Earth-Science Reviews, 14 (1978) 1–34
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Tectonics and Sedimentation a Century Later

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ABSTRACT

Dott, R.H., Jr., 1978. Tectonics and sedimentation a century later. Earth-Sci. Rev., 13: 1–34.

This paper is one of a series that commemorates the fiftieth anniversary of the founding of the Society of Economic Paleontologists and Mineralogists in 1926. At that time, thought about tectonics and sedimentation was dominated by the ruling hypothesis of continental accretion. Marginal geosynclines were thought to have been filled with sediments derived from borderlands of Precambrian rocks, and then welded tectonically to the continent. In the 1930s, the fundamental distinction noted by Bailey and Jones between graded graywacke-graptolitic slate suites and cross-bedded sandstone-shelly carbonate suites provided a prelude to Krynine's petrographic-tectonic sandstone clans. In the 1940s sedimentary petrography finally emerged from its heavy mineral era to broaden its vistas. Prior notions of evolutionary successions of sediment types linked to a supposed tectonic cycle (e.g. the European ophiolite-flysch-molasse sequence) became more explicit. Refinements of sandstone classifications by Folk, Pettijohn, Gilbert and others, coupled with Krynine's tectonic cycle and the stratigraphic syntheses of Krumbein, Sloss, Dapples, and others, led in the 1940s to the belief that tectonics is the ultimate sedimentary control.

Meanwhile the geosyncline had been dissected by Stille and Kay (1936—1951). Ideas about sources of geosynclinal sediments and paleogeography were revised to include volcanic islands and tectonic lands raised *within* geosynclines rather than borderlands of Precambrian rocks standing outside the geosynclines. In the 1950s and 1960s, sedimentologists exploited the turbidity current revolution and the combined paleocurrent-petrographic approach pioneered by Pettijohn to delineate in detail the paleography and provenances of orogenic belts. Provenance studies have recently reached a high level of sophistication thanks to the efforts of many workers (e.g. Blatt, Crook, Dickinson, Füchtbauer, McBride, Schwab, Suttner, etc.). This work has demonstrated clearly that the cratonic, volcanic and tectonic source land types proposed by Kay had all been important, but to varying degrees at different times and places. When plate tectonics arrived in the late 1960s, sedimentologists were all equipped to reinterpret their rocks using petrographic, paleocurrent and sedimentary structure analyses to help diagnose different types of plate boundaries and to aid in making palinspastic plate restorations. Thus the study of tectonics and sedimentation is alive and thriving a century later.

INTRODUCTION

Neither James Hutton nor Henry Clifton Sorby can be claimed as the fathers of sedimentation and tectonics; not even Johannes Walther could qualify. But it is a rather meaningless historical question anyway to ask who was THE father (or mother) of any line of study. The common fixation on firsts, which historians call precursoritis, together with adoration of a few right-thinking heroes, and the common habit of writing history in terms of direct lines of descent to present (correct) ideas all tend to falsify history. Professional historians refer collectively to these distorting tendencies as Whig History, so named because of a past tendency among English historians "to write on the side of the Protestants and Whigs, to praise revolutions provided they have been successful, and to produce a story which is a ratification if not the glorification of the present." (H. Butterfield, 1931, p.v.). Whiggishness or Presentism, then, refers to the past with reference to the present; it stresses likenesses to the present, and either ignores unlikenesses, or fails to assess adequately why there were such differences.

Any historical review must choose a starting point of some kind, however. Where, then, should a review of tectonics and sedimentation commence? The name sedimentology was not coined until the 1920s, nor sedimentary tectonics until the 1940s. Because this article is an outgrowth of an invited oral paper for a symposium on the first fifty years of the Society of Economic Paleontologists and Mineralogists (SEPM), I shall deal primarily with the past five decades. But first I shall sketch the background for my historical portrait that will briefly depict events extending back into the last quarter of the nineteenth century. Then the mid-twentieth century foreground can materialize. The illustrations record the evolution of paleogeographic concepts for geosynclines.

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BACKGROUND: THE GEOSYNCLINE AND EARLY SEDIMENTARY PETROGRAPHY

In the late nineteenth century, probably no geologic issue was of greater general interest than the cause of mountains. And surely early ideas of the geosyncline and the almost universally-held dogma that thick sedimentation must precede the crumpling and elevation of mountains qualifies as the first major linking of sedimentation and tectonics. I have recently discussed elsewhere the evolution of the geosynclinal concept prior to 1930 (Dott, 1974, 1978). Therefore, the contrasting interpretations of causality of James Hall and James D. Dana between sedimentation and subsidence as well as of the American shallow marine versus European deep marine views of the environments of deposition in geosynclines will not be treated further here. But, in passing, another point of difference should be recalled also, namely the American belief that geosynclines formed upon continental crust while Europeans considered them as intraoceanic. Marcel Bertrand's embryonic suggestion in 1894 of what later could be called a sedimentary-tectonic cycle for mountains, namely progression from presumed deep marine cherts and black shales upward through flysch (repetitive marine sandstone and shale) to molasse (nonmarine, massive coarse sandstone, conglomerate and coal) deserves special mention. W.M. Davis' and G.K. Gilbert's revolutionary geomorphic concepts, together with the new idea of isostasy, also formed important parts of the matrix of thought bearing upon the origin of mountains.

Meanwhile, sedimentary petrology, although invented by Sorby twenty years earlier, was finally beginning to emerge as a subdiscipline in the last two decades before the turn of the century. Pioneer petrographic studies of river sands focussed attention upon mineral criteria for provenance of detrital sediments. In the same period was born the long-standing (but erroneous) suggestion that abundant feldspar must indicate either an arid or cold climate and vice versa. And the *Challenger* reports invited analogies between modern marine and ancient marine sediments in the same period that Johannes Walther formally stated the special method of actualism that he termed *comparative lithology* (see Middleton, 1973). Amadeus Grabau introduced the principle into the English language in 1913 in his monumental *Principles of Stratigraphy*, which Grabau dedicated to Walther. In the same year, the first English textbook of sedimentary petrography was published by F.H. Hatch and R.H. Rastall. This was followed nine years later by H.B. Milner's classic, and in 1929 L. Cayeux and in 1933 P.G.H. Boswell contributed major treatises to the growing ranks of sedimentary petrography references.

With the publication in the 1920s of the National Research Council reports on sedimentation, which then became the first *Treatise on Sedimentation* (Twenhofel, 1932), with the founding of the Society of Economic Paleontologists and Mineralogists in 1926, and the creation of the *Journal of Sedimentary Petrology* in 1931, we find that sedimentology in America had gained a clear and lasting identity; it would soon begin to do so in Europe as well (Middleton, 1976). The rapid emergence of sedimentology as a specialty field was due in considerable measure to the stimulus of the rapidly expanding and enormously successful petroleum industry.

At the turn of the century, leading geologists tended to be Renaissance men well grounded in and fully cognizant of developments in all branches of geology, embryonic geophysics, and even in physics itself (e.g. Kelvin's heatage arguments, isostasy, radioactivity, and the like). By 1930, however, compartmentalization was well advanced and even within sedimentary geology there was a sharp dichotomy. A paleontologic group pursued index fossil stratigraphy to its ultimate conclusion, while a petrologic group tended to split either into a sand sifting or a heavy mineral sub-set. W.H. Twenhofel, who did so much to stimulate the professionalization of sedimentology, was exceptional in that he came out of the Schuchert biostratigraphic tradition, but tried to bridge the gap to the physical aspects of sedimentology. Subsequently other such notables as Hans Stille, Marshall Kay, Ph. H. Kuenen, L.L. Sloss, E.C. Dapples and W.C. Krumbein continued to try to blend stratigraphy, sedimentology and tectonics.

THE OLD GLOBAL TECTONICS

Mechanisms

Charles Schuchert in 1923 agreed fully with Kober "that shrinking of the earth is no longer hypothesis or theory, on the contrary it is knowledge built on ascertained fact." (p. 212). Shrinkage due to cooling had faded, to be sure, under the onslaught of both isostasy and radioactivity, but Chamberlin's popular planetesimal theory had rescued contraction by substituting a gravitational drive. Thus warping and crumpling due to radial contraction remained the favorite mechanism of global tectonics and mountain building well into the thirties. Continental drift as first conceived by F.B. Taylor (1910) might offer a partial alternative mountain-building mechanism by wrinkling of continental leading edges, but still a mechanism for drift was needed. And, as the 1926 American Association of Petroleum Geologists famous symposium in New York on Continental Drift showed, no satisfactory mechanism was yet conceived. Even if there had been, it is clear that most Americans still would have rejected drift (see Van der Gracht and others, 1928).

By 1930 a plethora of grand but speculative global hypotheses had appeared, but they were based almost entirely upon comparative descriptive tectonics. Fanciful mechanisms such as continental creep and sub-oceanic spread (both isostatically inspired by analogies from glaciers) and batholithic injection were among these (for further discussion, see Dott, 1978). J. Tuzo Wilson, who was a student during that era, was prompted to observe recently that "A lot of pedantic nonsense" was being taught in American universities at that time (The Massey Lectures, 1977).

It seems surprising in retrospect that the profound implications of radioactivity generated heat went so long unappreciated by geologists. Especially this is puzzling because of the intense scientific interest in the rapid advances of understanding of radioactivity within the first decade or two of the century by E. Rutherford, F. Soddy, R.J. Strutt, B. Boltwood and others. Geologist J. Joly even published a 287 page book entitled *Radioactivity and Geology* as early as 1909 in which, although he discounted the use of radioactivity for dating the earth, he invoked heat from radioactive minerals in sediments to weaken and facilitate deformation, and to contribute to volcanism and earthquakes.

> "The energy . . . is in fact transported with the sediments — the energy which determines the place of yielding and upheaval, and ordains that the mountain ranges shall stand around the continental borders. Sedimentation from this point of view is a convection of energy." (p. 111).

In 1915 Arthur Holmes argued that "nearly three-fourths of the present heat output must be supplied by radioactivity" (p. 112). And then in 1931 — only three years after publication of the AAPG symposium on drift — Holmes published his obscure paper (read orally in 1928) endorsing continental drift and suggesting thermal convection in the mantle as a mechanism for global tectonics. Though it was a long time before it bore fruit, this was a seed from which would sprout a new global tectonics thirty years hence. The delay in recognition of the importance of radioactive heat and even of radioactivity as the only valid basis of numerical dating of the earth (which only came in the early thirties and thanks largely to Holmes) seems attributable both to a mistrust of physics by geologists after their experience with Kelvin and to great uncertainties and resulting arguments amongst the radiophysicists and chemists themselves over decay (see Burchfield, 1975).

Continental accretion

Dana had presented a complete theory of continental and ocean-basin evolution, which integrated the geosyncline with his mountain-building theory (see Dott, 1978 for fuller discussion). The fundamental differences between continental and oceanic crust were confirmed and contraction of the earth seemed firmly established even before 1900. Lateral pressure caused by shrinkage was concentrated at margins between continents and ocean basins, said Dana. Geosynclines would form there first due to downbuckling and then would fail; the resulting synclinorium became stiff and unyielding, and the next geosyncline would form parallel to it and oceanward from it. Meanwhile, Eduard Suess in Europe also postulated that mountain building progressed outward from the shield or kratogen nuclei of continents in concentric belts. Indeed, his thinking seems to have been influenced considerably by Dana. Thus was born the hypothesis of continental accretion that was to rule the mid-twentieth century, particularly in America. In the early part of the century, notions of sequential igneous, metamorphic, and even ore-forming events were added to the geosynclinal concept of sedimentation followed by upheaval, so that in the thirties a grand tectonic (or geosynclinal or orogenic) cycle was envisioned with continental

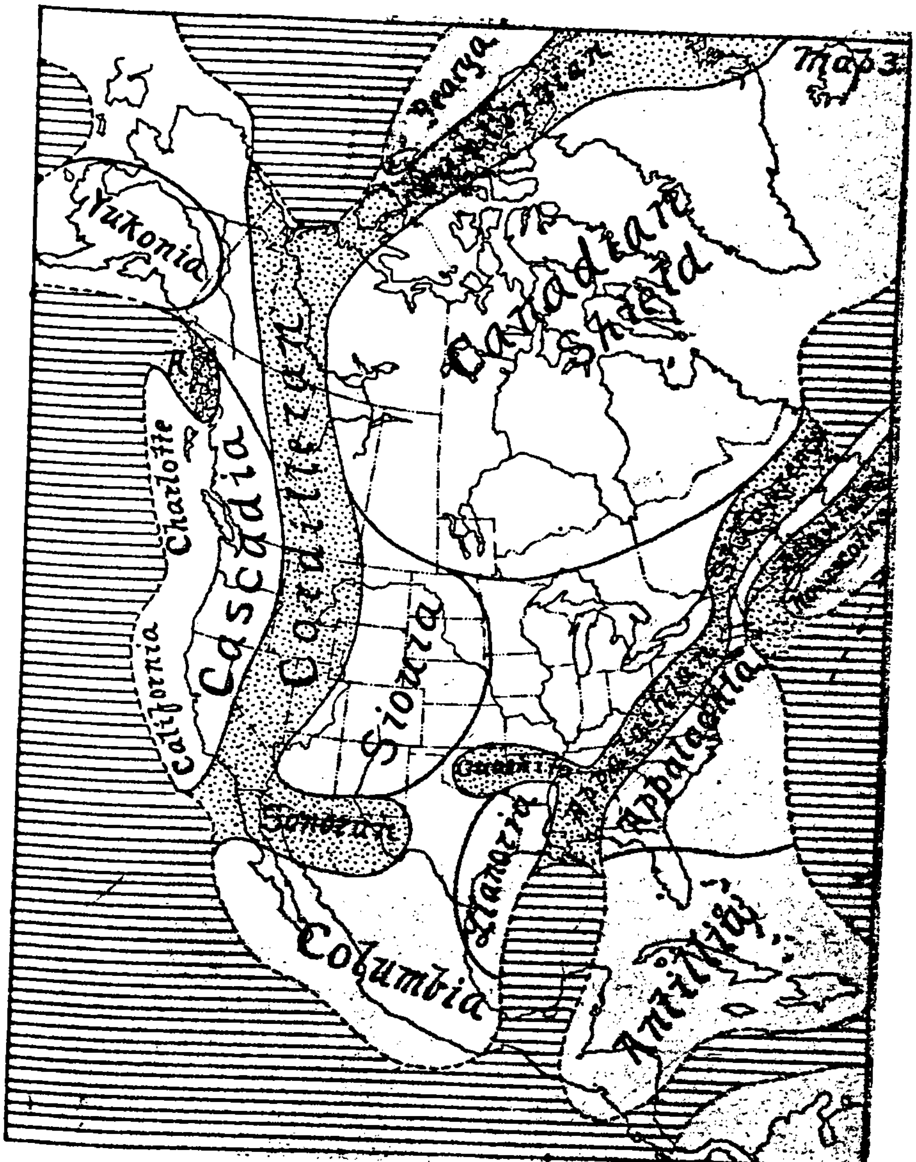

Fig. 1. Major Paleozoic tectonic elements of North America as conceived by C. Schuchert in 1923 (p. 215) for his presidential address to the Geological Society of America. It was the lead paper for a symposium on mountain building. Note the named borderlands seaward from each geosyncline.

growth the ultimate outcome. While the concept had its roots in the ideas of Hall, Dana and especially Bertrand, the new ideas of magmatic differentiation fit nicely into the broadened tectonic cycle, and implied a grand, slow differentiation of continental crust from the interior of the earth.

Borderlands

Hall, Dana, and the Rogers Brothers all recognized that Appalachian strata coarsen eastward, thus must have been derived from some land there. Dana incorporated into his theory of mountain building upwarped geanticlines complementary to the downwarped geosynclines. The idea of high borderlands standing outside the subsiding geosyncline and composed largely of Precambrian crystalline rocks caught on rapidly. In 1909, Schuchert had named and identified geanticlinal borderlands accompanying each of North America's ancient geosynclines, and in 1923 he refined their boundaries and renamed some of them (Fig. 1). But Schuchert believed that, instead of growing larger, North America has actually shrunk since the pre-Paleozoic He recognized that the Appalachia borderland had been comprised chiefly of old continental-type rocks, therefore the Appalachian geosyncline had lain *within* the continent. Both geosyncline and borderland had been integral parts of the continent originally, therefore American geosynclines must be fundamentally different from the Europeans' intercontinental or Mediterranean ones.

THE VOLCANIC ARC-TRENCH SYSTEM

E. Suess in his great tectonic treatise (1885—1909) had first focussed geologists' attention upon the festoons of volcanic islands and their associated deep trenches or foredeeps on the Pacific side of Asia. He linked them genetically with the curving old mountain ranges on the continents, and reasoned that the foredeeps were downwarped as the folded mountain ranges were pushed up and outward from the continent in successively younger concentric rings. While foredeeps include the deepest zones of the ocean floor, Suess reckoned that some of them had been partially filled with sediments (e.g. Tigris-Euphrates Valley and Persian Gulf in front of the mountains of Iraq and Iran), and others completely silted up were postulated in front of the Alps and Carpathians (confirming subsurface data did not yet exist, however).

Suess did not believe that thick sedimentation was a necessary precursor to mountains, and, indeed, he found little merit at all in the entire geosynclinal concept (see Hsü, 1973). While *Pacific-type* continental margins are characterized by arcs and foredeeps, Suess regarded *Atlantic-type* margins as quite different because they lack the arcs. He thought the latter formed by dismemberment somehow of now-disjunctive mountain belts, which formerly extended across the site of the Atlantic basin. One school of thought postulated a former Atlantic-like continent there, which had subsided and become converted to oceanic crust, while the drifters were soon to propose the alternate explanation of separation. The conversion of continental to oceanic crust was anathema to the descendants of Dana, who were firmly convinced of the exclusiveness of continental and oceanic crust, but continental displacement was equally unacceptable because of their corollary assumption of permanency of positions.

Interest in the Pacific arc-trench systems was stimulated greatly by Dutch studies in the East Indies in the thirties. The names of Molengraaf, Umbgrove, and Kuenen especially are linked with that work, which investigated the bathymetry and sediments of the sea floor around the islands, and also identified on several of the islands ancient manganiferous radiolarian cherts and shales considered to be deep-water deposits. Close analogies with the Alpine geosyncline became increasingly popular for Europeans because the Java trench seemed perfectly compatible with their view of a geosyncline as a deep trough and the chert-shale of the ophiolite sequence did, indeed, seem to match the Indonesian deep-sea sediments. In 1934 Vening Meinesz announced that the great negative gravity anomalies, which he had measured from a submarine, were characteristic of trenches. These anomalies were for many years interpreted, following Vening Meinesz, as mass deficiencies caused by less dense crust being down-buckled on a huge scale into the more dense mantle. This downbuckle or *tectogene* (Kuenen, 1936) can be thought of simply as a refinement of Suess' downwarped fore-deep, and as the latest garb for venerable lateral compression. The American David Griggs then performed scale-model experiments to test the hypothesis that increasingly popular thermal convection in the mantle might form a tectogene (1939), whose surface expression — the trench — became filled with geosynclinal sediments. Eventually buckling crumpled and raised the sediments into mountains. The experiments indicated this to be a plausible mountain-building mechanism, and the tectogene concept became very popular during the forties. At the same time, H.H. Hess of Princeton, another early student of island arcs, began in 1938 to emphasize the petrologic similarites between modern arcs and ancient mountain belts, especially the presence of linear serpentine zones (as in the original Alpine ophiolites). He directed the attention of Americans to serpentine belts in their own mountains, and suggested — in contrast to the European view — that ultramafic igneous intrusions from the mantle were squeezed up into the flanks of tectogenes at the time of initial downbuckling [1].

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[1] The *tectogene* is now dead, having passed quietly away in the mid-fifties. The negative anomalies are now interpreted as due largely to thick, low-density, perhaps only partially lithified and pervasively faulted rocks between the trench and the island arc (the *arc-trench gap*). Underthrusting of lithosphere beneath lithosphere may also contribute to the mass deficiency. Most of the *ultramafic rocks* are now considered to have been emplaced tectonically along low-angle shear zones as solid masses rather than as more-or-less vertical, magmatic intrusions.

In the thirties also, seismologists had discovered that earthquake foci beneath volcanic arcs lie along a broad zone inclined downward beneath the arcs. They found that very deep quakes (down to −700 km) occur only beneath such arcs, indicating something very fundamental structurally about the arcs. Then in 1954 Hugo Benioff suggested that the arc volcanoes are caused by melting along such zones due to repeated strain. The Benioff zones, as they have come to be known, invited the speculation that gigantic inclined shear zones exist along which portions of crust and upper mantle move obliquely past one another. Rather than supporting the tectogene, they called to mind early-century inferences of oceanic under-thrusting. The petrologist H. Kuno (1959) then related specific eruptions to specific earthquake events; he and W.R. Dickinson and T. Hatherton (1967) then suggested that compositions of magmas so generated were a function of depth (thus pressure) of melting along the Benioff zones.

STILLE AND KAY

During the late 1930s and 40s, an important synthesis of what had been recently learned about volcanic island arc and trench systems with the geology of ancient geosynclines was accomplished, and largely in North America. The German Hans Stille first noted that mountain belts have two broad subdivisions distinguished consistently by certain rock suites (1936a,b). These he named in 1941 the *eugeosyncline* ("truly or wholly a geosyncline"), a belt containing volcanic rocks and associated sediments, and *miogeosyncline* ("lesser geosyncline"), a belt of sediments lacking volcanic rocks. The miogeosyncline lay next to the stable continental interior, which he named the *craton* ("shield"). The larger entity, encompassing both the mio- and eugeosynclines and comprising the entire mobile zone delineated today by a folded mountain system, was named by Stille *orthogeosyncline* ("real" or "straight" geosyncline). The names were ponderous and it was confusing to have the same suffix applied both to the first-order feature and to its subsets. Nonetheless, it was useful to have some kind of verbal short-hand designations for these major elements and the terms were to enjoy wide currency. Being German, Stille was naturally influenced by European geology in developing his concepts of continental architecture and evolution, but he had visited North America personally in the early thirties, and he chose the western American Cordillera as the type example for his mio- and eugeosynclines.

Columbia University stratigrapher Marshall Kay had independently discovered the same ancient volcanic belts as Stille's eugeosynclines. During his thesis field work in the midwest in the late twenties, he had encountered Ordovician volcanic ash and wondered about its source. When he migrated east to Columbia, he was intrigued to find many more such layers in Ordovician strata in the Appalachians. Simultaneously, compilations for his stratigraphy courses revealed peripheral volcanic zones in both the Appalachian

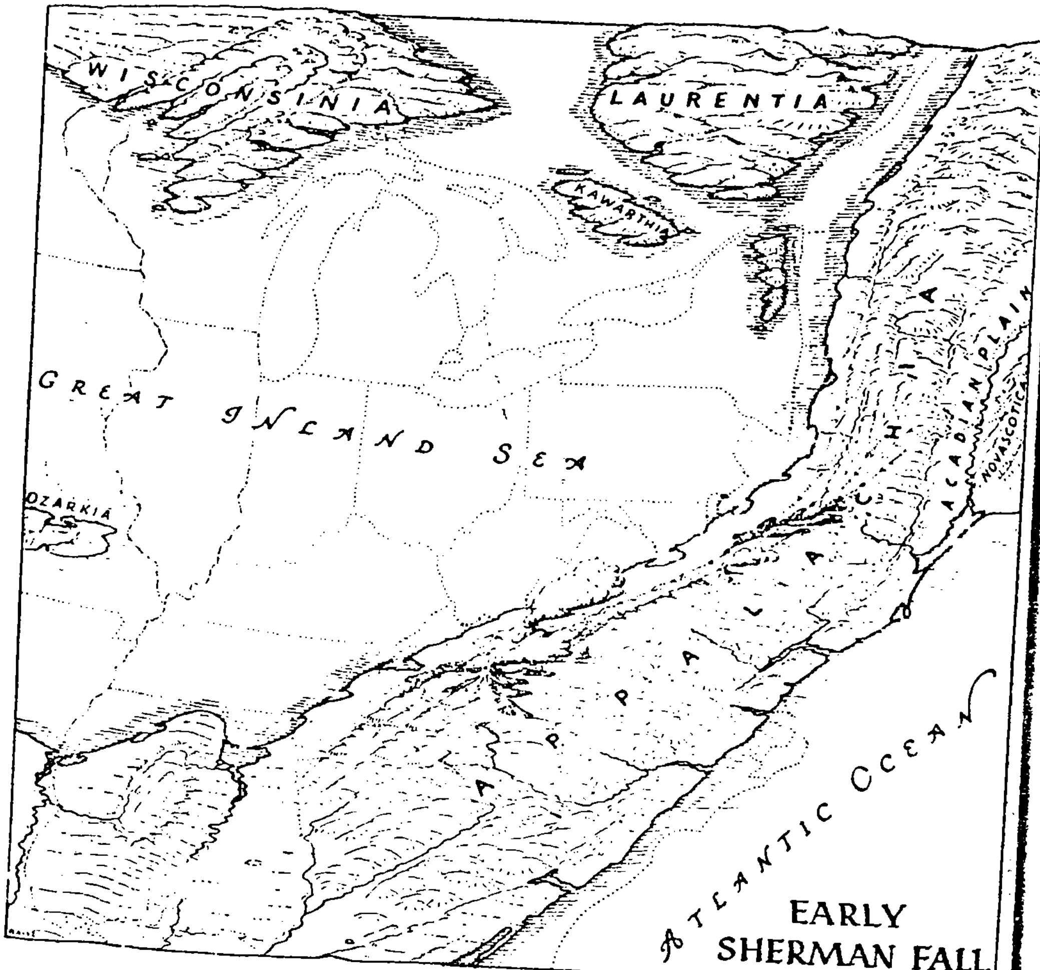

Fig. 2. Paleogeography of the classic Appalachia borderland as conceived in 1935 by Kay for mid-Ordovician (Trentonian) time. While most of Appalachia was still assumed to have been composed of pre-Paleozoic rocks (as in Fig. 1), several volcanoes are shown to provide sources for the many Ordovician altered volcanic ash layers found widely over the eastern states. (From Kay, 1935, p. 243).

and Cordilleran mountain systems. He began to construct paleogeographic maps of eastern North America showing volcanic centers. At first only one or two were simply superimposed upon the orthodox Appalachia borderland (Fig. 2), but when Marland Billings of Harvard reported in 1934 marine Devonian fossils in schists in the heart of that presumed pre-Paleozoic borderland in New Hampshire, Kay recognized that there were serious problems with the whole borderland concept (Fig. 3). He has recorded that basalts and radiolarian cherts seen on an International Geological Congress field trip to the Urals in 1937 further convinced him of the fundamental "contrast

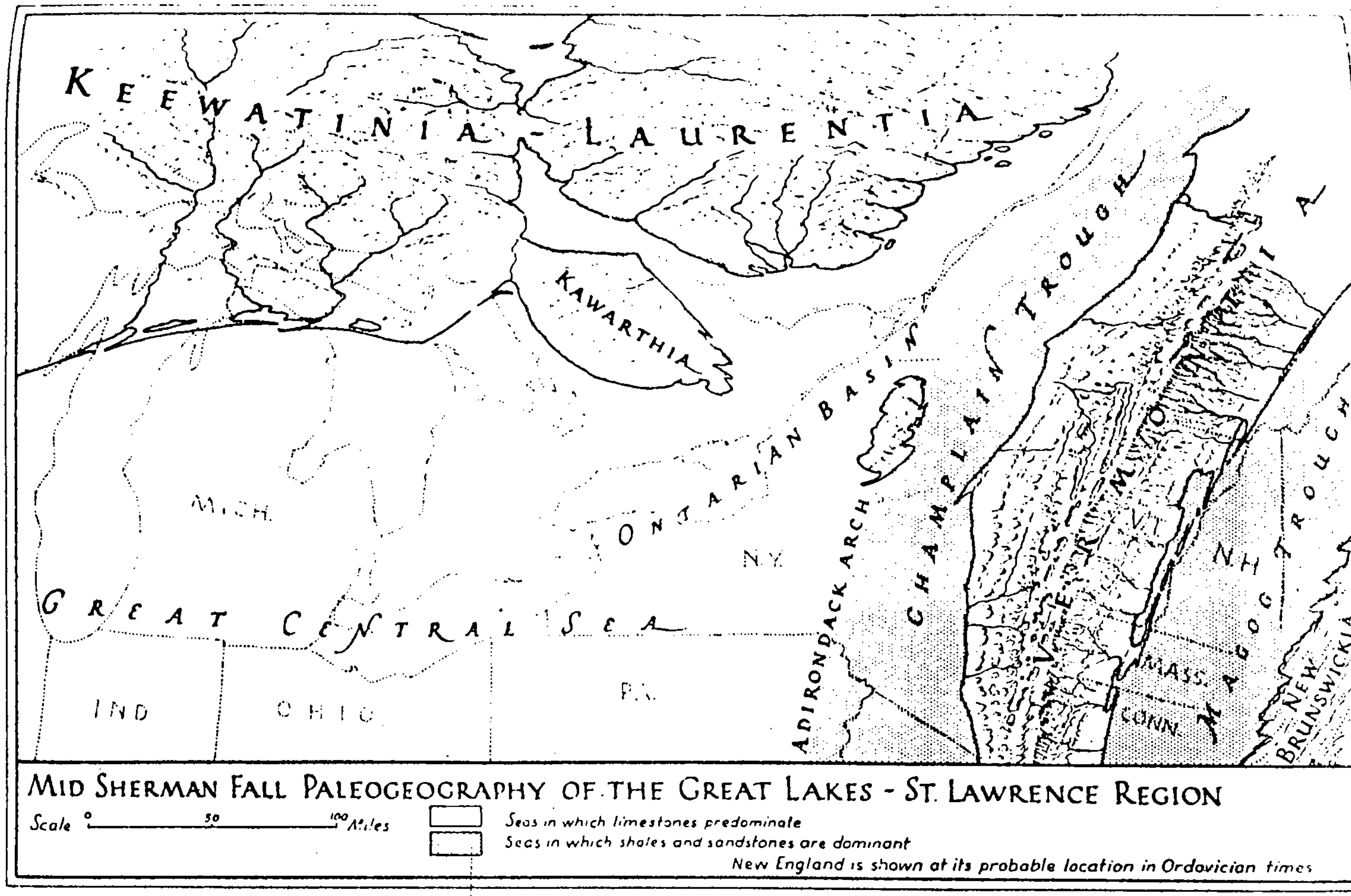

Fig. 3. Paleogeography for mid-Ordovician (Trentonian) time showing Kay's first concept of dual troughs (*Champlain* and *Magog*) separated by the ephemeral ***tectonic land*** Vermontia formed of only slightly older Paleozoic rocks rather than pre-Paleozoic ones as in the old Appalachia borderland concept (compare with Figs. 2 and 5). Later Kay envisioned volcanic islands within the Magog trough (see his famous restored cross section of the same region; Kay, 1951, pl. 9). (After Kay, 1937, p. 291).

between volcanic and non-volcanic geosynclinal belts" (Kay, 1974). Comparisons by Hess between island arcs and mountain belts, especially arcs like the Aleutian that even extend physically into mountain belts, made a deep impression upon Kay. Now he realized the full significance of the eu- and miogeosynclinal distinction, so he corresponded with Stille and adopted his terms. In 1944 Kay first introduced his concept of marginal volcanic belts similar to modern arcs, which were active more or less throughout the history of all mountain belts on the sites of the formerly hypothesized pre-Paleozoic borderlands. He elaborated his thesis in the late forties, and then in 1951 published his famous Memoir.

An interesting example of independent parallel development of thought occurred in 1947 with the publication by A.J. Eardley of a paper now largely forgotten comparing Paleozoic volcanic rocks along the Pacific margin of North America with modern island arcs. While Kay had been influenced by Stille and Hess, Eardley, who had studied at Princeton, apparently was led to the island arc comparison solely by Hess's influence. Both, however, had been anticipated many years before by W.G. Fearnsides (1910), who wrote of an Ordovician volcanic archipelago in Wales. Fearnsides had recognized shifting troughs (with graptolitic sediments), platforms (with shelly strata), and geanticlinal uplifts as well as volcanic islands.

Not only were the eugeosynclines the loci of volcanism, which periodically built *volcanic islands* up above sea level even while surrounding areas continued to subside and receive sediments, but occasionally severe structural disturbances (orogenies) deformed and metamorphosed the but-slightly-older geosynclinal volcanic and sedimentary rocks. These were uplifted then to form very large *tectonic lands* (i.e. raised largely by structural rather than volcanic processes). Commonly granitoid igneous rocks also formed during these upheavals. Thus the thick clastic geosynclinal sediments early recognized by Hall, Dana and others, were seen to be derived primarily from lands raised *within* (rather than next to) the geosynclinal tract, and they were derived largely by cannibalism from the erosion of contemporaneous and but-slightly-older sedimentary, volcanic and plutonic rocks rather than much older, pre-geosynclinal rocks. Kay never made the comparison, but his tectonic lands were essentially equivalent to the European *cordilleras*, which had been popular for thirty years (Fig. 4).

Initially Kay and Eardley provided much additional stratigraphic evidence supporting the comparisons of Stille and Hess of volcanic zones of ancient geosynclines with modern volcanic arcs. But soon Kay, especially, carried the stratigraphic analysis much farther. Heeding a warning by Hess, he was cautious in not insisting upon a simplistic one-to-one analogy between modern arcs and ancient volcanic belts, rather he took pains to emphasize the variability of both ancient and modern entities. He also detailed in his Memoir the stratigraphic evidence of successive uplifts of volcanic and tectonic lands, and of apparent migration of ancient volcanic and serpentine zones through time.

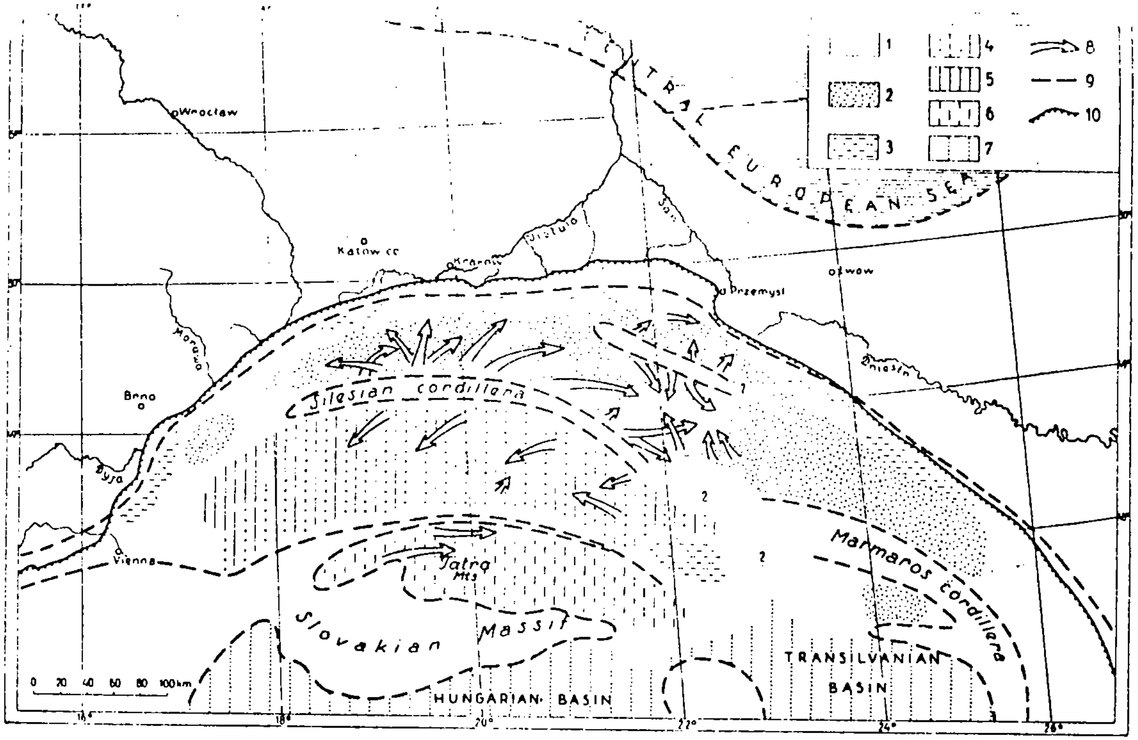

Fig. 4. Paleogeography of the Carpathian area of Poland during latest Eocene and Oligocene time. This restoration typifies the European conception of tectonic lands, called *cordilleras*, raised within a geosyncline to provide much of the clastic sediments (in this case flysch) to adjacent subsiding troughs. Note the similarity to Kay's tectonic lands (Fig. 3) (From Dzulynski et al., 1959, p. 1105).

Skeptics asked to have completely outlined on maps some ancient volcanic islands. This was virtually impossible in 1951, however, given the level of mapping refinement extant and the structural complexity of mountain belts. Moreover, no one knew in detail what an arc remnant should look like in the ancient record. Hsü (1973) has scolded Kay, A. Knopf (1948) and Americans in general for their handling of the volcanic rocks because "During the last decade (sixties) indiscriminate reference of all volcanics as part of eugeosynclinal sequences has led to confusion. The origin of andesitic volcanics is very different from that of ophiolites" (p. 79). While the last statement is true, it represents lightning-bug historiography that merely clouds the realities. It is not only unfair but irrelevant in 1973 to suggest that writers of the forties and fifties should have drawn a distinction that was not fully appreciated until twenty years later! In fact Kay was unusually aware for an American stratigrapher of the European literature and the petrologic controversy about ophiolites, spilites, and the like. Moreover, he did attempt on his stratigraphic compilation maps to distinguish different volcanic rock types (e.g. Kay, 1951, fig. 7, p. 37). He also plotted distributions of plutonic pebbles in relation to unconformities as clues to the history and paleogeography of his tectonic lands (e.g. 1951, pl. 9, p. 41). At the time Kay and Knopf were writing, not only was the true significance of ophiolite and spilite not yet proven (even in Europe), but the notion of a systematic evolutionary igneous cycle from mafic to acidic products through time was prevalent. In America, at least, it was more urgent to test that hypothesis than whether ophiolites all represent deep-sea floor.

Marshall Kay has been criticized most bitterly for his classification of geosynclines and especially for his choice of ponderous, Greek-derived names for categories additional to Stille's. There were no less than six of these additional "geosynclines." [2] In his mind, it was a perfectly logical extension of Stille's scheme to distinguish formally the complete spectrum of major patterns of subsidence and sedimentation revealed in all kinds of sedimentary basins. So much emotion was generated by the semantics of his classification that many workers failed to appreciate fully the significance of the different important patterns he meant to distinguish. For many, it seemed unfortunate that he applied the suffix "geosyncline" to basins within the stable continental interior (craton) as well as to mountain belts. To Kay,

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[2] So-called geosynclines recognized within the craton (or cratonic basins) included *autogeosynclines* (basins lacking adjacent land), *zeugogeosynclines* (basins linked to adjacent intracratonic highlands), and *exogeosynclines* (at craton margins with sediments derived from an uplifted miogeosyncline). *Epieugeosynclines* were seen as post-orogenic, non-volcanic troughs, *taphrogeosynclines* were fault-bounded depressions, and *paraliageosynclines* were coastal geosynclines (beneath coastal plains and continental shelves).

however (1951, p. 4; emphasis added),

> "The term geosyncline should be restricted to a *surface of regional extent subsiding through time* while contained sedimentary and volcanic rocks are accumulating; great thickness of these rocks is almost invariably the evidence of subsidence, but not a necessary requisite. Geosynclines are prevalently linear, but non-linear depressions can have properties that are essentially geosynclinal."

Given this definition, he was led (perhaps trapped?) to include deeply subsided cratonic basins.

Kay had a terse writing style, which also made it difficult for many others to see clearly the value of his distinctions. It has taken two decades for these handicaps to be finally overcome. It is ironic that, while others made fun of Kay's classification, he and Stille made relatively little of their terminology; patterns in the rocks were always emphasized rather than names. One lesson that could be taken from the fifties is that semantics provides a rather fruitless pastime.

REINTERPRETATION OF GEOSYNCLINAL SEDIMENTS

Meanwhile, important new sedimentologic insights also were developing during the thirties, which ultimately were to resolve the long-standing American-European controversy about depth of deposition of geosynclinal sediments. Barrell (1917) had emphasized that thickness of sediments was largely a function of relative crustal subsidence, regardless of whether due to isostatic loading or structural stresses (a question that was to continue to be contested for years). Several British geologists, including Fearnsides in 1910, E.B. Bailey in 1930, G.W. Tyrrell in 1933, and O.T. Jones in 1938, all recognized fundamental differences between the Paleozoic geosynclinal graywackes with graded bedding and associated graptolitic shales on the one hand, and shelly limestones and cross-bedded sandstones of the stable lowlands of Britain on the other. Thus sediment type seemingly also was related to subsidence. Bailey suggested that graded bedding indicates grains "poured in" to settle gravitationally in deep water. In 1933, micropaleontologist Manley Natland recognized that some California Plio-Pleistocene sandstones and conglomerates were interstratified in thick sequences with shales containing fossil Foraminifera that must have lived at depths of 1,000 meters or more. And C.I. Migliorini was impressed by characteristic graded bedding in sandstones in the Italian flysch and of the incorporation of shallow-marine fossil debris in many of them.

A synthesis of these separate observations and interpretations came with Kuenen and Migliorini's classic joint paper in 1950 proving experimentally that turbidity currents can transport and deposit sand in graded beds in relatively deep water (see also Bell, 1942; and Walker, 1973). Soon the rapidly expanding post-World War II oceanographic fleets were bringing up additional confirming evidence with many graded sands from the deep abyssal sea floor. Apparently both the American and European interpretations had been partly right and partly wrong all along. Repetitive sequences of graded graywackes as well as the radiolarian cherts and associated black shales could, after all, be deep-water sediments. Since 1950 the enormous volumetric importance of such deposits in geosynclines has come to be appreciated, while at the same time, the significant volumes of truly shallow-water and non-marine strata also have been documented.

Pettijohn pioneered in the refinement of sedimentologic approaches combining petrographic, sedimentary structure, and paleocurrent analyses, the application of which in the fifties and sixties produced tremendous strides in the unravelling of the complex changing paleogeography of geosynclines. The results clearly underscored the importance of all three of Kay's different sediment source lands (volcanic, tectonic, and cratonic). They also showed the enormously complex and rapid changes of environments of depsoition characteristic of eugeosynclines.

It is a curious historical irony that proof of abundant deep-water sediments in mountain belts revived Hallian subsidence by sediment loading. Even though less dense than underlying crust and mantle (as is glacial ice), large volumes of sediment must depress the crust isostatically. But for any given increment of subsidence, a greater sediment (or ice) thickness is required because of the lesser density of that sediment (or ice). Sea level is an upward limit for sedimentation (unlike glacial ice), therefore significant subsidence by loading with entirely shallow-water sediments had seemed to be ruled out. But if geosynclinal sedimentation were to begin in deep water, as the turbidity current mechanism allowed, then considerable subsidence would be produced until sedimentation filled up to sea level, as it ultimately must do unless external tectonic processes cause further depression.

Meanwhile, some thick, shallow-water late Cenozoic coastal deposits had been called geosynclinal. Those of the so-called Gulf Coast geosyncline, which became Kay's type example of a paraliageosyncline ("coastal geosyncline"), had produced considerable controversy in the 1930's as to the cause of subsidence. Taken in the abstract, the isostatic argument sketched above seemed to rule out loading simply by shallow marine sedimentation, yet H.N. Fisk and co-workers on the Mississippi delta presented empirical evidence suggesting subsidence in direct response to shifts of the delta's position. Today it is apparent that very large volumes of shallow-water deposits *where spread over a large region* also do produce downbending of the crust. The sea-level limit causes sediments to be spread laterally into deeper water, but they depress the crust in a wave-like fashion as sedimentation proceeds.

It seemed logical in the thirties and forties to class the young coastal accumulations as geosynclines simply because of their great thicknesses, and it followed inevitably that geologists would try to draw analogies between these essentially modern so-called "geosynclines" and more ancient, de-folded ones (orthogeosynclines of Stille and Kay). Such attempts only led to confusion because of an apple-and-orange fallacy; the fact that the two entities have a completely different tectonic genesis, while suggested by some (see Dott, 1964a), was not proven until plate tectonics arrived in the late sixties.

SEDIMENTARY TECTONICS

P.D. Krynine

We can make a general assessment of the prevailing view about tectonics (or diastrophism) and sedimentation in the thirties by examining W.H. Twenhofel's writings, for he was probably the single most prominent figure in American sedimentology of that decade. First, Twenhofel followed Barrell in regarding subsidence as the primary control upon sedimentation. He doubted, for example, that coarse orogenic sediments were generally preservable because he thought they would form topographically too high to survive. Therefore, volume should be a better measure of uplift than coarseness. Other factors listed by Twenhofel (1939) included chiefly physiography, diastrophism, climate and, to a lesser extent, fire, close-pasturage, and cultivation (these last two seemingly reflected his Wisconsin residence). He recognized interrelationship among some of these factors, for example physiography influences climate, but diastrophism influences both of those. Twenhofel gave more emphasis to climate and vegetative cover than have most subsequent workers; maturity of decay by weathering was a recurring theme, for example. While there were clear influences of W.M. Davis' physiographic concepts in Twenhofel's approach to sedimentology, he nonetheless cautioned that "The physiographic cycle has been assumed to produce a sedimentary cycle, but there are altogether too many variables to chance correlating what is observed in the geologic column with stages in the physiographic cycle." (1939, p. 22).

About the time that the SEPM was founded, a young Russian immigrant graduated from the University of California at Berkeley (1927), and began a three year career in petroleum geology in tropical Mexico. (He already had received a degree in geology from the University of Moscow in 1924). In 1936, P.D. Krynine earned the PhD from Yale, and took a position as petrologist at Pennsylvania State University in 1937, the first sedimentary petrologist ever to hold such a chair in America. From his early experiences in the tropics, and his PhD research on the Triassic arkoses of Connecticut, he had become acutely aware (as was Twenhofel) of the relative magnitudes or rates of a multiplicity of factors affecting clastic sediments. These must be considered as a whole. The approach developed by W.M. Davis " of describing landforms in terms of *structure*, *process* and *stage* — could be fruitfully applied to the classification of geotectonic elements" (Krynine, 1951, p. 743). Krynine damned with faint praise such prominent contemporaries as

Kay, Pettijohn, Krumbein, Dapples and Sloss. He was scathingly critical of most everyone else. Stratigraphy, for example, was once characterized as a "complete triumph of terminology over facts and common sense" (in Folk and Ferm, 1966, p. 853).

Krynine craved a fully comprehensive genetic scheme that would provide a general synthesis of sedimentary petrology and tectonics. So he strived to go beyond the descriptive stratigraphic-tectonic constructs of Kay, Stille, Cady and others. "Geosynclines are not different from landforms, which are made up of certain rocks . . . and cannot be divorced from these rocks: a dune does not exist without its sand. Similarly, a geosyncline can be recognized only because it is a certain body of sedimentary rocks... ." (1951, p. 744). (This passage seems to be a rejoinder to Kay's definition of a geosyncline as a subsiding surface as quoted above). P.D.'s 1940 paper on the petrology of the Third Bradford Sand, a major oil producer in Pennsylvania, was a hallmark because it showed how much could be learned from a detailed petrologic study of sandstones. It was a slight overstatement, however, that "...the genetic characteristics of an oil sand can be inferred very quickly from a competent petrographic examination of only a few critical thin sections" (Folk and Ferm, 1966, p. 856). In 1941 Krynine first presented his tectonic-sedimentologic synthesis in four abstracts for the Boston meeting of the Geological Society of America, but the most comprehensive and coherent statements are found in 1948 and 1951 papers.

Krynine seems to have taken M. Bertrand's 1897 paper as his inspiration[3]. Bertrand's distinction of different sediment types representative of several stages of development of a geosyncline was characterized as "more modern than most of the publications that have seen the light during the past few years... . As is usual with great pioneering work done before its time, Bertrand's ideas were promptly forgotten" (1951, p. 744—745). The Bailey-Jones distinction of graded geosynclinal graywackes from cross-bedded shelf quartz arenites received qualified approval as a refinement of subsidence-environment criteria. Next Tercier (1939, 1946) came in for praise for a more discriminating linkage between sedimentary and tectonic environments. And Pettijohn's 1943 classic treatment of Archean sedimentation received approval for its strong petrographic basis.

Krynine judged other approaches to over-emphasize rates of subsidence. He argued (1951, p. 746) that

> "Tectonism controls not only the rate of subsidence and the shape of geotectonic elements and related basins of deposition, but also controls to an equal degree the character of the source area, both in respect to relief (an obvious derivative of tec-

[3] It has been implied that Krynine plagiarized his entire concept from Russian Professor M.S. Shvetsov in whose class he sat in 1923—24. Although surely influenced by Shvetsov, it would signal an advanced case of precursoritis to subordinate Krynine's contribution; he clearly supplied abundant originality.

…ism) and, what is equally important in respect to petrography, mineral composition.... In a nutshell, the relations between diastrophism and sedimentation consist of numerous interdependent factors, which must be considered as a whole and cannot be split into independent variables. Emphasis on one single variable, such as rate of subsidence in the basin of deposition, may lead to highly misleading results." (Note that Twenhofel had earlier said much the same).

"Krynine's diastrophic or tectonic cycle combines subsidence with uplift, both in space and in time, and thus makes it possible (1) to classify sediments genetically in terms of areas of different tectonic activity (space relationships), which follow each other in a certain order (time relationships); and (2) to equate these areas with the morphologic units of the geotectonic cycle, such as geosynclines."

"Incidentally, this work proved to be successful only because a petrographically valid system of classification of sediments had at the same time been evolved by Krynine . . . which had made it possible to recognize and differentiate significant differences between sediment types."

Krynine's tectonic cycle had the following stages and characteristic sediments: (1) *Pre-geosynclinal or peneplanation stage* — first cycle orthoquartzites and carbonate rocks especially characteristic of stable continental platforms. (2) *Geosynclinal stage* — characterized by graywackes, the low-rank (feldspar-poor) ones typifying miogeosynclines whereas high-rank (feldspar-rich) ones typifying eugeosynclines. (3) *Post-geosynclinal or orogenic stage* — characterized by coarse, non-marine sediments, which are arkosic if granitic rocks were exposed to erosion. The cycle is closed (and a new one commenced) by peneplanation again.

The impact of Krynine upon sedimentology during the forties and fifties was profound, indeed. His prickly style was calculated to stimulate people, whether they agreed with him or not. It was also calculated to assure that he was not ignored. He proved beyond doubt that stratigraphers focussing only on geometries of strata were missing a great deal of important evidence for illuminating tectonic history if they ignored petrography. Pettijohn had simultaneously been investigating sedimentary rocks by petrographic means, but unlike Krynine, he complemented his microscopic work with much more attention to texture, sedimentary structures, and especially paleocurrent analyses. Meanwhile, W.C. Krumbein, L.L. Sloss and E.C. Dapples at Northwestern University were developing their approaches to regional stratigraphic or facies analysis. They recognized the significance of Krynine's scheme, and tried to adapt the major sediment clans to their sedimentary-tectonic elements, which included ***stable shelf***, ***unstable shelf***, ***intra-cratonic basin***, and ***geosyncline***. Although most people would have been flattered by the Northwestern recognition, Krynine had this characteristically pungent assessment: "They quote, but do not utilize, the author's ideas of the diastrophic and geosynclinal cycle of sedimentation (p. 745)... . The very moderate success of this (their) effort suggests that the proper genetic bridge between sedimentation and tectonism has not been quite established." (p. 746) (by implication it was still waiting for Krynine).

The Era of classification

During the 1950s there was feverish activity in sandstone petrography with the appearance of many new classifications. Most notable were Folk's 1951 formalization of the conceptually powerful notion of separately distinguishing compositional and textural maturity, and the classifications of Pettijohn (1957) and C.M. Gilbert (in Williams et al., 1954). Both of the latter emphasized texture as well as composition more than did Krynine's. Gilbert's classification, which was made in California, accommodated more consciously the almost endless varieties of volcaniclastic and other lithic sandstones than did the classifications that evolved in the eastern part of the continent where such types are far less common. Perhaps the most delightful highlight of this period was Krynine's provocative 1956 paper presented at the SEPM meetings in Chicago and entitled "Alice in Graywackeland". It was a scolding of those (most everyone else) who "mis-used" *graywacke*. At least thirty people followed P.D. off to an anteroom after his presentation, and the one-sided discussion rambled on there for at least an hour longer.

By 1960, sandstone classifications were proliferating at a breath-taking rate, and a host of misleading, loose generalizations had gained currency. Kay and others had emphasized repeatedly the great variability of sediment types in different classes of geosynclines, but the appeal of the supposedly genetic Krynine sandstone classification (and all of its many derivatives) was too strong. Wishful thinking and simplistic order imposed upon nature by man ignored the innumerable important exceptions that exist in the real world. Soon graywacke, turbidite, and flysch became more or less interchangeable with one another, and all three became equated with eugeosynclines even in the absence of associated volcanics. Likewise miogeosynclines became equated with thick carbonate rocks even though important clastic wedges also occur in them.

> "Sediments in the continental borders are of many kinds . . . Generalizations are somewhat misleading, for rocks that are relatively uncommon as a whole are preponderant in certain systems and regions . . . A class of geosynclines may be characterized by having some sorts of sediments in larger proportion than in another class, but changing conditions through time result in most kinds of sediments being present in some degree in all sorts of geosynclines." (Kay, 1951, p. 70).

Geosynclinal sedimentary cycles continued to be postulated in the fifties, too; Van der Gracht in 1931 had transplanted to America *flysch* and *molasse*, which were originally defined on megascopic criteria in Switzerland. The unfortunate connotation of respective pre- and postorogenic timing came along as baggage. Krynine subsequently developed his more sound petrographic basis for discriminating the so-called tectonic cycle, which has been mentioned already. Then in the 1957 edition of *Sedimentary Rocks*, Pettijohn attempted to make some order out of the chaos by showing that the sedimentary record commonly indicates *several* successive "cycles" of subsidence followed by sediment filling up to or even above sea level. Nevertheless, most workers dogmatically fixed upon a simple, single cycle linked to Krynine's three sandstone clans.

In retrospect the fifties bring to mind the sardonic observation attributed to Oliver Wendell Holmes that "No generalization is worth a damn, including this one." Still, out of this stormy period emerged a new, much better integrated and more genetic approach to sedimentary petrology. Neither geometric descriptive stratigraphy at one extreme nor sandstone petrography at the other was adequate alone. Regional paleocurrent and textural patterns as well as hydrodynamic analysis of sedimentary structures now supplemented those older approaches to provide a leap forward in knowledge of the sediments of many different tectonic elements, but especially of orogenic belts. This renaissance was to prepare sedimentology for an immediate role in assessing the new global tectonics when it developed in the late sixties (see Dickinson, 1971, 1974; Dott, 1974).

Climate — A neglected factor

The turn-of-the-century dogma that abundant detrital feldspar indicates an arid or cold climate with inhibited chemical weathering was one of those countless intuitively appealing, but empirically unfounded, generalizations that has long characterized geology. Reed in 1928 showed that feldspar was abundant in California sandstones of all ages and which represent wide variations of paleoclimatic humidity and aridity. At this same time, Krynine observed arkoses forming in the humid tropics due to great rapidity of erosion. From this he formulated the profoundly simple observation that "sedimentary characters were not the result of *absolute* conditions, but rather developed from the *relative* values of interacting factors... . By golly, fellows (he told classes), you can get 16 by adding 10 to 6, or equally well by squaring 4." (Folk and Ferm, 1966, p. 852).

Even so, Krynine himself and (especially) those who followed him, proceeded in the forties and fifties to subordinate climate by assuming that its effects were masked by tectonism, which also subsumed topography, rates of subsidence, and the like. But Garner (1959) showed in the Andes that from the same source rocks and topography, very different modern clastic sediment assemblages are derived from humid and arid areas. Crook (1967) also has argued for more consideration of climatic factors, and Young et al. (1975) have indicated that statistically significant differences in mineral assemblages should be recognizable in first cycle sandstones derived from climatically different provenances. Some strata seem inexplicable without an important climatic influence. An example may be the Baraboo and Sioux Quartzites, which contain practically no other significant phyllosilicate minerals than the pure aluminous silicate pyrophyllite. It is difficult to imagine how alumina became so purified in the original material if not by weathering.

Some current refinements

Krynine in the forties had helped to turn sedimentary petrographers' attention away from complete preoccupation with heavy minerals toward a more balanced consideration of the total assemblage. His student R.F. Folk extended the attention to quartz types, which was in turn critically reinvestigated by Folk's former student H. Blatt (Blatt and Christie, 1963) and also by L.J. Suttner (Basu et al., 1975). The feldspars as potential provenance indicators are now being reexamined critically as well (Blatt, 1967; Basu and Suttner, 1975). The experimental generation by J.T. Whetten and J.W. Hawkins (1970) of matrix by the breakdown of unstable primary detritus has confirmed W.A. Cummins' (1962) heretical suggestion that most graywacke matrix is diagenetic. Major advances in knowledge of the polytypes of phyllosilicates now promise the possibility of distinguishing rather definitively detrital from authigenic or diagenic micas, clays and chlorites (e.g. J.B. Hayes, 1970).

The more we look the more we find. Odom et al. (1977) provide added documentation of the dependence of sandstone composition on grain size, and have shown that classic supermature quartz arenites actually contain abundant feldspar in their fine fraction (<0.12 mm). Dickinson's detrital mode analysis scheme (1970) for compositionally immature, quartz-poor graywackes and arkoses has provided a much-needed systematic guide for dealing with these sometimes bewildering sandstones. The increasing recognition in the past decade of extensive zeolite and higher-grade (prehnite-pumpellyite and albite-epidote) alteration of these same types of sandstones has been another significant advance, although the results are often frustrating in the extreme. How many zeolite species *are* there in sediments (see Hay, 1966)?

As both a larger and more detailed base of petrographic information becomes available, we should be able to return to many of the oldest questions of sandstone petrography and sedimentary tectonics. Perhaps one day we can definitively say whether or not mature quartz arenites are possible as first cycle deposits, or if they are, after all, due primarily to intrastratal solution over time as Dapples has suggested (1972). Perhaps we can also judge at last the true significance of the general increase in compositional maturity of sandstones and shales (at least of cratons) with age (see Pettijohn et al., 1972). Further studies of vertical changes of composition of detrital sediments (e.g. Schwab, 1969) in relation to the larger-scale evolution of the crust will complement geochemical mass balance arguments (Garrels and Mackenzie, 1971). Some vertical changes in the stratigraphic record formerly attributed uncritically to tectonism, however, are now seen as due simply to delta distributary shifts, fluvial flushings of stored sediments — a perfectly normal behaviour of rivers — sea-level changes, or climatic changes. External worldwide influences such as sea-level changes must be filtered from the local tectonic and climatic effects, especially within orogenic belts, in order to separate local signals from background noise (Dott, 1964b).

During the 1950s interest in continental drift was revived on a different front by paleomagnetic studies, which seemed to demand that continents occupied different relative positions in the past. By 1960, drift had acquired a new respectability even in America, where during the twenties a president of the American Philosophical Society, W.B. Scott, had pronounced it "Utter damned rot". Ironically, after having seemed to provide a death stroke for drift in the thirties, it was geophysics that now resurrected it.

B.C. Heezen (1960) showed that ocean ridges have rift zones along their crests much like those of East Africa. In 1961 (published 1962), H.H. Hess, that erstwhile student of ocean basins, arcs and ultramafic rocks, speculated

Fig. 5. Kay's famous Ordovician paleogeographic map of North America showing paired geosynclinal belts after Stille — inner *miogeosynclines* and outer volcanic *eugeosynclines* — and of intra-geosynclinal *volcanic* and *tectonic* lands composed of contemporaneous rocks as opposed to the old, extra-geosynclinical borderlands composed of older, pre-Paleozoic rocks. (From Kay, 1951, pl. 1.)

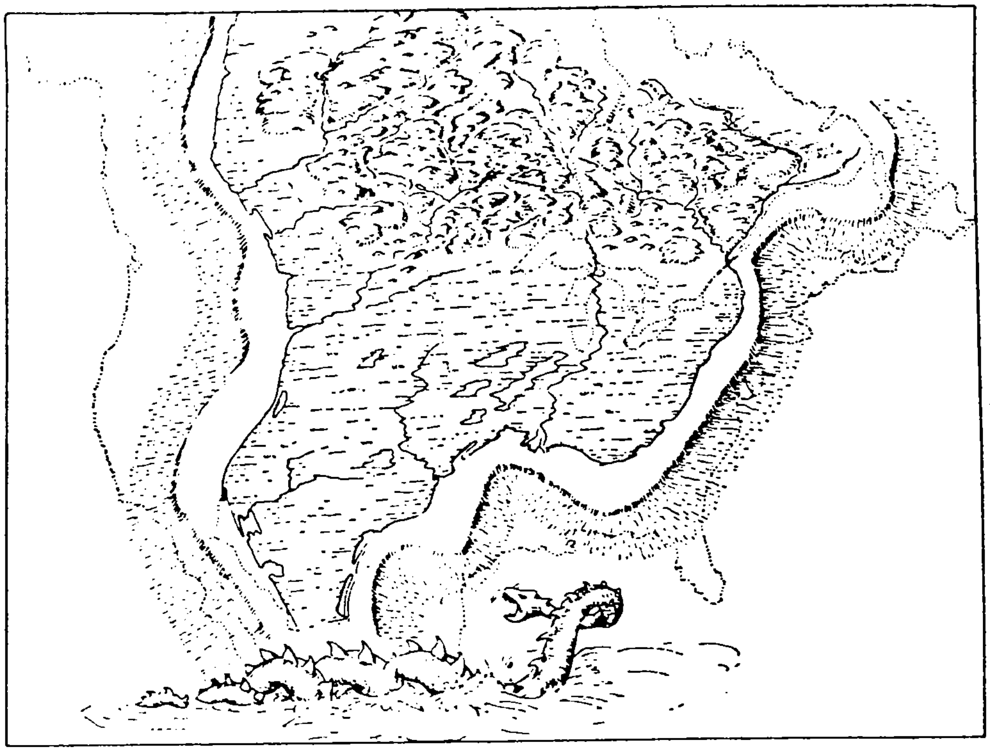

Fig. 6. Dietz's and Holden's (1966, Fig. 8) modification of Kay's Ordovician paleogeographic map (Fig. 5) interpreting the miogeosynclines as continental shelves (as Kay himself had done) and reinterpreting the eugeosynclines as entirely deep continental rises lacking any volcanic or tectonic islands. While there is compelling evidence in Ordovician rocks, for both volcanic and tectonic lands, Kay's map showed too many such islands; the most correct restoration probably lies between these two extremes.

that perhaps ocean ridges were not only sites of rifting and extension, but also of the addition of new oceanic crust by igneous activity. Thus he suggested that new crust forms as the sea floors continually spread away from the ridges. Mantle convection was invoked again as the driving mechanism for spreading. Sea-floor spreading was demonstrated to most people's satisfaction by Englishmen J.D. Vine and D.H. Mathews in 1963 with their explanation of oceanic linear magnetic anomalies symmetrical with the ridges as the result of known reversals of the earth's magnetic field.

Marine geologist R.S. Dietz was deeply impressed by Hess' original oral presentation, and began exploring its implications. Dietz had long been interested in the origin of continental shelves. Now he invoked a suggestion in earlier marine geophysical work by C.L. Drake and others offshore from eastern North America and hypothesized that shallow-water sediments beneath continental shelves were young counterparts of the Stille-Kay miogeosyncline while the deep-water continental slope and rise rather than vol-

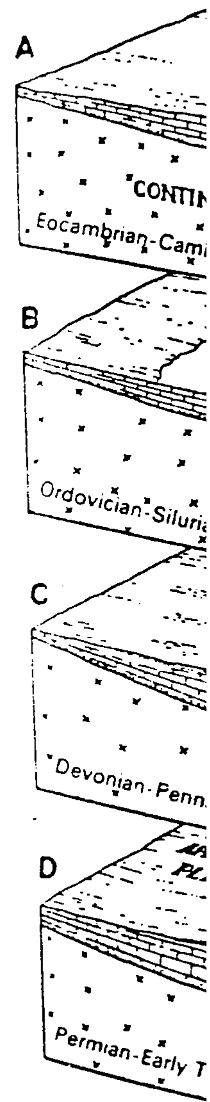

Fig. 7. Dietz's
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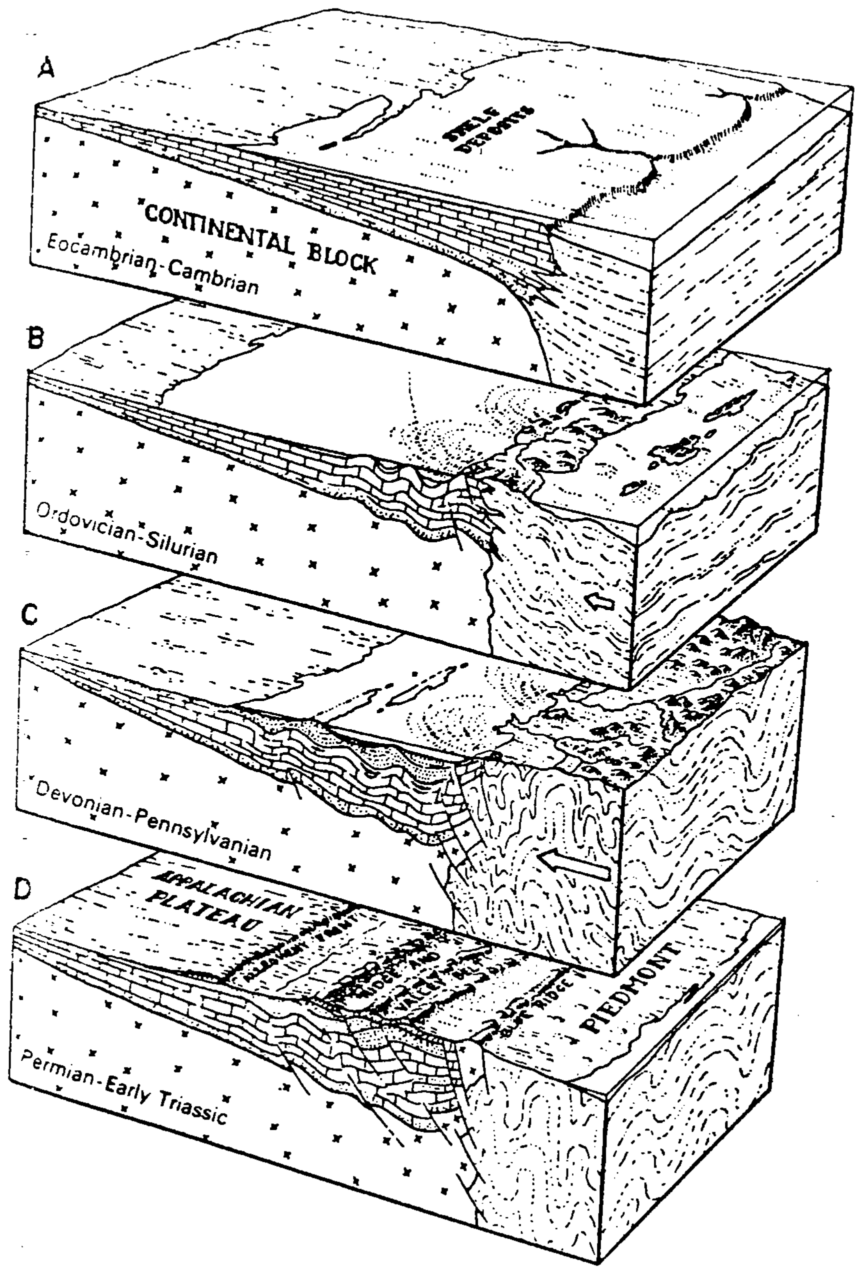

Fig. 7. Dietz's and Holden's (1966, Fig. 5) diagrammatic evolution of the Appalachian region according to plate tectonics. A. The shelf-rise (miogeocline) restoration for Eocambrian to mid-Ordovician time; that is, during a pre-subduction regime when eastern North America was a passive continental trailing edge. B. Beginning of the subduction regime (Late Ordovician), which continued intermittently through later Paleozoic time (C), until Mesozoic and Cenozoic time (D), when the region resumed a passive, trailing edge habit.

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canic arcs and marginal seas were analogues of eugeosynclines (Dietz, 1963; Dietz and Holden, 1966). He argued that the sediments form a seaward-thickening wedge rather than a downwarped synclinal form, and therefore substituted *miogeocline* and *eugeocline* as additions to the semantics of geosynclinal taxonomy (Figs. 5, 6, 7). In reality, Dietz's miogeocline-eugeocline couplet was practically synonymous with Kay's already-defined *paraliageosyncline* rather than mio- and eugeosyncline. In attempting to construct a complete theory, Dietz postulated a new tectonic cycle in which spreading first separated continents and cast them adrift. Continuing spreading widened the new ocean basin, and as this occurred, sedimentation at continent margins produced thick wedges of shallow shelf and deep rise deposits. Further spreading then crushed his eugeocline with an oceanic floor against the shelf edge and its continental floor. The deeper part of the crumpled wedge presumably became heated enough to produce metamorphism and igneous intrusions (Fig. 7). Finally, further compression pushed the now deformed wedge up into mountains at the end of the cycle (â la Dana). It was just the old tectonic cycle and continental accretion warmed over.

Dietz lacked first-hand knowledge of ancient mountain belts, thus also of the things long called ancient geosynclines (strictly orthogeosynclines of Stille and Kay). His simplistic picture violated the facts in several ways, and it suffered from being based only upon the single Appalachian model. Most importantly, there is no evidence of long-continued volcanism contemporaneous with sedimentation on continental rises. Even if there were, the andesitic suite so common in ancient eugeosynclines could not be expected there. Secondly, his scheme did not account adequately for the abundant evidence of episodic uplifts of tectonic and volcanic lands within eugeosynclinal belts repeatedly *throughout* their histories (see above discussion of Pettijohn).

Dietz's light writing style and his colleague J.C. Holden's clever cartoons gave appeal to the so-called actualistic model of geosynclines, especially for those not closely involved with ancient-mountain geology. Although his hypothesis had flaws, Dietz must be credited with arousing much interest, especially of marine workers, in the possible relations of the new sea-floor spreading hypothesis to ancient geosynclines and mountain building, and thus with stimulating research at the interface between marine and land geology. Especially valuable was his focussing the attention of students of ancient rocks upon the nature of continental shelves. J.T. Wilson in 1966 added much impetus to ideas of rifting of continents and spreading of new ocean-crust between. He suggested that a Paleozoic Proto-Atlantic ocean had formed in this way, then had closed by the subduction of oceanic material, finally culminating in collision of continents. J.C. Rodgers, in 1968, published an important paper drawing parallels between the Eocambrian to Ordovician carbonate rocks of the Appalachians with modern shelf carbonate banks on passive continental margins facing deep ocean basins (as in A of Figs. 7 and 8). Suddenly the stratigraphic record not only of the Appalachian belt but also of most other mountain ranges took on a new meaning.

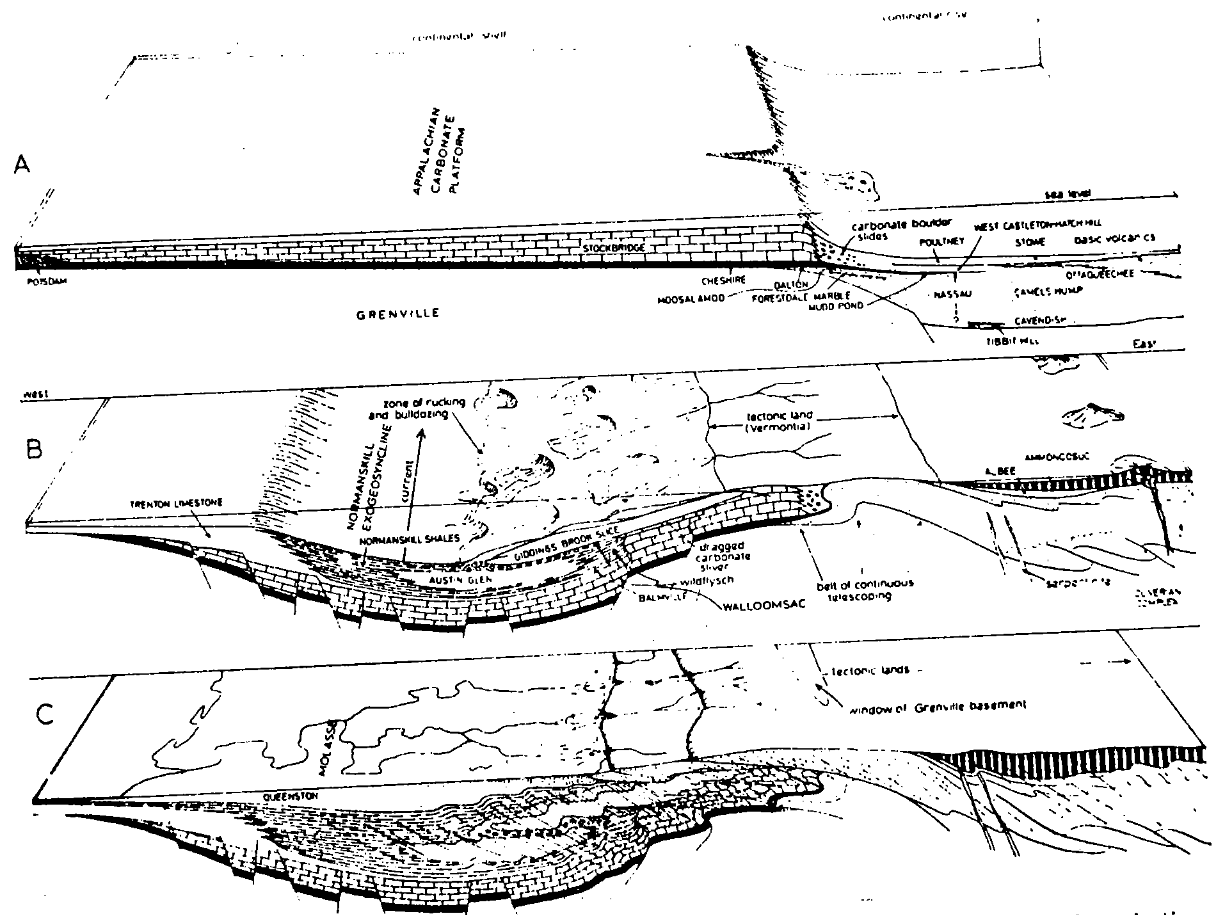

Fig. 8. Bird's and Dewey's (1970, fig. 7) restoration of the New England region in early Paleozoic time in terms of plate tectonics (compare Figs. 3, 5 and 7). A. Pre-Taconian (trailing edge). B. Early Taconian (early subduction). C. Late Taconian (continuing subduction).

Many, but not all, miogeosynclinal sequences (e.g. the pre-middle Ordovician of the Appalachians) represent vast shelf banks formed on rifted passive or trailing continental edges during early sea-floor spreading without active contemporaneous volcanism and orogeny. Those phenomena, so characteristic of island arcs and active mountain belts, came later during a subduction phase (e.g. beginning in mid-Ordovician time in the Appalachian belt; Fig. 8) when the continental margin became tectonically active. Thus the Dietz-Wilson-Rodgers revelations led to the recognition of profound tectonic discontinuities formerly overlooked (or underemphasized) in the histories of most mountain belts. The pre-orogenic carbonate bank strata formed on *passive* (*Atlantic-type*) continental margins find themselves crumpled and upheaved in mountains quite by accident when those margins are converted to the *active* (*Pacific-type*); unlike most of the clastic sediments and volcanic rocks of such regions, such banks had no genetic relation to mobile belt tectonics.

Meanwhile, in the early sixties, the "old" geosyncline was being challenged on another front, too, namely by some students of island arcs and ancient mountain belts. Hess (1960) questioned if arcs necessarily were destined to become mountains. Maybe they are just a different expression of the oceanic realm of mountain building tectonics. Dott (1964a) argued from the diverse positions of mountain belts with respect to continental cratons and ocean basins that mountain-building processes do not respect crust type. This being the case, then thick sedimentation must be strictly a matter of available source. An orthogeosyncline was seen as a sediment-filled mobile belt while trenches were sediment-starved ones. Suess seemed correct in doubting the necessity of thick sedimentation for mountain making. Meanwhile, the concept of accretion of orogenic belts to continental cratons was not standing up well to detailed field and isotopic dating evidence. Instead of adding new continental crust, it was becoming increasingly clear that such belts rework very old continental crust in complex mosaic patterns over time. Many of the old causality dogmas were being discredited and even reversed (Coney, 1970).

In 1965 the theory of plate tectonics was announced in essentially complete form by J. Tuzo Wilson, thus beginning one of the most dramatic revolutions of geologic thought ever. It swept the profession with unprecedented rapidity. This new global tectonics combined sea-floor spreading with continental drift, drawing initially upon global earthquake seismology patterns and marine geology and geophysics. Very soon geologists began applying the new concepts to the ancient rock record. In 1970, J.F. Dewey and J.M. Bird presented a plate-tectonic theory of mountain building, which was followed by other important contributions bearing upon the interpretation of geosynclines in plate-tectonic terms (Mitchell and Reading, 1969; Dickinson, 1971; Dott, 1974). The results have drastically altered many long-cherished conceptions of the geosyncline, and have at the same time greatly clarified much of the confusion that had developed around it (e.g. Fig. 8). At last the sig-

[illegible] of Kay's many different classes could be fully appreciated. Disregarding the cratonic basin types, many classic miogeosynclines, as noted above, represent carbonate banks on passive (trailing) plate margins and had nothing to do with orogenesis. Their presence in mountain belts is pure coincidence — an inheritance from a previous, totally different tectonic setting. Most eugeosynclines, as long thought, represent ancient magmatic arcs directly related to active orogenesis. Exogeosynclines represent the debris from (and deposited next to) rising mountains, and taphrogeosynclines are simply rift basins, many of which seem to be the result of initial continental rifting and sea-floor spreading. The epieugeosyncline seems to be so hybrid a category as to have little interpretive value; the paraliageosyncline has been discussed above. Some of Kay's tectonic lands have turned out to be the edges of adjacent continents uplifted by collision. Now that the genetic distinctions of geosynclinal classes are clear, it is doubtful that the names serve any further need. Indeed, there is every reason to believe that they confuse rather than clarify, and that new models for sedimentary basins deduced from the plate theory will be more fruitful (Dickinson, 1974).

CONCLUSIONS

While the geosyncline's geneology must be characterized as a semantical jungle, nonetheless it has provided a very important rallying theme for sedimentation and tectonics for over half a century. With the advent of a widely accepted unifying genetic theory in plate tectonics, the taxonomic emphasis of the past has diminished. The geosyncline has also suffered from oversimplified hypothesizing — a kind of Principle of Over Simplicity. As Marshall Kay once said, "Some things in nature are *not* simple." Orogenic belts are the most complex zones of the earth's crust, and to trying to understand them has required synthesis of many very diverse fields. More than its share of fallacies have been committed in the geosyncline's name. Most obvious is the single-model syndrome whereby early Americans considered only the Appalachian and early Europeans only the Alpine examples. The long-assumed causality between thick sediments and mountain uplift provides another example. More recently the vogue of an actualistic model or theory for the geosyncline as providing the latest path to truth has further clouded the scene. Kay's comparisons between eugeosynclines and volcanic arcs and between his paraliageosyncline and modern continental shelf-rise prisms were as actualistic as any other analogies.

Finally, the cynic might shrug and say "so what is new, really?" Taylor thought of continental drift and collisions as a cause of mountain building, Ampferer, Willis and others envisioned underthrusting (subduction) of oceanic crust beneath continents and arcs. Suess and Geikie doubted any causal link between thick sedimentation and mountain building, and Neumayr, Haug and Steinman had inferred existence of deep-sea floor rocks high in mountain ranges more than fifty years before sea-floor spreading and plate tectonics! But of course these originated as shrewd speculations, which could not be proven definitively over others in the market place of ideas because of the great ambiguities of the rock record in mountains.

We live in an era of model building in earth science, and the plate theory has evolved with breathtaking rapidity from an attractive new hypothesis to a somewhat dogmatic new orthodoxy. It was a wise person who said that "If your observations agree with your model, that's nice; but if they do not, then that is interesting." It is argued that science can disprove, but cannot, strictly speaking, prove hypotheses (a process sometimes called *strong inference*). Accordingly, it should be the goal of the scientist to try to falsify hypotheses by observation and experiment. R.J. Chorley (1962) also has argued that our models influence interpretations just as much as do data. Therefore, on all grounds we should not allow ourselves to be hypnotized by plate models or any others. All should be regarded with healthy skepticism as useful but transitory ideas to be tested. For evaluating pre-Mesozoic plate actions, we are dependent completely upon the ancient rock record, which means that sedimentologists inevitably must play a major role. Ancient magmatic arc edifices, for example, are almost entirely destroyed, therefore most of their record lies in sediments derived from them. Already it is clear that sandstone petrology can provide powerful clues to different types of ancient plate margins.

The sedimentologist, like any other scientist, is confronted with two conflicting passions, as William James put it long ago; one for *discovery of new knowledge* and the other for *avoidance of error*. These are in many ways contradictory of one another, and each person balances them differently according to his or her temperament. The second passion may prevent the cautious worker from satisfying the first, and vice versa. This inevitable circumstance illustrates what an intensely human activity science really is.

ACKNOWLEDGEMENTS

I am indebted to G.V. Middleton for encouraging me to prepare an oral presentation on sedimentation and tectonics for a symposium commemorating the fiftieth anniversary of the Society of Economic Paleontologists and Mineralogists in Denver in November of 1976. He also offered important critical advice as did F.J. Pettijohn and L.L. Sloss. A.D. Baillie, R.N. Ginsburg, and Cesare Emiliani suggested publication. Discussions over the years with Marshall Kay, E.C. Dapples, R.G. Walker, W.R. Dickinson, R.L. Folk, E. McBride, J.C. Ferm, H. Blatt, and many others have contributed materially to the writing of this paper. Lastly, I must acknowledge an enormous debt to the countless authors — both the few cited and the many uncited ones — whose writings have helped to make that undefinable but important and fascinating something called sedimentary tectonics.

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