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Interactive comment on "Measuring Subaqueous Progradation of the Wax Lake Delta with a Model of Flow Direction Divergence" *by* John B. Shaw et al.

John B. Shaw et al.

shaw84@uark.edu

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We thank the two reviewers for taking the time with our work, and providing valuable comments which we have addressed, improving the manuscript in the process. Below, we lay out our responses to their comments. We put reviewer comments in brackets. However, it is worth noting at the outset that their comments have led to two new components of the paper. First, in section 3.3 and figure 7, we have added an analysis of the distance between channel tip and critical divergence point (delta-l) as a function of unsteady hydrodynamic conditions. Second, in section 5.2.2 we have further interpreted the linear subaqueous landbuilding growth rates in comparison to the reduction in growth rate observed with subaerial monitoring techniques, and interpreted the findings as a result of increased island width, rather than any reduction in sediment accumulation.

We hope that the reviewers and AE find our new manuscript compelling and worthy of publication in ESurf.

[Anonymous Referee #1

General comments The study is reasonably well written and organized. My major concern is one of the study's significance. The authors have devised a method to find flow divergence and convergence. The distance between flow divergence and the channel tip (growth rate) is brought as the main contribution of this study. I personally do not understand the significance.]

The improved understanding of the delta front flow field presented here is significant because it provides a tool for measuring changes to subaqueous channel tips of the Wax Lake Delta using remote sensing for the first time. As described in the introduction, we currently have no way of measuring rapid and important subaqueous changes on deltas where expensive and time-consuming bathymetric surveys are not an option. Demonstrating a method that harnesses understanding of delta front processes represents progress toward monitoring the subaqueous realm, even if it is for only one delta.

[Is there anything here we can learn about process? It is good to see a comparison with Delft3D, but again the authors do not interpret their results. Why is flow diverging/converging? What are the morphodynamics that lead to this behavior? Do the authors expect the same behavior for other deltas? Why (not)?]

While our study's main contribution is the description of this flow pattern within remote sensing imagery. We also address the "why" question. The transition from flow direction divergence to flow direction convergence ãĂŰ(xãĂŮ_D ÌŇ) is associated with the bed transitioning from an adverse bed slope to a basinward one at the channel tip. We have expanded the introduction to this process in the introduction by including a qualitative explanation for what is happening (P4L9). In our opinion, the referenced mechanistic explanations of diverging converging flow are already compelling and capture the process reasonably well. Our contribution is to establish that it can be observed via remote sensing and used for delta monitoring. We have also expanded the discussion of what is required for the method to be applied in other systems (P9L23).

[A second, derived, conclusion of this study is about delta aerial growth, which the authors extract from channel tip locations. In this section there is also no interpretation or discussion about process understanding that can be derived from this data. Does this view of delta area change we way we think of delta morphodynamics, in general or specifically of the Wax Lake Delta?]

We interpreted the data based upon recent theory and measurements from the Wax Lake Delta and other prograding deltas in Section 5.2. The individual channel progradation rates were discussed for the single channel with significantly larger progradation rate and the apparent lack of soft avulsion within the data Section 5.2.1). In the new manuscript we have significantly expanded the discussion of area growth because it is interesting that subaerial growth rates have reduced while the subaqueous ones have remained constant. This is likely due to the importance of channel proximity for marsh aggradation, and has important implications for the Wax Lake Delta and coastal restoration initiatives (Section 5.2.2). We thank the reviewer for prodding us on this, because our interpretation of decoupling or progradation and aggradation is novel and could be a very important process on Wax Lake Delta and controlled sediment diversions elsewhere.

[Overall, I have to conclude that the study does not address a relevant scientific question, and that a shift towards process understanding would require a significant departure from the presented manuscript.]

We disagree with this assessment. The relevant scientific frontiers addressed here are (a) fluid-morphology interaction on a complex delta front and (b) delta evolution. The contribution to a process understanding of frontier (a) is the discovery of widespread convergence of flow direction near channel tips that is consistent and predictable with quantitative uncertainty. The contribution to frontier (b) is the application of this pattern to the first remote-sensing analysis of the subaqueous portion of a prograding delta, revealing remarkable similarities and differences compared to subaerial growth rates.

[Specific comments P2/3: Section 2 reads like an unorganized mix of different topics ranging from river mouth bars to flow patterns to hydrological connectivity and streak lines. I would ask for better organization and preferably subheadings.]

We agree that the paragraph on P2/3 was too long and have broken it up into three components. Section 2 now has an introductory paragraph, a paragraph on delta front bed morphology, a paragraph on flow patterns and streaklines, and a paragraph on flow direction convergence. We hope that this organizes the section.

[P3L7: remove “strong”. Both Leonardi and Nardin modeled relatively low energy marine environments.] Removed.

[P3L18: I strongly suspect streaklines do not track depth-averaged flow, but rather that this case study was performed in a setting where surface flow directions are a good approximation of the depth-averaged flow.]

We agree that three-dimensional flows can make it so streaklines do not track flow direction. We also agree that in the case of the Wax Lake Delta, this does not appear important. We now add that “streaklines should indicate flow direction where three-dimensional flow patterns and unsteady changes to flow are minimal (P3L19).”

[P3L19: how can Shaw et al (2016b) claim reasonable accuracy if validation was done months after the remote sensing images were obtained. I would rephrase this to read more like: “despite limitations in the validation, Shaw et al found reasonable agreement between streaklines and morphology: : :” or similar.]

We have changed the text to read: “Shaw et al. (2016b) showed that such streaklines depict similar flow directions to direct measurements months apart. (P3L17)”

[P5L11: what is Dcr?]

Dhat_cr was an incorrect remnant from a previous notation scheme. The correct term was x_D ÌŇ , which is the critical divergence point along an axial channel transect. This line has been amended (P5L12)

[Fig 1: difficult to read. Perhaps here or in figure 2 explain the structure of the divergent/convergent streak lines.]

We have altered Figure 2 to label the diverging and converging flow direction zones.

[P5L26: what is a “7% uncertainty for a delta”?] We find that the sentence is needless and somewhat confusing so we have deleted it.

[P6L6: the median delta-l for the modelled deltas are within the range of the grid size of the model. Is delta-l even significantly different from a zero mean?]

We agree that delta-l is in some cases (A1e1) indistinguishable from zero. However, in other cases it is definitely not equal to zero. Hence, we only choose to conclude that delta l is generally positive and on the order of a few hundred meters or less.

If this comment was asking how a median value could be smaller than the grid resolution, this is because x_D ÌŇ is linearly interpolated from D ÌŇ(x), it’s location is given at a subgrid scale. We write this in lines P5L18.

[P8L23: with steady boundary conditions Delft3D produced a “significant distribution in delta-l” so winds/tides are unlikely to be a major concern. The authors then follow with a statement that Delft3D variability was less than half the Wax Lake delta variability. So winds/tides could a significant factor?]

We have addressed this issue with a new figure showing the effect of discharge and tides on measured values of delta l from the Wax Lake Delta (Figure 7). As discussed on P6L3-10, these analyses show that there is not a significant effect of these parameters on delta l. Unfortunately, there was insufficient data to assess the effect of wind setup on delta l (P8L27), although we argue that it is unlikely to have a major effect given the wide distribution of delta l even in a single image.

[P10: Why is this a better characterization of delta growth? There are still deltaic deposits beyond the channel tips.]

We do not claim that this method is a "better characterization of delta growth." Instead, we argue that delta growth is a complex process, and multiple approaches can lead to an understanding of this complexity. This is exemplified in our new interpretation of a gradual decoupling of progradation and aggradation rates (P10L25)

[Anonymous Referee #2

There are valid questions about the universality of the technique, including: Streaklines might not be good flow indicators everywhere, and subject to wind and tide forcing. Flow convergence offshore of channel tips may not be universal. Applying the model requires making some measurements or assumptions to justify the choice of delta-l.

However, the authors mostly address these limitations head on, and provide potential users of the method with the tools to decide whether it might be applicable in their own setting. Given the clarity of the presentation here, other scientists should find it straightforward to apply this technique to their own work. Whether those studies will confirm that the assumptions are valid across many locales remains to be seen, but I expect this paper to be read and the technique to be used by other workers.]

We appreciate that our effort to address the limitations of the model went over well. We have tweaked the discussion section addressing limitations to say "If the model can be validated in similar settings," which makes it clear that we have no evidence that it will work beyond the Wax Lake Delta and the models, yet.

[I support publication in ESurf with only minor revisions, listed below.]

[P5L11: Dhat_cr is location where Dhat is zero? unclear]

Dhat_cr was an incorrect remnant from a previous notation scheme. The correct term was x_D ÌŇ , which is the critical divergence point along an axial channel transect. This line has been amended (P5L15)

[P3L27: It would be good to specify that these are spatial accelerations, to avoid confusion] We agree that this needs to change. We have changed “acceleration” to “spatial velocity change.” As discussed in Shaw et al. 2016, A_check does not have units of velocity. The change can be found on P3L27.

[P6L11: Here you fit a regression line to time vs. delta-l. The slope was small, but the t-test showed that you couldn’t reject the null hypothesis of no trend (i.e. zero slope). So doesn’t that mean that there might indeed be a trend, and therefore that you cannot say for sure that stationarity exists? My suggestion would be to show the regression line in figure 6 along with error bounds. That should be pretty clear that whatever trend exists is small, and confirm the visualization.]

We like this approach because it doesn’t rely too much on a “failure to reject.” We have changed figure 6 to add a linear trend and a 50% confidence interval. We also updated the text to read “The slope that was found (1.6 ± 2.6 m/yr for D1e1) would introduce a small error to Δl relative to the uncertainty of Δl (order 100 m) even if it slowly grew over many decades. This near-stationarity suggests that Δl can be assumed constant in time, even as a delta progrades. (P6L27)”

[P7L10: I don’t see what distribution on delta-l is being assumed for the Monte Carlo simulation. Is it simply uniform over the grey boxes in Figure 5?]

The Monte Carlo sampling was performed by randomly sampling one of the 21 values of Δl that was measured at Wax Lake Delta. Hence, no distribution was assumed.

Perhaps with confusion stemmed from referencing Fig. 5a (incorrect) rather than Fig. 6a (intended). The text now reads: “The location of x_η ÌĆ was determined by randomly sampling (with replacement) one of the 21 measured values of ∆l that were measured on the Wax Lake Delta (Figure 6a), and then estimating the location of the channel tip (x_η ÌĆ =x_D ÌŇ -∆l).” (P7L23)

[Figure 3: If I’m understanding this correctly, the method shown is to estimate the paths of the channels, then extend the channel line beyond the last known channel tip location, then calculate divergence based on streak lines, then use the divergence field to locate the channel tips. So the method shows the distance that the channel tip is along a known or assumed flow path, but doesn’t necessarily identify the lateral location of the tip. That means that some information about the channel’s path in the subaqueous reach beyond the shoreline is necessary. I think that should be mentioned in the text.]

The reviewer understands the method correctly. We mentioned this briefly in Section 3.2, but have now expanded the section to be explicit. It now reads: “The method is designed to estimate one channel tip location that is along the subaqueously defined distributary channel axis. The benefit is that this channel axis is easily defined in imagery, but it means that the method cannot account for bends or branches in the subaqueous reach. (P5L21-23)”

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