# Comments and Reply on "On Orogeny and Epeirogeny in the Study of Phanerozoic and Archean Rocks"

## COMMENT

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### Introduction

Hermes and Borradaile (1985, *Geoscience Canada*, Volume 12, p. 148-151) should be congratulated on trying to make some sense of a complex, highly controversial, and difficult subject; one which is unlikely to be resolved imminently. However, we found several parts of their paper confusing, and in some cases, unintentionally misleading. Much of the first half of their paper was perfunctory given the nature of the subject, and the second half of their paper on Archean tectonics is at variance with much of our current understanding of Archean terranes in Canada. Consequently, we have written this discussion to further the debate on the issues raised by Hermes and Borradaile.

### Diastrophic Terminology

Although most of our criticism of the paper concerns the latter half of the paper, we do have some comments with regard to the first part of the paper. These are made in order to clarify terminology, and to discuss more fully the nature of modern tectonic regimes (and hence, facilitate comparison with older terranes). Hermes and Borradaile (1985) recognize two types of tectonic regimes, active or mobile areas and stable or cratonic areas. This bimodal view has influenced thought since the late 1890s, when the terms orogeny and epeirogeny were introduced (Gilbert, 1890; Upham, 1894). As shown in Table 1, our present understanding of the Earth shows that the situation is more complex. Stable and mobile areas exist in both the oceanic and continental crust; as of yet, no formal terminology has been developed for describing diastrophism in the oceanic regime. In continental areas, diastrophism occurs in a number of plate-tectonic settings, not all of which were recognized when the terms orogeny and epeirogeny were introduced. Island-arc and continental collision zones result in mountain building, or orogeny, in both the sense used by Hermes and Borradaile (1985) and Bates and Jackson (1980). Platform regimes and intracratonic basins may experience periods of warping and vertical movements (*e.g.*, glacial rebound) which cause deformation, and which can classically be considered as epeirogenic or cymatogenic (King, 1959). An additional category of diastrophism exists at present, consisting of zones of regional extension (*e.g.* Basin and Range in the western US), rift-zones of several types, plateau uplift (*e.g.*, the Colorado Plateau), plateau volcanism, and regional subsidence or uplift. Some of these phenomena can be considered as epeirogenic. While the distinction between rift and cratonic regimes at present may not seem to be particularly important, when we examine the past, it becomes exceedingly important because direct evidence of orogeny (*i.e.*, mountain building) is absent in the past. Structures such as thrusts, complex folding, and so on, can occur in the extensional environments noted above, and in the rock record they may be difficult to distinguish from classic orogens. If the Archean shields in Canada represent extensional (rift) zones in sialic crust (*e.g.*, Easton 1985a; Thurston *et al.*, 1986), then the term epeirogenic gives a misleading view of the nature of tectonic regimes in the Archean. Obviously, in future a term should be introduced to describe this type of tectonic activity that is not truly orogenic, but not a term as broadly

**Table 1** Modern tectonic regimes and diastrophic terminology

| Tectonic | Regime | Tectonic Features | Terminology |
|---|---|---|---|
| Continental crust | Stable areas | platforms, basins, shields (miogeosynclines of Kay, 1951) | - epeirogenic or cymatogenic (*e.g.* glacial rebound)<br>- anorogenic (magmatism) |
| | Mobile areas | island-arc, continental collision (eugeosynclines of Kay, 1951) | - orogeny |
| | | rifting, extension subsidence, uplift (taphrogeosyncline and many others of Kay, 1951) | - epeirogenic? |
| Oceanic crust | Stable areas | basins, plains, hot-spots | - no existent terminology |
| | Mobile areas | ridges, trenches | |

defined as "epeirogenic" currently is. Thus, in proposing retention of the original meanings of the terms orogeny and epeirogeny, Hermes and Borradaile (1985) should have discussed more fully the broad range of "epeirogenic" activity that exists prior to making their recommendation.

While we agree with Hermes and Borradaile (1985) that orogeny is not always a short-term process, similarly, epeirogeny is not always slow or long-lived. Officer and Drake (1985) describe epeirogenic activity on the order of 10,000-100,000 years, which is considerably shorter than the time scale that Hermes and Borradaile (1985) discuss for Archean "epeirogenic activity".

In their discussion of Archean terranes, Hermes and Borradaile (1985) had five main points which they felt indicated that Archean tectonic activity could best be described as epeirogenic rather than orogenic. We will discuss each point in turn.

**Archean Metamorphism and Deformation**

First, Hermes and Borradaile (1985) "know of no long ranges of mountains with linear tectonic patterns nor long belts of metamorphism" produced in Archean time. However, modern mountain belts are not simple fold belts, but rather are complex fold and thrust belts, commonly cut by complex shear zones, or in the case of the Cordillera, are collages of suspect terranes. In this context, the Abitibi Subprovince, for instance, represents a fold belt with a strike length of over 800 km, extending from Wawa to Chibougamau, or a tectonic collage (Ludden *et al.*, 1986). Stott (1985) has established the existence of regional scale folded belts of supracrustal rocks in the Uchi Subprovince. Metamorphic belts in a well-established orogenic area (Richardson, 1970; Thompson, 1976) consist of a series of thermal anticlines, therefore Hermes' and Borradaile's (1985) conception that linear metamorphic belts are the norm in orogenic belts is puzzling.

As noted by Hermes and Borradaile (1985), given the present state of knowledge, it may indeed be difficult to equate $D_1$ and $D_2$ ... $D_n$ from area to area in the Archean. However, structural (Colvine *et al.*, 1984) and isotopic (Frarey and Krogh, 1986; Krogh *et al.*, 1984; Corfu and Stott, 1985) evidence suggests that post-tectonic intrusions in the Abitibi and Wabigoon Subprovinces occurred almost universally at 2680 to 2670 Ma. With just one high-precision geochronological study of the absolute age of deformation events (Corfu and Stott, 1985), it is still perilous to correlate deformation events from area to area within one subprovince, nevertheless, the synchroneity of post-tectonic plutons within subprovinces (Krogh *et al.*, 1984; Easton *et al.*, in press) does suggest that inter-area correlation of deformation events may be possible in future, and that deformation within a subprovince may indeed be "belt"-like on a regional scale.

Hermes and Borradaile (1985) assert that regional application of deformation sequences is ruled out by the "absence of long mobile zones bounded by single blocks of more rigid terrane". This sweeping statement is contrary to the observation that, for instance, the Wabigoon Subprovince is bounded to the north by the Winnipeg River Subprovince which contains substantial areas of older crust (Clark *et al.*, 1981; Beakhouse, 1982, 1985b; Krogh *et al.*, 1976) and to the southeast by the Wabigoon Diapiric Axis (Edwards and Sutcliffe, 1980; Thurston and Davis, 1985), an area which includes granitoid gneisses whose field relations (Sutcliffe and Fawcett, 1979; Sutcliffe, 1986) suggest that they are older than the supracrustals of the Wabigoon Subprovince.

**Table 2** Molasse occurrences of Archean age in the Superior Province of Ontario

| Subprovince | Location | Source |
|---|---|---|
| Uchi | Bamiji Lake | Wallace (1985) |
| | Birch Lake | Ayres and Thurston (1985) |
| | Oxford Lake | Hubregtse (1976) |
| Wabigoon | Sioux Lookout | Turner and Walker (1973) |
| | Conglomerate Lake | Amukun (1980) |
| | Crowdoch Lake | Beakhouse (1985a) |
| | Sunshine Lake | Blackburn *et al.* (1985) |
| Abitibi (Wawa) | Shebandowan | Shegelski (1980) |
| | Dore conglomerate | |

**Archean Sedimentation and Tectonism**

In their second point, Hermes and Borradaile (1985) suggest that Archean tectonism was restricted in extent and related to short-lived centres of uplift. As such, Hermes and Borradaile (1985) suggested that the Archean is characterized by the absence of one of the marks of younger mountain belts: widespread flysch and molasse. Flysch is an extensive pre-orogenic sediment produced by rapid erosion of an adjacent rising mountain belt, which, in some regions, is characterized by abundant turbidites.

Extensive turbidites found within Archean sedimentary terranes such as the English River Subprovince (Breaks *et al.*, 1978) or the Quetico Subprovince (Percival and Stern, 1984) represent a felsic and mafic volcanic provenance from adjacent greenstone belts. These sediment-rich zones have lateral continuity of greater than 500 km of strike length. Archean sediments in greenstone belts also represent continental margin (Barberton and Pilbara) or continental shelf sequences (Zimbabwe)(Bickle and Eriksson, 1982).

The general pattern in greenstone belts is one of basal volcanic sequences overlain by fluvial sediments rarely, more typically extensive turbidites giving way upward to fluvial sediments (Eriksson, 1981). Bickle and Eriksson (1982) suggest that basin subsidence in the Archean is regional in scope comparable in mechanical development to modern sedimentary basins which involve the entire lithosphere, and that the stratigraphically high turbidites of the Canadian greenstone belts or indeed the basal turbidites of the Indian greenstone belts (Goodwin, 1981) can be regarded as flysch of regional extent. Indeed, in the Zimbabwean greenstone belts, Wilson *et al.* (1978) have proposed regional correlations which suggest that craton-scale flysch deposits occur. Regional-scale turbidite wackes in the English River Subprovince have been correlated with three cycles of volcanism during the interval 2960 to 2740 Ma (Thurston and Breaks, 1978). The extent of the deposits (650 km), the consistent lithofacies (Breaks *et al.*, 1978; Thurston and Carter, 1970), and the consistent association with banded iron formation at Bruce Lake (Breaks *et al.*, 1978), Lake St. Joseph (Meyn and Palonen, 1980), and Miminiska (Wallace, 1981) suggest these sequences represent an example of Superior Province flysch.

Now to the question of Archean molasse deposits; which are post-orogenic sediments derived from erosion in a fluviatile setting after the early phases of a folding event. In many young mountain belts, molasse occurs in narrow "pull-apart" basins associated with regional scale transcurrent faults (Miall, 1981; Schubert, 1980; Norris and Carter, 1982). The Timiskaming type units in the Archean are a mixed alkaline to calc-alkaline volcanic and fluviatile sedimentary package which generally unconformably overlies a typical volcanic-rich greenstone belt sequence (Ayres and Thurston, 1985). Structural studies by numerous authors (Poulsen, 1984a; Shegelski, 1980; and references therein), show that Timiskaming type units have been deposited post-$D_1$ and pre-$D_2$, where local deformation sequences have been worked out. Poulsen (1984a, b) has shown that preserved Timiskaming-type sequences have been deposited in "pull-apart" basins analagous to molasse in younger fold belts. A compilation of these occurrences is given in Table 2. We believe that it is safe to conclude that Archean pull-apart basins are comparable in extent to those in modern fold-belts.

**Archean Folding**

Third, Hermes and Borradaile (1985) note that in Archean sequences, recumbent folds are rare and where they exist, they are not always accompanied by a penetrative fabric-forming event. The predominant Archean fold types with steep axial surfaces and sideways closing are commonly adjacent to granitoid domes and are attributed (Borradaile, 1982) to the tilting of early recumbent folds during later deformation. Hermes and Borradaile (1985) suggest that recumbent folds produced without generation of a penetrative fabric essentially by gravity sliding are "not normal in linear post-Archean "orogenic belts"". Although, it may not be "normal", it is observed along the flanks of the Sierra Nevada batholith, although those nappes do possess a cleavage. Recumbent folds without development of penetrative fabric are present in the upper sequence of many metamorphic core complexes in the Cordillera (Coney, 1984). If core complexes are the mark of a Hercynian-type orogen, then Hermes' and Borradaile's (1985) assertion that recumbent folds without penetrative cleavage do not form part of modern orogens is open to question.

### Basement in the Late Archean

Their fourth point was that without the existence of a large identifiable area of older basement, the comparison of Archean terranes with modern orogenic belts is fraught with peril. Yet, is basement rare in the Archean? It is true that basement-supracrustal contacts in granite-greenstone terranes are rarely preserved, and that late granitoid bodies commonly intrude along the basement-supracrustal interface obliterating the contact relationships. We feel, however, that there is ample evidence for Archean pre-greenstone basement in the Superior Province based on two lines of evidence.

(1) ***Geochronology:*** Many granitoid bodies are older than surrounding greenstones (Davis *et al.*, 1982; 1985; Davis and Trowell, 1982; Krogh *et al.*, 1976).

(2) ***Unconformities:*** Greenstone-basement unconformities are present in both the Superior (Joliffe, 1955; Baragar and McGlynn, 1976) and Slave (Stockwell, 1933; Henderson, 1975) Provinces. When granitoid areas of the Superior Provinces are examined, many areas consist of elliptical bodies of foliated Na-rich granitoids and meta-igneous gneisses cut by later massive granitoids (Edwards and Sutcliffe, 1980; Card, 1979; Card *et al.*, 1980). Limited geochronology to date suggests that sodic granitoids, foliated granitoids, and meta-igneous gneisses are generally older than the surrounding greenstones (Krogh *et al.*, 1976; Thurston and Davis, 1985; Corfu and Andrews, in press; Noble *et al.*, 1985) and field relations are almost universally consistent with this view. In general then, in our view, there are substantial areas of pre-greenstone granitoids. Therefore assertions about the scarcity of Archean basement are not in accord with the available evidence.

As to the nature of the unique composition of the Archean crust, we point the authors to Taylor and McLennan (1985) who provide an exhaustive review of the subject and present a best estimate for Archean upper, lower, and total crust composition. If Archean crust is unique, it is due to the presence of komatiites, and the dominance of sodic granitoids. There is growing evidence that Archean volcanism is dominantly bimodal (MacGeehan and MacLean, 1980; Thurston *et al.*, 1985); and it is misleading to state that "40% of the now visible Archean crust is composed of volcanic rocks with tholeiitic and calc-alkaline rocks in a 3:2 ratio".

### Isotopic Ages

Fifth and finally, Hermes and Borrdaile (1985) draw attention to the nearly uniform ages of large areas of the crust that have been thermally active since the Archean, and cite these as evidence that Archean tectonics were epeirogenic phenomena. We have three main arguments against this evidence.

First, K-Ar and Rb-Sr ages in the Slave and Superior Provinces are consistently 100 to 150 Ma younger than corresponding U-Pb zircon ages on plutonic, volcanic and deformational events (Easton, 1985a,b; 1986a,b). Thus, there is most likely a significant time interval, on the scale of modern ocean basin opening and closings, between the tectonic events which ended greenstone belt development and those which were responsible for this regime of crustal stabilization and isotopic adjustment. The possibility does exist that the same tectonic process, even with the time separation, may ultimately have caused both events. An alternative interpretation is that the difference in U-Pb zircon and K-Ar and Rb-Sr ages does represent a 100 to 150 Ma period of cooling of the crust after greenstone belt development. However, this epeirogenic or cymatogenic cooling period does not preclude the possibility that greenstone belt development was orogenic.

Second, K-Ar and Rb-Sr ages are consistently younger than U-Pb zircon ages in other mobile belts of Proterozoic (Wopmay Orogen, Bear Province, Easton, 1983; Churchill Province, Stockwell, 1982; Grenville Province, Easton 1985b, 1986a, b; St. Francois Mountains, Missouri, Bickford and Mose, 1975; Australia, Page, 1976) and Phanerozoic age (Appalachian Orogen, Rankin *et al.*, 1983). Since this pattern is common in younger orogenic belts, it could be argued that these K-Ar and Rb-Sr "cooling" ages are evidence for orogenic rather than epeirogenic activity in the Archean.

Third, Hermes and Borradaile (1985) refer to some early studies that refer to elongate-domal or belt-like contours of Phanerozoic orogenic belts (Dewey and Pankhurst, 1970; Borradaile and Hermes, 1980). Similar contours were recognized in the Grenville Province (Harper, 1967; Baer, 1976); which consequently then, should be considered as orogenic. However, additional age data in the Grenville shows that these contours are in part an artifact of limited data, or the misinterpretation of the effects of other geologic features (Easton, 1986a, b). In particular, the contours plotted around these ages are commonly the same as, or less than the error limits on the ages, and thus, are not statistically meaningful. Excess Ar effects were not considered, and in the case of the Grenville Province, these are particularly noticeable near the Grenville Front, and serve to increase ages (Wanless *et al.*, 1970; Dallmeyer and Rivers, 1983; Easton, 1986b). A variety of rock types and dated materials are commonly included in these contour plots, and regional geology and metamorphic isograds are not taken into consideration, all of which serve to distort the cooling history of an area. In the case of the Grenville, a data set screened to eliminate or minimize these effects did not show an elongate-domal or belt-like pattern (Easton, 1985a, 1986a, b). Thus the presence or absence of these age contours cannot be used as evidence for orogenic or epeirogenic activity. Again, in the case of the Grenville Province, Ar-Ar cooling histories (York, 1984; Berger and York, 1981a, b; Lopez-Martinez and York, 1983; Clark and Hanes, 1985; Dallmeyer and Rivers, 1983) show very complex cooling histories even in geographically close areas, making it unlikely that the cooling process was regular and belt-like. Similar Ar-Ar studies are needed in the Archean to better constrain geologic events after the cessation of greenstone belt activity at ca. 2660 Ma in the southern Superior Province and 2640-2620 Ma in the Slave Province before definitive statements can be made about the cooling history of the Superior Province after the "Kenoran Orogeny".

### Summary

In summary, while we agree with Hermes and Borradaile (1985) that orogeny and epeirogeny are useful terms that should be retained, it is probably incorrect at present to assume that diastrophism in the Archean was purely an epeirogenic process. We would like to close with the thought that although tectonic process in the Archean may be characterized by a number of distinctive traits, it does not necessarily follow that the Archean cannot be understood by comparison with Phanerozoic and younger orogenic belts.

Combined references for ***COMMENTS AND REPLY*** begin on p. 127.

## REPLY

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Our article (Hermes and Borradaile, 1985) was clearly not intended as a monograph on orogeny and epeirogeny. Our objective was to illustrate that orogeny is not a short-lived (Stillean) event of an episodic nature nor should it be equated with rock deformation in general. We still maintain that the term orogeny, in its classic sense, is a useful term for international communication and from the original literature we contrasted this with epeirogeny. Never did we imagine that anyone could misinterpret us as proclaiming that these are the only two modes of tectonic response. Much of Thurston's and Easton's discussion is parochial, focussing on examples from the Superior Province which, in its elongate subprovince structure is atypical of Archean regions, or on specific items from the literature. In the latter cases they show that from the complexity of nature it is always possible to select a specific example to augment an argument.

Thus we return the readers to the theme we actually discussed. The term orogeny has a certain connotation for the majority of geologists in the world who do think of orogeny in the senses of Gilbert (1890) and Upham (1894). To recapitulate Gilbert (1890):

> "The displacements of the earth's crust which produce mountain ridges are called orogenic ... Having occasion to contrast the phenomena of the narrow geographic waves with those of the broader swells, I shall take the liberty to apply to the broader movements the adjective epeirogenetic ... The process of mountain formation is orogeny ..."

and from Upham (1894) on orogeny:

> "... processes of formation of mountain ranges by folds, faults, upthrusts and overthrusts affecting comparatively narrow belts and lifting them up in great ridges ..."

As an example of successful international communication in this area, we cite the project "Data for Orogenic Studies" (Spencer, 1974). Therein 81 authors provided objective material on 46 different orogenic belts. All of these contributors used the original concept of orogeny, not tied to any particular hypothesis or model. (Incidentally, nowhere in these works do we find any lithological or tectonic patterns comparable to Archean greenstone belts.)

If workers in an Archean Shield loosely use "orogeny" as a synonym for some aspect of tectonism with disregard to the original definitions, they mislead the worldwide community of geologists.

Moreover, we must remember that in a subject such as tectonics, the nature of the database precludes any individual from having first-hand experience of each type of region and each type of tectonic scheme. Since we must rely, therefore, on the written word, it is essential that we honour historical precedent in the literature. Our recommendation (Hermes and Borradaile, 1985, p. 149) was that: "New hypotheses should be expressed more fully if we wish to preserve communication for an international audience, rather than to redefine simple, useful terms." In this regard we note Thurston's and Easton's discomfort, for they refer to the *Kenoran "orogeny"* — the quotation marks are theirs!

In conclusion, we wish to emphasize the way in which certain concepts overlap, and the restricted way in which Gilbert's and Upham's useful terms are applied. In Figure 1a, we show the essence of a few relevant concepts ... D, E, F, G ... within the field of tectonic deformation (*sensu lato*).

In Figure 1b, we indicate the separateness of terms that were originally used to define some concepts and groups of concepts. Orogeny is clearly defined in terms of simple concepts (*cf.* Figure 1a and Figure 1b). For a few authors, cited in our original article, the term was more encompassing — occasionally its use mistakenly (or deliberately?) gave the impression that it would be equated with all rock deformation or was defined by some microstructural or radiometric event. We hesitate in speculating what Thurston and Easton understand by orogeny (after all they can equate "basins" and "shields" with stable areas, their Table 1), but it might include for them parts of F and E in Figure 1a.

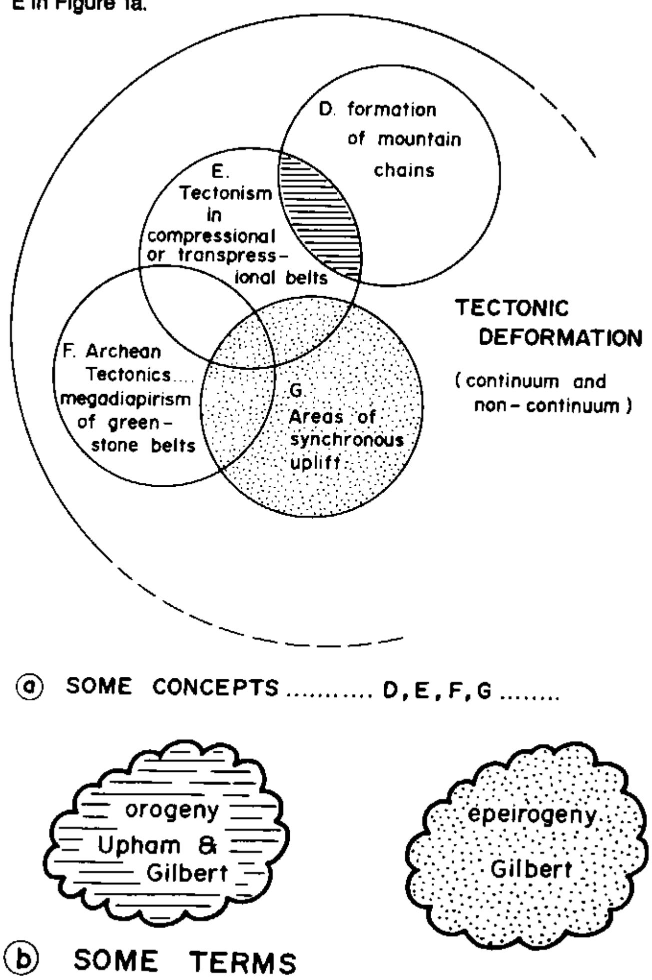


**Figure 1** *(a) Some processes are illustrated (... D, E, F, G ...) in terms of simplified elements within the overall field of tectonic deformation. Partial overlaps do not have areas proportional to the importance of the overlapping conditions or the probability of their occurrence. It is not yet known if D and F should have some overlap.*
*(b) the concepts of Gilbert and Upham are illustrated with ornaments corresponding to the processes in Figure 1a.*

## COMMENT

# Epeirogeny: Is It Really Orogeny or Theology?

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Debates such as those between Hermes, Borradaile, Thurston and Easton (published in previous pages of this issue) are reminiscent of theological debates about the meaning of terms appearing in the Bible. Is it fruitful to attempt to go back and read the minds of earth scientists of another era to try to find out what they really meant? Should we still be discussing terms that may have outlived their usefulness? We now know so much more about surface geology and crustal processes than did Gilbert, Stille and Gilluly at the time they made their well known contributions. We now have a vast body of theory and hypothesis relating to crustal and mantle dynamics that goes under the general term "plate tectonics" to aid us in our interpretations. The interminable debate about the terms "flysch" and "molasse" is another of these debates about old, pre-plate tectonic terms, a debate which has no end, and to which this writer reluctantly admits having contributed (Miall, 1984a).

The purpose of this note is to discuss the proposition that there may be no such thing as epeirogeny. Using our knowledge of modern plate tectonic processes it can be argued that epeirogeny is not occuring at the present day. Most researchers now accept the validity of plate tectonic interpretations for the post-Gondwana break-up of the earth's continents and their subsequent interaction. Most are willing to use plate tectonics as a basis for working hypotheses about the entire Phanerozoic record, and many extend this back to the early Proterozoic. I would argue, therefore, that there may not have been any "true" epeirogenesis since at least the end of the Archean. I deliberately exclude the Archean from this discussion, having no first-hand experience of Archean geology to draw on.

The basis for this proposition is that, increasingly, all tectonic processes can be shown to relate to events at plate margins, including those occuring in the middle of otherwise stable cratons. It therefore makes no sense to arbitrarily separate out the effects of these processes into two categories, epeirogeny and orogeny, implying that there are fundamental differences in the causes of these classes of events.

Cratonic uplift or upwarp (including "basement uplift") and cratonic basin subsidence, usually slow and long lived, are the events most often categorized as epeirogenic. Mantle plumes and phase changes in the deep crust or mantle are usually invoked as the causes of these processes. To my knowledge the concept of phase changes has never been proven. The idea of mantle plumes, one of the earlier bases for plate tectonic hypotheses, has also been controversial. Some intraplate and spreading-centre volcanism, such as in Iceland, the Walvis Ridge, and in the Emperor-Hawaiian chain, seem to be explicable using the mantle plume hypothesis, but broad, regional, intracratonic events, of the type usually labelled as epeirogenic, are not readily attributable to mantle plumes.

Continental glaciation results in crustal loading and depression and sea-level fall during times of maximum ice volume, with subsequent rapid isostatic rebound during interglacial periods. The evidence of raised and tilted beaches, relict sediment blankets on continental shelves, and the stratigraphic evidence of Quaternary transgressions and regressions, are ample evidence of these processes, which are often compared to classic Phanerozoic epeirogeny because of the "broad swell"-like nature of the disturbance, to use Gilbert's original term.

If we examine some classic examples of pre-Quaternary epeirogenic events we find that in every case, with the exception of those noted above, they can be explained by reference to plate-margin processes. Plates are rigid, in the short term, and are capable of transmitting plate margin stresses for thousands of kilometres into plate interiors. In the longer term they behave in a visco-elastic manner.

Broad, regional disconformities and unconformities, of the type first described by Sloss (1963) have commonly been attributed to cratonic upwarp (*e.g.* Sloss, 1984). However, we now suspect that many are the product of eustatic sea-level change, caused by long-term changes in the volume of oceanic spreading centres as a result of changes in global spreading rates (Donovan and Jones, 1979). Cloetingh (1986) has also demonstrated that intraplate stresses resulting from plate collision can cause tilting and sea-level change thousands of kilometres away on the opposite side of the plate.

Basement intracratonic uplifts are commonly thought to be epeirogenic in origin. Examination of a few classic examples shows that this is probably a misleading way to interpret them. The Colorado Plateau is a good example. Cross (1986) now interprets Cenozoic uplift of the plateau as the product of shallow subduction of the Farallon Plate. Laramide-type "basement uplifts", such as the Wind River Fault in Wyoming, have now been shown, by deep seismic reflection profiling, to be caused by thrust faulting extending to depths of at least 24 km, possibly to depths exceeding 30 km, indicating decoupling near the base of the crust (Smithson *et al.*, 1978). They are therefore caused by compressional tectonics related to convergent plate movements on the west margin of the Cordillera, not to vertical cratonic uplift. The Boothia Uplift, in the Canadian Arctic, was interpreted as a cratonic basement uplift by Kerr (1977), but later work suggests that it, also, may have been produced by deep-seated thrust faults (Miall, 1983; Okulitch *et al.*, 1986), and Miall (1986) suggested that the uplift reflects stresses transmitted across the North American plate during the Caledonian plate collisions with Europe.

Many cratonic arches, such as the Cincinnati Arch, the Findlay Arch, the Algonquin Arch and the Sweetgrass Arch, have been attributed to epeirogenic movement. Quinlan and Beaumont's (1984) elegant study now shows that these are flexural bulges in the crust produced by thrust sheet loading during the development of foreland basins. Again, the ultimate cause is crustal shortening during convergent plate movements.

All the intracratonic uplifts noted above may represent older structures that have been reactivated, or depend on pre-existing fabrics for their position and orientation.

The Basin and Range province of the Western Cordillera, although "epeirogenic" in the sense that it encompasses a widely distributed and distinctive pattern of faulting and volcanism, is "orogenic" in the sense that the origin of the distinctive structural style is related in some way to Cordilleran convergent plate movement and terrane accretion. Part, at least, of the province, consists of symmetric half-grabens, possibly with major low-angle detachments at depth (Allmendinger *et al.*, 1987). This structural style probably resulted from gravitational collapse of a tectonically thickened crust.

What of the classic epeirogenic cratonic basins such as the Williston, Illinois and Michigan Basins? It is important to separate our ideas about the initial creation of these basins from those invoked to explain the subsequent history of subsidence and filling. The work of Beaumont (1981) and Quinlan and Beaumont (1984) shows that much of the history of sedimentation can be related to the crustal response to thrust-loading and the generation of foreland basins, specifically the flexural interaction between the foreland peripheral bulges and the basins themselves. This brings us back to the question of the origins of these basins. The conventional epeirogenic models of heating, thermal subsidence and/or mantle phase changes have met with little support, from lack of direct evidence. Miall (1984b, p. 442) summarized other suggestions. Many cratonic basins are located over rifts or sutures in the Precambrian basement, which may have persisted as lines of weakness, and the basins may have been initiated by rejuvenation of these zones by later crustal stresses transmitted horizontally from plate margins.

Many well-established rift systems are located along earlier, commonly Precambrian, lineaments (see discussion in Miall, 1984b, p. 372). It remains debatable whether such rifts are "active", in the sense that they were caused by asthenospheric upwelling (which could be classified as an epeirogenic process), or "passive", that is to say, caused by differential stresses during plate evolution (Baker and Morgan, 1981). The latter seems more likely. The deep, linear cratonic basins that the Russian geologists termed aulacogens are also related to plate margin rifting, rather than to a specific class of intracratonic epeirogenic processes (Burke and Dewey, 1973). Lake Baikal, a rift basin deep in the Asian continental interior, probably originated as a result of oblique crustal extension during the Himlayan Orogeny, when convergent stresses caused by the Indian collison were transmitted thousands of kilometres across the interior of what is now China and the Soviet Union, generating systems of sinistral strike slip faults throughout west China (Molnar and Tapponnier, 1975).

To conclude: what are we left with to call epeirogenic? Perhaps mantle plumes; perhaps mantle upwelling as a cause of "active" rifts - if such a process has, in fact, occurred. This is a much more restricted use of the term than that implied by the original definition and that which has evolved through subsequent useage. Perhaps epeirogeny is another tectonic fossil that should be quietly abandoned.

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