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Interactive comment on "Developing and evaluating a theory for the lateral erosion of bedrock channels for use in landscape evolution models" *by* Abigail L. Langston and Gregory E. Tucker

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Dear authors,

we have received three reviews for your paper now. Two of them were solicited (one anonymous, one by Dimitri Lague), and one unsolicited (by Aaron Bufe). All three of these appreciate the aims of the paper, but also highlight serious problems. In my mind, the most important points are these:

- Reviewer #1 points out the connection of lateral erosion to meandering, the minimum

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requirements for the modelling of which have been studied by Knutson and Howard (1984). The considerations in this paper should be discussed and incorporated in any revisions of the model.

- Reviewer #2 is concerned about grid resolution issues and the treatment of channel width. I think these points are very important and should be taken seriously. I'd like to point out here the paper of Stark and Stark (Am. J. Sci. 2001), who developed a sub-grid approach to treat channels, which may be instructive for dealing with this criticism. A related point here is the frequency of contact of the flowing water with the banks, as made by Bufe, the importance of which has been pointed out by Hancock and Anderson (GSAB 2002).

- Both reviewers #1 and #2 also pointed out some previous treatments of bank erosion/lateral mobility/meandering, for instance the above-mentioned paper by Knutson and Howard, but also the treatments within CAESAR (e.g., Coulthard and van de Wiel, ESPL 2006) and EROS (e.g., Carretier et al., ESurf 2016). A review of these treatments would be appropriate, highlighting of their different merits and why another (new) approach is necessary.

Many of these points culminate in the statement made explicitly by reviewer #1 at the end of the review. You construct some model and explore its dynamics to some extent, but the question remains as to why we should believe that it is a true or even useful description of reality. I agree here that the paper could benefit from a well-defined research hypothesis and a set of criteria that could be used to evaluate the model against field data or compare it against the performance of other available models.

All three reviews provide detailed comments and give suggestion in which direction the paper could be developed. Please treat all comments seriously and provide a detailed rebuttal. I will send out the paper for a full review again.

Best wishes and good luck with revisions,

Jens Turowski

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