Empiricism and model building in stratigraphy: The historical roots of present-day practices

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ABSTRACT: The science of Stratigraphy has, since its inception in the late eighteenth century, been characterized by two contrasting research modes or "cognitive styles" (Rudwick 1982). Empirical (inductive) descriptive stratigraphy began with William Smith, led to the establishment of a data base of stratigraphic units (Murchison, Sedgwick, Lapworth), and formed the basis for modern work to establish and refine a detailed chronostratigraphic time scale (Van Hinte, Berggren). Other workers (Hutton, Lyell, Darwin, Chamberlin, Ulrich, Umbgrove, Sloss, Vail) have sought to identify underlying geological controls, and have built deductive models to explain earth processes, beginning with Hutton's uniformitarianism. Many such models sought evidence of regularity or cyclicity in earth processes ("the pulse of the earth"), including the modern "global-eustasy" model of Vail.

There is an ever present danger that models can drive the analysis and presentation of data, particularly where stratigraphic models have been invoked to explain, clarify or codify the stratigraphic record. These problems are not new. Attempts to apply European chronostratigraphic units to North American stratigraphy in the early twentieth century were accompanied by expectations that unit boundaries would be marked by lithologic events, such as unconformities. These expectations were not supported, and this may have been the basis for North American attempts to establish alternative stratigraphies, including what became sequence stratigraphy. Ulrich (1911) thought that stratigraphic successions were created by "diastrophic cycles", and was concerned that regional correlations of these successions did not appear to be supported by the biostratigraphic evidence. Barrell (1917) was one of the first to understand the problems created by the lack of representation of long intervals of time in the geologic record, and developed ideas concerning the relationship between base level change and sedimentation that we now term "accommodation."

Modern work on the chronostratigraphic time scale is based on empirical principles, culminating in the definition of global section and boundary stratotypes for the major chronostratigraphic units. However, a controversy has recently arisen over the preference by some geologists to use distinctive marker events to define boundaries. In some cases, this involves introducing hypotheses about the global extent and geological superiority of such events, rather than relying on the accumulated historical record of biostratigraphic and other data.

INTRODUCTION

Geology is historically an empirical science, firmly based on field data. Hallam (1989, p. 221) has claimed that "Geologists tend to be staunchly empirical in their approach, to respect careful observation and distrust broad generalization; they are too well aware of nature's complexity." However, interpretive models, including the modern trend towards numerical modeling, have become increasingly important in recent years.

The empirical approach to geology, including the building of models, is *inductive* science, whereas the use of a model to guide further research is to employ the *deductive* approach. This methodological difference was clearly spelled out for geologists by Johnson (1933). Frodeman (1995) recently reviewed the work of the German philosopher Heidegger, arguing that the practice of the science of geology illustrates a process termed the *hermeneutic circle*, in which induction and deduction supposedly follow each other in an iterative process of observation, generalization and theorizing (*induction*), followed by the construction of hypotheses and the seeking of new observations in order to test and abandon or refine the theory (*deduction*). Ideally, this is a continuous and circular process (text-fig. 1), but we have argued elsewhere (A. D. Miall and C. E. Miall, this volume, in a paper that expands on the implications of hermeneutics for stratigraphy) that at the present day there are separate groups of stratigraphic researchers that are separately following these two different methodological approaches in partial isolation from each other. The purpose of the present paper is to argue that this dichotomy has deep historical roots; that from the mid-nineteenth century to the present, the inductive and deductive approaches to the science of stratigraphy have largely been followed by different groups of researchers having different objectives, and that throughout much of the history of the science, the groups have had little to do with each other.

Since modern stratigraphic studies began in the late eighteenth century a central theme of stratigraphic research has been the empirical construction of a vast data base of descriptive stratigraphy, focusing on the occurrence and relative arrangements of formations and their contained fossils. This data base now constitutes what has come to be called the chronostratigraphic time scale. In recent years, methods of determining the ages of beds by other means, such as by radiometric dating, magnetostratigraphy and chemostratigraphy have added depth to this data base. As we show here, research into the preserved record of deep geological time has grown into an enormously complex, largely inductive science carried out mainly in the academic realm. From this, a descriptive (inductive) classification of Earth history has been built, consisting of the standard eras, periods, epochs, etc.

At various times deductive models of Earth history have been proposed that have had varying levels of success in contributing to our understanding of the Earth's evolution. There have also been many attempts to develop deductive models of stratigraphic processes, including the *cyclothem* model of the 1930s, and modern *facies models* and *sequence stratigraphy*. Ideas about the tectonic setting of sedimentary basins have also included several bold attempts at model building, including the pre-plate tectonic *geosyncline theory* of Kay (1951), the modern *petrotectonic assemblage* concepts of Dickinson (1980, 1981), and the various geophysically-based basin models of McKenzie (1978), Beaumont (1981) and many later workers.

Some of the concepts of sequence stratigraphy, that evolved from seismic-stratigraphy in the 1970s, constitute one of the most recent and most elaborate attempts to develop deductive stratigraphic models. These included the Exxon global cycle chart (Vail et al. 1977; Haq et al. 1987, 1988), which, if it had proved to be a successful explanation of the stratigraphic record, could potentially have become the dominant paradigm, entirely replacing the old inductive classification of geologic time, and largely supplanting the complex set of methodologies with which it was being constructed. As we have discussed elsewhere (A. D. Miall and C. E. Miall 2001; C. E. Miall and A. D. Miall 2002), the two distinct intellectual approaches resulted in the development of two conflicting and competing paradigms which are currently vying for the attention of practicing earth scientists. In a companion paper (A. D. Miall and C. E. Miall, this volume) we argue that current research in the field of cyclostratigraphy may be following a similar pattern of development

The history of stratigraphy since the end of the eighteenth century has encompassed the following broad themes:

1) Recognition of the concept of stratigraphic order and its relationship to Earth history, and the growth from this of an empirical, descriptive stratigraphy based on sedimentary rocks and their contained fossils.

2) The emergence of the concept of "facies" based on the recognition that rocks may vary in character from place to place depending on depositional processes and environments. This was one of the first deductive models developed to facilitate geological interpretation.

3) Recognition that rocks are not necessarily an accurate or complete record of geologic time, because of facies changes and missing section, and the erection of separate units for "time" and for "rocks".

4) Development of a multidisciplinary, empirical approach to the measurement and documentation of geologic time, an unfinished science still actively being pursued to the present day.

5) Attempts at different times to recognize patterns and themes in the stratigraphic succession and to interpret Earth processes from such patterns. Facies models and sequence stratigraphy are amongst the main products of this effort.

6)Attempts to extract regional or global signals from the stratigraphic record and to use them to build an alternative measure of geologic time, based on an assumed "pulse of the Earth."

These themes may be further generalized into a descriptive, inductive approach to the science (themes 1 to 4), which we here categorize as the *empirical paradigm* of stratigraphy, and a distinctly different, interpretive approach to the subject (themes 5 and 6), that we term the *model-building paradigm*. Elsewhere, we have examined the model of global eustasy as applied to sequence stratigraphy (A. D. Miall and C. E. Miall, 2001, C. E. Miall and A. D. Miall, 2002), the use of climate proxies in stratigraphic studies (A. D. Miall and C. E. Miall in prep) and the use of cyclostratigraphy as a potential basis for a refined geologic time scale (A. D. Miall and C. E. Miall, this volume).

The purpose of the present paper is to summarize the parallel development of these two broad approaches to stratigraphy, The discussion is not intended to be historically complete; there is a substantial literature that addresses the history of stratigraphy. Our purpose is only to establish the body of ideas from which the modern controversy over sequence stratigraphy may be better understood. As we hope to show, the mainstream of stratigraphic research has been the development of an ever more precise and comprehensive chronostratigraphic time scale based on the accumulation and integration of all types of chronostratigraphic data. This paradigm is essentially one of meticulous empiricism which makes no presuppositions about the history of the Earth or the evolution of life or of geological events in general. The Vail/Exxon sequence models exemplify a quite different paradigm, but one that inherits an equally long intellectual history. They are the latest manifestation of the idea that there is some kind of "pulse of the earth" that is amenable to elegant classification and broad generalization.

These two strands of research correspond to two of the modes or "cognitive styles" of research in geology that were described by Rudwick (1982). Descriptive stratigraphy and the development of the geological time scale corresponds to the "concrete" style of Rudwick (1982, p. 224), who cites several of the great nineteenth-century founders of stratigraphic geology as examples (e.g., William Smith, Sedgwick, Murchison). These individuals were primarily concerned with documenting and classifying the detail of stratigraphic order. In contrast, the "abstract" style of research includes individuals such as Hutton, Lyell, Phillips and Darwin, who sought underlying principles in order to understand Earth history.

The development of descriptive stratigraphy

The development of the science of descriptive stratigraphy is described by Hancock (1977), Conkin and Conkin (1984), and Berry (1987). Earlier discussions that include much valuable historical detail include those by Teichert (1958) and Monty (1968). The following summary is intended only to emphasize the evolution in methodologies that took place from the latest eighteenth century until they stabilized during the mid-twentieth century.

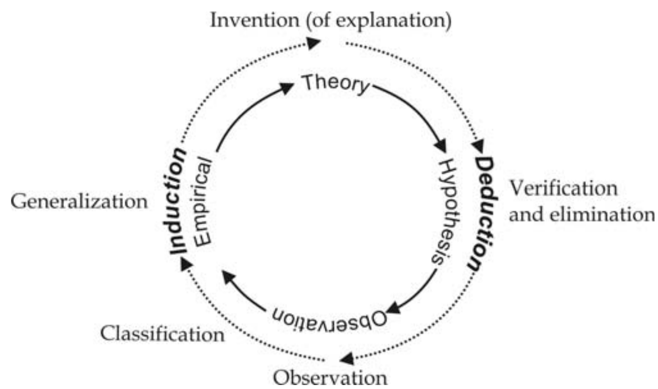
Stratigraphy is founded on the ideas of Richard Hooke and Nicolaus Steno, physician to the Grand Duke of Tuscany. The *Law of Superposition* was enunciated by Nicolaus Steno in his work *Prodromus*, published in 1669. This law, simply stated, is that in any succession of strata, the oldest and first formed must be at the bottom, with successively younger strata arranged in order above. As described by Berry (1987), several rock successions were described during the eighteenth century, primarily because of their importance to mining operations, but no fundamental principles emerged from this work until the four-fold subdivision of the Earth's crust was proposed by Abraham Gottlob Werner.

The foundation of modern stratigraphy is attributed to William Smith, a surveyor for contemporary canal builders, who became interested in the rocks that were being dug into as a series of canals were constructed across southern England (Hancock 1977, p. 3-4; Berry 1987, p. 56-57). His knowledge of geology was self-taught, owing nothing to such illustrious predecessors as James Hutton. As Hancock (1977, p. 5) noted, Smith's work was entirely empirical, free of any attempt at grand theory, and free of any influence of theology—an important point considering the powerful influence of biblical teachings at the time. Smith's work began in the Jurassic strata around Bath, in southwest England. He recognized that the stratigraphic succession was the same wherever he encountered it, and that particular strata could also be characterized by particular suites of fossils. From this inductive base, Smith evolved the deductive principle that he could identify the stratigraphic position of any outcrop by its distinctive rock types and fossil content. He committed his observations to maps that showed the outcrop patterns of his succession, and over a period of about twenty five years he gradually compiled a complete geological map of England and Wales, the first such map of its kind ever constructed (Smith 1815). Because Smith was not a member of the landed or aristocratic class in England, his work was largely ignored until late in his life, when he was appropriately honored by the Geological Society of London. The story is told in detail by Winchester (2001) and Torrens (2001). Others were describing local successions of strata during the late eighteenth and early nineteenth centuries, such as Cuvier and Brongniart who documented the Tertiary strata of the Paris Basin in the first two decades of the nineteenth century (Conkin and Conkin 1984; Berry 1987, p. 66), but Smith was the first to show that rocks, with their contained fossils, constituted mappable successions. Cuvier was more concerned with the history of life on earth (Hancock 1977, p. 6). Brongniart was amongst the first to appreciate the importance of Smith's contribution in creating the possibility of long-distance correlation based on fossils alone, independent of rock type (Hancock 1977, p. 7).

As knowledge of regional stratigraphy evolved in various parts of Europe, the fourfold primary subdivision of Werner was broken down locally into various "series", and these, in turn, were commonly subdivided into local "formations." This was an entirely piecemeal operation, reflecting local interests, but from this gradually evolved a body of descriptive knowledge of rock successions and their contained fossils. As noted by Berry (1987, p. 63):

> Many of the widely used descriptive units did bear fossils that, when analyzed using the principle of faunal succession, proved to be a fossil aggregate diagnostic of a time unit in an interpretive scale; thus many descriptive units became interpretive ones, and today bear the same names. Among the major units of the interpretive time scale that were originally descriptive rock units are the Cambrian, Carboniferous, Jurassic, Cretaceous and Tertiary. Units that were based on interpretation of fossils from their inception are the Ordovician, Devonian, Permian, and the Tertiary Epochs.

During the 1820s and 1830s such workers as Young and Bird in Yorkshire, and Eaton in New York, recognized that some formations changed in character as they were traced laterally (Hancock 1977, p. 7-8). Amand Gressly (1838; see translation in Conkin and Conkin 1984, p. 137-139) was the first to systematize the observation of such changes, with the introduction of the term and concept of *facies*, based on his work on the Jurassic rocks of the Jura region of southern France. For example, he was aware of the differences between the limestones with contained fossils of coral-bank environments, oolitic deposits



TEXT-FIGURE 1
The hermeneutic circle, based on Wallace (1969). The terms around the periphery are those of the five "stages of analysis" of Johnson (1933).

(which we now recognize to be beach deposits), and the "oozy" deposits of deeper-water environments, all of which may form at the same time, and may also form one above the other as environments shift over time. This type of change can be documented by careful observation of rocks in outcrop, by studying the vertical succession of rock types or by tracing an individual set of beds laterally, perhaps for many kilometres. Gressly proposed two new laws: the first that in different places formations ("terrains" in the French terminology) may consist of rocks of different petrologic and paleontological character (the original meaning of the term *facies*, which we still retain), and, secondly, that a similar succession of facies may occur in both vertical and lateral arrangement, relative to the bedding. As Hancock (1977, p. 9) pointed out, this predated Walther's proposal of the law of the correlation of facies by some 56 years. The study of facies became a central activity of stratigraphic work in the 1960s, with the establishment of the *process-response facies model*, as noted above.

With Gressly's concept of facies in place, the stage was set for the next important development, that of the introduction of the concept of the *stage*, by another French geologist, d'Orbigny (1842-1852). He recognized the vertical variability in fossil assemblages within individual series, and realized that stratigraphic successions could be subdivided into smaller units based on careful categorization of these succeeding fossil assemblages. These he called *stages*. D'Orbigny also used the term *zone*, but nowhere clearly defined it (Monty 1968). Teichert (1958) argued that d'Orbigny was inconsistent in his usage, sometimes using the term *zone* as a synonym for *stage*, and sometimes as a subdivision of a stage. He established, if informally, the idea of the ideal "type" of succession, a locality where the stage was well represented, and from this has grown the concept of the *type section* or *stratotype,* to which formal importance has now been assigned as the first point of reference for establishing the character of a stratigraphic unit. Many of the stage names d'Orbigny erected are still those used worldwide to this day. He was aware of the concept of facies change and of the variability in the nature of stage boundaries, from conformable to unconformable. As Conkin and Conkin (1984, p. 83) noted, the importance of d'Orbigny's work is his consolidation and adaptation of ideas that already existed in embryonic form, and the scope of his stage classification, which included the erection of some twenty-seven stages for the Paleozoic and Mesozoic.

Hancock (1977, p. 12) suggested that the true foundation of biostratigraphy came with the work of the German stratigrapher Albert Oppel (1856-1858), whose work also concentrated on the Jurassic succession of western Europe. Oppel extended the ideas of d'Orbigny about the subdivision of successions based on their contained fossils to a more refined level. He recognized that careful study of the contained fossils would permit a much more detailed breakdown of the rocks, into what he called *zones*. He investigated "the vertical distribution of each individual species at many different places ignoring the mineralogic character of the beds" (Oppel, as quoted in Berry 1987, p. 127). Some species were discovered to have short vertical ranges, others long ranges. Each zone could be characterized by several or many fossil species, although commonly one species would be chosen to be used as the name of the zone. Oppel built up stages from groups of zones. Stages were referred to as *zonegruppen*, or groups of zones (Teichert 1958). These would usually fit into the stages already defined by d'Orbigny, but as Hancock (1977, p. 13) noted, in some places his zones spanned already-defined stage boundaries. This was the beginning of a practical problem that has still to be fully resolved; but in many cases, such as at the base and top of the Jurassic, Oppel's zone boundaries coincided with the System boundaries. In practice, zones became the foundation upon which the whole framework of biostratigraphy, the zone, stage, series and system, was gradually built. Teichert (1958, p. 109) emphasized the importance of Oppel's original description of zones as "paleontologically identifiable complexes of strata," not as subdivisions of time. The original concept was therefore clearly inductive—the recognition of a zone depended on the field geologist finding specific fossils in the rocks.

In their summation of the work of d'Orbigny and Oppel, Teichert (1958, p. 110) and Hancock (1977, p. 11) were at pains to emphasize the empirical, descriptive nature of the stage and zone concepts. They suggested that they were later distorted by the introduction of concepts about time that, they claimed, served to confuse the science of stratigraphy for some years. Teichert (1958) attributed these misconceptions to individuals such as H. Hedberg and O. H. Schindewolf. Hancock (1977) argued that Hedberg's influence was detrimental to the development of clear stratigraphic concepts. We address these problems later. However, careful examination and translation of d'Orbigny's original statements by Monty (1968) and Aubry et al. (1999, appendix A) cast a different light on this historical work. Aubry et al. (1999, p. 137) pointed out that in the mid-nineteenth century no clear distinction between rocks and time had been made, and that paleontology was the only means of long-distance correlation. Selected translations from d'Orbigny's work clearly indicate that he envisioned stages as having a time connotation (Aubry et al. 1999, p. 137-138).

Shortly after Oppel's work was completed, Charles Darwin's *The origin of species* was published, and provided the explanation for the gradual change in the assemblage of species that Oppel had observed. However, resistance to the concepts of the stage and the zone remained strong through the remainder of the nineteenth century in Britain and the United States, where the concept of facies had also still not taken hold, and reliance for correlation tended to still be placed on the lithology of formations. (Hancock 1977, p. 14-15).

International agreement on the definition and usage of most stratigraphic terms was attempted at the first International Geological Congress in Paris in 1878. A commission was established, that subsequently met in Paris and in Bologna. At the latter meeting, in September-October 1880, the commission:

> Decided on definitions of stratigraphic words like series and stage, and listed their synonyms in several languages …. Rocks, considered from the point of view of their origin, were formations; the term was not part of stratigraphic nomenclature at all, but concerned how the rock had been formed (e.g., marine formations, chemical formations). Stratigraphic divisions were placed in an order of hierarchy, with examples, thus: group (Secondary Group) [what we would now term the Mesozoic], system (Jurassic System), series (Lower Oolite Series), stage (Bajocian Stage), substage, assise (Assise à *A. Humphriensianus*), stratum. A distinction was made between stratigraphic and chronologic divisions. The duration of time corresponding to a group was an era, to a system a period, to a series an epoch, and to a stage an age (Hancock 1977, p. 15).

Definitions of the terms zone and horizon were added to the published record of this meeting by the secretary of the commission, based on national reports submitted by the delegates, but full international agreement was slow in coming (Hancock 1977, p. 16). Nonetheless, by the early twentieth century the following basic descriptive terms and the concepts on which they were based had become firmly established, if not universally used:

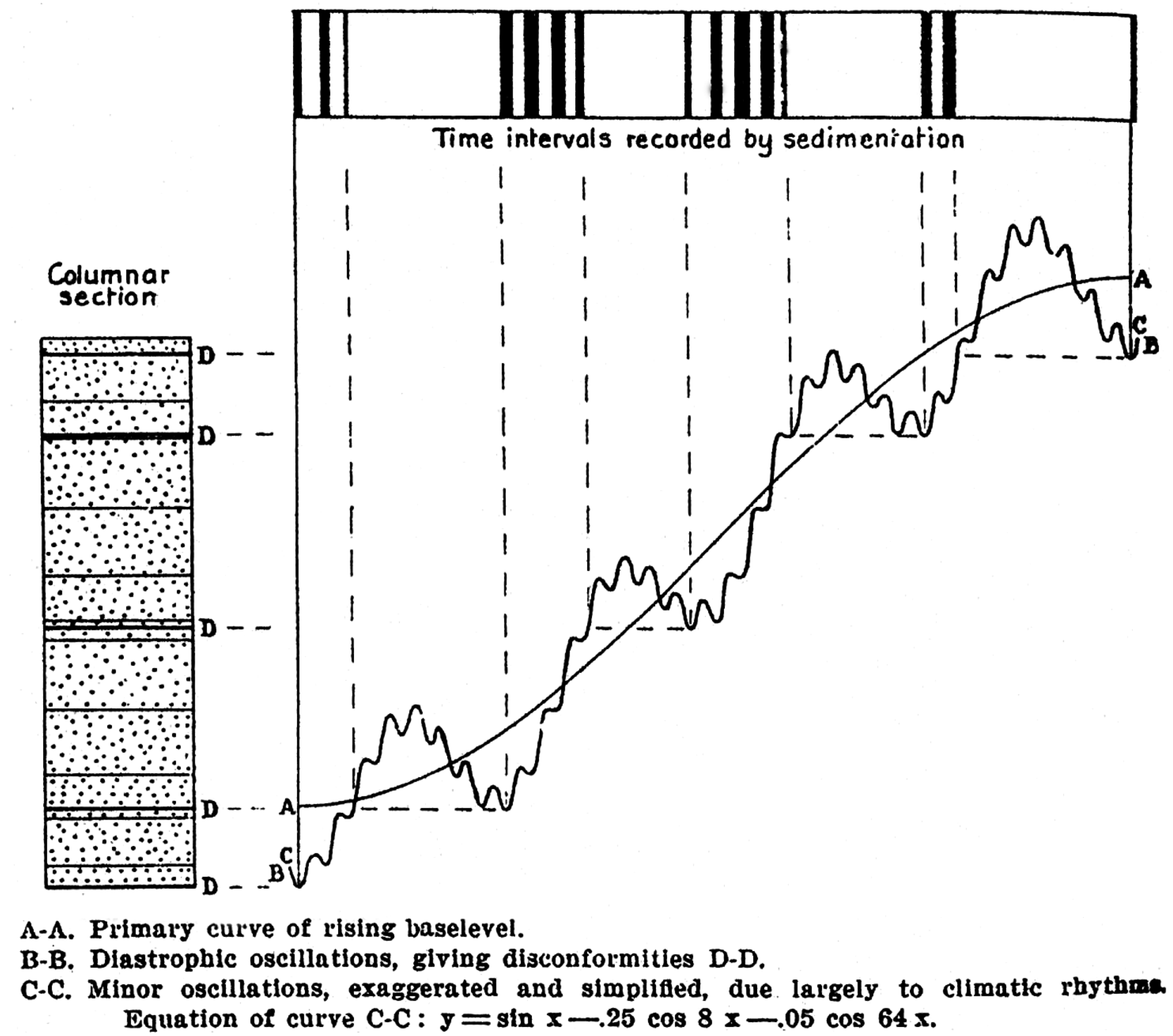
- Law of superposition of strata
- Stratigraphic outcrop maps based on the succession of sedimentary rocks with its contained fossils
- Facies
- Stage
- Type section or stratotype
- Zone

According to Teichert (1958), the term *biostratigraphy* was introduced by the Belgian paleontologist Dollo in 1904, for the "entire research field in which paleontology exercises a significant influence on historical geology."

Codification of these principles by the mid-twentieth century is illustrated by the work of Schenk and Muller (1941). Various national and international stratigraphic codes and guides have been developed that standardize and formalize the definitions of stratigraphic terms and set out the procedures by which they should be used. An international guide was published in 1976 (Hedberg 1976), with a major revision appearing in 1994 (Salvador 1994). A code for North America, based on the international guide, appeared in 1983 (NACSN 1983).

Do stratigraphic units have "time" significance?

As long ago as 1862 Huxley wrote: "neither physical geology nor paleontology possesses any method by which the absolute synchronism of two strata can be demonstrated. All that geology can prove is local order of succession." (quoted in Hancock 1977, p. 17). As an example, Huxley suggested that there was no way to prove or disprove that "Devonian fauna and flora in the British Isles may have been contemporaneous with Silurian life in North America, and with a Carboniferous fauna and flora in Africa." The variations in fauna and flora could simply be due to the time it took for the organisms to migrate. There is therefore a need for a distinction between "'homotaxis' or 'similarity of arrangement' and 'synchrony' or 'identity of date"



TEXT-FIGURE 2
Barrell's (1917) explanation of how oscillatory variations in base level control the timing of deposition. Sedimentation can only occur when base level is actively rising. These short intervals are indicated by the black bars in the top diagram. The resulting stratigraphic column, shown at the left, is full of disconformities, but appears to be the result of continuous sedimentation.

(Hancock 1977, p. 17). Conkin and Conkin (1964) suggested that this concept was first enunciated by DeLapparant (1885; as cited and translated by Conkin and Conkin 1984, p. 243.), although Callomon (2001, p. 240) stated that the Principle of Biosynchoneity", whereby beds with similar fossils are assumed to be the same age, is "usually ascribed to William Smith". As geologists accumulated a very detailed knowledge of the succession of fossils, the assumption that the same succession of fossil assemblages indicated synchroneity assumed the status of a truism. However, until the development of radiometric dating and the growth of modern chronostratigraphy (see below) the true "time" value of fossils remained a problem, because biostratigraphy provides only relative ages. The basic assumption about the temporal value of fossils was first made most clearly by Lyell (1830-1833) and, according to Conkin and Conkin (1984; see in particular their Table 1, p. 2), subsequent developments by Bronn (1858), Phillips (1860), Lapworth (1879), DeLapparent (1885) and Buckman (1893) established the main framework upon which this part of modern stratigraphy is built.

One of Lyell's (1830-1833) most important contributions was his detailed study and classification of Tertiary deposits, based on their contained fossils. Berggren (1998) provided a succinct summary of this important contribution. Lyell's subdivisions of the Tertiary were based on the idea that, through the course of time, contemporary faunas become more and more like those found at the present day. Under a heading "The distinctness of periods may indicate our imperfect information" he stated:

> In regard to distinct zoological periods, the reader will understand …. That we consider the wide lines of demarcation that sometimes separate different tertiary epochs, as quite uncon-

> nected with extraordinary revolutions of the surface of the globe, as arising, partly, like chasms in the history of nations, out of the present imperfect state of our information, and partly from the irregular manner in which geological memorials are preserved, as already explained. We have little doubt that it will be necessary hereafter to intercalate other periods, and that many of the deposits, now referred to a single era, will be found to have been formed at very distinct periods of time, so that, notwithstanding our separation of tertiary strata into four groups, we shall continue to use the term *contemporaneous* with a great deal of latitude (Lyell 1833, v. 3, p. 56-57).

This quote contains most of the modern concept that units defined on the basis of their fossil content may have global significance with regard to contemporaneity, but that the preserved record may be imperfect. Lyell's "lines of demarcation" are what we would now define as chronostratigraphic boundaries. These were commonly drawn at unconformities until the introduction of modern practices, as described below.

Phillips (1860, p. xxxii), in comparing fossil successions between localities in different parts of the world (he mentioned several Paleozoic successions that had been described from Europe and North America), suggested that "the affinity of the fossils is accepted as evidence of the approximate contemporaneity of the rocks." Phillips (1860, p. xxxvi) referred to the work of Charles Darwin to explain the succession of forms, replacing the theologically-based assumptions about the catastrophic destruction and remaking of life that had dominated earlier interpretations of the geological record.

Towards the end of the nineteenth century geologists developed some terms to distinguish between rock subdivisions and implied time; for example, the 1880 Bologna congress recommended the use of the term "age" for the rock equivalent of the time term "stage." The need for a "dual system" of nomenclature for time and for rocks was emphasized by Williams (1894).

S. S. Buckman, in a series of papers on the biostratigraphy of the English Jurassic strata, proposed a new concept and a term to encompass it. This was the *hemera*, defined by Buckman (1893, p. 481) as "the chronological indicator of the faunal sequence." Buckman intended the hemera to be "the smallest consecutive divisions which the sequence of different species enables us to separate in the maximum development of strata." This unit of time was intended to correspond to the *acme zone*, the rock unit representing the maximum occurrence of a particular zone species. If the record is complete, the span of time of a given hemera should be present in the rocks even beyond the facies changes that limit the extent of the original zone fossils. There has always been the potential for confusion between a reference to a time span and the rocks that were deposited during that time span. Buckman (1898, p. 442) noted that, for example, terms such as Bajocian and Jurassic had been used to refer to rocks of that age and also to a specific span of time.

Most of the work in the nineteenth and early twentieth centuries that addressed the issue of how geologic time is represented in the rocks approached the subject from the point of view of the fossil record. We have touched on some of the key developments in the preceding paragraphs. For example, Buckman (1893, p. 518) said

> Species may occur in the rocks, but such occurrence is no proof that they were contemporaneous …their joint occurrence in the same bed [may] only show that the deposit in which they are embedded accumulated very slowly.

And in a later paper:

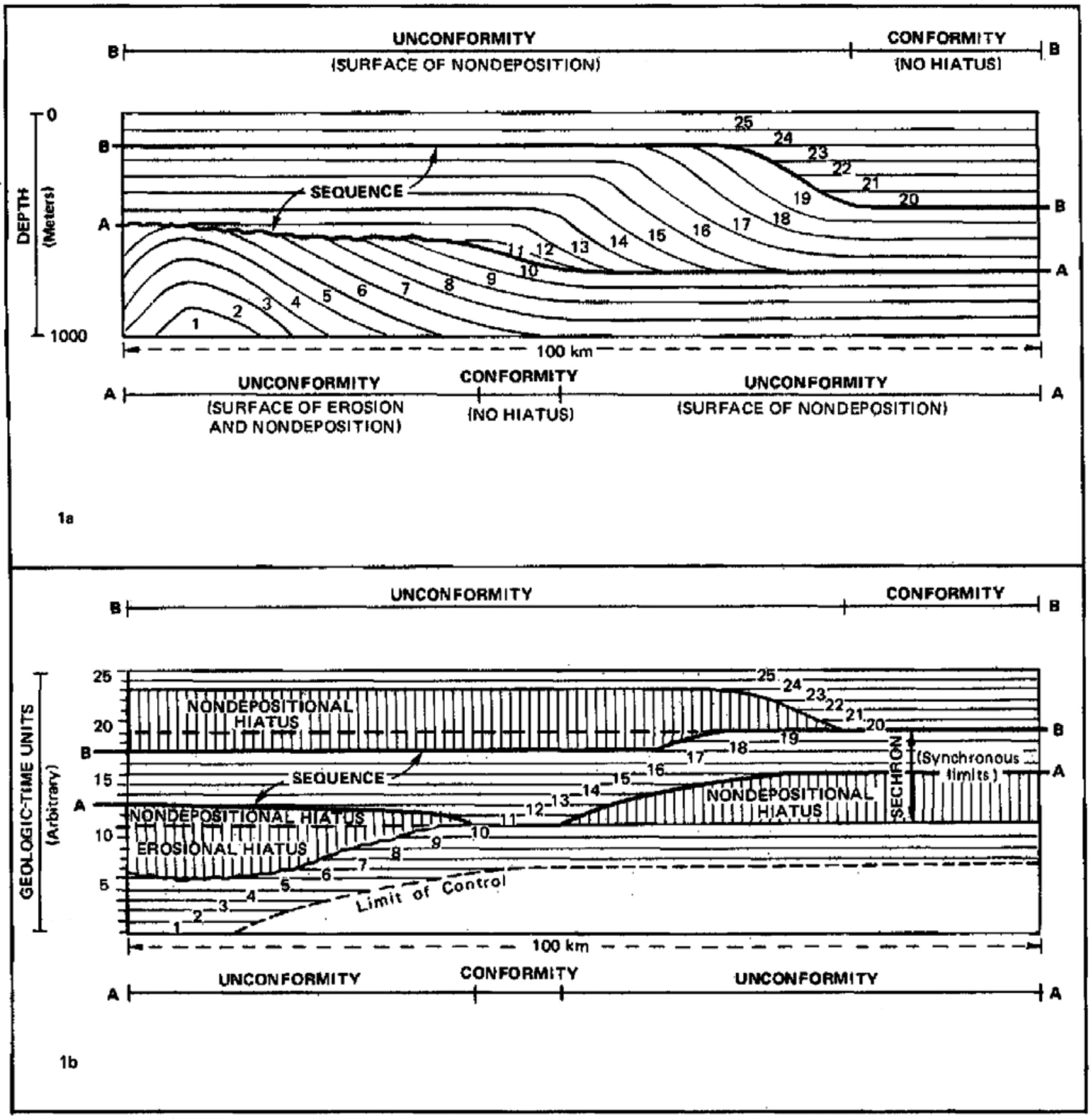
> The amount of deposit can be no indication of the amount of time … the deposits of one place correspond to the gaps of another (Buckma 1910, p. 90).

A very different approach was taken by Barrell (1917), in what became a classic paper, exploring the origins of stratigraphic units in terms of depositional processes. Aided by the new knowledge of the Earth's radiogenic heat engine and a growing understanding of sedimentary processes, Barrell worked through detailed arguments about the rates of sedimentation and the rates of tectonism and of climate change

Combining many of these ideas together, Barrell (1917, Fig. 5) constructed a diagram showing the "Sedimentary Record made by Harmonic Oscillation in Baselevel" (text-fig. 2). This is remarkably similar to diagrams that have appeared in some of the Exxon sequence model publications since the 1980s, and represents a thoroughly modern deductive model of the way in which "time" is stored in the rock record. Curve A-A simulates the record of long-term subsidence and the corresponding rise of the sea. Curve B-B simulates an oscillation of sea levels brought about by other causes—Barrell discussed diastrophic and climatic causes, including glacial causes, and applied these ideas to the rhythmic stratigraphic record of the "upper Paleozoic formation of the Appalachian geosyncline" in a discussion that would appear to have provided the foundation for the interpretations of "cyclothems" that appeared in the 1930s (see below). Barrell showed that when the long-term and short-term curves of sea-level change are combined, the oscillations of base level provide only limited time periods when sea-level is rising and sediments can accumulate. "Only one-sixth of time is recorded" by sediments (Barrell 1917, p. 797). This remarkable diagram anticipates 1) Jervey's (1988) ideas about sedimentary "accommodation" that became fundamental to models of sequence stratigraphy ("accommodation" is defined as the space made available for sediment to accumulate as a result of a rise of base level above the basin floor), and 2) Ager's (1973) point that the sedimentary record is "more gap than record." This important paper did not appear to influence thinking about the nature of the stratigraphic record as much as it should, as demonstrated by the fact that the rediscovery of the ideas by Jervey, Ager and others is largely attributed to the rediscoverers, not to Barrell (Wheeler 1958, in the first of an important series of papers to which we return below, comments favourably on Barrell's "frequently neglected base-level concept"). The point relevant to the discussion here is that Barrell demonstrated how fragmentary the stratigraphic record is, and how incomplete and unreliable it is as a record of the passage of the continuum of geologic time.

Other workers of this period who were cognizant of the significance of gaps in the stratigraphic record were Grabau (1913), who first defined the term *disconformity* as a major time break between units that nevertheless remained structurally parallel—conformable—to one another, and Blackwelder (1909) who wrote an essay on unconformities. Barrell (1917, p. 794) added the new term *diastem*, for minor sedimentary breaks.

A much-needed updating in stratigraphic concepts and terminology was undertaken by Schenk and Muller (1941). They "tried to clarify the distinction between the interpretive nature of 'time' and 'time-stratigraphic' units in contrast with the purely descriptive rock or stratigraphic term 'formation.'"



TEXT-FIGURE 3
Example of a "Wheeler plot" (bottom panel), showing its derivation from a conventional stratigraphic cross-section. Such plots clarify the time-relationships of rock units, particularly the variable duration of the bounding unconformities as a unit is traced from place to place. This example (top panel) shows a line drawing made from a reflection-seismic cross-section (from Vail et al., 1977), showing three unconformity-bounded sequences, and a series of time lines, numbered 1 to 25. In the chronostratigraphic diagram, below, the time-lines are the horizontal lines.

(Berry 1987, p. 7). They formalized the system of nomenclature that is in use today:

Time division (for abstract concept of time)	Time-stratigraphic division (for rock classification)
Era	-
Period	System
Epoch	Series
Age	Stage
Phase	Zone

Arkell (1946), the specialist in the Jurassic System, said: "A stage is an artificial concept transferable to all countries and continents, but a zone is an empirical unit" (cited in Hancock 1977, p. 18). By this statement he was essentially adopting the rock-time concepts of Buckman's hemera for units of the rank of the stage, suggesting that stages had some universal time significance. This statement represents a deductive interpretation of the meaning of the fossil record, and perpetuated the confusion between "rocks" and "time." Hancock (1977) blamed

Arkell for bringing into the modern era the controversy over the relative meaning of chronostratigraphic and biostratigraphic (zone) concepts. A biozone is an empirical unit based on the rock record, and can only be erected and used for correlation if the fossils on which it is based are present in the rocks. Stages are simply groupings of zones.

Confusion between the meaning of "rock" units and "time" units appeared to be widespread during the early part of the twentieth century. Teichert (1958) summarized the various approaches taken by French, German, British and North American stratigraphers and paleontologists up to that time. To him it was clear that there were three distinct concepts (summarized here from Teichert 1958, p. 117):

> *Biostratigraphic units*: the zone is the fundamental unit of biostratigraphy, consisting of a set of beds characterized by one or more fossil species.
>
> *Biochronologic units*: the unit of time during which sedimentation of a biostratigraphic unit took place. These are true relative time units.
>
> *Time-stratigraphic units*: units of rock which have been deposited during a defined unit of time. "Arrangement of rock-stratigraphic units within a time-stratigraphic unit and assignment to such a unit are generally made on a variety of lines of evidence both physical and paleontologic, including extrapolation, conclusion by analogy, and sometime merely for reasons of expedience."

The distinguished American stratigrapher Hollis Hedberg picked up on Arkell's idea about the meaning of the stage. He stated (Hedberg 1948, p. 456) "The time value of stratigraphical units based on fossils will fluctuate from place to place in much the same manner as the time value of a lithologic formation may vary." He proposed defining a separate set of chronostratigraphic units that corresponded to, and could be used to define, units of time. In 1952 he became the Chairman of a newly established International Commission on Stratigraphy, and one of his achievements was to establish this new set of units. These ideas became formalized and codified after many years work in a new international stratigraphic guide (Hedberg 1976). For example, he defined a chronozone as a zonal unit embracing all rocks anywhere formed during the range of a specified geological feature, such as a local biozone. In theory, a chronozone is present in the rocks beyond the point at which the fossil components of the biozone cease to be present as a result of lateral facies changes. Hedberg (1976) used a biostratigraphic example to illustrate the chronozone concept but, clearly, if the fossil components are not present, a chronozone cannot be recognized on biostratigraphic grounds, and its usefulness as a stratigraphic concept may be rather hypothetical (Johnson, J. G. 1992). However, other means may be available to extend the chronozone, including local marker beds or magnetostratigraphic data. We discuss this in the next section.

Hedberg (1976) suggested that the stage be regarded as the basic working unit of chronostratigraphy because of its practical use in interregional correlation. As Hancock (1977, p. 19) and Watson (1983) pointed out, Hedberg omitted to mention that all Phanerozoic stages were first defined on the basis of groups of biozones, and they are therefore, historically, biostratigraphic entities. In practice, therefore, so long as biostratigraphy formed the main basis of chronostratigraphy, no useful purpose was served by treating them as theoretically different subjects. Stages could be defined by more than one system of biozones, which extended their range and reduced their facies dependence, but, although this improved their chronostratigraphic usefulness, it did not change them into a different sort of unit. We return to this point in the next section (the modern definition of the term "stage" is quite different, as discussed below).

Harry E. Wheeler of the University of Washington pointed out the problems in Hedberg's concepts of time-stratigraphic units (Wheeler 1958, p. 1050) shortly after they appeared in the first American stratigraphic guide (ACSN 1952). He argued that a time-rock unit could not be both a "material rock unit," as described in the guide, and one whose boundaries could be extended from the type section as isochronous surfaces, because such isochronous surfaces would in many localities be represented by an unconformity. Wheeler developed the concept of the chronostratigraphic cross-section, in which the vertical dimension in a stratigraphic cross-section is drawn with a time scale instead of a thickness scale (text-fig. 3). In this way, time gaps (unconformities) become readily apparent, and the nature of time correlation may be accurately indicated. Such diagrams have come to be termed "Wheeler plots." Wheeler cited with approval the early work of Sloss and his colleagues, referred to in more detail below:

> As a tangible framework on which to hang pertinent faunal and lithic data, the *sequence* of Sloss, Krumbein and Dapples (1949, pp. 110-11) generally fulfills these requirements. Paraphrasing these authors' discussion, a *sequence comprises an assemblage of strata exhibiting similar responses to similar tectonic environments over wide areas, separated by objective horizons without specific time significance* (Wheeler, 1958, p. 1050; italics as in original).

Sequences came later to be called simply "unconformity-bounded units," whereas Wheeler's description of them is a logically inconsistent mixture of empirical description and tectonic interpretation. He proposed a new term for time-rock units, the *holostrome*, which consists of a sequence (in the Sloss et al. sense) together with the "erosional vacuity" representing the part of the sequence lost to erosion. Such a vacuity would not be obvious on a conventional stratigraphic cross-section, appearing simply as the line corresponding to an unconformity. However, an erosional vacuity might constitute a significant area of a Wheeler time plot and would require some knowledge of the lateral variations in the age of the units immediately above and below the unconformity in order for it to be drawn in accurately. Although Wheeler's concepts and plots are now commonplace in geology (they are cited in Vail's early work), the term *holostrome* has not become an accepted term.

The recognition of the importance of suites of stratigraphic units bounded by unconformities, based on the work of Levorsen, Sloss, Wheeler and others, led to suggestions for the formal recognition of such units in stratigraphic guides and codes. The proposal for a type of unit called a "*synthem*" by Chang (1975) represents the first formal proposal of this type. Chang (1975, p. 1546-1547) considered the importance of tectonic cycles in the generation of such deposits, quoting Grabau's (1940) concept of "pulsations", and he was aware of Vail's early work on eustatic cycles (citing presentations by Vail's group at a Geological Society of America meeting in 1974). However, he concluded that "In recognizing the unity or individuality of a synthem, the involvement of as little subjective judgment as possible is desirable." In other words, Chang was at pains to adhere to a descriptive, empirical concept in his definition. Synthems were included in the International Stratigraphic Guide (Hedberg 1976, p. 92) as an additional class of time-significant unit, although, because of the variable age of the bounding unconformities, they were not categorized as strictly chronostratigraphic in character. Unconformity-bounded units were accorded their own chapter in the revised version of the International Stratigraphic Guide (Salvador 1994, Chap. 6), and also formed the basis for a new type of stratigraphy, termed *allostratigraphy*, in the North American guide (NACSN 1983). But not everybody was content with these developments. Murphy (1988, p. 155) pointed out that "The statement that they [synthems] are objective and non-interpretive …. assumes that particular unconformities have tangible qualities by which they can be distinguished and identified; this assumption is false." He went on to argue that many unconformities can only be recognized on the basis of knowledge of the units above and below, and interpretation of the nature of the unconformity surface itself, and that therefore unconformity-bounded units are not a class of objective stratigraphic unit. This objection has been almost totally ignored in the rush to adopt sequence stratigraphic methods, although attempts to incorporate sequence concepts into formal stratigraphic guides and codes have yet to achieve agreement.

The development of modern chronostratigraphy

One of the central themes of the development of stratigraphy has been the work to establish an accurate geological times scale. Why? McLaren (1978) attempted to answer this question. Here are his nine reasons:

> Some of these geological problems and questions include: (1) rates of tectonic processes; (2) rates of sedimentation and accurate basin history; (3) correlation of geophysical and geological events; (4) correlation of tectonic and eustatic events; (5) are epeirogenic movements worldwide … (6) have there been simultaneous extinctions of unrelated animal and plant groups; (7) what happened at era boundaries; (8) have there been catastrophes in earth history which have left a simultaneous record over a wide region or worldwide; and (9) are there different kinds of boundaries in the geologic succession (That is, "natural" boundaries marked by a worldwide simultaneous event versus "quiet" boundaries, man-made by definition).

It is, in fact, fundamental to the understanding of the history of Earth that events be meticulously correlated in time. For example, current work to investigate the history of climate change on Earth during the last few tens to hundreds of thousands of years has demonstrated how important this is, because of the rapidity of climate change and because different geographical regions and climatic belts may have had histories of climate change that were not exactly in phase. If we are to understand the Earth's climate system thoroughly enough to determine what we might expect from present day human influences, such as the burning of fossil fuels, a detailed record of past climate change will be of fundamental importance. That we do not now have such a record is in part because of the difficulty in establishing a time scale precise enough and practical enough to be applicable in deposits formed everywhere on Earth in every possible environmental setting.

Until the early twentieth century the geologic time scale in use by geologists was a relative time scale dependent entirely on biostratigraphy. The standard systems had nearly all been named., based on European data, by about 1840 (Berry 1987; Callomon, 2001). Estimates about the duration of geologic events, including that of chronostratigraphic units, varied widely, because they depended on a variety of estimation methods, such as attempts to quantify rates of erosion and sedimentation (Hallam 1989). The discovery of the principle of radioactivity was fundamental, providing a universal clock for the direct dating of certain rock types, and the calibration of the results of other dating methods, especially the relative scale of biostratigraphy. Radiometric dating methods may be used directly on rocks containing the appropriate radioactive materials. For example, volcanic ash beds intercalated with a sedimentary succession provide an ideal basis for precise dating and correlation. Volcanic ash contains several minerals that include radioactive isotopes of elements such as potassium and rubidium. Modern methods can date such beds to an accuracy typically in the ±2% range, that is, ±2 m.y. at an age of 100 Ma (Harland et al. 1990), although locally, under ideal conditions, accuracy and precision are now considerably better than this ($\pm 10^4$-10^5 years). Where a sedimentary unit of interest (such as a unit with a biostratigraphically significant fauna or flora) is overlain and underlain by ash beds it is a simple matter to estimate the age of the sedimentary unit. The difference in age between the ash beds corresponds to the elapsed time represented by the pile of sediments between the ash beds. Assuming the sediments accumulated at a constant rate, the rate of sedimentation can be determined by dividing the thickness of the section between the ash beds by the elapsed time. The amount by which the sediment bed of interest is younger than the lowest ash bed is then equal to its stratigraphic height above the lowest ash bed divided by the rate of sedimentation, thereby yielding an "absolute" age, in years, for that bed. This procedure is typical of the methods used to provide the relative biostratigraphic age scale with a quantitative basis. The method is, of course, not that simple, because sedimentation rates tend not to be constant, and most stratigraphic successions contain numerous sedimentary breaks that result in underestimation of sedimentation rates. Numerous calibration exercises are required in order to stabilize the assigned ages of any particular biostratigraphic unit of importance.

Initially the use of radiometric dating methods was relatively haphazard, but gradually geologists developed the technique of systematically working to cross-calibrate the results of different dating methods, reconciling radiometric and relative biostratigraphic ages in different geological sections and using different fossils groups. In the 1960s the discovery of preserved ("remanent") magnetism in the rock record led to the development of an independent time scale based on the recognition of the repeated reversals in magnetic polarity over geologic time. Cross-calibration of radiometric and biostratigraphic data with the magnetostratigraphic record provided a further means of refinement and improvement of precision. The techniques are described in all standard textbooks of stratigraphy (e.g., Miall 1999; Nichols 1999).

These modern developments rendered irrelevant the debate about the value and meaning of Hedberg's (1976) hypothetical chronostratigraphic units. The new techniques of radiometric dating and magnetostratigraphy, where they are precise enough to challenge the supremacy of biostratigraphy, could have led to the case being made for a separate set of chronostratigraphic units, as Hedberg proposed. However, instead of a new set of chronostratigraphic units, this correlation research is being used to refine the definitions of the existing, biostratigraphically based stages. Different assemblages of zones generated from different types of organism may be used to define the stages in different ecological settings (e.g., marine versus nonmarine) and in different biogeographic provinces, and the entire data base is cross-correlated and refined with the use of radiometric, magnetostratigraphic and other types of data. The stage has now effectively evolved into a chronostratigraphic entity of the type visualized by Hedberg (1976). This is the essence of the procedure recommended by Charles Holland (1986, Fig. 10), one of the leading spokespersons of the time for British stratigraphic practitioners. For most of Mesozoic and Cenozoic time the standard stages, and in many cases, biozones, are now calibrated using many different data sets, and the global time scale, based on correlations among the three main dating methods, is attaining a high degree of accuracy. The Geological Society of London time scale (GSL 1964) is an important milestone, representing the first attempt to develop a comprehensive record of these calibration and cross-correlation exercises. Wheeler's (1958) formal methods of accounting for "time in stratigraphy" (the title of his first important paper), including the use of "Wheeler plots" for showing the time relationships of stratigraphic units, provided much needed clarity in the progress of this work. Time scales for the Cenozoic (Berggren 1972) and the Jurassic and Cretaceous (Van Hinte 1976a, b) are particularly noteworthy for their comprehensive data syntheses, although all have now been superseded. The most recent detailed summation and reconciliation of the global data base were provided by Harland et al. (1990) and Berggren et al. (1995).

In the 1960s, several different kinds of problems with stratigraphic methods and practice had begun to be generally recognized (e.g., Newell 1962). There are two main problems. Firstly, stratigraphic boundaries had traditionally been drawn at horizons of sudden change, such as the facies change between marine Silurian strata and the overlying nonmarine Devonian succession in Britain. Changes such as this are obvious in outcrop, and would seem to be logical places to define boundaries. Commonly such boundaries are unconformities. However, it had long been recognized that unconformities pass laterally into conformable contacts (for example, this was described by Whewell 1872). This raised the question of how to classify the rocks that formed during the interval represented by the unconformity. Should they be assigned to the overlying or underlying unit, or used to define a completely new unit? When it was determined that rocks being classified as Cambrian and Silurian overlapped in time, Lapworth (1879) defined a new chronostratigraphic unit—the Ordovician, as a compromise unit straddling the Cambrian-Silurian interval. The same solution could be used to define a new unit corresponding to the unconformable interval between the Silurian and the Devonian. In fact, rocks of this age began to be described in central Europe after WWII, and this was one reason why the Silurian-Devonian boundary became an issue requiring resolution. A new unit could be erected, but it seemed likely that with additional detailed work around the world many such chronostratigraphic problems would arise, and at some point it might be deemed desirable to stabilize the suite of chronostratigraphic units. For this reason, the development of some standardized procedure seemed to be desirable.

A second problem is that to draw a significant stratigraphic boundary at an unconformity or at some other significant stratigraphic change is to imply the hypothesis that the change or break has a significance relative to the stratigraphic classification, that is, that unconformities have precise temporal significance. This was specifically hypothesized by Chamberlin (1898, 1909, as noted above), who was one of many individuals who generated ideas about a supposed "pulse of the earth." In the case of lithostratigraphic units, which are descriptive, and are defined by the occurrence and mappability of a lithologically distinctive succession, a boundary of such a unit coinciding with an unconformity is of no consequence. However, in the case of an interpretive classification, in which a boundary is assigned time significance (such as a stage boundary), the use of an unconformity as the boundary is to make the assumption that the unconformity has time significance; that is, it is of the same age everywhere. This places primary importance on the model of unconformity formation, be this diastrophism, eustatic sea-level change or some other cause. From the methodological point of view this is most undesirable, because it negates the empirical or inductive nature of the classification. It is for this reason that it is inappropriate to use sequence boundaries as if they are chronostratigraphic markers.

How to avoid this problem? A time scale is concerned with the continuum of time. Given our ability to assign "absolute" ages to stratigraphic units, albeit not always with much accuracy and precision, one solution would be to assign numerical ages to all stratigraphic units and events. However, this would commonly be misleading or clumsy. In many instances stratigraphic units cannot be dated more precisely than, say, "late Cenomanian" based on a limited record of a few types of organisms (e.g., microfossils in subsurface well cuttings). Named units are not only traditional, but also highly convenient, just as it is convenient to categorize human history using such terms as the "Elizabethan" or the "Napoleonic" or the "Civil War" period. What is needed is a categorization of geological time that is empirical and all encompassing. The familiar terms for periods (e.g., Cretaceous) and for ages/stages (e.g., Aptian) offer such a subdivision and categorization, provided that they can be made precise enough and designed to encompass all of time's continuum. A group of British stratigraphers (e.g., Ager 1964) is credited with the idea that seems to have resolved the twin problems described here. McLaren (1970, p. 802) explained the solution in this way:

> There is another approach to boundaries, however, which maintains that they should be defined wherever possible in an area where "nothing happened." The International Subcommission on Stratigraphic Classification, of which Hollis Hedberg is Chairman, has recommended in its Circular No. 25 of July, 1969, that "Boundary-stratotypes should always be chosen within sequences of continuous sedimentation. The boundary of a chronostratigraphic unit should never be placed at an unconformity. Abrupt and drastic changes in lithology or fossil content should be looked at with suspicion as possibly indicating gaps in the sequence which would impair the value of the boundary as a chronostratigraphic marker and should be used only if there is adequate evidence of essential continuity of deposition. The marker for a boundary-stratotype may often best be placed *within* a certain bed to minimize the possibility that it may fall at a time gap." This marker is becoming known as "the Golden Spike."

By "nothing happens" is meant a stratigraphic successions that is apparently continuous. The choice of boundary is then purely arbitrary, and depends simply on our ability to select a horizon that can be the most efficiently and most completely documented and defined (just as there is nothing about time itself that distinguishes between, say, February and March, but to define a boundary between them is useful for purposes of communication and record). This is the epitome of an empirical approach to stratigraphy. Choosing to place a boundary where "nothing happened" is to deliberately avoid having to deal with some "event" that would require interpretation. This recommendation was accepted in the first International Stratigraphic

Guide (Hedberg 1976, p. 84-85), although Hedberg (1976, p. 84) also noted the desirability of selecting boundary stratotypes "at or near markers favorable for long-distance time-correlation", by which he meant prominent biomarkers, radiometrically-datable horizons, or magnetic reversal events. Boundary-stratotypes were to be established to define the base and top of each chronostratigraphic units, with a formal marker (a "golden spike") driven into a specific point in a specific outcrop to mark the designated stratigraphic horizon. Hedberg (1976, p. 85) recommended that such boundary-stratotypes be used to define both the top of one unit and the base of the next overlying unit. However, further consideration indicates an additional problem, which was noted in the North American Stratigraphic Code of 1983 (NACSN, p. 868):

> Designation of point boundaries for both base and top of chronostratigraphic units is not recommended, because subsequent information on relations between successive units may identify overlaps or gaps. One means of minimizing or eliminating problems of duplication or gaps in chronostratigraphic successions is to define formally as a point-boundary stratotype only the base of the unit. Thus, a chronostratigraphic unit with its base defined at one locality will have its top defined by the base of an overlying unit at the same, but more commonly, another locality.

Even beds selected for their apparently continuous nature may be discovered at a later date to hide a significant break in time. Detailed work on the British Jurassic section using what is probably the most refined biostratigraphic classification scheme available for any pre-Late Cenozoic section has demonstrated how common such breaks are (Callomon 1995; see Miall and Miall, this volume). The procedure recommended by NACSN (1983) is that, if it is discovered that a boundary stratotype does encompass a break in time, the rocks (and the time they represent) are assigned to the unit below the stratotype. In this way, a time scale can be constructed that can readily accommodate all of time's continuum, as our description and definition of it continues to be perfected by additional field work. This procedure means that, once designated, boundary stratotypes do not have to be revised or changed. This has come to be termed the concept of the "topless stage."

The modern definition of the term "stage" (e.g., in the online version of the International Stratigraphic Guide by Michael A. Murphy and Amos Salvador at Murphy and Salvador at www.stratigraphy.org) indicates how the concept of the stage has evolved since d'Orbigny. The Guide states that "The stage has been called the basic working unit of chronostratigraphy. … The stage includes all rocks formed during an age. A stage is normally the lowest ranking unit in the chronostratigraphic hierarchy that can be recognized on a global scale. … A stage is defined by its boundary stratotypes, sections that contain a designated point in a stratigraphic sequence of essentially continuous deposition, preferably marine, chosen for its correlation potential."

The first application of the new concepts for defining chronostratigraphic units was to the Silurian-Devonian boundary, the definition of which had begun to cause major stratigraphic problems as international correlation work became routine in post-WWII years. A boundary stratotype was selected at a location called Klonk, in what is now the Czech Republic, following extensive work by an international Silurian-Devonian Boundary Committee on the fossil assemblages in numerous well-exposed sections in Europe and elsewhere. The results are presented in summary form by McLaren (1973), and, more extensively, by Chlupác (1972) and McLaren (1977) (see also Miall 1999, p. 116-117). As reported by Bassett (1985) and Cowie (1986), the establishment of the new procedures led to a flood of new work to standardize and formalize the geological time scale, one boundary at a time. This is extremely labour-intensive work, requiring the collation of data of all types (biostratigraphic, radiometric and, where appropriate, chemostratigraphic and magnetostratigraphic) for well-exposed sections around the world, and working to reach international agreement amongst *ad-hoc* international working groups set up for the purpose. In many instances, once such detailed correlation work is undertaken it is discovered that definitions for particular boundaries being used in different parts of the world, or definitions established by different workers using different criteria, do not in fact define contemporaneous horizons (e.g., Hancock 1993). This may be because the original definitions were inadequate or incomplete, and have been subject to interpretation as practical correlation work has spread out across the globe. Resolution of such issues should simply require international agreement; the important point being that there is nothing significant about, say, the Aptian-Albian boundary, just that we should all be able to agree on where it is. Following McLaren's idea that boundaries be places where "nothing happens", the sole criterion for boundary definition is that such definitions be as practical as possible. The first "golden spike" location (the Silurian-Devonian boundary at Klonk: Chlupác 1972) was chosen because it represents an area where deep-water graptolite-bearing beds are interbedded with shallow-water brachiopod-trilobite beds, permitting detailed cross-correlation between the faunas, permitting the application of the boundary criteria to a wide array of different facies. In other cases the presence of radiometrically datable units or a well-defined magnetostratigraphic record may be helpful. In all cases, accessibility and stability of the location are considered desirable features of a boundary stratotype, because the intent is that it serves as a standard. Perfect correlation with such a standard can never be achieved, but careful selection of the appropriate stratotype is intended to facilitate future refinement in the form of additional data collection.

Despite the apparent inductive simplicity of this approach to the refinement of the time scale, further work has been slow, in part because of the inability of some working groups to arrive at agreement (Vai, 2001). In addition, two contrasting approaches to the definition of chronostratigraphic units and unit boundaries have now evolved, each emphasizing different characteristics of the rock record and the accumulated data that describes it. Castradori (2002) provided an excellent summary of what has become a lively controversy within the International Commission on Stratigraphy. The first approach, which Castradori described as the *historical and conceptual approach*, emphasizes the historical continuity of the erection and definition of units and their boundaries, the data base for which has continued to grow since the nineteenth century by a process of inductive accretion. Aubry et al. (1999, 2000) expanded upon and defended this approach. The alternative method, which Castradori terms the *hyper-pragmatic approach*, focuses on the search for and recognition of significant "events" as providing the most suitable basis for rock-time markers, from which correlation and unit definition can then proceed. The followers of this methodology (see response by J. Remane, 2000, to the discussion by Aubry et al., 2000) suggest that in some instances historical definitions of units and their boundaries should be modified or set aside in favour of globally recognizable event markers, such as a prominent biomarker, a magnetic reversal event, an isotopic excursion, or, eventually, events based on cyclostratigraphy. This approach explicitly sets aside McLaren's recommendation (cited above) that boundaries be defined in places where "nothing happened," although it is in accord with suggestions in the first stratigraphic guide that "natural breaks" in the stratigraphy could be used or boundaries defined "at or near markers favorable for long-distance time-correlation" (Hedberg 1976, p. 71, 84). The virtue of this method is that where appropriately applied it may make boundary definition easier to recognize. The potential disadvantage is that is places prime emphasis on a single criterion for definition. From the perspective of this paper, which has attempted to clarify methodological differences, it is important to note that the hyper-pragmatic approach relies on assumptions about the superior time-significance of the selected boundary event. The deductive flavour of hypothesis is therefore added to the methodology. In this sense the method is not strictly empirical (as has been demonstrated elsewhere, assumptions about global synchroneity of stratigraphic events may in some cases be misguided. See A. D. Miall and C. E. Miall, 2001; see also companion paper in this volume, which discusses cyclostratigraphy).

The hyper-pragmatic approach builds assumptions into what has otherwise been an inductive methodology free of all but the most basic of hypotheses about the time-significance of the rock record. The strength of the historical and conceptual approach is that it emphasizes multiple criteria, and makes use of long-established practices for reconciling different data bases, and for carrying correlations into areas where any given criterion may not be recognizable. For this reason, this writer is not in favour of the proposal by Zalasiewicz et al. (2004) to eliminate the distinction between time-rock units (chronostratigraphy) and the measurement of geologic time (geochronology). Their proposal hinges on the supposed supremacy of the global stratotype boundary points. History has repeatedly demonstrated the difficulties that have arisen from the reliance on single criteria for stratigraphic definitions, and the incompleteness of the rock record, which is why "time" and the "rocks" are so rarely synonymous in practice.

Ongoing work on boundary stratotypes is periodically recorded in the IUGS journal *Episodes*, and is summarized in web pages at www.stratigraphy.org. The reader is referred to Aubry et al. (2000, including the discussion by Remane which follows) and to Castradori (2002) for additional details about this controversy. The latter article provides several case studies of how each approach has worked in practice.

For our purposes the importance of the history of stratigraphy set out here is that the work of building and refining the geological time scale has been largely an empirical, inductive process (with the exception of the hyper-pragmatic approach discussed above). Note that each step in the development of chronostratigraphic techniques, including the multidisciplinary cross-correlation methodology, the golden spike concept, and the concept of the topless unit, are designed to enhance the empirical nature of the process. Techniques of data collection, calibration and cross-comparison evolved gradually and, with that development came many series of decisions about the nature of the time scale and how it should be measured, documented, and codified. These decisions typically were taken at international geological congresses by large multinational committees established for such purposes. For example, the International Stratigraphic Guide, first published by Hedberg (1976), was an official product of the International Subcommission on Stratigraphic Classification of the International Union of Geological Sciences' Commission on Stratigraphy. For our purposes, the incremental nature of this method of work is significant because it is completely different from the basing of stratigraphic history on the broad, sweeping models of pulsation or cyclicity that have so frequently arisen during the evolution of the science of Geology, a topic to which we now turn.

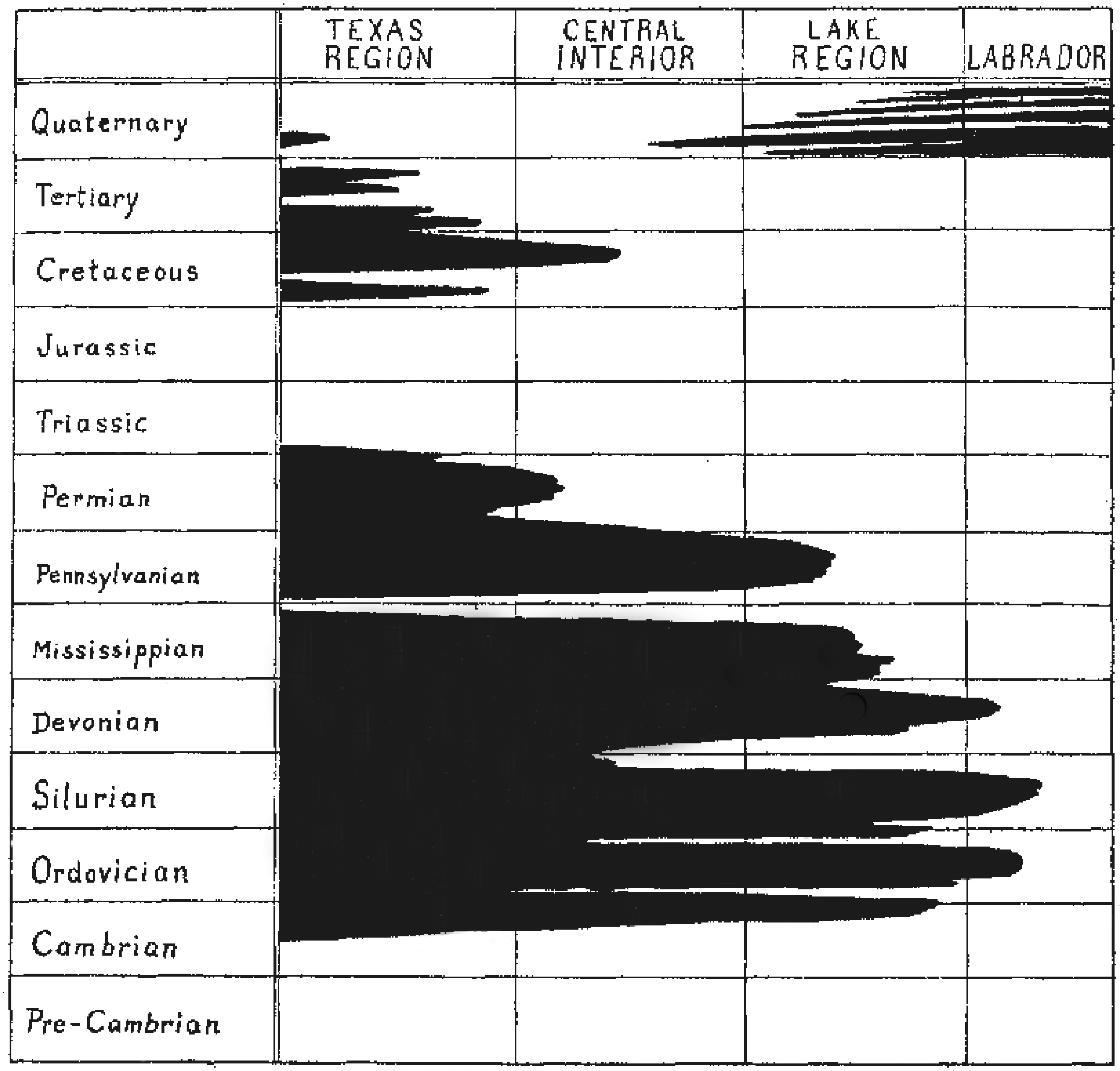
The continual search for a "pulse of the earth"

The self-appointed task of geologists is to explain the Earth. Given that the Earth is a complex object affected by a multiplicity of processes, there is a natural drive to attempt to systematize and simplify these processes in our hypotheses of how the Earth works. Numerical modeling, which has become popular in many fields with the advent of small but powerful and cheap computers, is but the most recent manifestation of this tendency, and is now widely used by earth scientists. The purpose of this section is to show how the idea of a worldwide stratigraphic pattern, as exemplified by the Exxon sequence model of the 1970s, is but the latest manifestation of a theme that runs through the entire course of modern Geology.

Two themes that recur throughout the evolution of geological thought are *pattern recognition* and *cyclicity*. Zeller (1964) demonstrated the ability of geologists to recognize patterns in data where none exist. In a famous psychological experiment he constructed simulated stratigraphic sections from lists of random numbers (in fact, digits from lists of phone numbers in a city phone directory), using the numbers to determine rock types and bed thicknesses. Professional geologists were then asked to "correlate" the sections, that is, to identify "beds" that extended from one "section" to the next. All were able to do so and, moreover, were able to develop comparisons with actual patterns of repetitive vertical order of rock types (sedimentary cyclicity) that had been well documented in the local outcrop geology and were well known to the professional geological community. Zeller explained these results thus:

> Psychologists, anthropologists, and philosophers of science have long recognized the fact that there is a fundamental need in man to explain the nature of his surroundings and to attempt to make order out of randomness …. The Western mind does not willingly accept the concept of a truly random universe even though there may be much evidence to support this view. …. Science, to an extent matched by no other human endeavor, places a premium upon the ability of the individual to make order out of what appears disordered (Zeller 1964, p. 631).

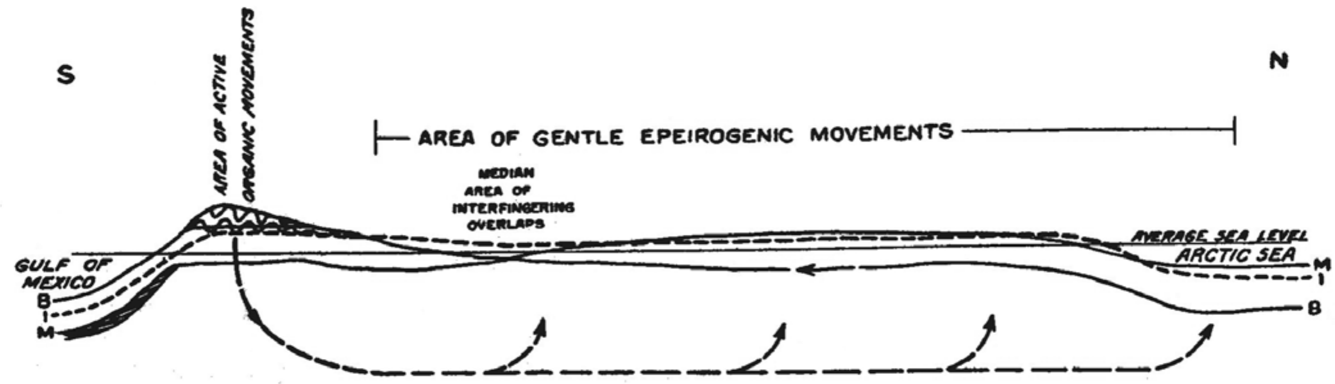
Dott (1992a) compiled a series of studies of a particular recurring obsession of geologists, that of the idea of repeated changes in global sea-level. The idea that the formation and subsequent melting of continental ice caps would affect sea levels by first locking up on land, and then releasing back to the oceans, large volumes of water, is attributed to a newspaper publisher, Charles Maclaren (1842), and appeared in his review of Louis Agassiz' glacial theory. The Scotsman James Croll was the first to develop these ideas into a hypothesis of orbital forcing in 1864, but the idea received no serious attention until the 1920s. The word "eustatic," as applied to sea levels, and meaning sea-level changes of global scope, was proposed by Suess (1888) (these historical developments are summarized by Dott, in his introduction to the volume). But, as Dott's book demonstrates, ideas about the repetitiveness or periodicity of earth history have existed since at least the eighteenth century.



TEXT-FIGURE 4
The broad stratigraphic patterns of the North American continent, as they were known in the early twentieth century. The "principal periods and areas of sedimentation" are shown in black, and the "time and extent of erosional conditions and corresponding unconformities" are shown in white. From Blackwelder (1909). This diagram was drawn to illustrate the concept of the widespread unconformity, and the data it illustrated formed the basis for the concepts of rhythmic diastrophism of Ulrich (1911), the regional petroleum evaluation studies of Levorsen (1943), and the sequences of Sloss (1963).

Why should periodicity be such a powerful opiate for geologists? Obviously, periodicity comes naturally through the universal human experience of diurnal, tidal, and seasonal cycles. And it has ancient roots in the Aristotelian Greek world view of everything in nature being cyclic. The answer must lie more directly, however, in the innate psychological appeal of order and simplicity, both of which are provided by rhythmically repetitive patterns. For geologists the instinctive appeal to periodicity constitutes a subtle extension of the uniformity principle, which is in turn a special geological case of simplicity or parsimony (Dott 1992b, p. 13).

To Charles Lyell, the founder of modern Geology, uniformitarianism included the concept that the Earth had not fundamentally changed throughout its history, and would not do so in the future. Earth history was not only directionless, but might also be cyclic (Rudwick 1998; Hallam 1998b). Lyell did not accept Darwin's ideas about organic evolution until late in his career, but held the opinion that most life forms had always been present on Earth, and if any were absent from the fossil record it was because of local environmental reasons or because the record had been destroyed by post-depositional processes, such as metamorphism. Lyell believed that in the future:



TEXT-FIGURE 5
Ulrich's (1911) model of diastrophic periodicity. The model called for tilting of the crust and isostatic adjustment following thrusting.

> Then might those genera of animals return, of which the memorials are preserved in the ancient rocks of our continents. The huge iguanodon might reappear in the woods, and the ichthyosaur in the sea, while the pterodactyl might flit again through the umbrageous groves of tree ferns (Lyell 1830; cited in Hallam 1998b, p. 134).

Lyell's ideas about the circularity of earth history were quickly discredited and discarded. However, his combination of inductive and deductive science and the attempt at building a grand, all-encompassing model that ultimately failed is uncannily similar to the modern story of sequence stratigraphy that we set out in our earlier work (A. D. Miall and C. E. Miall 2001).

Through the latter part of the nineteenth century and, in fact, until the modern era of plate tectonics, most theories of Earth processes included some element of repetition or cyclicity. These theories were developed in the absence of knowledge of the earth's interior, an absence that was not to be fully corrected until the development of the techniques of seismic tomography in the 1970s (Anderson 1989), which revealed for the first time how the Earth's mantle really works. The impetus for the development of theories of cyclicity presumably arose from the tendency to seek natural order, as described by Zeller, Dott, and others. The more well-known of such theories were proposed by some of the more prominent geologists of their times, and typically seemed to represent attempts to reconcile and explain their knowledge of Earth's complex history accumulated over a lifetime's work.

Among the more important such theories was the model of worldwide diastrophism proposed by Chamberlin (1898, 1909; useful summaries and interpretations of Chamberlin's ideas are given by Conklin and Conklin 1984, and Dott 1992c) and elaborated by Ulrich (1911). In some fundamental ways this model contains the basis of modern concepts in sequence stratigraphy, although the papers are not cited by the main founder and "grandfather" of modern sequence stratigraphy, L. L. Sloss, in his first major paper (Sloss 1963), or in his later work.

Chamberlin opened his paper with this remark:

> It was intimated in the introduction to the symposium on the classification and nomenclature of geologic time divisions published in the last number of this magazine [Journal of Geology] that the ulterior basis of classification and nomenclature must be dependent on the existence or absence of natural divisions resulting from simultaneous phases of action of world-wide extent (1898, p. 449).

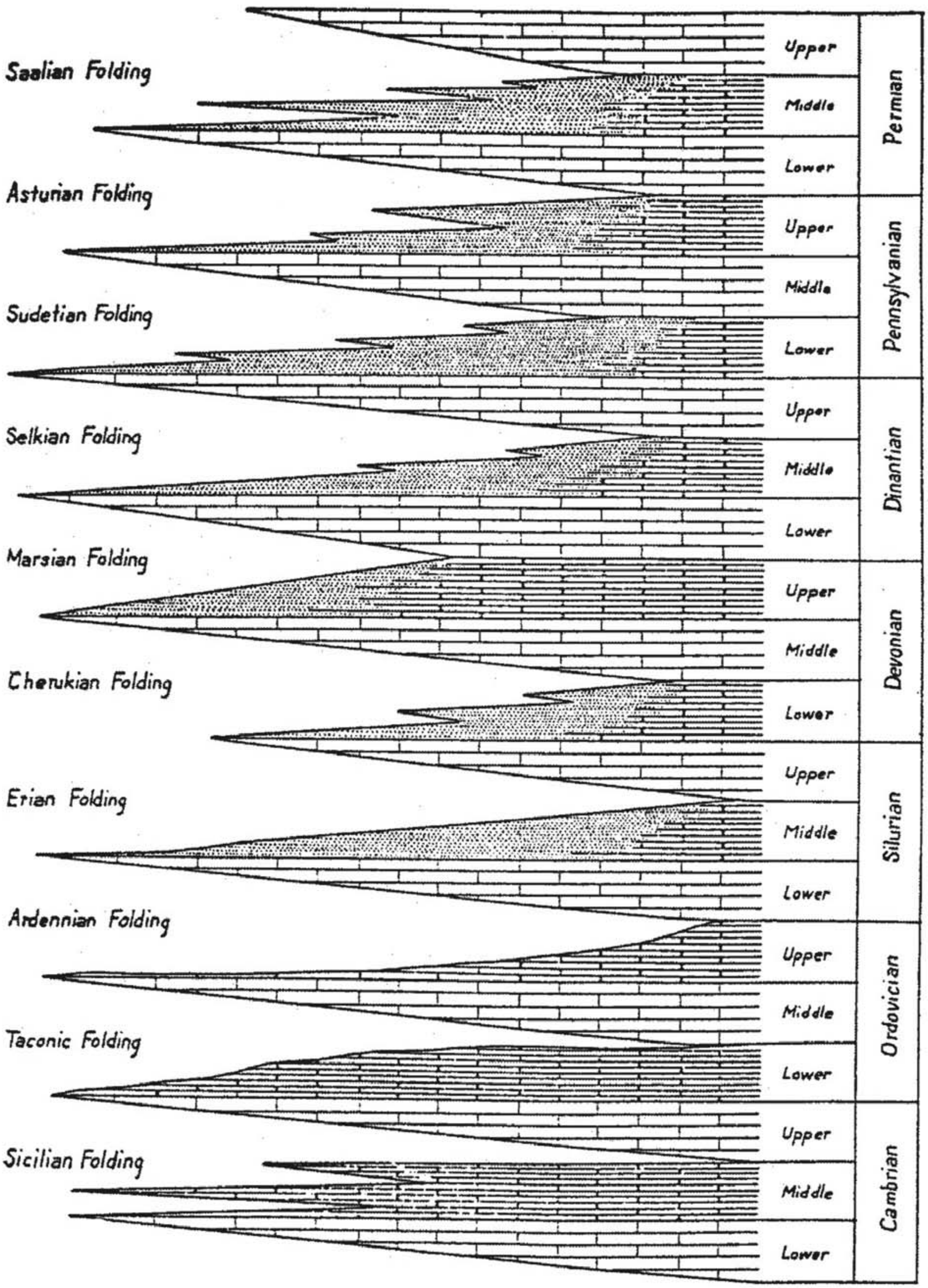
Chamberlin made note of the widespread transgressions and regressions that could be interpreted from the stratigraphic record, and he understood the importance of regional uplift and erosion as the cause of widespread unconformities, which he termed "base-leveling." He suggested that "correlation by base-levels is one of the triumphs of American geology." (Chamberlin 1909, p. 690) and emphasized that "*the base-leveling process implies a homologous series of deposits the world over*." (emphasis by italics as in the original).

The concept of widespread unconformities, which was later to form the basis for the sequence stratigraphy of Sloss and Vail (see below) appears to have been an inevitable, inductive product of the mapping and data collection that was gradually being carried out at this time to document the North American continent. Blackwelder (1909), in an essay on unconformities, published a diagram (text-fig. 4) that contains, in embryo, the sequences eventually documented and named in detail by Sloss (1963). Knowledge of these broad stratigraphic relationships seems to have formed the basis for much subsequent theorizing, although few references to this specific paper can be found in later work. Barrell (1917) referred to a different study by Blackwelder. Wheeler refers to it in his 1958 paper.

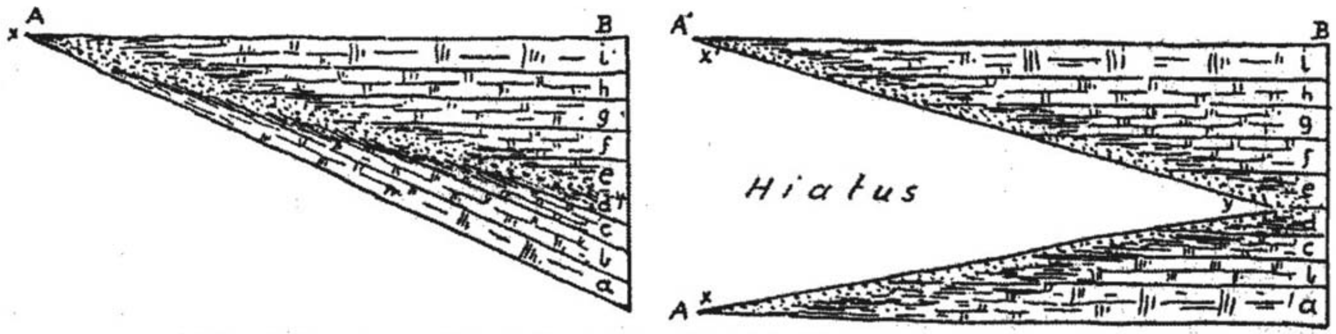
Chamberlin suggested that the base-levelings were caused by "diastrophism," that is, regional uplift and subsidence of the Earth's crust. He suggested that the movements were periodic.

> Reasons are growing yearly in cogency why we should regard the earth as essentially a solid spheroid and not a liquid globe with a thin sensitive crust. I think we must soon come to see that the great deformations are deep-seated body adjustments, actuated by energies, and involving masses, compared to which the elements of denudation and deposition are essentially trivial. Denudation and deposition seem to me clearly incompetent to perpetuate their own cycles. It seems clear that diastrophism is fundamental to deposition, and is a condition prerequisite to epicontinental and circum-continental stratigraphy (Chamberlin 1909, p. 693).

According to Chamberlin the worldwide episodes of diastrophism would have four important outcomes: 1) diastrophic uplift and subsidence of the Earth' surface would cause the development of worldwide unconformities; 2) such episodes of uplift and subsidence would affect global sea levels



TEXT-FIGURE 6
Grabau's curve of eustatic sea-level events, illustrating his "Pulsation Theory" (Grabau 1936).



TEXT-FIGURE 7
Grabau's model of offlap-onlap relationships, shown on the right in the form of a chronostratigraphic diagram (Grabau 1906).

(Chamberlin did not have a term for this. The word "eustasy" emerged later, from the work of Suess, as noted above); 3) the rise and fall of the ocean, in alternately expanding and contracting the area and depth of the seas, would affect the living space and ecology of life forms, and would therefore be a major cause of organic evolution, which would explain the worldwide synchroneity of successive faunas; and 4) uplift and subsidence would also affect the area of the Earth undergoing erosion, which would, in turn, control the level of carbon dioxide in the atmosphere. Chamberlin was one of the first to realize the importance of CO_2 as a greenhouse gas (he did not use this term, either), and attributed Earth's changes in climate through the geologic past to this process. As Dott (1992c, p. 40) noted, with this theory Chamberlin provided much of the foundation of modern sequence stratigraphy and of modern ideas about climate change. He illustrated the formation of continental-margin sediment wedges by progradation, the sediment being derived by uplift and "base-leveling." These process, because of their effect on the stratigraphic record, provided the "ultimate basis of correlation," for Chamberlin.

In his paper, Ulrich (1911) developed Chamberlin's ideas further. He complained (p. 289) about the "Paleontological autocrat," a symbolic representation of the authority of biostratigraphic correlation which was, by its massing of detail, making it difficult to perpetuate the broad, sweeping generalizations about stratigraphic correlation that he preferred. He was also dubious about the supposed diachroneity of rock units, regarding such a process as insignificant relative to the regional correlatability of geological formations (Ulrich 1911, p. 295). Here is an excellent example of the model-building paradigm at work—in assessing the stratigraphic record Ulrich placed higher value on his interpreted generalizations than on the actual empirical evidence from the rocks. Ulrich opposed the idea of "dual nomenclatures" for rocks and for time, preferring to see his natural stratigraphic subdivisions as a sufficient basis for stratigraphic classification. The following quotations from this paper provide a remarkable foretelling of many of the principles of sequence stratigraphy:

> In my opinion a rhythmic relationship connects nearly all diastrophic movements. For a few the meter is very long, for others shorter, and for still others much shorter. The last may be arranged into cycles and these again into grand cycles, the whole arrangement probably corresponding in units to the divisions of an ideal classification of stratified rocks and, so far as these go, of geologic time. …. As I shall endeavor to show …. Diastrophism affords a true basis for intercontinental correlation of not only the grander cycles by also of their subordinate stages. …. The principle of rhythmic periodicity being recognized, it seems to me merely a matter of time and close comparative study of sedimentary rocks and faunal associations to determine the time relations of interruptions in sedimentation in any one section to similar interruptions in another (Ulrich 1911, p. 399).

> Displacement of strandline chiefly relied on in proving periodicity of deformative movements.—The only thing that moves … and which, therefore, offers the most reliable criteria in determining the periodicity and contemporaneity of diastrophic events, is the level of the sea. …. Whatever the qualifications, there yet remains the fact that the strandline is contemporaneously and universally displaced (Ulrich 1911, p. 401-402).

> Accuracy in correlation, whether narrow or intercontinental in scope, depends solely on the uniform application of the criteria and principles adopted, and that if our practice is thoroughly consistent we shall finally succeed in discovering physical boundaries what will separate the systems so that none will include beds of ages elsewhere referred to either the preceding or succeeding period (Ulrich 1911, p. 403).

In these three paragraphs we see in embryo the concepts of a cycle hierarchy, the idea of sedimentary accommodation, and the idea of the preeminent importance of the sequence boundary as a time marker. His model of diastrophic periodicity is illustrated in text-figure 5.

Ulrich is referring here to the idea of "natural" subdivisions of geologic time into what we would now call sequences, as a practice to be preferred to the use of the European-based stage and series nomenclature. In the North American successions with which Ulrich was familiar, most of the boundaries between the series and stages occurred within conformable stratigraphic successions, and this was reason for him to question their validity and usefulness. In his paper Ulrich provided diagrams that illustrate sedimentary overlap, and discussed the implications of these structural arrangements for documenting marine regression and transgression.

The Chamberlin-Ulrich model was very influential on later generations of geologists. It undoubtedly influenced Joseph Barrell of Yale University, whose classic 1917 paper begins in this way:

> Nature vibrates with rhythms, climatic and diastrophic, those finding expression ranging in period from the rapid oscillation of surface waters, recorded in ripple mark, to those long-defended stirrings of the deep titans which have divided earth history into periods and eras.

Barrell (1917, p. 750) was aware of climatic cycles and discussed what we would now call orbital-forcing mechanisms (e.g., the "precession cycle of 21,000 years"). He discussed the major North American orogenic episodes and their influence on the broad patterns of stratigraphy, referring (p. 775) to the rise and ebb of sea level, which "pulsates with the close of eras, falling and then slowly rising again," as "the most far-reaching rhythm of geologic time." However, the main focus of this important paper is on attempts to establish the rates of geological processes and the measurement of the length of geologic time, given the new impetus to the study of this problem provided by the discovery of radioactivity. He refers to "diastrophic oscillation," but only from the understanding such a process may provide for the interpretation of the stratigraphic record, not as a fundamental mechanism to be used as a basis for the definition of geologic time.

Having compiled a great deal of information about the nature and rates of Earth processes, and having assessed the ages of the major eras in earth history, including that of the major diastrophic episodes, Barrell (1917, p. 888) suggested that "There appears to run through geologic time a recurrence of greater crescendos which in their average period approach in round numbers to 200,000,000 years." But then, after some discussion of this periodicity, he warned that "There is a human tendency, however, to seek for over-much regularity in nature and it is doubtful if much weight should be attached to this cycle of approximately 200,000,000 years. Although extremely suggestive of a new perspective, there are not enough terms, nor are they sharply enough defined, above those of lesser magnitude to give this indication more than such suggestive value" (Barrell 1917, p. 889-890). On the basis of this discussion Barrell does not appear to have been one of those who regarded some "pulse of the earth" as central to geologic history.

A succession of late Paleozoic deposits that is widely exposed in the continental interior of the United States has had an exceptionally important influence on the development of ideas about cyclicity in the geological record. Johan August Udden is credited with being the first to recognize (in 1912) that a coal-bearing succession of Pennsylvanian age in Illinois contained a repetition of the same succession of rock types, which he attributed to repeated inundations of sea during basinal subsidence (Langenheim and Nelson 1992; Buchanan and Maples 1992). In 1926, the Illinois Geological Survey began a stratigraphic mapping study of these deposits, under the direction of J. Marvin Weller. "As this study proceeded he [Weller] was impressed by the remarkable similarity of the stratigraphic succession associated with every coal bed. …. Their studies showed that the Pennsylvanian system in the Eastern Interior basin consists of repeated series of beds or cyclothems, each of which is composed of a similar series of members" (Wanless and Weller 1932; they did not formally acknowledge Udden in this work). This paper contained the definition of the term *cyclothem*, for a particular type of cyclic or repeated pattern of sedimentation. Cyclothems are typically no more than a few tens of metres in thickness and, we now know, each represents a few tens to hundreds of thousands of years of geologic time. They are particularly characteristic of Upper Paleozoic successions, for reasons that Shepard and Wanless (1935) were to suggest. Mapping by Weller and his colleagues was the first to demonstrate that these cyclothems are present over much of the continental interior of the United States. Weller suggested diastrophism as the cause of the cycles (Weller 1930), but a different mechanism was proposed a few years later. "It happens that there is abundant evidence of the existence of huge glaciers in the southern hemisphere during the very times when these curious alternations of deposits were being formed. A relation between these continental glaciers and the sedimentary cycles has been proposed recently by the writers" (Shepard and Wanless 1935). The authors proceeded to provide a sedimentological interpretation of how climatic and eustatic oscillations associated with the formation and melting of continental ice caps could have generated the succession of deposits that characterize the cyclothems. They relegated diastrophic causes to a secondary role in cyclothem generation, suggesting that tectonic movements would have been too slow. Thus was borne a very important hypothesis about the relationship between cycles of glaciation, sea-level change and sedimentation, although nobody seems to have made the connection between the Wanless-Shepard cyclothem hypothesis and the MacLaren-Croll orbital forcing concept until relatively recently (Crowell 1978). The Wanless and Weller paper also clearly established the cyclothem as a stratigraphic concept, in the sense that the cyclothem constitutes a distinct type of mappable unit, distinct from the formation, which they described merely as "a group of beds having some [lithologic] character in common" (Wanless and Weller 1932, p. 1003).

Through the first half of the twentieth century theories of Earth processes tended to include ideas about periodicity or rhythmicity. In part these ideas were fueled by the new knowledge of the driving force of radiogenic heat in the Earth's interior (e.g., Joly 1930). As noted by Dott (1992b, p. 12) "by the 1940s the enthusiasm for global rhythms was overwhelming." This can be seen in the title of some of the major books of this period: Grabau's (1940) *The rhythm of the ages*, which contained his "pulsation theory" (Johnson, M. E. 1992), and *The pulse of the Earth* (Umbgrove 1947) and *Symphony of the Earth* (Umbgrove 1950). Other "pulsation" theories of the period are noted by Hallam (1992a).

Of particular importance to our theme is the work of Grabau (1940), who developed a comprehensive theory of eustatic sea-level change based on the ideas of cyclic crustal expansion of the ocean basins (Johnson, M. E. 1992). Grabau compiled a eustatic sea-level curve for the Paleozoic, based on his own wide-ranging stratigraphic compilation, which showed episodes of continental transgression interspersed with episodes of tectonic uplift and regression (text-fig. 6). Grabau based his documentation of sea-level events on offlap-onlap relationships, just as did Vail some thirty years later (text-fig. 7). Although Grabau was noted for his massive data compilation, he did very little field work of his own after 1920, shortly after he moved from the United States to China (Johnson, M. E. 1992). As to his methodology, M. E. Johnson (1992, p. 50) quoted Grabau as follows:

> It is not a question of coining a plausible theory of world evolution and then attempting to apply it superficially to the history of all continents. The theory is rather a summation of the critical study of stratigraphic and paleontological facts from all parts of the works assembled by me during a period of more than 30 years. (Grabau 1936a, p. 48).

In this statement Grabau was in effect claiming to be carrying out inductive science—the building of a hypothesis from dispassionately collected data. He was answering a criticism by Hans Becker (cited in Grabau 1936a) in which "Becker doubted the wisdom of applying an untested theory to the whole world." Becker argued that the proper approach would be to "begin such an attempt in one continent and check the results with the facts gathered in other parts of the earth." Becker clearly suspected that Grabau was being model-driven, and he argued for the classic empirical observation and replication approach. M. E. Johnson's (1992, p. 50) conclusion about all this is that Grabau's ideas were "not a theory in search of data, but rather a set of data somewhat reluctantly entrusted to a theory of murky crustal mechanisms." Johnson argues that Grabau came late in his career to his model of eustasy and that it therefore represents an empirical construction.

Another influential model was that of Hans Stille (1924) who postulated an alternation of epeirogenic and orogenic episodes affecting all the continents. He named some thirty orogenic episodes which were believed to have global significance (his work is summarized by Hallam 1992a). As recently as the late 1970s, Fischer and Arthur (1977) plotted graphs of organic diversity through the Mesozoic-Cenozoic, which they compared with Grabau's eustatic cycles, and believed demonstrated a 32-million-year cyclicity

By the late 1920s the ideas of Chamberlin and Ulrich about the periodicity of earth processes had become very popular, but were strongly opposed by some skeptics. For example, Dott (1992c, p. 40) offered the following quote from this period:

> So much nonsense has been written on various so-called ultimate criteria for correlation that many have the faith or the wish to believe that the interior soul of our earth governs its surface history with a periodicity like the clock of doom, and that when the fated hour strikes strata are folded and raised into mountains, epicontinental seas retreat, and the continents slide about, the denizens of the land and sea become dead and buried, and a new era is inaugurated. This picture has an epic quality which is very alluring and it makes historical geology *so very understandable*, but is it a true picture? (Berry 1929, p. 2; italics as in original).

The work of Chamberlin, Ulrich, and Grabau, and the development of the cyclothem concept were essentially academic and theoretical, and did not appear to directly affect the practice of stratigraphy, particularly as it was carried out by petroleum geologists. The distinguished petroleum geologist A. I. Levorsen was one of the first to describe in detail some examples of the "natural groupings of strata on the North American craton:"

> A second principle of geology which has a wide application to petroleum geology is the concept of successive layers of geology in the earth, each separated by an unconformity. They are present in most of the sedimentary regions of the United States and will probably be found to prevail the world over (Levorsen 1943, p. 907).

This principle appears to have been arrived at on the basis of practical experience in the field rather than on the basis of theoretical model building. These unconformity-bounded successions, which are now commonly called "Sloss sequences," for reasons which we mention below, are tens to hundreds of metres thick and, we now know, represent tens to hundreds of millions of years of geologic time. They are therefore of a larger order of magnitude than the cyclothems. Levorsen did not directly credit Grabau, Ulrich, or any of the other theorists, cited above, who were at work during this period, nor did he cite the description of unconformity-bounded "rock systems" by Blackwelder (1909). Knowledge of these seems to have been simply taken for granted.

Larry Sloss of Northwestern University is commonly regarded as the "grandfather" of sequence stratigraphy, for two reasons. Firstly, his classic 1963 paper provided the foundation for the modern science, with its detailed documentation of the six fundamental sequences into which the North American cratonic Phanerozoic record could be subdivided. Secondly, Sloss was the doctoral supervisor of Peter Vail, who showed how sequences could be recognized from modern seismic-reflection data and thereby provided a critical practical tool for petroleum geologists (Vail et al. 1977). Sloss (1963, p. 111) cited Levorsen's 1943 paper, and referred to the ideas as Levorsen's "layer cake" geology. Sloss (1963) suggested that the sequence concept "was already old when it was enunciated by the writer and his colleagues in 1948" and that "many other workers of wide experience have informally applied the sequence concept since at least the 1920s," although he did not cite any of the earlier work of Chamberlin, Ulrich or Barrell. He would undoubtedly have been aware of (but did not cite) Blackwelder's (1909) essay on unconformities, which includes a diagram of "the principle periods and areas of sedimentation" within North America, a diagram which contains Sloss's sequences in embryonic form (text-fig. 4). Sloss may also have been thinking of cyclothems as representing a type of sequence, although these units are of a smaller order of thickness than Sloss's six major sequences, and are not mentioned in his paper. Van Siclen (1958) had already developed a sedimentological model for cycles such as the cyclothems (building on the work of Rich 1951), which Sloss also did not cite, but which was to be re-invented by sequence stratigraphers Henry Posamentier and John Van Wagoner as part of their adaptation of seismic stratigraphy for use on outcrop and drill data (Van Wagoner et al. 1990)

Vail's work is the latest manifestation of the "cyclicity" theme in the evolution of geologic thought, and is dependent on the acceptance of the reality of "patterns" in the rock record. In contrast to the work on descriptive stratigraphy and chronostratigraphy described in the previous sections of this chapter, Vail's science is clearly deductive in nature, and constitutes a distinct paradigm that has, since the 1970s, coexisted with the paradigm of empirical stratigraphy.

DISCUSSION

The contrasting approaches to stratigraphy described in this paper are encapsulated by the simple diagram shown here (text-fig. 8). Text-figure 8A illustrates the empirical, inductive approach to stratigraphy, in which each data point is assessed on its merits, and no assumptions about correlation are made. This approach places higher value on multivariate and quantifiable methods of correlation, and is driven by the supposition that correlations need to be rigorously tested because of the complexity of processes that generate the stratigraphic record (e.g., *complexity paradigm* of A. D. Miall and C. E. Miall 2001). By contrast, text-figure 8B illustrates the deductive approach: observation is based on an a-priori model which holds that a particular event or outcrop has special significance relative to some global model. Given one well-exposed or otherwise well-documented event, it is presumed that all other similar and stratigraphically nearby events are additional data points lying on that event. Correlation with the first event may then be cited as evidence of the success of the theory (e.g., the *global-eustasy paradigm* of A. D. Miall and C. E. Miall, 2001). Stratigraphers have generated problems when they have added an interpretive overlay to the type of empirical classification and correlation methodology shown in Fig. 8A. Examples include the inappropriate designation of biostratigraphically-based subdivisions, such as stages, as chronostratigraphic units by Arkell and Hedberg, and the assumption of the global correlatability of sequences by Vail and his colleagues. It could be argued that the "hyper-pragmatic" approach to the definition of chronostratigraphic unit boundaries also exemplifies the approach shown in text-figure 8B.

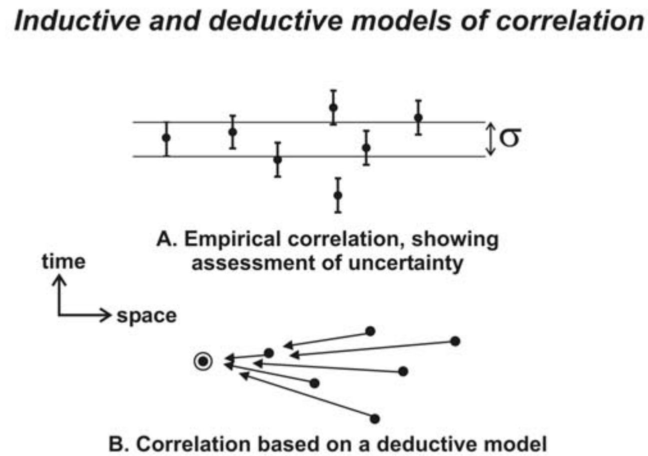
Despite the comment in the preceding section about the lack of direct citations, it would seem likely that the sequence concepts of Levorsen, Sloss and, ultimately, Vail, owe much to the American work by Chamberlin, Blackwelder, Ulrich, Grabau and others that found the European system of stages difficult to apply to American geology because the "natural breaks" (the unconformities) occurred in all the wrong places (not at the stage boundaries). Sequences as a basis for the subdivision of geologic time are therefore primarily an American concept, as were most of the models of rhythmicity or pulsation of Earth processes (Chamberlin, Grabau). Reviewing the topic of cyclicity shortly after Sloss had published his key 1963 paper, Weller (1964, p. 609) wrote:

> A review of American geologic literature shows that the idea of more or less worldwide periodicity in diastrophism and marine transgression and regression was firmly held for many years. Attempts were made at first to equate the orogenic and marine cycles with the standard geologic systems. As important discrepancies came to light, however, several influential American geologists sought to revise the systems in conformity with their interpretations of the presumed cycles.

Weller cited the work of Chamberlin, Ulrich and Grabau, but did not cite Sloss, and claimed that American and European workers, alike, largely accepted the "periodic" theories. He went on to describe embryonic concepts about "grand cycles" in North American stratigraphy, that Sloss was to make his own.

Weller may have been correct about European acceptance of the "periodic" theories, but the critical publications in this field were largely American, whereas the main European stratigraphic interests, until Vail's work swept the field in the 1970s, seemed to be on the measurement of time, and on perfection of the system of stages and zones, an approach that evolved naturally towards the development of modern chronostratigraphic methods. The proposals for the definition of stratotypes at places where "nothing happened," and the "golden spike" concept, originated with British stratigraphers (Ager, McLaren, Cowie, Bassett, Holland, Harland), although they were championed by an American, Hollis Hedberg.

Perhaps one reason for the differences in stratigraphic approach is the differences in motivation for carrying out the work. Levorsen was a practicing petroleum geologist, used to dealing with large swaths of stratigraphy. Sloss, although an academic, carried out much of his regional stratigraphic work as a consultant for the petroleum industry, and so large-scale regional correlation was one of his major concerns. In the post-WWII phase, when Sloss was beginning his work, much of the interior and western regions of North America were still in what petroleum geologists would call the "frontier" stage of exploration, for which regional syntheses would be of considerable utility. Europeans, by contrast, had been documenting the regional stratigraphy of their continent for more than one hundred years. Perfection of the minutiae of correlation was a central focus of stratigraphic research, and attempts to define broad cyclic patterns in the European stratigraphic record (e.g., Ramsbottom 1973, 1977), were strongly resisted in some quarters (e.g., George 1978). The regional approach to the stratigraphy of Europe became important with the development of subsurface exploration of the North Sea basin in the 1960s. Vail and Todd (1981) were amongst the first to attempt to develop a generalized stratigraphic synthesis from the North Sea basin, based on the newly available form of data, seismic stratigraphy. We have suggested elsewhere (C. E. Miall and A. D. Miall, 2002) that it was the "black-box" character of the new seismic technique and the cycle chart developed from it that gave this work its authority, and contributed to the rapidly established popularity of the new seismic methodology in many European circles.



TEXT-FIGURE 8
Two approaches to the correlation of the same set of stratigraphic events. The seven events are shown in the same time-space relationship in both diagrams. A. Age, with error bar, is determined independently for each location. The two parallel lines indicate one standard deviation around the first dated event, at the left. The degree of correlation of each event with the first event is then assessed on a statistical basis B. A selected observation (circled dot) is assumed to have a particular significance with respect to a stratigraphic model, and all stratigraphically nearby events are interpreted to correlate with it, as suggested by the arrows. (from A. D. Miall and C. E. Miall 2001).

Despite the interest in broad theories, geologists by and large remain true to the principles of geology as an observational, empirical science. At several stages in the evolution of their science geologists have found the need to restate and defend this, especially at times of rapid changes, such as during the emergence of modern physics in the early twentieth century, and during the development of computer applications and of geophysical methods after WWII.

In his landmark paper on cycles and rhythms in geology, and the measurement of geologic time Barrell (1917, p. 749) complained about physics and physicists thus:

> Not only did physicists destroy the conclusions previously built by physicists, but, based on radioactivity, methods were found of measuring the life of uranium minerals and consequently of the rocks which envelop them. … Many geologists, adjusted to the previous limitations, shook their heads in sorrow and indignation at the new promulgations of this dictatorial hierarchy of exact scientists. In a way, this skepticism of geologists was a correct mental attitude. The exact formulas of a mathematical science often conceal the uncertain foundation of assumptions on which the reasoning rests and may give a false appearance of precise demonstration to highly erroneous results.

Teichert (1958, p. 116) said:

> In an era of glorification and triumph of the physical sciences we are sometimes tempted to believe that all geological issues may be amenable to analysis and solution by experiment and mathematical formulation and to forget that geology occupies the borderland between physical and the biological sciences.

Teichert (1958, p. 116) noted that geology is, nonetheless, like other sciences in that it can generate predictions for testing, such as the locations of mineral deposits or oil pools.

The distinguished British petrologist H. H. Read (1952) is famous for this remark that the "best geologist is the one who has seen the most rocks," while the eminent American sedimentologist Francis Pettijohn (1956, p. 1457) said "Nothing, my friends, is so sobering as an outcrop. And many a fine theory has been punctured by a drill hole." Hallam's (1989) remark on this topic was quoted in the opening sentence of this paper. But field work is not a cure-all. Ager's (1970, p. 424) warning—and he could have been referring to any of the grand theories that have evolved over the last two hundred years—remains as true as it ever was: "many a geologist travels the world merely to seek confirmation of his own favourite backyard theory and ignores everything else."

We need to cease apologizing for labeling stratigraphy a "descriptive science." That word "description" conceals the emergence of an ever more remarkably rich and complex data base of facts about Earth's tectonic, climatic, paleogeographic and evolutionary history. Revelations from this empirical data base have encouraged the development of increasingly sophisticated tools for observation, including the whole deep-sea drilling enterprise and the methodologies for subsurface remote sensing (reflection seismic, seismic tomography, wireline logging), without which none of our modern theories about the Earth would be more than mere conjectures. Our best computer models of climate change, crustal subsidence, or sequence stratigraphy, are only as good as the carefully collected facts that are used as input and to set boundary conditions. As Torrens (2002, p. 252) said, citing John Dewey (1999), "core, field-based geology is [still] the most important, challenging and demanding part of the science." In the age of high-speed computing, the roadside outcrop and the drill core remain as important as they ever have been. And earth scientists need to keep returning to them to ensure that the results of advanced laboratory methods, statistical analysis and numerical modeling remain firmly grounded in those "descriptive" facts.

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