

Jason,

I appreciate you passing along the memo and giving me the opportunity to respond to it. In general I don't have any major quarrels with the contents of the memo. The purpose of that paper was really to stand as a case study describing linkages between physical processes in sandy-braided rivers, and sandbar habitats. In my attempts to understand sandbar dynamics in the lower Platte River I found the literature lacking in such linkages and set out to create a physically-based framework for both understanding and quantifying sandbar habitat extents. Below are my comments on the memo.

\*\*\*Please note that these comments are not an established opinion or position of the United States Geological Survey (my current employer). I am making these comments in my role as the author of a paper written while I was a graduate student at the University of Wyoming.\*\*\*\*

(1) Program scientists (Mr. Farnsworth) were peer-reviewers of the paper. In the interest of transparency and the long-term record, it should be mentioned that substantial communication occurred between the authors of the paper and Mr. Farnsworth during peer-review.

(2) The concept of the increasing elevation gap between formative flow and bar top-surface elevation is noted in the paper as an 'improvement' over assuming a mean depth because it complies with observations across the scientific literature. That is, it reflects what we observe in nature. The contribution in the paper was that no one had applied a common measurement method to track genetic bars across a range of formative discharges. There is evidence from another paper (attached) that the gap exists regardless of duration of flood, and that it may be a function of Froude and sediment suspension numbers. The fact that Program scientists have not observed an increasing gap with increasing discharge is odd, but there could be several reasonable explanations for it. For example, my quick look at the data from the Program 2017 paper indicated at least one data point was included where the bars were not genetic (not formed by the flow you related it to because they did not move...maybe vegetated). My data also only include the highest parts of bars (upper 50% of bar topography...method outlined in my 2013 paper). Depending on how flows decline (steady/unsteady), and what happens with bar erosion between emergence and the time of data collection, the lower portions of bars can be deposited and modified by flows other than the peak. This is easy to identify in the field, but would be substantially harder from LiDAR. Given that the range of stages (depths) in the central Platte is less than that of the LPR, the bar sizes are undoubtedly smaller, and the problem is even harder using remote sensing data. I am not criticizing the Program's use of LiDAR, I am simply pointing out that identification of multiple depositional levels within a sandbar is much easier when surveying in the field, and may be a reason why the Program sandbar height model does not reflect what is observed elsewhere in nature.

(3) The memo mentions that our paper "cautions against using the Program relationship". In fact, the published paper does not caution against the use of any Program-specific relationship.

(4) The memo paragraph on modeling did not mention the fact that there were real differences in modeling results between the Program model and our model that were unresolved during the peer-review process. It is true that our sandbar-height model increased inundation potential in the reach of the LPR downstream from the Elkhorn relative to the Program model. We wrote a simulation in R to examine the effect of using our model relative to the Program model. Our model predicted a decrease in the percentage of years with adequate nesting windows (>49 days after emergence) from 83% to 79% in that reach. However, in the reach upstream from the Elkhorn, our model increased the percentage of years with adequate nesting windows from 73 to 98%. The Program also simulated changes in sandbar

inundation potential, but the Program simulation indicated decreases in percentage of years with adequate nesting windows in both reaches. There is good physical reasoning why our sandbar height model should increase inundation potential downstream from the Elkhorn, and decrease inundation potential upstream. Our sandbar height model and the Program model cross around 1100 cms, which makes our model predict higher bars below 1100 cms and lower bars for flows above that magnitude. In the reach upstream from the Elkhorn, particularly the reach where the simulation was done (North Bend), the 2-year return flood is ~660cms. That is, flows of 1100 cms are more rare at North Bend than at Louisville, so our sandbar height model should necessarily predict fewer inundating events there than the Program curve. The fact that the Program model does not show this behavior is at odds with that physical reasoning. It is possible that the difference in results between the simulations is due to the fact that the papers used different rating curves, or because of slight differences in model assumptions regarding timing of sandbar habitat availability.

(3) Finally, there is evidence in the literature indicating that terns tend to nest on higher bars, and on higher parts of bars. While some of this evidence is not statistically significant (Ziewitz et al., 1992), other papers present much stronger evidence (Schwalbach, 1988; Smith and Renken, 1991; Catlin et al., 2019), and still others present evidence that is supportive but not strong (Brown and Jorgensen, 2008; Alexander et al., 2013). To be completely transparent, the memo should probably note that mean sandbar height is not necessarily an accurate representation of nest height. I remember reading a Program report that showed the extent of sandbar acreage available above some height criteria in the Program area of the Platte. So I know there have been efforts made to examine the higher parts of bars in the Program area, I am simply noting that it would seem in the Program's interest to robustly establish tern and plover nest height selection behaviors and statistics. I know that would be difficult in the Program area, but other nearby reaches (LPR, Missouri, and Niobrara) could assist in that regard. The current evidence is reasonably strong, but an investigation aimed specifically at this aspect of tern and plover nesting ecology would be a substantive contribution that would allow for a more robust inundation analysis. I have data from my dissertation fieldwork that could be used for such an analysis and, based on his recent publications, I would assume that Dan Catlin has data on nest locations after the 2011 flood from the Missouri River that could be coupled with LiDAR to examine the problem.

Again, thank you for the opportunity to respond to the contents of the memo. Apologies for taking so long to respond. I wrote much of this email in the week or so after receiving it, then spent much of the intervening period in the field, so it was hard to find time to finish a coherent response.

Regards,

Jason Alexander