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Re.: Comments on the ISSC draft report on sequence stratigraphy

Dear Don,

The ISSC Report stems primarily from Embry's short course notes and his 2002 conference proceedings publication, so there is little doubt regarding who is the main contributor to this document. I therefore want to draw your attention to a few key aspects, which you are implicitly validating when signing this Report.

Please take the time to read ALL general comments and specific comments. The issues that I am raising are not necessarily listed in the order of their importance. The publication of this Report, in my opinion, can turn into an embarrassment for the ISSC, both at ethical level (e.g., the evident bias of the task group; excessive and selective criticism; misrepresentation of one's work) and scientific level (i.e., key scientific flaws). I will elaborate in my comments.

General comments:

I. A more appropriate title for this Report would be "**A Guide to Sequence Stratigraphy *in Outcrop and Core***". This is because all your criteria for the identification of sequence stratigraphic surfaces are exclusively based on outcrop and core data. Your presentation is not representative for sequence stratigraphy *in general*. The exclusive usage of outcrop and core data only gives us an *incomplete* image of the sequence stratigraphic framework.

One must realize that sequence stratigraphy is all about *integration* of all types of data, which *seismic* form an important (if not essential) component of. After all, sequence stratigraphy developed from seismic stratigraphy, *by adding* outcrop, core and well-log data to the seismic data. Let's keep in mind that sequence stratigraphy is different from lithostratigraphy or other types of stratigraphy that are exclusively based on the types of data you favour, and it is unique in its objective of analyzing *stratal stacking patterns*, and changes thereof, in a time framework. The recognition of some of these changes in stratal stacking patterns requires the usage of seismic data.

Once you ignore a major data set, your analysis only reveals *a portion* of the entire sequence stratigraphic framework, i.e. only those aspects that can be emphasised by using the particular type(s) of data you favor. For example, if you only use outcrop and core, you can only map those surfaces that can be recognized on the basis of *grain-size changes*. What about *stratal terminations*? Surely their identification is an important step in the workflow of sequence stratigraphic analysis, but I do not recall reading much about this in your Report. Is this because cores, well logs and most outcrops do not help much with this?

In some cases we simply do not have all the types of data that would allow the construction of a complete model, and therefore our model will be incomplete. That does not mean that those surfaces which we do not see on the particular data set that is available to us, do not exist (or are not “scientifically valid”). Our model is as good as the data available, and if our data base is incomplete the model will suffer accordingly. This reminds me of the ostrich who seems to think that the world ceases to exist once he sticks his head into the sand.

Instead of presenting these facts fairly (e.g., “we choose to define only those surfaces of sequence stratigraphy that can be recognized on the basis of such and such data”), you go on a rampage against the correlative conformities, to the extent of developing unrealistic deductive theories (e.g., your entire discussion on the BSFR) and a fictional story regarding model-driven versus data-driven sequence stratigraphy.

We must not forget that the major “consumer” of sequence stratigraphy is the industry. Those of us who work in academia or geological surveys have less access to seismic data, and therefore tend to overemphasize the importance of other data sets. This is exactly the case with this ISSC Report. However, the *majority* of practitioners in the stratigraphic community do integrate seismic data in their sequence stratigraphic analysis. How are the recommendations of your Report going to help (or be relevant to) these people?

II. Excessive criticism targets *selectively* other contributors to the field of sequence stratigraphy, and also my own textbook (Catuneanu, 2006). This criticism is based on misconceptions and misrepresentations of one’s work, and therefore it is inappropriate. Such selective criticism also gives the impression that political agenda overrides science in your Report.

The misrepresentation of one’s work is a recurring theme in your Report, and this is targeting on occasion my own textbook. I am not sure if this is an expression of honest misunderstanding or a deliberate action. I am inclined to think it is the latter because Ashton is well aware of my real position on some debated issues (as opposed to what is stated in your Report) from personal communications.

Please, also, use the word “scientific” more carefully, or do not use it at all. You are using the word “scientific” when you refer to the T-R model, and infer that just about everything else is not science. Ashton is notorious for this practice in his talks as well – quite unfair to a number of people who made significant contributions to the development of sequence stratigraphy. Fact is that your own Report contains enough misconceptions to make its science questionable.

I will elaborate on these aspects under “specific comments” with reference to page numbers.

III. **Model-driven versus data-driven sequence stratigraphy**:

The fundamental theme of this Report, which is the (self) validation of Embry’s “T-R sequence” (renamed in this Report as “depositional sequence”!) over all other approaches, is based on *one basic assumption*: that the correlative conformities used by all other models (with the exception of Galloway’s) are “abstract time surfaces”, as referred to in your Report (also labeled in your Report as “theoretical”, “hypothetical”, or “invisible”). Based on this assumption, an entire sand castle is built vis-à-vis “model-driven” versus “data-driven” approaches in sequence stratigraphy. In reality, the two types of correlative conformities that Ashton is referring to are *actual clinoforms* (as opposed to “abstract”, “theoretical” or “hypothetical” surfaces) with their own low diachroneity (as opposed to "time surfaces"). They do correspond to changes in depositional trend (aggradation to erosion on the continental shelf and vice versa, which trigger changes in stratal stacking patterns across these surfaces offshore, thus providing observable, mappable criteria), and hence are *mappable* (as opposed to "invisible") on seismic lines. According to your Report (e.g., p. 13), seismic data are acceptable for an inductive (data-based) approach – and I think everybody agrees with this. My textbook provides a comprehensive discussion on the *nature* of these surfaces (real clinoforms), their *diachroneity along dip and strike*, and *mappable criteria*. I think this is worth reading before perpetuating this decades-old idea of "abstract time surfaces". E.g., read at least Chapter 7 for a discussion on the timing and diachroneity of all types of sequence stratigraphic surfaces, including correlative conformities, and also Chapter 4 for definitions and field examples/case studies.

There is a profound misusage of terms in your definition of "model-driven / deductive" versus "data-drive / inductive" sequence stratigraphy. In reality, all current approaches are data-driven, as mapping is based on data and criteria are clearly defined. The deductive versus inductive discussion is only relevant to the underlying assumptions regarding the mechanisms responsible for relative sea-level changes, but early shortcomings (such as the global-eustasy model) have been understood and fixed within the last decade(s). As such, the distinction between "model-driven" and "data-driven" sequence stratigraphy, as presented in your Report, is misleading and must be discarded.

People working with seismic lines or volumes have published *data* extensively (including seismic examples of correlative conformities), thus promoting a data-driven approach. Once one realizes that all surfaces of sequence stratigraphy, including the correlative conformities under scrutiny, are real, mappable surfaces (actual clinoforms and not some hypothetical time surfaces), then the entire core discussion of the ISSC Report on the "model-driven" versus "data-driven" sequence stratigraphy is falling apart. Implicitly, the validation of the T-R model as the only "*scientific*" approach to sequence stratigraphy, worth of the Seal of Approval of the ISSC, is also falling apart.

One thing must be clear, that we all want to base sequence stratigraphy on *observable, mappable criteria*. The usefulness of some types of data over other types of data in the process of mapping will evidently vary, but this is a completely different issue. If one does not have the right set of data in order to map a surface, it does not mean that surface does not exist! For example, if one works only with well logs or outcrops, the mapping of the regressive surface of marine erosion is possible, but the mapping of correlative conformities is difficult. If one works only with seismic data, the mapping of correlative conformities is possible, but the mapping of the regressive surface of marine erosion is difficult because this surface comes too close to the subaerial unconformity and the two may be amalgamated within a single reflection. But if one *integrates* well logs, outcrops and seismic data, then *all surfaces are mappable*.

I have noticed that you are particularly critical of Posamentier's approach to sequence stratigraphy, one of your favourite "deductive" targets, but it is apparent that you are not aware of his practical reasons for his choices. Fact is that on seismic lines, Posamentier's correlative conformity (i.e., the basal surface of forced regression) is one of the *most prominent surface* in the marine portion of the basin (and I am not only talking about a potentially diachronous facies contact at the base of the submarine fan complex, but also higher up on the slope and shelf where its diachroneity is as low as that of a maximum regressive surface along a dip line – e.g., see your Figure 7), and it may be *easier to map* than the maximum regressive surface or the correlative conformity *sensu* Hunt and Tucker (1992). This is why Posamentier chose the basal surface of forced regressive as his correlative conformity, *based on observable, mappable criteria*. This is data-driven approach, and not model-driven as you state so emphatically in your Report. Point is that we need to be more flexible and try to understand the reasons behind each approach before going on a rampage of denigration against one's work.

IV. **Type 1 versus Type 2 sequence boundaries**:

I also note a serious misinterpretation (or overinterpretation?) in your Report with respect to the definition of type 1 and type 2 sequence boundaries (copy-and-paste from Embry, 2002). Normally this discussion would not matter, because the usage of type 1 and type 2 sequence boundaries was abandoned anyway (at least by its proponents: see discussion in Posamentier and Allen, 1999). However, you use these concepts to classify what you call "deductive models" into "Type 1 versus Type 2" categories. Your interpretation of the meaning of type 1 and type 2 sequences is surprisingly erroneous: when these concepts were proposed, the emphasis was on the physical attributes of the *unconformable* portion of the sequence boundary (major versus minor erosion on the shelf), and not on the timing of the correlative conformities, as you infer. In fact, Posamentier and Vail (1988) only talk about type 1 and type 2 *unconformities*.

A more detailed discussion about types 1 and 2 sequences follows under "specific comments".
………………………………………………..

There are other key *scientific inaccuracies* in your Report, some aimed to trash the "competition", and some to breathe life into the sometimes theoretically impossible boundary of the T-R sequence. I will provide examples of such inaccuracies under my "specific comments", with reference to page numbers.

Please keep in mind that I do not have a personal agenda in this process of standardization. There is no doubt that *all models* have their own merits and pitfalls, which is why we have a spectrum of approaches today, and that none of them can offer the perfect solution for all case studies. I am therefore looking for that common ground which defines the foundation of sequence stratigraphy that is worth standardizing. Flexibility is a real virtue in sequence stratigraphy, and I think this is the reason why my textbook has met with reasonable success – it was Elsevier's 2006 best seller, with a number of universities having adopted it for teaching, and as a result with a request for a second edition from the publisher. The only reason I got involved in this standardization effort last year is because many potential readers of the ISSC publications simply do not know what is true and what is false in your claims, and they may fall for your statements. Not only that this would be unfair to many contributors to sequence stratigraphy, but it would do more harm than good to the stratigraphic community by taking sequence stratigraphy into a new dark age of misconceptions and misrepresentations of facts. This is why I have formed the **International Working Group on Sequence Stratigraphy** in 2006, which consists of 20 of the most renowned stratigraphers in the world, to provide a balanced and unbiased approach to standardization. The IWGSS includes Vitor Abreu, Janok Battachariya, Mike Blum, Robert Dalrymple, Pat Eriksson, Chris Fielding, William Fisher, Bill Galloway, Martin Gibling, Kate Giles, John Halbrook, Robert Jordan, Christopher Kendall, Andrew Miall, Jack Neal, Henry Posamentier, Brian Pratt, Keith Shanley, Ron Steel and Charles Winker. As you probably know, these people belong to different "schools" of thought, which means that *any document we can write together will be entirely unbiased*. This is in sharp contrast with the ISSC task group on sequence stratigraphy, which Embry has formed to include some of his closest friends in the T-R group.

Specific comments:

Abstract:

- only the **surfaces used in the T-R model** (Embry's own, and supported by his close friends/followers Johannessen and Beauchamp) are validated by this Report. The bias of this ISSC task group is evident.
- "**hypothetical time surfaces**": no such thing. The two surfaces the Report refers to (i.e., "basal surface of forced regression" and "correlative conformity") are real clinoforms, with their own low diachroneity (see Chapter 7 in Catuneanu, 2006 for a full discussion). The issue of *mappability* is different from the issue of being *hypothetical versus real*, and it is *data-dependant*. See further discussion below.
- the term "**depositional sequence**" was coined by the Exxon group with a specific meaning, i.e. with correlative conformities that form *independently of sedimentation* (as opposed to the T-R sequence boundary). You cannot simply hijack the term "depositional sequence". One of the problems with sequence stratigraphy is the usage of identical terms with different meanings by different schools. We are supposed to sort out this mess; instead, you just add to the confusion by proposing to use the *same term* for yet another stratigraphic unit (i.e., by renaming the "T-R sequence" as "depositional sequence").
- "**unrecognizable time surfaces**": unrecognizable only if one does not have access to the right data set. There are published seismic examples of these surfaces. They are not "time surfaces" either. *These are fundamental misconceptions that undermine the validity (and the science) of this entire Report*.
- "**not scientifically valid**": delete reference to "scientific" or "unscientific". This conclusion of the Report is based on flawed "science" (either honest misunderstanding, or purposely avoided published literature that invalidates key conclusions in this Report).
- "**lowstand, highstand and forced regressive systems tracts**" are portrayed in this Report as "not scientifically valid" – same comment as above.
- empirically-based methods (or "**data-driven**", as referred to in the Report) versus theoretically-based concepts with no empirical support (or "**model-driven**", as referred to in the Report): delete such inference, see "General comments" above.

Pages 3, 5. Embry (2002): this is a conference proceedings publication, which is not peer-reviewed. I am not sure if it is appropriate to base so many key points in your Report on such publication.

Page 3: "**practical and scientifically valid methods and terminology** …". I am sure Dr. Cita did not mean to provide one particular group (or sequence stratigraphic "school") with the opportunity of using ISSC as a vehicle for advertising or validating their own ideas to the rest of the stratigraphic community. Dr. Cita may or may not be aware of the debates in the field of sequence stratigraphy, and of the differences that exist between different "schools". As far as I know, Dr. Cita *only appointed Embry* to form a task group, and not the entire task group as inferred in your Report. Embry was the one selecting the members of the task group, which ended up consisting in majority (3 out of 5 at least, for what I know) of his own group of friends and/or followers.

Page 4. Delete reference to the books listed in the second paragraph. It is inappropriate, unfair and misleading. In my own book (Catuneanu, 2006), I am discussing *all* approaches, including the one that you advocate, and *I provide field examples/case studies for every single concept* mentioned in the book. It is therefore inaccurate to label my work as the result of a deductive model that lacks empirical observations. Moreover, later in your Report you cite my work (e.g., on pages 31, 33 – reference to situations where the RSME may not form, 35, 40, 44, 45, 47, 49, 54, etc.) as a source for improved understanding on a number of key aspects. It is therefore inappropriate to state that this work "negatively affects the understanding and application of sequence stratigraphy". Also, do you really mean to say that contributions brought by people like Posamentier have hampered the development of sequence stratigraphy? This is ridiculous, since all Embry's contributions (perhaps with the exception of his ideas on hierarchy) feed on concepts developed by others, including Posamentier. Your statement looks a lot like personal attack based either on political agenda or poor understanding of what these works are all about.

Pages 6, 7. References to Miall (2004) and Miall and Miall (2004) are misleading. When discussing inductive versus deductive approaches, Miall referred to the deductive models that attempt to identify the underlying geological controls on base-level changes and sequence development, including the global-eustasy model of Vail and the associated global-cycle chart. The citations in this Report are out of context and twisted to infer that the "two approaches have led to two different sets of stratigraphic surfaces and consequent units for sequence stratigraphy". This is far from reality. There are no different sets of stratigraphic surfaces, one inductive and another one deductive. They are all data-based, and part of the same sequence stratigraphic framework. You choose to emphasize only part of this framework. Field examples are available for all surfaces.

Page 8. "The deductive or model-driven approach … based on *a priori* input parameters such as rates of sediment supply and base level change". I believe you are here referring to forward modeling. People do inverse modeling as well, and there is nothing wrong with such numerical exercises. Forward modeling is used to simulate stratigraphic architectures under specific circumstances, but this has nothing to do with the definition of sequence stratigraphic surfaces.

All sequence stratigraphic surfaces are defined on the basis of stratal stacking patterns observed on one set of data or another. Among all types of sequence stratigraphic surfaces, the maximum regressive, maximum flooding, and transgressive ravinement surfaces are the best examples of stratigraphic contacts whose timing depends on the interplay between the rates of sediment supply and base-level change *at the shoreline* (the latter part of the sentence, in *italics*, is a key aspect of sequence stratigraphy, which you fail to recognize in your Report – see your discussion on the BSFR).

Page 19. "**The type 2 sequence boundary** …": statements in this paragraph are incorrect. Posamentier and Vail (1988) did not indicate that the correlative conformity of a type 2 sequence boundary forms at the start of base-level rise. Both type 1 and type 2 correlative conformities are shown to from at unspecified points in time *during eustatic fall*. In fact Posamentier and Vail (1988) did not even focus on correlative conformities, but rather on type 1 versus type 2 *unconformities* (see general comments). They also stated that these unconformities may be equivalent (point that is further emphasised by Posamentier and Allen, 1999), and that the difference between them rests in the amount of erosion on the shelf (*major versus minor*). This difference was explained by Vail et al. (1984), using the ratio between the rates of eustatic fall and subsidence *at the shelf edge*.

Page 19. " … the SMST of a Type 2 sequence represented only regressive strata deposited during early rise". This is also incorrect. Both systems tracts (LST and SMST) were supposed to include deposits of eustatic fall and rise. The difference between the architecture and type of deposits of the type 1 versus type 2 sequences and systems tracts was explained by Vail et al. (1984) and Posamentier and Vail (1988) as a function of the *shoreline position* during the formation of type 1 versus type 2 unconformities (*below or above the shelf edge respectively*). This is why the type 1 sequence (with an LST) was shown to include a deep-sea fan (high sediment supply to the deep-water setting – shoreline below the elevation of the shelf edge; *major erosion* on the shelf), whereas the type 2 sequence (with a SMST) was not supposed to include a deep-sea fan (low sediment supply to the deep-water setting – shoreline above the elevation of the shelf edge; *minor erosion* on the shelf).

If your theory was correct, the Hunt and Tucker model (which you classify as a "Type 2 sequence") should have not included deep-sea fan deposits. In reality, the entire point was to shift the position of the CC from the base to the top of the deep-sea fan, while using the type 1 unconformity (which allows the formation of the deep-sea fan complex) as a portion of their sequence boundary.

Page 19. "The significant differences between a Type 1 and a Type 2 sequence boundary … have not been understood or appreciated by many subsequent workers up to the present day (e.g., Catuneanu, 2006)". Delete this, it is ridiculous… *You* have misunderstood the types 1 and 2 sequence boundaries, and built an entire science-fictional story on your interpretation. This is why it is "inexplicable" to you why Posamentier and Allen (1999) "claimed that the two boundary types are equivalent". Perhaps you should contact Posamentier to clarify these issues.

Bottom line is that the *major versus minor erosion* on the shelf that defines type 1 versus type 2 *unconformities* has nothing to do with the variety of depositional sequence models (Posamentier's versus Van Wagoner's versus Tucker's), as you infer in your classification of "deductive models". The types 1 and 2 *unconformities* were applicable to all these models, as the distinction between types 1 and 2 sequence boundaries was all about the nature of the *unconformity* and not about the timing of the correlative conformity!

Page 21. "…**flawed nature of the 1988 model**…". Henry Posamentier never claimed that the basinward termination of the subaerial unconformity joins with his "correlative conformity". His interpretations, based on real data, are not "illogical" – if you take the trouble to understand his reasons (and these are *practical* reasons, based on the *mappability* of different surfaces on seismic lines, as I explained in the "general comments"). This Report does not make any constructive attempt to understand Posamentier's approach, or anything else for that matter that is outside of the T-R model. It is more a matter of principle that the ISSC should make the effort of looking at all contributions to sequence stratigraphy from an unbiased platform. It is also unbelievable how offensive is the language you use in what is supposed to be an unbiased ISSC document. This seems even more out of order since your own Report is permeated by superficial science.

Page 21. Next paragraph: "… **they apparently didn't realize it**, ….". Hunt and Tucker (1992) did not use the type 2 sequence boundary in their revised model. This is why they did not make reference to it, and not because they "didn't realize it". The misunderstanding is in your Report – it is a copy-and-paste error from Embry (2002).

Page 24, first paragraph: "These two models basically represent a revised Type 1 model and a revised Type 2 model …". This is incorrect (see above).

Page 24, second paragraph: here you are talking about historical priority, i.e. that Naish and Kamp (1997) should have not used the term "regressive systems tract" since this term "was already in use for an entirely different type of sequence unit (Embry and Johannessen, 1992)". You are making exactly the same mistake in this Report, by renaming the "T-R sequence" as "depositional sequence". The latter term is already in use for an entirely different type of sequence unit. Is thus appears that your Report is yet another "fine example of thoughtless nomenclature", if we were to use the same language that you apply to Naish and Kamp (1997).

Page 25, Figure 7: your comment in the figure caption about "the lack of any criteria for recognizing a BSFR" is inaccurate and shows your lack of experience with seismic data. On seismic, the BSFR (including the portion landward of the submarine fan, which you do not indicate as a facies contact) is often the most prominent mappable surface among all other sequence stratigraphic surfaces in the marine portion of the basin. This is the reason why Posamentier uses it as his correlative conformity.

Page 26, first paragraph: I believe that Galloway used the same systems tracts as Posamentier. Yet, the former approach is described in your Report as *data-driven / inductive*, whereas the latter is labeled as *model-driven / deductive*. Once two models use the same systems tracts (and implicitly map the same set of surfaces as systems tract boundaries), then they are on par, irrespective which ones of these surfaces are assigned the status of sequence boundary. Your criticism is clearly selective.

Page 27, Figure 8: first off, these are not the only empirical models. All current models are empirical, as all surfaces are mappable on one set of data or another. Deductive were only the early assumptions regarding the underlying controls behind the changes in relative sea level. Part B of this figure is highly idealized in the favor of the T-R model. In reality, one can drill and find both the MRS (younger) and the SU (older) in the same borehole, which makes this combination of surfaces an "impossible boundary". There are plenty of published case studies that show this stratigraphic relationship.

Page 30, first paragraph: the empirical recognition of sequence stratigraphic surfaces is not limited to outcrop sections or core, as you imply. Seismic data and well logs are equally legitimate (valid and acceptable) data sets. Sequence stratigraphy did evolve from seismic stratigraphy – see my "general comments". Same limitation is carried over in the list of criteria on the same page.

Page 33: "Plint (1988) interpreted the surface to be formed by scouring … and such a surface is also part of the proposed deductive models (e.g., Catuneanu, 2006)". Why are you citing my book here? Am I proposing any ("deductive" or otherwise) model? All models I am discussing were published already, and as a balanced approach should be, I am presenting *all* concepts, *with field examples*. Ironically, only three lines below this critical citation you use insights from my text to improve your own understanding regarding the conditions required by the RSME to form. The RSME becomes a systems tract boundary (and hence a surface of sequence stratigraphy) where it reworks the underlying BSFR, and this was published by Plint and Nummedal (2000). If you have a problem with this concept, and want to refer to it in a critical manner, reference the original source. Your manner of referencing is misleading.

Page 38, top paragraph: you define the maximum regressive surface as separating "coarsening upward strata from fining upward strata" in both shallow-water and deep-water settings. This is only correct for shallow-water settings, but it is incorrect for deep-water settings. There is a major difference between the processes and products of shallow-water versus deep-water settings, and this aspect is explained in detail in my textbook, in Chapters 4, 5 and 6. I made Ashton aware of this issue, but he chooses to ignore this published work because it creates an obvious problem for the T-R model. The top of the coarsening-upward trend in the deep-water setting marks the position of the correlative conformity (highest input of, *and coarsest*, riverborne sediment into the deep-water environment at the end of base-level fall; commonly at the top of high-density turbidites/frontal splays), whereas the *younger* MRS is unrecognizable, being part of the overlying fining-upward trend (commonly within a package of leveed channels related to lower-density turbidity flows) – read in my book for full details (e.g., summary diagrams and associated discussions and examples in Chapters 5 and 6). As such, *the recognition problems of the CC and the MRS are reciprocated between the shallow-water and the deep-water settings*. The MRS in the deep-water setting does not satisfy your second criterion on page 30.

This is a very important point with respect to the *mappable criteria* for sequence stratigraphic surfaces, because it shows that mappability varies not only with the data set (e.g., seismic versus well logs versus outcrops), but also with the depositional setting. In a deep-water setting, grading (coarsening- versus fining-upward) works to identify the correlative conformity, while the MRS becomes "invisible" if we were to follow your criteria. In reality, there is a chance that the MRS can also be mapped based on seismic data. The only scenario that allows the MRS in the deep-water setting to be mapped on the basis of grading is where the MRS and the CC coincide, i.e. the LST is missing all together. Please do not invoke your "non-actualistic /sinusoidal curve" theory to explain that lowstand systems tracts never exist. You are forgetting about sediment supply. There is a huge body of evidence that lowstand systems tracts are present in the rock record (perhaps with the exception of the Sverdrup Basin), and nobody denies that the base-level curves are asymmetrical – e.g., read the caption of Figure 3.19 in my textbook. The sine shape of the reference curve that is shown in any textbook is simply *generic* (as there could be infinite variations of shapes in the real world, depending on the driving mechanisms on cyclicity), and nobody is building a model on the basis of it. I recall seeing a beautiful reference *sine curve* in Embry (2002), and it was nothing wrong with that particular aspect in that paper.

Page 38, same first paragraph: "The MRS sometimes is present in nonmarine clastic strata … Embry, in press)". You even provide field criteria for its recognition. Once you accept that the MRS exists in fluvial deposits, you implicitly *validate the stratigraphic relationship in your Figure 14*, which is actually the norm in many cases. This contradicts the bold statement you make in the caption of Figure 14 (page 58), where you claim that such stratigraphic relationship was never documented with field data. See also my comments on your Figure 14.

Page 38, second paragraph: "In deeper water, high subsidence areas, the change from shallowing to deepening may not coincide with the MRS (Vecsei and Duringer, 2003)". The paper that you need to reference here is Catuneanu et al. (1998) – this is where Ashton learned for the first time about the offset between grading and bathymetry (as a reviewer of my paper).

Page 39, middle paragraph: Referring to the "… diachroneity parallel to the shore …", "Previous analyses of the relationship between the MRS and time relied on a non-actualistic, sinusoidal base level curve and came to the interpretation that such a surface can be quite diachronous (Catuneanu et a., 1998; Catuneanu, 2006). However, when actualistic base level curves are employed (Embry, in press) …".

The reference to my work is misleading – my modeling did not *rely* on any particular shape of the reference curve. It will be interesting to see what kind of modeling Embry (in press) did to demonstrate his points or whether your statements are simply qualitative (deductive). As explained above, all reference curves are *generic*, and the shapes do not even matter when you start inputting the effects of sediment supply. The shape of the reference curve of base-level changes will actually change along strike. More importantly, when we talk about strike diachroneity, it is the change in subsidence rates along strike, as well as the change in sediment supply along strike, that control the degree of diachroneity of the MRS (and of the MFS for that matter; e.g., Martinsen and Helland-Hansen, 1995). Cobban's ammonite data in Montana document diachroneity of transgressions and regressions within the range of ammonite zonation (i.e., > 0.5 My diachroneity). The diachroneity along strike of the correlative conformities is less because their timing is independent of sediment supply (read Chapter 7 in my textbook for full details).

You tend to use very qualitative/equivocal terms throughout the Report (e.g., "usually low", or "approximate time barrier"), showing some weakness in the quantitative understanding of the actual numbers that are involved. Numerical modeling, as well as process sedimentology (e.g., what kind of processes are involved in the deep-water environment during the various stages and events of the base-level cycle, in contrast to shallow-water processes) would enhance your insights significantly. You use data from other publications, but *selectively*, showing or citing only those parts that favor your theory. Have a look at Kenneth Miller's isotope work on sea-level cycles on "passive" margins – many of these cycles are more symmetrical than you infer with your actualistic versus non-actualistic qualitative (deductive?) theory. Same thing you can observe on curves derived from a combination of data sets (e.g., Galloway, 1989). In fact, as explained above, the shape of base-level cycles does not even matter. You are underestimating sediment supply, which explains the formation of *thick lowstand topsets* in basins characterized by active tectonism, even though, quite possibly, subsidence rates at the onset of base-level rise may have been high. Nevertheless, the LSTs are there, and every time this happens the MRS (younger) does not meet the SU (older), making the T-R sequence boundary theoretically (and practically) impossible.

Page 40, top paragraph: technically, Helland-Hansen and Gjelberg (1994) used the term "surface of maximum regression". At the same time, and independently, I started to use the term "maximum regressive surface" as part of my PhD work (term printed in my thesis – defended 1996). I proposed the term "maximum regressive surface" to Ashton around 1994, and I remember him reacting as if he heard that for the first time.

Page 41, first paragraph under BSFR: "… have used the BSFR as the time at the start of base level fall at the shoreline and thus the start of forced regression at that locality. This revision was not well conceived because during much or even all of base level fall at the basin edge …".

Your statement implies that your reference curve of base-level changes is taken at the basin edge, even though the shoreline may be tens or hundreds of kilometres away from the basin edge. How is this reference curve relevant to the definition of sequence stratigraphic surfaces, when the maximum flooding surface, for example, marks the timing of maximum *shoreline* transgression, irrespective of what may happen at the basin margin (e.g., subsidence in the basin center may drive the transgression of the shoreline, whereas the basin edge may experience uplift)? These issues are explained in detail in my book, e.g. read Chapter 7.

Given the variability of subsidence rates along both dip and strike within a basin (or even the manifestation of coeval subsidence and uplift), what happens at the basin edge, or in the basin center, may not matter for the timing of any sequence stratigraphic surface, including the BSFR. The term "basal surface *of forced regression*" makes specific reference to that particular type of *shoreline* shift (i.e., forced *regression*), and has nothing to do with the tectonism of the basin margin. Your statements that "the start of forced regression occurs at many different times during an interval of base level fall", or that "forced regression may not even occur during some times of base level fall…" make no sense, and this is because your reference curve is taken at the wrong location within the basin. The relevant reference curve is in the *shoreline* area (and not at the basin margin or basin center), because all sequence stratigraphic surfaces (and systems tracts) are defined in relation to the *shoreline* shifts. For example, a MRS marks the turnaround from *shoreline* regression to transgression, irrespective of the tectonism that may affect the basin margin, and even though the basin center may experience continuous relative rise at the time. Please read Chapter 7 in my book, so I do not have to go on with more explanations here.

You are missing entirely the relevance of *downstream versus upstream controls* on fluvial processes and the formation of sequence stratigraphic surfaces such as the SU (or any other). You need to catch up with the work of Mike Blum and many others on downstream (closer to the shoreline) versus upstream (closer to the basin margin) controls on sequence stratigraphic architecture, and you will understand why it is wrong to take the reference curve of base-level changes at the basin margin. A lot of this material is summarized in various sections of my book.

You state that "This revision was not well conceived …", when your own understanding of these issues can be improved significantly. This is ridiculous.

Page 42, second paragraph: "Plint and Nummedal …" etc… read the entire paragraph. How do you know there are "many such clinoforms" along strike, since you claim that they are "invisible"? It sounds a lot like a deductive theory, since I know you did not do any modeling. Yes, there is diachroneity along strike because of variations in subsidence rates. The diachroneity of maximum regressive and maximum flooding surfaces along the same transect will be even higher, because of additional variations in sediment supply.

Since when does a concept have to be old in order to qualify as "empirical"? What if we were to realize in 2010 that other surface(s) or stratigraphic units can be mapped in the rock record, perhaps based on new technologies? Would they not be empirical because they have not been around for long enough? The RSME has only been recognized in 1988, same year when Jervey has published his work on "accommodation". Yet, you consider the RSME an empirical surface, but you describe surfaces emphasized by Jervey or Posamentier in the same year as deductive because they do not have "empirical roots". Your judgement is inconsistent, and you should ask Jervey or Posamentier whether they have seen any *data* before publishing their models.

By the way, you have nothing positive to say about Jervey's work (his name in your Report is always associated with the non-scientific "deductive" models), even though the concept of "accommodation" is arguably one of the most important contributions to sequence stratigraphy.

Page 42, next (third) paragraph: "… it is impossible to recognize…" because "…it occurs within a succession of coarsening-upward strata and no sedimentological variation or change in grain size trend…". No one is trying to identify offlap on well logs or core… If you were to work in a frontier basin with seismic data, how are you going to recognize the top of a coarsening-upward trend? Please read the "general comments" above.

Page 42, same paragraph: "Catuneanu (2006, p. 129) admits 'the BSFR … has no physical expression in a conformable succession of shallow water deposits". This is a citation out of context (which I would call *misquotation*, because it misleads the reader), upon which you draw the bold conclusion that "thus it is widely accepted that the BSFR is a theoretical surface…". This cannot be further from my intended meaning. I do provide seismic examples for the BSRF, showing it as a *real* clinoform (as opposed to a theoretical surface). If you read the entire paragraph in my book, from which you have selected only a portion to make your point, you will see that the lack of physical expression is "when working with well-log data". Also, do not use the word "admits", as it infers that the BSFR is my own concept which I am trying to defend. I find this misquotation / misleading practice rather unprofessional. Same thing happens when you discuss the concepts of high- and low-accommodation systems tracts later in the Report.

Page 43, Figure 11, caption: " … hypothetical and unrecognizable BSFR and CC which are abstract time lines. … Proposals to use such 'invisible surfaces' … have no credence". These two surfaces are not "hypothetical", they are actual clinoforms. They are not "unrecognizable" or "invisible", if you have the right set of data. As you indicate in this figure, the MRS is recognized as the top of a coarsening-upward trend. Following your logic, how do you recognize the top of a coarsening-upward trend on a seismic line, if you do not have well logs or core? Are you going to use the seismic definition for a MRS that I provide in my book, thus validating seismic data as acceptable for an inductive approach, or are you going to tell the company that they need to drill a well before a MRS can be mapped?

Embry (2006 – CSPG presentation) did make an attempt to interpret a seismic line, without any well-log or core support, which makes me think that seismic data must be acceptable after all for constructing a sequence stratigraphic model.

Page 44, second paragraph: "As discussed above, the BSFR of … Catuneanu (2006)…". Once again you are implying that this is my own concept. If you want to make a reference, you need to go back to the original work that proposed this surface / term.

Your discussion of the relationship between the diachroneity of the BSFR and the variations in subsidence rates across the basin is completely off track. If your theory was correct, then the timing of ALL sequence stratigraphic surfaces would be affected by these variations, and so they would ALL be highly diachronous. That of course is not the case. Read the section on shoreline shifts versus grading versus bathymetry in my Chapter 7. The variations in subsidence rates *along dip*, which you are talking about in this instance, only affect the bathymetry in the basin, but not the timing of sequence stratigraphic surfaces (as, for example, there is only one moment in time when the shoreline starts its forced regression along any dip-oriented transect – triggering the change in stratal stacking patterns from aggradation & progradation to degradation/ downstepping & progradation; same goes for all other three events of the base-level cycle). The variations in subsidence rates along strike do affect the timing of ALL sequence stratigraphic surfaces, as discussed above. See also Chapter 7 for full details.

Page 44, last paragraph under BSFR: You are limiting your criteria to "well exposed outcrops and core". Remember that sequence stratigraphy is not lithostratigraphy. Depositional trends, which according to your own definition form the object of study of sequence stratigraphy, can be observed on seismic lines. You cannot eliminate seismic data from sequence stratigraphic analysis simply because you do not work much with such data. More than 50% of the practitioners of sequence stratigraphy around the world do their work with seismic data. How are your recommendations useful to these people?

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I am now starting to skip through your Report. The conceptual flaws that I have pointed out so far affect evidently other sections of the Report.

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Page 45: you cite my text to make your point, but conveniently omit to say that I do provide examples for the correlative conformity on seismic lines.

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Page 49, your Summary of Surface Evaluation: needs to be re-evaluated in the light of the above.

I am starting to realize that your Report attempts to be a "**Guide to Sequence Stratigraphy in Outcrop and Core**", notwithstanding erroneous assumptions that still need to be fixed regarding the universal applicability of the T-R model. Your Report is not representative for Sequence Stratigraphy in general. This is because all your criteria for surface identification require outcrop or core, as you state. Nothing wrong with such a publication, excepting that it is not what the mission of the ISSC task group is all about. The exclusive usage of outcrop, core and well-log data only gives us an incomplete picture of the entire sequence stratigraphic framework. See "general comments".

Page 50, your summary: For both BSFR and CC: "deductive time surface" – they are neither deductive nor time surfaces. "…and they cannot be recognized by empirical analysis" – Of course they can, you just need to use the appropriate type of data. This "analysis" of yours leads to the discussion of what units of sequence stratigraphy are acceptable and which ones should be "rejected". No surprise, only the systems tracts of the T-R model are shown to be acceptable.

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Page 54, section on ***Depositional sequence***: You propose to rename the T-R sequence as "depositional sequence". This is wrong at several different levels – see my comments on your Abstract and on page 24 of your Report (above). The term "depositional sequence" is already in the literature with a completely different meaning, and this is why you used the term "T-R sequence" in the past. Depositional sequences have correlative conformities whose timing is *independent of sedimentation*, as it is defined in relation to changes in base level (either the onset or the end of base-level fall). You reject the correlative conformity on page 56 (top paragraph) based on poor arguments. You keep saying that the onset of rise surface cannot be recognized in *outcrop or core*, but you forget that sequence stratigraphy includes the analysis of seismic data as well.

If we rename the "T-R sequence" as "depositional sequence", how should we call the sequence used by Posamentier for example? Should we use the same name for different stratigraphic units, or should we just unilaterally discard everybody else's work?

Page 55, Figure 13. It is your *self assessment* that Embry's combination of surfaces for a sequence boundary "has no obvious drawbacks", while the correlative conformities of the real "depositional sequence" "lack any objective criteria for recognition". Great job of self validation, but many of your peers have a different opinion. As explained before, there are objective criteria for mapping the correlative conformities.

Page 56: "Thus an unconformable portion of the shoreline ravinement is a correlative surface of the SU". This is incorrect. You are talking about a younger unconformity cutting through an older unconformity. That makes a composite unconformity, but they are not correlative.

Page 57, middle paragraph: "On the basis of the Jervey Model, the MRS and the SR do not **theoretically** join with the basinward termination of the SU … Catuneanu, 2006". You are misquoting my text again. I have not based my comments on the "Jervey Model", but on the countless number of published case studies; so there is nothing "theoretical" about this. In the next paragraph you become equivocal about these issues, saying that your theory is supported by "many empirical observations". "Many" (or even "most") is not something that allows us to generalize, especially when there are *many other empirical observations* that demonstrate the contrary. I have discussed both possibilities in my textbook, so your referencing should be fairer – it is misleading to cite me in a "theoretical" context, when in fact I present *case studies* for every concept I discuss.

Page 58, Figure 14. Last sentence in the figure caption: "There is no empirical or theoretical support for sinusoidal changes in base level and the above described stratigraphic relationships [NB: this stratigraphic relationship refers to a MRS that is separated from the underlying SU by a lowstand systems tract, making thus possible to intercept both surfaces in the same borehole – as both surfaces are part of the same composite boundary of the T-R sequence, this stratigraphic relationship invalidates the T-R model, whose boundary becomes theoretically impossible] have never been documented with actual data". First of all, the base-level curve does not need to be sinusoidal for such stratigraphic relationship to occur. Secondly, this stratigraphic relationship is *common*, and documented in numerous case studies worldwide, based on various data sets including seismic, well logs, outcrops and core. Excellent case studies have been referenced by Posamentier and Allen (1999), Yoshida, Willis and Miall (1996), Miall (1997), etc, etc. Even

Embry (in press) seems to discuss (from what you state in this Report) criteria for recognizing a MRS within a fluvial succession, above the SU.

Page 59, last paragraph: the situation is not rare, and the CC is mappable. What you are stating in this paragraph is that if the T-R model fails to work in a particular case study (i.e., where lowstand topsets are preserved in the rock record), then sequence stratigraphy cannot be applied altogether! This sounds slightly biased. This is the reason we have several different approaches in sequence stratigraphy, i.e. because none will work in all circumstances, so one needs to maintained flexibility and an open mind.

Page 60, Figure 15: every time a lowstand systems tract /topset is documented above the SU (e.g., Yoshida et al., 1996), the model in this figure fails. Also keep in mind that the lowstand topset *onlaps the SU*, so it is wedging out towards the basin margin. This means that if this topset is preserved (as documented in many case studies) at some distance from the shoreline, then it can only be thicker near the coastline at the point of maximum regression. Your Figures 15 and 16 present only the particular situation where lowstand topsets are missing entirely. The usage of the MRS as a depositional sequence boundary is totally inappropriate. The real correlative conformities of the depositional sequence are clearly defined in the literature, and they are different from the MRS.

Page 63, Figure 17: within a submarine fan complex, the MRS is the most cryptic surface of all (see comments above).

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Page 65, last paragraph: once again, the reference to my book is inaccurate. You are implying that I draw the systems tract boundary between fluvial LST and the overlying TST at the diachronous facies contact that develops between a backstepping estuary and the age equivalent (transgressive) fluvial facies. This is not at all what I am saying in my text. I have discussed this issue in detail in my book, and I pointed out where the systems tract boundary (MRS in coastal and fluvial settings) is. Your style of referencing one's work is highly inaccurate and misleading. Even if you did not go through my book to understand my position on this, Ashton was well aware that I am not doing what the Report is implying, from personal communications. I can only interpret your choice of words and inferences in your Report as deliberately misleading, which is rather unprofessional. In this context of the LST–TST boundary, you reference my book saying "… an unacceptable sequence stratigraphic practice", which is a gross misrepresentation of my work.

Page 66, Figure 19: see my earlier comments regarding your interpretation of type 1 versus type 2 sequence boundaries, and actualistic versus non-actualistic curves. This table misrepresents both Posamentier's and Tucker's work. Before the late 1990's, everybody was trying to find both type 1 and type 2 sequence boundaries (i.e., *unconformities*), *irrespective of the model of choice* – hence, you cannot separate models into these categories. Your classification of approaches into actualistic and non-actualistic is highly artificial (i.e., "non-actualistic"): nobody meant to say that the base-level cycles are perfect sine curves (see earlier comments), as *all reference curves are generic*. Otherwise, Embry (1993, 1995, 2002) would qualify as "non-actualistic" as well – and that would be sacrilege. When you say [Depositional sequence (this paper)], you are forgetting that *the ISSC Report is not meant to be an opinion paper*, but a document that moderates constructively all existing approaches.

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Page 68, first paragraph under **Low and High Accommodation Systems Tracts**: "They have been rarely applied …". This is inaccurate. There is a significant amount of work, and published case studies involving these concepts, by Brian Zaitlin, Ron Boyd, Dale Leckie, Bill Arnott, and many others. You also cite Dahle et al. (1997) – this is from reading my textbook, as I have not seen you at that conference in Cape Town (and the concepts you are referring to are not defined in the abstract itself), but you should be aware that the concepts were around for some time before that.

Page 68, same paragraph: "… strongly advocated by Catuneanu (2006)". You are referencing my book as if the concepts of low- and high-accommodation systems tracts were my own, or as if I was trying to implement them whether they are mappable or not. This is not the case at all. As I explained in other occasions, my textbook presents concepts that are available in the literature, with examples. In fact it is fair to say that I am discussing both the logic for applying these concepts *and the pitfalls* (like for example the problems in drawing a precise boundary between these systems tracts). Therefore, your depiction of my work is quite unfair.

Page 69, same paragraph: "Catuneanu (2006, p. 230) avoids discussing how one might draw a contact ...". Once again, this is inaccurate; I am not avoiding anything. I indicate the field criteria for the identification of these systems tracts (based on work by Dale Leckie, Ron Boyd and Brian Zaitlin; see Figure 6.14 in my book) *and the pitfalls with respect to the mappability of their bounding surfaces*. Is this not a fair and balanced discussion? If you really want to criticize these concepts, why don't you refer to the original work of Zaitlin or Leckie for example – both of whom are in Calgary and Ashton knows very well?

Once again, you seem rather selective with what people you choose to criticize.

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I have run out of time and motivation to continue reading your Report. The flaws that I have pointed out are so fundamental that reading the final conclusions seems pointless. I therefore stop here and wish you good luck with whatever you want to do with this Report.

Best regards,
Octavian