaxa as younger examples, but this is a reflection of the aforementioned radiation events that led to the introduction in these deeper water regimes of additional behavioral groups (Pickerill, 1980). The important point is that the occurrence of ichnotaxa in these deeper water habitats, some of which are extremely deep burrowers (e.g., Pickerill and Keppie, 1981), is also cause for concern regarding the supposed biotic controls of community structure and species diversity. I again emphasize that the omission of deep-water examples by Miller and Byers does not detract from their overall conclusions; rather it merely reinforces them.

In summary, the paper by Miller and Byers is most timely and opportune in that they have correctly pointed out some of the pitfalls casually ignored by paleontological theorists. Despite my criticisms, I believe the paper will not prove to be "provocative" as such but rather will provide a realistic and cautionary note that such theorists will be wise not to ignore.

REPLY

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Charles W. Byers, *Department of Geology and Geophysics, University of Wisconsin, Madison, Wisconsin 53706*

Pickerill correctly has emphasized the need for careful taxonomic work on trace fossils, a discussion of which was beyond the scope of our paper. In addition, he has extended our observations: deep-burrowing early Paleozoic animals were not restricted to marine shelf depth; they lived in deep-water environments as well. We appreciate his comment.

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Comment and Reply on "Transport and concentration of molybdenum in granite molybdenite systems: Effects of fluorine and sulfur"

COMMENT

Edet E. Isuk, *Department of Comprehensive Science, Morgan State University, Baltimore, Maryland 21239*
John H. Carman, *P.O. Box 1116, Hempstead, Texas 77445*

Tingle and Fenn (1984) have presented their preliminary results on the effects of fluorine and sulfur on molybdenum transport and deposition in granite systems. While we agree for the most part with their conclusions, we find it necessary to correct their misstatement of our suggestion in an earlier study (Isuk and Carman, 1981) regarding the mechanism of Mo transport in silicate melts.

Tingle and Fenn (1984) incorrectly attributed to us the interpretation that the association of Mo with silica-rich rocks is suggestive of metal complexing with "aluminosilicate" ligands. Rather, what we postulated was "silicate and/or hydroxylated silicate" complexes (Isuk and Carman, 1981), on the basis of our experimental results. The data presented by Tingle and Fenn (1984) do not preclude such a solution mechanism a priori. Our experimental system $Na_2Si_2O_5$-$K_2Si_2O_5$-MoS_2-H_2O obviously did not contain aluminum. However, the need for additional studies in Al-bearing systems did not escape us, and we specifically so stated (Isuk and Carman, 1981, p. 2234).

We agree with Tingle and Fenn (1984) that partitioning of Mo into the vapor phase as might be generated during magma ascent or decompression is important in Mo transport and deposition. This strong partitioning of Mo into the vapor phase in equilibrium with coexisting alkali silicate liquid has been reported in other studies (Isuk, 1976; Isuk and Carman, 1981). The presence of anhydrite ($CaSO_4$) in some quench products reported by Tingle and Fenn (1984) substantiates the interpretation that sulfur dissolves in silicate melts principally as SO_4^{-2} (Isuk, 1983) at relatively high f_{O_2}, ostensibly utilizing the NBOs (nonbridging oxygens) of the melt. The effect of S is thus to deplete the melt in NBOs necessary for metal complexing, increasing melt polymerization and effectively decreasing Mo solubility in the silicate melt.

During vapor evolution, the fractionation of S and Mo into the vapor phase is most likely to result in Mo deposition as MoS_2 because of the strong chalcophile affinities of molybdenum (Isuk, 1983). We find no apparent conflict between this interpretation and Tingle and Fenn's (1984) conclusion that "vapor phase evolution and the presence of S are primary controls on Mo ore deposition."

REPLY

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Philip M. Fenn, *Corning Glass Works, Sullivan Park, Corning, New York 14830*

We apologize for the misspelling of John H. Carman's name in our paper.

We wish to clarify the statement to which Isuk and Carman raise objections in paragraph 2 of their Comment. We included the Isuk and Carman (1981) reference to guide the interested reader to a study where Mo complexing with "silicate and/or hydroxylated silicate" complexes had been postulated for Mo dissolved in alkali silicate melts. We agree with Isuk and Carman that Mo complexing with such ligands is a possible interpretation of the presence of Mo in the silica-rich spherules observed in our experiments. These possible complexing ligands in our vapor spherules were referred to as "alkali aluminosilicate ligands" in a general sense to include all members of the anionic framework. The point we were trying to make in our original statement is that it is speculation, given our present experimental data, to infer Mo complexing with silicate ligands as the mechanism of Mo transport in the vapor phase.

Although the presence of quench anhydrite in our experiments may suggest that "S dissolves in silicate melts principally as SO_4^{-2}," two critical assumptions implied by this statement must be recognized. First, the structure and composition of *stable* minerals may provide qualitative information about the structure of the melt, but it is not clear that quench crystals can be interpreted this way. Second, the quench anhydrite we reported (Tingle and Fenn, 1984) is associated with vapor bubbles and most likely represents material quenched from the vapor phase and *not* the melt. It is very speculative to suggest that water vapors with dissolved silicate are structurally similar to silicate melts; this extrapolation is probably not valid.

The mere association of Mo and silica does not indicate complexing, and it remains to be shown that Mo-silicate complexes play an important role in the solution of Mo in silicate melts or magmatic vapors. Several spectroscopic methods (for example, Raman, EXAFS, XANES, infrared, or NMR spectroscopy) are now available which would allow us to study the solubility mechanisms of base metals. This and the previous discussion indicate the need to design future experiments for obtaining structural information about the solution of base metals and S in magmas and magmatic vapors.

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Comments and Reply on "Shapes of streamlined islands on Earth and Mars: Experiments and analyses of the minimum-drag form"

COMMENT

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Komar (1983) presented an interesting result that streamlined islands in rivers have shapes that are closely similar to symmetrical airfoils, with length:width ratios averaging 3 to 4. He suggested that a minimum total drag is achieved at such length:width ratios, supporting the hypothesis that these islands acquired shapes that minimized the drag or resistance to the flowing fluid when they were formed. He further showed that streamlined islands in three different rivers in the United States have an average length:width ratio of 4.30, and the maximum width of the island is 0.65 to 0.70 of the length from the pointed lee, just as found in airfoils.

We have tested these concepts for streamlined islands in the Gulf of St. Lawrence and the St. Lawrence estuary in eastern Canada (Fig. 1). The inset part of Figure 1 shows the shapes of three islands used in this study. Table 1 lists the length:width ratios and the location of maximum width as a fraction of the length (measured from the pointed lee) for sev-

TABLE 1. LENGTH TO WIDTH RATIOS AND POSITION OF MAXIMUM WIDTH AS FRACTION OF LENGTH MEASURED FROM POINTED LEE FOR STREAMLINED ISLANDS IN GULF OF ST. LAWRENCE AND ST. LAWRENCE ESTUARY

Island	Water body	Length L (km)	Width W (km)	L/W	Position of maximum width
Orléans	Upper St.	32.4	8.5	3.81	0.63
Madame	Lawrence	2.6	0.7	3.71	0.73
Brothers 2	Estuary	0.2	0.05	4.00	0.33
Girues		6.7	1.5	4.47	0.81
Canot		1.6	0.5	3.20	0.56
Grand		1.2	0.3	4.00	0.75
Pélerin du Milieu 1		1.3	0.4	3.25	0.77
Pélerin du Milieu 2		1.5	0.5	3.00	0.66
Gros Pélerin		1.3	0.4	3.25	0.83
Gros Cacouna		2.7	0.8	3.38	0.74
Aux Basques	Lower St. Lawrence Estuary	2.0	0.6	3.33	0.60
Anticosti	Gulf of St.	223.1	54.1	4.12	0.53
Au Marteau	Lawrence	1.4	0.5	2.8	0.73
Biron		7.4	2.2	3.36	0.74
Prince Edward		167.0	48.0	3.48	0.66
Pictou		8.0	2.1	3.81	0.63
Average				3.56	0.67

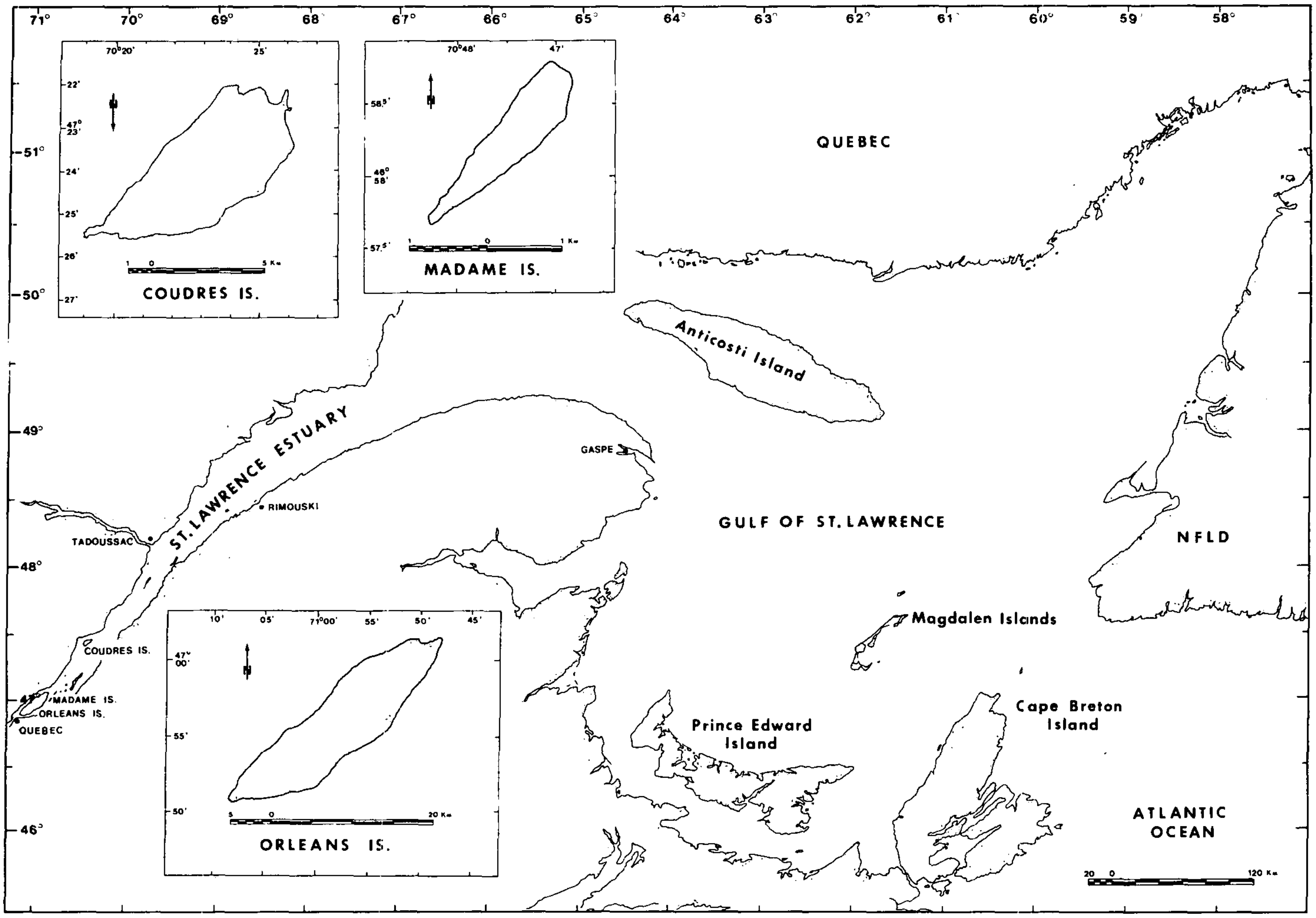

Figure 1. Gulf of St. Lawrence and St. Lawrence estuary.

eral islands. The overall average value for the length:width ratio is 3.56, and the overall average for the position of maximum width is 0.67.

It can be seen that the agreement is reasonably good. Thus, our data provide one more independent verification of the hypothesis proposed by Komar (1983).

COMMENT

Marion I. Whitney, *Central Michigan University, Mt. Pleasant, Michigan 48883*

Although Komar (1983) has given a good analysis of how water erodes an island, I believe his conclusion concerning wind erosion is incorrect and misleading. He admitted that wind is capable of creating a wake but said, "a wind-erosion origin can be ruled out on the basis of other evidence." Yet he failed to state this other evidence. Wind erosion cannot be ruled out because on Earth wind does erode streamlined forms similar to the Martian example in the Ares Valles channels that Komar used. Also, while he referred to a wind-erosion concept for these "islands," he gave no direct reference to any authors who have demonstrated such a concept.

Wind is capable of producing streamlined forms such as yardangs by aerodynamic processes wherein negative flow opens streamlines some distance upwind of the yardang. The streamlines, or positive flow, close on the lee and enter into the erosion, though much of the erosion is accomplished by subsidiary flow lines, such as negative and secondary flow, beneath the positive flow. These subsidiary flows transport vortices that bring suspensates such as silt and fine sand into erosional contact with the rock surfaces and thus create fluting. Most of the erosion occurs in the lee.

In Egypt, yardangs occur by the millions, ranging from a few metres to kilometres in length and in lithology from lake-bed silt to granite and quartzite, but the vast majority occur in parallel ranks in siliceous limestone on the central plateau (see Fig. 1). Here aridity has dominated for most of the past 0.25 Ma, the past 10 000 yr being hyperarid. Most of the yardangs present high, steep, fluted faces into the monodirectional wind and decline and taper leeward. Such a form in a monodirectional wind regime would preclude much sandblast erosion. Instead, erosion occurs largely by the aerodynamic fluting process. In many cases every surface of a yardang is covered with coarse, burnished flutes separated by knife-edge ridges. Fluting on the windward faces forms a fan-shaped pattern. If sandblasting were very signficant, it would destroy this fluting. Because the streamlines open upwind, most of the sand and granules are shunted aside, and an aureole develops within the granule cover, showing loci of the opening and closing of the streamlines. An inner zone adjacent

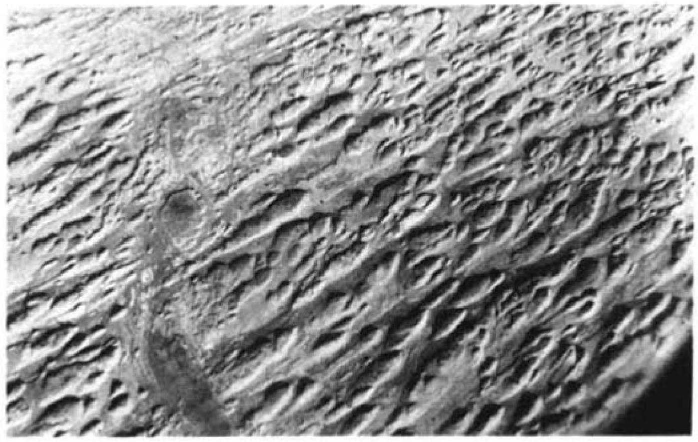

Figure 1. Streamlined yardangs on limestone plateau of central Egypt. Northwesterly monodirectional wind is from upper right. Ancient stream system at left is in reverse topography, and new yardang system is developing across it.

to the base of the yardang is in some cases a fluted bedrock moat. The type of fluting in it indicates formation by the negative flow that moves from leeward to windward. In the moat at the windward base, the right and left lines of negative flow meet and rise, creating a lowered pressure in which the rate of erosion is reduced. This commonly leaves a sharp-crested ridge or septum of bedrock dividing the moat. On the Egyptian yardangs this septum is usually only a few metres long, but it is a prominent keeled feature flanked by moats at the blunt end of Komar's example (Whitney, 1979b).

Some yardangs have a short windward keel and a long declining crestal keel. Both negative flow and the closure of the streamlines serve to fashion the keels, but in late stages, top surfaces may become flattened, resembling the Ares Valles example. To the left of the blunt end of this example there is another "island" that is keeled (Whitney, 1979b). This feature also has an undercut zone at the base of its blunt end such as many yardangs have (Whitney, 1983).

All over the area around these Ares Valles "islands" the surface is covered with hugh flute patterns, one of which crosses from the top of Komar's (1983) example to the top of the next "island" in the lee, thus appearing like wind fluting (Whitney, 1979b).

Wind-blast experiments show that wind-blown atmospheric dust and vorticity transported in flow lines can erode, shape, flute, and pit rock surfaces (Whitney and Dietrich, 1973; Whitney, 1978, 1979a; Dietrich, 1977). Likewise, McCauley et al. (1977, p. 169) stated that yardangs are of aerodynamic origin. The McCauley team performed a prolonged wind-blast test on very friable material, producing yardang-like features rapidly enough that the researchers could make a movie of the erosion.

In the area of profound wind erosion, the ancient stream systems of Egypt were largely converted to chaos terrain or to reverse typography, as at the left in Figure 1. Here the interfluves were lowered by wind, and a new generation of yardangs is now developing across the elevated stream trend.

If millions of yardangs can be formed and stream channels obliterated by wind within a few thousand years on Earth, then surely in several billion years wind on Mars can produce yardang-like features. Hence, we cannot disregard wind erosion on Mars. In fact, it deserves a good deal more attention than it has been given because wind erosion has been in progress as long as Mars has had an atmosphere.

REPLY

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Although a wind-erosion origin of the Martian channels has been proposed by Whitney (1979b) and by Cutts and Blasius (1981), the evidence arguing against such a formation is clear and definite. This is readily apparent in the general reviews of the channels presented by Baker (1982), Carr (1981), and the Mars Channel Working Group (1983), but it is discussed more specifically by Nummedal et al. (1983) in their comments on the hypothesis as developed by Cutts and Blasius (1981). It was for this reason that in my paper (Komar, 1983) I did not give serious consideration to a wind-erosion origin for the streamlined islands found within these Martian channels.

In brief, the arguments as presented by Nummedal et al. (1983) against a wind-erosion origin of the Martian channels involve the following: (1) The channels indicate flows that were consistently down the regional gradient and must have been in response to the downslope component of gravity, not driven by horizontal pressure gradients as are winds. (2) Wind streaks in the channel area demonstrate that the prevailing winds blow mainly against the channel gradients. (3) The channels are in general well defined, with distinct and fairly steep walls, and many are sinuous—features that are incompatible with a wind-erosion origin. (4) There are abundant examples of ponding of the flow followed by notching of the downslope bounding ridge, clearly the product of a flow, such as water, with a free upper surface. On the basis on this and other evidence, Nummedal et al. (1983) concluded that the wnd-erosion hypothesis of Martian channel formation is "demonstrably inadequate in explaining the most fundamental morphological characteristics of the channels." They further indicated that there is "a very strong consensus among planetary scientists favouring the concept that the original channel formation was due to the sudden release and flow of large quantities of water and/or an aqueous slurry."

As indicated in my paper (Komar, 1983), taken by themselves the streamlined islands cannot be used to argue in favor of a water-erosion versus a wind-erosion origin, because streamlining is inherently a mechanism that eliminates the turbulent wake region formed by the flow in the immediate lee of the island. The processes may be much the same for turbulent winds as for turbulent flows of water, and the final products (yardangs and water-eroded islands) will have similar shapes. My detailed analysis of the streamlined islands is, however, more definite in arguing against a glacial-erosion formation of the channels as proposed by Lucchitta et al. (1981); although drumlins are also similar in appearance, they differ in detail and in average length:width ratios because drumlin formation does not involve the elimination of a turbulent wake region (Komar, 1984).

Although yardangs definitely do occur on Mars (Ward, 1979), the streamlined islands found within the outflow channels are clearly a primary feature associated with channel formation rather than of secondary origin such as later wind modifications. All the islands are oriented with the expected flow directions along the channel, and in places they even form what might be described as braided-island systems (Trevena and Picard, 1978). In addition, W. Ward (1983, personal commun.) has undertaken a study of the aerodynamics of yardangs, similar to my study of water-eroded islands, and found that on average they tend to have length:width ratios greater than the 3:4 proportions required to minimize the drag, approaching those proportions only after a very long period of erosion. The sizes of the Martian islands, being tens of kilometres in width as well as in length, are also much greater than typical of yardangs either on Earth or Mars.

Wind erosion can be ruled out as a primary agent in the formation of the Martian outflow channels and associated streamlined islands.

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Comment and Reply on "Episodic accumulation and the origin of formation boundaries in the Helderberg Group of New York State"

COMMENT

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Anderson et al. (1984) reported that the Helderberg Group is rife with hiatal horizons separating carbonate facies deposited in noncontiguous environments; some of these coincide with member and/or formation boundaries, and the facies between surfaces constitute shoaling-upward sequences deposited in contiguous environments. They suggested that these features have significance to sedimentary geology. Their major conclusions (Anderson et al., 1984, p. 123), with comment, are as follows.

"The Helderberg Group consists entirely of . . . shallowing-upward cycles separated by . . . isochronous nondepositional surfaces. Within each [sequence], facies are gradational as a result of gradual shallowing of paleoenvironments." This is not a unique revelation arising from work by Anderson et al. (1984). Most cratonic carbonates consist of subtidal to supratidal sequences recording repeated migration of environments in space; a broad spectrum has now been documented from a variety of modern and ancient settings (e.g., James, 1979). Laporte reported these in the Helderberg Group in 1967.

"At [sequence] boundaries, facies change abruptly as a stratigraphic response to rapid, geologically instantaneous base-level rises." As in other cratonic units, Helderberg Group cycles consist of stacked proximal-over-distal facies; intervening transgressive sequences are thin or lacking (e.g., James, 1979). Although the rarity of deepening-upward sequences remains one of the basic problems in carbonate geology, three scenarios are plausible: (1) rates of sediment production and sea level were invariant and basins subsided episodically; (2) rates of sediment production and subsidence were constant while sea level rose episodically; and (3) rates of subsidence and positions of sea level were invariant and sediment production and deposition rates varied episodically. In the latter case, base-level rises were gradual, possibly spanning more time than that represented by regressions. As there are insufficient data to evaluate the importance of these three scenarios, it is (at least) premature to suggest that base-level rises were "instantaneous."

"The [regressive sequences] are . . . time-stratigraphic units." Critical to the validity of this statement is whether regressive sequences arose through vertical accretion or lateral progradation. Although Anderson et al. (1984) presented no evidence by which to choose, their claim of lateral synchroneity requires that they accept vertical accretion. However, this is a minority opinion; most workers (e.g., Wilson, 1975; James, 1979) have interpreted such sequences as recording repeated lateral progradation of contiguous environments. As such, they are not time-stratigraphic units.

"At any single locality, most formation and member boundaries in the Helderberg Group coincide with [sequence] boundaries. These contacts are sharp surfaces separating . . . facies that represent [noncontiguous] paleoenvironments." Given that the carbonates in question consist of thin regressive cycles lacking intervening transgressive facies, it is requisite that boundaries between thicker members or formations should coincide with some of the sequence boundaries.

"Therefore, most formation and member boundaries did not form as a result of gradual migration of adjacent paleoenvironments." Nearly 20 years ago the significance of Helderberg Group facies sequences were recognized. Laporte (1967) stated: "Three facies can be recognized . . . : supratidal, intertidal, and subtidal. . . . The three . . . existed contemporaneously, retreating and advancing continuously with the result that, today, they form a complex facies mosaic." It is only in a context of long-term migration of environments that Laporte (1967) concluded: "During [overall] . . . marine transgression . . . these environments . . . migrated westward with time. . . ." To imply that earlier workers were unaware of short-term variations and recognized only gradual changes is to misrepresent them.

"At different localities the same formation boundary may coincide with different [sequence] boundaries. . . . Thus, an apparently diachronous formation boundary . . . is not a single diachronous surface; actually it is a stratigraphic series of isochronous surfaces. . . ." In any given sedimentary sequence there may exist a spectrum of isochronous surfaces ranging from bedding planes to sequence boundaries. Recognizing that formation boundaries coincide with one or more sequence boundaries is no more revealing than acknowledging that formation boundaries coincide with one or more bedding planes, as is the case in all sedimentary units. The scale of the isochronous surface is irrelevant.

"The actual mappable surfaces in the Helderberg [Group] are isochronous [sequence] boundaries . . . no actual diachronous surfaces exist in the Helderberg Group." If, as acknowledged by Anderson et al. (1984), a formation boundary occurs at different isochronous surfaces at different localities, then by definition that lithostratigraphic surface, no matter how spatially complex, is diachronous. If the term "surface" is restricted to include only chronostratigraphic surfaces, obviously they cannot be diachronous. Such a restriction, however, adds little to our understanding of sedimentary rocks.

"[With regard to] the stratigraphic record as a whole, . . . the only actual stratigraphic surfaces . . . traceable for any significant distances . . . may well be isochronous surfaces. . . ." Central to the validity of this statement is the definition of "significant distance." At a scale of bedding planes such an assertion is unassailable; at a global scale it is absurd.

"If [this] interpretation of formation boundaries is valid . . . the concept of the formation as a fundamental stratigraphic unit is in question." As Anderson et al. (1984) pointed out in their introduction, rock-stratigraphic units (including formations) are properly defined by lithologic homogeneity irrespective of genetic or chronologic attributes. It is illogical to apply alleged genetic and temporal inadequacies in order to suggest that the initial concept is flawed. Formations as fundamental stratigraphic units are useful and valid entities, the presence or absence of regressive facies sequences notwithstanding.

Finally, Anderson et al. (1984) proposed PAC, the acronym for punctuated aggradational cycle. To what end? Identical facies sequences have already been termed shallowing- or shoaling-upward cycles (SUC), regressive carbonate sequences (RCS), and subtidal-intertidal-supratidal sequences (SISS). Nor is it clear whether the Helderberg Group sequences are episodic vertical aggradation cycles (EVAC) or episodic lateral progradation cycles (ELPC). Given the wealth of present terminology, we would not object should the term PAC meet an early demise.

REPLY

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We infer from the comments of Wilkinson et al. that their criticisms stem from one basic conflict: they are unwilling to acknowledge the premise of our general hypothesis (the hypothesis of punctuated aggradational cycles) as a model with which to evaluate the data presented in our paper. We make this inference because in their second as well as their concluding paragraphs they deny the uniqueness of PACs and because some of their specific criticisms concerning surfaces and time appear to be illogical if they are analyzed within the context of the general hypothesis.

Whether PACs are pervasive in the stratigraphic record (as we would claim) or not, the *concept* is certainly unique. We have defined PACs as small-scale basin-wide allogenic cycles that are manifest in all facies in which deposition could have been influenced by abrupt rise of relative sea level. To claim that PACs are the same as the commonly documented subtidal to supratidal carbonate sequences (e.g., James, 1979) misrepresents our concept. Shallowing-upward tidal sequences are only one local manifestation of PACs.

In our study of the Helderberg Group we have assumed and applied the PAC hypothesis. Working deductively we have been able to divide completely this stratigraphic sequence into PACs. We have traced individual PACs for distances in excess of 100 km, have correlated sequences of PACs over an outcrop distance of more than 200 km, and are now engaged in extending these correlations into Pennsylvania, Maryland, and West Virginia. If these correlations are valid, then PACs must be allogenic and cannot be autogenic as implied by Wilkinson et al. From this conclusion it logically follows that PACs are time-stratigraphic units that cut across major facies (Fig. 1).

Wilkinson et al. argue that PACs cannot be time-stratigraphic units if they accumulated internally by progradation. To the contrary, we think that the internal process of accumulation (aggradation vs. progradation) is irrelevant to the question of whether or not PACs are time-stratigraphic units. If PACs are basin-wide in extent and if their bounding surfaces are isochronous, they as a whole are time-stratigraphic units regardless of minor internal diachroneity produced by local progradation.

Wilkinson et al. do not concede a difference between common bedding planes and PAC boundaries. In contrast, we contend that PAC boundaries, which are basin-wide in extent, are fundamentally different in origin from common bedding planes, which are only local phenomena. Furthermore, unlike common bedding planes, PAC boundaries always mark abrupt changes to deeper facies and are relatively widely spaced stratigraphically. This spacing (1–5 m) suggests a recurrence interval of thousands of years or more for the events that produced these surfaces. It seems to us that these features, coupled with our correlations, preclude the possibility that PAC boundaries are local autogenic phenomena (Wilkinson et al.'s third scenario) and require instead an allogenic mechanism (either Wilkinson et al.'s first or second scenario).

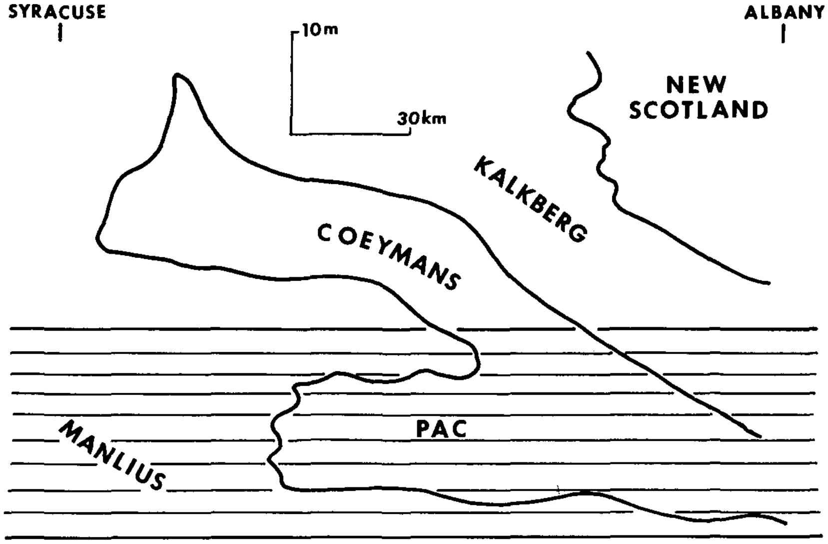

Figure 1. Relationship between time-stratigraphic PACs bounded by isochronous surfaces and major facies delineated by diachronous formation boundaries, cross section of Helderberg Group, New York State.

We believe that it is important to correct inaccuracies in Wilkinson et al.'s statements concerning the contributions of Laporte. Laporte did *not* mention subtidal to supratidal sequences in his 1967 paper. He only described the Manlius Formation as a complex mosaic of supratidal, intertidal, and subtidal facies of local extent, which retreated and advanced continuously during Manlius accumulation. The resulting facies patterns are not defined as shallowing-upward sequences and are not correlated among localities. In fact, Laporte implied a local autogenic mechanism as an explanation for facies migration. James (1979, p. 110) showed a figure described as an "actual sequence of several shallowing-upward sequences from the Manlius Fm., New York State (from Laporte, 1975)." The shallowing sequences (3 or 4) in this figure are defined and interpreted by James, not by Laporte, and the section is not an actual locality (see Laporte 1975, Fig. 29.2). In contrast, we demonstrate that the Manlius is completely divisible into at least seven PACs in the Hudson Valley and that these are traceable and therefore allogenic in origin.

Finally, we come to the central point of our paper, the hypothesis that diachronous formation boundaries are not actual diachronous surfaces but are instead artificial (diagrammatic) constructs of stratigraphers who have assumed gradual and continuous stratigraphic accumulation. We emphatically disagree with Wilkinson et al. when they say, "If . . . a formation boundary occurs at different isochronous surfaces at different localities, then by definition that lithostratigraphic surface, no matter how spatially complex, is diachronous." To the contrary, it seems that separate and essentially parallel surfaces cannot be a single lithostratigraphic surface. If a given formation boundary is defined at more than one isochronous surface at different localities, then there is no validity to connecting those data points with a diachronous line on a stratigraphic diagram, something that generally has been done by stratigraphers who assume gradualism and autocyclicity. We do not observe such diachronous surfaces; instead, the only observable stratigraphic surfaces, other than common bedding planes, are isochronous PAC boundaries.

Our projections to the stratigraphic record as a whole are made as testable hypotheses. If our conclusions are valid for the Helderberg Group, we think they constitute sufficient basis to suggest that they may have broad, if not universal, application. Whether or not these conclusions have general application will require extensive testing by stratigraphers in a variety of stratigraphic settings.

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