BRIEF BOOK REVIEWS

MESOZOIC AND CENOZOIC SEQUENCE STRATIGRAPHY OF EUROPEAN BASINS, edited by P.-C. de Graciansky, J. Hardenbol, T. Jacquin, and P.R. Vail, 1998. SEPM Special Publication 60, 786 pp., hardcover $175.

Sequence Stratigraphy: The Next Generation

When Vail and his coworkers first proposed seismic stratigraphic models for the passive margins of the Atlantic Coast in the late 1970s (published by Vail et al., 1977, *AAPG Memoir* **26**:49-212), their claims were relatively modest. They argued that large-scale unconformities bounded packages of sediment that they called sequences (forever pre-empting its use in geology in its original meaning), and that these sequences were controlled by eustatic changes of sea level. Their most contentious proposal was the "Vail sea-level curve," with its peculiar saw-toothed pattern that suggested instantaneous regressions and slow transgressions (since rethought, and now called an "onlap-offlap curve"). Their first papers were controversial, but criticism was muted because their data were buried in proprietary Exxon files, and few scientists could determine how they had made their interpretations, or how to test them.

In the 1980s and early 1990s, the claims of sequence stratigraphers grew broader (see papers in Wilgus et al., 1988, *SEPM Special Publication* **42**; Posamentier et al., 1993, *Internat. Assoc. Sedim. Spec. Publ.* **18**; Weimer and Posamentier, 1993, *AAPG Memoir* **58**; Loucks and Sarg, 1993, *AAPG Memoir* **57**). Many of the early tenets of sequence stratigraphy were applied not only to passive margin marine sediments (where there is a plausible link to eustasy), but even to terrestrial strata that were far from sea level. How such stratigraphic packages could be interpreted as having global eustatic control was never fully explained, but geologists applied sequence stratigraphic principles nonetheless. Soon any set of layered rocks could be viewed with "sequence-colored glasses," without worrying whether there was a good theoretical explanation for this interpretation, or what the concept of a sequence actually *means*. Some of the problems of the early "Vail sea-level curve" were ironed out and resulted in the sequence chart of Haq et al. (1987, *Science*, **235**: 1156-1167; *SEPM Spec. Publ.* **42**: 71-108, 1988). This time scale and "sea-level curve" has been widely used and cited ever since, even though it was based on a number of problematic biostratigraphic data which immediately invalidated it (Gradstein et al., 1989, *Science*, **241**: 599-601).

Throughout the accelerating momentum of sequence stratigraphy, a small but persistent minority of geologists (mostly outside the oil industry) continued to point out fundamental flaws in some of the assumptions of sequence stratigraphy (see papers discussed by Hallam, 1992, *Phanerozoic Sea-Level Changes*, Columbia University Press; Prothero and Schwab, 1996, *Sedimentary Geology*, W.H. Freeman; and especially the detailed analysis by Miall, 1997, *The Geology of Stratigraphic Sequences*, Springer-Verlag). Because these critiques were published in widely read and available journals and books, it would seem that the sequence stratigraphers would answer their critics, and strengthen their discipline by learning from their mistakes. Instead, Miall and Miall (2000, *Earth Science Reviews*, **54**:321-348) argue that sequence stratigraphic literature has become essentially a cult belief, a self-contained paradigm with unquestioned, unshakeable assumptions. They show that sequence stratigraphy has become completely self-referential, unwilling and unable to listen to criticism. Miall and Miall (2000) provide a detailed analysis of the recent literature, and indeed it is striking that virtually none of the recent sequence-stratigraphy papers cite anything but other sympathetic papers in the same camp, and never even bother to answer the criticisms that have been leveled. I found the same to be true in my own experience. During the publication of a recent paper pointing out serious problems with sequence stratigraphic correlations tested by high-resolution magnetobiostratigraphy (Prothero, 2001, *Jour. Sedim. Res. B*, **71**:526-536), sequence stratigraphers simply refused to review or even read the paper, let alone constructively criticize it, and I've heard no comment or rebuttal from them even though it was published months ago.

So where is Sequence Stratigraphy: The Next Generation, going? A striking example of the future of sequence stratigraphy is *Mesozoic and Cenozoic Sequence Stratigraphy of European Basins*, edited by P.-C. de Graciansky, J. Hardenbol, T. Jacquin, and P.R.

Vail. As Miall and Miall (2000, Table 4) demonstrate, virtually every paper in this volume cites only papers of sympathetic authors, and rarely or never cite critical papers. Hardenbol et al. (1998, *SEPM Spec. Publ.* **60**:3-13) provided an updated correlation chart that supersedes the grossly outdated chart of Haq et al. (1987, 1988), yet few of the authors in this volume used the more recent chart; 34 out of 45 cite the outdated Haq et al. (1987, 1988) chart. Most of the papers continue to assert the fundamental assumption of sequence stratigraphy: that sequence boundaries are based on global sea-level changes, and that sequences can be reliably correlated to charts like those of Haq et al. (1987, 1988) or Hardenbol et al. (1998). But as pointed out by many authors (e.g., Aubry, 1991, *Jour. Geophys. Res*, **96B**: 6641-6679; summarized in Miall, 1997), the sequence chart is so full of events (119 sequence boundaries in Haq et al., 1987, 1988; 221 in the Hardenbol et al., 1998, chart) that virtually any stratal package with unconformities can be matched to it. As Miall and Miall (2000) pointed out, the global synthesis charts of Hardenbol et al. (1998) continue to add more and more sequence boundaries based on local unconformities, even when the papers in SEPM Volume 60 don't even show those same unconformities in other basins. In other cases, they cite examples (e.g., Deramond et al., 1993, *Geol. Soc. London Spec. Publ.* **71**:193-219) where the correlations were contorted to fit the sequence chart, and when they didn't, circular reasoning and special pleading were invoked to dismiss the mismatch. Clearly, it is unthinkable to question the fundamental assumptions of the cycle chart. Without high-resolution biostratigraphy to test such correlations, it is impossible to tell if these correlations are valid or simply random matching of a busy pattern (e.g., Miall, *Geology* **20**, 787-790). As shown when high-resolution magnetobiostratigraphic tests have been undertaken, such sequence stratigraphic correlations failed miserably, or were misleading at best (Aubry, 1991; Prothero, 2001).

The one potentially independent test of the entire eustatic hypothesis would be the global oxygen isotope curve. Such a correlation was rejected in the analysis of Williams (1988, *SEPM Spec. Publ.* **42**:31-37). In SEPM Volume 60, Abreu et al. (1998, *SEPM Spec. Publ.* **60**: 75-80) make a superficial comparison between the newest version of the oxygen isotope curve and the outdated Haq et al. (1987) curve, but there is no rigorous statistical analysis or curve-fitting exercise to establish whether the similarities are non-random, and whether the mismatches can be overlooked. Even more troubling is their suggestion that the rapid eustatic fluctuations in the curve require significant ice volume in the Cretaceous and Cenozoic, an assertion that is not supported by any geologic evidence, and contradicted by the abundant evidence of a greenhouse climate that would prohibit significant ice caps (Eyles, 1993, *Earth Sciences Reviews*, **35**:1-248). However, such hypotheses are required if the sequence stratigrapher believes in all of the "events" on the cycle chart, and believes that they are eustatically controlled, because no other mechanism has been shown to change sea level that quickly and dramatically. The alternative would be to accept that many of the events on the cycle chart are not real, or at least not eustatically controlled, but artifacts of compiling many local sea-level records into a synthetic chart.

So what is the future of sequence stratigraphy? Miall and Miall (2000) argue that it is a "revolution in trouble," and that the field has become almost cult-like in its strict adherence to dogma, and unwillingness to learn from criticism, or to grow and change, seek new directions and incorporate new ideas. Certainly, it seems that the podia of national meetings such as the Geological Society of America were completely dominated by sequence stratigraphic talks only a few years ago. Yet at the 2001 GSA meeting in Boston, there were relatively few of them. Has the bandwagon run its course? Have the limits of the predictive power of sequence stratigraphy been reached? Will its practitioners continue to do the same old thing year after year, or will they find new directions to apply their ideas, and change as the evidence indicates they must? Only time will tell.

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