Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2015-526-RC1, 2016

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Vegetative impacts upon bedload transport capacity and channel stability for differing alluvial planforms in the Yellow River Source Zone" *by* Z. W. Li et al.

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Received and published: 22 February 2016

In this study, the authors attempted to explain the different channel plainforms of four reaches in the source area of Yellow River, China using (partially) measured water discharge, stage, and cross section data, as well as qualitative description. First, I think this area is unique regarding to the world large rivers and thus is worth studying. Second, the river dynamics in this area is very complex and hence is very hard to capture. Therefore, I think this study is significant and potentially very useful for understanding the river environment in source areas for large rivers in general. However, I think the authors need to fix a series of problems in the current manuscript before it reaches the level for publication. I describe them in details: Introduction: the authors spend

too many sentences describe the progress in channel plainforms, in particular braided and anabranching rivers (pages 2 to 5). Instead, I think they should reduce these part. In the meantime, they should expand the studies on river diversity in the source area of Yellow River (lines 17-31, page 5) and explain what we need to explore further in details, which could lead to the objectives of this study. There are quite a few English problems in this section. Sections 2 and 3: these two should be combined into one section. Also, Figs 1 and 2 should be combined. Section 4: it is very important that the authors specify what value of Manning's n are used for each of the four reaches when they introduce their model because comparison of their difference would provide a quantitative means of showing the impact of vegetation on river morphology. The sentence in lines 4-5 on page 9 does not make sense. Section 5.1.1: Page 10, Lines 14-15: what is the difference between middle and high flood stages? It might be more informative if this description is tied to Figure 8. For example, can one say that middle flood stage may be represented by the high discharges in September and high flood stage may be reflected by the high discharges in July? Page 10, Line 18: if a water depth of 2.0 m represents the bankful discharge, then what does the water depth of 3.0 m represent? Can I say that h = 2.0m is the height at the top of the stable bars in middle channels? Page 10, lines 23-32: the message delivered by this paragraph is very vague. It seems to me that the data in October in both 1968 and 1984 follow the curve formed by the data in June and July. If the authors believe there are significant difference between June and July, and August and September, why not use the data in the two periods to run non-linear regression (power function) and see if the exponents of the two are significantly different? The authors should explain quantitatively the geomorphological significance of the two different trends in Fig. 8a and 8b (i.e., the trend formed by the data in March and April against the trend formed by the remaining data). Also, the difference between the high scatter trend for low discharges (probably low flood stages) and regular trends for high discharges (probably high flood stages) should be elaborated. The key is to explain why channels in this reach is semi-braided and semi-anabranching. My guess is vegetation on bars assures that during low and middle flood stages, bars and islands are relatively stable, while during high flood stage, they are unstable. Figure 8 should be used to make this point clear. Page 11, Lines 1-11: this paragraph is about Fig. 9. I think the figure shows a completely different aspect of stream channels in this reach: channel morphology before 1976 is different from that after 1976. This difference is represented by the two different trends of the data. The authors should run non-linear regression to establish power functions for the two different trends in each listed month and then link this difference to the possible difference of vegetation cover in the two different time periods. This would strengthen the analysis a lot. Fig. 10 is not well tied to the data shown in Figs. 8 and 9. It is nice, but there lacks evidence to support it. Section 5.1.2: First, the authors should mentioned Fig. 11 first and then Fig. 12. Second, the big problem here is that the postulation raised here (lines 15-19 on page 11) is not fully supported by the only data shown in Fig. 11. The stage data in Fig. 11 are not sufficient to argue the change of flow regime exactly because the channels here are anabranching channels. This means that the same flow stage in different seasons might be associated with different water discharges. Maybe there are no water discharge data available in this reach. If this is the case, the authors should re-think their arguments: the fact that these channles are stable means that sediment (bedload) supplied from upstream (i.e., the Dari reach) is balanced by the sediment transport capacity in this reach. One way might be useful is to compare the supplied bed load based on the prediction made for the Dari reach with the transport capacity predicted in this reach. The authors should expect that they are similar or very close to each other. Then, the impact of vegetation on the hydraulics might be reflected in Manning's n used in the bedload model. Comparing this value with the one used in the Dari reach may show the impact of vegetation on the stable status of this reach. Section 5.1.3: This reach is a tributary. If the authors have no water discharge data in this tributary, I suggest to delete this part completely from the current manuscript. This is because only showing a postulated diagram (i.e. Fig. 13) is insufficient to convince the readers about the status of this reach. Section 5.1.4: This reach is unstable. Again, just using the temporal changes of channel cross

sections between three years (i.e., Fig. 15) is not enough to explain how vegetation affects them. Again, I think it is very important for the authors to predict bedload transport rates and then use them to calculate the mean sediment load in this reach. By comparing this (or these) mean value(s), the authors may argue that why the reach is not stable. In the meantime, comparing the value of manning;s n used in this reach with those used in the first and second reaches along the main river would provide evidence of the impact of vegetation on river morphology. Minor points: Lines 5-8 on page 6: this description is very confusing; Lines 17-18 on page 7: why should the stable reach have high bedload transport capacity? Lines 4-5 on page 9: what does the rivers in an arid area have anything to do with rivers in the study area? Figure 1: Please mark R1, R2, R3, and R4. Also, only use the arrow to show the direction of flow. In the legend, 'Tributary' and 'Trunk stream' should be reversed. Please use 'Main stream' rather than 'Trunk stream'; Figures 3-6: these figures should be combined into one figure; Figure 8: please use the same legend for the two figures; Figure 14: it does not help much in understanding the difference between the regular and flood conditions;

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