Empiricism and model-building in stratigraphy: Around the hermeneutic circle in the pursuit of stratigraphic correlation

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ABSTRACT: The discipline of geology has varying theoretical and methodological approaches to the study of the earth and its processes. By focusing on the ways in which geological theories are constructed and tested through the use of various methodologies, it becomes evident that research approaches compete with each other to legitimate their own constructions of scientific knowledge. It also becomes clear that uncritical adherence to theoretical and methodological approaches and assumptions may obscure rather than illuminate objects under study.

The scientific process in geology is exemplified by the *hermeneutic circle* (Frodeman, after Heidegger), in which empirical observation, generalization and theorizing (*induction*), are followed by construction of hypotheses (including models) and renewed observations to test and refine or abandon a theory (*deduction*). Ideally, this is a continuous and circular process, whereby theoretical assumptions are put to the test, but history demonstrates that the inductive and deductive approaches have largely been followed by different groups of stratigraphers with different objectives. Further, these stratigraphers have tended to work in isolation from each other.

According to Hallam, "geologists tend to be staunchly empirical in their approach", but are also inveterate model builders, attempting to explain their universe by developing deductive models. Two contrasting case studies illustrate empirical and model-based approaches to dating and correlation. A synthesis by Callomon (1995) of Jurassic ammonite biostratigraphy, based on a century of data collection, and the inductive building of a biozone scheme, reveals numerous gaps and considerable local stratigraphic variability in the studied sections in southern England. By contrast, a comparison by Gale at al. (2002) of two sections in India and France using a sequence model for correlations was interpreted by them in terms of global uniformity of sequence-generating processes and eustatic sea-level control.

Modern dating methods should be rigorously empirical, including the cross-correlation of multivariate dating techniques and the use of non-events as boundary markers ("golden spike" concept). Extreme caution needs to be employed in introducing such deductive concepts as "global cycles," "event stratigraphy" and "cyclostratigraphy" into methods of high-resolution chronostratigraphy. Currently, cyclostratigraphers have established the Milankovitch model of orbital forcing as the centerpiece of a research program to document climate change and to provide a basis for a high-resolution time scale. There are at least three problems with this approach: 1) Researchers downplay the probability that orbital frequencies may have differed in the geological past. 2) There is a tendency to make assumptions about stratigraphic completeness and constancy of sedimentation rate that may not be valid. 3) Independent chronostratigraphic calibration of cyclostratigraphic data is insufficiently precise, and cannot at present provide adequate constraints on cyclostratigraphic models based on tuning, filtering, and other statistical techniques.

INTRODUCTION

Paradigms in research are defined as "basic belief systems based on ontological, epistemological and methodological assumptions" (Guba and Lincoln 1994, p.107). Paradigms guide the production of knowledge by specifying the *nature* of reality and how it can be *known* (Kuhn 1962). The discipline of geology has varying theoretical assumptions and approaches, and various internal distinctions among objects and methods of study. By focusing on the ways in which geological theories are constructed and tested through the use of various methodologies, we can better understand how research approaches may obscure rather than illuminate the reality they purport to explain.

In the closing chapter of his book on geological controversies Hallam (1989, p. 221) concluded: "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." This is certainly true, but geologists are also inveterate model builders. They attempt to reduce nature's complexity to a more manageable level by simplifying it. In this paper we attempt to draw attention to a long-standing and ongoing tension between the empiricists in stratigraphy, as exemplified by those involved in the construction and perfection of the geological time scale, and the model builders, of which practitioners of cyclostratigraphy comprise the most important current example. We address the problems that can arise when observation is conditioned by the expectations arising from the use of deductive models of geological processes.

Hermeneutics

A useful way to encapsulate geological methodology is to make use of the concept of the *hermeneutic circle* (Miall, this volume, text-fig. 1), in which empirical observation, generalization and theorizing (*induction*), are followed by construction of hypotheses (including models) and renewed observations, in order to test and refine or abandon a theory (*deduction*). Hermeneutics originated as a theological approach to the search for and interpreting the spiritual truth in the Bible—the word is derived from the Greek hermeneus, meaning an interpreter (Bullock and Stallybrass 1977). The philosophers Dilthey and Heidegger demonstrated the broader utility of the concepts as the basis for the "art, skill, or theory of interpretation, of understanding the significance of human actions, products and institutions" (Bullock and Stallybrass 1977, p. 281). These ideas are widely used in the social sciences (Wallace 1969), and Frodeman (1995) introduced the concepts to geologists in a discussion of how the geological sciences and geological methodology differ from those of the "hard" physical sciences, such as physics and chemistry.

Although geology is, of course, an observational science, many of its important hypotheses and constructs are not amenable to testing in the way that experimental methods are used to test new ideas in the physical sciences. We cannot replicate the past, but nonetheless, reconstructions of geological history, such as a sequence-stratigraphic reconstruction or interpretations of paleogeography typically contain within them testable hypotheses, such as predictions of subsurface stratigraphic extent. These may commonly be directly tested by further observation, such as the careful siting of an exploratory well. Much geological work consists of the assembling and reconciliation of disparate forms of data. Thus, Dott (1998) has referred to geology as a "synthetic science." Further, Frodeman (1995) has emphasized the importance of the idea of "narrative logic" and the significance of "explanations that work" in geological interpretation. A form of hermeneutics was already understood and practiced by geologists by the end of the nineteenth century. Geologists currently make use of the "multiple working hypothesis" methodology of G. K. Gilbert and T. C. Chamberlin, and an important paper by Johnson (1933) set out the basis for the deductive and inductive approaches in the geological sciences (Miall, this volume, text-fig. 1).

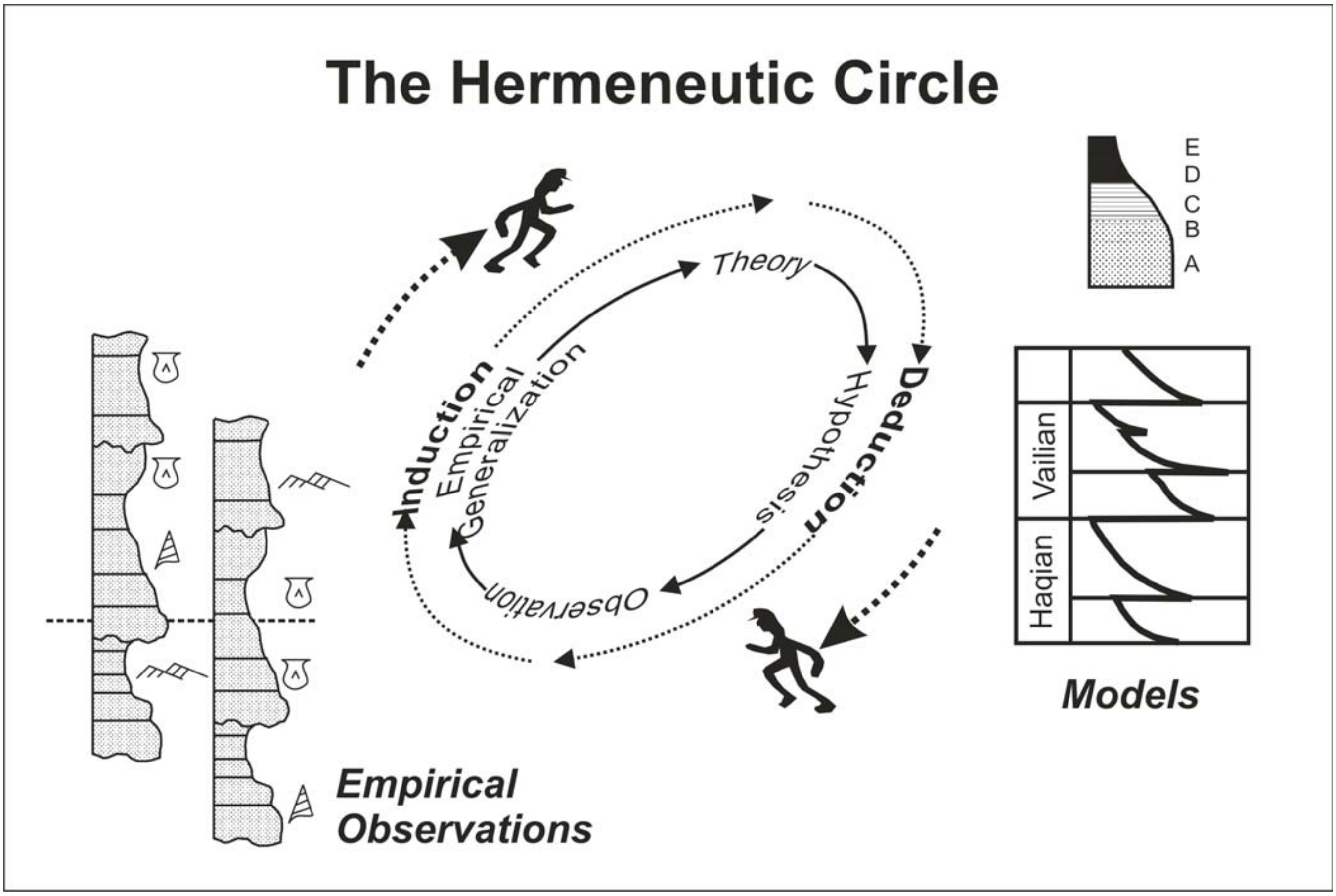
Baker (1999) pointed out that modern numerical simulation is a form of deduction in which assumptions regarding processes, starting conditions and boundary conditions are used to construct algorithms, which are essentially numerically-based hypotheses about the relationships between variables. To some extent, numerical simulation in Geology serves a purpose analogous to that of the experiment in Physics and Chemistry. To this extent, Geologists can propose testable hypotheses and follow Popper's dictums regarding falsifiability. A major difference with methods of the experimental sciences is that the experiments of Physics and Chemistry are carried out in carefully controlled laboratory conditions in which every parameter and boundary condition is controlled. This is not the case with simulations of the geological past, which, even when using the largest and most powerful of modern computers, represent extreme simplifications of reality. It may never be possible to determine and define for the purpose of such experiments all the necessary boundary conditions of scenarios for the geological past, an exercise even more complex than that of modern meteorology that uses forward modeling from observed data to predict future weather. As Cleland (2001, p. 989) pointed out in connection with numerical geological experiments, whereas many experiments may be run, the method cannot determine which, if any, of the results correspond to a past reality, just which are the most likely, based on how well the results correspond to observations of the geological record.

A major problem with an interpretive science such as geology is the degree to which interpretations may influence the collecting of observations. The very act of making observations is necessarily guided by the body of ideas and hypotheses that prevails at the time the observations are made. As Kuhn (1962, p. x) explained, "*paradigms*" are ". . . universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners." Elsewhere (Miall and Miall 2001) we have shown how differences in guiding hypotheses about sequence stratigraphy have led to two completely different paradigms that affect the collection and interpretation of observations in that field. In a companion study (Miall, this volume), the deep historical roots in geology of these two paradigms are explored.

The relationship between observation and interpretation in geology is shown in a hermeneutic context in text-figure 1, and in text-figure 2 we illustrate the point we made briefly in Miall and Miall (2001, p. 323) about the changing nature of stratigraphic data. Our views of "what works" have changed dramatically as the science has evolved. Until the 1950s, stratigraphic practice consisted primarily of what we now term lithostratigraphy, the mapping, correlating and naming of formations based on their lithologic similarity and their fossil content. In the 1960s, the revolution in process sedimentology led to the emergence of a new science, facies analysis, and a focus on what came to be termed "autogenic" processes, such as the meandering of a river channel or the progradation of a delta. Most stratigraphic complexity was interpreted in facies terms, and the science witnessed an explosion of research on process-response models, otherwise termed facies models. The revolution in seismic stratigraphy in the late 1970s changed the face of stratigraphy yet again, with a new focus on large-scale basin architecture and regional and global basinal controls. In the 1980s, sequence-stratigraphic methods and terminology came to dominate stratigraphic observations and documentation. These changing interpretations are shown as three different sets of annotations of the same outcrop photograph in text-figure 2. Each deductive model in turn informed the kinds of observations made on the rocks.

In the course of twenty years, therefore, the kinds of data geologists looked for in the rocks, and the "explanations that worked" in describing them, underwent two wholesale changes. The rocks did not change, but the "objective" facts that geologists extracted from them did. This attests to our improved understanding of our own subject, but the details of the evolution of this science, as with any other, are also influenced by human factors. The fact that scientific journals include "Commentary" or "Discussion" sections attests to the fact that apparently dispassionate observation can, nonetheless, lead to different interpretations and to controversy. Scientists accept this, while they remain reluctant to accept that human factors play an important role in scientific development. How important is the reputation of the scientist in furthering a new idea? How important is "fashion"?

The development of the process-response facies-model concept in the late 1950s (Potterr 1959) was a major breakthrough in the development of sedimentology as an effective instrument for understanding sedimentary rocks (Miall 1999, provided a recent discussion of the facies model concept, particularly as it has been applied to fluvial deposits, pointing out the strengths and weaknesses of the facies-model approach). However, as might have been expected, the application of deductive concepts to the incomplete and commonly qualitative data that characterize stratigraphic data sets has not been without controversy. For ex-



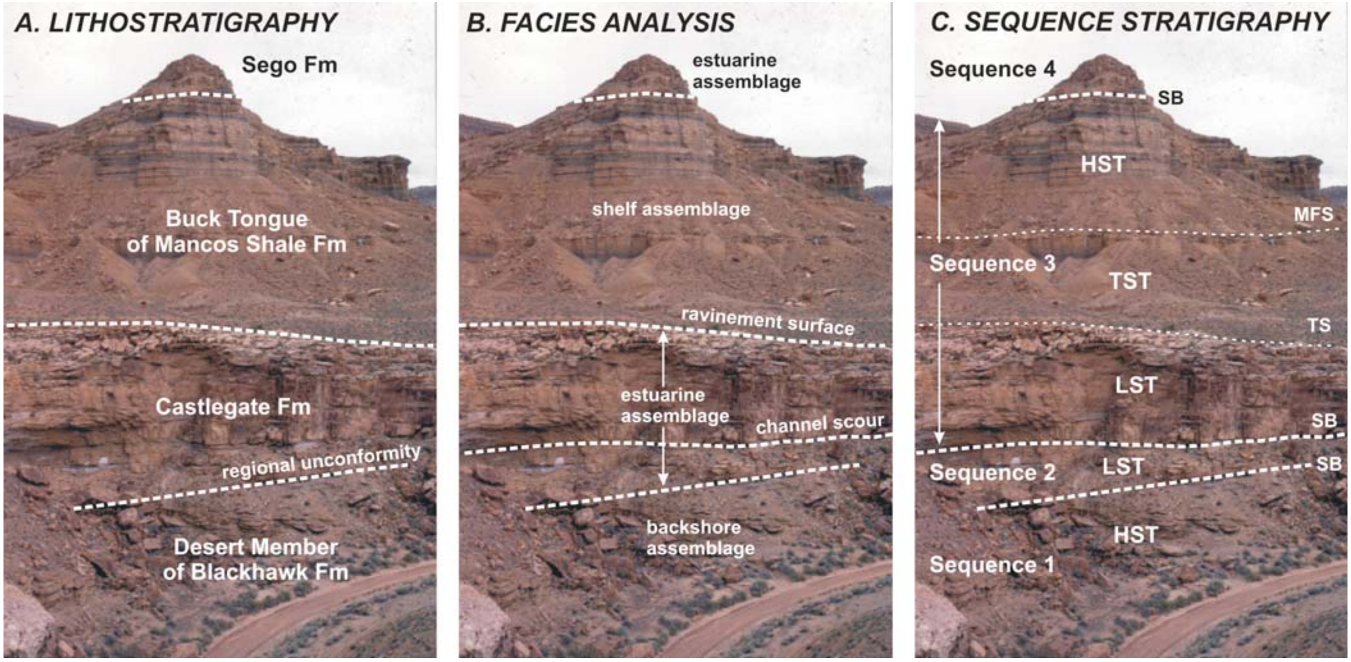
TEXT-FIGURE 1
The hermeneutic circle, illustrated with geological examples. The feedback loop between observations (the stratigraphic sections at lower left) and models (the turbidite model and the Vail cycle chart as examples in the upper left) should be continuous, leading to iterative improvements in the models, but controversy surrounds both these examples (see Shanmugam 1997, on turbidites and Miall and Miall 2001 on the global cycle chart).

ample, those of us who were active in the 1970s may remember a time when turbidite models were first popularized, and all thinly bedded lithic arenites tended to be reinterpreted as turbidites. In his definitive explication of the facies-model concept, Walker (1976) employed the development of the Bouma turbidite model (the column with the A-E divisions shown schematically in the upper right of text-fig. 1) to illustrate the inductive and deductive processes involved in developing and elaborating the turbidite concept. However, twenty years later, Shanmugam (1997) argued that too ready an acceptance of the Bouma turbidite model has distorted both observation and interpretation of many of the arenites in the ancient record that were formed by sediment-gravity-flow processes. Many of his arguments focus on the problem arising from the fact that the deductive turbidite model led field geologists to expect to find confirming evidence of their starting deductive model rather than encouraging them to seek new data to evaluate and refine (or discard) the model as inappropriate.

It is our thesis that in the study of geologic time, inductive and deductive methodologies have largely been employed by different sets of stratigraphic practitioners who have historically had little to do with each other (Miall and Miall 2001). We have suggested that this has been the case essentially since the foundation of the discipline of stratigraphy in the early nineteenth century (Miall, this volume). Here we focus on the modern work underway to establish a tightly constrained stratigraphic history of Earth.

Model building in chronostratigraphy

By far the most influential model in stratigraphy over the last twenty years has been what we term the *global eustasy paradigm* of sequence stratigraphy (Miall and Miall 2001). Some of the essential characteristics of this model are summarized in text-figure 3. The core principle of this model is the belief that the global cycle chart, as first proposed by Vail et al. (1977), and subsequently revised by Haq et al. (1987, 1988) and Graciansky et al. (1998), may be used as "an instrument of geochronology" (Vail et al. 1977, p. 96). We have explored the construction, composition and implications of the global eustasy model elsewhere (Miall and Miall 2001). In summary, the defining feature of the model is the belief that sequence boundaries are global chronostratigraphic indicators, and that their ages are not influenced by the tectonic behaviour of sedimentary basins. From this first principle follows the use of pattern recognition as a means of correlation. In correlation charts constructed with time as the ordinate, correlation lines will always be parallel ("railroad correlation lines") — this was one of the distinguishing characteristics of the first published chart to show the relationship of regional cycle charts to each other and to the derived global chart (Vail et al. 1977, Fig. 5, p. 90).



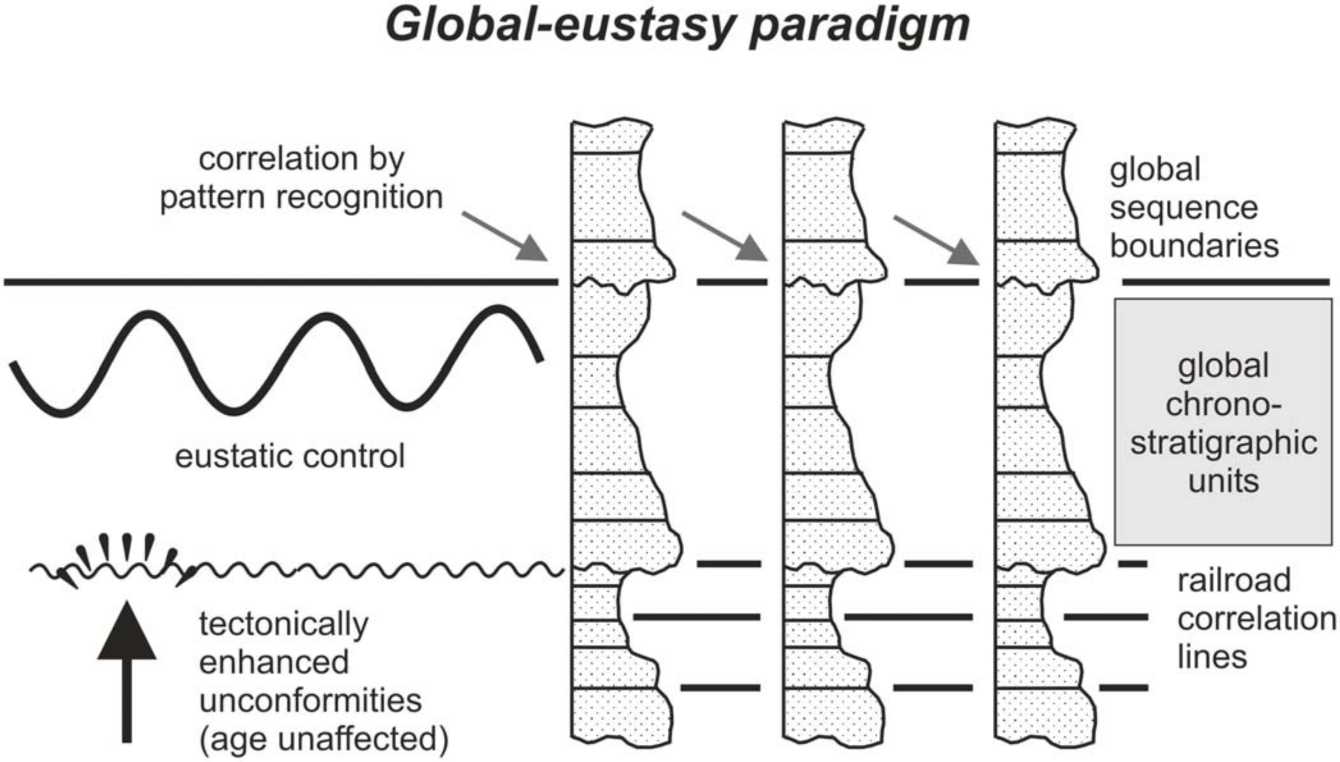
TEXT-FIGURE 2
An outcrop in the Book Cliffs of Utah (Tusher Canyon, near Green River) interpreted using three successive deductive models of stratigraphic interpretation. A, based on Fouch et al. (1983); B, C, based on van Wagoner (1990) and Yoshida (2000). Sequence-stratigraphic terminology: SB=sequence boundary, TS=transgressive surface, MFS=maximum flooding surface, LST, TST, HST=lowstand, transgressive and highstand systems tracts.

Despite the considerable body of critical discussion about the global cycle chart that has appeared since the mid-1980s, adherents of the global eustasy model continue to practice a science that emphasizes the predominant importance of this deductive model. Elsewhere (Miall and Miall 2001) we have documented instances in which empirical data have been discarded or overruled in favor of a specific outcome of the model. The dominance of the global–eustasy model among one group of practitioners is illustrated in the study of the correlations between Cenomanian (Upper Cretaceous) sections in the Anglo-Paris Basin and in southeast India. This work specifically set out "to demonstrate that sea-level changes are globally synchronous and therefore must be eustatically controlled." (Gale et al. 2000, p. 291). The key data diagram in this short paper is a chart showing the relationships between sequence and systems tracts in the two basins (text-fig. 4). The ordinate in this diagram is an arbitrary scale which assigns each sequence equal space. Sequence boundaries, and even systems tracts, can therefore be correlated between the two basins, which are on opposite sides of the world, using parallel "railroad-line" correlation lines. There is no indication that the sections in the two basins are of very different thicknesses (which can be deduced from a derived sea-level curve included in the paper), nor is there any suggestion in the diagram that the sequences might represent varying time intervals or that they may include significant disconformities. Indeed, it is claimed that the sequences correspond to the 400-ka Milankovitch-band eccentricity cycles, identified in a different study of another basin by one of the authors, based on spectral analysis of grayscale reflectance data of chalk. The only actual empirical data provided in this paper are the ranges of key ammonites, shown relative to the sequence and systems-tract boundaries, and they reveal some discrepancies in the sequence correlations.

As Kuhn (1962) has observed, "Results which confirm already accepted theories are paid attention to, while disconfirming results are ignored. Knowing what results should be expected from research, scientists may be able to devise techniques that obtain them." In the authors' eyes, therefore, their correlation diagram (Gale et al. 2002, Fig. 3) serves the purpose of a "challenge successfully met" (Kuhn 1996, p. 204), the challenge of correlating sequences of events in widely spaced basins having no tectonic relationship to each other. As Kuhn (1996, p. 205) also noted, "The demonstrated ability to set up and to solve puzzles presented by nature is, in case of value conflict, the dominant criterion for most members of a scientific group." The work of Gale et al. (2002) appears to present a "puzzle" to those who doubt the reality of global eustasy. But does it? Perhaps it all lies in how the data are presented.

We cite here a different opinion, based on a different kind of test of sequence correlations: Prothero (2001) carried out a series of correlation tests on rocks of Paleogene age in California. Amongst his conclusions:

> Sequence stratigraphic methods are now routinely applied to the correlation of strata in a wide variety of depositional settings. In many cases the sequence boundaries are correlated to the global cycle chart of Haq et al., (1987, 1988) without further testing by biostratigraphy or other chronostratigraphic techniques. Emery and Myers (1996, p. 89) noted that "sequence stratigraphy has now largely superceded [sic] biostratigraphy as the primary correlative tool in subsurface basin analysis." The last decade of layoffs of biostratigraphers from most major oil companies also seems to indicate that some geologists think they can get along fine without biostratigraphic data. Where the biostratigraphic data are very low in resolution or highly facies-controlled, perhaps sequence stratigraphic correlations work better.



TEXT-FIGURE 3
The elements of the global–eustasy model, based on the methodologies first proposed in Vail et al. (1977), and most recently employed by Graciansky et al. (1998).

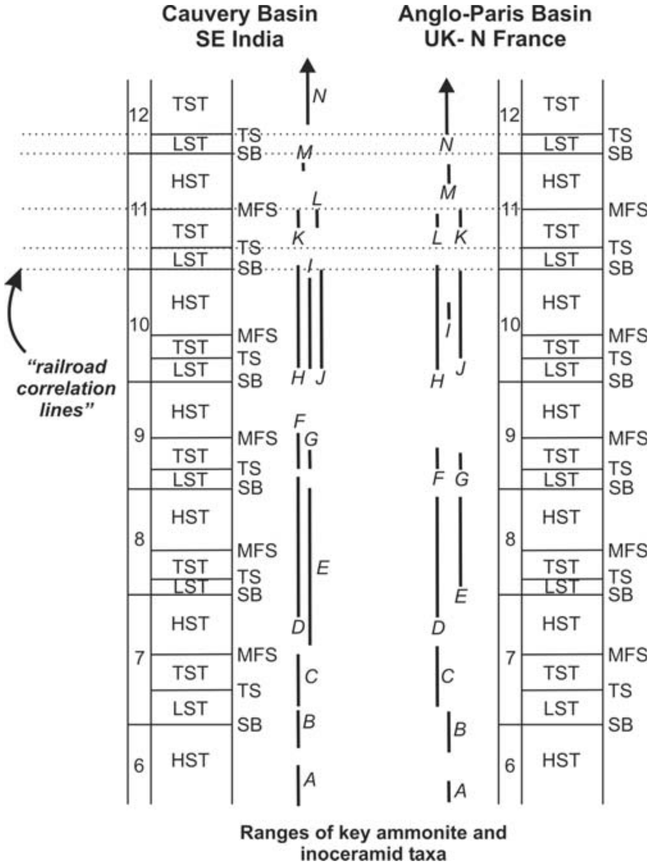
But if there is any lesson that two centuries of geological investigation since the days of William Smith have taught us, it is that *biostratigraphy is the ultimate arbiter of chronostratigraphic correlations*. The literature is full of lithostratigraphic correlation schemes that have failed because of insufficient attention to biostratigraphy. ….. Blindly correlating stratigraphic events to the outdated onlap-offlap curve of Haq et al. (1987, 1988) without determining whether biostratigraphic data support their correlations, continues the trend of poor science.

We turn now to another study of high-resolution ammonite biostratigraphy, one that tells a completely different story. The Jurassic strata of western Europe are of central importance in the history of Geology. Williams Smith invented the concept of the geological map with his early work in the Jurassic outcrops near Bath, England (Winchester 2001). Gressly (1838) developed his ideas about facies working on the Jurassic strata of the Jura Mountains. Concepts of the stage, the zone and the hemera were all worked out from detailed biostratigraphic study of Jurassic strata in England, France and Germany (see Miall, this volume). As a result, there is an immense body of knowledge available dealing with the biostratigraphy of these rocks. We refer here to a single study, one by Callomon (1995), who returned to the very detailed work of S. S. Buckman and R. Brinkmann during the first decades of the twentieth century on the Middle Jurassic Inferior Oolite formation of southern England.

Building on this early work, Callomon (1995) identified fifty-six faunal horizons based on ammonites in the Inferior Oolite, a shallow-marine limestone succession some 5 m in thickness, spanning the Aalenian and Bajocian stages. Callomon's calculations demonstrate that these horizons average 140 ka in duration, but without any implication that these horizons might be equally spaced. Sedimentological study shows the succession to contain numerous scour surfaces and erosion surfaces, but only the detailed biostratigraphic data could have revealed the complexity of the stratigraphic picture that Callomon presents. A detailed comparison between thirteen sections through this short interval, spaced out along an 80-km transect across Somerset and Dorset, in southern England, shows that each section is different in almost every detail (text-fig. 5). Approximately half of the faunal horizons are missing in each section, and it is a different suite of missing events in each case. None of the horizons is present in all of the thirteen sections. Callomon (1995, Table 5) demonstrated that on average the sections are only 43% complete.

The 'gaps' united by thin bands of 'deposit' are evident. The time durations that left no record … or whose record has been destroyed … are often greater than the time intervals … between the biochrons of adjacent faunal horizons. What is less evident, however, is any coherent relationship between the lengths of the gaps and their positions, such as might be explicable by simple sequence stratigraphy—and this across a distance of only 80 km in a single basin (Callomon 1995, p. 140).

Note that Figure 5 also uses an arbitrary ordinate, but in this case an argument can be made that it probably represents an insignificant distortion of the original empirical data. Callomon's paper includes samples of three of the actual stratigraphic sections, plotted with a thickness ordinate, which show that the ma-



TEXT-FIGURE 4
Correlations between Cenomanian sections in southeast India and the Anglo-Paris Basin, plotted on an arbitrary ordinate. Note the exact parallelism of all correlation lines between key surfaces (sequence and systems tract boundaries) throughout this diagram (adapted from Gale et al. 2002).

jor biozone increments are indeed of comparable thickness (Callomon 1995, fig. 4). Callomon's paper is, in fact, rigorously empirical. The interpretation of continuous, if fragmentary, successions in text-figure 5 has been added by us for the purpose of this paper to emphasize the patchiness of the geological record, a point to which we return below. An interpretation of this stratigraphic pattern would seem to require calling on a variety of simultaneous, interacting processes, such as slight tectonic movements, variations in sediment supply and in the strength of marine currents, as well as possible changes in sea-level. In other words, this example of the stratigraphic record would seem to be a good example to bring forth in support of what we have called the *complexity paradigm* (text-fig. 6).

The differences between the Gale et al. (2002) study and that by Callomon (1995) cannot be attributed to differences in geology, such as differences in the depositional environments of the rocks. Undoubtedly, the Middle Jurassic succession of southern England is replete with erosion surfaces, but so are the sections studied by Gale et al. (2002). These authors base their sequence definitions on the recognition of erosion surfaces, which they classify as sequence boundaries, and on facies changes from shelf to non-marine deposits, upon which they based their definitions of systems tracts. Even successions that are entirely marine in origin have typically been found to contain numerous breaks in the record, and complex patterns of non-correlating disconformities and diastems when subjected to detailed biostratigraphic study (e.g., Aubry 1991, 1995). The presence of numerous diastems even in deep-marine deposits has long been known (Fischer and Arthur 1977).

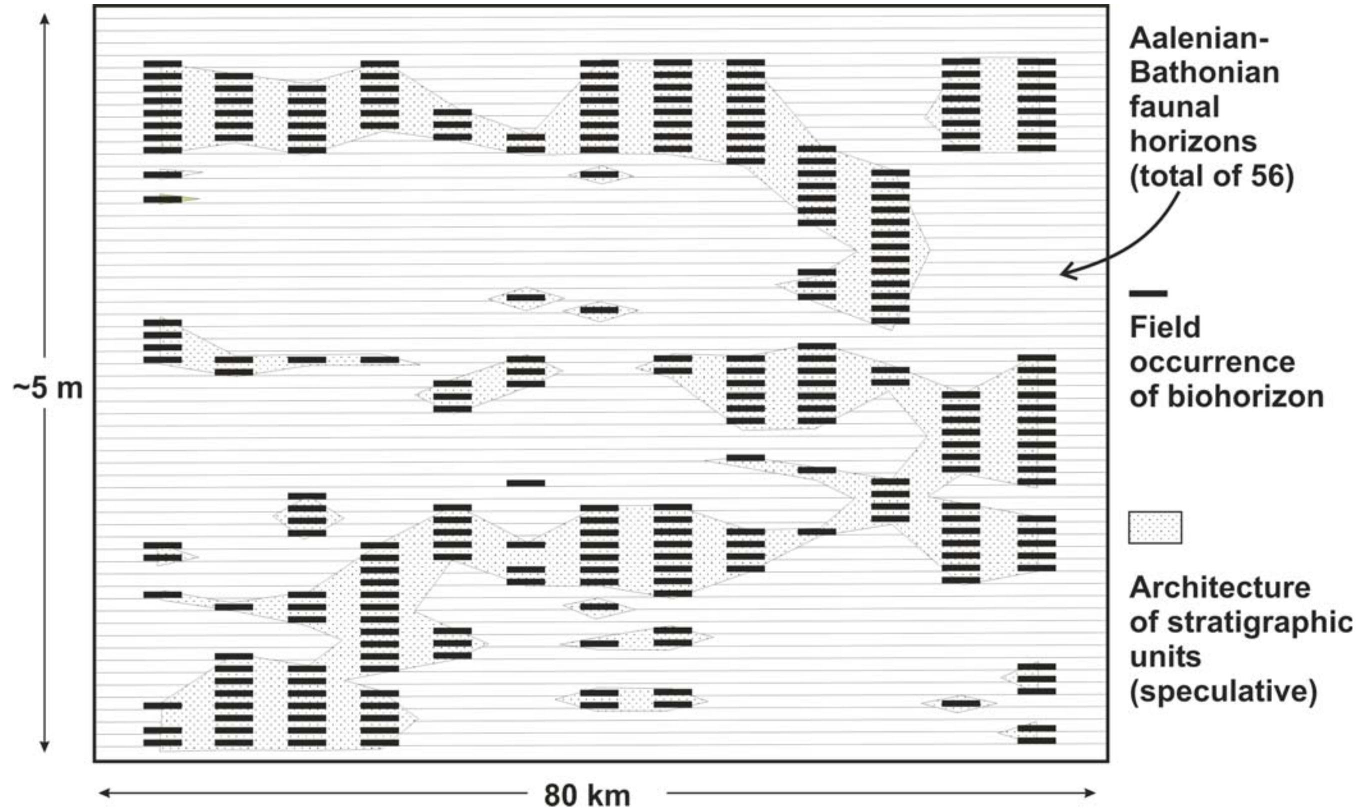
Clearly, the two studies can each be assigned to one of the two paradigms that we developed in Miall and Miall (2001), Gale et al. (2002) to the global-eustasy paradigm and Callomon (1995) to the complexity paradigm. We suggest that the differences between the two studies have nothing to do with the geology described by the authors, and everything to do with the way the data have been presented, based on what Stewart (1986) has termed the "Interests perspective." Building on Kuhn (1962), Stewart (1986, p. 262) suggested that when there is competition between two or more scientific theories or paradigms "the key process determining choices between paradigms is persuasion based on widely shared values, such as quantitative predictions, accuracy of results, simplicity, and scope." The choice between paradigms "is not determined by which one can explain the most 'facts'—for what is accepted as a fact depends to a large degree on one's accepted paradigm." The choice between paradigms can also represent "desires to protect the basis of one's previous intellectual contributions" (Stewart 1986, p. 263). For example, one of the six authors of the Gale et al. paper, is also a co-author of several of the papers we classify in our earlier work (Miall and Miall 2001) as representing the global eustasy paradigm. As we noted elsewhere (Miall and Miall, 2002, p. 322), the authors who accepted and used the global eustasy model and the global cycle chart as unproblematic premises for their own research, gave social support to these practices and thereby, contributed to "transforming them into 'facts of measurement and effect estimation'" (Fuchs 1992, p. 50; Latour 1987). Further, they contributed to the transformation of these "facts" into an unproblematic *black box* and "an unquestioned foundation for subsequent scientific work" (Fuchs 1992, p. 48). As Golinski (1998, p. 140) has summarized, "when an instrument . . . assumes the status of an accepted means of producing valid phenomena, then it can be said to have become a 'black box.'" We return to the concept of the black box below.

Representation of time in stratigraphy

One of the major differences between the two studies examined thus far (Gale et al. 2002; Callomon 1995) is the difference in the way the authors treat missing time. One of Callomon's main results was the demonstration of the extremely fragmentary nature of the preserved record of the Inferior Oolite in southern England. By contrast, the implication of the Gale et al. (2002) study is that sedimentation is virtually continuous.

Geologists have had to repeatedly remind themselves that the stratigraphic record is highly incomplete. Ager (1973) is famous for his remark that the "stratigraphic record is more gap than record." Barrell (1917) effectively demonstrated this nearly a century ago with a diagram, reproduced in our companion paper (Miall, this volume, text-fig. 2). He was able to demonstrate that "Only one-sixth of time is recorded" by sediments (Barrell 1917, p. 797). As Wheeler (1958, p. 1047) noted, "the temporal value of such significant events as non-deposition and erosion are reduced to zero in a section whose vertical dimension is adjusted to thickness of the stratal record."

Modern studies that have been carefully constrained by high-resolution chronostratigraphy confirm the generality of



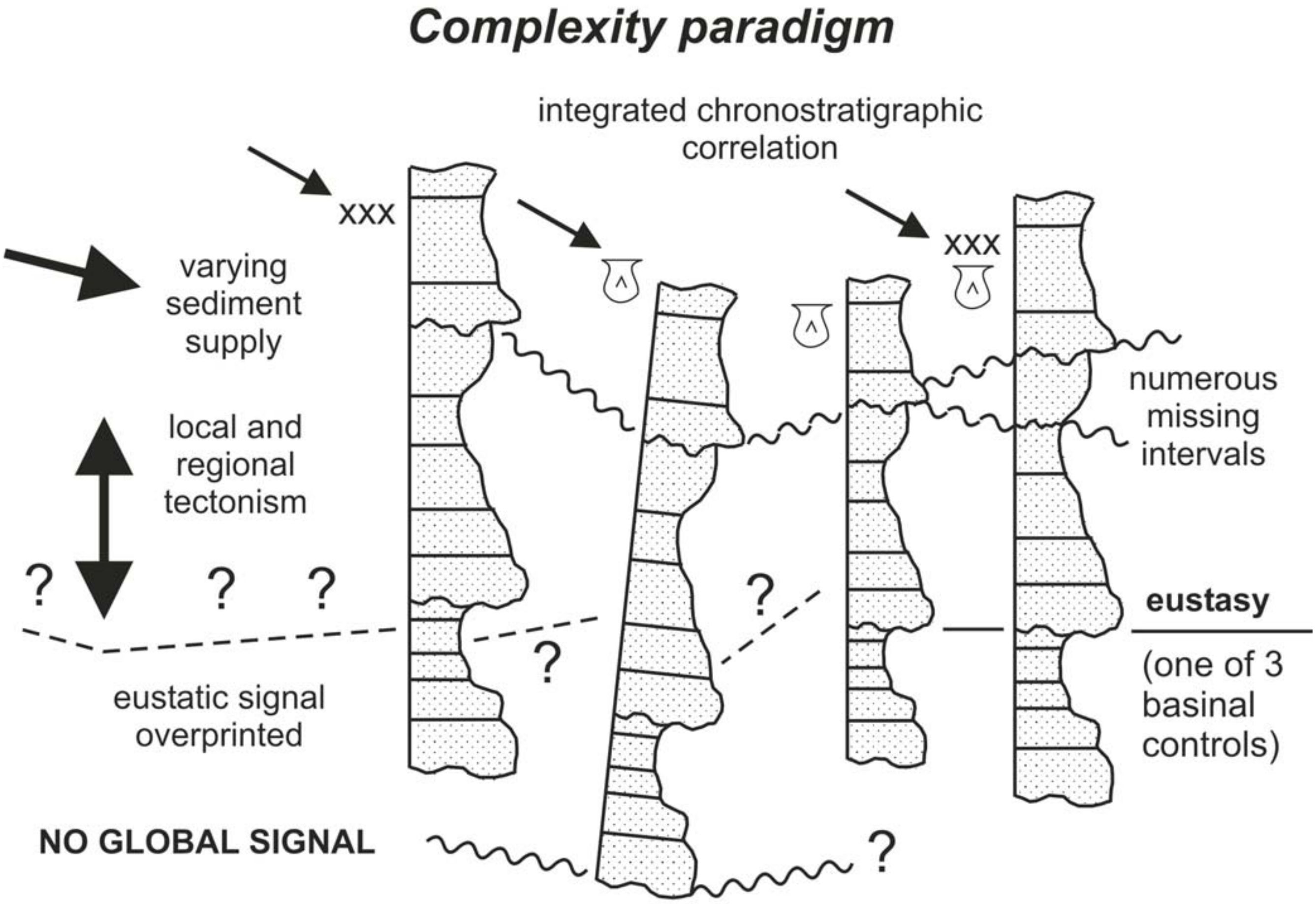
TEXT-FIGURE 5
The fifty-six ammonite faunal horizons in the Inferior Oolite of southern England, simplified from Callomon (1995, fig. 5), shown for simplicity as horizontal lines. The presence of each horizon in each of the sections is shown as a rectangular box, and the possible occurrence of conformable units within and between the sample sections is indicated by the shaded-in areas (our addition).

Barrell's point. Smith (1993) argued that stratigraphic sections may be essentially complete at the 0.5 m.y. scale in some fluvial and pelagic sections but only at the 2 m.y. level in some terrigenous shelf sections. Aubry (1995) demonstrated that deep-marine records typically consist of relatively continuous sections spanning up to a few million years, separated by hiatuses of similar duration, but that the age distribution of the hiatuses is systematic on only a local to regional scale. Text-figure 7 reproduces a chronostratigraphic correlation chart of the Castlecliff Formation, a very well-exposed Pliocene-Pleistocene section in the Wanganui Basin of New Zealand. This has been selected as a reference section for this stratigraphic interval, and has revealed numerous important details about the nature of high-frequency sequences and systems tracts (e.g., Naish and Kamp 1997). Calibration of this section against the oxygen isotope record has revealed the duration of the disconformities—the areas delimited by diagonal ruling in the centre column. They total approximately 53% of the elapsed time represented by this section.

In fact, considerably more than half of the elapsed time is represented by hiatuses in stratigraphic records of shallow-marine to coastal sequences. Text-figure 7 was drawn assuming that the intervals of preserved section, the sequences (the white boxes in the centre column) represent continuous section. More detailed examination of the processes involved in the accumulation of the systems tracts and lithosomes, of which shallow-marine to coastal sediments are composed, reveals the probability of numerous sedimentary breaks of shorter duration (e.g., see Devine 1991). It has long been known that there is an inverse logarithmic relationship between the sedimentation rates that may be calculated from stratigraphic sections and the total age range of the section within which the measurements are made (Sadler 1981). Sedimentation rates calculated from modern environments for the accumulation of individual lithosomes, such as storm sequences or tidal bundles are in the order of 10^2 to 10^4 m/ka. Shelf sand ridges and the fill of tidal or fluvial channels accumulate at rates of 10^0 to 10^{-1} m/ka. The sedimentation rates for complete high-frequency sequences are in the range of 10^{-1} to 10^{-2} m/ka (Miall 1991). At each increase in the scale of the system being measured, the section includes a greater number of more significant hiatuses, hence the drop in overall sedimentation rate.

The point of this is that the stratigraphic record is typically far more gap than record (text-fig. 8). This helps to explain the apparent meaninglessness in the pattern of correlations in the Inferior Oolite (text-fig. 5). It implies enormous scope for miscorrelation. It calls into question the value of deductive models that imply "railroad line" correlations of sequences and systems tracts over enormous distances and between areas of varying tectonic setting and sediment supply. In fact, it calls into question any stratigraphic model that is not based on a rigorous, empirical, chronostratigraphic basis.

In their first publications Vail et al. (1977) were at pains to emphasize the chronostratigraphic value of sequences and of sequence boundaries. They illustrated chronostratigraphic plots of sequences constructed using "Wheeler diagrams", which are



TEXT-FIGURE 6
Summary of the complexity paradigm, as described in Miall and Miall (2001).

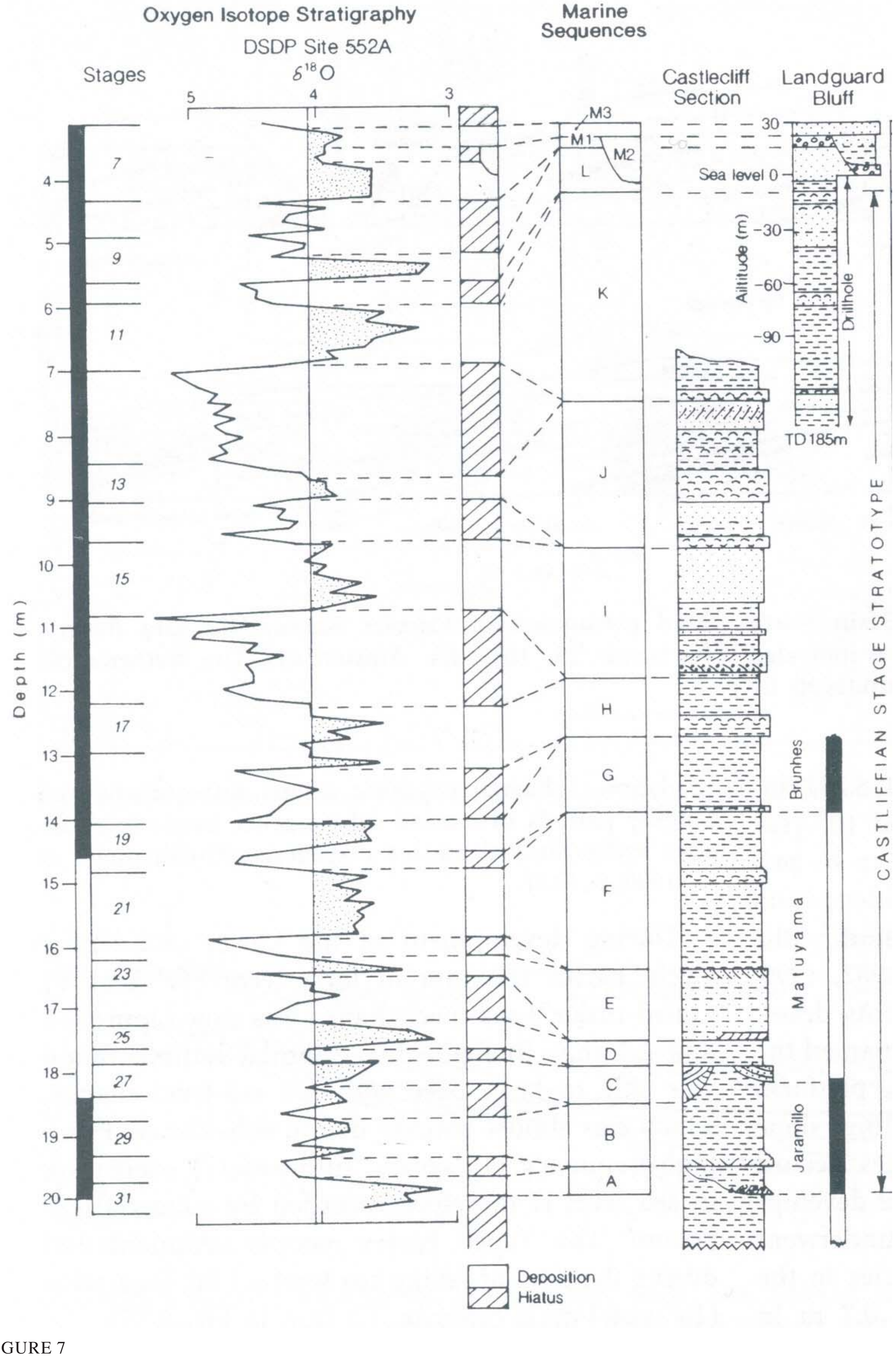
stratigraphic cross-sections drawn using time as the ordinate, instead of thickness. However, in most subsequence applications of sequence stratigraphy, Vail and his colleagues have not paid rigorous attention to the time unaccounted for at sequence boundaries. Many of the sequences in the new global cycle chart (Graciansky et al. 1998) are of Vail's "third-order" type, that is, they have durations of a few millions of years, at most. Ages of sequence boundaries have commonly been assigned to the nearest 0.5 m.y., yet there is no recognition of the missing time at the sequence boundaries—the implication seems to be that the ages of the surfaces above and below the sequence boundaries are comparable.

Model building in the new chronostratigraphy

The basis for modern chronostratigraphic methods should be rigorous empiricism. For example, the purpose of defining chronostratigraphic boundaries within continuous sections —where "nothing happened", to use McLaren's (1970, p. 802) felicitous phrase—is precisely in order to avoid having to interpret the meaning of unconformities or the significance of a prominent stratigraphic event that might otherwise make for a readily recognizable local marker. In other words deductive models are avoided at all costs. As Berggren et al. (1995b) put it in their Introduction to a major compilation of new studies of geologic time: "The biostratigrapher deals not so much with falsification of rival hypotheses, the definitive mode of scientific reasoning described by Karl Popper, but rather with the progressive enhancement of what is already know."

That does not mean to say that other factors have not come into play in the selection of boundaries and of boundary stratotypes. Simmons et al. (1997) referred to "political and personal preferences and pressures" in the choice of one location over another. However, by and large the principle is to let the definition of boundaries evolve from the natural empirical data base that is built up world wide for each interval of geologic time. Such data bases are now complex and multifaceted, involving not just multiple biostratigraphic records, but radiometric dating, magnetostratigraphy, and a growing body of chemostratigraphic evidence (e.g., see Gradstein et al. 1995). Notably, however, there remain serious local problems relating to the diachroneity of biohorizons, limitations in the use of magnetostratigraphy at low latitudes, imprecision of radiometric ages, and the effects of species variation on oxygen isotope stratigraphy (Kidd and Hailwood 1993; Smith 1993). Elsewhere (Miall, this volume) we discuss a recent shift in the emphasis on empirical data synthesis in the definition of chronostratigraphic units, with some workers favouring the use of prominent marker horizons as the basis for definition, even where these require revisions of long-established stage boundaries.

The whole edifice of our geological time construction is in danger of losing its empirical purity if deductive models are brought in to play a major role in the perfection of the time



TEXT-FIGURE 7
Chronostratigraphic correlation of the Castlecliff Formation of the Wanganui Basin, North Island, New Zealand (Kamp and Turner 1990).

scale. These range from the local problems alluded to above, any of which are susceptible to the effects of personal choice and bias, to much larger and potentially more fundamental problems. The dangers and contradictions involved in the use of models based on sequence stratigraphy have now been well aired (we address this above; see also Miall and Miall 2001). Many of the same comments could be applied to the use of "event stratigraphy" as a basis for dating and correlation. While the most famous of geological "events" in the stratigraphic record, the Cretaceous-Tertiary boundary, now appears to be extremely well established worldwide as a universal geological marker (but see the final section of this paper), other events are not so clearly demarcated. Extinction events, major volcanic events, regional storm events and other large scale, sudden processes on Earth are commonly assumed to have left regional or global stratigraphic signatures. While many of these may turn out to be of global significance (e.g., the extinction event marking the end of the Permian), all such events are the product of deductive models that build an hypothesis of extent and significance from an originally small, localized field data base. There exists a strong temptation to look for confirming evidence of preconceived concepts or models, rather than to rigorously test and attempt to falsify preliminary hypotheses. Stratigraphers should beware! These remarks are relevant to the application of the so-called "*hyper-pragmatic*" approach (Castradori, 2002) to the definition of chronostratigraphic boundaries (see Miall, this volume).

The greatest dangers, in our view, now involve the growth of the field of "cyclostratigraphy." House (1985, following a suggestion by G. K. Gilbert in 1895) was amongst the first to propose that the record of orbital frequencies preserved in the rock record could assist in the refinement of a geologic time scale by providing calibration of biostratigraphic data with a 10^4-10^5-year precision. House and Gale (1995, Preface) remarked that "the reality of orbital forcing of climate was established as a fact" in the 1970s. Is this a reasonable statement? It is instructive to trace the development of this idea in the geological literature.

Orbital forcing as a "black box"

Most instruments currently used in geological laboratories require specialized staff dedicated to their use, who fully understand the workings and the limitations of the procedures. Other scientists, however, simply take the numbers generated by these instruments at face value as input into their own research. Those scientists who accept and use these instruments and/or the results they produce as unproblematic premises for their own research, give social support to these practices and thereby, contribute to "transforming them into 'facts of measurement and effect estimation'" or what have been termed *black boxes* (Fuchs 1992, p. 50; Latour and Woolgar 1986; Latour 1987). As Fuchs (1992, p. 49) has observed, "only the support of other scientists can turn statements into facts" As Fuchs (1992, p. 48) has further noted:

To become a fact, a statement must determine the conditions under which other statements made by other people are possible. That is, a scientific statement must be accepted by other scientists as the basis or starting point for their own work.… The more other scientists use a statement as the premise on which to build their own statements, the more they turn that statement into an unproblematic black box and an unquestioned foundation for subsequent scientific work. Scientists do this to the extent to which they are convinced that their own work depends on a statement. As Latour and Woolgar (1986, p. 259) stated: "The activity of creating black boxes, of rendering items of knowledge distinct from the circumstances of their creation, is precisely what occupies scientists the majority of the time."

Elsewhere (Miall and Miall, in prep.), we have suggested that, to earth scientists, the orbital behaviour of the Earth, including its three main variables, eccentricity, precession and obliquity, is a closed black box based on astronomers' proprietary knowledge. For example, de Boer and Smith (1994, p. 3) stated "Theoretical astronomy provides us with predictions of the periodicities at which orbitally forced sedimentation rhythms should occur in the stratigraphic record." Further, we have argued that the orbital control of glaciation during the late Cenozoic has become a black box, based on the classic work of Emiliani (1955) and Hays et al. (1976). House and Gale (1995, Preface) remarked that "the reality of orbital forcing of climate was established as a fact" in the 1970s. This quote would appear to suggest that orbital control of climate, in general, is now a more general black box, with unconditional application to all of geological time. Is this a reasonable statement? How did this come about?

It is instructive to make use of an analytical procedure suggested by Latour and Woolgar (1986, p. 76-80), who developed a classification of scientific statements to explain how tentative concepts turn into unquestioned black boxes. According to this approach, statements can be classified as follows:

Type 1: Conjectures and speculations

Type 2: Claims of relationships or statements of tentative relationships between scientific facts/processes

Type 3: Statements that contain references to prior work in order to support a suggested relationship

Type 4: Textbook-like statements of established facts or relationships

Type 5: Taken-for-granted facts that require no supporting reference

Below, we trace the evolution of the "Milankovitch theory" (Ruddiman 2001, p. 212) as applied to the ancient geological record from a tentative, type-1 conjecture to a type-4 statement of established facts.

The influence of orbital forcing during the more distant geological past had first been proposed during the nineteenth century, but remained speculative in the absence of critical data. By the 1930s, evidence for a major glaciation affecting the southern continents (Gondwana) during the Carboniferous-Permian had been collected and, during this period, sediments containing clear evidence of cyclic sea-level changes—the classic "cyclothems," were described from the mid-continent area of the United States. These were speculatively attributed to glacioeustatic control, in a type-1 statement:

> 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).

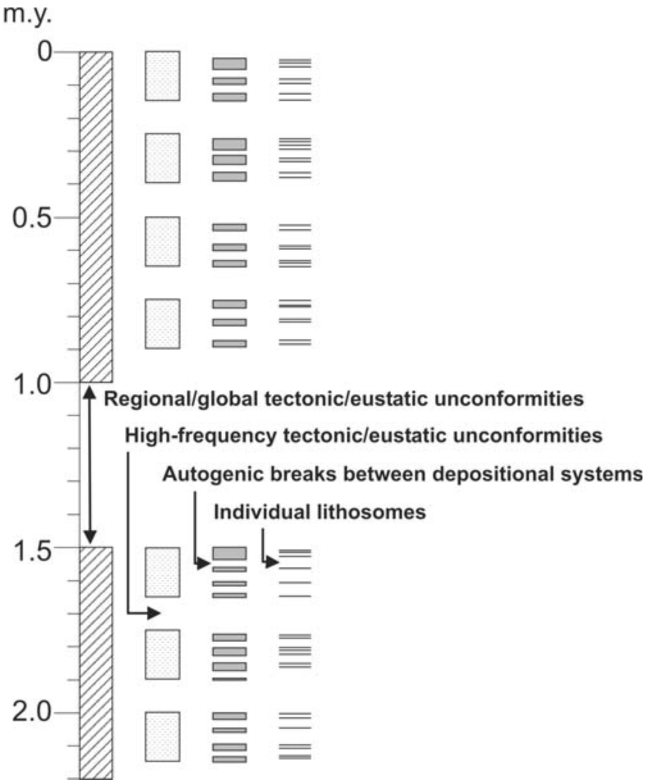
Developments in theoretical chemistry (Urey 1947) suggested that isotopes of given elements would fractionate (evaporate, dissolve, etc.) according to temperature conditions. Emiliani (1955) was the first to explore the application of this idea to geological data. He referred to earlier work on the oxygen isotope composition of modern sea water and continental ice, and measured ratio data on calcium carbonate specimens from selected drill cores from the modern oceans. The cores were divided into a series of stages corresponding to varying temperature values. He was able to show how the fluctuations in the $^{18}O/^{16}O$ ratio through these core stages correspond to cycles of glacial cooling and interglacial warming through the Pleistocene glacial epoch. He referred to the work of Milankovitch and others on orbital cycles, and claimed that "General agreement between ages of insolation minima, and even core stages, supports the conclusion that summer insolation at high northern latitudes and Pleistocene temperatures may be related." This claim of a relationship between experimental observations relating strictly to the Pleistocene ice age is an example of a Type 2 statement of a tentative relationship between scientific facts and/or processes.

Building on Emiliani (1955), Hays et al. (1976) provided a detailed analysis of the succession of oxygen isotope variations in two drill cores in the Southern Ocean, southwest of Australia, that firmly established orbital forcing as the major control on the fluctuation between glacial and interglacial periods over the last 400,000 years. This relationship has been replicated many times since then, and is given as a type-4 statement in all modern textbooks. For example, Ruddiman (2001, p. 212) refers to this as the "Milankovitch theory". Hays et al. (1976) is regarded as the definitive primary reference on this topic.

Hays et al. (1976) carried out a detailed laboratory analysis of the ä^{18}O content of selected foraminifera through two drill cores, also obtained from the Southern Ocean. In their paper great care was taken to set out the possible problems and limitations in the method. Their presentation contains many type-2 and type-3 statements. For example, they cited earlier work on the dating of coral terraces in New Guinea and Hawaii that record climate-related (glacioeustatic) sea-level changes. They attempted to use a "chronology [for the drill-core data] that is completely independent of the astronomical theory" in order to test the presence of the predicted Milankovitch frequencies in the core data (Hays et al. 1976, p. 1127). They made explicit 1) the assumption of continuous sedimentation rate in their sample drill cores and 2) "the assumption that the radiation-climate system is time-invariant and linear." These authors were able to demonstrate that the climatic signal in the rocks is complex. The three major forcing processes (eccentricity, precession, obliquity) go in and out of phase over time, and climate change may not occur immediately in response to changes in the orbital forcing functions. "There, a confusing pattern of leads and lags among all the curves is displayed." The authors state "We regard the results of the time-domain test as strong evidence of orbital control of major Pleistocene climatic changes", but given the less than perfect match between the model and the data, they state that the relationship "is as constant as could be expected from a geological record."

Hays et al. (1976, p. 1131) concluded:

> Having presented evidence that major changes in past climate were associated with variations in the geometry of earth's orbit, we should be able to predict the trend of future climate. Such trends must be qualified in two ways. First, they apply only to the natural components of future climatic trends—and not to such anthropogenic effects as those due to the burning of fossil fuels. Second, they describe only the long-term trends, because they are linked to orbital variations with periods of 20,000 years and longer.



TEXT-FIGURE 8
An illustration of the breakdown of sequences into packages of sediment bounded by hiatuses at increasing levels of detail.

It is interesting, in light of present day concerns about anthropogenic warming, to note that based on available astronomical knowledge about current orbital variations, Hays et al. (1976) predicted a long-term global cooling over the next 20,000 years (Emiliani 1955, p. 571, predicted the commencement of a new ice age in about 10,000 years).

Hays et al. (1976) expressed many cautions and constraints about the data and their results, and they emphasized the complexity of the relationships they obtained, even using excellent data from drill cores through Recent sediments. Given this, it would clearly be a challenge to demonstrate the effects of the Milankovitch processes in the more distant geological past where dating is far more imprecise, and the climatic effects, especially in a non-glacial world, far more subtle. Nonetheless, a major theme in geological research, especially since this ground-breaking paper was published, has been to attempt to do exactly that.

One of the first major conferences to address the issue of orbital forcing in the distant geological past was held at the Lamont-Doherty Geological Observatory (Columbia University) in 1984. In the Introduction to the Proceedings volume the editors state

> Another purpose [of the conference] was to review the geological evidence which now suggest to some investigators that or-

> bitally-driven climatic variations occurred in Pre-Pleistocene times. (Berger et al. 1984, p. xi).

They then summarized the program on this topic and concluded that rhythmically bedded deposits occur in these pre-Pleistocene sequences is obvious from the data presented.

But what of the astronomical control hypothesis?

> Another conference objective was to assess the accuracy of the astronomical part of the theory, particularly with regard to the dependability of calculations of orbital variations made for intervals of time many millions of years in the past. … For older epochs [more than a few million years back] the mathematical machinery employed … is not yet capable of producing reliable values. One approach is to develop a more general theory from which the frequencies and possibly the phases of key orbital variations could be deduced. Another is to use the geological record to obtain estimates of past variation frequencies, and use these estimates to constrain the predictions made by celestial mechanics

The editors are here treating the concept in a series of type-2 statements. Note the direction in which the research is being aimed: to use geological data in an attempt to determine Milankovitch periodicities in the geological past. Notably, Fischer and Schwarzacher (1984, p. 167), in one of the papers presented at the 1984 conference dealing with the distant geological past (in their case, the Cretaceous) stated

> This conference has made it plain that … constancy of orbital periods is not to be taken for granted.

Alfred Fischer became one of the leading researchers in this field. In 1986 he published a major review of "climatic rhythms recorded in strata," one of the stated objectives of which was to assess the evidence for Milankovitch forcing in the geological record. Referring to the nineteenth-century speculation on this topic, he remarked on "Gilbert's assumed constancy of the orbital cycles, which remained to be tested." (Fischer 1986, p. 356). The paper then set out a series of geological examples, in which Fischer claimed to have demonstrated Milankovitch-type periodicity in the sedimentary cycles. However, he repeatedly used qualifying terms, such as "extrapolation", "approximately the same timing," "about 900,000 years," "roughly matched," etc., all of which classify his suggested relationships into type 2 statements.

The term "*Milankovitch frequency band*" is used in this paper and elsewhere to refer to cycles that appear to have periods or frequencies within the approximately 10,000 to 500,000 time range of the modern orbital frequencies. This term, commonly abbreviated to "Milankovitch band", or "Milankovitch cycles," (Fischer 1986, p. 353) has the status of a geological definition which explicitly relates cycles in the geological record to the cyclic mechanism of orbital climate forcing first theorized by Milankovitch. This is comparable to what Latour and Woolgar (1986) and Latour (1987) have termed "modalities". These are statements that modify another statement, such as a qualifying clause. Latour (1987, p. 23) defined a "positive modality" as "a sentence that leads a statement away from its condition of production, making it solid enough to render some other consequences necessary." To be led away from its "condition of production" is to assign that condition the status of a black box. To label a geological arrangement of sediments as Milankovitch cycles is to set aside the kinds of problems and limitations described by Hays et al. (1976). It is essentially to say that if these cycles have frequencies falling within the designated range of durations (10-500 ka), they were caused by orbital forcing. Fischer and Bottjer (1991, caption to Fig. 1), in an introduction to a special issue of *Journal of Sedimentary Research* on "orbital forcing and sedimentary sequences," explicitly stated that "Cyclicity in the Milankovitch band is related to orbital variations." This is clearly a type-4 statement.

Designating cycles as falling within the Milankovitch band turns type 2 statements into type 3 statements. This practice has been roundly criticized by Algeo and Wilkinson (1988). The presence of rhythmicity and an estimation of cycle durations of the appropriate magnitude does not prove orbital forcing as the cycle-generating mechanism. As stated by Algeo and Wilkinson (1988):

> Despite an often-claimed correspondence between cycle and Milankovitch orbital periods, factors independent of orbital modulation that affect cycle thickness and sedimentation rate may be responsible for such coincidence. For example, nearly all common processes of sediment transport and dispersal give rise to ordered depositional lithofacies sequences that span a relatively narrow range of thicknesses ... Further, long-term sediment accumulation rates are generally limited by long-term subsidence rates, which converge to a narrow range of values for very different sedimentary and tectonic environments (Sadler 1981). In essence, the spectra of real-world cycle thicknesses and subsidence rates are relatively limited, and this in turn constrains the range of commonly-determined cycle periods. For many cyclic sequences, calculation of a Milankovitch-range period may be a virtual certainty, regardless of the actual generic mechanism of cycle formation.

Acknowledging these difficulties, Weedon (1993, p. 44) suggested that independent chronostratigraphic dating of cyclic successions provided only "first order approximations" of cycle periods. Fischer and Bottjer (1991, p. 1064) acknowledged such problems as the numerous gaps in the sedimentary record, but then stated:

> On the other side stand pragmatic stratigraphic observations. Various facies of the stratigraphic record are pervaded by repetitive, oscillatory patterns, with timing in the Milankovitch frequency band.

These authors discussed such potential problems as variations in orbital frequencies through geological time and variations in sedimentary accumulation rates, and then stated:

> We make the heuristic assumption that if an observed rhythm has a period in the Milankovitch frequency band, it is probably related to one of the several astronomical cycles with periods in that band.

Similarly, de Boer and Smith (1994, p. 4) noted:

> … time control is limited and depends on interpolations and extrapolations, generally with the assumption that the rate of sedimentation has been constant.

Several paragraphs later, however, these researchers suggested that "another analytical approach is possible in which precise estimates of time are unnecessary." They refer here to the use of spectral analysis to reveal repeated frequencies. With this statement they have shifted the research program to a deductive model, designed to demonstrate predicted frequencies.

In 1995, in a landmark volume establishing new time scales and new methodologies for refining these scales, Herbert et al. (1995, p. 81) introduced "Milankovitch cycles" with reference to the work of Hays et al. (1976) and others, and stated that:

> Stratigraphers may now be in a position to invert the usual argument; the "Milankovitch" hypothesis may be used *in appropriate settings* to improve standard time scales. (italics as in original)

The Milankovitch model is here on the verge of becoming a closed black box (a type-4 statement), but the italics function as a kind of residual caution. However, rather than an attempt to use empirical data to "constrain the predictions made from celestial mechanics", as recommended by Fischer and Schwarzacher (1984), rather than a careful attempt to evaluate possible differences in orbital parameters in the geological past, present work seems designed merely to confirm the reality of the Milankovitch signal by demonstrating orbital parameters similar to those acting at the present day.

In 1998 the Royal Society of London held a symposium on the topic "Astronomical (Milankovitch) calibration of the geological time scale", which was attended by many of the leading researchers in this field. The results were published in the Philosophical Transactions of the Royal Society (Shackleton, et al. 1999; papers in this set are referred to by author below, but are not listed separately in the references at the end of this paper). The symposium led off with an astronomical study of orbital frequencies by Laskar. He concluded that calculation of planetary motions cannot accurately retrodict Earth's orbital behaviour before about 35 Ma, because of the long-term chaotic behaviour of the planets and because of drag effects relating to the dynamics of the Earth's interior. However, he suggested that "The uncertainty of the dissipative effects due to tidal dissipation, core-mantle interactions, and changes in dynamical ellipticity are real, but if the geological data are precise enough, this should not be a real problem for the orbital solution." This amounts to a reversal of the black box: astronomers relying on accurate data from the geologic record in order to improve their astronomical calculations.

However, a review of the remaining papers, constituting the body of the symposium proceedings, does not reassure us that such data are becoming available. Most of the papers consist of detailed studies of cyclic geological data which have been subject to time series analysis, filtering and tuning in order to highlight cycle frequencies. Most of these studies result in the reconstruction of frequencies similar or identical to those known from the present day, despite the warnings of Laskar (1999), or earlier warnings of similar character (e.g., Berger and Loutre 1994) that cycle frequencies could be significantly different in the more distant geological past. Having said this, these frequencies are not reported with any consistency. For example, a "long eccentricity" period is variously reported as having a period of 400 ka (Gale et al.), 404 ka (Olsen et al., Hinnov and Park, Herbert), 406 ka (Shackleton et al.), and 413 ka (Hilgen et al), without reference to any such long-term variation in frequency.

All these individual studies represent analysis of "hanging" or "floating" sections, that is, sections that are not rigorously tied in to any existing cyclostratigraphic stratotype (because these do not exist for pre-Pliocene strata), but are dated according to conventional chronostratigraphic methods, that is, by use of biostratigraphy, with or without independent radiometric or magnetostratigraphic calibration. The problem with this is that even the best such chronostratigraphic calibration is associated with significant error, as much as ±4 Ma in the Jurassic (Weedon et al.). There is, therefore, no method to rigorously constrain tuning exercises. For example, a one-million-year error in age range would encompass 48 potential precessional cycles with a frequency of 21 ka. Calibrations may, therefore, be affected by very large errors. In addition, nearly all the individual studies report variations in sedimentary facies, and actual or suspected missing section, indicating difficulties in arriving at reliable estimates of sedimentation rate for the depth-time transformation. The interpretation of frequencies in many of these papers is constrained by the identification of a cycle "bundling", such as the 1:5 bundling that is said to characterize the combined effect of eccentricity and precessional cycles, but such bundling is rarely precise and, in any case, also ignores the warnings of Laskar and others that cycle periods may have been significantly different in the distant past. Individual cycles, like magnetic reversal events, are difficult to individually characterize. As Murphy and Salvador said of the latter (online version of the International Stratigraphic Guide by Michael A. Murphy and Amos Salvador; www.stratigraphy.org), reversals "have relatively little individuality, one reversal looks like another." Comparisons and correlations between cycle strings are therefore all too easy to accomplish, and there may be little about such successions to indicate the presence of missing section. Torrens (2002, p. 257) referred to what he called the "bar-code effect" of dealing with "basically repetitious, often binary" data of orbital cycles and magnetic reversals:

> The problem is that, if one bar-line is missed or remains unread, the bar-code becomes that, not of the next object, but that of a quite different object. The proximity of the next object, becomes no proximity at all.

We suggest that attempts to develop a time scale with an accuracy and precision in the 10^4-year range by calibrating it against conventional chronostratigraphic dates up to two orders of magnitude less precise represents a fundamentally flawed methodology. The best that can said about the Royal Society symposium is that the results are "permissive"—they point to a possible future potential, but one that is very far from being realized. Nonetheless, the editors of the symposium proceedings, state, in their Preface: "We believe that the calibration of at least the past 100 Ma is feasible over the next few years."

By 2003, the assumption of Milankovitch control was verging on a type-5 condition. Van der Zwan (2002) generated orbital signals from subsurface digital gamma-ray data run on fluvial, deltaic and turbidite sediments from the Niger Delta. These are sedimentary environments characterized by frequent breaks in sedimentation and by very large fluctuations in sedimentation rate as a result of changes in river and tidal energy, storm activity, submarine landslides, etc.—not environments conducive to the preservation of rhythmic regularity generated by an external signal. Nevertheless, his time-series analysis generated a power spectrum and, with the aid of dates provided by biostratigraphy, he converted the cycles to a time frequency, claiming that these demonstrated the periodicity of orbital eccentricity. When questioned about this methodology, Van der Zwan responded (e-mail correspondence, 2003):

> With respect to the key question, the validity of the method, for me the ultimate test is whether it is possible to forward model the stratigraphy of the calibration well correctly using these climatic input signals. In this respect, the fact that Milankovitch cyclicity is recorded in both pollen spectra and gamma ray records convinced me of the presence of such climatic signals in the geological record.

And this:

> An ultimate final control is whether the input parameters used make geological sense.

An analysis of a Paleocene pelagic rhythmic succession in Italy (Poletti et al., 2004) provides a good example of the "inversion" of the Milankovitch argument. These researchers extracted cyclic frequencies from their data by spectral analysis, using a constant sedimentation rate calculated from the age range of the section (using the biostratigraphic time scale of Berggren et al. 1995). They commented:

> The apparent disagreement with eccentricity, obliquity and precession periods does not imply a stochastic distribution. Parts of the succession may actually be missing due to hiatuses not detectable by the traditional biostratigraphy and the calculated sedimentation rate may thus not reflect the real deposition history and account for the apparent disagreement.

Their next step was to adjust the sedimentation rate so that the resultant cyclic frequencies matched present-day orbital frequencies. The ratios of some of the cycle lengths in the adjusted cycle spectrum then matched the ratios between some of the Milankovitch frequencies, although the relative strengths of the cyclic signals varied considerably from those of the present day.

Here is one of the problems with Frodeman's "narrative logic." How do we tell when the logically elegant is, nonetheless, wrong? Experimentation that makes use of numerical models is commonly used in the Earth Sciences to verify or validate a quantitative model. However, Oreskes et al. (1994) have pointed out the logical fallacy involved in "affirming the consequent"—the demonstration by numerical modeling of a predicted relationship. This does not constitute confirmation of the model; it merely indicates a certain probability that one family of solutions to a problem is feasible. Denzin (1970, p. 9) defined the "fallacy of objectivism" as the researcher's belief that if "formulations are theoretically or methodologically sound they must have relevance in the empirical world."

To summarize this section, between the time of the initial, cautious work of Hays et al. (1976) and Berger et al. (1984) and the present day, the "Milankovitch theory" has been inverted. At first, researchers attempted to use geological data to explore the range of cyclic climatic periodicities in the geological past, with a view to testing the evidence for the presence of an orbital-forcing signature. Now, the demonstration of cyclic periods falling somewhere within the "Milankovitch band" is enough for researchers to assert that the controlling mechanism was orbital forcing. Initial cautions based on an understanding of the incompleteness of the geological record have given way to a predisposition to respect the power of time series analysis to generate the expected signals. At least one astronomer has even suggested that geological data may be used to constrain the astronomical calculations (Laskar 1999). For Laskar, geological data had become the black box. Geologists experienced in the incompleteness and inconsistencies of field data and knowledgeable about the warnings associated with the use of time series analysis offered by signal theorists (Rial 1999, 2004), would be very skeptical about this last approach. Rial (1999) warned that "chronologies based on orbital tuning cannot be used because orbital tuning subtly forces the astronomical signal into the data."

Cyclostratigraphic interpretations of time in the sedimentary record.

The tuning of stratigraphic records with the use of preserved orbital signals has become a fruitful area of research (House and Gale 1995; Hinnov, 2000; Weedon, 2003), but much remains to be done to clarify possible changes with time in orbital frequencies, and the broad framework of absolute ages within which refined cyclostratigraphic determinations can be carried out.

Cyclostratigraphic studies depend on a hierarchy of five theoretical assumptions (text-fig. 9):

1. The section is continuous, or

1A. (alternate): Discontinuities in the section can be recognized and accounted for in the subsequent analysis;

2. Sedimentation rate was constant;

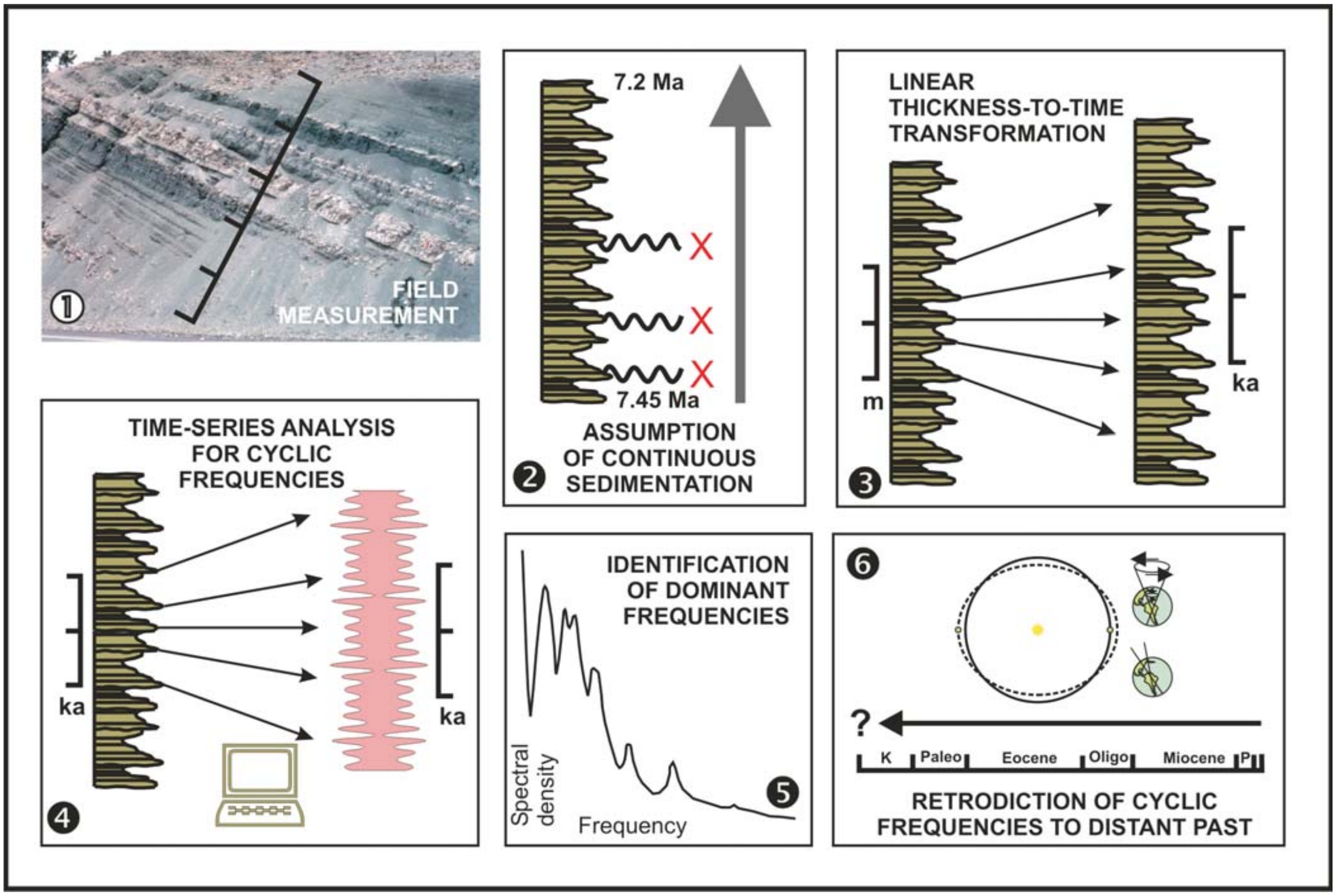
3. Orbital frequencies can be predicted for the distant geological past, based on independent age-bracketing of the section;

4. Thickness can be converted to time using a simple sedimentation-rate transformation;

5. The variabilities in stratigraphic preservation (facies changes, hiatuses) can be effectively managed by pattern-matching techniques.

Do we have reliable tests of any of these assumptions? At present there are still many questions. "Bundling" of three to six cycles into larger groupings has been suggested as one distinctive feature of orbital control, indicating nesting of obliquity or precessional cycles within the longer eccentricity cycles (Fischer 1986; Cotillon 1995), but given the natural variability in the record, and the tendency of geologists to "see" cycles in virtually any data string (Zeller 1964), this approach needs to be used with caution.

Few researchers refer to the possibility of variations in orbital behaviour through geologic time; yet a considerable amount of work has been carried out on this problem by astrophysicists, and some of this is published in mainstream geological literature. For example, Laskar (1999) concluded that calculation of planetary motions cannot accurately retrodict Earth's orbital behaviour before about 35 Ma. Berger and Loutre (1994) suggested that the obliquity and precessional periods have steadily lengthened through geologic time. Taking only their calculations for the Late Cretaceous-Cenozoic, and omitting any consideration of chaotic behaviour, they indicated that the 19,000-year precessional period would have been 18,645 years at 72 Ma, and the 41,000-year obliquity period would have been 39,381 years. What this means is that at 72 Ma, the Earth, over a 10-m.y. time period, would have experienced ten more precessional and obliquity cycles than the present-day periods would have predicted. This represents a very significant difference, and one that cannot be ignored if cyclostratigraphic data are to be used to refine the geologic time scale. Yet no mention of this is made in current literature on this topic. And this may not be all. Murray and Holman (1999) demonstrated that the orbits of the giant outer planets are chaotic on a 10^7-year time scale. The implications of this result for the inner planets, including Earth, have yet to be resolved, but Murray (pers. comm., 2003) suggested that Earth's orbit has undoubtedly been affected by similar gravitational effects on a comparable time scale.



TEXT-FIGURE 9
The construction and analysis of the time proxy. 1) Field measurements of some physical or chemical parameter are made relative to their position in a succession of strata; 2) The age of the succession is determined from tie points at the top and bottom, and continuous sedimentation is assumed; 3) Based on the calculation of an average sedimentation rate thickness or position in the succession may be transformed to time; 4) Field data on the calculated time ordinate are subjected to time-series (Fourier) analysis; 5) Output of the analysis consists of spectral density plots, from which the dominant cyclic wavelengths may be read; 6) Cyclic frequencies are compared to those characterizing the present-day orbital behaviour of Earth in order to compare the present to the past.

So far, a reliable cyclostratigraphic (astrochronologic) time scale is only available for the youngest Cenozoic strata, back to about 5 Ma (Hilgen 1991; Berggren et al. 1995a). The problem, as we perceive it, is that a powerful model may, once again, be used to drive the development of correlations rather than the empirical data being used to test the model. Remarks about orbital control being a demonstrated "fact" tempt geologists to ignore the null hypothesis and to readily assume that cyclic successions were deposited under the influence of orbital forcing. Cyclostratigraphy can only work in continuous sections or where the existence of hiatuses has been carefully evaluated. As this paper has attempted to demonstrate (see also, in particular, Aubry 1991, 1995), evaluation of unconformities is one of the most difficult and most neglected aspects of stratigraphic study.

A good example of the potential pitfalls in an otherwise interesting paper is the study of Miocene pelagic carbonates reported by Cleaveland et al. (2002). The objective of this paper was to use the preserved orbital signature in the rocks to "tune" ages derived from biostratigraphic and radiometric data, based on calibration against a theoretical orbital eccentricity curve. Their data set consisted of a set of $CaCO_3$ values obtained by analysis of a tightly sampled limestone section. Sample position in the section was first converted to age based on an average sedimentation rate derived from the ages obtained from radiometric dates on ashes at the top and bottom of the section. This requires two assumptions: constant sedimentation rate, and absence of hiatuses. This derived data set was then subjected to two separate smoothing procedures to enhance the visibility of the predicted cycle frequency (two more assumptions). The result is twenty cycles, the same number as in a theoretical orbital eccentricity curve for the geologic interval. The match between the massaged data set and the theoretical curve is visually excellent. The results, which now incorporate five assumptions (four from the data and the fifth being the theoretical curve), were then used to adjust the age of an important stage boundary, determined from a biohorizon that occurs within the section.

The radiometric ages on the ashes, 12.86±0.16 Ma and 11.48±0.13 Ma, yield extreme possible age ranges for the studied section of between 1.09 and 1.67 m.y. Using the ages within this range preferred by the authors, the cycle frequencies calculate to about 94 ka. This was equated to a supposed 100 ka eccentricity frequency by Cleaveland et al. (2002), although the present-day observed eccentricity frequencies (House 1993, p. 12) are 54, 106 and 410 ka, not 100 ka, and the theoretical eccentricity curve on which Cleaveland et al. (2002) based their comparison includes a frequency of 95 ka. The range of possibilities for cycle frequency in the Cleaveland et al. (2002) data that are yielded by the error limits of the radiometric ages extends from 73 to 111 ka, which just about encompasses the 106 ka frequency reported by House (1993). This, however, was not the number used by Cleaveland et al. (2002), and their use of a 100-ka value is not explained. The work on stratigraphic incompleteness by Smith (1993) and Aubry (1995) should serve as a warning about the potential for missing cycles and, consequently, errors in correlation and rate calculations in studies of this type.

An example of the use of multiple tie points for age transformation is provided in another interesting study by Roof et al. (1991), which is the first paper in a special issue of *Journal of Sedimentary Research* devoted to "Orbital forcing and sedimentary sequences." A core some 235 m long yielded 23 biostratigraphic control points, in the form of planktonic foraminiferal recoveries. These were converted (transformed) to ages, using the standard geological time scale of Berggren et al (1985). Most ages are expressed in the Roof et al. (1991) paper (their Table 1) to the nearest 10,000 years, although no error ranges are indicated (text-fig. 10A). A simple, linear arithmetic transformation is assumed throughout the rest of the paper, in that all core logs are plotted with time as the ordinate instead of depth, based on the calculated average. The data, once transformed, were taken as an empirical standard against which other measurements were plotted, and were then used in time series analysis to extract orbital frequencies. These authors chose not to tune their data, with the result that spectral analysis generated a wide range of frequencies for different intervals of the core. In only some of the core do these frequencies compare with those suspected to be the result of orbital forcing.

An alternative, hypothetical, age-versus-depth relationship is shown for the same data in text-figure 10B, in which the possibility of discontinuities and variations in sedimentation rate are emphasized. The age-versus-depth plot shows that sedimentation rates between individual control points vary by nearly an order of magnitude, from 15 to 100 m/m.y. (this range could be substantially more if error in age assignment was taken into account). Based on the poor correlation with predicted Milankovitch frequencies, Roof et al. (1991) concluded that shifts in ocean currents and sediment delivery in their sample area (Gulf of Mexico), especially that delivered by the nearby Mississippi River, accounted for much of the variability in sedimentation rate, and that this overprinted climatic effects within much of the core.

There is nothing wrong with the science in these papers in so far as they are reports of specific field case studies. Problems may arise, however, when results of this type are used as confirming evidence for the general model of orbital control of sedimentation and astronomical calibration of the geological time scale. For example, the presentation by Cleaveland et al. (2002) of their correlations is followed by this statement:

> Our astronomical correlation is made based on a combination of pattern matching between the carbonate and eccentricity curves, radiometric age constraints provided by the two volcanic ashes in the section, and results from spectral analysis of the tuned data set. Of other possible correlations consistent with the radiometric age constraints, we found greatest enhancement in the obliquity and precessional bands when the correlation shown in Figure 3 is applied, and so favor this correlation over other possibilities, despite some discrepancies between the amplitudes of corresponding carbonate and eccentricity peaks.

These authors have made use of the qualitative "pattern matching" method, which is easily susceptible to bias and error. However, they did note internal discrepancies and possible alternative correlations. The assumptions of the model, therefore, must be continuously borne in mind.

What is to be done?

As Latour (1987) has pointed out, tentative ideas can become closed "black boxes" by the repetitive piling on of chains of arguments until original doubts and qualifications are set aside or forgotten. Shrader-Frechette (2000, p. 19), for example, has demonstrated similar processes in her study of hydrogeological models of nuclear-waste disposal that have been developed to provide technical support for proposed underground disposal facilities. Such work, according to Shrader-Frechette, has the character of *naïve positivism* wherein practitioners "forget that all science is laden with methodological value judgments – about how to interpret data, how much data is necessary, how many samples are required, which curve best fits the data, and so on." In research where the science is complex, with results dependent on data of varying quality from many sources, or in cases where the technology is not fully developed, the power of the preconceived idea may overwhelm "objectivity", and the impact of social influences become more clearly evident. The impact of social influence, however, cannot be separated from the scientific practices it generates and is ultimately influenced by.

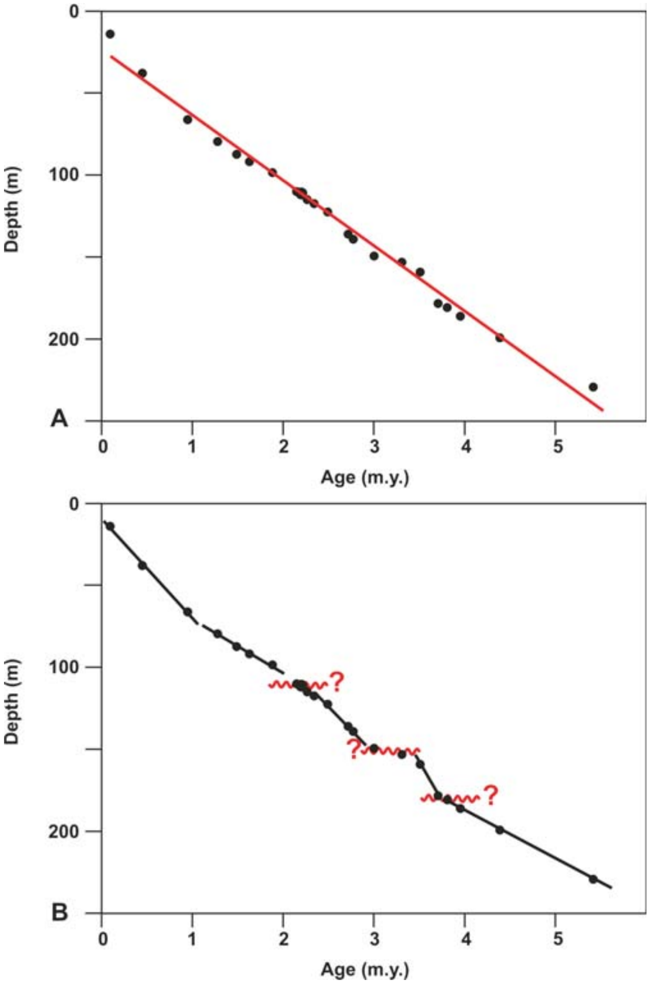
Torrens (2002), in a wide-ranging discussion on the topic of "stratigraphic precision" noted the difficulties and controversies that have arisen over the study of the K-T boundary. At many locations, physical lithostratigraphic, biostratigraphic and geochemical (iridium anomaly) data suggest different local scenarios for the terminal Cretaceous event, and in some cases, the more detailed the study, the more incomplete the record appears and the more anomalies are encountered. The point here is that even given a clear problem such as the description and interpretation of the K-T boundary event, characterized as it is by a highly distinctive geochemical signature, the incompleteness of the record combines with the need to generate hypotheses from the incomplete data to create scientific uncertainty, from which the social influences described in this paper inevitably arise.

Stratigraphic studies that incorporate too many theoretical assumptions should not be used in the work to refine the geologic time scale. Otherwise assumptions build on assumptions, and the "results" that emerge are likely to demonstrate whatever preconception the researcher started with. The use of statistical techniques is popular amongst many workers, probably because of the implied rigor. However, statistics are a "black box" to many earth scientists, and many of the popular methods permit ad-hoc decisions about "constants" and "filters" or depend on assumptions that may be seriously questioned. *Spectral analysis*, which is now a popular technique used to extract cyclic frequencies from digital data sets, is a good example. As noted above, its use requires a major starting assumption that stratigraphic successions are continuous and that thickness can be simply transformed into time. The "results" may then be smoothed and filtered in several successive steps, so that it is not altogether surprising that frequencies coincidentally matching those of orbital processes emerge at the end of the "analysis." In fact, specialists such as G. Weedon (2003, p. 209) refer to tuning of older (pre-Pliocene) sections as being "conducted using the orbital solution as a tuning target."

Independent tests of such relationships are extremely difficult to devise. Tipper (1993, 1994) suggested several tests to be used in an examination of Vail's cycle charts. He suggested (Tipper 1993, p. 380) that the Exxon chart represents a "normal scientific hypothesis" that deserves to be tested as an entity. Elsewhere (Tipper 1993, p. 380) he suggested that the "events" in the chart are just like any other stratigraphic events, and that our complaint that none of the events in the chart has received independent global confirmation (Miall and Miall, 2001) "is also true for all the other stratigraphic correlation tools that are now generally accepted, from biostratigraphic zonation through to isotope stratigraphy and magnetostratigraphy." He further suggested (Tipper 1993, p. 384) that "any version of the chart will nevertheless be acceptable for stratigraphic correlation if it is believed to be at least as good as any other correlation tool." These arguments are presented as a basis for a design of various statistical tests of the validity of the chart. We regard it as problematic to treat the chart as a simple homogeneous entity. As we have argued elsewhere (Miall and Miall 2001) it represents at least three superimposed hypotheses, none of which has adequate empirical support. The same kinds of arguments could be made with regard to cyclostratigraphic correlations.

An even more dubious proposal is that made by Woronow et al. (2002) to use a procedure based on *Bayesian Belief Networks*. These workers categorize those using traditional methods as "frequentists," and their approach as one requiring the assigning of probabilities to events to be based on "witnessing a large number of exact repeats of an event." Woronow et al. (2002) proposed an alternative "Bayesian" approach to the development of statistical relationships, in which "personal beliefs", "subjective probability" or "expert opinion" may all be used to weight predictions and perform sensitivity analyses. Their proposal was directed at "geoscience systems", including the risk assessment of petroleum prospects. We suggest that, if applied to the kinds of geologically incomplete data sets discussed in this paper, no procedure could be more guaranteed to mask problems and perpetuate error.

Descriptions of the development of the cyclostratigraphic time scale, such as that by Berggren et al. (1995a, p. 1273-1274) make it clear that the developers of the scale are well aware of the problems discussed in this paper. The dangers are best minimized if the time scale is developed gradually backwards in time using multiple data sources so as to reduce the likelihood of skipping specific orbital cycles. While much excellent work on orbital forcing of sedimentation is being carried out on older strata, such as the studies of the Cretaceous of the Western Interior by Sageman et al. (1997), the use of temporally isolated data-sets of this type to construct a time scale is fraught with problems, because of the issues raised above. An attempt along these lines was made by Heckel (1986), who confidently identified orbital frequencies from a synthesis of the Pennsylvanian cyclothems in the US Midcontinent, but was roundly criticized for doing so by Klein (1990), based on the wide margins of error that should have been taken into account in the age range of the Pennsylvanian. Nevertheless, most of the more recent research discussed in this article, including the 1998 Royal Society of London symposium, have attempted exactly the same kind of analysis, that is, the identification of specific orbital frequencies in "hanging" or "floating" sections in which the age constraints have been provided by conventional chronostratigraphy.



TEXT-FIGURE 10
A. Age control for a drill core. Raw data from Roof et al. (1991); linear line of correlation (depth-to-age-relationship) inserted by eye. B. An alternative (hypothetical) interpretation of the same data set, emphasizing the possibility of variations in sedimentation rate and discontinuities in the section.

Hinnov (2000), in a lengthy review article and Weedon (2003) in a book on time series analysis, both acknowledged the difficulties discussed here. For example, Hinnov (2000, p. 422) stated "because the accuracy of the orbital theory does not permit direct calibration between pre-Cenozoic stratigraphy and orbital signals, interpretation of these older sequences is restricted to general statistical comparisons between data and orbital theory." Hinnov (2000, p. 425), like Rial (1999, 2004, p. 425) commented on the "suspect" nature of tuned stratigraphy because "power can be shifted from nearby uncorrelated frequencies into orbital frequencies." Hinnov (2000, p. 422) and Weedon (2003, p. 31) both discussed the difficulties of the thickness-time transformation which is, of course, the fundamental basis for cyclostratigraphy. Weedon (2003, p. 32) warned about the difficulties in generating meaningful time series from successions that contain "event beds" such as turbidites or tempestites, because the spacing and thickness of such beds are largely to entirely independent of orbital control and introduce non-systematic variations in sedimentation rate.

There is no place for superimposed, inadequately grounded theoretical assumptions in the construction of the geologic time scale. The hermeneutic circle is, after all, a circle. We need to be as adept at climbing the upward-directed arrow of theory based on rigorous observation as we are skilled at avoiding the downward slide of making our observations fit our deductions.

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