Response to Reviewers' Comments

We would like to thank all three reviewers for their useful and thorough review of our manuscript. We believe it will be substantially improved by the changes suggested below.

Response to Carl Legleiter (Reviewer #1)

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.

We have gone back and forth on how to organize the introduction, and we are of the opinion that having a separate 1-paragraph section at the end would be a little unusual. In general, we think of the last paragraph of an introduction as precisely what is suggested here: the place where the purpose and scope of the paper is articulated. If the editor prefers, however, we are happy to comply with Dr. Legleiter's suggestion.

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.

There are two primary reasons why we chose to use the NLCD dataset in this paper rather than going back to the original Landsat data. The first is that NLCD is a well-established product that is widely used across many disciplines. Its classification methods are thoroughly described, and as such it is probably the most robust and consistent classification over the Mississippi Basin currently available. In order to use the raw Landsat widths we would have to develop and/or test several different classifications methods, and we wanted this paper to primarily focus on the width results. However, we are currently working on a global river width data product for which we do exactly what Dr. Legleiter suggests. As such, this paper should be thought of as one step in a larger enterprise to map and analyze river widths over large areas. We will clarify our reasons for selecting the NLCD dataset here and elsewhere in the paper.

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.

Although we calculated widths for all reservoirs, we removed them from our analysis by intersecting the output of RivWidth with a layer delineating reservoir extent. We will clarify this point in the manuscript, as requested.

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.

We will clarify the size of the channels examined, as requested. In essence, while we measured as many channels as possible, we primarily analyzed channels wider than 100 m.

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.

The spacing of width measurements is always either 30 m or sqrt(2)*30 m, depending on how adjacent centerline pixels are arranged. We are not aware that pixels in meander bends are any more or less likely than other pixels to have different spacing. We will clarify in this in the revised text.

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.

In response to points 6 and 7: From the perspective of calculating basin area, which is the sole purpose of the DEM in this case, we believe there is likely to be little difference between the results of the 30 m NED and the 90 m hydrosheds product. Because the NED is not hydrologically conditioned a priori, it would take a very large amount of work to get accurate basin areas. This would require not just burning in the RivWidth streamlines but also correcting errors upstream of these streamlines. We initially attempted to use a 30 m DEM (the raw SRTM product) instead of hydrosheds, but we found that the processing capacity of our computers was unable to handle the order of magnitude greater intensity. Given the large number of data points used here, assigning an average of <10 rivwidth pixels to each DEM pixel is not likely to substantially impact statistical measures used here.

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.

We include the Arkansas data in figure 4 to demonstrate the difficulty in extracting mean discharges from drainage areas in that basin. We agree that it would be useful to specify which data was included/excluded. We will include the following table in the revised manuscript, along with accompanying text replacing the final sentence of 2.3:

"In 7 accounting units containing RivWidth measurements in the Missouri, 12 in the Lower Mississippi, and the entire Arkansas basin (excluding the White River), lack of gauging stations, substantial precipitation variability, or large-scale water withdrawals precluded gauge-based discharge estimation. These subbasins, along with those not containing rivers large enough to be measured by RivWidth, are not considered in the DHG portion of our analysis."

				Region	
	Ohio	**Upper Mississippi**	**Missouri**	**Arkansas**	**Lower Mississippi**
Accounting units excluded from DHG estimates	None	None	100200, 100302, 100402, 100500, 100901, 100902, 101301, 101302, 101303, 101600, 101702, 101800, 101900, 102100, 102500, 102802	All basins other than 110100 (Upper White River) excluded)	080202, 080204, 080302, 080403, 080701, 080702, 080703, 080801, 080802, 080901, 080902, 080903
Total area included (*excluded*) in DHG	527900 km^2 (*0 km^2*)	429200 km^2 (*0 km^2*)	727600 km^2 (*621700 km^2*)	57900 km^2 (*584400 km^2*)	119600 km^2 (*129400 km^2*)

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?

As specified earlier, we will be more explicit about the size of channels observed here. The answer is largely addressed in Figure 7, but we will clarify here, as requested.

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.

We are unclear on how using the original images would help improve this part of the analysis. Keep in mind that it is very unlikely that USGS width measurements would coincide with cloud-free image collection dates. As such, even with the original imagery it wouldn't be possible to directly compare *in situ* widths against remotely sensed widths without making most of the same assumptions used here. We are not sure how Dr. Legleiter envisions Figure 7 being modified if we had used the raw imagery. Perhaps he would like to clarify.

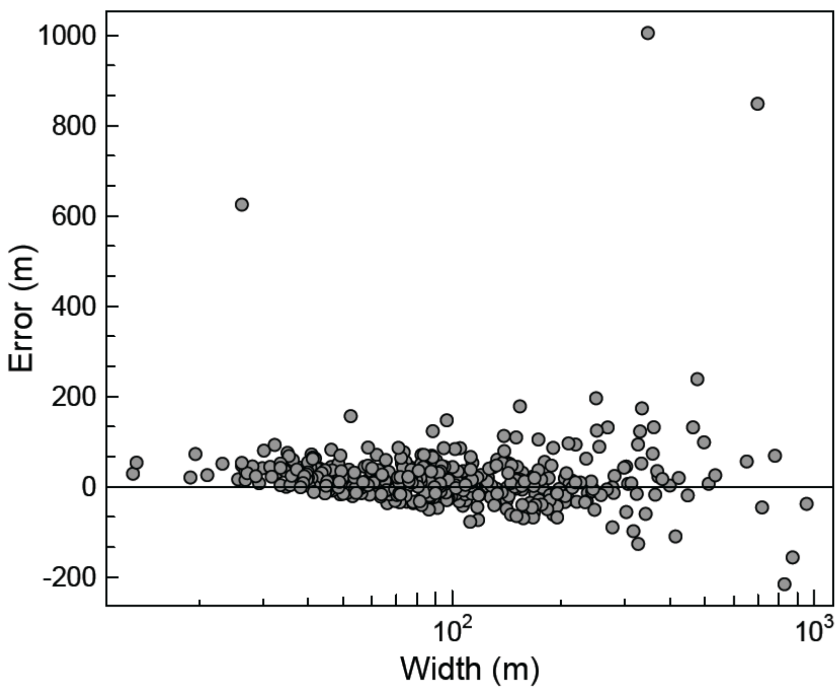
11. Pg. 3608, lines 1-3: So this is essentially an extrapolation to smaller streams. OK, but I think the assessment of the validity of this extrapolation needs to be more clear; right now, it's kind of buried in a very confusing Figure 6.

We agree that this text could be clarified, and we propose to change it as follows:
"We use two separate methods to estimate the actual length of rivers between 50 and 100 m in the Mississippi Basin. First, comparison with USGS gauge data suggests that RivWidth measured ~68% of gauges 50-100 m in width. We use this percentage as a correction factor, dividing the number of 50-100 m river measurements made here by 0.68 to estimate the correct number of measurements (the dashed box in figure 6). Second, we use equation 2 to extrapolate from the distribution of measurements for rivers wider than 100 m to those between 50 and 100 m in width (the dot in figure 6). These two methods produce nearly identical values."

As a side note, there must have been some sort of software-related degradation in the version of Figure 6 viewed by Dr. Legleiter. The version that we see in the manuscript pdf is quite legible and entirely consistent with what we submitted.

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.

We will include a substitute Figure 8 showing error as a function of USGS measured width. Changes to the figure do not alter our analysis in any significant way.



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.

We have examined the work of Legleiter and Kyriakidis, and we agree that it might potentially offer a more robust method for calculating orthogonals. However, implementing it in RivWidth would be a substantial undertaking, and it is unclear how much this effort would change the results shown here. If the editor feels it is essential for us to do so, we will comply. However, we believe that this modification is better suited to upcoming work of a global nature.

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?

Figure 10 does not exclude the lower-discharge data. We offer the following revision to page 3610, paragraph 1 to clarify why we separately calculated DHG for > 10 m^3/s rivers:

"However, these values include 38 654 width measurements corresponding to discharge values less than 10 m3/s, which are lower than would be expected for rivers greater than 30 m based on width-discharge relationships from Moody and

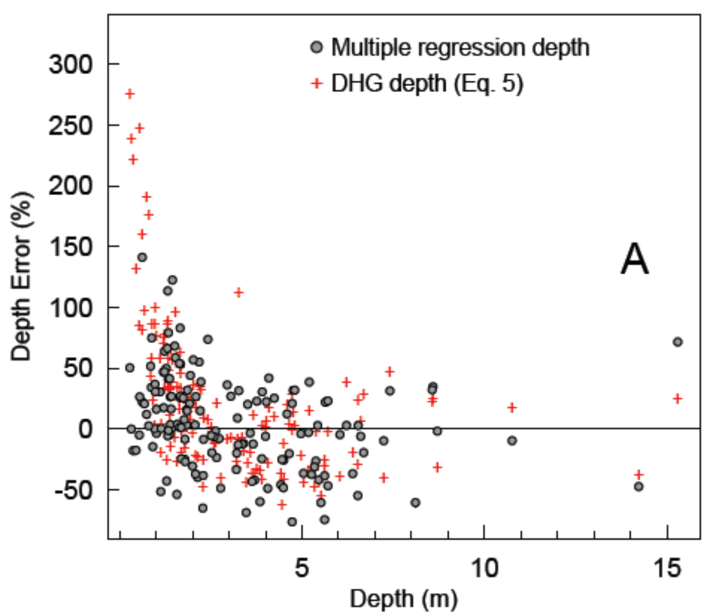
Troutman (2002) and Leopold and Maddock (1953). 89 % (34 573) of these low-discharge measurements are found in the Missouri sub-basin, where braided streams with high width-depth ratios are common. Of 38 USGS gauging stations with mean discharge < 10 m^3/s, width is overestimated in all with a mean bias of 52 m (Fig. 8). As such, it is likely that basin-wide widths for discharges below 10 m^3/s result from the inability to resolve multiple channels at the 30 m resolution of the NLCD. If we remove these anomalous measurements, the width DHG equation becomes:"

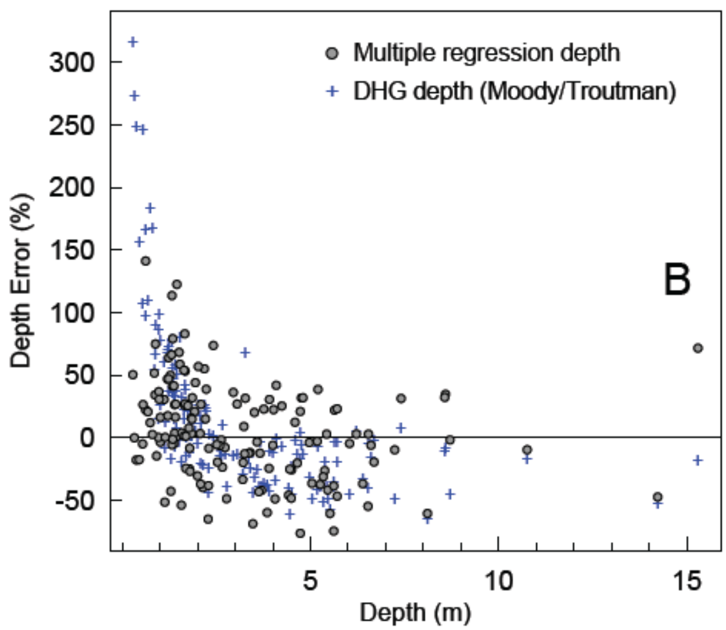
15. Pg. 3610, line 15: OK, but what about the Missouri, why so much less variation explained in that basin? Even if you don't go into this here, you should at least mention that it will be discussed later.

We will indicate here that the case of the Missouri will be discussed in more detail in the Discussion Section (section 4), as requested.

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?

We agree that it would be more useful to show figures with width on the x axis, so we will include the following updated Figures 12 A-B (and will change the text to match):





17. Pg. 3611, line 21: Maybe include the relation for the Mississippi here again, just to facilitate comparison to the Yukon relationship.

We will restate the relationship for the Mississippi here, as requested.

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.

We had intended this sentence to specifically make the point identified by Dr. Legleiter here: the fact that e.g Moody and Troutman include much smaller streams likely improves the fit of their regression line compared to our study, which does not include very small streams. We will modify the text to clarify this point.

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.

We agree that it would be useful to include a bit more discussion of the impact of physiographic setting on DHG in reference to the differences observed

between the Missouri and Ohio Basins. We will add the following text on Page 3613, line 5:
"In addition, the substantially drier climate and greater topographic relief in the upstream portions of the Missiouri, relative to the Ohio or Upper Mississippi, may also influence the variations in DHG observed here by affecting the balance of water and sediment supplies in the different subbasins."

20. Pg. 3614, lines 9-11: Another reason for looking into the Fagherazzi/Legleiter and Kyriakidis method of describing the centerline and computing orthogonals.

We agree that this is the principle area in which moving to the Legleiter/Kyriakidis method would improve our analysis. As mentioned above, though, this would be a major undertaking and we are not convinced that it would change any of our results or conclusions significantly. As such, we propose to implement it for future papers rather than in this one. In this paper, we propose to add the following sentence on page 3614, line 10: "Other methods of calculating orthogonals to the river centerline, especially implementation of algorithms described by Legleiter and Kyriakidis (2006), may help to minimize this source of error in future studies."

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.

We will add the suggested text or something very similar, as requested.

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 R2 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.

Technical corrections:
We will make all of the requested modifications. The one caveat is noted above: there must be some difference in how various pdf readers deal with Figure 6. The figure is very clear in both readers tested here, but we will do further testing to make sure it appears correctly in all readers.

Response to Mark Fonstad (Reviewer #2)

1. It isn't clear how reservoirs along the rivers were handled in the extraction and computation of the widths.

As discussed in response to Dr. Legleiter's point #3: Although we calculated widths for all reservoirs, we removed them from our analysis by intersecting the output of RivWidth with a layer delineating reservoir extent. We will clarify this point in the manuscript, as requested.

2. P. 3600, line 5: strictly speaking, it isn't really correct to say that these relationships were "derived". Derivation implies deduction, whereas these relationships are developed through empirical induction.

We will replace the word "derived" with "developed" on this line.

3. P. 3600, line 15: Assumptions don't really "characterize" variability; the DHG estimates do.

We will modify this sentence to read: "Results suggest that channel geometry derived from remotely sensed imagery better characterizes variability in river form than do estimates based on DHG."

4. P. 3601, line 11: Actually, description of the DHG goes all the way back to da Vinci in the early 1500s.

It is true that understanding of the basic principles of fluvial networks go back much further than was discussed in our original draft. We propose to modify the text on Page 3601, line 10 as follows:
"Because of their wide-ranging importance to science and engineering, spatial patterns of channel shape have been studied since at least the work of Leonardo Da Vinci in the 16th century (Humphrey and Abbott, 1867; Bellasis, 1913; Shepherd and Ellis, 1997)"

Shepherd, R.G. and B.N. Ellis (1997), Leonardo da Vinci's Tree and the Law of Channel Widths—Combining Quantitative Geomorphology and Art in Education, *Journal of Geoscience Education, 45*, 425-427.

5. P. 3601, line 20: There are some versions of these equations that have physically derived DHG relations, mostly their exponents.

It is true that there are some studies that have attempted to determine DHG coefficients and, especially, exponents based on physical principles. We will change this sentence to read ". . . are exponents and coefficients either derived

from physical characteristics of the channel or, more commonly, calculated empirically."

6. P. 3603 and 3604, multiple places: the issue of return period influencing the coefficients and/or exponents in the DHG formulations has been handled here with an assumption of the width being representative of "mean growing season streamflow". While this is a reasonable starting point, it should be made clear that understanding the problem of return period on these relationships is very important to better interpreting these river systems and the methods to analyze them.

We agree that analyzing widths at a constant return period is important, which is why we discuss it in detail in section 2.2. We believe that adding additional discussion of this point here would more or less duplicate what we've already written less than a page later. If, however, the editor feels that we need to add additional text addressing this point, we will do so.

7. P. 3606, lines 16 – 18: it would be useful to know if the authors split their data prior to calibration, in order to save some data for validation, or whether all the data were used for both calibration and validation.
<Zach>

8. P. 3607, equation 2: What quantity does "n" refer to? Is it the number of river pixels with a corresponding width, or some other metric?

Yes, "n" refers to the number of river pixels with a corresponding width. We will add text on Page 3607, line 10 indicating this:
", where *n* is the number of pixels of a corresponding width and *W* is the width."

9. P. 3607, line 8 and figure 2: Widths below 100 meters are almost certainly drastically under-represented, because small river widths are clearly going to be mixed in with land signal or covered by vegetation. I'm not sure of the utility of even having the bars below 100m. Fractal river theory would suggest these bars should continue their exponential count upwards with decreasing width until a much smaller length scale is reached than 100m.

We agree that there are major limitations to our dataset below 100 m, and we included the bars in Figure 6 for widths less than 100 m to show that we are not capturing all rivers at these widths. We will add the following sentence on Page 3607, line 10 emphasizing this:
"Bars for rivers <100 m in width are included in Figure 6 to indicate the distribution of width data analyzed here, but because we do not capture all rivers at these widths our dataset cannot be used to describe the true distribution of rivers <100 m wide."

10. P. 3609, lines 3-5: What about “error” due to USGS widths at unusually narrow “stable” locations where gauges are often placed? A source of systematic bias?

We absolutely agree that USGS gauges underestimate the true distributions of widths, but we are not sure it causes significant problems in this case because we are comparing widths at the same location. We have two additional papers (one in press for an AGU monograph, one in review at *GRL*) that systematically tackle the problem of underestimation of river widths from DHG relationships compared to measured distributions. As such, we would prefer to not add discussion on the topic in this paper, since it will be handled much more comprehensively elsewhere.

11. P. 3609 and 3610: Equations 3 – 6 would benefit from having standard error of the estimates (or similar metrics) included.

We will add r^2 values to all of these equations to indicate goodness of fit, as requested.

12. P. 3613, line 25: This point raises an important physical question. Why would having width help in reducing uncertainty?

We agree that this question should be more clearly addressed. We will clarify by adding the following text on Page 3613, Line 25:
“This improvement results from the underlying assumption of continuity in the relationship between depth, discharge, width, and velocity; measuring width while assuming locally constant flow eliminates one degree of freedom from the depth equation.”

13. P. 3613 and 3614: Cross-correlation of W and D, as well as autocorrelation in W would be interesting and useful metrics at some future stage, and they may influence some of the statistical tests already described.

We absolutely agree that cross-correlation between different variables, along with spatial autocorrelation in widths, would be very interesting to examine. We would prefer to leave them to another paper.

14. Alternate river-centered coordinate approaches, such as Legleiter and Kyriakidis, have also discussed these issues.

As discussed in response to points 13 and 20 in Dr. Legleiter’s review, we agree that work by Legleiter and Kyriakidis can potentially improve future studies of river width. However, implementing and testing an algorithm similar to theirs on a continental scale will require a large amount of work. We propose to acknowledge their work on the subject here (see our response to point 20 in Dr. Legleiter’s review) and begin work on implementation and testing of some of their ideas for a future study.

Response to Reviewer #3 (Anonymous)

1. The paper indicates that it considers channel form, but in fact it addresses only the cross-section geometry. Channel form also includes the downstream dimension, reflected by channel slope and by planform shape (e.g. meander length). Bjerklie (2007) (Bjerklie, D.M. 2007. "Estimating the bankfull velocity and discharge for rivers using remotely sensed river morphology information", Journal of Hydrology 341: 144-155), Williams (1986)(Garnett, W.P., 1986. River Meanders and Channel Size, Journal of Hydrology, 88 147-164), Jansen and others (1979)(Jansen, P., van Bendegom, L., van den Berg, J., de Vries, M., Zanen, A. 1979. Principles of River Engineering, Pitman, p. 509) have shown that the meander length (and thus channel planform features in general) are correlated to channel cross-section depth and velocity (and thus flow resistance). Similarly, the channel slope is also a key variable in the relation between cross-section geometry and discharge. Therefore these additional channel form variables can provide important predictors for channel geometry-discharge relations. It is suggested that, in keeping with the stated goal to consider channel form and use remote sensing as a primary data source, channel slope and planform be discussed in the introduction and in the discussion/conclusion section as variables that could/should be included to develop more robust, and more reach specific channel geometry relations. It is recognized that channel slope and planform shape data sets are not readily available, however remote sensing offers the opportunity for these to be measured throughout a channel network.

We agree that there is more to channel form than just width and depth, although we feel that full consideration of other variables such as planform shape and slope are beyond the scope of this paper. We already mention slope in the introduction, but we will add a mention of planform shape, along with the Bjerklie and Garret references, to Page 3600 lines 21-24. In addition, we will add the following sentence to the discussion section (Page 3614, line 23): "In addition, because RivWidth produces maps of river centerline it may be useful in characterizing the planform shape of rivers (e.g. via indices of sinuosity and braiding), which would help to reveal downstream patterns in river form. Additionally, intersection of river centerlines with a high-resolution DEM would allow estimation of mean slope, another key variable in understanding river form (Bjerklie 2007)."

2. It is important to understand that the relations developed here are assumed to be associated with a particular flow event – the mean flow – but the width measured is not necessarily associated with the mean flow, and therefore contributes to some of the variability in the width distribution particularly as it relates to the mean discharge. Additionally, the mean discharge itself is not necessarily comparable between gaging stations due to differing lengths of record and time periods. These

issues introduce unknown errors that are not easily addressed. However, in the discussion the possible implications of these issues could be pointed out and the importance of statistical approaches to analyzing and interpreting the data discussed.

We acknowledge the uncertainty resulting from the use of mean discharge with imagery incorporating non-mean flows in our response to Reviewer #1's comment 2. To address the comparability of mean flows across gauging stations, we will clarify our methods by adding the following in page 3605, line 6:
"We removed gauges with fewer than 10 years of mean discharge data and those with no discharge or channel measurements after 1970".

3. The resolution of the pixels used to estimate the widths will have a greater effect on accuracy the smaller the river becomes. River size has a lot to do with the error and thus it might be helpful to develop the hydraulic geometry relations for different size classes of rivers and see if there is a large difference.

While it is true that the fixed pixel resolution of the NLCD data will result in greater percentage error for smaller rivers, the absolute error (in meters) appears to remain quite similar for rivers of different size, at least down to relatively small rivers (figure 8). In addition, comparing rivers of different sizes inevitably would inevitably also require comparing rivers with differences in other characteristics (channel substrate, slope, etc.). We agree that additional comparisons among DHG relationships for different types of rivers would be useful, but we believe that to do it well would require an additional paper rather than additions to this one.

4. There is no discussion of whether a minimum size of watershed and or discharge was used to reduce the number of discharge relations developed. This should be included in section 3.4

The 30-m resolution of the NLCD data inherently restricts measuments to larger rivers, and the resulting range of watershed sizes and discharges here is narrower than in most existing studies (e.g. Moody and Troutman, 2002). For clarification, we will add the following to page 3609 line 22:
"Measured widths correspond to discharges ranging from 2.6 m^3/s to 19200 m^3/s and drainage areas from 169 km^2 to 2940000 km^2."

5. Additional data and study of regional relations between hydraulic variables in a cross-section have been published by the USGS (see Osterkamp, W. R.; Hedman, E. R., 1982, Perennial-streamflow characteristics related to channel geometry and sediment in Missouri River basin, USGS Professional Paper: 1242 (http://pubs.er.usgs.gov/publication/pp1242)). These previous studies can be referenced to provide additional comparison and validation.

We thank the reviewer for pointing us to this useful paper. We will add it to our references and cite it in the text on Page 3601, line 28.

6. Bjerklie and others, 2003, (Bjerklie, D.M., S. Lawrence Dingman, Charles J. Vorosmarty, Carl H. Bolster and Russell G. Congalton, 2003. "Evaluating the potential for measuring river discharge from space", Journal of Hydrology, vol. 278 no. 1-4 pp. 17-38) showed the importance of multivariate equations and inclusion of channel slope to improve the predictive qualities of general hydraulic relations for rivers. This is expected, as the more pertinent information is brought to bear to the prediction, the prediction will be better. The implication of multi-variate relations is that they are no longer directly derived from continuity and dimensional analysis. To accommodate multivariate relations, perhaps it can be suggested that the original definition of hydraulic geometry be expanded to include flow resistance as indicated by channel planform and slope.

We will add the following sentence acknowledging the potential of multivariate equations for prediction of river characteristics such as discharge (Page 3614, line 21):
"In addition, multivariate equations for prediction of streamflow (e.g. Bjerklie et al., 2003) often combine river width measurements with data on slope and other river form data. "