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THE PLACE AND POWER OF MYTH IN GEOSCIENCE: AN ASSOCIATE EDITOR'S PERSPECTIVE

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ABSTRACT. Distinguishing between myth and science is subtle, for both seek to understand the things around us. The characteristic style of mythic thinking is to place special emphasis on a selective conjecture, based typically on the initial observation or recognition of a phenomenon, which is thereafter given privileged status over alternate interpretations. Concepts in geoscience are quite commonly mythic in that sense. The outdated notion of geosynclines as deterministic precursors of orogeny is an apt example, as are central current ideas about suspect terranes, mantle plumes, and global sequence stratigraphy. Geomyths stimulate investigation, but also may retard further progress by dismissing contrary views. Improved understanding of geologic history could be attained more efficaciously by appreciating the mythic quality of many nascent ideas in geoscience, and resisting the temptation to accord geomyths favored status over competing hypotheses.

INTRODUCTION

In our discourses, we commonly counterpose myth, taken to be imaginary, and science, conceived as an approach to reality. Both myth and science, however, strive to explain the same features of the natural world. Distinguishing between the two is difficult, posing a challenge that accounts for the persistence of pseudoscience in popular culture.

In the days when a god supposedly drove a chariot of fire across the heavens to make the sun shine, that explication of night and day seemed no more mysterious to most people than life itself. It is not easy even now to define myth separately from science. Each attempts a comprehensive view of our surroundings, and each provides an explanation for some salient package of observations giving rise to quandary. It is perhaps the defining style of mythic thinking to build selectively upon some particular conjecture that is given privileged status against contradictory observations. Rejecting any myth typically requires more conclusive evidence than its initial invention employed.

BACKGROUND

I contend that geomyths have played a role in the evolution of our science, and are still with us today. A preordained geotectonic cycle, derived conceptually from the durable myth of geosynclines, held center stage only a few decades ago until plate tectonics showed a better way to reconcile observation with concept. At present, tectonics has its suspect terranes, petrology its mantle plumes, and stratigraphy its global sequences. Each of these concepts is a myth in the sense that each expands selectively from a narrowly focused appraisal of an issue to embrace far-ranging analysis, and each places an extra onus of argumentation upon potential detractors.

The logic of any geomyth rests upon an inductive leap extrapolated from some particular observation or assumption inferred to lie at the root of an issue being addressed, and rejects the spirit of the familiar method of multiple working hypotheses (Chamberlin, 1897). The style of myth instead posits dominance of a single perception to the exclusion of others. This approach makes geomyths difficult to falsify, yet most agree (with Gilbert, 1886) that essential tests of scientific ideas must embody attempted disproof, rather than a search for confirmation (Popper, 1962).

Geomyths tend to be more durable but also less specific than leading or ruling hypotheses. Hypotheses are abandoned, or changed in substantial ways, if their predictions fail. When predictions of the extant version of a geomyth fail, however, the characteristic response is to change underlying assumptions, or evaluations of constraints, in ways that keep the core of the geomyth essentially intact.

My examples of geomyths are drawn from my own personal research experiences, and those with different interests and expertise could doubtless highlight others. Applying the term myth to an idea is not meant to be pejorative, but to describe the manner of its origin and retention.

GEOSYNCLINAL THEORY

The idea that geosynclines predestine orogeny was built on the valid observation that folded sedimentary strata of the valley-and-ridge province of the Appalachian chain are thicker than flat-lying strata of equivalent age beneath the Appalachian Plateau to the west (Knopf, 1948). As ultimately developed, the logic linking subsidence and thick geosynclinal sedimentation to later mountain-building along the same linear trend gave rise to the concept of a fixed geotectonic cycle (Cady, 1950), holding that the inherent nature of geosynclines leads inexorably to paroxysmal orogeny. Diverse views of the impulses for geosynclinal subsidence and the nature of geosynclinal sedimentation emerged over the decades, but the presumed genetic linkage between geosynclines and orogeny persisted despite shifting viewpoints (Hsü, 1973).

The notion survived the observations that some mountain belts lack excessively thick strata, and that some thick piles of sediment seem never to have fostered orogeny. These aberrant observations, in apparent conflict with theory, were treated conceptually by supposing that some otherwise satisfactory geosynclines were simply starved for sediment, and that some thick prisms of sediment are not "true" geosynclines. The latter were termed *ortho*geosynclines, composed of parallel eugeosynclinal and miogeosynclinal belts, to distinguish them from comparably thick sedimentary accumulations that fail to foreordain orogeny (Kay, 1951).

Caught up myself in the seductive geosynclinal web of logic, I once argued on the basis of andesite geochemistry (Dickinson, 1962) that eugeosynclinal crust similar to the modern crustal profile has been present since mid-Paleozoic time beneath western parts of the North American Cordillera in ground we now infer to have been accreted from ocean to continent almost entirely after mid-Paleozoic time. My arguments were largely circular, however, and appreciating the mechanisms of plate tectonics soon showed that eruptions of andesite are related to plate descent along subduction zones (Dickinson, 1970), rather than to any supposedly eugeosynclinal environment of crustal magmatism.

The advent of plate tectonics made the geosynclinal myth unnecessary. Various kinds of "geosynclines", ortho- or not, were seen by actualistic analogy as different types of sedimentary basins in different plate settings (Mitchell and Reading, 1969; Dewey and Bird, 1970; Dickinson, 1971a). No one bothered with formal refutation of the geosynclinal theory of orogenesis, but most moved at once to other models for orogeny as a function of plate interactions (Dewey and Horsfield, 1970; Dickinson, 1971b). Geoscientists are still engaged collectively in modifying and improving the plate models. Geosynclines are precursors of mountain belts only if we define the stratified rocks of mountains as geosynclinal deposits, but this is tautology without fundamental meaning (Hsü, 1982).

Coney (1970) picturesquely discarded the traditional geotectonic cycle: "saying geosynclines lead to orogeny is a little like saying fenders lead to automobile accidents". He had in mind, of course, the deformation that crumples and dislocates thick sedimentary accumulations deposited along continental margins when an intervening ocean basin is closed by plate consumption. Having no further need for the mythology of geosynclines, we essentially dropped the word from our geovocabulary, saving only the derivative term *miogeocline* (Dietz and Holden, 1966) for sediment prisms deposited along passive continental margins.

SUSPECT TERRANES

Terrane analysis was devised as an antidote for overly facile plate interpretations of orogenic belts, with the North American Cordillera as exemplar (Coney and others, 1980). The aim was to define objectively a finite number of fault-bounded components of an orogen as strictly descriptive entities, each with its own consistent internal stratigraphy (including basement where exposed) not demonstrably contiguous with the stratigraphy of adjacent terranes. Named terranes bear a hierarchic relationship to orogenic systems as a whole similar to the relationship, in American stratigraphy, of formally named formational units to overall depositional systems. Each *tectonostratigraphic* (or *lithotectonic*) terrane is treated as a separate entity because it displays a degree of internal continuity that is interrupted at terrane boundaries more abruptly than facies change can readily explain (Keppie and Dallmeyer, 1991).

Orogenic terranes displaying stratigraphy that contrasts with the cratonal stratigraphy of a continental interior (Laurentia in the North American case) are designated "suspect" terranes, meaning that they might have been accreted to the continental block by tectonic transport which occurred after some or all the rock masses within each suspect terrane were formed. Explicit recognition of suspect terranes can be helpful in dissecting the accretionary anatomy of orogenic belts (Williams and Hatcher, 1982; Monger, 1993), although a widespread tendency to regard all terranes designated suspect as necessarily exotic to the adjacent craton is a questionable approach (Hudson, 1987).

For evaluating potential movements of suspect terranes, heavy emphasis is placed in practice on remanent magnetizations preserved by rock masses within suspect terranes as apparent recorders of paleolatitude (Irving, 1979; Irving and Wynne, 1990; Beck, 1992; Irving and others, 1996). This emphasis derives from the perspective that paleomagnetism is a more objective criterion than geologic interpretations, which may be faulty (Coney, 1989). Discordances between paleomagnetic vectors recorded by suspect terranes and the vectors expected from their present positions with respect to adjacent cratons are accordingly taken as presumptive evidence for tectonic transport of the suspect terranes relative to the cratons.

Arguments that some paleomagnetic discordances within deformed orogenic belts can be interpreted instead as the results of pluton tilt, sediment compaction, or acquisition of remanence during the folding of tilted strata call this presumption into question (Butler and others, 1991, 2001, 2002). Preferential reliance on the paleomagnetic signature of deformed rocks as a faithful record of paleolatitude has the hallmark of mythic thinking, and seems unnecessarily restrictive. Evaluation of terrane motions can be made with confidence only by adopting a multidisciplinary strategy (Cowan, 1994; Cowan and others, 1997; Mahoney and others, 2000). In its absence, conflicts between paleomagnetic and other geologic evidence for and against postulated terrane displacements (Irving and others, 1985 *vs.* Vandall, 1993; Gastil, 1991 *vs.* Beck, 1991; Monger and Price, 1996 *vs.* Wynne and others, 1996) seem likely to persist unresolved.

There is no inherent contradiction between terrane analysis and plate tectonics, for the history of any terrane can be interpreted in the context of plate models (Dickinson and Lawton, 2001). Terrane analysis proceeds initially, however, without regard to specific plate models, from the perspective that terrane amalgamation results from obscure plate interactions that are too uncertain to provide a reliable guide to terrane relationships (Howell and others, 1985). In this context, the differentiation of discrete terranes can be viewed as a goal unto itself. Without close attention to the varying tectonic nature of terrane boundaries (Ernst, 1984; Şengör 1990; Şengör and Dewey, 1990; Hamilton, 1990), terrane nomenclature can proliferate (Karl and Mull, 1993) without elucidating generic tectonic relationships. Allegiance to the original spirit of terrane analysis requires striving beyond simple delineation of terranes, and documentation of their paleomagnetic signatures, to perceive the plate interactions that produced their internal character and placed them in close juxtaposition.

MANTLE PLUMES

Once it was understood that plate tectonics is the "way the world works" (Wyllie, 1976), it became clear that most volcanism on Earth occurs either along midocean ridges and other rifts at divergent plate boundaries, or along magmatic arcs where eruptions are triggered by plate descent at convergent plate boundaries (Martin and Piwinskii, 1972). The sites where intraplate volcanism of lesser net volume is concentrated were termed "hotspots", initially with no specific connotation of origin.

By recasting the observation of Chubb (1957) that linear chains of intraplate Pacific islands and seamounts commonly display a monotonic age progression, Wilson (1963) argued that each was formed as the Pacific plate drifted over a long-lived hotspot in the underlying mantle. Motion of Pacific lithosphere over deeper mantle is so rapid (~100 mm/yr) that any magma hearth active for even a few million years can generate a hotspot track hundreds of kilometers long (Dickinson, 1998).

To bring hotspot behavior under the logic of a global system, Morgan (1971) postulated that each salient hotspot is the place where a hot mantle plume, rising by columnar advection from near the core-mantle interface, reaches the surface of the Earth. Such deep-seated features ought to be almost fixed in position within the mantle, relative to the rapid motions of more surficial plates, hence potentially define a spatial framework from which to gauge "absolute" rates of plate motion.

Although the issue remains controversial (DePaolo and Manga, 2003 *vs.* Foulger and Natland, 2003), much of the extant petrologic and tectonic literature accepts the equivalence of mantle plumes and magmatic hotspots even though observed hotspots fail many plume predictions. Geometric analysis has shown repeatedly that all oceanic hotspots cannot be fixed relative to one another, because mutually fixed hotspots would generate hotspot tracks at azimuths incompatible with established relative plate motions (Norton, 2000). Moreover, some Pacific island-seamount chains are the products of multiple or migratory hotspots, or involve eruptions distinctly out of age-space sequence (Dickinson, 1998).

On the other hand, a range of igneous phenomena not linked directly to hotspot tracks have also been attributed to plume activity. For example, isolated hotspots distributed over wide areas have been attributed to plume magma that spreads laterally beneath the lithosphere for thousands of kilometers from a single deep-seated columnar source (Ebinger and Sleep, 1998). Voluminous but distinctly transient flood-basalt eruptions have been attributed to the effects of plume heads impinging on the surface ahead of rising plume tails (Richards and others, 1989; White and McKenzie, 1995).

Arguments that plume theory is fatally flawed (Anderson, 1998; Hamilton, 2002) have met a mixed reception by plume enthusiasts. If one is permitted conceptually to vary the shapes and sizes of plumes, to allow plume magma to travel arbitrarily long distances just beneath the lithosphere, and to vary the intensity of advection by postulating plume heads and tails of different volume and buoyancy as circumstances require, then plume theory becomes too flexible to test by observing surface geology. To date, however, seismic tomography has failed to provide robust evidence for narrow plumes at depth (Fukao, 1992; Kárason and van der Hilst, 2000).

If plumes are but another attractive geomyth, clinging to the plume concept can only serve to delay recognition of differences among hotspots, which may reflect a range of mantle phenomena, each potentially informative of Earth behavior but all now masked under the plume umbrella. Viewing "plume" as just shorthand for any of various kinds of anomalously hot mantle seems to me a liberating point of view. A recent analysis of the Yellowstone hotspot (Christiansen and others, 2002) indicates the complexity of the geologic processes that may give rise to igneous activity commonly ascribed to mantle plumes.

GLOBAL SEQUENCES

The notion of unconformity-bounded stratal sequences (Posamentier and Allen, 1999), inferred to be eustatically controlled and globally correlable, was developed to interpret the vast body of previously unavailable subsurface stratigraphic information generated by seismic reflection profiling of sedimentary basins. The need for a fresh means of correlation was pressing because normal methods of biostratigraphy are impractical to apply to the huge volumes of unsampled strata imaged by seismic reflection.

This analytical challenge was putatively solved by the remarkable postulate that seismic reflectors are chronostratigraphic horizons (Vail and others, 1977; Vail, 1992), constituting a graphic proxy for biostratigraphy rather than being just another format to display physical stratigraphy. This conclusion is clearly untenable in a literal sense because seismic reflection is a function of acoustic impedance, defined as seismic velocity times rock density (Sheriff, 1977). Variations in physical rock properties cannot be rigorously time-dependent.

The assumption that seismic reflectors are chronostratigraphic must rest, therefore, on trust that lateral migration of the sedimentary events or environments responsible for depositing stratigraphic layers of consistent physical character and resultant properties is rapid with respect to time frames of interest for stratigraphic correlation. Even though the generation of each reflector requires a finite interval of progradation or lateral migration of process, the requisite time interval can be short in comparison to the time frame relevant for correlation.

It seems likely, however, that strict equivalence of stacked reflector patterns with chronostratigraphy is a claim that bears close examination before full acceptance in any specific case. The conceptual problem involved is illustrated by the standard treatment of unconformities in seismic stratigraphy. Although the length of the hiatus at a given unconformity is quite properly taken to vary areally, little attention is paid in practice to the truism that a given surface of unconformity may represent, from place to place, different intervals of time that do not overlap. A salient type of time-transgressive unconformity is formed by progressive coastal ravinement as marine transgression advances on a subsiding lowland coast receiving sediment throughout the interval of shifting strandlines (Nummedal and Swift, 1987).

Adherence to the rubric that seismic stratigraphy is chronostratigrapic has led to the recognition of supposedly synchronous global stratigraphic sequences controlled by cyclic eustasy (Haq and others, 1987, 1988; Hardenbol and others, 1998), which is undetectable by independent means and for which there is no known mechanism operative at the tempo inferred. Critics have pointed out that autocyclic sedimentary processes influence local seismic stratigraphy as strongly as allocyclic processes such as eustasy (Poulsen and others, 1998), that much sequence stratigraphy reflects regional tectonism (Summerhayes, 1986; Ettensohn, 1994; Nystuen, 1998) or fluctuating sediment supply (Fulthorpe and Carter, 1989) rather than eustasy, that the stratigraphic evolution of basin margins is influenced by flexural effects from sediment loading that can mimic supposed eustatic controls (Watts and Thomas, 1984), that the spacing in time of inferred global cycles is too close to be tested by biostratigraphic data (Miall, 1992), that carefully controlled magnetostratigraphy conflicts locally with age inferences derived from global cycle charts (Prothero, 2001), and that a systematic reduction in the apparent average duration of global eustatic cycles through Mesozoic-Cenozoic time is better viewed as an artifact of methodology than as a valid record of Earth behavior (Dickinson, 1993).

As criticisms of global sequence stratigraphy are largely ignored by its proponents (Miall and Miall, 2000), the literature on sequence stratigraphy has evolved along two parallel tracks, one *pro* and one *con*, with little intersection of thought. The postulate of global eustatic cycles at the "Vail scale" shares with myth the prime characteristic of using an *a priori* assumption, that seismic reflectors are chronostratigraphic, as the prime means for resolving issues of correlation which remain unresolved if the assumption is faulty.

summary overview

An important concern is whether geomyths retard our thinking, or encourage research that leads to improved concepts. My prejudice is that investigations pursued under the aegis of various geomyths often embody too narrow a focus, failing to embrace a wide enough view of the phenomena in question. The seductive attraction of any myth is to eliminate conceptual uncertainty by providing comprehensive solutions for challenging puzzles, yet we learn new things only by acknowledging uncertainty and addressing paradox forthrightly.

Without rejecting the mental stimulus that geomyths afford, we could benefit from resisting any tendency to accord special status to the observations and arguments that underpin them. Why, in order to gain a sympathetic hearing, should the opponent of a geomyth have to assemble evidence against it more conclusive than the original evidence in favor of it?

The advantages of innovative induction can be preserved without lapsing into a mythic mode by reasoning with the guidance of *strong inference* (Platt, 1964), whereby inductive inferences are succeeded at once by deductive predictions (based on the inductive inferences) designed deliberately to test each inductive leap for potential disproof. When an inference is found wanting in any respect, it is then immediately adjusted or rejected, and the process repeated with an improved inference. If sequential tests are pursued with vigor, this approach guarantees that inductive inferences remain falsifiable, and do not grow inadvertently into untested geomyths having more durability than ultimate utility.

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