

# ***Interactive comment on “Spatial stochastic and analytical approaches to describe the complex hydraulic variability inherent channel geometry”*** **by N. Hadadin**

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Received and published: 7 October 2011

Dear reviewer Referee #2

I would like to thank you for reviewing the article. Please find below your comments versus my replies, (Corrections are shown in a red-colored text).

comment: 1. Does the paper address relevant scientific questions within the scope of HESS? Yes. The manuscript does address the important topic of estimating properties of channel geometry for stream restoration and other hydrologic applications.

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reply: this the only positive comment from the reviewer, that is ok.

comment: 2. Does the paper present novel concepts, ideas, tools, or data? No. The idea of relating drainage area to characteristics of channel geometry is not new and there are other publications that use this approach. The author needs include these references in the manuscript and explain how this manuscript offers a new and unique contribution to this topic. Examples of similar publications include: Moyer, D.L., and Bennett, M.R., 2007, Development of relations of stream stage to channel geometry and discharge for stream segments simulated with Hydrologic Simulation Program–Fortran (HSPF), Chesapeake Bay Watershed and adjacent parts of Virginia, Maryland, and Delaware: U.S. Geological Survey Scientific Investigations Report 2007–5135, 83 p.

Lotspeich, R.R., 2009, Regional curves of bankfull channel geometry for non-urban streams in the Piedmont Physiographic Province, Virginia: U.S. Geological Survey Scientific Investigations Report 2009–5206, 51 p.

Wohl, E., J.N. Kuzma, N.E. Brown, 2004, Reach-scale channel geometry of a mountain river, Earth Surf. Process. Landforms 29, 969–981.

reply: The idea in this paper is new for incised channel, it present new data for incised channel. The references that the reviewer cited are for stable channels (streams in the Piedmont Physiographic Province, Virginia are stable channels) . Also, this study related to HSPF function tables were initially generated for the gaging stations. The result of my manuscript can apply for ungaged station. For the third reference this paper takes the flow discharge as independent variable which is different idea from my research. I used drainage area as independent variable not flow discharge. There are many references talk about channel geometry but I highlighted on those that related to my research. The reviewer needs to distinguish between stable and incised channel. My manuscript is focused on incised channel. Also, I think the reviewer is in confuse, he doesn't understand what I mean by incised channel and what are thier phases.

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Please see Figure (1) for Incised Channel Evolution Sequence

comment: 3. Are substantial conclusions reached? No. The manuscript does not include a results section and skips from a methods section to a discussion and conclusions section. Furthermore, the regression equations shown on figures 2 through 9 have fairly weak relations between drainage area and channel characteristics. No validation experiments are performed to show how these equations would work in a predictive mode at ungauged basins, which is the primary motivation for the work. Furthermore, the author does not present prediction intervals around the regression equations to address their uncertainty.

reply: "Result" section will be added. Figure 3, the relations between channel width, depth, flow discharge and cross sectional area versus drainage are strong not weak as the reviewer mentioned, these relations are with coefficient of Determination ( $R^2$ ) range from 0.73 to 0.83 for CEM Types II and III. The accuracy in predicting the hydraulic variables (flow discharge, width, depth, longitudinal slope. . . is more reasonable in the streams CEM Types II and III than the streams CEM Types IV and V, this will be addressed in revised manuscript. This is the new finding from this research. For validation there aren't enough data

comment: 4. Are the scientific methods and assumptions valid and clearly outlined? The use of linear regression to relate channel geometry to basin characteristics is a valid approach, although not novel.

reply: There are many researchers used linear regression and it is a valid approach and the three references that the reviewer mentioned in the second comment based on the linear regression, as I have used.

comment: 5. Are the results sufficient to support the interpretations and conclusions? No. Please see the answer to question (3). In order to understand the predictive power of the regression equations, the author must include prediction intervals or present cross validation results. There is limited interpretation of the results other than reporting

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basic regression statistics.

reply: validation of result need additional data , for example, the result I got for slope is consistent with The United States Army Corps of Engineers (USACE, 1990), see second paragraph page 6972

comment: 6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? The author does not describe how the channel geometry features were computed. Were the features computed from field observation? If so, please cite the relevant field protocols. Other than this question, I believe the results could be reproduced based on the data and descriptions presented.

reply: the data I used is from channel evolution channel (CEM), Colorado State University open file by Prof. Chester Watson

comment: 7. Do the author give proper credit to related work and clearly indicate their own new/original contribution? No. Please see the answer to question (1). The author does not make reference to other publications that relate properties of channel geometry to basin characteristics and, therefore, the manuscript is not put into the context of other work in this area. The author needs to mention if only unimpaired locations were used and explain the reasons why the Yazoo Basin was selected for this study. The author also needs to explain the significance of the Type I-V reaches and why this demarcation is important to this study.

reply: I surprise from this comment, this is illogical and unscientific comment. I think the reviewer did not look to the references, all the references that I mentioned are related to channel geometry. Yazoo Basin was selected because in this basin there are many incised streams that appropriate to this study, and the data are available to conduct such this research.

comment: 8. Does the title clearly reflect the contents of the paper? No. The title

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of the manuscript does not reflect the contents of the manuscript, particularly the use of the tem “stochastic” and “analytical.” It is unclear where in the manuscript these two properties are addressed in the author’s approach and these terms are not used anywhere else in the manuscript in a meaningful way. From what is presented in the text, the author uses linear regression to relate drainage area to characteristics of channel geometry, which is neither “stochastic” nor “analytical.”

reply: the title is changed to: statistical analysis to describe . . . . .

comment: 9. Does the abstract provide a concise and complete summary? No. The abstract is too long and does not discuss the unique contributions of the manuscript. I also do not believe the regression results show a “good fit” (p. 6968; line 15). This is not shown in the manuscript.

reply: the abstract is modified

In this paper many of the regression results are reasonable see figure 3 Also there are reasonable results in table 4, unfortunately, the journal team made error in preparing the text, and they duplicated table 3 and table 4 (these two tables are the same). Anyway, I correct this mistake in revised manuscript.

comment: 10. Is the overall presentation well structured and clear? No. The manuscript lacks a results section and the introduction lacks a clear problem statement and hypothesis. Results are buried in the methods and discussion sections. It should be noted that the abstract and introduction are the longest sections of the manuscript. There is also no study map presented, which is particularly relevant given that this is an international journal.

reply: Result section will be added and study map is added too,

comment: 11. Is the language fluent and precise? No. The author needs to better organize the manuscript and be clear about the unique contribution of this work. Relevant literature needs to be cited and the abstract and introduction need to be shortened

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and made more concise.

reply: there are many statements are rephrased, and the abstract becomes shorter

comment: 12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Figures 3-9 need to define the symbols Q, W, A, d.

reply: these hydraulic variables are defined in symbols and abbreviations see page 6983

comment: 13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? There are more substantive issues with the manuscript that need to be addressed before this can be evaluated.

reply: all the substantive issues in our point of view I replied them, see my responses on your comments, also some corrections is appear in red colored text in revised manuscript

comment: 14. Are the number and quality of references appropriate? No. Please see answers to questions (1) and (7).

reply: other references are added, note there are tons of references about the hydraulic geometry, but I highlighted on those that related to my work.

comment: 15. Is the amount and quality of supplementary material appropriate? There is no supplementary material presented.

reply: supplementary materials were presented, such as: map of the site is added figure 2, and the data were presented in table 1 and 2

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/8/C4347/2011/hessd-8-C4347-2011-supplement.pdf>

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