Earth Surf. Dynam. Discuss., 2, C556–C564, 2015
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Earth Surface Dynamics Discussions

Interactive comment on "Morphology of the Kosi megafan channels" *by* K. Gaurav et al.

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Received and published: 19 January 2015

Comments from referees in black color text
Authors's response in blue color text

This paper analyses field measurements from the Kosi megafan to determine the morphological characteristics of braided and isolated channels over the fan area, to elucidate whether commonalties can be found between these two types of channel. This paper provides a valuable set of field measurements in an area where these are scarce; however, the structure of the paper, number and type of measurements taken and the lack of depth in the interpretation and discussion of what is presented leads me to suggest that the paper would need to be substantially re-written and incorporate more data before it could be accepted for publication. Further details related to this are provided

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below.

Specific comments: Field data: the chosen location of the transects is unclear and a more detailed explanation about sampling strategy used needs to be undertaken, as well as clearer indication of their location on the map provided. There is also a disparity between the number of measurements from the braided and isolated channel examples, and explanation needs to be give as to why this is. Detailed measurements of width, depth and discharge have been provided, but only a small number of locations had associated water surface slope and grain size, again it needs to be expanded on why this is the case. And little was made of the grain size measurements that were taken. A major problem with the data presented is the lack of any reference to the bedload data – this is an important consideration in the method that they have presented (as the authors themselves state in the conclusion) and I cannot see how they can draw any conclusions without these data included.

The sampling strategy was dictated by the physical accessibility of rivers. The duration of measurements was long and performed in difficult field conditions, especially for alongstream surface slope measurements. This explains the limited number of threads measured despite two field campaigns exclusively devoted to this acquisition. This also probably explains why such a dataset does not exist at present for large sandy streams. Although we agree that the number of measurements is limited it is, at present, the only database on such a subject and it is representative of what can be done during a field based PhD on large morphologic objects. Furthermore statistical analyses show that the dataset can be used for comparison between thread types

Little mention is made of the fact that the study area is located on a megafan – some consideration needs to be given to the fact that fans can react differently to other braided rivers systems and in particular slope and downfan grain size are important considerations, both of which have been overlooked in the data presented. In the field

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site overview it was mentioned that sediment composition was consistent over the fan area – more data or references need to be provided to support this as this is rarely the case in megafan environments.

Our attention here is not on megafans and we will try in the paper to stick to its objectives but we followed the reviewers point as concerns the grain size. Distributions were measured at different places near all hydrologic and morphologic sampling sites. And yes the grain size varies along the Kosi and seepage channels. Our measurements now enable us to fully include specific grain sizes in our analysis and not to depend on any average value of the grain size over the fan.

The data interpretation and discussion section of the paper are weak, and a more robust explanation of the results needs to be made. In particular more emphasis needs to be given to relating the findings from this study to broader fluvial research and what these results will bring to braided river research and a more detailed understanding of the Kosi megafan. At the moment there are valid research questions set out in the introduction, but the data and conclusions do not adequately address these in the remainder of the paper.

We probably have been unclear about our goal and need to restate our objective here. All sedimentologic and geomorphologic analyses of braided channels assume that individual threads behave similarly to single threads of meandering channels. Yet, to our knowledge, this has never been proven on the field. Furthermore we now show in the introduction that the physics of flow and sediment transport that sets the regime equations at the thread level in a braided channel is lost through the scale integration process. It is therefore of much interest to study the geometry of single threads from a braided channel and compare them to threads of meandering channels in order to

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see if the knowledge acquired during the last decades on hydraulic geometry of single thread rivers can be extended to braided threads. This is the purpose of our study.

The structure of the paper as it stands is confused. There are sections of the results that belong in the methods or even the introduction, the authors need to make sure that the theory is clearly presented at the outset, they then present and explain thoroughly the data that they are presenting and then discuss what this means in the latter stages of the paper – new theory that aren't related to their own data shouldn't be included in the results section. These have been highlighted in the list of detailed corrections below.

Our purpose in writing was to have readers follow our progressive reasoning on the matter discussed. We understand the criticisms made and have reorganized the paper to fit reviewers 1 and 2 comments while keeping a causal chain of thoughts.

Terminology is used inconsistently and I have highlighted examples of word usage that is not appropriate. Particular care needs to be taken with the words 'thread' 'stream' 'channel' which all seemed to be used throughout the paper, better to select one of these and use it consistently. Also consider changing 'decomposition' when talking about the division of channels, this generally means to break down rather than divide and so is confusing in this context. The paper would benefit from a thorough edit to ensure consistent terminology.

The reviewer is right and we have changed the terminology to be consistent throughout the paper. We will stick to the terminology defined by Métivier and Barrier (2012) in their review paper on chanel metamorphosis.

Minor Corrections:

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Abstract Lines 2 and 3: consider changing terminology from ‘streams’ to channels Line 7: change “Their average slope. . .” to “The average slope. . .” Additional information to be added to the abstract to outline what the main results were and explain the importance of the main findings in the context of wider river research.

Thanks to the reviewers’ comments we have clarified the terminology in the introduction, and the text. We have also expended the abstract. We try to be consistent and use the terminology of Schumm (1977) that we adopted earlier in Métivier and Barrier (2012).

Section 1: Introduction The aim and research questions of the paper need to be clearly identified at the end of the introduction.

This paper is intended to compare the morphology of threads of sandy braided and meandering channels. Although the similarity of these two types of threads has been postulated before and forms the basis of many sedimentologic studies, it has never been really assessed.

General comments:

Section 2: The Kosi River megafan 1025 Line23: ‘a series of avulsions’ – need to expand on this – when did these occur and what was the magnitude? Some indication of the flow regime would be useful 1026 Line 9: Why ‘seepage channels’? Not sure about the word usage, aren’t these the isolated channels you are looking to compare? 1026 Line 11-12: Where is the evidence to support this statement? This is a large area, and it would not be uncommon to have variations in the sediment composition and granulometry, especially considering that this is on a fan you would expect a downfan change in the grain size, so you need to have either field evidence or a reference that will corroborate this sentence – you measured grain size and so this could possibly be

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used?

1025 Line 23: Although the reviewers' questions are legitimate they are quite far from the objective of this paper. Yet we provide references in the section presenting the Kosi fan and channel.
1026 Line 9: This is a sound suggestion also made by reviewer 1 and we changed 'seepage channel' to 'Residual channel'
1026 Line 11-12: Yes the grainsize changes on the fan surface although not by orders of magnitude. In order to waive any restriction on our analysis we have measured the grainsize at different locations so that we can estimate the grain size at each measurement site.

Section 3: Field measurements 1026 Line 17: How was the location of the grain size and slope measurements determined? Why weren't a greater number of samples created to allow for statistical analysis? 1026 Line 17: How was the sediment sample collected? Was this a single point sample from the bed? And at which position in the channel? 1027 Line 7: expand on the method used to calculate the relative error. What about measurements of suspended sediment and in particular bed load? These data are needed to answer the aims of the paper.

1026 Line 17: The only reason for the limited number of samples is the time needed for collection especially for slope samples. given the limited duration of field trips and field access difficulties we could not do more then what is presented here. Yet in the modified manuscript we show that statistically significant conclusions can be drawn
We have added more sediment samples from the Kosi fan in order to be able to calculate the mean grainsize at each measurement site. location of sand samples is now given in a kml file and a paragraph has been added.
1026 Line 17: These are the point samples of the river bed and were taken from the

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central part of the channel.
1027 Line 7: Expended the method to calculate relative error in the revised version of the manuscript.
In this study we do not account neither for bedload and suspended sediment load. We intend to do this in the future but as the comparison we perform, at the scale of individual threads, has never been performed prior to this work we believe it is usefull on its own as we argue in the text.

Section 4.1: Cross sections 1028 Line 14: You mention aerial images of the channel have you used these in this study, if so expand on this and if not I don't see how it fits into this section 1028 Line 15: consider changing "decomposition" – word usage is not appropriate, 'division' would be better 1028 Line 17: again consider word usage "decompose" 1028 Line 18: the method of calculating the bar using water depth: channel depth is very dependent on the water level / discharge at the time of the measurement – to confirm that this was appropriate it would be good to demonstrate the annual average flow for the study period to show that this was a period of high flow and that the flows were comparable over the 2 study periods. 1028 Lines 16-21: this should be included in the method rather than the results

1028 Line 14: We have rephrased this sentence according to your suggestion as we haven't used aerial images in this study.
1028 Line 15: we changed according to your suggestion.
1028 Line 17: changed as per reviewers' suggestion.
1028 Line 18: We have now written a paragraph on braided thread identification using an automated criterion based on the ratio of flow depth to maximum flow depth. Given the small number of measurements levelled in 2012 it is impossible to perform a statistical comparison of discharge between the two years. Yet as we only have 6 measurements (all of braided threads) that fall within the range of values measured in 2013 we added them to the dataset.

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1028 Lines 16-21: You are right and we changed accordingly in the modified method section.

Section 4.2: Regime relations for the Kosi fan threads 1028 Line 23: again consider the word usage “decomposed” 1029: I do not see the relevance of this section. It is quite long and contains lots of elements that could be summarised in the methods and doesn’t actually show any relationships. It could be condensed to only the salient points

1028 Line 23: The threshold theory now appears in the discussion section that has been greatly expended to account for most of your comments and those of other reviewers.

Section 5: Conclusions The conclusions are not appropriate to the aim of the paper and are very vague. At the end of the section the authors state that they are undertaking further work and I feel that for the research to adequately answer what they are hoping to they will need to include these data. An in depth discussion of the results is needed, explaining what they show that is unique and of interest to wider fluvial research.

This point is now addressed in much more detail. we hope the objectives of this paper and our analysis will now become apparent and legitimate to the reviewer.

Tables It would be better to label each of the sites with a unique identifier rather than relying solely on the coordinates – this makes it very difficult to compare which sites from Tables A1/A2 are associated with Table A3 or shown in Figures 3 and 4.

According to this suggestion, we added an id to label measurement tables A1, A2 and

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A3. Then we positioned the cross-sections of figures 3 and 4 on figure 1.

Figures Figure 1: An overview map of the area would be useful – most of the readers would not be familiar with the places named on the main map and so geographical context would better able readers to locate the field site Figure 2: Why are the long term trends shown for 2005-2007? Is it possible to show the annual discharge data for the study period as well? Figures 3 and 4: Would be easier to interpret if the sites on the figures were labelled rather than the coordinates. Figure 5: There are not enough measured points for the slope for any analysis to be done on it and so it would be better to remove the lines associated with this Figure 6: Same point as for Figure 5.

Thanks to the reviewers' suggestions we have modified the figures accordingly:
Figure 1: we have added an overview map on figure 1,
Figure 2: we are showing trends of discharge for year 2011,2012 & 2013.
Figures 3 and 4: we have labelled it on figure 1.
Although Figure 5 & 6: Still this is important to show.

Interactive comment on Earth Surf. Dynam. Discuss., 2, 1023, 2014.