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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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Comments from the referee in black text
Author response in blue text

General comments:

This paper shows a valuable set of measurements that is an important addition to remotely sensed data, which is often used to study large rivers. This data is valuable for determining river dynamics and evolution.

In this paper, the data is used to test theoretical predictions of channel properties. I have several concerns about the validity of the methods and the choose of location for

the analysis, which I address below. One of the main things, and that is something the authors point out themselves, is that sediment load has been ignored, yet very important.

Similarly, the paper addresses a very theoretical issue and the conclusions are very abstract. In the introduction, the authors mention some of the important topics river research. In order for this paper to be relevant in the field of fluvial geomorphology, the authors need to address the geomorphological problems more specifically and provide tangible conclusions. This would require some 'translation' of the theoretical results to the broader field. I believe this is required for considering publication in Earth Surface Dynamics. In addition, the structure of the paper needs improvement, detailed comments on the structure is addressed below.

Perhaps the authors should consider to combine their measurement with their ongoing campaign that does involve sediment transport measurements, as mentioned at the end of the paper. A paper based on these combined measurements could clear some of the main issues and provide a better link to fluvial processes and the resulting geomorphological evolution.

It might be best that the authors submit a "new" paper instead of revising this paper, given the concerns that may require some major rewriting of pieces.

This comment of the reviewer shows that there is a misunderstanding that we need to waive. 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 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. Let us recall that sedimentologists and geomorphologist rely on this assumption to compare processes occurring in braided and meandering rivers. This is what we do in the first part of our analysis of the dataset we produce. The potential influence of sediment fluxes and vegetation, be there some, is embedded in the residuals of the correlations we show. Let us then recall that regime equations are of very wide use in fluvial geomorphology to compare braided and meandering rivers, understand the reasons behind these patterns, and assess discharge relationships from remote sensing. An understanding of scale integration in a braided stream would therefore be very useful too.

At present we do not have access to sediment transport data especially bedload and will not have until a sound technique has been developed for complex rivers such as the Kosi. Yet the data we gathered offers, for the first time, the possibility to compare the section morphology of individual meandering and braided threads. Our sole purpose here is therefore to compare the morphology of threads for single thread (e.g. meandering) and multiple threads (e.g. braided) streams as this has never been done before.

One main concept used in this paper is the threshold channel theory, which predicts the geometry of channels with no sediment supply. As you mention yourself, (page 1030, line 5) this theory does not hold for this case since there is a large amount of sediment influx. Subsequently, you simplify the threshold-equations for W,H and S and fit this to the actual data. The whole section on threshold channels seems redundant, as you prove yourself. Why don't you skip this section and start right away with dimensionless W, H and S? This is where all subsequent analyses are based on? Furthermore, what is the difference between your W*, H* and S*, and the dimensionless parameters of, for example, Gary Parker? Please prove some insight in why you need new equations and why previous dimensionless parameters are not useful here. So the threshold theory

does not consider sediment load, but the 'fitted' equations doesn't do this either?

We use the threshold theory for discussion purposes not to prove that it predicts the morphology of threads. Let us recall that threshold theory is what is behind Lacey's law. Up to now this theory, that predicts the form of a threshold channel, is the only theory where an unambiguous and physically demonstrated analysis explains the observations made everywhere in the world that, to leading order, the width of a single thread channel stream scales with the square root of discharge. Many other more complex descriptions exist that try to include and parametrize the influence of other parameters such as vegetation and sediment transport. Yet they are all empirical to a certain degree and remain to be demonstrated. This is why we did not try to include any a prior knowledge on the effect of sediment transport on the morphology of the channels as is usually done.

The parameters mentioned by the reviewer are the same as those of Parker et al. (2007). We do not strictly use "new" equations. The equations we used are equivalent to the ones proposed by Glover and Florey (1951) and Henderson (1963). The formulation is that of Glovers and Florey with the use of the dimensionless parameters of Parker et al. (2007). We probably did not stress this enough in our argument in revised version of the manuscript we have make it more clear.

This paper is a bit top-heavy, there are quite some subjects mentioned in the introduction, there is a lot of methodology, but only limited actual results and hardly any discussion. I believe the paper would improve if many of the aspects discussed in the introduction are studied in the paper: is there an effect of vegetation, are there differences of different grain sizes, what is the added or combined value of these measurements and remotely-sensed data? Could your results add to recent advances in river remote sensing on large rivers in this region (e.g. Passalacqua et al, 2013, Marra et al, 2014). This potential set of data deserves to be studied more extensively!

We thank the reviewer for mentioning these references we did not know at the time of writing. They have been useful to help us clarify the reasons why we develop our analysis and how it links to the research mentioned. We have now improved the structure of the final manuscript.

The introduction needs to be rewritten. Parts of interesting information for in the introduction appear later in paper, in the results section (page 1028, lines 10-17). The paper misses a clear goal. I understand you seek for the similarities between channel segments of braided rivers and channels of single-threaded rivers, but it is unclear to me why this is valuable information. One thing I like is that you state that these measurements are valuable in addition to aerial images, but the paper does not elaborate on this matter. To make that claim, you need to provide detailed imagery and show the added value of your data. Furthermore, you need to explain the threshold channels theory in the introduction, this is now explained in the results section after the actual results (e.g. page 1030, line 5-8). And explain why this is important to use

Again, thanks to the reviewer, his comment shows that we need to be clearer about our objectives. let us rewrite that our sole purpose here is to compare the morphology of threads of meandering and braided streams. This kind of comparison has never been done before, and yet it is needed to fully understand the hydraulic geometry of braided threads. Furthermore sedimentologists and geomorphologist rely on this hypothesis to compare processes occurring in braided and meandering rivers. We will stress this point in the introduction.

This paper needs to be restructured. Parts that are relevant for the introduction appear in the results section. Most of the results section contains methods. There is not really a discussion; the only real discussion is in the conclusions. And some results are mentioned for the first time in the conclusions. I have some concerns about the relevance of the data from the residual channel that the authors refer to as 'seepage

channels'. If these are residual channels, their shape and slope are probably dictated by the previous channel when it was still connected to the source of water and had a larger discharge and stronger seasonal variations. After it became abandoned, there was still water in there but I doubt if this has a discharge that has altered the channel such that the channel geometry and slope are related to the current flow regime. The authors should show evidence that this is not the case or address this concern and analyses what the effect is on their analyses.

The point raised by reviewer 1 (also by Reviewer 3) is important and we have addressed it more carefully in the corrected version of the manuscript. We show that the residual channels exhibit morphology that attest to a clear adaptation of the channel to the altered flow conditions namely meandering loops and a high sinuosity that can not be observed on the braid plain.

Words like threads and channels seem to be used interchangeable. I suggest the authors check for consistency and provide a sentence of their definition in the introduction. I would stick to: braided channel and single-threaded channel. Avoid using 'braided thread'. If you want to be strict, you should use braided river (not channel) as such river is made up of individual channels. Anyway, be consistent.

We agree about this issue of consistency and will give a clear definition of the terms. This has been discussed this at length elsewhere Métivier and Barrier (2012) and we will strive to use a coherent terminology.

I would change 'seepage channel' to 'residual channel', which is the proper geomorphological term (but do mention they are fed by groundwater).

We will use the term residual channel as suggested by the reviewer.

Miscellaneous comments:

The abstract misses a description of the results. In the abstract, you write what is measured, but not, for example, the main results shown in figures 3-5.

We have done accordingly

In the introduction, please elaborate on the governing processes of channel pattern. What are the effects of sediment and vegetation? Describe things as cohesion and describe the relation of stream-power on channel pattern (e.g. Van den Berg, 1995).

We have rewritten the introduction to state our objectives more clearly. We do not study the channel pattern here but the morphology of threads. The potential influence of vegetation and cohesion is shortly addressed in the discussion section.

1024, 1-5. Write all methods in past tense (or be consistent).

We try to keep to the present tense

1024, 5. 'both' is confusing next to 'channels', rephrase.

Done

1024, 7. Remove 'also'.

Done

1024, 9. 'threshold channel theory' requires more elaboration here.

We have clarified the reasons to use the threshold theory that appears now in the discussion section.

1024, 16. I don't think a river 'selects' a pattern, the pattern is a result of the fluid mechanics.

We rephrased this sentence

1024, 22. A reference to Leopold et al. 1957 would suffice here.

Done

1024, 17. There governing parameters are not 'Possible'.

Done

1024, 21. 'multiple-threads channel', multi-threaded channel, or braided channel.

Done (Changed to 'braided river')

1024, 24-25. 'within the same channel' is confusing, could be deleted. And are you sure all these references are relevant and needed?

Done, we have removed 'within the same channel' from the sentence. We think all these references are relevant, as these are the only few works available in the literature which discuss the issue of braided and single channels morphology.

1025, 1. You state that experiments agree with this observation. But only one reference

is given. There are many more experiments on braided rivers (e.g. Van Dijk, Vande Lageweg, Kleinhans, Tal, Bertoldi). Do these experiments show similar results?

We added Hundey and Ashmore 2009 and Tal paper as a reference. Van Dijk, Vande Lageweg, Kleinhans, Bertoldi reference are not relevant here because they do not study braided rivers at the thread level.

1025, 2. 'is collection' → 'is a collection'.

Done

1025, 12. You state that the fan consists of homogeneous material. This is a very important assumption and I doubt this is true. Do you have measurements or references that support this claim? Actually, later (line 22-23), you show that there is a strong gradient in grain sizes which will have an effect when comparing different reaches of the channel. Please show that this is not an issue, or account for grain size differences in your analysis.

We now provide grainsize measurements to address this discussion.

1025, 17. Large is better. You indeed measure a lot, but it requires much more effort to measure. Earlier you cite and base your research aims on flume experiments, which are very small (yet very useful). Please elaborate and specify why large is useful here. Keep in mind that large rivers tend to show different behavior (e.g. Ashworth and Lewin2012 and the work of Latrubesse), this claim is somewhat controversial, but you could contribute to this discussion.

We do not clearly see why fluid mechanics of large rivers would be different from that of "normal" ones. The Kosi fan is interesting because of its setting as it provides the

possibility to study different thread types under the same climate and environment, not because it is large.

1026, 3. Can you shortly explain from the literature why the river changes to meandering here? What factors are important here?

There is no explanation for this change at present. We mention this in the corrected version

1026, 6. Can you put the channels in the earlier mentioned diagram of van den Berg, to show if you can explain why some are straight and some are braiding?

The empirical graphs of Van den Berg are based on total width of braided streams and do not "explain" changes in planform. We do not see the added value of such a plot.

1026, 11. I think it is the other way around: the fan is mad by fluvial deposits. The composition of the fan is the result of the river.

You are right. This sentence was a little confusing and meant to say that the sediments composing the bed of both channel types were identical especially in size. In the updated manuscript we have changed this accordingly.

For the entire methods section, please use past tense of things done in the past.

Methods summerize the work done in the field. As we are not native english speaker we follow recommendations from "The Elements of style", and try to keep to present and perfect tenses.

1026, 19. I think this device measures velocity not discharge. You calculated the

discharge.

We rephrased the paragraph on ADCP accordingly.

1025, 24. Sentence subject: The device does not compute this, you did. (I assume).

Yes we rephrased the sentence

1026, I have a feeling you cite literature here that uses the same equipment as you. But this is not relevant here. Please only cite technical documents or studies that digs into stuff like calibration, processing techniques, or comparison with other data and not studies that just happen to use this equipment.

we think all these references are relevant here as they discuss the processing technique and other important technical specifications.

1027, 10. Methods here are presented chronologically (we did a, then we did b, etc,). It should be clear to the reader how your measurements relate to the research questions and then show what you measured and shortly how. Also, the analyses are missing from the methods. I suggest you restructure the methods and add the methodology that is now part of the results section.

These sections have been significantly modified to fit all reviewers' recommendations.

1027, 16. Where does this 11% come from?

This is the maximum deviation of measured velocities from their mean value.

1027, 26. Figure 3 does not show aspect ratio.

You are right and we have changed the references in the corresponding sentences

1027, 28. You state the maximum flow is in the center of the channel. Figure 3a and c show a different pattern.

Yes the sentence has been modified accordingly

1028, 5-6. This a bit of an awkward description of the well-known morphology of a braided river. Rephrase.

This paragraph has been rewritten.

1028, 11. Wetted area depends on river stage / water height (which is a result of discharge).

Yes we added a quote to the influence of discharge on stage.

1028, 12. This may be relevant to explain in the introduction.

This sentence has been moved towards the discussion section

1028, 16. Objectives should go in the introduction. You can shortly remind the reader of your objectives at specific places in a paper to boost the readability, but I don't think that that is necessary in a paper of this length.

We have modified the methods section according to the comments made before.

1028, 18. You manually detect bars (which would go to methods). But, you could

also do this analysis automatically and explore the effect of different choices in bar definition.

We have automated the technique to detect bars and channel, now we used a criteria of 15% of maximum depth as a threshold.

1028, 23-27. This should be in methods.

Done

1029, 20. Where is the chezy factor based on? The grain size, derived from flow measurements?

Following Glover and Florey (1951), the Chezy friction factor is assumed to be a constant here.

1029, 6-end. This should be in methods. Also, as you prove later, this method is not valid for this case (see my main comments above).

We have clearly separated results and discussions section and included a paragraph on the threshold theory in the discussion section. As we quoted earlier the threshold theory is not meant to explain the data, although it does to some point, but to be used for scaling purposes and detrending of width and depth.

1029, 23. Should this be median grain size?

Yes this is the median grain size.

1030, 2. Factor 2: you could add these factors in the figure to guide the reader.

This has changed, since we are using the new depth threshold (15%) value.

1030, 12. You state that the relation accord with the observations. Yes, but isn't this the result of fitting the curve to the data?

This is not the result of fitting the curve to the data. Here we just show that despite the considerable scatter in the datasets, the trend of width, depth and slope scaling of the Kosi fan threads are coherent with the predictions of the threshold theory.

1031, 1-9. Please explain better what this means.

This statistical analysis is used to check wether detrended width and depth still exhibit a dependancy on discharge. We have rewritten the analysis and a table is now available to summerize statistical tests performed in the result section.

1031, 19. I don't think you can conclude statistically equivalence based on an overlap in mean +/- standard deviations. It seems they are, but there are proper tests for that.

You are absolutely right and we performed these tests and others.

1031, 24-27. This is very interesting. As you say, there is little data on this. But apart from that, are there mechanisms that may explain this difference?

We have very few measurements of slope values, therefore at this stage it will be difficult to give any explanation even for mechanisms. We are also not certain how this plot will look like with large number of slope measurements for braided and single-thread channels.

1032. The conclusions contain new results and a lot of discussion.

You are right, we now have a true discussion section.

1032, 15-16. You say the braided channels have a higher aspect ratio. And you mention that this data has very large scatter. In section 4.4 you reject similar differences with less scatter (I think).

A thorough statistical analysis is now available.

1032, 19-25. You make a very bold conclusion that the (very small and based on scattery data) difference in aspect ratio is caused by different sediment loads. Since you do not consider sediment load in your equations, this idea (or as you put it: 'guess') should be in the conclusion.

We discuss this point un more detail in the new discussion section.

Figure 1. Make this bigger and make clear which locations are part of the next figures (e.g. number the locations?)

This is now done in figure 1, the size of which has been enlarged

Figure 3. State in the caption what type of data this is (ADCP I guess, and what for bed measurement?)

You are right. Figure 3 has been modified accordingly.

Figures 3 and 4. Consider plotting the channel sections at the same scale and with the same colorbar so the differences between them are more clear. You can combine figure 3 and 4.

This is a sound suggestion and we have normalized the velocity scales for each figure. Yet, and for the sake of clarity, we still separate figures 3 and 4 because of the significantly different flow depth scales.

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