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Interactive comment on "A segmentation approach for the reproducible extraction and quantification of knickpoints from river long profiles" *by* Boris Gailleton et al.

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We thank reviewer 1 (Stefan Hergarten) for his thoughtful review on our manuscript. Overall, the reviewer is not convinced by the merit of producing a full research paper out of developing a reproducible method to quantify knickpoint morphology from river long profiles. We propose clarifications on the different concerns raised and strongly believe that our manuscript would be of benefit to the readers of ESURF and the wider geomorphic community as a full research paper.

First, I am not completely convinced that the automatic detection of distinct knickpoints is still such a great step in fluvial geomorphology. Knickpoints are fundamental for

understanding the effect of sudden temporal changes or discontinuities in lithology, and they were a primary measure in morphometry at times where high-resolution DEMs were not widely available. However, one may question whether finding such distinct points automatically has really a greater potential than analyzing river profiles or even entire drainage networks as a whole. This might reduce the importance of this work a, but does of course not question the merit of this work.

We acknowledge that this manuscript describes a method and neither explores geomorphic processes nor presents a detailed case study. The reviewer points out that high-resolution DEMs are becoming more widely available, and in addition knickpoints are fundamental features to help us understand landscapes. Over the last decades dozens of papers have been published making inferences about how channels incise, and how landscapes evolve and have evolved though time on the basis of the locations of knickpoints and knickzones.

The reviewer then suggests that there is greater potential for "analyzing river profiles or even entire networks as a whole". Analyzing river profiles and networks how? Any quantitative analysis of profiles or an entire network needs a method. Choices made in creating the method have implications for the results, and therefore the interpretation of the results. So we strongly believe that these choices and their implications should be clear to workers using the method. Which is why we think methods papers are important.

We do not understand the reviewer's contention that knickpoints are "fundamental for understanding the effect of sudden temporal changes or discontinuities in lithology", but at the same time not worth finding. The numerous studies which use the spatial location, magnitude, and evolution of knickpoints suggests that understanding discontinuities in river steepness is important, and that their spatial distribution reveals important information about landscape evolution. They will exist in specific locations regardless of the resolution of the topographic data. We have tested the algorithm using lidar data and our sensitivity analysis with regards to resolution degradation suggests that knickpoints found in 1 metre resolution data can also be found in lower resolution data. That is because they are distinct topographic entities with a distinct location. So we reject the suggestion that high resolution data will make knickpoint detection obsolete. Flying lidar over a landscape doesn't make the waterfalls disappear.

Following on from this, we set out briefly here why we believe our study is useful to the community. Techniques for identifying knickpoint locations have a number of pitfalls, and choices made by authors can alter the results: for example, selection of knickpoints by humans is extremely difficult to reproduce. The main motivation for our approach is reproducibility and not pure automation, which is a more minor point of this study. Interpretation of knickpoint locations strongly depends on method choice. In this contribution we have explored the implications of these choices, and compared them with methods of other authors as well as independent field datasets. This allows future workers to make informed decisions about different knickpoint extraction methods and know the strengths and weaknesses of these methods, all of which will affect the confidence and nature of geomorphic interpretation of the results.

But as my most severe doubt, I see the new aspects presented here as a piece of a mosaic. If I understood the concept correctly, the new part is applying the TVD method from signal processing to the ksn values described in Sect. 2.3.1., while the earlier steps of the analysis are apparently based on previous work. And this key point is not explained very well. I would have expected more explanation why this is a particularly good concept in the context of river profiles going beyond the comparison of the entire procedure with other approaches.

As is the case with the development of many methodologies, ours relies on many previous studies. The most important is an algorithm from Mudd et al., 2014, JGR that proposes a statistical framework to derive χ-elevation gradient (ksn in our case) using a segmentation approach. We set out in the manuscript (i) the reasons this approach is relevant to build upon; (ii) the difficulties of objectively identifying knickpoint location only using its raw results; and therefore (iii) the motivation of our method development.

Adapting a TVD algorithm is the first step that focus the dataset on discrete significant gradient change (e.g., slope-break knickpoints). After adapting this algorithm we develop an entire process described in the manuscript to extract and transform these discrete changes in k_{sn} into dataset of objective and quantified knickpoints. Furthermore, we propose an additional new approach for vertical step knickpoints. Finally, we describe strategies to interpret and thin this dataset while keeping full reproducibility of results for two fairly different case studies, to avoid constraining the algorithm to one single case study and context. The effect of parameters and datasets (e.g., grid resolution) are explored and discussed with extended sensitivity analysis in the specific context of knickpoint extraction. We believe that all these additions make our method significantly different from the other algorithms it is based on and justify the relevance of our manuscript.

Taking into account that entire packages such as TopoToolbox are published as a short communication in Earth Surface Dynamics, the recent manuscript would not be well placed as a full research paper in my opinion. In order not to be misunderstood – this is a nice piece of work, but if we are honest, each comprehensive package such as LSDTopoTools from your group contains many important and innovative components, and it would not be realistic to derive a full research paper from each of them. My recommendation would be either focusing the manuscript on the essential new part and submit it as a (very) short communication or including the methodical aspects into a later paper where scientific results are derived using the method going beyond the test cases presented here.

Providing open-source and documented code is crucial for making our research easily reproducible, testable and improvable. However it also generates a significant risk of mis/over interpretations of its results, as the software would ultimately produce results in any context. We therefore believe that it is crucial to provide such analysis with extended discussion on its use and in comparison to other existing methods, sensitivity analysis on the different parameters, example of uses on different landscapes, and clear statements on how to constrain it. Other scientists who might come to use our methods should be aware of what the algorithms can provide and what the limitations might be. The reviewer suggests that we drop this analysis in the appendix of a case study. However, inadequate discussion of methodology along with failure to publish software is one of our major frustrations. We do not agree with the suggestion that we follow this approach. The reviewer then suggests we just publish an overview of the general software. This again we feel would be a major disservice to users of our methods since such a paper could never go into the details of each method; the development and testing of each method typically represents many months of effort, not to mention the many years of CPU time we devote to testing on multiple landscapes. We strongly feel that our approach of publishing the details of the method and our efforts to fully explore its capability are the most beneficial for the geomorphology community.

I am, e.g., not sure whether the definition of χ was indeed introduced in the conference contribution by Royden et al. (2000) more than 10 years before it became popular; at least I did not find it in the cited abstract.

This is stated in Perron and Royden (2013) on page 571, 2nd column, l.6-7: *The use of this coordinate transformation [referring to equation 6b exposing χ] to linearize river profiles was originally proposed by Royden et al. (2000) (. . .).*

As a second example, the lower sections of page 5 read as if 2014 was more recent than 2017.

We acknowledge the need of a clarification here: the algorithm has been newly developed (2018) within TopoToolBox (citable as 2014) as the author (Wolfgang Schwanghart) details in the presentation of this feature (https://topotoolbox.wordpress.com/2018/06/29/finding-knickpoints-in-river-profiles/).
Albeit unpublished (yet), we thought it was important to test our own method against others with the same goal of knickpoint extraction while taking completely different approaches. Wolfgang Schwanghart has also reviewed the paper and does not seem

to object to our reference to the new method, but we agree that we should clarify the sequence of introduction of these tools.

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