Referee comment on Manuscript # hessd-11-3599-2014 – submitted by Carl J. Legleiter 20 April 2014

"Quantifying river form variations in the Mississippi Basin using remotely sensed Imagery" by Z. F. Miller, T. M. Pavelsky, and G. H. Allen

General Comments:

This paper describes a remote sensing approach to estimating river width (and, in some cases, depth) from remotely sensed data for a large watershed. The study is well-grounded in both previous literature on hydraulic geometry and current research directions in the use of remote sensing to facilitate large-scale hydrologic modeling. In that sense, the authors do a nice job of establishing the context for their work, and the general principles of their analysis are solid. Some encouraging results emerge from the study as well, in terms of the ability to infer width from even a distilled classified version of remotely sensed data in a fairly automated manner. Also, this paper highlights (but could emphasize more clearly) a potentially important finding that width is far more variable than data collected at gaging stations, and upon which downstream hydraulic geometry relations are based, would suggest. Similarly, the discrepancy among the different sub-basins of the Mississippi as far as the predictability of width (and depth), raises some interesting geomorphic questions that might be elaborated (or at least speculated) upon further. In general, I am supportive of the paper, but I do have a number of comments below that I would like the authors to consider, mainly regarding the choice of input data sets and some potential technical improvements to the RivWidth cross-section definition. There are also quite a few places where the clarity of the text could be improved, though the quality of the writing is generally quite good. Overall, I commend the authors for an interesting, well-done study and look forward to further work from this group.

Specific Comments:

1. Pg. 3603, first paragraph: This is an important paragraph that you might consider setting apart as a separate "Purpose and scope" section or something along those lines.
2. Pg. 3604, line 3: In light of this assumption, why use the NLCD rather than the original image data, which you could pair directly to the discharges recorded on the image dates. I suspect the authors have some rationale for the use of the NLCD, but I think the reasoning behind this important choice needs to be articulated clearly and explicitly in the manuscript.
3. Pg. 3604, line 10: What about reservoirs, those must have been removed, too? I'm thinking of the long, narrow lakes on the Missouri, that might be misinterpreted as regular channel reaches by an automated algorithm.
4. Pg. 3604, first paragraph: Somewhere in here you should specify which size of channels were included in this analysis – that is, how wide does a river have to be to obtain a reasonable width estimate with 30 m classification maps? This is an important point to make early on because it establishes the size and types of channels to which your results pertain.

5. Pg. 3605, line 14 – What is a typical spacing between RivWidth measurements, and how variable is this spacing? Figure 2e suggests that the spacing between widths varies as a function of planform and would be greater in meander bends. Some more detail on the spacing of width measurements seems warranted.
6. Pg. 3605, line 19 – Why such a coarse-resolution DEM rather than a ~30 m NED DEM? If there is some reasoning behind this choice, it should be explained.
7. Pg 3605, line 21 – What are the implications of pairing many width measurements with a single DEM pixel? This method dictates that many along channel width measurements will all be assigned the same discharge. If you had used a 30 m DEM and "burned" the stream into the DEM, this issue could be avoided altogether.
8. Pg. 3606, first paragraph – Seems like you had to exclude a lot of data, so a more explicit listing (perhaps a table) of what you actually used would be helpful. Also, if you had to exclude the entire Arkansas basin as the text suggests, why is it included in Figure 4? More importantly, why the broad range in predictive strength of the drainage area – discharge relation? Further discussion of these results would be welcome.
9. Pg. 3606, line 24 – This goes back to an earlier comment – what range of stream size is described by your analysis – how small of channels do your results apply?
10. Pg. 3607, lines 10-22: This whole discussion points to the question of why the NLCD was used rather than the original image data. I can imagine some reasons, but the authors should provide some solid rationale for this important choice, as this paragraph and Figure 7 clearly highlights some of the limitations imposed by using the NLCD.
11. Pg. 3608, lines 1-3: So this is essentially an extrapolation to smaller streams. OK, but I think the assessment of the validity of this extrapolation needs to be more clear; right now, it's kind of buried in a very confusing Figure 6.
12. Pg. 3608, lines 5-21 and Figure 8: Why not plot the width error against the width rather than the drainage area, which seems unnecessarily indirect if this plot is based on data from gaging stations where width was measured in situ.
13. Pg. 3608, line 26: this issue of orthogonals to the centerline was addressed by Legleiter and Kyriakidis, which provides an alternative approach that might be helpful. Fagherazzi et al also discuss how initial centerline vertices can be filtered to provide a smoother representation from which perpendiculars can be derived.
14. Pg. 3610, line 2: "expected" on the basis of what? Not clear why some of your data was excluded. Please try to explain this part of your analysis more carefully. Do the results in Figure 10 exclude the lower-discharge data?
15. Pg. 3610, line 15: OK, but what about the Missouri, why so much less variation explained in that basin? Even if you don't go into this here, you should at least mention that it will be discussed later.
16. Pg. 3611, lines 5-13 and Figure 12: Would it be better to present these results as a function of measured depth rather than discharge, which would make it easier to link Figure 12 to Table 3. Expressing in terms of discharge seems unnecessarily indirect and confounds the error in the discharge-drainage area relation, too, right?

17. Pg. 3611, line 21: Maybe include the relation for the Mississippi here again, just to facilitate comparison to the Yukon relationship.
18. Pg. 3612, line 14: OK, but conversely, what are the implications of excluding small- (or what I would consider even moderate-) sized streams from your analysis? Keep in mind that there are many more small streams than large streams in the world.
19. Pg. 3613, line 3: OK, human impacts are probably part of the reason for the disparate results in the Missouri basin, but other factors probably contribute as well and should be mentioned. For example, the Missouri is generally drier than the Ohio and Mississippi and drains an area of higher relief (the Rockies) than the purely plains (Mississippi) or eastern (Applachian) streams. I think this goes back to more fundamental controls on channel form and behavior related to the relative magnitudes of water and sediment supply a la Lane's balance.
20. Pg. 3614, lines 9-11: Another reason for looking into the Fagherazzi/Legleiter and Kyriakidis method of describing the centerline and computing orthogonals.
21. Pg. 3615, line 9: Important to add "based on in situ measurements from a limited number of carefully selected gaging stations" or something along those lines.

Technical Corrections:

1. Pg. 3601, line 23: delete "with"
2. Pg. 3601, line 30 (and throughout): italicize *in situ*?
3. Figure 6: In my version the axis label text is illegible and you can't really make out the dashed line in the figure, nor the x-axis itself. The lines need to be more distinct and the caption is confusing. I think this figure needs to be reproduced at least and perhaps a complete reworking to clarify the content, too.
4. Figure 7, reverse the x-axis so numbers increase from left to right
5. Pg. 3609, line 8: Should this be 2.8?
6. Pg. 3610, equations 5 and 6: report R^2 values for these regressions, as you have for other expressions in the manuscript.
7. Pg 3610, line 5: Should be Figure 12, not 13, as there is no Figure 13.
8. Pg. 3610, line 20: do you mean over-estimate? Seems inconsistent with the rest of the paragraph.