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.

Many thanks for your kind words and enthusiasm about the manuscript, and your critical review, adding very valuable suggestions. We will re-evaluate the chosen uncertainty for the parameters and add additional uncertainties where possible. Please find these details further below.

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

We agree and changed figures according to suggestions.

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.

We will add a sheet with all the calculations that have been done, so that the reader can start very fast from there with their own calculations. We will also include our own used data in the spreadsheet as an example and a verification.

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.

We removed the section on restoration from the second chapter, also suggested by the other reviewer. Lithogenetic interpretation was moved into a table (as suggested by the other reviewer), including most important details. The manuscript has been shortened by ca. 3500 words, removing all repetitions, abundant results and discussions, in agreement with the three reviews.

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.

We added more detail on the reconstruction of the hydrological parameters and rephrased the potential causes part of the little ice age and peat reclamation.

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

There are indeed some examples of Dutch cases (De Moor, Vandenberghe, Kasse, Hobo), but also many examples of non-dutch case studies in the 3th and 4th paragraph (Lewin, Slowik, Lespez, Notebaert and Verstraeten, Hoffmann, Kondolf, Piegay, etc.)

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.

We sharpened the aim and focused more on the methodology of the palaeohydrological reconstruction in the introduction. We removed the aim of identifying the causes, but will only shortly elaborate on the potential causes in the discussion. So we put the focus more on the reconstruction than on the identification of the causes.

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?

We agree and changed the scope, also in line with the other reviewers. We now focus more on the identification of the channel pattern change and methodology. See previous comment

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

We removed the section on river restoration from the study area description.

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

Thanks

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.

We changed this and gave a better reasoning for each parameter on its uncertainty in the method section.

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.

Checked and changed

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

We agree and we changed the approach. In fact, Brownlie uses variables that are known, and of which we can vary the uncertainty. However, Manning is a subjective estimation of what the river looked like in the past. We changed the approach and only use the Brownlie, and we will compare the calculated Chézy value with values known from rivers of similar size and with similar river pattern.

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?

We understand the confusion, thanks. We changed the research questions accordingly. In fact, after we have identified the channel pattern change, we identify which parameters have changed, and we used the empirical models to test whether they can explain the channel pattern change.

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?

We checked this, and raised the computed runs to 10.000 times.

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.

Lithogenetic interpretation was moved into a table (as suggested by the other reviewer), including most important details. This section is important, because it is the fundament of the palaeohydrological reconstruction in which the palaeodimensions are derived from the cross-sections. Hence, the interpretation of the lithogenetic units is an important element in the manuscript

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.

We agree and followed the suggestions. We reorganised the discussion according to the suggestions. We included the " channel pattern reorganisation" into the meandering phase. We removed section 5.5.

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 (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.

Agree, and merged this part with section 5.2. In addition, we shortened this part. See previous comment

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".

We followed the suggestion by Referee#3, adding and/or.

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

Agree and changed

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

Agree and changed

P6, l 6-7, hard to understand the value assignments. Consider rewriting to "with an average annular discharge Qm of 22.8 m3 /s and a mean annual flood discharge Qmaf or 160 m3 /s".

Agree and changed

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

Agree and shortened

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.

Agree, in this phase of the manuscript we can only take conclusions from its dimensions, but indeed not from the age. The study is needed to investigate this. We rewrote this section.

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.

Checked and changed

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".

Agree and changed

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?

The sieving was meant to make an estimation for the gravel content in the lithological description. Purely meant as a field-based method to make a fast estimation, sufficient for the aim of this method: distinguishing the lithogenetic units

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

Changed

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

Checked, 2 mm is correct

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.

Yes, almost all sand, see Fig. 6. We changed the text slightly to make this more clear

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."

Changed according to suggestions

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?

Details for the age estimation and effects of Bayesian constraining are provided in Quik and Wallinga (https://doi.org/10.5194/esurf-2018-30). Some of the younger deposits are particularly poorly-bleached, and for those the cartographic evidence is leading and provided more accurate and precise ages. The Bayesian procedure overall resulted in smaller uncertainties..

We removed this sentence "apart from the final Bayesian.... from historical maps", because we already mention that only the laboratory analysis followed the same procedure. Their Bayesian analysis was a post-laboratory calculation. We added a short explanation why we did not use Bayesian analysis.

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

Changed, we used a piston corer, forgot to mention.

P9, l 21, add HCl concentration

Changed

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 %?

We reviewed this assumption. We introduced a standard deviation based on different assumptions for the channel dimensions (see comment to other reviewers), by 1) introducing two knickpoints and 2) determining it for both palaeochannels. Then we calculated the average and standard deviation of Hbf. Consequently, this approach also affects the other channel determined dimensions (A, P, R, W).

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 %?

We agree and changed the assumption. We used the differences in surface and bottom elevation as a measure for the uncertainty of H_{bf}. See previous comment.

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

We included an uncertainty range for the porosity of sand based on literature

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

There indeed is an uncertainty in the ages as shown by Quik & Wallinga (submitted), but the order of development of the scrolls in the scroll-bar sequence is known. Here we are primarily interested in the trend of the palaeodischarge over time, in particular comparison of the palaeohydrological conditions that existed at the start of the meandering phase.

The age estimate uncertainties are relevant for comparing the reconstruction to possible drivers, and are considered in our (condensed) discussion of these. Including these uncertainties in the figures showing trends in time is extremely complex, and if possible, would resulting in a blurred picture masking trends over time. Therefore we choose not to include the uncertainty of the ages.

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.

We moved the sentence a few sentences down.

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

We found that taking a range of the Manning coefficient, the uncertainty becomes so high that it's rather useless. In fact, estimating the Manning coefficient was a matter of estimating what the river could have looked like (vegetation, irregularity) and comparing it with similar rivers, but this information is unknown. Based on reviewer comments, we have decided to only use the Brownlie formula, because it includes known variables and their uncertainty. We now compare the calculated Chézy with literature values.

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.

We moved this comparison to the discussion and added more detail on the rivers for which we averaged the Chézy value.

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

Sentence was removed, because no details were given on how they estimated the Chézy.

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

We added the uncertainty for the porosity based on Nimmo's work. For the intermittency there is no uncertainty.

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

Agree and changed

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"?

We agree that the stability diagram is an essential part of the reconstruction. However, we refer in the introduction to the use of empirical channel models, which we further elaborate in the methodology section (here). We decided not to move this section to the introduction as it would make the introduction too long and unbalanced.

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

Changed

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.

Changed

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

Changed

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

Changed

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.

We removed the comment

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 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.

We removed this comment as Chezy is now based on the Brownlie equation, we don't use the other approaches anymore. See previous comments.

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.

We added some discussion on the uncertainty in the graphs in section 4.4. An equal bankfull discharge would mean a large change of the parameters for Palaeochannel X&Q, which are unrealistic.

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?

Removed this statement

P29, l 7-14, this part contains very limited information but instead many repetitions of already discussed material.

We rewrote this section. The repetition is caused because most of the discussion was already written in the results, therefore we moved it from the results into this section.

P29, 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.

We agree and rewrote this section.

P31, l 31-32, if this is no tin the scope of the paper, then why referring to this topic?

Removed this phrase.

P32, 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.

Removed this line

P32, 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.

We removed it to prevent repetition, and discussed this part in the introduction.

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.

We moved this section to the introduction as suggested by the other reviewer, where it supports the likelihood that more rivers changed from laterally stable to meandering during the Holocene.

P33, 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.

We removed this section on stream restoration to shorten the length of the manuscript and to keep the focus.

P33, l 29, change "discharge increased" to "discharge potential increased".

We rewrote the conclusion

P33, l 30, change "exploitation has contributed" to "exploitation has probably contributed".

Rewrote the conclusion

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.

Final ages are all presented in the same framework; following the revised manuscript of Quik & Wallinga (in press) we adopted the CE framework for this. Intermediate results are also presented reported in the appropriate unit, for the OSL ka ages are presented in addition to CE, as these relate directly to the reported palaeodose and dose-rate.

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.

We changed the dashed lines into solid lines and also added them to panel b to c and b to d. The legend is poorly readable due to the low quality of the images. We decided not to add a background, because detail would get lost of the meander bend surroundings, also when transparent the surroundings will be hardly visible. However, the higher quality of the images improves the readability significantly.

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".

We agree that the image quality is not optimal. This is because Huisink had a low quality image in her article, which is very likely due to the quality of the GPR output 18 years ago. Here the main goal is to illustrate the different subsurface features (palaeochannel, coversand, fluvioperiglacial) and showing that a symmetrical palaeochannel is present, but lateral accretion surfaces are lacking. This interpretation was not done by ourselves, but by Huisink already. For the actual data we refer to their work. For our own data (Fig. 5) we agree that it is essential to deliver good quality GPR images.

We changed the caption so that it becomes more clear that the interpretation was done by 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 rescaled to an appropriate font size. But currently, there is a mismatch in the size of the figures with respect to what they tell.

The delineation of the scroll bar deposits and palaeochannel is an essential step in the reconstruction, and should be fully visible in Figure 4. We tried making the drawings smaller (so they would fit on one page), but too much detail was lost. We shortened the lithogenetic interpretation, therefore these figures become even more important.

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.

We improved the quality of the images. The caption explains the uncertainty shown in Fig. 7c,d. This is the standard deviation of the Bayesian deposition model determined by Quik and Wallinga.

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.

We changed the resolution of the figures. We removed fig. 8gh, because they were not that important for the reader. We keep the legend in panel a, because here there is sufficient space, and the legend immediately explains the lines in panel a.

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

Removed and replaced the dashed lines. We also added a log-plot to see more detail.

Figure 11, why is Prathoek missing in above panel?

As explained in the method section, we merged both meander bends together, because the same discharge and streampower are expected. For the IP this is different, because the IP is determined by channel-dependent parameters.

Interactive comment on Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2018-31, 2018.