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Interactive comment on "Mass balance, grade, and adjustment timescales in bedrock channels" *by* Jens Martin Turowski

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I thank the reviewer for taking to the time to read and comment on my paper. I am very grateful for the comments. This is a quick reply to the reviewer's comments on bar wavelength and its relationship to cover. I have looked again at the suggested papers and give a short summary below. Hopefully, this can instigate some further exchange before the discussion closes. In case I have overlooked anything, I am happy to receive further pointers.

The reviewer is unhappy with the relationship between cover and bar wavelength used in the model, stating that "Gravel bars do not increase their wavelength as cover increases" (comment to page 15 line 6, emphasized elsewhere). She/he cites three

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papers as support, two modelling papers by Nelson and Seminara, 2011, and by Inoue et al., 2016, as well as an experimental study by Chatanantavet and Parker, 2008.

As a reply, first it needs to be noted that a change in the bar geometry affects only the lateral erosion equation (eq. 24 in the paper). This propagates into the response time of width for a widening channel, but not the response times of cover, of bed slope or for the width for a narrowing channel. It also does not affect the steady state channel morphology presented in Fig. 4. Without having done the necessary calculations, I expect that the effects on magnitude and scaling of response times will be minor. In summary, a change in the dependence of bar wavelength on cover would not majorly change the overall arguments and conclusions of the paper.

Second, looking through the suggested papers, I cannot find material supporting the reviewer's statement. I will briefly summarize the relevant points below.

The experiments of Chatanantavet and Parker (2008) were not set up to study bar morphology. They mention that alternating bars were present in the experiments (e.g., paragraph 13), but do not give details on their morphology or how their wavelength scales with cover. As such, the paper does not contain relevant information beyond the observation that alternating bars were present.

The Nelson and Seminara (2011) paper is not concerned with bars. However, there is another paper by the same authors, Nelson and Seminara (2012), which is. Consequently, I assume that the reviewer intended to refer to this particular study.

The two mentioned modelling studies (Nelson and Seminara, 2012, and Inoue et al., 2016) use slightly different modelling approaches, but reach similar conclusions. In fact, with regards to bar wavelength, Inoue et al. (2016) explicitly summarized the results of both as follows (page 8, left-hand column, 2nd full paragraph): "Nelson and Seminara (2012) conducted a linear stability analysis of bars on the bedrock and analyzed the wavelength of infinitesimal bars. The findings of their analysis are as follows: (1) regions where alternate bars form on the bedrock are determined not only by the

width/depth ratio, but to some degree by the ratio of $\tau = \tau$ c; and (2) the wavelength of the bars increases with decreasing sediment supply rate. The analysis by Nelson and Seminara (2012), unlike the simulation of this study, did not consider localized bedrock erosion by bedload; therefore, it is not possible to compare the two simulations quantitatively. However, the two models show a similar tendency to form longer wavelength bars when the sediment supply is lower." I draw attention to the concluding sentence of the paragraph, where Inoue et al. (2016) stated that bars feature longer wavelength for lower sediment supply. The latter condition implies less cover. As such, the statement is in contradiction to the claims of the reviewer, and in qualitative agreements with my assumption.

That said, the reviewer draws attention to an unphysical behavior that I had not previously noticed: Due to the linear relationship, at low cover, bar wavelength becomes short. This is in contradiction to the analysis of Nelson and Seminara (2012), who demonstrated that short bars are unstable and cannot persist (their Fig. 3). Furthermore, it leads to bedload paths at 90° angles to the main line of the channel when predicted bar wavelength is much smaller than channel width (pointed out by the reviewer in the comment to page 19, eq. B6). As a consequence, for low sediment supply rates, either the bar wavelength needs to be fixed in accordance with the stability criteria suggested by Nelson and Seminara (2012), or bars would disappear entirely, implying that lateral erosion is not possible within the assumptions of the model.

I will add a discussion of these points to the revised paper.

With best wishes and thanks for reviewing the paper, Jens Turowski

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