“Shoehorning” in Sequence Stratigraphy and its Consequences

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The current debate on sequence stratigraphic methods, surfaces and units centres around two different approaches. One approach, called the empirical approach, advocates the use of five empirical surfaces (subaerial unconformity, shoreline ravinement, maximum regressive surface, maximum flooding surface and regressive surface of marine erosion) for correlation and defining units in sequence stratigraphy. Two types of sequences (R-T and depositional) and two types of systems tracts (transgressive systems tract and regressive systems tract) are defined by various combinations of these surfaces (see ISSC report).

The use of empirical surfaces, which are defined and delineated on the basis of observable characteristics, assures that sequence stratigraphy is a material-based stratigraphic discipline. ISSC was founded by Hollis Hedberg as a key part of his efforts to promote international agreement on the need to separate material-based stratigraphic methods and units (lithostratigraphy, biostratigraphy) from non-material, time-based ones (chronostratigraphy). Given this heritage, ISSC naturally favours a material-based foundation for sequence stratigraphy. This “separation of church and state” philosophy also dominates the NACSN Code

The other approach, called the deductive approach, combines the above empirical surfaces with two time surfaces, the start of base level fall (BSFR) and the start of base level rise (CC) for correlation and the building of sequence stratigraphic units. This results in a number of different sequence types and four different types of systems tracts, termed lowstand, transgressive, highstand and falling stage (forced regressive) systems tracts. Some of the sequence types are bound only by empirical surfaces whereas others have a combination of empirical surfaces and time surfaces. Only one systems tract is bound by empirical surfaces (TST) whereas the other three incorporate one or both of the time surfaces in their boundaries. Thus most of the units which are defined through this approach (see Catuneanu, 2006 and IWGSS draft report) are hybrid, material-based/ time-based units. Such “mixed” units are exactly what Hedberg and the architects of the NACSN Code fought so hard against in their campaigns to separate material-based methods and units from time-based ones so as to produce reasonable, pragmatic guidelines for stratigraphic methods and unit definition.

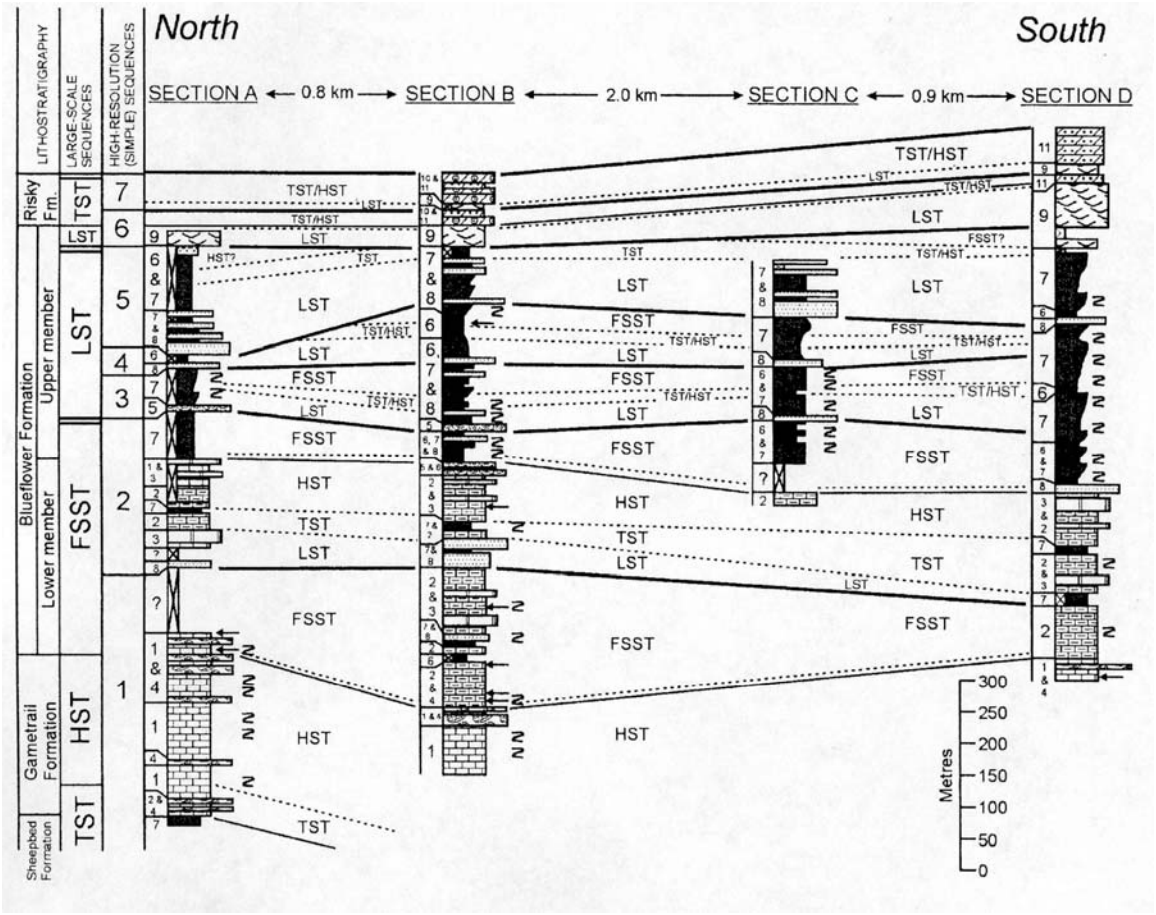
With that introduction, it is very instructive to see how scientists actually apply the mixed material-based/time-based approach to real world stratigraphic successions. This provides an acid test of whether or not the proposed time surfaces (BSFR, CC) are actually observation-based surfaces supported by empirical data, or, like most other conceptual time surfaces, are abstract and unrecognizable by empirical methods in most situations.

Unfortunately and somewhat inexplicitly, Catuneanu (2006) and the IWGSS report do not contain any examples of applications of the "mixed" approach to real world rock successions. However, two recent contributors to the Debate, Bob Dalrymple and Alex MacNeil, have pointed me towards publications which demonstrate the application of the mixed approach, with its four systems tracts, to well exposed strata. They both claim that such examples provide strong support for the adoption of such an approach and units by ISSC. This is exactly the type of feedback we are looking for. Clearly, if a number of examples of the sound application of the deductive approach can be found, thus demonstrating the "realness" of the deduced time surfaces, ISSC would have no problem with recommending such an approach to stratigraphers throughout the world.

In preparation for writing the ISSC Report on Sequence Stratigraphy, the ISSC sequence stratigraphy task group went through numerous papers to find out what sequence stratigraphic methods, surfaces and units were in use. The TG then recommended the use of those methods, surface and units which were material-based and rejected those which were time-based and which did not appear to have any notable empirical support. The detailed logic behind these decisions and subsequent recommendations is found in the ISSC Report. Regarding the mixed, material-based/time-based approach with its four systems tracts, we found that in every case we examined that its application was "forced" and that the authors inappropriately "shoehorned" their stratigraphic succession into such a classification scheme. Such shoehorning was done mainly by designating observation-based stratigraphic surfaces (mainly within trend facies contacts) as the required time surfaces (BSFR and CC) while at the same time ignoring the fact that such surfaces were often highly diachronous and that they had no logical relationship to the events they were purported to represent (e.g. start base level fall).

It became clear to us that, given such inappropriate shoehorning and force fitting, the deductive or "mixed" approach had no empirical support, and worse, it resulted in non-actualistic interpretations of sedimentology and depositional history. Bob and Alex have provided me with two examples of this rather common and unfortunate phenomenon. I appreciate having the examples selected by those who favour the deductive approach so that I cannot be accused of selectively choosing non-representative examples (straw men) to "prove" my point.

The first example of the application of the four systems tracts is that by MacNaughton, Narbonne and Dalrymple (2000), all very competent and well respected sedimentary geologists. They applied the four systems tract classification scheme to a succession of slope carbonates and siliciclastics of latest Proterozoic age and delineated 7 sequences, most of which were subdivided, in ascending order, into a LST, TST, HST and FSST. It is instructive to examine how they delineated the BSFR (time surface at the start of base level fall) which separates the HST from the overlying FSST and the CC (time surface at start of base level rise) which separates the FSST from the overlying LST. The stratigraphic cross section which illustrates their delineated systems tract boundaries (their Fig 5) is below.



In some cases their BSFR (base FSST and the time surface at the start of base level fall) is placed at the within trend facies contact between clean limestones below and somewhat argillaceous strata above. To quote them (p.1010), “at the first appearance of beds or partings of siliciclastic shale”. By designating such a facies boundary as the time surface which represents the seafloor at the start of base level fall, the authors are claiming that, at the moment base level started to fall, argillaceous material was suddenly brought into the basin and began to be deposited everywhere at that moment in time. Is this sedimentologically reasonable? Do the authors really want the readers to believe such a depositional history? I hope not.

They also sometimes place a BSFR within an overall coarsening upward, siliciclastic succession (e.g. sequences 3 and 4) but do not indicate the criteria for placement except

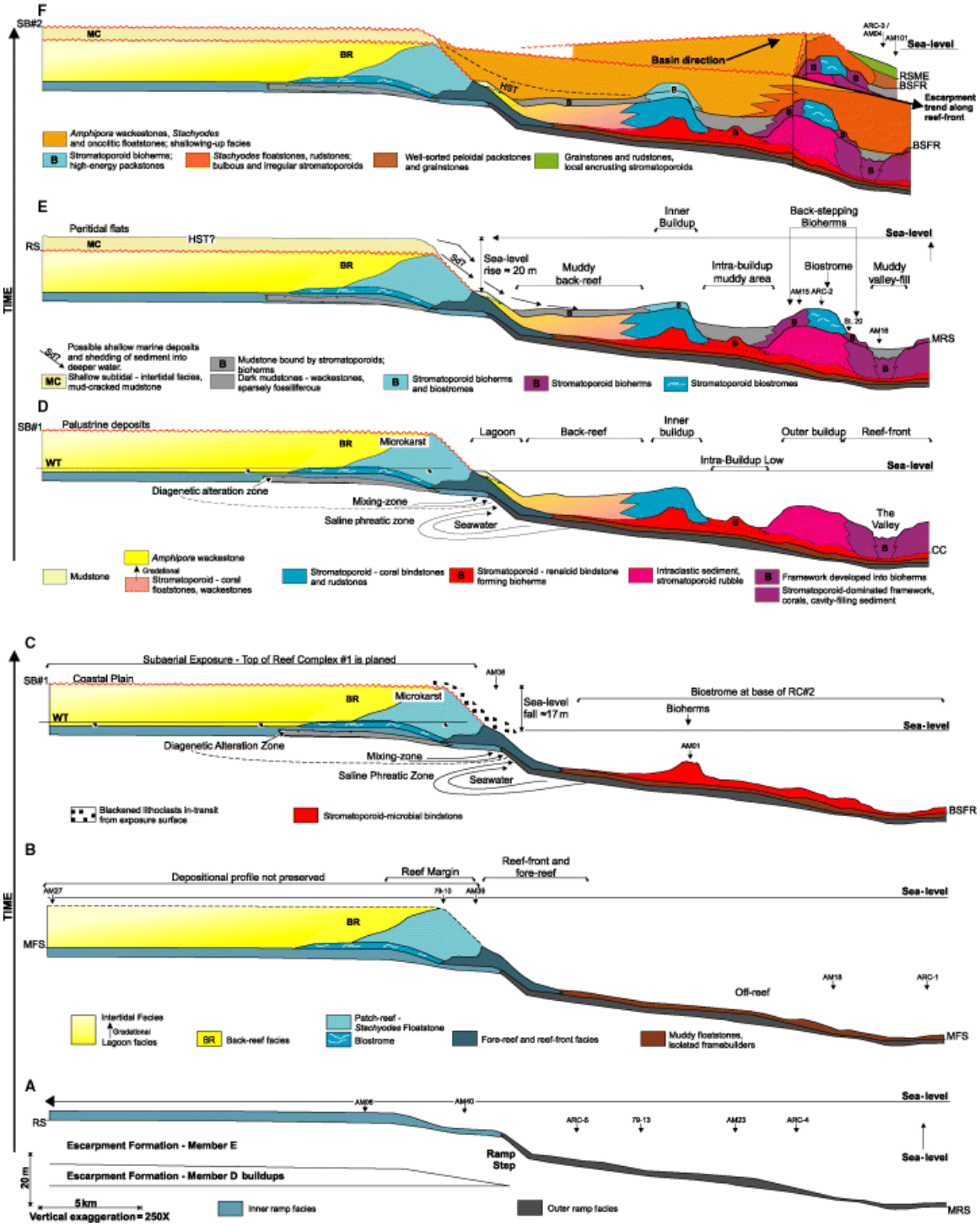
to imply they chose the level where supply increases. Again there is no logical support for such a placement and such a nebulous, nondescript facies boundary has no relation to a time surface at the moment of start base level fall when supply rate to the slope would not have been affected.

Examination of the stratigraphic surfaces that MacNaughton, Narbonne and Dalrymple designated as the CC in their succession (the basal contact of the LST and the time surface at the start of base level rise) is also revealing. They have chosen the base of a turbidite facies (usually siliciclastic) as the CC ("the abrupt base of a package of thick massive, quartz sandstone", p.1010). Thus they envision, and would have the reader believe, that at the moment when base level started to rise, there was a sudden flood of coarse material onto the slope. This is actually the opposite of what might be expected at the start of base level rise when coarse material begins to become stored on the shelf. Thus we have another example of somewhat non-actualistic and hard to accept sedimentological and depositional history interpretations and reasoning.

One has to ask how it came about that three competent and highly regarded sedimentary geologists made such questionable sedimentological/depositional interpretations. To me, the answer is that authors felt obliged to apply sequence stratigraphy to their succession. This led them to shoehorn (force) their excellent sedimentological data into the entrenched, four systems tract classification scheme. To apply such an approach, they had to identify a BSFR and a CC to act as the boundaries of various, "required" sequence stratigraphic units such as an LST and an FSST. Furthermore, because such time surfaces have no observable characteristics, they were forced to use "compromise" stratigraphic surfaces they could delineate and correlate. For this they chose within trend facies contacts for the time surfaces and this choice resulted in the above described, non-actualistic, sedimentological interpretations. Such are the consequences of shoehorning good data into unworkable classification schemes.

If MacNaughton et al (2000) had applied the empirical approach to their succession and recognized only material-based, sequence stratigraphic surfaces, they would have had to delineate only MRSs and MFSs. Both these observation-based entities can be readily recognized and this would have allowed the succession to be subdivided into a number of either R-T sequences (MFSs as boundaries) or depositional sequences (MRSs as boundaries). Furthermore, each sequence could have been objectively subdivided into a TST and a RST. Such a framework would have allowed well supported and actualistic interpretations of the depositional history and avoided the questionable interpretations which result from trying to delineate specific time surfaces in the strata.

The second example of the application of the four systems tract approach is that of MacNeil and Jones (2006) who are undoubtedly very competent and accomplished carbonate sedimentologists. They did a sequence stratigraphic analysis on a Late Devonian reefal, carbonate ramp succession and recognized two sequences, each with a LST, TST, HST and FSST. The diagram below (their Figs 4 and 11) illustrates their facies and sequence interpretations for 6 stages of development.



They recognized two BSFRs and one CC. The lower BSFR was placed at a facies contact between a biostromal unit and underlying finer grained carbonate sediment which

coarsened upward from a MFS and which was equivalent to a bioherm which developed on a “ramp step”. This contact was sharp and presumably scoured and starved at most locations. They interpreted this starved surface formed when the inner ramp and bioherm became exposed, thus shutting down carbonate supply. This is reasonable but the problem with using such a surface as the BSFR is that the inner ramp and bioherm did not become exposed at the start of base level fall. As shown on the B stage section of their diagram, the ramp and bioherm were not at sea level at the end of rise. These areas did not become exposed until sometime after the start of fall. Thus the time surface at the start of fall lies somewhere in the strata below the base of the biostrome and has no recognizable features. The MacNeil and Jones interpretation of their lower BSFR at a facies contact, which most likely developed during fall, is not tenable and demands the implausible interpretation that the shelf and marginal reef were exposed at the moment of start sea level fall.

Their interpreted upper BSFR provides insight into another intractable problem. As shown in the phase E section of their diagram, a well characterized MFS developed after the inner ramp was re-flooded. This is a useful and bona fide surface. MacNeil and Jones then claim that the MFS over much of the outer ramp is also a BSFR by making the unsupported assumption that sediment directly overlying the MFS in this area was deposited during base level fall (i.e. sediment produced during the preceding phase of regression and BL rise was not deposited there).

I would emphasize it is impossible to pinpoint the stratigraphic level which represents the start of base level fall. Consequently, it is impossible to know if strata directly overlying the MFS were deposited during rise (“HST”) or were deposited during fall (“FSST”). It may well be that the sediment which overlies the MFS at all localities was deposited during rise. A reasonable case can be made for this. Alternately a case could be made that everywhere on the outer ramp the sediment overlying the MFS was deposited during fall. The bottom line is there is no way to choose between these competing interpretations and thus it is not reasonable to superimpose a definitive interpretation on the strata overlying the MFS by ARBITRARILY calling them either HST or FSST. By using the empirical method and designating them as RST, such an unjustifiable and arbitrary interpretation can be avoided. In summary, MacNeil and Jones’s second BSFR is not reasonable because it forces an unsupportable, speculative and basically unnecessary interpretation on the strata.

MacNeil and Jones identified one CC and it separated their first and second sequences in the area of the outer ramp. It is seen on the Phase D section of their diagram and it separates the underlying biostrome from more segregated bioherms and flanking strata. Overall the contact is a within trend facies change within an overall shallowing upward succession of facies. MacNeil and Jones interpret that this contact developed when base level fell far enough to allow the carbonate factory to get well established and produce much more sediment. Thus is not an unreasonable interpretation but there is no reason or evidence to suggest that this happened at the end of base level fall. As illustrated in the Phase C section, there was still considerable water depth when this event occurred. This contact may well have developed during fall and the entire biohermal succession of the

outer ramp may have been deposited during fall. Notably there is NO indication of base level rise during the deposition of the biohermal deposits of the outer ramp. This is clearly indicated on the MacNeil and Jones diagram which shows NO onlap on the unconformity during the deposition of the biohermal sediments. Also note that their sea level is at exactly the same level in the Phase D section as it is in Phase C section despite their claim of base level rise during this time. Thus, by way of their diagram, even MacNeil and Jones acknowledge that base level was not rising during the deposition of the biohermal sediment.

Overall there is no evidence that the biohermal sediments were deposited during BL rise and thus there is no justification for placing a CC at their base. The empirical method of placing all the strata between the MFS below and the recognized (and justifiable) MRS at the top of the biohermal strata in an RST avoids special pleading and unsupported interpretations. Note also that the MRS joins the unconformity (an SR-U) on the Phase E section and thus allows an empirical and readily delineated depositional sequence boundary to be drawn in the succession. Trying to push a sequence boundary along an unsupported CC is just another example of forcing the strata into a model rather than letting empirically supported surfaces dictate the boundary placement.

In summary, both these examples of the application of the deductive, “mixed” model, with its required recognition of time surfaces, demonstrate the pitfalls of such an approach. One notable consequence is that many unreasonable, non-actualistic interpretations of sedimentological processes and depositional history are being added to the literature by respected scientists. Our survey revealed there are innumerable published examples of such highly questionable interpretations, all of which resulting from futile attempts by authors to recognize abstract time surfaces so as to allow the delineation of the LST, HST and FSST in outcrop, well and seismic data.

I think it is time for those who continue to advocate for a mixed, material-based/time based approach as advocated by Catuneanu (2006) and IWGSS to provide appropriate examples of the application of the four systems tracts to real world stratigraphic successions. Right now we are seeing an epidemic of forced applications of the entrenched, “mixed” model and a resulting plethora of untenable interpretations which sometimes require miracles, as opposed to established processes, for their acceptance. ISSC is trying to bring some common sense and actualism to the application of sequence stratigraphy to a variety of sedimentary successions and such an effort demands that abstract time surfaces not be part of the methods and unit definitions.