DAVID BLOOM

Has there been an oceanic margin to western North America since Archean time?

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ABSTRACT

Western Canada consists of a series of progressively younger orogenic belts from the Archean Slave province westward to the present continental margin. Each belt was superimposed on the westernmost rim of its predecessor. With no sign of preliminary rifting, each cycle started with deposition of a continental terrace wedge. Each cycle then evolved through various styles of compression orogeny but was never terminated by continental collision. Thus the data indicate that there has been a western continental margin to Canada since Archean time—a margin that has been both passive and active but which was neither generated by rifting nor destroyed by continental collision. The pattern is similar farther to the south for middle Proterozoic to Holocene time, but is less clear for early Proterozoic time.

INTRODUCTION

In this note I examine the hypothesis that there may have been a continental margin and thence oceanic crust to the west of the North American craton since the end of Archean time. I make one fundamental assumption—that the Earth has had about the same volume since 4.0 b.y. B.P. Until there is definite evidence to the contrary and despite the elegant arguments of Carey (for example, Carey, 1973), I see no reason to assume otherwise.

Proponents of plate tectonics and particularly the Wilson cycle maintain that ocean basins, and thus continental margins, are strictly ephemeral features, existing only during episodes between initial rifting and final collision. Strata from past continental margins have generally survived collision processes only sufficiently to give geologists tantalizing glimpses of ancient oceans. It has become a credo of many geologists who work on ancient continental-margin sequences that the margin must first have been generated by rifting (for example, Hoffman and others, 1973).

The criteria for recognition of rift-generated margins may be summarized as follows: (1) margin-parallel vertical faults defining half-grabens, (2) initial alkalic and subsequent tholeiitic magmatism in extensional fissures related to the faulting, (3) an erosionally leveled and thinned cratonic margin covered first by "restricted basin" sequences and later by more typical marginal sediments, and (4) evidence (such as the aulacogens of Burke and Dewey, 1973) for the former presence of triple-ridge junctions. If these criteria are absent from a given continental-margin sequence, then one should be most chary of interpretations involving initial rifting.

The fact that continuous wedges of sedimentary rocks lie unconformably across the trends of older belts is often taken as evidence for truncation of those belts by rifting. Although this is commonly true, can be seen quite clearly on the margins of present Atlantic, and has been well documented for its precursor (Iapetus), such features are not definitive of rifted margins. For example, there now are two stable, accreting continental margins (the Mediterranean coast of France and Italy and the Arabian Sea coast of Pakistan) where the predominant trends of the basement are perpendicular to the coast. Yet these trends *and their terminations* are the products of collision processes, not of rifting.

Most continental crust probably had been created by the end of the Archean (2.5 b.y. B.P.; Burke and others, 1976). The Archean cratons of the world had been stabilized with their present thickness and freeboard by this time. Most of the remaining continental crust became stabilized with its present thickness and freeboard by the end of the early Proterozoic (1.7 b.y. B.P.; Windley, 1977). As far as I am aware, continental crust is effectively indestructible. Such crust occupies approximately one-third of the Earth's present surface: if a constant-volume Earth is assumed, the same must have been true at the end of the Archean. Consequently, the remaining 70% of the Earth's surface must have been covered by a different type of crust. Because this area must have accommodated the world's oceans (Wise, 1974), there seems no harm in calling it "oceanic"—without necessarily implying anything about its composition or structure.

There must, therefore, have been continent-ocean margins, with the continents of similar composition and thickness to today's, at the end of the Archean. There is, then, no a priori

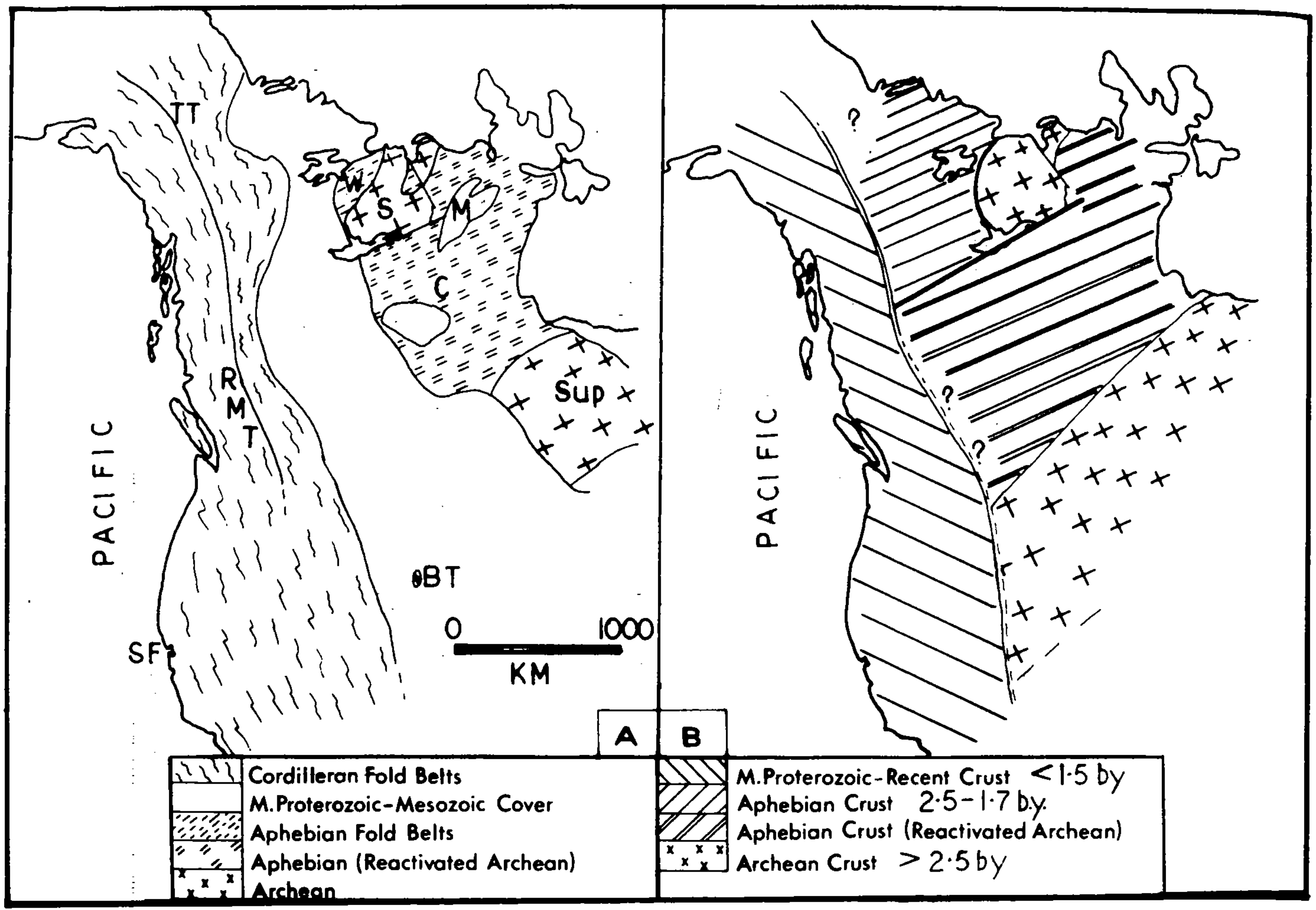

Figure 1. (A) Geology of northwest Canada. S = Slave province. C = Churchill province. W = Wopmay orogen. Sup = Superior province. BT = Bear Tooth uplift. M = MacDonald fault. TT = Tintina trench. RMT = Rocky Mountain trench. SF = San Francisco. (B) Probable extent of each major province of western North America with younger cover removed. One of the successive continental margins (see Fig. 2) is shown with double lines.

requirement for continental margins to have been generated by rifting. Equally, there is no a priori requirement for destruction of such margins by collision. These "perennial" margins may have been in extension (Atlantean), in compression (Pacific or Andean), or in shear (strike-slip) at various times in their history, but there must exist the possibility that such margins could have existed continuously since Archean time. The problem, of course, is where to look for such a margin.

The Pacific is the world's largest ocean. It would need a long-lived dominant spreading system in either the Atlantic or Indian Ocean to cause the closure of the Pacific and collision of Asia with North America. Paleomagnetic data for the Phanerozoic (Smith and others, 1973) show that the expansion of the Iapetus Ocean was not sufficient to cause closure of the paleo-Pacific. Paleomagnetic data further imply that North America had an ocean off its western margin throughout the Proterozoic (Piper, 1976). I would therefore propose that the present western margin of North America is the obvious place to look for the geologic evidence for a perennial margin.

THE WESTERN MARGIN OF NORTH AMERICA

The western margin of the North American craton in Canada consists of orogenic belts that are successively younger to the west from the Archean Slave province (Fig. 1). Descriptions of the evolution of each of these belts will be but briefly

TABLE 1. AGE AND NATURE OF STRUCTURAL UNITS OF WESTERN CANADA

Tectonic unit	Age (m.y.)	Type of margin	References
Cordilleran orogens	0	Strike slip and Andean	Gabrielse and others (1977), Wheeler and Gabrielse (1972), Churkin and Eberlein (1977), Tipper (1977)
Cordilleran orogens	300	Pacific and Andean	Gabrielse ana Dodds (1977), Templeman-Kluit (1977), Hyndman and Riddihough (1977), Anderson (1977), Stewart (1972), Monger and others (1972)
Cordilleran orogens	700	Pacific(?)	
Racklan—East Kootenay orogen	750	Andean(?)	Eisbacher (1977), Aitken (1977)
Belt-Purcell strata	1,600	Atlantean	Harrison (1972), Wheeler and Gabrielse (1972), Churkin and Eberlein (1977)
Great Bear batholith	1,900–1,700	Andean and possibly strike slip	Badham (1973a, 1973b), Hoffman and McGlynn (1976)
Hepburn batholith	2,000–1,900	Andean(?)	Hoffman (1973)
Coronation geosyncline	2,200–2,000	Atlantean	Hoffman (1973)
Slave province	2,460		

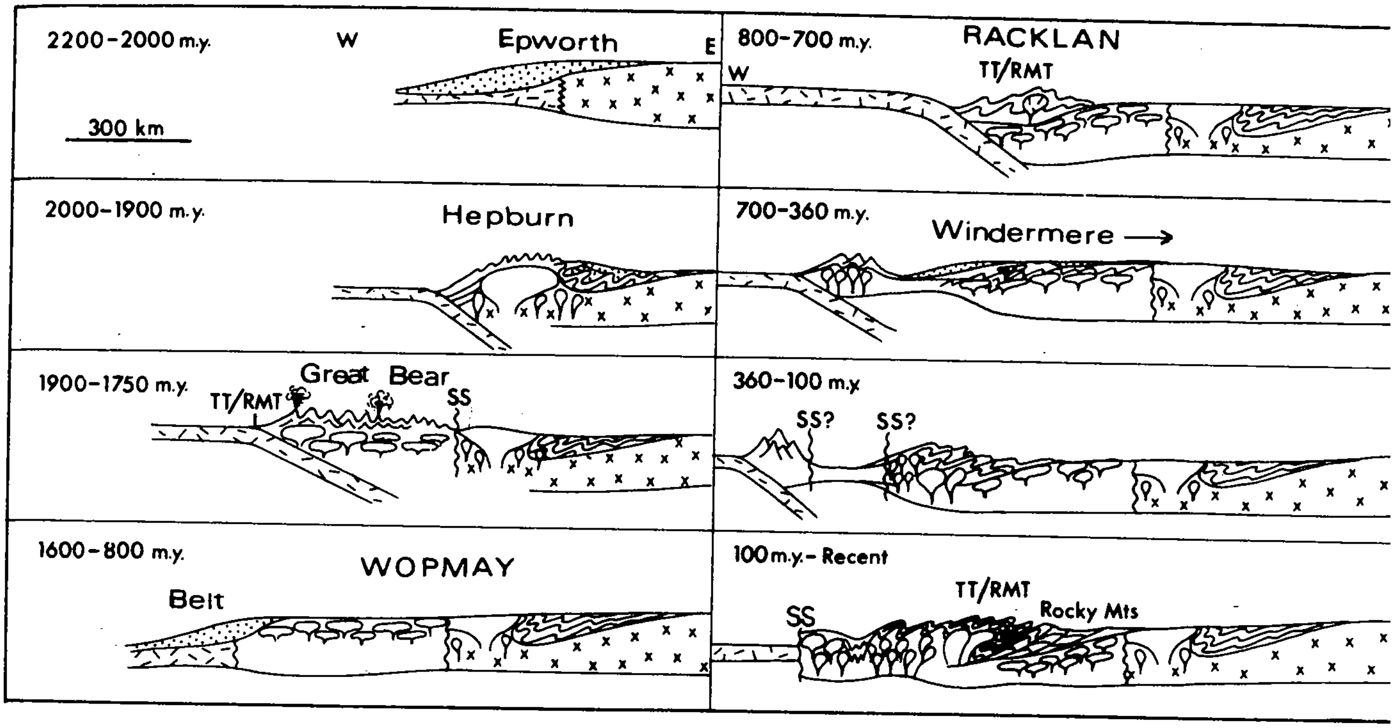

Figure 2. A series of speculative cross sections of northwest Canada from the Slave province to the Alaskan coast from early Aphebian to Hol cene time. SS marks the site of potential strike-slip motion out of the plane of section. The sections are taken north of the MacDonald fault. Althou the sections are highly speculative and other interpretations for both Aphebian and Phanerozoic tectonics have been made (see references in Table 1 no model yet published has suggested continental-collision events at the various western margins, and all are refinements of subduction-based interpretations.

summarized here, and the reader is referred to the more complete histories referenced in Table 1. The Slave province had cooled and stabilized with its present crustal thickness by about 2.5 b.y. B.P. and was cut by diabase dike swarms between 2.4 and 2.1 b.y. B.P. (Leech, 1966). Lower Proterozoic rocks of the Coronation geosyncline (Hoffman and others, 1970) were deposited on the western margins of the craton but were derived from the east (Hoffman, 1973). The sedimentary sequence, now exposed in the Epworth foldbelt, evolved in a manner typical of modern continental-margin prisms but quite different from ensialic lower Proterozoic sequences elsewhere in Canada (for example, Circum-Ungava geosyncline, Dimroth, 1972; Southern province, Card and others, 1972).

The Coronation geosyncline probably evolved between 2.2 and 1.9 b.y. B.P. (Fig. 2). Nowhere is there evidence in the Slave province or in the lower geosynclinal strata of any of the criteria for initial rifting. Hoffman and others (1973) have argued that the graben at Great Slave Lake and the one at Bathurst Inlet (aulacogens in the sense of Salop and Scheinmann, 1969) are the "failed arms" of triple-ridge junctions and the relics of an early Proterozoic craton-rifting event. However, I have argued (Badham, 1978) that the evidence is by no means compelling and have presented an alternative model involving the deflection of compression between two stable Archean blocks during the stabilization of the Churchill province.

The later evolution of the geosyncline is characterized by the development of flysch and molasse wedges derived to the west from an uprising orogenic core whose roots are now seen in the Hepburn belt (Fig. 2). To the south, this belt is exposed at deeper levels and contains remobilized Archean material (Nielsen, 1975; Frith and others, 1977) showing that the Slave province extende beneath much of the geosynclinal pile. To the west of the Hepburn belt a volcano-plutonic complex (the Great Bear batholith, Fig. 2) developed between 1.9 and 1.7 b.y. B.P. This terrane evolved independently from the Hepburn belt with which it is now juxtaposed by the dextral strike-slip Wopmay fault. The batholith itself is transected by numerous synorogenic to post-orogenic dextral faults (Badham, 1973a; Hoffman and McGlynı 1976).

The sedimentologic, metamorphic, structural, and igneous histories of this early Proterozoic (Wopmay, Fig. 2) orogen are remarkably similar to those of Phanerozoic orogens. It has been described as having developed initially from a passive (Atlantean margin and later to a compressive (Andean) and possibly occasionally strike-slip margin (Badham, 1973a, 1973b). There is no evidence of a terminal collision event. High-level plutonic debris similar to the Great Bear batholith has been recognized in drill cores from beneath the Phanerozoic cover to the west of Great Bear Lake and into the Cordillera. This basement can be traced seismically from Great Bear Lake to the Tintina and Rocky Mountain trenches (Wheeler and Gabrielse, 1972; Fraser and others, 1972).

To the south in the United States, Archean and lower Proterozoic strata are poorly exposed. The Churchill province probably continues west and south to central Idaho, and south of this there are intermittent exposures of the extension of the Superior province. The structural trends of both provinces are at the high angle to the Cordillera (Fig. 1), but little is known of the original nature or evolution of these belts in this area.

The Archean and early Proterozoic provinces of western

North America are overlain unconformably by middle to upper Proterozoic Belt-Purcell strata. These form a virtually continuous band from the Yukon to at least central Utah. There is general agreement that they are the remains of a continental terrace wedge (Gabrielse, 1972), derived entirely from the craton. There is no evidence of any kind for rifting in the basement or in the earliest Beltian strata. Furthermore, there is no indication of any derivation of clastic material from the west except locally along re-entrants (Harrison, 1972; Maxwell, 1974). It is concluded that the Beltian strata were laid down on an already extant and stable continental margin. The apparent truncation of earlier trends beneath these strata is a result of their original termination and not of their rupture by rifting.

At about 0.75 b.y. B.P. the Beltian strata were deformed, metamorphosed, and intruded by granite during the Racklan-East Kootenay orogeny. This event is ascribed to subduction (Monger and others, 1972) and may possibly represent the initiation of a marginal arc and small ocean-basin system (Fig. 2). This change correlates well with the probable initiation of the major phase of expansion of the Iapetus Ocean (Dewey, 1969). There is certainly no evidence of a collision event, and subsequent Proterozoic sedimentation (Windermere Group) shows no sign that clastic material was derived from the west. Stewart (1972) contended that the Windermere Group lies unconformably on a rifted margin, but offered only the presence of local tholeiitic basalt sequences as evidence. The greatly altered chemistry of these volcanic rocks makes highly contentious any interpretation of their original chemistry, and thence of their tectonic environment of eruption. One must agree with Maxwell (1974, p. 832):

> In view of the essential parallelism of Windermere and Beltian strata (even though generally separated by an angular unconformity) and the regional absence of a western cratonic source of sediments following Beltian sedimentation, it seems reasonable to date the initial continental rifting (if indeed it occurred) as earlier than the beginnings of Beltian sedimentation.

The clastic sediments of the Windermere Group give way upward to upper Proterozoic-lower Paleozoic carbonates, although more westerly strata indicate the presence of an offshore arc and marginal basin. This arc system expanded and shed flysch eastward in the late Paleozoic and finally accreted to the continental margin in the early Mesozoic. A new arc system was initiated offshore in the Mesozoic and similarly expanded and accreted through Mesozoic and early Tertiary time. Strike-slip movement, which may have commenced as early as the Middle Jurassic (Tipper, 1977), became the predominant feature of the margin in the Late Cretaceous and was interspersed with episodic and local subduction (Badham and Halls, 1975) until the Miocene. Since then, much of the Pacific margin of North America has been only in shear (Churkin and Eberlein, 1977).

CONCLUSION

It has been shown that the North American craton evolved by the accretion of successively younger continental-margin orogenic belts (Figs. 1 and 2). The main additions have been by offshore growth and later addition of island-arc systems (Churkin and Eberlein, 1977). The real extent of crustal growth is debatable, but not within the scope of this paper. There is evidence that many of the Phanerozoic arcs may have grown not directly on oceanic crust but on cratonic slivers rifted (or strike-slip faulted) from the original margin; it may be that the total crustal growth was very small.

The total lack of evidence either for rifting at the beginning of each Atlantean stage or for continental collision at the end of the various active stages justifies the contention that the North American craton has had an oceanic margin on its western side ever since the inception of the stable continent-ocean couple at 2.5 b.y. B.P.

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BOOK REVIEW

A color illustrated guide to carbonate rock constituents, textures, cements, and porosities. By Peter A. Scholle. Memoir 27. Published by American Association of Petroleum Geologists, Tulsa, Oklahoma, 1978, xii + 241 p., $19.50 (one price, no member discount).

Of this book, the author states that his intent "is to make available to geologists who may not be specialists in carbonate petrography a volume which illustrates the major grains, textures, cements, and porosity types found in carbonate rocks." An earlier version of this work was apparently an "in-house" publication of Cities Service Oil Company.

Scholle's book is essentially an atlas or picture book of some 171 photomicrographs of limestones and dolomites. Most are ordinary photomicrographs: these are in color. A few are scanning electron micrographs: these are black and white. About half are illustrative of skeletal grains; the remainder are grouped as other carbonate grains (pellets, ooids, intraclasts, peloids), other minerals (dolomite, evaporitic minerals, silica and iron and phosphate minerals, including glauconite), carbonate cements, carbonate textures (burrows, crusts, dissolution fabrics, replacement and neomorphic textures, compaction and deformation fabrics, and geopetal fabrics) and, finally, porosities of various sorts.

The photomicrographs are uniform in size, about 3 x 4½ in. (11.5 x 7.8 cm) placed three to a page. They are generally of good quality.

Text material is confined mainly to a brief paragraph accompanying each picture. Included also is a glossary of terms (some 83 entries) and a selected bibliography (of about 69 entries, some cited more than once). There is a section on techniques (staining, X-ray diffraction, cathodoluminescence, scanning electron microscopy, and microprobe)—a section much too brief to do more than alert the reader to the usefulness of these techniques. The book contains an index.

The author does not pretend that the book is a complete treatise or textbook. It is a book of pictures of what one might expect to see looking at peels and thin sections of limestones and dolomites. The part dealing with skeletal grains is straightforward descriptive material. Not so with much of the remainder. The recognition of many features such as pellets, intraclasts and replacement and neomorphic fabrics requires considerable interpretative skill. This is especially true of cements and porosities. The explanations appended to the photomicrographs must, because of their brevity, be somewhat dogmatic.

I found no illustration of chamosite and siderite among the iron minerals, nor of microstylolites (macrostylolites appear under "compaction and deformation" and not under "dissolution fabrics"). Authigenic feldspar, fairly common in carbonates appears in but one photomicrograph. The title of the book is not strictly correct as, is commonly the case, most students of carbonate rocks forget the bedded iron carbonates. Although not as common as limestones or dolomites, they are valid carbonate rocks. Although the bibliography is brief, it might well have included Fredrico Bonet's "La facies Urgoniana del Cretácico Medio de la región de Tampico" (Associación Mexicana de Geólogos Petroleros Boletín, v. 4, p. 153-262, 1952), which contains an excellent collection of photomicrographs (illustrating, among other things, the grumose texture not mentioned in Scholle's book).

One can say, in summation, that the book will prove helpful to students of carbonate rocks. For those whose familiarity with micropetrology is limited, the interpretation of textures and mineralogy will require more than a casual look down the barrel of a microscope, and Scholle's book is not an adequate guide for such persons. Nonetheless, the author has added to the growing list of useful picture atlases of rock fabrics.

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FORUM

Editor's Note: Geology *welcomes contributions to the Forum not exceeding four typed double-spaced pages.*

Comment and reply on 'Has there been an oceanic margin to western North America since Archean time?'

COMMENT

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Recent systematic mapping of Athapuscow aulacogen (Hoffman and others, 1977) and the northern part of Wopmay orogen (Hoffman and others, 1978), combined with new petrogenetic studies by several authors (cited below), suggest that Badham's (1978b) hypothesis of a perennial continental margin since the Archean is erroneous. In the light of new data, I wish to discuss Badham's rejection of rifting at the beginning, and of collision at the end, of the Coronation cycle (2.15 to 1.75 Ga ago; 1 Ga = 10^9 yr).

Badham asserted that "nowhere is there evidence in the Slave province or in the lower geosynclinal strata of any of the criteria for initial rifting" (Badham, 1978b, p. 623). Consider the Athapuscow aulacogen, the most prominent early Proterozoic structure in the Slave province. Rift-related activity began near the mouth of the aulacogen about 2.15 Ga ago with intrusion of at least two alkaline igneous complexes (Davidson, 1978; Burwash and Cavell, 1979), the larger of which is in part peralkaline (A. Davidson, 1978, personal commun.). Both complexes are cut by aulacogen-parallel diabase dikes, part of a swarm at least 250 km long. Locally, the dikes are overlain by Union Island Group sedimentary rocks that are stagnant-water (probably lacustrine) deposits. This group also contains two basalt centers, trace elements of which have been studied by Goff and Scarfe (1976). They characterized the older one as alkalic and the younger one as a "notably primitive" continental tholeiite, comparable to the "rift-related Tertiary basalts of west Greenland" (S. Goff, 1977, personal commun.). The Union Island Group is onlapped by a transgressive, continental-shelf succession which, near the mouth of the aulacogen, contains numerous volcanic centers, trace elements of which attest to an originally tholeiitic character (Olade, 1975). Thus, evidence of rifting has been greatly augmented since the original aulacogen hypothesis was proposed (Hoffman, 1973; Hoffman and others, 1974).

The early development of the continental margin itself is more difficult to decipher because its leading edge has been subducted (in the general sense of Bally, 1975) tens of kilometres beneath the foreland fold-and-thrust belt of the Wopmay orogen. However, there are major volcanic rocks, being studied by R. M. Easton, at the base of the Coronation geosyncline. Basalt flows (Vaillant Formation) underlie sedimentary rocks of the outer continental shelf, and voluminous basalt and rhyolite (Akaitcho Group) occur in the lower part of the continental rise (Hoffman and others, 1978). There is also at least one alkaline igneous complex (Martineau and Lambert, 1974), of the same age as those in the aulacogen. All in all, evidence for initial rifting is at least as impressive as in the Appalachians.

In characterizing the Wopmay orogen as Andean and asserting that "there is no evidence of a terminal collision event," Badham (1978b, p. 623) stated a view I once shared (Hoffman, 1973; Hoffman and McGlynn, 1977). This position has been criticized by Burke and others (1977), who interpreted the foreland fold-and-thrust belt as a zone of continental collision following west-dipping subduction of oceanic lithosphere, rather than a back-arc thrust belt related to east-dipping lithospheric subduction. The following arguments, taken together, strongly support the collision model: (1) in the foredeep, flysch greatly exceeds molasse; (2) the flysch is of granitic-metamorphic, not arc-volcanic, provenance; (3) thrusting involves not only the continental shelf but the continental rise as well; (4) much of the thrusting predates intrusion of the Hepburn batholith; (5) the batholith is granitic, not tonalitic, in composition, and (6) the batholith itself has suffered severe east-west flattening and north-south elongation, inexplicable in the Andean model. Burke and others (1977) interpreted the Great Bear batholith as a post-collision zone of crustal thickening and basement reactivation (Dewey and Burke, 1973) and gave Tibet as an example. Alternatively, the batholith may result from crustal melting triggered by upwelling of hot asthenosphere around a sinking slab of cold subducted lithosphere detached following collision (Bird and others, 1975). In either case, the overwhelmingly granitic composition of the batholith and the potassic nature of associated volcanic rocks (Hoffman and McGlynn, 1977) require that collision be of intercontinental, not continent–island-arc type. A simple collision model, however, does not explain the 230-km-long string of calc-alkaline (Badham, 1979b) laccoliths in the Athapuscow aulacogen, which are most readily explained by a late east-dipping subduction zone (Hoffman and others, 1977). A complex continent-microcontinent-continent collision model may be in order, involving (1) initial west-dipping subduction of oceanic lithosphere leading to continent-microcontinent collision, (2) flip to east-dipping subduction with the new trench located on the west side of the accreted microcontinent, and (3) final collision resulting in an enormous swath of northeast-trending right-slip and northwest-trending left-slip faults, which postdate the Great Bear batholith and the Athapuscow laccoliths and which extend far beyond the Wopmay orogen.

In summary, the new data seem to me to support those aspects of my earlier synthesis (Hoffman, 1973) most assailed by Badham (1978a, 1978b) and to deny those aspects he defends.

REPLY

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Hoffman and his colleagues have certainly seen more of the lower Proterozoic provinces of northwest Canada than I. His mapping and data have been fundamental in all attempts to explain the origin of this complicated region. His comments are therefore most welcome. Much of the debate between us has been aired in previous publications, and there seems little point in reiterating in detail here.

Of the two major areas of contention, the "rifting" part seems to me to be the most important. There is no disagreement that "rifting" occurred at 2.2 to 2.1 Ga ago. There is disagreement on its nature and significance. Briefly, it is my opinion that the "rifting" was associated with strike-slip faulting and that it began before and continued after Hoffman's rifting event. Despite Hoffman's implications to the contrary, the faults were *not* initiated as normal faults defining a parallel-sided graben 2.15 Ga ago. My arguments are summarized in Badham (1978a, 1979a). I would be far happier with Hoffman's hypothesis if there were similar indications of rifting (in the sense of Hoffman) in the Bathurst graben and in the Archean foreland of the Coronation geosyncline.

In this debate I have deliberately avoided the use of the term "aulacogen." As originally defined (see Salop and Scheinmann, 1969), the East Arm is an aulacogen. The reinterpretation of *all* aulacogens as failed arms of rift-generated triple-junctions (Burke and Dewey, 1973) adds a genetic significance to the word with which I fundamentally disagree and which leads to major circular arguments. My disagreement stems from the fact that many of the "type" examples (as the Benue Trough) have detailed tectonic histories that are incompatible with an origin simply and primarily as aulacogens (as in Burke and Dewey, 1973). However, this is not the place to air *this* argument, which forms the subject of a paper in preparation. The circular argument is this: if an aulacogen is defined with a genetic connotation, then any graben that is described as an aulacogen immediately has a prescribed genesis. Thus by calling the East Arm graben the "Athapuscow aulacogen," Hoffman is thinking—and asking his reader to think—in genetic terms. Thus it is easy for him to say that "rift-related activity began near the mouth of the aulacogen about 2.15 Ga ago"; the reader is asked to assume that events previous to this were not rift-related but were fortuitous. Yet these events occurred and occurred apparently in the same style and with the same controls as those after the "rifting." To my mind they must be explained.

I stand by my conclusions that there is no evidence for *initial* rifting. My model not only allows for faulting interacting with a continental margin 2.15 Ga ago but, in my opinion, better explains the geotectonic history of the Churchill province as well—a feature not explained in Hoffman's interpretations.

This argument apart, I agree with much of Hoffman's data. Indeed, I described and analyzed some of the alkaline complexes that predate much of the Great Slave Supergroup (data are summarized in Badham, 1975, 1979b; they are fully documented in Badham, 1979a).

Arguments about the nature of the "terminal" events rest on far scantier data and are consequently far more speculative. Hoffman's recent data (Hoffman and others, 1978) add interesting complications which certainly require more complicated models than both he and I envisaged earlier. The Great Bear batholith cannot be interpreted in terms of collision and crustal thickening, for as both Hoffman and I have pointed out, its remarkable feature is that it was intruded at very high levels into its own ejecta; yet it, and its ejecta, are still there. There has been none of the isostatic imbalance and consequent erosion that characterize collision. I prefer at present to think in terms of accretion of much of the orogen essentially by strike-slip motion, rather in the way that parts of the coast ranges in Alaska and the Yukon have been formed. Although the northern cordilleras were built in part by collision, no one would deny that there has ever been anything but oceanic crust to the west. So I stand by my contention that no major ocean was closed by the late early Proterozoic (about 1.75 Ga) orogenies in the Northwest Territories.

Hoffman and others (1977) related the calc-alkaline diorites in the East Arm to a subduction event 1.75 Ga ago. I am unable to accept this on the evidence available but can propose no sensible model for their generation. (I will deal with this further in a paper that I have prepared on the petrochemistry of upper Aphebian diorites from the East Arm of Great Slave Lake.) To be quite frank, there are too few data to resolve any of the arguments about the late early Proterozoic events, and I have no doubt that there will be speculations and arguments for some time to come.

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Comment and reply on 'The terminal Cretaceous event: A geologic problem with an oceanographic solution'

COMMENT

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Gartner and Keany's (1978) proposed oceanographic solution to the problem of the terminal Cretaceous event is an intriguing speculation—that a low-salinity surface layer rapidly spread over the world ocean from an isolated, brackish Arctic Ocean during late Maastrichtian time. However, it is an unfounded speculation in that its basic premise relies on a brackish Arctic Ocean during the Late Cretaceous. Ironically, Gartner and Keany stated that their ultimate proof awaits the recovery of undisputed deep-water Upper Cretaceous sediments from the Arctic Ocean basin (Gartner and Keany, 1978, p. 711). Such a sediment core (Fl-437) containing a rich, normal-marine silicoflagellate and diatom biota of late Maastrichtian age was recovered from the Central Arctic Alpha Cordillera in 1969, at a depth of 1,584 m (Ling and others, 1973), and has been discussed by many Arctic investigators (for example, Clark, 1974; Herron and others, 1974; Clark, 1977a, 1977b; De Laurier, 1978). Gartner and Keany's proposed solution therefore possibly creates more problems than it solves.

The proposed model requires, according to its authors, a severance of the circulation between the Cretaceous Arctic Ocean and all other oceans. However, the affinity of the Arctic Late Cretaceous silicoflagellate and diatom species with both Russian and Pacific Late Cretaceous marine floral elements suggests fairly good marine connections between the Arctic and the Pacific. Although the 1973 Arctic report was the first record of Cretaceous silicoflagellates from the deep sea, since then the same species have been reported in Deep Sea Drilling Project cores from the South Pacific and Indian Oceans (Hajos, 1975; Perch-Nielsen, 1975a; Bukry and Foster, 1974). The diatom assemblage is currently being studied in detail in our lab, but preliminary results suggest that these species are normal marine and exhibit affinities with Late Cretaceous species in other parts of the world, notably the USSR and the Pacific.

Further, a second sediment core (Fl-422) recovered 115 km away from the Upper Cretaceous core contains an abundant and diversified silicoflagellate and diatom assemblage of Paleocene age. The presence of this assemblage indicates that similar normal salinity conditions persisted in the Arctic over the time period spanning the Late Cretaceous terminal event.

There is little question that core Fl-437 is of Late Cretaceous (probably Maastrichtian) age, normal marine (certainly not brackish), deep-water, and recovered from the Central Arctic—the necessary requisites of Gartner and Keany's "ultimate proof." The only unresolved question we can imagine is that of "undisputed" origin of the sediment in the Central Arctic Ocean basin. The Fl-437 core represents a slump block of tuffaceous sediment enclosed by Holocene sediments. However, the observed bedding and high water content of the core as well as the distant (115 km) recovery of similarly slumped Paleocene sediments from core Fl-422 argue against a lengthy transport from another environment. The irregular topography of the area of recovery, the Alpha Cordillera, further makes slumping an expected event.

Gartner and Keany stated that the critical question remains, "Was the Arctic Ocean brackish during late Maastrichtian time?" Although the model they proposed is intriguing *if* the Arctic Ocean was brackish during the Late Cretaceous, on the basis of paleontologic evidence, there is no support for such a premise. Indeed, on the basis of two cores recovered to date, the data are convincing that the Late Cretaceous–early Cenozoic Arctic Ocean was an open-marine ocean with good southern connections.