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PERSPECTIVE

Is there something exciting in today's sedimentology?

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I present here some personal, and obviously biased thoughts and memories on the development of sedimentology during the last decades. My "tale" is limited to clastic sedimentology, the field of my personal experience, and is by no means exhaustive. I hope that other colleagues whose professional life has been more or less parallel to mine will do the same concerning, let's say, carbonate deposits or provenance studies.

Sedimentology today is no doubt a mature branch of Earth sciences: it includes several aspects and specializations, and is linked to other sectors, especially stratigraphy, basin analysis, petrography, paleontology and geomorphology.

At its birth (during the period 1930s-1950s), sedimentology mainly consisted of textural and compositional analysis of sand, sandstone and carbonates, under the stimulus of the oil industry and the purpose of correlation between drilling wells. Heavy minerals, for example, enjoyed a period of popularity, like grain size and shape. Field work and observation of macroscopic features like sedimentary structures were done by stratigraphers and general geologists.

A turning point was the publication, in 1950, of the famous paper by Kuenen and Migliorini on turbidity currents. It produced a veritable "tsunami wave" of new studies carried out for the most part in the field (under the influence of the geologist Migliorini) but also in the laboratory (flumes) and in theory of mechanical transport and deposition (under the influence of Kuenen, a genial practitioner of hydraulics). Gerard Middleton became the main exponent of sediment mechanics after Kuenen, and very few people, like the unforgettable Stan Dzulynski, worked both in the field and in the lab.

The idea of Migliorini and Kuenen represented a radical change of perspective in the study of sediments (an intellectual revolution? see later for my restraint in using this word) because it implied that sand-usually considered a continental and littoral - shallow marine deposit - could reach well below wave base, as far as abyssal depths in the oceans. As for all new and unexpected ideas, there was initially some skepticism and also a fierce opposition by some French geologists (Philippe Mangin, Marguérite Rech-Frollo and others). On the other hand, new generations of geologists coming out of universities in the 1950s and early 1960s (including myself, along with Dutch, English, Polish and Italian colleagues) supported more or less enthusiastically the new "paradigm" and contributed to its affirmation in mainstream geological sciences.

Doing sedimentology at that time was very exciting for various reasons:

- looking at rocks with a new and fresh eye;
- discovering aspects of strata that had been previously overlooked;
- enlightening the concept of *event* in stratigraphy (instantaneous, massive deposits separated by intervals of geologic "quiet" time);
- reducing the gap between catastrophist and gradualist visions of natural processes (sporadic events of big magnitude are not exceptions to physical laws; see Dott, 1983);
- connecting sparse and apparently unrelated data into a coherent conceptual framework;
- building up a wholly new data base (vertical and lateral variations of textures and structures in individual beds);
- and last but not least, forming a new group of scientists.

The main excitation, however, came from the lively discussions and exchanges of ideas during meetings and field trips. They gave the participants, particularly the young ones, the feeling of taking part in a new stream of geological thinking, of being within a "movement" that was "making history" (I beg your pardon for this abused expression, but we did actually have this sensation). I personally remember a fantastic excursion organized through the Apennines, in 1965, by the late Giuliano Sestini, who invited some big names such as Ph. Kuenen, A. Lombard, A. Bersier, M. De Raaf, A. Seilacher, G. Kelling and D. Meischner. You can imagine how much you could learn from these guys, being available several days in a row.

Notice that in the pioneering stage of turbidite studies, the term turbidite was not commonly employed. The big debate concerned a lithotype, the *greywacke* (a "dirty", matrix-rich type of sandstone, defined in the Paleozoic of Germany) on one hand, or the "flysch", or "flysch-type" deposits, a "tecto-facies" typical of alpine *s.l.* chains with its two main varieties, arenaceous and calcareous. Somebody regarded greywacke and flysch as synonymous (which they are not) and so part of the discussions were centered on nomenclature and definitions. However, this is not very interesting or exciting.

The word turbidite became of common usage after the publication in 1962 of the seminal book by Arnold Bouma, and the "Bouma sequence" has been considered *the* signature of a turbidity current since that time. In the previous pioneering stage of research, sedimentologists had

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looked for graded bedding and sole markings to recognize turbidites, and current structures had been largely utilized for paleogeographic purposes, i.e. determining provenance and paths of turbidity flows in ancient basins, i.e. in *flysch* formations (see, for a synthesis, Potter and Pettijohn, 1963).

The Bouma sequence was the first depositional model (or, at least, looked like that) applied to assumed deep-water deposits, more or less at the same time when the "fining-up sequence" was defined and became popular in fluvial deposits (Bernard and Major, 1963). Then followed other "models", such as the "coarsening upward" sequence marking beach or delta progradation. Basically, each model consisted of a package of beds (the *bedset* of Campbell, 1967) showing a vertical evolution of texture and structures, i.e. of *facies*. It was observed in modern environments that bed packages could accumulate or be "disactivated" without imposing external or "forcing" changes to the environment of deposition, like sea level variations or tectonic movements (causing uplift or subsidence). The assumption of a depositional system in equilibrium with internal, local controls on changes of sedimentation (for example, switching of active channels by avulsion), and the emphasis on the *cyclical nature* of these changes (*autocyclic* mechanisms) became very popular among sedimentologists, whose aim in those years (1960s and 1970s) became "to erect" facies models for recognizing all or the main sedimentary environments. The autocyclic philosophy was overextended at the expenses of traditional "allocyclic" factors. After a while, however, depositional models showed their limitations and problems: they looked clearcut and applicable in alluvial, littoral and nearshore settings, but less in glacial, eolian and deep-water ones. Furthermore, there arose problems of hierarchy in the vertical distinction and arrangement of sedimentary bodies, and it was not always clear whether a facies represented a process (or mechanism) or an environment (or sub-environment) of deposition. This was the problem, in particular, with the Bouma sequence, a process-oriented model among a majority of environment-oriented models.

The Bouma sequence fits into a longitudinal section of a single turbidite bed, from a proximal to a distal end, and thus represents an individual event (the attempts, for example by Roger Walker and Gian Clemente Parea in the late 1960s to express more precisely or quantify proximality and distality, remained within the limits of the Bouma scheme).

A character by the name of Emiliano Mutti enters at this point (circa 1970). His experience with the exploration sector of the oil industry (the main "factory" and training school of facies models) suggested to him an analogy between coastal deltas and "deep water deltas", which were later named submarine fans. Little was known at that time about modern fans and other deep-water clastic systems. He imagined that, arriving at the base of a slope, turbidity currents were obliged, like rivers, to expand, slow down, bifurcate in various channels and build up a gently sloping and convex apparatus, similar to a delta or an alluvial fan: in other words, a deep-water distributary system.

The factual data for making this assumption consisted in: a) field observations of turbidite facies not fitting the Bouma model; and b) bed-by-bed measurements by the thousands (provided by Mutti and myself) showing that sandstone bed thickness, though appearing to vary erratically in many cases in turbidite formations (mostly classical *flysch* of perialpine areas), showed ordered, systematic changes in several packages rich in sand. By analogy with alluvial and deltaic systems, fining- and thinning-up packages ("*sequences*") suggested the filling of channels, whereas coarsening- and thickening-up sequences reminded of prograding deltaic lobes.

Roger Walker was following the same line of reasoning in the U.S. and Canada, and the "fan model" was the object of heated discussions, opening the door to a new period of excitement, the one that I felt most and which culminated more or less in 1975 when the IAS Sedimentological Congress was held in Nice with a historical excursion in the Apennines.

After that, various studies tried to introduce variations in the basic model, to refine or modify it (just to recall some issues: the definition and geometry of "lobes", the existence of by-pass areas with little or no deposition, and the interpretation of puzzling tractive structures) but the excitement gradually subsided. When, in the 1980s, the accumulation of data from cruises along and across continental margins demonstrated not only that several types of submarine fans existed but that each fan was almost unique and different from the others, or no fan at all was present but other base-of-slope systems, a serious blow was inflicted to the possibility of adopting a deep-sea fan model, and, in spite of discussions in *ad hoc* groups (COMFAN meetings), disappointment and disease began to spread among researchers. The lack of new perspectives resulted in turbidites ceasing to be a hot topic in sedimentology, in spite of some attempts to revive interest in them by focusing on connections with fluvio-deltaic systems, hyperpicnal flows, and so on. Add to this that 1) the autocyclic approach lost appeal in favour of external controls on sedimentation, particularly the eustatic changes which formed the base of the "new verb" of *sequence stratigraphy*; 2) the advances in computer-based science and information processing, especially of subsurface data, made field work less and less attractive to the young generations of geologists.

Anyway, leaving turbidites aside, facies analysis and its models (facies sequences) became, in the last two decades of the twentieth century, a well established technique, needing refinements, additions and completions more than changes of perspective. The whole body of sedimentology eventually became consolidated and its adepts were mostly occupied in what the philosopher of science Thomas Kuhn called "normal science", with no changes in fundamentals and basic approaches. According to Kuhn, normal periods are those dominating in the practice of science and are interrupted by "abnormal", shorter periods of rapid changes, when the basic concepts and procedures are questioned and challenged (his "scientific revolutions"). Many sedimentologists were attracted by sequence stratigraphy and paid less and less attention to facts (field data), becoming more and more busy in interpreting or reinterpreting previous data in the light of sequence boundaries, systems tracts, maximum floods surfaces and so on. In my undoubtedly biased opinion, this work was and it is useful but not at all exciting or promising of substantial intellectual developments. Basically, I see it as more or less sophisticated *pigeonholing*, with more stratigraphy and nomenclature involved than sedimentology.

I'm not sure to have seen real revolutions in my professional life, apart from the advent of plate tectonics which affected the whole spectrum of earth sciences but not so much the study of sediments. Concerning the history of sedimentology, the term "revolution" is perhaps too demanding and I prefer to use "exciting period", which anyway implies the basic requirement of a revolution: a "paradigm shift". I thus passed through two such periods: 1) the consolidation and first application of the turbidite concept, and 2) the definition and applications of facies models.

Well, this is about the past. But now what? Are exciting things happening now in the field of sedimentology? I'm afraid not. When I look around (publications, meetings), I get the impression that we are in a period of "normal science", i.e. "refinements, additions and completions" (in a word, "pigeonholing"). This is not to say that new, significant and important contributions do not appear (however, I purposely avoid making a list here of exemplifying "selected papers"), but only that the "climate" is not that of unstable and questioned paradigms. Exciting changes occurred indeed in the last three decades, but mostly in related and associated fields, like stratigraphy (see isotopic stratigraphy, seismic stratigraphy, sequence analysis…), paleoceanography, paleoclimatology, Quaternary sciences, etc., and not in sedimentology itself. For some aspects, sedimentology has played a role in these changes, but mostly an ancillary one.

I'm aware that old people tend to idealize the time of their youth but, in spite of this bias, I still recognize exciting advances in many sciences, from astronomy and cosmology to planetology, from biogeochemistry to ecology, from climatology to oceanography, not to speak of, say, the study of mind, neurobiology or system science.

Why not in sedimentology?

If I'm wrong, I hope that somebody intervenes for disclaiming or contradicting my point of view. After all, this note is intended as a provocation and calls for contributions to a discussion.

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