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Earth Surface Dynamics Discussions
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Interactive comment on "Late Holocene channel pattern change from laterally stable to meandering caused by climate and land use changes" *by* Jasper H. J. Candel et al.

Anonymous Referee #1

Received and published: 11 June 2018

I read a manuscript in a very well defined shape. The language, organisation, amount of references and overall quality is high and if the figures can be polished/optimised, the technical part will be of very good quality. I consider the topic of the study timely, relevant and well placed in the scope of the journal. The methods appear to be mostly adequate and thoroughly described. I especially welcome the general attempt to account for the uncertainty inherent to several of the inferred parameters, although there are further uncertainties that should be added to reach a more comprehensive capture of the total model uncertainty.

Technically, I find the use of dotted and dashed lines in many of the figures disturbing.

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They make it sometimes very difficult to actually see the data that is to be visualised. For example in fig. 10C the dashed lines obscure the course of the data almost completely. Please think of reworking most of the dotted and dashed lines. In many cases they are not needed to make a distinction in the plots.

I strongly encourage the author(s) to provide along with the study also the code and data they used to generate their results. This would make it possible to reproduce their work and also increase the impact of the study. I have not doubt about the validity, rigour and correctness of the material but without seeing it I can hardly judge these points. Beyond that, readers of the paper will be happy to already have a starting point to proceed with if the code and data were presented along with the article.

I had the impression that there are some sections that are too inflated with information and detail, much more than what is actually needed to support the statements they are about to make. For example, the study area section, especially the restoration part, is interesting to read but very detailed, as well. Please consider restricting the content to what you essentially need to support your methods and the subsequent discussion/interpretations.

Likewise, there are results reported in great detail that are not used to a reasonable extent, any more. The classic example for this is section 4.1. Such details may become part of supplementary materials but unless you need this for the discussion, it is not needed in the scope of the manuscript.

The abstract is mostly clear and gives a good overview of the topic and the main findings and their interpretations. It should however shed some more light onto the most detailed part of the study: the development and application of the calculus to describe hydrologic parameters and channel metrics.

The introduction is well organised and follows a consistent flow of context. The references might imply that it is almost exclusively Dutch scientists that have worked on that topic. If that is the case, fine. If not, it might be good to also present adequate

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references from other regions. But this is just a suggestion that may help improving the manuscript.

The scope of the study as expressed at the end of the introduction is not a good match with what I read later on. The actual study goes way beyond the short summary of "detecting channel pattern change" and "identifying causes". Please give more details about the approach, as well. The field and especially the numeric work is a considerable and innovative part of your work and should be reflected by the scope definition.

Concerning the second part of the scope ("identifying causes"), this part is not ideally resolved, neither by your data nor by the discussion. In the latter part, you mainly cite other people's work and make a proposition that the Little Ice Age meteorological conditions and/or land use changes have had an influence on the observed/modelled results. But you do not and cannot easily go beyond this general statement. So maybe this part of the scope should not be a central goal?

The study area description is fine, though in parts a bit too long. Please see detailed comments.

The field methods description is in most cases conclusive and well understandable. See detailed comments below for some adjustments.

The calculus description is less consistent. I acknowledge the idea of accounting for parameter uncertainty. But this must be done comprehensively and with justification. For several parameters there are either no uncertainties given or they appear out of the blue. See details below.

The order of the equations does not match the order in which the text refers to them. So either re-order the equations or tweak the text to match the equations.

The Chezy coefficient was assumed/estimated by several approaches. This is fine but in the discussion the average of all these approaches was used as the most likely value. I do not see a justification for this attempt. Are all these approaches equally likely or

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equally valid? If not, how and why was the final average coefficient value estimated?

Overall, sections 3.7 – 3.11 introduce a large set of assumptions and equations/models. These are not well reflected in the introduction and scope of the manuscript. So, do you really need all these models to make your points and interpretations, or the other way around, are your research questions adequately addressed in the beginning to prompt such a large set of concepts and models?

The set of parameter values were sampled and computed 200 times in a Monte Carlo approach. Are you sure that 200 MCMC runs are enough to cover the effects of variability adequately enough? From my experience with models that contain way less parameters I always needed much longer Markov chains to reach stable uncertainty estimates. Can you show that 200 is ok? Or have a test of convergence with number of model runs?

The results are mostly well presented. However, section 4.1 gives a very detailed picture of the lithology that is not used later to an extent that would justify this detail. I suggest to move this section to the supplementary materials to keep the story of the manuscript tight enough to be followed easily. Alternatively, make better use of the details in the discussion.

The discussion sections should be reorganised to be more logical. I suggest to focus on time and not necessarily flow of context. You can/should start with the “laterally stable phase”, then “channel pattern change”, then “meandering phase”, then “channel pattern reorganisation”. This would keep the chain of information much more concise. You can implement sections like 5.2 into this system. I would also suggest to shorten section 5.5 considerably and have it as a conclusion theme. See details below.

Sections 5.2.1 and 5.2.2 are very detailed but mainly bring together findings from other studies, focusing on potential impacts of climate change and land use change. Please shorten and condense it to what you actually need to support your findings. It would be much more appropriate to have these two sections organised together with section 5.2

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(causes of channel pattern change) but also to make more links to your actual results.

Actually, it is not really possible to disentangle the effects of “Little Ice Age weather” and “land use change” from your data situation. It can be either or both that may have drive your system of channel pattern change. Please mention this issue. It is no problem to have the effect of both.

P 1, l 13-14, “changes in climate or land cover”. There are certainly more that just these two drivers that can lead to changes in a regime. Consider changing to “ changes in, for example, climate or land cover”.

P1, l 17, “proven” is not a good term in the scientific approach. Consider replacing by “constrained”.

P2, l 20, consider changing “are documented of channel pattern changes” to “of channel pattern changes are documented”.

P6, l 6-7, hard to understand the value assignments. Consider rewriting to “with an average annular discharge Qm of 22.8 m^3/s and a mean annual flood discharge Qmaf or 160 m^3/s”.

P6, l 15-22, too detailed. Consider shortening significantly to an extent that matches the scope of the study.

P6, l 31-35. Actually all you can say is that the cutoff happened before 1720 AD. There is no information that supports the statements like “shortly before” or “date from the same period”. Consider rewriting to stay with the available constraints.

P7, l 23, check overall the journal’s definition of figure reference format rules, i.e., if “(Fig. 1(c)-(d))” is the right way.

P8, l 6. The use of “respectively” makes it very hard in this sentence to identify the cases in which you used which device. Please rewrite like “In case we we used this device. In case B we use that device”.

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P8, l 12, did I read this correctly, that you sieved material from a 3 cm wide auger/corer to estimate the gravel content? Is this a representative sample size, or in other words, over which depth interval did you have to average to get sufficient material for sieving?

P8, l 28, Add manufacturer info to grain size device (Beckman Coulter, Malvern, Horiba, etc.) to make clear which device you used.

P8, l 29. Check units. Is it 2000 m or μm?

P8, l 30. Why was the Fraunhofer model valid? Was it "just" sandy material with minimum clay content? If not, the Mie model might be more appropriate.

P8, l 33-34. Consider rewriting to simplify. E.g., "We used the scroll bar OSL burial ages determined by Quik and Wallinga. For details on the method see this reference."

P9, l 16, what is the consequence of the different age determination procedure for the palaeo channels? Are the Baysian constrained ages comparable to unconstrained ages? Are just the errors larger?

P9, l 18, how were the radio carbon samples taken? From a corer or a pit?

P9, l 21, add HCl concentration

P9, l 31, Why did you assume a standard deviation of 5 %? Why this value? Does this come from the uncertainty arising from the GPR results? It should at least be justified somehow. Otherwise I could ask, why was it not assumed to be 0.5 % or 50 %?

P10, l 9, same as above, why the 5 %? Can you say something beyond "expert judgement"? It would considerably improve the impact and value of the study and since there are quite large uncertainty ranges in some of your results these input uncertainties may be crucial to evaluate the results. You can for example also think of sensitivity analysis. What would happen if you set the standard deviation to 1 % or 15 %?

P14, l 13. Is there any uncertainty available for the porosity value? Can you estimate a plausible value?

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P14, l 14. Is there any uncertainty available for the age differences? Yes there is. So this should be included in the MCMC approach.

P14, l 24-25, the sentence does not fit very well, here. Consider shifting it to a more appropriate place where it does not cause a break in context.

P15, l 6, Is there any uncertainty available for n? Can you justify why you chose 0.028 for this parameter?

P15, l 12, Which type of rivers were these 79? sand bed? low land? Some detail is needed to understand the validity of averaging over this number of rivers.

P15, l 15, Who was the expert that suggested the value of the Chezy coefficient?

P15, l 24-25, give uncertainty estimate for intermittency and porosity parameters. Or say there is no uncertainty.

P15, l 27, consider new paragraph between “available” and “In the second”.

P16, l 8-14. This is vital information about the stability diagram. Please deliver this earlier to the reader, e.g., when you first mention this diagram type. What is meant by “interpreted as a lower threshold, rather than a hard threshold”?

P21, l 12, define or quantify the term “very similar”, you have the data to do so.

P21, l 17, define or quantify the term “extremely slow”, you have the data to do so. Also, you can make use of the uncertainty information.

P22, l 8, provide uncertainty information for slope of X.

P22, l 11, provide uncertainty information for slope of Q.

P22, l 11, what mean “relatively linear”? You should test and quantify. Actually I could also interpret a piecewise linear model with a break around 1850.

P25, l 8-10, why did you choose the “middle of the ranges” and what is the “middle”? See above, why should the full range of estimated values for C be equally valid or

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likely? If they were equal, why would you make a distinction between “all rivers”, “rivers without bars” and so on? Why did you use a standard deviation of 2 units? Please justify these apparently arbitrary assumptions. It is fine to include uncertainties, but their foundations must be reasonable.

P25, l 12, the values 32 and 38 are really really hard to map out on figure 9 a. And anyway does not everything in this figure drown in the uncertainty polygon? Please discuss your values with respect to the large uncertainty range.

P26, l 7, “was probably limited”... not necessarily. You simply cannot resolve this statement with your data. Just that the phenomenon could be explained with option A (sediment transport is higher than bar growth) does not mean that option B (external sediment input) is not also contributing. Or would these two options be mutually exclusive?

P28, l 7-14, this part contains very limited information but instead many repetitions of already discussed material.

P28, l 19-28, there is a lot of general information and unknown statements in this section. Please make a better connection to the results section. You have a lot of quantitative results, so please use them to support the statements made, here.

P30, l 31-32, if this is no tin the scope of the paper, then why referring to this topic?

P31, l 11-12, I do not think it is actually possible to resolve whether or not increased sediment input played a role, so I would not mention this, here. See comment somewhere above.

P31, l 14-33, very broad and general. The main point I read from this paragraph is that we need more detailed field studies to pursue the question. Try to make more out of this material. It would be a valid goal to investigate if the one case you found in your study is an “outlier” or the “regular case”. Anyhow, the paragraph in its current shape does not present/discuss your results. You have to make a story out of it or leave it.

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Likewise, the second paragraph comes a bit out of the blue. How does the Geul river come into play and why does it come into play, here? This section needs more context or should be skipped. Currently, it does not really match the section header.

P32, l 5-15, this part is also very broad/general and arm waving. Consider shortening significantly and link it much better to your concrete findings, i.e., what your case study can contribute to this overall picture. Overall, I suggest to shorten this part and have it rather a conclusion item than part of the discussion.

P32, l 29, change “discharge increased” to “discharge potential increased”.

P32, l 30, change “exploitation has contributed” to “exploitation has probably contributed”.

Table 1, It would be better to have the radio carbon and OSL ages at the same scale. This concerns both, years versus kilo years and AD years versus absolute years. At the moment things are hard to bring together.

Figure 1, replace dashed lines in panel b and f. Also, consider using solid lines to illustrate the zoom from panel to panel. Add similar “zoom lines” also from b to c and b to d. Provide a solid or at least partly transparent background to legends. The legend contents are really hard to see. Add legend frame in panel b.

Figure 2, image quality is not good. Either this is due to the manuscript stage compression or other. It would be essential to add a higher resolved image of the GPR output. Also, the thick yellow lines are masking the raw data too much. Figure 5 does a much better job by showing both, raw and interpreted options. Alternatively, think of using thinner and semi-transparent lines. In figure caption there is repetition with “modified after Huisink” and “adapted after Huisink”.

Figure 4, Please decrease the size of the drawings and have all of them on one page. The context density of the drawings is not too high, you can scale them smaller without loosing much of the content. Of course the axes labels and plot drawing texts must be

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rescaled to an appropriate font size. But currently, there is a mismatch in the size of the figures with respect to what they tell.

Figure 7, what do the errors want to tell in panels c and d? Overall, the resolution of the images are not really great Consider saving such plots are EPS vector data.

Figure 8, figure quality/resolution is bad. Please avoid the dashed and dotted lines (e.g., panel g), they make it hard to see the data clean. Shift legend from panel a to panel c and d.

Figure 10, dashed lines make it hard to see any trends

Figure 11, why is Prathoek missing in above panel?

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