Received Date : 18-May-2016

Accepted Date : 23-May-2016

Article type : Reply to Comment

Corresponding author email id: pmb42@liv.ac.uk

Reply to comment of O.Catuneanu and M. Zecchin on Non-unique stratal geometries: implications for sequence stratigraphic interpretations", by: P.M. Burgess[1] and G.D. Prince[2], Basin Research (2015) 27, 351-365

1. School of Environmental Sciences, University of Liverpool, Jane Herdman Building, Liverpool, L69 3GP, United Kingdom. Corresponding author.
2. IHS Inc., Enterprise House, Tetbury, Gloucestershire, GL88RX

Abstract

Catuneanu and Zecchin's comment on Burgess and Prince (2016) makes four main assertions: 1) Non-unique stratal geometries are irrelevant to the workflow of sequence stratigraphy because there are more important unique stratal geometries 2) Numerical forward modelling is less able to tell us how depositional systems may work than interpretation of the rock record 3) There is a statistical norm in the rock record that guides how we should interpret stratal geometries, and 4) non-uniqueness has long been recognised, explained and accounted for as unimportant in the sequence stratigraphic model and method. We examine each point and argue that the assumptions, evidence and arguments presented are insufficient to support these points. In summary, to claim that a stratal geometry is unique is logically problematic because it is an attempt to prove a

negative, namely that other examples of similar geometries generated by different mechanisms do not exist. Also problematic is sole reliance on interpretation of outcrop or subsurface examples to understand and predict how depositional systems work, because the distinction between observation and model-influenced interpretation is often blurred. A statistical norm to guide what interpretation is most correct from several possibilities would certainly be useful, but requires independent evidence in support, and this is currently lacking. Finally, the non-uniqueness described in Burgess and Prince (2015) is an example of an area of stratigraphy that requires to be better understood, not ignored. Combined analysis of outcrop, modern systems and numerical and analogue modelling is an experimental approach that can improve both sequence stratigraphic models and methods.

Introduction

Catuneanu and Zecchin's comment on Burgess and Prince (2015) provides a welcome opportunity to further consider and discuss the issue of non-uniqueness in stratal geometries, and how this issue impacts sequence stratigraphic models and methods. Catuneanu and Zecchin (2016) takes a very different view than Burgess and Prince (2015), asserting that "The non-unique variability highlighted by Burgess and Prince (2015) … is irrelevant to the workflow of sequence stratigraphy". While we understand why they are keen for this to be true, we feel that the assumptions, evidence and arguments they present are not sufficient to support this assertion. Catuneanu and Zecchin (2016) makes various points, but they can be grouped and summarised into four key conclusions:

1. Non-unique stratal geometries are "irrelevant to the workflow of sequence stratigraphy" because, as well as non-unique stratal geometries, there are also "unique stratal geometries that are diagnostic to the definition of all units and surfaces of sequence stratigraphy".

2. Numerical forward modelling is less able to tell us how depositional systems may work than the rock record, because numerical model behaviour stems from assumptions, and "one should not make the mistake of assuming that results from numerical modelling are superior to those drawn from real data (e.g., field observations), and that they better represent the reality."
3. There is a "statistical 'norm' in the rock record" that guides how we should interpret stratal geometries, and makes issues of non-uniqueness unimportant.
4. The non-uniqueness described in Burgess and Prince (2015) has long been recognised, explained and accounted for in the sequence stratigraphic model and method, so there is no need to consider it further.

Our responses to each of these points are given below, making use of numerical stratigraphic forward modelling examples where appropriate to demonstrate key elements of our response.

Unique or non-unique, that is the question

Catuneanu and Zecchin (2016) assert that recognition and consideration of non-unique stratal geometries is "irrelevant to the workflow of sequence stratigraphy" and "obscures the simple workflow of sequence stratigraphy" which can be successfully followed using "unique stratal geometries that are diagnostic to the definition of all units and surfaces of sequence stratigraphy", and " 'unique' stratal stacking patterns that define systems tracts". They give the following examples of "unique geometries":

1. "stratal stacking patterns of forced regression (i.e., progradation and downstepping of the coastline, irrespective of coeval fluvial processes of aggradation, bypass, or erosion)"
2. "normal regression (i.e., progradation and upstepping of the coastline, typically accompanied by fluvial aggradation)"

3. “transgression (i.e., backstepping of the coastline, irrespective of coeval fluvial processes of aggradation, bypass, or erosion). ”

However, we do not agree that the above three cases are examples of demonstrably unique stratal geometries. Examples 1 and 3 are shown in Burgess & Prince (2015) to be possibly non-unique; apparently similar stratal geometries can occur due to forcing by processes others than changes in the rate of accommodation creation or destruction. Even if not identical they may be practically indistinguishable, especially when using poor, incomplete or low-resolution data, a problem that Catuneanu et al. (2009) also pointed out. The second example of a unique geometry given by Catuneanu and Zecchin (2016) is arguably less likely to be unique than the other two, because if topset aggradation can occur independently of rate of relative sea-level rise, there is a large range of accommodation-supply magnitudes and rates of change that could also account for normal regression. In all three cases, analysis of shoreline trajectories, to demonstrate presence or absence of downstepping, may not resolve the resulting uncertainty because of problems with defining palaeo-horizontal, for example due to differential compaction, or just due to a lack of necessary information (Helland-Hansen & Hampson, 2009).

Additional criteria cited by Catuneanu and Zecchin (2016) as unique features indicative of falling stage deposition are:

1. “sharp-based shorefaces bounded at the top by subaerial unconformities”
2. “stratigraphically compressed forced regressive shorefaces, in contrast to the expanded normal regressive shorefaces “
3. “occurrence of the regressive surface of marine erosion at the base of a forced regressive shoreface”
4. “stratigraphic foreshortening of forced regressive deposits, if the gradient of the forced regressive shoreline trajectory is steeper than the shelf gradient”

These criteria as described are mostly interpreted rather than directly observed entities. As such there is a possible issue with circular reasoning, whereby a model is used to interpret the strata, and the ability to interpret following this model is taken to indicate that what was predicted by the model is what actually happened. Our concern is that, in the absence of further independent evidence for the processes that determine a particular stratal geometry, interpretations and conclusions of this type are examples of circular logic and therefore have only minor relevance as evidence of uniqueness.

Perhaps the most fundamental issue we see with Catuneanu and Zecchin (2016) is that simply stating that particular features, or even combinations of features, are unique does not prove them to be so. To robustly demonstrate that something is uniquely indicative of a particular cause, you need to demonstrate that it never occurs due to other causes; this is an example of trying to prove a broad negative which, outside of mathematics, is obviously very difficult. For example, there is no particular reason to conclude that a relative sea-level fall is the only mechanism that could be responsible for a sharp-based sandstone unit deposited in a shoreface setting. An increase in energy of waves incident on a coastline could, presumably, increase the grainsize and change the geometry of a shoreface, leading to formation of a sharp-based sandstone unit, capped by a subaerial unconformity because normal aggradation continues for some height above sea-level before deposition terminates for some period of time (e.g. Storms & Hampson, 2005, figure 6). Other criteria listed by Catuneanu and Zecchin (2016) may also turn out upon further experimental analysis to be similarly non-unique.

In summary and moving forward, much work remains to be done to determine the range of mechanisms that can create commonly observed stratal geometries, across a range of scales from outcrop to seismic images of basin margins, ancient and modern. This is the point that Burgess and Prince (2015) was trying to make, and the comment of Catuneanu and Zecchin (2016) has further demonstrated just how important ongoing work will be in testing the uniqueness of stratal geometries.

All models are wrong, but which ones are most useful, and how do we tell?

Catuneanu and Zecchin (2016) claim that numerical forward modelling is less able to tell us how depositional systems may work than the rock record, because numerical model behaviour stems from assumptions, and "one should not make the mistake of assuming that results from numerical modeling are superior to those drawn from real data (e.g., field observations), and that they better represent the reality." In other words, numerical models cannot tell us as much about the behaviour of sedimentary systems as the rock record, which "will ultimately validate which ones of these scenarios are most common and/or realistic in nature". We certainly agree that to use numerical models alone to determine how depositional systems work would be foolish; ideas and possibilities highlighted by numerical models are generally only hypotheses that need to be tested further, for example by analogue experiments, and also ultimately by tests to see if the numerical models can successful account for geometries observed in nature in outcrop, and in subsurface data such as seismic images. However, we would argue that there is a substantial and fundamental issue that interpretations of outcrop and subsurface data rarely, if ever, provide unique insight into the accommodation and supply history responsible for observed stratal patterns. This point was a key motivation for the work in Burgess and Prince (2015), along with the observation that the sequence stratigraphic method is currently influenced by a sequence stratigraphic model that makes numerous assumptions about how depositional systems work and respond to external forcing (e.g. Posamentier et al. 1988; Catuneanu, 2006; Catuneanu and Zecchin, 2013), but many of these assumptions remain questionable.

For example, how can outcrop data determine whether fluvial strata are deposited during forced or normal regression? To do so would require robust independent evidence of the relative sea-level history during deposition of particular fluvial strata. Given the challenges with shoreline trajectory analysis outlined above and in for example Helland-Hansen and Hampson (2009), this can be more difficult than it first appears. Simply interpreting a rising shoreline trajectory and then saying that fluvial aggradation has occurred during rising relative sea-level rather than falling may be more interpretation than objective observation, and therefore could be problematic.

The key point is that only through a combination of techniques, for example analysis of the best outcrop and seismic data combined with experimental numerical and analogue forward modelling, can we really begin to understand how depositional systems work. So we would not suggest that "results from numerical modelling are superior to those drawn from real data (e.g. field observations)," but we would argue that results drawn only from interpretation of outcrop or subsurface data are not superior either, largely because of the difficulty in make a clear enough distinction between observation and model-influenced interpretation.

We can illustrate the problem with an example from Catuneanu and Zecchin (2016) who assert the falling stage topset aggradation described in Burgess and Prince (2015) and Prince and Burgess (2013) is a consequence of "a model assumption that the fluvial profile at the onset of forced regression is steeper than the trajectory of the forced regressive shoreline". It is not clear why Catuneanu and Zecchin (2016) suggests that we assumed a fluvial profile steeper than the regressive shoreline trajectory. They refer to figure 5 of Burgess and Prince (2015) but while in that case the initial topography is indeed steeper than the regressive shoreline trajectory, this initial topography is not in fact the fluvial gradient. In all cases the fluvial gradients are less than the initial basin margin slope. Contrary to what Catuneanu and Zecchin (2016) assert, there is no direct *a priori* assumption of fluvial gradient or equilibrium profile by Burgess and Prince (2015) or Prince and Burgess (2013), only an assumption of sediment transport by a diffusional process with given initial conditions and a given diffusion coefficient κ, where sediment flux is proportional to the product of the topographic gradient and this diffusion coefficient (Begin et al., 1981; Granjeon and Joseph, 1999). It is the choice of κ as a model parameter, along with the sediment input volume, that largely determines fluvial gradient through time in the resulting models. Low values of κ tend to lead to steeper fluvial gradients because sediment flux is lower and hence less able to move the supplied sediment into the basin, leading to greater aggradation and steeper gradients on the coastal plain. This seems to us more physically reasonable than the assumption that fluvial aggradation is a geometric process determined by a conceptual equilibrium profile for which there is arguably neither substantial explanation nor evidence.

Catuneanu and Zecchin (2016) further assert that “in this situation, the progradation of the forced regressive coastline does lead to a decrease in fluvial gradient and energy with time, which favors topset aggradation during forced regression (Posamentier & Allen, 1999; Catuneanu, 2006)”. This assertion is also false; there is no decrease in fluvial gradient through time in the examples shown in Figure 5 in Burgess and Prince (2015). This is demonstrated in results generated for this reply (Fig 1) which shows model output similar to Prince and Burgess (2013) with three different κ values for a 50m and a 100m relative sea-level fall cases. Five out of the six models show topset aggradation during fall in relative sea-level (Fig. 5, A,B,C,F, & G) despite constant or slightly increasing fluvial gradient through time. The model run with little or no fluvial aggradation is due to the combined effect of a 100m relative sea-level fall combined with a relative high rate of fluvial sediment transport (high k value). In all cases the fluvial gradient increases through time (Fig 1 D & H), directly contradicting what Catuneanu and Zecchin (2016) state should happen, because in this numerical model fluvial aggradation is occurring without any decrease in fluvial gradient.

This example illustrates the problem with some aspects of the current sequence stratigraphic method as described by Catuneanu and Zecchin (2016) where conceptual models may drive interpretation of outcrop or even other models, even when there is little independent evidence to support the interpretation. If sequence stratigraphy is limited to an observational method alone this is not necessarily a problem (e.g. Neal et al. in press) but this is perhaps more difficult than some suggest. We would therefore argue that the optimum approach is to combine outcrop, numerical and analogue modelling into an experimental approach to improve our understanding of how stratigraphic systems work (Muto et al, in press). For example, using modern and Pleistocene deposystems and analogue and numerical experiments to provide more detailed understanding of how and why fluvial aggradation occurs would be useful, and would help ensure that when it is difficult to separate model and method, the models are more robust.

The statistical norm of the rock record

Catuneanu and Zecchin (2016) state that there is a “statistical ‘norm’ in the rock record”. They imply that this should guide how we interpret strata and consider non-uniqueness because, although “deviations from this model are of course possible”, they are less likely to occur. Therefore most interpretations should follow the “statistical norm” that is implied to be dominant control by variations in accommodation. This would be a powerful argument if a statistical norm were established with independent evidence that most stratal geometries were indeed controlled by some specific forcing process, but no such evidence is presented. Instead, the evidence, according to Catuneanu and Zecchin (2016) are “numerous case studies”. These case studies, in which interpretation follows the standard sequence stratigraphic model, are not independent evidence because, for the most part, they simply reflect a dominant model for interpretation. Therefore we would argue that this appeal to a statistical norm may simply be further circular logic.

A key example highlighted in Burgess and Prince (2015) is the interpretation of deep-marine fan systems as lowstand deposits. Catuneanu and Zecchin (2016) defend this preferred interpretation as “a statistically most likely scenario, as indicated by numerous case studies”, and so the circle of logic is now complete. They justify this position with “the deep-water stratigraphy is to a large extent a consequence of what happens on the shelf”, which is doubtful given that data derived from a source to sink approach increasingly suggest that what happens in a basin is also very much a consequence of what happens throughout the denudational area that feeds the basin (e.g. Blum and Hattier-Womack, 2009). As our knowledge of source-to-sink systems develops, so presumably our appreciation of all-stand fans (Covault and Graham, 2010) and multiple controls on deep-water systems (e.g. Blum and Hattier-Womack, 2009) will develop to provide the independent evidence currently lacking.

We already known that which we need; the standard model and the nature of scientific progress

Catuneanu and Zecchin (2016) claim that the non-uniqueness described in Burgess and Prince (2015) has long been recognised, explained and accounted for in the sequence stratigraphic model and method. While we do appreciate the hard work and thought that has gone into development of the method and the model, we feel that this attitude is problematic for two reasons.

Firstly, a few publications mentioning an issue is not at all the same as widespread recognition and addressing of the problem. For example, one may argue that it has long been recognized that sediment supply can vary through time, and that this, alongside changes in accommodation, is a primary control on stratal architecture. However, as Burgess (2016) pointed out, it remains rare that variable sediment supply is included as a control in interpretation of strata. Given this, we feel that the points about non-uniqueness and multiple controls raised in Burgess and Prince (2015) are fair and important.

Secondly, arguably it is possible to read Catuneanu et al. (2009) and Catuneanu and Zecchin (2016) as suggesting that that standardization of sequence stratigraphy represents perfection of stratigraphic knowledge, whereby we know all that we need to know about how strata accumulates and how they should be observed and interpreted. If this is a fair representation of any attitudes to sequence stratigraphy then there is a problem as others have also already pointed out (Helland-Hansen, 2009), because we know from the history of such claims of complete knowledge in other scientific areas that it is unlikely to be true. Science proceeds by continual development through testing hypotheses or models against data and refining or discarding those models. The tests should be as independent of the models as possible. This, and the sheer complexity of Earth surface systems, is a key reason why we argue that the optimum approach is to combine outcrop and subsurface analysis with potentially independent numerical and analogue modelling analysis (Muto et al, in press). When properly integrated with ongoing standardization efforts, this would be a robust and powerful expression of the scientific method applied to stratigraphy, likely to generate substantial progress in understanding and predicting stratal geometries.

Conclusions

While we thank Catuneanu and Zecchin for raising again this important point, we cannot agree with their assertions. The non-uniqueness described in Burgess and Prince (2015) is an example of an area of stratigraphy that we still do not understand well enough, and therefore we do not think it is wise to dismiss it as unimportant for the sequence stratigraphic workflow. The optimum approach is to combine analysis of outcrop, modern systems and numerical and analogue modelling into an experimental approach that can improve our understanding of how stratigraphic systems work, and therefore improve our ability to generate and interpret results from the sequence stratigraphic method.

Acknowledgements

We would like to thank Didier Granjeon and IFP for development of and permission to use a version of the Dionisos stratigraphic forward modelling software.

No conflict of interest declared.

References

BEGIN, Z.B., MEYE, D.F, AND S A SCHUMM, S.A. (1981) Development of longitudinal profiles of alluvial channels in response to base level lowering, Earth Surface Processes and Landforms, 6, 49-68.

BLUM, M.D., AND HATTIER-WOMACK, J. (2009) Climate change, sea-level change, and fluvial sediment supply to deepwater depositional systems. In: Kneller, B., Martinsen, O.J., and McCaffrey, B., (eds), External Controls of Deep-Water Depositional Systems, 92, 15-39.

BURGESS, P.M. (2016), The future of the sequence stratigraphy paradigm: Dealing with a variable third dimension, Geology, 44, 335-336.

BURGESS, P.M. AND PRINCE, G. (2015) Non-unique stratal geometries: implications for sequence stratigraphic interpretations, Basin Research, 27, 351-365.

PRINCE, G. & BURGESS, P. (2013) Numerical Modelling of falling-stage topset aggradation: implications for distinguishing between forced and unforced regressions in the geological record. Journal of Sedimentary Research, 83, 767-781.

CATUNEANU, O. (2006) Principles of Sequence Stratigraphy: Amsterdam, Elsevier, Oxford.

CATUNEANU, O., ABREU, V., BHATTACHARYA, J., BLUM, M., DALRYMPLE, R., ERIKSSON, P., FIELDING, C., FISHER, W., GALLOWAY, W., GIBLING, M., GILES, K., HOLBROOK, J., JORDAN, R., KENDALL, C.G.ST.C., MACURDA, B., MARTINSEN, O., MIALL, A., NEAL, J., NUMMEDAL, D., POMAR, L., POSAMENTIER, H., PRATT, B., SARG, J., SHANLEY, K., STEEL, R., STRASSER, A., TUCKER, M., & WINKER, C. (2009) Towards the standardization of sequence stratigraphy. Earth-Science Reviews, 92, 1–33.

CATUNEANU, O., ZECCHIN, M. (2013) High-resolution sequence stratigraphy of clastic shelves II: Controls on sequence development, Marine and Petroleum Geology, 39, 26-38.

CATUNEANU, O., AND ZECCHIN, M. (2016), Comment on “Non-unique stratal geometries: implications for sequence stratigraphic interpretations”, by: P.M. Burgess and G.D. Prince, Basin Research (2015) 27, 351-365

COVAULT, J.A. & GRAHAM, S.A. (2010) Submarine fans at all sea-level stands: Tectono-morphologic and climatic controls on terrigenous sediment delivery to the deep sea. Geology, 38, 939–942.

GRANJEON, D., & JOSEPH, P. (1999) Concepts and applications of a 3D multiple lithology, diffusive model in stratigraphic modeling. In: Numerical Experiments in Stratigraphy: Recent Advances in Stratigraphic and Sedimentologic Computer Simulations (Ed. by J. W. Harbaugh, W. L. Watney, E. C. Rankey, R. Slingerland, R. H. Goldstein. and E. K. Franseen), SEPM Special Publication, 62, 197–210.

HELLAND-HANSEN, W. (2009) Discussion: Towards the standardization of sequence stratigraphy, Earth Science Reviews, 94, 95-97.

HELLAND-HANSEN, W., AND HAMPSON, G.J. (2009) Trajectory analysis: concepts and applications, Basin Research, 21, 454-483.

MUTO, T., STEEL, R.J., AND BURGESS, P.M. (in press) Contributions to sequence stratigraphy from analogue and numerical experiments, Journal of the Geological Society of London.

NEAL, J.E., ABREU, V., BOHACS, K.M., FELDMAN, H.R., AND PEDERSON, K.H. (in press) Accommodation succession sequence stratigraphy: observational method, utility, and insights into sequence boundary formation, Journal of the Geological Society of London.

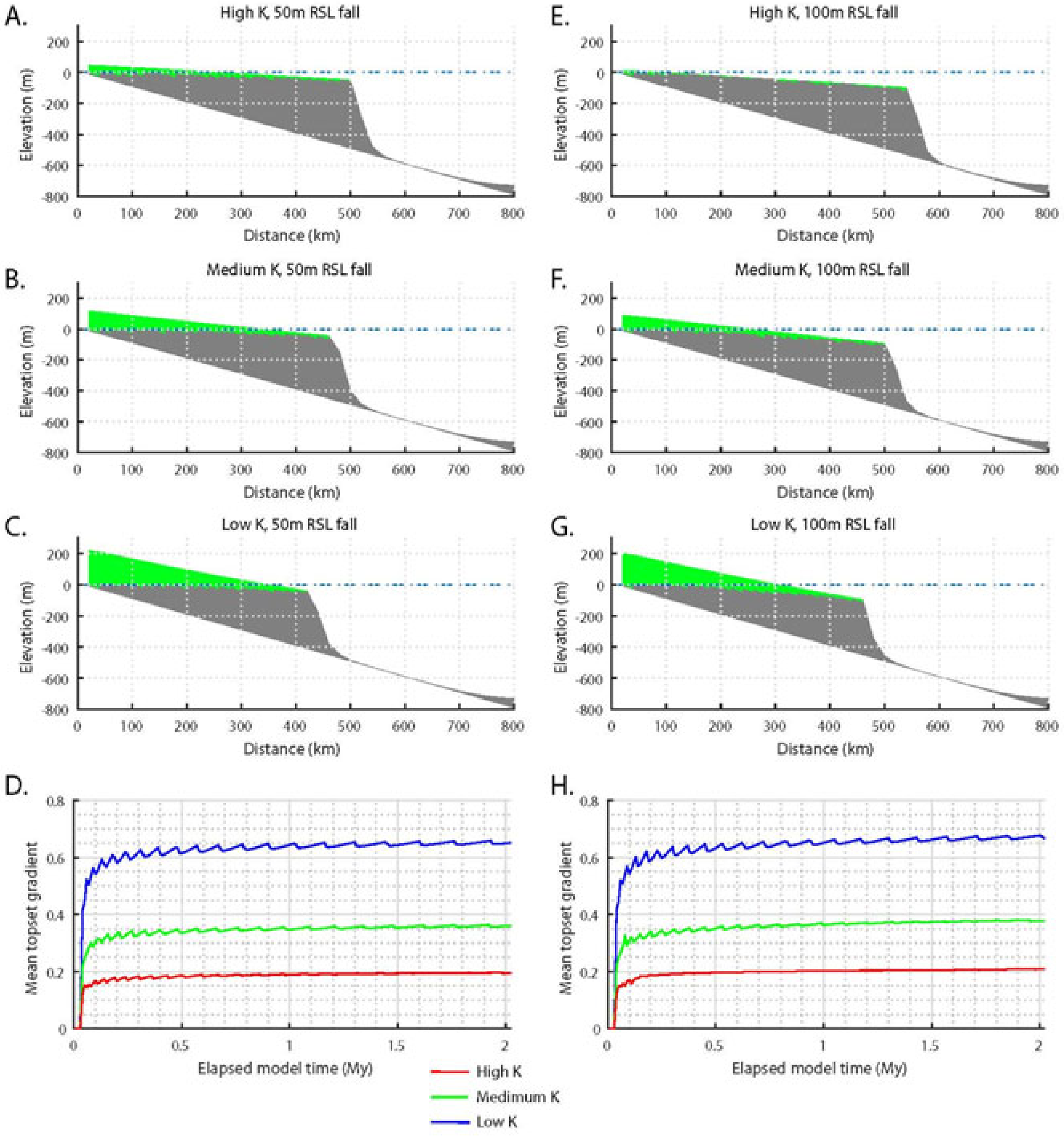
POSAMENTIER, H. W., AND ALLEN, G. P. (1999) Siliciclastic sequence stratigraphy: concepts and applications. SEPM Concepts in Sedimentology and Paleontology No. 7.

POSAMENTIER, H.W., JERVEY, M. T. & VAIL, P. R. (1988), Eustatic controls on clastic deposition I - conceptual framework. In: Wilgus, C. K., Hastings, B. S., Kendall, C. G. ST. C., Posamentier, H. W., Ross, C. A. & Van Wagoner, J. C. (eds) Sea-level Changes: an Integrated Approach. Society of Economic Paleontologists and Mineralogists, Special Publications, 42, 109-124.

STORMS, J.E.A., & HAMPSON, G.J. (2005) Mechanisms for forming discontinuity surfaces within shoreface-shelf parasequences: sea-level, sediment supply, or wave regime? Journal of Sedimentary Research, 75, 67–81.

Figure caption

Figure 1. A series of 2D models of basin margin strata created using the numerical stratigraphic forward model Dionisos to show how fluvial topsets aggrade during relative sea-level fall for various sediment transport rates with no decrease in fluvial gradient in any of the runs. (A-C) Cross sections of modelled strata for (A) high (κ=1-400km^{2}ky^{-1}), (B) medium (κ=1-200km^{2}ky^{-1}) and (C) low (κ=1-100km^{2}ky^{-1}) rates of sediment transport with a 50m relative sea-level fall over a 2My duration in each case. All show topset aggradation throughout the RSL fall, though less well developed with higher sediment transport rates (D) The time series of mean topset gradients for the 50m fall runs with low, medium and high sediment transport rates shows an initial increase adjusting for the model initial conditions, but then is constant to gradually increasing gradient through time in each case. (E-G) Cross sections from similar model runs with a 100m relative sea-level fall. The highest transport rate (E) shows very little topset aggradation, but the lower transport rate cases (F and G) still aggrade even with this larger RSL fall, and once again in all cases there is no decrease in topset gradient (H) that could be claimed to be related to the aggradation.


A.
High K, 50m RSL fall
E.
High K, 100m RSL fall
B.
Medium K, 50m RSL fall
F.
Medium K, 100m RSL fall
C.
Low K, 50m RSL fall
G.
Low K, 100m RSL fall
Elevation (m)
200
0
-200
-400
-600
-800
Distance (km)
0
100
200
300
400
500
600
700
800
D.
H.
Mean topset gradient
0.8
0.6
0.4
0.2
0
Elapsed model time (My)
0
0.5
1
1.5
2
High K
Medimum K
Low K