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Geoscientific Model Development Discussions

Interactive comment on "Quantitative stratigraphic analysis in a source-to-sink numerical framework" *by* Xuesong Ding et al.

Xuesong Ding et al.

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(General comments) Comment 1-1: "Quantitative stratigraphic analysis in a source-to-sink numerical framework" by Xuesong Ding et al. is a clearly written and thoughtful submission that can be a strong contribution after significant technical clarification is included. It might also be better titled, considering the content is dominated by a comparative analysis of alternate sequence stratigraphic interpretation methods using manual and automated means to compare the fit of results with pyBadlands Stratigraphic Forward Model (SFM) input and output.

Response: We added a new section (4.1) in which we quantified the timing and development of stratigraphic surfaces and depositional units based on the temporal evolution

of stratal stacking patterns (Page 9, Figure 5). The stratigraphic analysis in section 4.1 also serves as a reference for comparison with interpretations resulting from the two tested methods. We restated the three aims of our work in the Introduction, which are (1) use SFM as a tool to quantify the development of stratigraphic architecture under the interplay between accommodation change and sediment supply (Page 2, lines 19-20); (2) evaluate the performance of the trajectory analysis and the accommodation succession method on the interpretation of stratal architecture predicted with pyBadlands (Page 3, lines 6-7); (3) integrate quantitative stratigraphic analysis within pyBadlands based on the trajectory analysis and accommodation succession method (Page 3, lines 13-15).

Comment 1-2: The approach used is novel, applying different interpretation techniques on the output of a SFM and comparing the results of each technique against time-dependent SFM inputs and outputs. Unfortunately, there are flaws in the analysis that stem from a blurring of observations that are the foundation of interpretation methods and the forcing mechanism inferred to drive them. Firstly, a β-factor of 1 to 2.5 over 150 km produces a subsidence profile which increases so much toward the basin that 10 million year duration, 50m "eustasy" cycles don't produce basinward shifts of facies (depositional sequence boundaries) resulting from negative shelfal accommodation that is a key factor to interpretation with either shoreline trajectory (ST; Helland-Hansen and others '94-'09) or accommodation succession (AS; Neal and others '09-'16) methods.

Response: The imposed three cyclical eustatic sea level changes do induce progradation stacking, basinward shifts of facies and formation of subaerial unconformities during sea level fall. The average rate of sea level change is 10 m/Myr. The imposed thermal subsidence over the shelf (150 km to 250 km on a dip-oriented cross-section) has a subsiding rate ranging from 0 to 16.7 m/Myr. Therefore, the shelfal accommodation varies with the fluctuation of eustatic sea level. The imposed stretch factor β is taken from McKenzie's model (1978) and is within a normal range for passive margin. We acknowledge that the prescribed increase in β-factor from 1 to 2.5 over 150 km is large. We considered a test case with half the thermal subsidence. Similar progradation stacking, basinward shifts of facies and formation of subaerial unconformities are observed for this test case (see the figure below), in which the whole depositional package is accumulated $\sim$ 30 km further basinward.

Comment 1-3: Application of ST method is disadvantaged as presented because the SFM produces a trajectory the authors had to invent ("descending transgressive trajectory class" or DTTC) in order to fit geometries with known sea-level conditions. This is a limitation to methods that are explicitly linked to sea-level change.

Response: Thank you for raising this issue. We understand the limitation of applying the shoreline trajectory analysis to our test case as the developed clinoforms are of shelf-lope scales over long-term (tens of millions of years) rather than shoreline scales (tens of thousands of years). We pointed this out in the Discussion (Page 16, lines 22-24). The numerical tools we provided to extract time-dependent shoreline positions based on a given sea-level forcing would also be useful for short-term SFM experiments.

Comment 1-4: The AS method explicitly avoids sea-level requirements and focuses on stratal terminations at key surfaces that bound different stacking patterns. This method allows interpretation to adjust to dipping strata that was initially horizontal (clinoform topsets – coastal plain aggradation). ST method builds from the assumption of trajectory from horizontal, so differentiating relative to AS is artificial (a function of forcing it to fit the sea level curve). THIS is the actual insight from Ding et al.'s paper – apply ST or AS methods but do not force them to fit a sea level curve. We don't observe sea level in stratigraphy, we infer it. We observe stratal terminations, shoreline trajectories, vertical and lateral stacking of facies associations, and key bounding surfaces that record significant changes in these observations.

Response: We agree with this comment and appreciate the objective characters of the AS method. When following the workflow of the ST and AS method to define different

stratigraphic units, we did not force them to fit a sea level curve. On the contrary, we clearly see the mismatches between the timing of stratigraphic surfaces and changes in sea level (Page 9, Figure 5), which are induced by basement subsidence and variations in sediment supply. We understand the importance of inferring sea level from stratigraphic record, and aimed to quantify the stratigraphic evolution for known forcing conditions (including sea level) to provide insights into reconstruction of contributing factors in natural systems. The fact that dA-dS is a proxy for the derivative of sea level change with respect to time, rather than a direct proxy for sea level change (see Comment 3 by Reviewer #1) is a reminder that sea level cannot be directly inferred from stratigraphic analysis.

Comment 1-5: The erosion feature of pyBadlands produces interesting 2D truncation geometries updip (but this was not demonstrated in the Wheeler diagram (fig. 5c) and might produce more interesting relations in shoreline trajectory if β-factor were reduced.

Response: Thank you for raising this interesting point. The truncation geometries are nearly horizontal when they are formed, and then evolves into upward dipping due to basement subsidence. The original Wheeler diagram was automatically constructed based on paleo-depth and therefore recorded instantaneous sediment deposition. Based on this comment, we added the final stratal thickness, which indicates the erosion of the progradational stacking, to the Wheeler diagram (Page 10, Figure 6b).

Comment 1-6: For scaling comparison, I suggest you refer to the physical flume model and resulting interpretation published in Martin et al. 2009 (Martin, J., Abreu, V., Neal, J. Sheets, B. 2009. Sequence stratigraphy of experimental strata under known conditions of differential subsidence and variable base level. AAPG Bulletin, 93, 503–533.)

Response: Thank you for referring us to this insightful paper, which we mentioned in the introduction (Page 2, line 13) and discussion (Page 15, line 26) of the revision.

The scalability of the Stratigraphic Forward Model would make it possible to carry out a scaling comparison to the work of Martin et al. (2009), however, such a comparison would distract from the message of this manuscript and it would require sufficient work to warrant a separate study

Comment 1-7: In summary, there are ways this experiment could be run that would make a better comparison of interpretation methods or the paper could more directly highlight shortcomings of interpretation methods that are explicitly linked to sea-level change. The approach in Ding et al. is innovative for using SFM to volumetrically quantify δA/δS or (δA - δS if you wish) and I encourage the authors clarify their purpose (change the model or change the conclusions and application) so this good work is more on target.

Response: Thank you for this advice, based on which we clarified the aims of our work (see the reply to comment 1-1).

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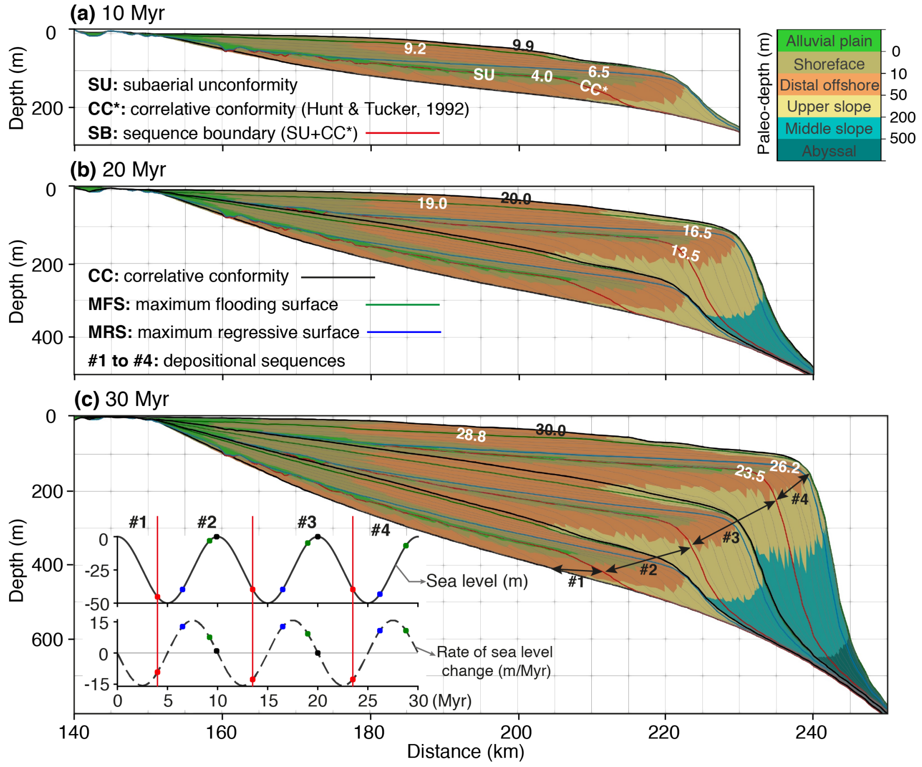

Fig. 1. Figure 5

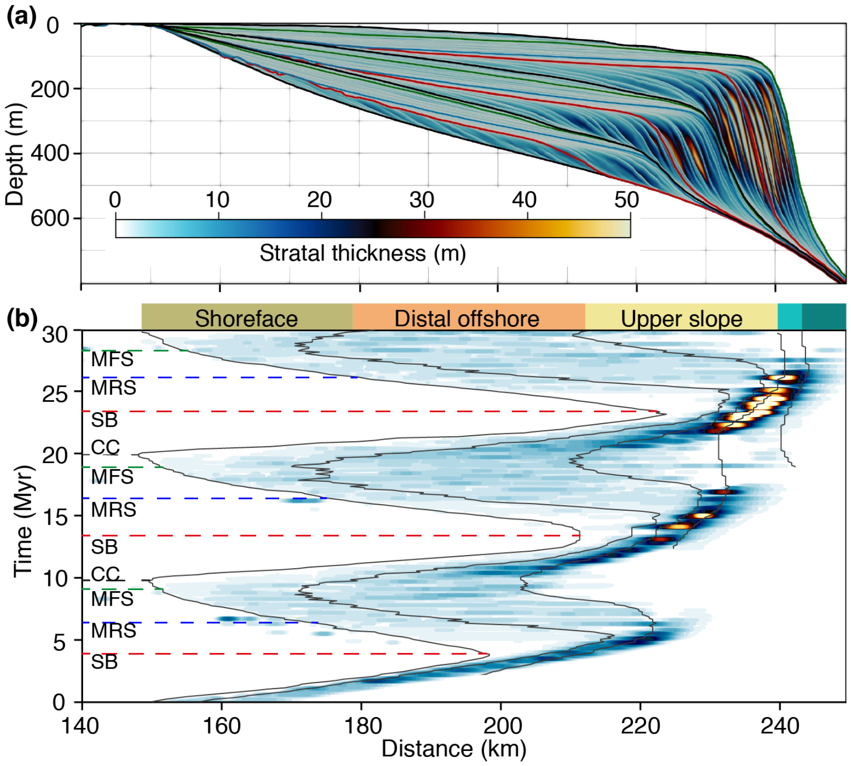

Fig. 2. Figure 6

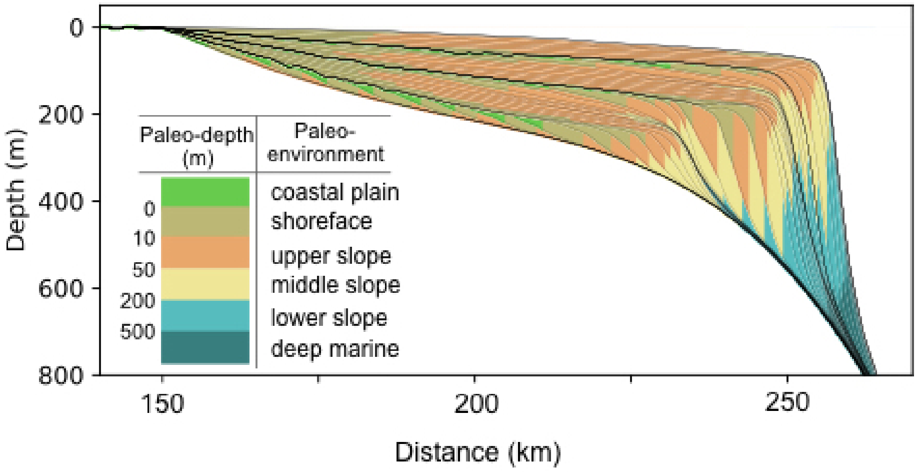

Fig. 3. Supplementar figure to Comment 1_2. Predicted stratal stacking pattern from a new case forced with half of the original subsidence. Other forcing parameters remain the same.