PERSPECTIVES

IN DEFENSE OF FACIES CLASSIFICATIONS AND MODELS

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‘‘Classifications are theories about the basis of natural order, not dull catalogues compiled only to avoid chaos’’ (Stephen Jay Gould, 1989).

INTRODUCTION

In a descriptive science like geology, there will always be a need for classifications. These serve two purposes: they attempt to create order out of apparent chaos, as aids to memory, and they are an attempt to understand genesis. The best classifications are those that highlight differences in origins of geological features, and are therefore particularly useful for purposes of prediction and extrapolation, for example in geological exploration.

There will always be a creative tension between those who focus on the minute differences amongst geological attributes and those who like to simplify and generalize. This is the tension between those, the purists, who seek constantly to perfect our understanding of descriptive detail and of processes, and those, the synthesizers, who think they know enough details for the time being, and want to move onto larger problems. It is the same as the debate amongst taxonomists between the ‘‘lumpers’’ and the ‘‘splitters’’. Facies classification and facies models offer plenty of fuel for debate between the advocates of both sides of this argument. Anderton (1985) provided an excellent discussion of the various levels of complexity that are embraced by the modeling process.

The purists like to point to classifications that have failed, as proof of the failure of the idea of classification. For example, the geochemical classification of igneous rocks proposed by Shand (1947) was cited by Bridge (1993) and M.R. Leeder (personal communication 1997) as an example of a complex system that did not catch on and was not used, no doubt because it was not based on any understanding of genetic reality that withstood the test of time. By contrast, Kay’s (1951) classification of geosynclines was widely used until the 1960s, at which time it virtually vanished, to be replaced by classifications based on plate tectonics. Ingersoll and Busby (1995) now propose 27 varieties of sedimentary basin, based mainly on plate-tectonic principles—more types than there were geosynclines. Is this being objected to? No, in fact the contrary (e.g., see Dickinson 1997), because it is now recognized that, at least according to current plate-tectonic concepts, this is a genetically based classification and therefore has deep meaning, much like a phylogenetic classification of organisms. Allen’s (1963) Greek-letter classification of cross-bedding is another example of a classification that was used for a time and then was gradually abandoned, probably because of its unnecessary complexity, and because the categories did not obviously reflect the improved understanding of bedform genesis that emerged during the 1960s. The only remnant of this classification to persist into occasional modern usage is his term ‘‘epsilon cross-bedding’’ for the low-angle heterolithic cross-bedding that we now recognize to be the product of lateral accretion on channel meander bends. Replacement and abandonment is always likely to be the fate of classifications. But does this mean that we should avoid the attempt to classify? I do not think so. At the very least, classifications represent way stations on the road to perfect understanding. At best, they offer a common language for description and interpretation.

A few years ago Walker (1990) found it necessary to discuss developments in facies studies, including the emergence of ‘‘architectural-element analysis’’ and the new genetic stratigraphies, such as sequence stratigraphy. As he stated then, ‘‘many basic ideas have been recycled over the years.’’ Recent developments in clastic studies suggest that a restatement of some of these ideas is necessary yet again. This note refers mainly to work in fluvial and submarine fan deposits.

FLUVIAL LITHOFACIES AND FACIES MODELS

A need for a classification of fluvial lithofacies began to emerge in the 1970s, when numerous descriptions of modern and ancient deposits were accumulating in the literature. However, most were described using unique systems of nomenclature, rendering comparative studies almost impossible. Reinterpretation of the descriptions in terms of a standard classification revealed a number of common themes in terms of lithofacies types, facies assemblages, and vertical profiles (Miall 1977, 1978; Rust 1978), and this began the work of recognizing and systematizing the variety of processes in fluvial systems that is still continuing (Miall 1996). The classification of fluvial sandstone facies is based in part on fluvial hydraulics. It has been demonstrated that common bedforms are predictable in form and are generated under predictable conditions (Ashley 1990). This justifies the erection of a simple classification of those lithofacies that represent the preserved deposits of these bedforms. Revisions and improvements to this classification have not ceased; others have developed their own, and in fact the system has been adopted and adapted several times by others without reference to the original work, indicating that the ideas have, to a considerable extent, developed a life of their own. The use of predetermined classifications in clastic sedimentology was debated at length by Bridge (1993, 1995) and Miall (1995) with regard to the specific application of classification techniques to fluvial deposits.

Turning to fluvial facies models: the explosion of descriptive work in fluvial sedimentology has led to the discovery of numerous variants on the original simple suite of models (the braided and meandering models of Walker 1976b), and this has stimulated a variety of attacks on the concept of the model itself, rather than a recognition that the models were intended only as mental concepts, as teaching aids, and as temporary fixed points to aid in the comprehension of nature. The proliferation of facies studies led Dott and Bourgeois (1983) to remark that by the early 1980s fluvial facies models had ‘‘multiplied like rabbits so that every real-world example now seems to require a new model. Such proliferation defeats the whole purpose of the conceptual model by encouraging excessive pigeonholing, which obscures rather than reveals whatever unity may exist among the variants’’. This had, indeed, become a genuine problem with fluvial studies.

A possible solution to the problem of fluvial facies models emerged when detailed work on large outcrops of fluvial systems by workers such as Allen (1983) revealed some common themes amongst fluvial deposits of all types, such as the occurrence of large bar forms developed by lateral accretion in virtually all fluvial settings. Coupled with new concepts about natural hierarchies of clastic depositional units and their bounding surfaces, which were first expressed most clearly for eolian systems by Brookfield (1977), this led to a new ‘‘architectural-element’’ approach to the description and classification of complex clastic systems (Allen 1983; Miall 1985). This work had, as its basis, the idea that the different types of depositional units or architectural elements within various fluvial systems retain a degree of similarity (lithofacies assemblage and vertical profile, internal and ex-

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ternal organization and geometry) regardless of the fluvial style of the river system in which they occur. The proliferation of fluvial styles to which Dott and Bourgeois (1983) correctly refer becomes less of a problem using the architectural-element methodology. Instead of attempting to select the appropriate model from a large range of styles to ''fit'' or ''match'' a limited data base, the fluvial style can be built empirically from the evidence of the assemblage of architectural elements and their mutual relations (e.g., Cowan 1991; Bromley 1991; Miall 1994). Walker (1976a) referred to this type of empirical, observationally based exercise as developing a ''summary of the environment'', but such summaries have typically been built with an existing facies model in mind, which brings us, full circle, back to the problem of the proliferation of facies models. The concept of the facies model, as originally set out by Walker (1976a), has not changed with the architectural-element approach. It has simply been elaborated to focus on deposits at the ''element'' scale, and to make use of two- and three-dimensional data and a much larger data base than was available to sedimentologists in the 1970s.

SUBMARINE FAN LITHOFACIES AND FACIES MODELS

The evolution of concepts about deep-marine depositional systems shows many parallels. The simple, all-purpose submarine fan model of Mutti and Ricci Lucchi (1972) and Walker (1978) served the geological community well for about a decade, but is now all but obsolete. The wealth of marine geological data now available has demonstrated an enormous ranges in size, shape, physiography, and facies composition of modern fans, enough to render such a model virtually meaningless except as an introductory teaching aid. Also called into question are some of the terms used to describe submarine fans. Shanmugam and Moiola (1988), in particular, in a very useful review, demonstrated the inconsistencies and contradictions that have crept into the literature in the use of such terms as upper, middle, and lower for fan subdivisions, the many varied ways in which the term ''lobe'' has been used, and the confusion that has arisen over the concepts of transport ''efficiency''. All such terms should probably be abandoned, although Shanmugam and Moiola (1988) attempted to arrive at compromise definitions of the three fan subdivisions which, in my view, are not very successful, consistent, or useful.

We also now realize that some of the basic concepts on which early fan models were based are flawed, such as the idea that fan lobes develop by progradation and that they always develop coarsening- and/or thickening-upward sequences. These ideas had been central to the vertical-profile model of Walker (1978). Vertical aggradation may, in fact, be the dominant process, especially where the fan is banked against a fault-bounded basin margin. Vertical trends in grain size and bed thickness then may not develop. Recent work on the Amazon fan showed that channel avulsion is the major process that determines stratigraphic architecture within this giant fan. Cyclicity, of the type predicted by the Walker model, is absent (Hiscott et al. 1997). Anderton (1995) expressed skepticism about the reality of facies cyclicity in turbidite systems in general, and the processes that have been interpreted to be the cause of such cyclicity. He made a convincing case for the predominance of random processes in fan deposition.

Ancient fan deposits have for many years been assigned to fan subenvironments using the facies associations A to G defined by Mutti and Ricci Lucchi (1972) and the interpreted relationships between these facies associations and depositional environments. But Shanmugam and Moiola (1988; and in Bouma et al. 1985) showed that the distribution of these associations is not as simple as had long been assumed. All occur in more than one subenvironment, and facies F and G can occur anywhere on a fan. As descriptions of facies associations the seven assemblages are still useful; each can still be interpreted in terms of a limited range of depositional processes. However, it is now clear that recognition and mapping of the assemblages is, by itself, inadequate as a basis for developing a facies model for a fan deposit.

Recent syntheses of submarine fan sedimentology emphasize the multiple controls on fan character. The primary control is that of tectonic setting (Shanmugam and Moiola 1988). The main distinction is between the large, mud-rich fans of mature, divergent plate margins and the small, sand-rich fans of convergent margins. Classifications of fans based on shape (radial versus elongate; Stow 1985) are not very discriminating, because shape is a dependent variable, reflecting the tectonic setting of the basin. An attempt to incorporate new data regarding the complexity of sediment sources into fan classifications, and to discriminate fans according to grain-size variability, led Reading and Richards (1994) to propose a new and much more elaborate classification. Mutti and Normark (1987) took a different approach, similar to the architectural methods suggested for fluvial systems. They erected a hierarchy of depositional scales, and discussed some of the major lithofacies assemblages or ''elements'' that constitute turbidite systems. There are, in fact, many similarities between fluvial systems and coarse-grained, channelized turbidite systems (Miall 1989).

The work of Mutti and Normark (1987) and of Reading and Richards (1994) represent contrasting (but complementary) attempts to incorporate new data into submarine-fan classification, focusing primarily on the architectural scale of fan components and on the range of fan sizes and compositions, respectively. The principles of simplification and synthesis that underlie the concept of the facies model still apply, but at a more complex level that (hopefully) much better reflects the reality of the real world of submarine fans, as revealed by modern marine data. Again, the concept of the facies model, as set out by Walker (1976a), survives by the incorporation of new data, and each fixed point in the classifications still fulfills the four principles of the facies model; each serves as a norm, a framework and guide for interpretation, and a predictor for new geological situations.

This is not the place to enter the debate about the designation of deep-marine depositional systems as ''lowstand wedges'' and ''slope fans''.

A major debate on the validity and usefulness of facies classifications and facies models was recently launched by Shanmugam (1997), with a particular focus on the Bouma model and its five ''divisions''. The Bouma model has long been invoked as the classic example of the application of facies-modeling methodology. For example, Walker (1976a) used this model to illustrate his original formulation of the four principles of the facies model. More recently Miall (1995, p. 379) asked, ''... who would now object to the use of Bouma's (1962) five divisions (A–E) as a framework for the field description of turbidites?'' Shanmugam (1997) does so object, and his discussion of the history and usage of the Bouma sequence concept and model, and the development of what he terms the ''turbidite mind-set'' shows why this is so. He has demonstrated rather thoroughly the misuse of the original model by subsequent workers. Bridge (1995) also touched on some of these same points about turbidites in criticizing Miall's (1995) reference to the Bouma sequence model as a success story to be emulated. The misuse of the turbidite model by workers on deep-marine sandstones had even led to the description of something called the ''non-turbulent turbidity current'', which Shanmugam called the ''ultimate example of an oxymoron''.

What has happened here, and is there a systemic problem with certain sedimentological methods? I do not think so. Shanmugam's critique of the Bouma model does not mean we should abandon the model, only use it more critically. Shanmugam's work is, in fact, an application of Walker's method whereby we use a model as a guide to future observations in order to determine whether or not they are in accordance with existing concepts. He also refers to Anderton's (1985) critique of modeling techniques and methods. But it is precisely because we have in our minds a good concept of what a ''true'' Bouma turbidite should look like, that we can readily appreciate how far off the track many sedimentological descriptions and interpretations have strayed, when someone like Shanmugam comes along and brings us up short with different observations. It turns out that many deep-marine sands may not be turbidites at all. But this does not prove that

Bouma was wrong or that the concept of the facies model is wrong, only that these elegant concepts may have been applied too carelessly.

THE PRACTICAL UTILITY OF QUALITATIVE MODELS

Classification and simplification have been popular with working geologists and students. The Geological Association of Canada ''Facies Models'' volume, now in its third edition (Walker and James 1992), has sold nearly 70,000 copies, more than any of the ''blockbusters'' published by the AAPG, and surely one of the best sellers of all time in the technical geological field. This book is full of classifications and simple models, including the fluvial work referenced above, Walker's syntheses of shelf and turbidite systems, and James' elegant diagrams dissecting the subenvironments of reefs and their growth stages (does this constitute an identifiable ''Canadian school'' of sedimentology?). This work is popular because it is useful in teaching and preliminary interpretation, but nobody ever claimed that the book represents the last word in description and interpretation, and it should certainly not be used as the final authority for research purposes. In the most recent edition Walker's (1978) elegant submarine fan model, which was the focus of the turbidite chapter in the first two editions of this book, has been replaced by diagrams showing a range of fan styles, based on the results of modern marine geological research, and the fluvial chapter has evolved from the braided-meandering dichotomy to a discussion of architectural elements and multiple fluvial styles.

One of the most useful applications of facies classifications and facies models has been for the rapid logging and interpretation of subsurface data, particularly in the early stages of basin exploration. The use of these data for core analysis has a long history (e.g., Shawa 1974), and many textbooks and courses have been built on the reconstruction of facies models from vertical profiles—the one-dimensional samples through reservoir units that are normally all that are available to the petroleum geologist (Visher 1965; Fisher 1969; Fisher and Brown 1972; Pirson 1977; Berg 1986). Attempts to use the new three-dimensional architectural concepts for mapping fluvial deposits in the subsurface have met with mixed success (e.g., Doyle and Sweet 1995), because of the difficulty of mapping and correlating three-dimensionally complex sand bodies with limited subsurface data, but at least the knowledge of the three-dimensional complexity of these deposits is now informing the interpretations that are being made of subsurface data. The development of quantitative models for petroleum production purposes from the descriptive bases discussed here is an important topic that is beyond the scope of this discussion. Many production models are now built on numerical simulations of channel architectures, such as those generated by Bridge and Mackey (1993a, 1993b).

Warnings about the dangers of oversimplification in the use of classifications and models are common in the literature describing them (e.g., Miall 1980, 1985; Bridge 1993, 1995, in the case of fluvial deposits), but this does not prevent such misuse, or misunderstanding of their purpose. For example, Neil Wells, reviewing Miall (1996) in Geotimes (February, 1988, p. 32) suggested that the ''codification of facies and architectural elements [in this book] risks promoting rigid classification, superficial observation, and simplistic interpretation.'' Donn Gorsline (personal communication, 1984) told me the story of a graduate student of his who came to him deeply upset one day and about to quit the program, because he could not make his rocks fit any of the standard models, and felt that he had failed as a sedimentologist. Donn had to gently point out to him the purpose of research, and to suggest that perhaps the student had now in fact found the perfect thesis topic.

Sequence stratigraphy, both as a body of concepts for the teaching of stratigraphy, and as a research tool, has also been heavily dependent on models, especially those in the by-now virtually classic papers of Posamentier et al. (1988), Posamentier and Vail (1988), and Van Wagoner et al. (1990). Yet this area, too, has been bedeviled by the misunderstanding and misuse of models, to the extent that the original proponents of the models have had to become apologists for them, pointing out in such overview papers as Posamentier and James (1993) and Weimer and Posamentier (1993) that the models were never intended to be of universal application (we could argue about the details).

There is no question but that classifications and models in sedimentology have been used in too facile a manner as research tools by many workers, but this does not mean that the approach is wrong, only that all due caution is required in the application of the models. Classifications and models that genuinely reflect common themes in the composition, structure, and genesis of sedimentary units are likely to survive, thrive, and be used, possibly with numerous revisions and accretions. Those that do not will be ignored. Such is the marketplace of ideas that constitutes modern science.

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