Comments on the ISSWG Report "Toward the Formalization of Sequence Stratigraphy"

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This document presents a different approach to sequence stratigraphic methods, surface and unit delineation and terminology from that offered by the ISSC Report on Sequence Stratigraphy. As such, it provides insight into where there is common ground between the two approaches and highlights the significant differences which need to be discussed and hopefully resolved. In the ISSC report, these two approaches were referred to as model-driven and data-driven. It seems there is some opposition to such terminology for the two approaches and in this document the two approaches will be simply called deductive and empirical. The reader is referred to Miall (2004) for a detailed discussion of these two, fundamentally different, scientific approaches which are used in most scientific disciplines.

An excellent example of the use of these two scientific approaches is in particle physics. Many specific particles have been identified through experiments and these empirical particles are part an inductive model. A few particles have been deduced (hypothesized) to exist on the basis of large scale observations of galaxies and the deduced concept of "dark matter". One such hypothetical, dark matter particle is the neutralino. Currently, experiments are being designed to try to demonstrate that it really exists. Notably, just because no empirical data have been gathered so far to support the realness of the neutralino, does not mean such a deductive particle has no value. Deduction and the hypotheses derived from such an approach are a very important part of the scientific process as well explained by Miall (2004). However, it is important to keep deductive and hypothetical entities such as a neutralino separate from for real entities (e.g. a quark) which are well established through empirical data derived from experimental or historical science. Deduction often guides scientific investigation but empirical data are required to establish the validity of any deduced concept or entity.

The ISSWG approach to sequence stratigraphy is deductive because it hypothesizes the existence of 7 surfaces of sequence stratigraphy on the basis of the Jervey Model (sinusoidal base level change, constant sediment supply). Notably, five of these deduced surfaces match five surfaces established with empirical data gathered over the past 200 years. Two of the deduced surfaces, called the basal surface of forced regression and the correlative conformity, are not yet supported by empirical data from rocks (ie they are equivalent to neutralinos). There are other differences between the deductive approach and the empirical approach when it comes to defining and delineating various surfaces of sequence stratigraphy and these differences are discussed below in specific comments. These specific comments are tied to statements (in quotations) taken from the ISSWG Report which is on the USC Sequence Stratigraphy website.

Specific Comments on the section "Introduction: background and rationale"

1) "that leaning toward one particular model or another will never lead to the formal acceptance, by all 'schools', of any sequence stratigraphic concept. Instead of trying to empower one model over another, we rather need to understand why these models are different"

I think it is much more productive to talk about approaches to sequence stratigraphy rather than "models" or "schools". As discussed above and in the ISSC report, there are really only two approaches, a deductive one which starts with a model (hypothesis) and deduces surfaces to be used for unit delineation, and an empirical approach which uses surfaces recognized by empirical data for unit delineation. There is no doubt that it is essential to understand the different results of these two, fundamentally different approaches.

2) "The need for standardization is evident when one considers the present state of nomenclatural chaos in the field of sequence stratigraphy. The usefulness of the method is overshadowed by confusing or even conflicting terminology, which gives a sense of helplessness to any inexperienced geologist who tries to pick up the method from reading research papers produced by different groups"

I agree completely with this statement and I am sure reasonable consensus can be reached for surfaces and units which both approaches recognize.

3) "On the 30th birthday of sequence stratigraphy"

It seems that those who prefer a deductive approach think sequence stratigraphy did not exist before 1977. As discussed in the ISSC report, sequence stratigraphy began in 1949 with the definition of a sequence and was quite popular in the 1950s and early 60s. I was using it in the early 70s.

4) "ISSC Task Group intends …deciding which one of the existing sequence stratigraphic models is most suitable (or 'practical') to carry the ISSC recommendation on methodology and terminology for sequence stratigraphy."

As demonstrated in our report, the ISSC TG chose an empirical approach over a deductive one for defining surfaces and consequent units in sequence stratigraphy. This choice was made because it was determined that two of the hypothesized surfaces in the deductive approach were not supported by empirical data. Hence, considerable uncertainty existed as to their realness (recognizable in well exposed rocks) and thus to their suitability for correlation and unit definition in sequence stratigraphy.
Our position is, if we are unable to provide guidance on how stratigraphers might recognize such deduced surfaces in outcrop and core, then it would be irresponsible to recommend the use of such proposed surfaces in sequence stratigraphy. So far our TG has not received or discovered any empirical data which demonstrate how such deduced surfaces can be recognized in well exposed strata.

Specific Comments on the section "Sequence models: core vs. trivial aspects in sequence stratigraphy"

5) "Core aspects include the correct identification of all sequence stratigraphic surfaces (Fig. 5), irrespective of the status assigned to them by different models, and implicitly, the correct identification of the different genetic types of deposits separated by these surfaces."

I agree that this is of major importance. The identification and characterization of the specific surfaces of sequence stratigraphy which mark changes in depositional trend is fundamental to sequence stratigraphic methods and unit delineation. As has been emphasized previously, the main difference between the deductive approach of ISSWG and the empirical one of ISSC is the viability of the two deduced time surfaces (clinoforms, paleo-sea floors) for use in sequence stratigraphic methods and unit delineation.

6) As a result of the interplay between sedimentation and available accommodation at the shoreline, four main events are recorded during a full cycle of base-level changes (Figs. 3, 5):

1. Onset of forced regression (onset of base-level fall at the shoreline);
2. End of forced regression (end of base-level fall at the shoreline);
3. End of regression (during base-level rise at the shoreline);
4. End of transgression (during base-level rise at the shoreline).

These four events control the timing of formation of all sequence stratigraphic surfaces and systems tracts, and are recognized by all sequence stratigraphic 'schools'.

This nicely summarizes the deductive approach of ISSWG to sequence stratigraphy. The chosen model has four events and it is deduced that there is a specific type of sequence stratigraphic surface formed as a consequence of each event (event specific in the words of the ISSWG report). These deduced sequence stratigraphic surfaces are then used to define sequence stratigraphic units.
The empirical approach of ISSC simply uses surfaces of sequence stratigraphy which have been recognized by empirical methods over the last 200 years, The first one recognized was the subaerial unconformity (Hutton ~1788) and the last one was the regressive surface of marine erosion (Plint, 1988). As described in the ISSC Report, all of the empirically recognized surfaces of sequence stratigraphy are also deduced by the above model thus adding theoretical support to them. This allows further understanding of such surfaces and their relationship to each other.
In the empirical approach, the surfaces came first through observation, followed by a general, inductive model to explain the generation of such surfaces (observation followed by reasoning). For example Plint first recognized a RSME in empirical data from Cretaceous strata in western Canada and he then proposed a model for its generation.
This contrasts with the deductive approach in which the time surfaces at the start and end of base level fall are hypothesized to exist in strata as recognizable (mappable) stratigraphic surfaces. So far no empirical, rock-based data have been presented to support such a hypothesis (reasoning with no observation).

7) **"**, the four main events of the base-level cycle mark changes in the type of shoreline trajectory, and implicitly, changes in stratal stacking patterns in the rock record."

This is a good example of the deductive approach. The model has four different events and thus there should be four different changes in "stacking pattern" with each change marking a specific sequence stratigraphic surface. This is an acceptable hypothesis but unfortunately it has never been demonstrated through empirical

data that all four hypothetical changes in stacking pattern can be recognized in a variety of stratigraphic settings.

8) "What is relevant to this discussion is that sequence stratigraphic surfaces and shoreline trajectories, whose timing depends on the four main events of the base-level cycle, are core concepts *independent* of the sequence stratigraphic model of choice. These core concepts are validated by all 'schools',"

This is a misleading statement. The four events and their hypothesized related surfaces are only part of the deductive approach to sequence stratigraphy. This is NOT a core concept validated by all "schools". It is only a core concept for the deductive approach and has no role in the empirical approach beyond guiding future research to determine more empirical surfaces.

9) "a generic definition of a 'sequence' that satisfies all 'schools', while leaving the selection of sequence boundaries to the discretion of the individual, provides the flexibility that allows one to adapt to the particularities of each case study, and the "freedom to experiment with new concepts and ideas""

The ISSC report provides such a generic and flexible definition of a sequence and sequence boundary and this aspect of sequence stratigraphy is not in dispute except perhaps in terms of the exact wording of a generic definition.

10) "a full cycle of base-level changes includes two stages of sediment-driven 'normal' regression (lowstand and highstand), an intervening stage of 'forced' regression driven by base-level fall, and a stage of shoreline transgression (Fig. 7). Each of these stages results in the formation of a particular genetic type of deposits, with characteristic stratal stacking patterns and sediment distribution within the basin"

As mentioned earlier, these deduced (hypothesized) four different units with "characteristic" stacking patterns have never been empirically demonstrated and this is the main source of the differences between the deductive and the empirical approaches.

11) "The large data-base of sequence stratigraphic work that is already available demonstrates that no model is foolproof, and that the relative success of each approach may vary with the case study."

The empirical approach is basically foolproof in most settings simply because it only uses well established, well cahracterized and widely accepted entities. On the other hand, the deductive approach is always potentially problematic until empirical data confirm the "realness" all the various hypothetical entities (e.g. BSFR) associated with the model.

12) "correlative conformities form independently of sedimentation, corresponding to the events that mark the onset and end of base-level fall at the shoreline"

Here is the basic reason why these two clinoforms (paleo-sea floors) cannot be recognized in rocks. Because they "form independent of sedimentation", there will be no change in sedimentation trend or facies to characterize such hypothetical

surfaces in most, if not all, depositional settings. Thus they are "invisible" and cannot be considered to be real surfaces at this time (dark matter neutralinos).

13) "Correlative conformities may be difficult to map in shallow-water systems"

This is somewhat of an understatement given that no one has ever mapped (recognized) them in shallow marine settings. Right now a more realistic statement would be "correlative conformities are impossible to map in shallow-water systems on the basis of outcrop and well data"

14) "the genetic stratigraphic approach recognizes the importance of separating forced regressive, normal regressive and transgressive deposits as distinct genetic wedges".

Bill Galloway can perhaps correct me here, but to my knowledge, no published study, which employed genetic stratigraphic sequences, subdivided such sequences into "forced regressive, normal regressive and transgressive deposits".

15) "The "transgressive-regressive" (T-R) sequence model"

This empirical approach is not a model but a "packaging scheme" (i.e. a specific type of sequence) using the empirical surfaces. Another sequence type, an R-T sequence, is also available for the empirical approach, as described in the ISSC report.
As pointed out in both the ISSWG and ISSC reports, there are also different packaging schemes for the deductive approach. These should also not be referred to as different models. Currently, there is only one dominant model, the Jervey Model, and it forms the basis of the deductive approach.

16) "a number of pitfalls of this approach include: maximum regressive surfaces may be cryptic in deep-water systems, where they may form within an undifferentiated succession of leveed-channel low-density turbidites (Posamentier and Kolla, 2003; Catuneanu, 2006)"

Here is a classic collision between the empirical approach and the deductive approach. In deepwater settings where turbidites are present the MRS is empirically placed at the top of the coarsest, thickest turbidite unit. This is how the MRS is empirically defined (change in grain size trend from coarsening to fining).
However, using the Jervey Model, the deductive approach hypothesizes that the MRS should "form within an undifferentiated succession of leveed-channel low-density turbidites" and thus be "cryptic". Such a hypothesis has never been substantiated by empirical data.

17) "Lowstand normal regressive deposits are documented from a variety of depositional settings including fluvial (e.g., Kerr et al., 1999; Leckie and Boyd, 2003), clastic coastal to shallow-water (e.g., Plint, 1988; Plint and Nummedal, 2000; Hampson and Storms, 2003; Ainsworth, 2005), clastic deep-water (e.g., Posamentier and Kolla, 2003) and carbonate platforms (e.g., Cathro et al., 2003; Schlager, 2005)"

I have gone through each of these references with a fine toothed comb and not a single one has a documented "lowstand normal regressive deposit" (regressive unit with the landward portion of the unit onlapping an SU). In fact, some clearly show that such a deduced unit is unrecognizable (e.g. Hampson and Storms Fig 5). This figure is reproduced in a later part of this document when the CC is discussed.

Specific Comments on the section "Principles of standardization"

This section of the report nicely lays out the model and the deductive approach to sequence stratigraphy. The one statement that sums it up beautifully is in point 1 "Seven sequence stratigraphic surfaces are defined relative to the four main events of the base-level cycle"
The 7 surfaces promoted by ISSWG are NOT defined on empirical data but on the basis of deductions from the model. This leaves no doubt that the ISSWG approach to sequence stratigraphy is a deductive one.

18) "Inherent difficulties in recognizing any of the sequence stratigraphic surfaces do not negate their existence or validity. In most cases, this is just a reflection of lack of sufficient data."

Here again is where the empirical approach and the deductive one clash. The empirical approach only recognizes a given surface once sufficient empirical data are available to demonstrate its "realness". In the deductive approach of ISSWG, if a surface hypothetically exists, then that is good enough to use it in sequence stratigraphic methods and for unit delineation regardless if it has any empirical support or not.
The "lack of sufficient data" to demonstrate the realness of a deduced surface is not seen as a problem by ISSWG. To those who follow an empirical approach and want to provide practical guidance to stratigraphers throughout the world (ie ISSC), this is a major problem. The ISSC position is that sufficient empirical data are required before a deduced surface is accepted as real and suitable for use in sequence stratigraphy. This is non-negotiable.

19) "Integration of outcrop, core, well-log and seismic data affords the most effective application of the sequence stratigraphic method"

This is certainly an ideal case and the availability of data from all four sources would allow the most effective application. However, any surface employed in sequence stratigraphy first and foremost must be defined and be recognizable in well exposed rock (core and outcrop). Once such a surface is defined and characterized, attempts can be made to interpret its occurrence on much lower resolution data sets such as well logs and seismic. For example a subaerial unconformity is defined on the basis of empirical rock data but most stratigraphers interpret the occurrence of such a surface on seismic and log data.
It is unacceptable for a primary surface of sequence stratigraphy to be defined on the basis of low resolution seismic data (minimum of 5-10 metres of uncertainty and no characteristics) and then expect it to be applicable to rocks. A seismic reflector is NOT a stratigraphic surface! Seismic data can help guide research into the realness of deduced stratigraphic surfaces by narrowing down the stratigraphic position of

such a deduced surface. So far seismic data have not been used in this way to try to characterize the two hypothetical time surfaces, BSFR and CC.

20) "Different genetic types of deposits (forced regressive, normal regressive, transgressive) need to be separated as distinct systems tracts, "

The telling word is this statement is NEED. The model dictates that different types of genetic deposits are generated during sinusoidal base level change. Given this, it becomes imperative that such deduced, different deposits are recognized as specific units of sequence stratigraphy, regardless if there is any hope of actually differentiating them in the real world. It is hard not to use the term "model-driven" in this situation.

One major problem that has resulted from this "need" to identify the four different systems tracts is that many authors, perhaps wanting to be "thoroughly modern" stratigraphers, have shoehorned their data into this deductive model. To do this they have used very inappropriate surfaces to try to approximate the two time surfaces which one has to recognize to satisfy the "need" of four systems tracts. The use of such inappropriate surfaces demands the concoction of non-actualistic, sedimentological interpretations which border on miraculous (i.e. a suspension of the laws of physics). The sedimentary geology literature over the past 18 years is replete with such sedimentological nonsense which results from authors trying to make their data "fit" the model.

For example, many authors have used the base of a turbidite succession as the BSFR and this demands an interpretation that there was a sudden flood of turbidites everywhere into the deep basin at the instant when base level started to fall. Such a sedimentological scenario is non-actualistic to say the least. A similar "impossible" interpretation is demanded by placing the BSFR at the base of a shallow water unit such as a sandstone or lime grainstone, a common practice.

Such absurdities all relate back to the unrealistic expectations and the "need" to recognize the two time surfaces and the four systems tracts. They will continue to happen until stratigraphic guides/codes either: 1) acknowledge that the two time surfaces are not real, mappable surfaces and cannot be used to bound sequence stratigraphic units OR 2) provide stratigraphers which clear guidance on how to reliably identify such time surfaces in well exposed strata.

ISSWG is contributing to this major problem by 1) continuing to claim that the hypothesized time surfaces are real and should be used to delineate units and, 2) failing to provide stratigraphers with reliable criteria for identifying such surfaces in well exposed strata. I hope some of the members of ISSWG can see that such an approach is unproductive and not beneficial to the science of stratigraphy.

21) "Where two or more sequence stratigraphic surfaces are superimposed, always use the name of the youngest surface."

This is a common error which causes confusion. Sequence stratigraphic surfaces are not "superimposed" which implies two or more are present at one horizon. One type of surface sometimes erodes through one or more other sequence stratigraphic surface. For example, when the shoreline ravinement erodes a subaerial unconformity, it is a mistake to think the SR and the SU are "superimposed". The SU is gone and the SR is left.

Specific Comments on the section "Recommendations"

22) "sequence stratigraphy studies the *sedimentary response to changes in base level, and the depositional trends that emerge from the interplay of accommodation (space available for sediments to fill) and sedimentation"*

This is certainly an improvement of some previous definitions of sequence stratigraphy (e.g. that of van Wagoner et al, 1990) but it still does not focus on the primary methods of sequence stratigraphy. Sequence stratigraphy involves 1) the recognition and correlation of stratigraphic surfaces which represent changes in depositional tend and 2) the description and interpretation of the strata bound by such surfaces.

23) "<u>Sequence</u>: a relatively conformable succession of genetically related strata bounded by unconformities or their correlative conformities (Mitchum, 1977). A sequence corresponds to a full cycle of base-level changes"

This definition also leaves something to be desired. Because it is useful to define specific types of sequences, we need a better generic definition. The definition of a sequence provided in the ISSC Report adequately handles this. It is also not a good idea to include in the definition the highly interpretive element of a sequence corresponding to a full cycle of base level change. This greatly restricts the meaning and puts unwanted constraints on it. It is much better to have a purely descriptive definition like that provided in the ISSC report.

24) "The addendum to the original definition of Mitchum (1977) that a sequence corresponds to a full cycle of base-level changes is required to separate a sequence from component systems tracts."

This is simply not so, if a sequence is properly defined in the first place. Using the ISSC definition one can define different types of sequences. Any given type of sequence can then be divided into component systems tracts on the basis of recognizable surfaces of sequence stratigraphy which lie within the sequence. I can understand why the deductive approach wants to include such a highly interpretive element into the definition but such an addition has no place in the empirical approach,

25) "Parasequences are stratigraphic units bounded by 'flooding surfaces' (Van Wagoner et al., 1988, 1990), which, depending on circumstances, may be represented by transgressive ravinement surfaces, maximum flooding surfaces, maximum regressive surfaces, or facies contacts within the transgressive systems tract"

Defining a parasequence as a sequence stratigraphic unit that can be bound by many different types of surfaces including lithostratigraphic ones is a recipe for hopeless confusion. Is a unit bound by MFSs equivalent to one bound by SRs and to one bound by MRSs and to one bound by a lithologic change from sandstone to shale? Of course not, but that is what the current ISSWG definition of a parasequence tells us.
If one wants to properly define a parasequence, the specific type of surface which bounds such a unit must be clearly stated or else the term has to be generic like a

sequence with different types of parasequences defined. As argued in the ISSC Report, such specific types of parasequences would be redundant because they would be equivalent to already defined types of sequences (e.g. a parasequence bound by MFSs is exactly the same as a genetic stratigraphic (R-T) sequence (remember the statement "the definition of a sequence is independent of temporal and spatial scales").
If the ISSWG wants to retain the parasequence as a bona fide unit in sequence stratigraphy, an acceptable definition of such a unit needs to be formulated. Also the issue of scale has to be addressed. In basins where many of the basin margin unconformities are not preserved (not an uncommon occurrence), are the large scale units (100s of metres thick), which are bound by large scale MRSs, called parasequences? The ISSC Report recommends such units be called depositional sequences. Would ISSWG call them parasequences?
The current ISSWG recommendations on parasequences are wholly inadequate and will lead to substantial confusion rather than enlightenment.

26) "A sequence may be subdivided into component systems tracts, which consist of packages of strata that are genetically distinct (e.g., forced regressive, normal regressive, transgressive). The original definition by Brown and Fisher (1977) is generic and devoid of nomenclatural ambiguity, and thus remains acceptable by all 'schools': Systems tract: a linkage of contemporaneous depositional systems, forming the subdivision of a sequence

This definition of a systems tract is not a good one and is certainly not accepted by the empirical approach to sequence stratigraphy. The B+F definition implies systems tracts have time surfaces for boundaries because only that would result in encapsulating "contemporaneous" depositional systems. It also begs the question of what a depositional system is and how one draws the boundaries of such an entity. To avoid confusion and misunderstanding, a generic systems tract is best defined as a component unit of a sequence bound by surfaces of sequence stratigraphy (see ISSC Report). This opens the door to defining specific types of systems tract being bound by specific types of sequence stratigraphic surfaces.

27) "The timing of systems tract boundaries is set by the four main events of the base-level cycle"

This again emphasizes the deductive nature of the ISSWG approach. Such an interpretive constraint on a systems tract boundary is not conducive to erecting units which have reasonably objective boundaries. It is reminiscent of Posamentier and Vail (1988) tying system tract boundaries to the sea level curve. Hopefully some ISSWG members can still recall all the trouble and confusion that caused.

28) "Systems tracts are interpreted based on stratal stacking patterns, position within the sequence, and types of bounding surfaces"

Only the last criteria, "types of bounding surfaces" has any merit for defining specific types of systems tracts. Stratal stacking patterns can help to delineate specific types of surfaces in some settings and thus can contribute to systems tract recognition in a secondary manner. Using the ISSC definitions, position within a

sequence has no relevance for systems tract identification and such a criteria is an artifact of the deductive approach.

29) "Each type of shoreline shift (forced regression, normal regression, transgression) is associated with the formation of a particular genetic type of deposits (cf. systems tract;"

It remains to be seen if this hypothesis will be confirmed by empirical data. Highly equivocal interpretations with seismic data do not constitute adequate empirical data in this regard. For example the seismic section in Figure 9 of the ISSWG Report can readily (and more simply) be reinterpreted in terms of empirical surfaces (BSFR = MFS ; CC = MRS; MRS = small scale MRS; SU = SU or SR-U) and the depositional sequence and two systems tracts (RST, TST) of the ISSC Report can be recognized. Without any lithologic data, the interpretation of this line in terms of sequence stratigraphy will always be equivocal. The bottom line is that data from well exposed strata are required to confirm or deny any hypotheses derived from the Jervey Model. Shoehorning seismic data into the model is not a productive activity.

30) "The separation of forced regressive, normal regressive and transgressive deposits is central to sequence stratigraphy and petroleum exploration, because each such genetic wedge (systems tract) is characterized by distinct sediment dispersal patterns and includes different petroleum plays"

Such a separation may be central to the deductive approach of ISSWG but has no relevance when it comes to an empirical approach. It would be nice if such a subdivision could be achieved but empirical data are required to do this, not just a desire to do so.

31) "The following seven sequence stratigraphic surfaces can be defined relative to a reference curve of base-level changes"

Again, a nice example of the deductive approach to sequence stratigraphy. In the empirical approach, the various surfaces are defined on specific characteristics. Their genesis is often interpreted within the context of a base level model and autocyclic models are also helpful. It is emphasized such interpretations have no role in their definition. Thus a subaerial unconformity is often interpreted to form during a base level fall but it is recognized it may also be formed during base level rise in some instances. It is critical to separate defining characteristics from interpretations of origin.
Notably, NO defining characteristics are presented in this document for any of the deduced surfaces. Such characteristics are the most important feature of any proposed surface. It is stated that such characteristics are in Catuneanu (2006). They should be clearly and succinctly summarized in this document. The information currently supplied for each surface is minor and trivial compared to the characteristics of each surface.

32) "<u>Correlative conformity</u> (*sensu* Hunt and Tucker, 1992)":

If the ISSWG is going to persist with proposing that the CC of Hunt and Tucker is a real surface, then a real name is required for it. The CC of Posamentier is the BSFR

and that is an acceptable name for this hypothetical surface. CC of Hunt and Tucker is a most "user-unfriendly" term.

Of course the main issue with this proposed surface is it realness. On seismic data a reflector can often be correlated basinward from the approximate termination of the SU/SR-U and such a reflector should in theory contain the CC surface (as mentioned earlier a reflector is not a surface). This is illustrated on Figure 9 of the ISSWG Report.

Notably, no one has ever determined in a proper scientific fashion what stratigraphic surfaces are in such a reflector. Calling the reflector the "correlative conformity" and saying this proves the realness of the CC as a stratigraphic surface is of no help whatsoever because the reflector likely contains a number of stratigraphic surfaces.

From my field experience and an extensive review of the literature, it appears that in almost all cases, the sequence stratigraphic surface which joins the basinward termination of the basin flank unconformity (SU and/or SR-U) is an MRS. There is good theoretical support for such an observation as discussed in the ISSC Report. As noted earlier, the reflector labeled as CC on Figure 9 can be readily interpreted as an MRS. The strongest evidence for this is the stratigraphic relationships apparent on adjacent seismic sections which clearly show onlapping marine strata on this reflector. The minor progradational unit overlying this reflector on Figure 9 (labeled as LNR) appears to be a local shelf edge delta which developed in the TST over a small area. It does not appear to be present on adjacent lines.

Below is a portion of Figure 5 from the Hampson and Storms (2003). Note how the KSB, GSB1 and DSB unconformities correlate with MRSs (called "flooding surfaces").

From an empirical point of view, the MRS correlates with the basin flank unconformity and a reasonable base level/sedimentation model can be induced to support this . If ISSWG wants to dispute this interpretation, then we need some solid empirical data which supports their concept of the CC.

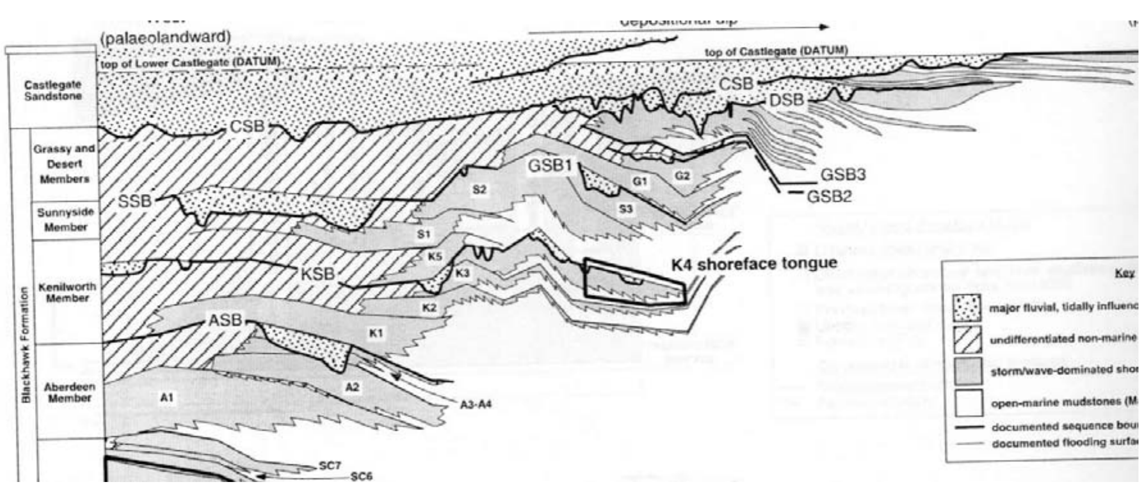

Figure 1 A portion of Figure 5 from Hampson and Storms (2003). Note that the "correlative conformities" of unconformities labeled as KSB, GSB1 and DSB are MRSs.

33) Correlative conformity (Posamentier et al., 1988; Posamentier and Allen, 1999):

- Synonymous terms: basal surface of forced regression (Hunt and Tucker, 1992).
- Origin: oldest clinoform (paleoseafloor) associated with offlap.
- Timing: onset of base-level fall at the shoreline (i.e., onset of forced regression).

The BSFR (correlative conformity of Posamentier) is a very problematic surface even for the deductive approach. The identification of such a "clinoform (paleo-sea floor)" demands that the point on the subaerial unconformity, which coincides with the start of offlap, be accurately identified. Only then is there any hope of identifying a clinoform which terminates up dip at such a point. To accomplish this, the SU must be well preserved and observable. However even if it is, it is virtually impossible to identify the point of start of offlap on the SU due to subsequent erosion on the SU during fall, Finally, even if such a point can be miraculously identified , the problem still remains how to recognize a "clinoform" which ties back to such a point.
The bottom line is that there is virtually no hope of identifying such a surface unless it can be demonstrated that such a clinoform (paleo sea floor) has recognizable characteristics that allow its identification in isolation (individual sections). This has never been done and, from a theoretical point of view, there seems to be little hope that such characteristic features even exist (nothing special happens sedimentologically speaking at such an event).
The insistence of ISSWG that such a surface is real and can be used to define sequence stratigraphic units is baffling to me, given the substantial field and subsurface experience of many of the members of ISSWG. I can only suggest that ISSWG members, who work in the real world and have at least an ounce of pragmatism in their bodies, do some serious soul searching on this one.

Specific Comments on the section "Discussion"

34) "For example the mapping of both "correlative conformities" of the depositional sequence model relies on the preservation of offlapping stratal terminations in the rock record and on the ability of to map the subaerial unconformity towards the basin margin."

This is an understatement because it needs even more than this to be able to recognize the hypothetical time surfaces. In fact, one would need a near perfect, continuous outcrop over many kilometres to have any hope of doing this, and even then, it would be doubtful if it could be achieved.
Given this, it would seem ridiculous to have such totally unrealistic requirements as the primary basis for defining sequence stratigraphic units in Stratigraphic Guides/Codes. This is certainly the current position of the ISSC TG.

35) "Nonetheless, inherent difficulties in recognizing any of the sequence stratigraphic surfaces do not reflect lesser value for modeling or exploration, but rather a lack of sufficient data"

The obvious problem, that the inherent difficulties in recognizing the two time surfaces are likely insurmountable, cannot be swept under the rug by claiming a lack of data. ISSWG has to come to grips with this major problem in their proposed deductive approach to unit delineation in sequence stratigraphy. We are

not talking about value for modeling but value for defining stratigraphic units which can be delineated in the real world.

36) " recent process-response insights into the shifts in the type of gravity flows that are expected to occur across the four main events of the base level cycle show that predictable facies changes may accompany the formation of correlative conformities and maximum flooding surfaces while the maximum regressive surface is likely to be cryptic"

These statements are purely deductive hypotheses for which there are no supporting empirical data whatsoever. The fact that ISSWG wants to base their sequence stratigraphic methods and units on such deductive speculations is somewhat unsettling.

Specific Comments on the section "Conclusions"

37) "First and foremost, personal preferences of applying one model over another (Figs. 2, 3) need to be separated from what is fair and reasonable to formalize in stratigraphic codes. Only the common ground that is acceptable by all can provide the unbiased solution to formalizing sequence stratigraphy to the satisfaction of all 'schools'."

Preferring one model or school over another is not the issue at hand. Such models and schools don't even exist; only a few different packaging schemes are out there for the two different approaches to sequence stratigraphy. The only question is, do the various stratigraphic bodies which make recommendations regarding stratigraphic practice (e.g. ISSC, NACSN) want to base their recommendations for sequence stratigraphy on an empirical approach or a deductive one. Given that the recommendations for every other material-based stratigraphic discipline are based on an empirical approach, one might assume the same approach would be desirable for sequence stratigraphy.

38) "the definition of sequence stratigraphic surfaces (their identification and mapping is more important than choosing which one(s) should be assigned the status of sequence boundary); and the definition of different genetic types of deposits"

I would hope everyone agrees with the above statement. If so, it then comes down to the question can be the two time surfaces (correlative conformities, clinoforms, paleo sea floors) be identified and mapped with any semblance of objectivity and with scientific methods. Given that this has yet to be demonstrated, the above stated "core" principle of ISSWG is at serious odds with what is being proposed. Such a major incongruency needs to be resolved.

39) "Significant departures from the standard sequence stratigraphic model that predicts particular responses to changes in base level are well documented"

One might expect that as more data are collected, more and more departures from the Jervey Model will be recognized. This is not surprising given the simplicity of any model compared to the complexities of the real world. This is just one more reason why an empirical approach rather than a deductive one is necessary for

recommendations on methods and unit delineation. Models change but empirically defined entities do not. The subaerial unconformity as recognized by Hutton over 200 years ago is the same as the SU we delineate today. How we explain the generation of an SU has changed a lot over the years but such interpretations have no bearing on the realness of the SU.