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Interactive comment on "Geomorphic signatures of the transient fluvial response to tilting" *by* Helen W. Beeson and Scott W. McCoy

Anonymous Referee #2

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Beeson & McCoy present a set of numerical modeling experiments and topographic analyses of select rivers draining the western slopes of the Sierra Nevada aimed at identifying characteristic signatures of tilting in bedrock river morphology. The authors primarily use linked 1-D river profile models to demonstrate that rigid block tilting generates patterns of incision, channel steepness, and knickpoint type and distribution dissimilar from those generated by a step change in uniform rock uplift rate or rock erodibility or a major truncation of headwater drainage area. They suggest that the magnitude and time since tilting can be recovered from bedrock river morphology, including tilt magnitude in landscapes with heterogeneous lithology âĂŤ based on modeling tests with mid-channel bands of more and less erodible bedrock. The authors also use a 2-D landscape evolution model to show that channels flowing in tilt direc-

tion have elevated gradients during transient adjustment and that tilting induces stream captures in the direction of tilt. Finally, the authors analyze the bedrock river profile morphology of select rivers draining the western Sierra Nevada, a landscape thought to have experienced late Cenozoic tilting, to show that the landscape indeed displays the signatures of forward tilting identified in this study.

As the authors nicely articulate, quantitative characterizations of the transient response of bedrock rivers to tilting are lacking, even though tilting is widely documented in mountain ranges. This study therefore addresses an important outstanding question in tectonic geomorphology. Notwithstanding the many simplifications and sources of uncertainty in the modeling and topographic analyses, I find that theses limitations are reasonably explained to the reader (though I point out a few places where I think more qualification would be useful below). The study therefore provides valuable insight and advancement (even if only an important first step) towards identifying decipherable topographic signatures of tilting. The paper is well written and the authors are quite thorough in their analyses, considering various tilt orientations and magnitudes and stream power incision slope exponents in their modeling experiments - in places, making the main text a bit cumbersome (I suggest a few places where I think the main text reference to supplementary material could be shortened).

The study certainly inspires a number of follow-up questions (What about different tilt histories and/or lithologic patterns? How do thresholds for river incision or sediment flux dynamics impact the topographic signatures of tilting?), but I think this is a good thing, demonstrating the unique contribution of the work to an interesting and outstanding research question. In my view, the authors have made judicious choices in what specific questions to address and what assumptions to make in their modeling, and I don't think the authors should significantly expand the scope of the work.

That said, my main critique of the manuscript is that the 2-D landscape evolution modeling and application to the Sierra Nevada feel a bit underdeveloped relative to the 1-D river profile modeling. These are both critical litmus tests for evaluating if and when topography may record a decipherable signature of tilting. It's great that the authors included these components in the first place, but I think the manuscript would be significantly strengthened if one or both or these analyses dug a little deeper into the results and discussion of the modeling experiments and topographic analyses already performed.

In particular, for the landscape evolution modeling, I wonder about the frequency and rates of divide migration associated with both gradual divide migration and discrete capture events and the river network response times. How does discrete vs. gradual drainage reorganization impact channel morphology (and how does this relate back to the 1-D profile modeling scenarios of tilting and major truncation/beheading of mainstem rivers)? How does tributary junction angle modify the morphology of tributary knickzones or steepness patterns, and what affect does this have on river profile analyses? I also suspect that drainage reorganization in the landscape evolution model is sensitive to the model discretization and flow routing algorithm you use (as mentioned on pg. 22, line 5), and it would be nice to see some test/statement of how robust the results are to these factors. It feels a little like a missed opportunity to perform the 2-D landscape evolution modeling and not take a closer look at the diversity of river profile morphologies that develop in the evolving river network.

For the Sierra Nevada analyses, at pg. 25, Line 9, why do you chose to use a concavity theta = 0.45? Does this linearize and/or collapse mainstem and tributary rivers in the region on chi plots? How do the knickpoint travel times tau (and inferred timing of the tilting event) vary for n = 2/3 or n = 5/3? What about tilting associated with postglacial rebound? Granger & Stock (2004) measure tilt angles up to $\sim$0.08 deg in the past $\sim$15 kyr from cave deposits in the Southern Sierra Nevada near the San Joaquin and Kings Rivers. How would this secondary tilting event affect your results, considering even the case in which 90-95% of tilting occurred $\sim$5 Ma and the remaining 5-10% occurred since the LGM? Is it reasonable to analyze channels above the Pleistocene glacial limit? To what extent does this topography retain its glacial form (are valleys

v-shaped above 1000 m?) and how, even downstream of the glacial limit, could Quaternary changes in discharge and sediment supply affect river profile morphology? Are there any knickpoints you can definitively associate with tilting rather than a transient response to deglaciation or heterogenous lithology? In Figure 11, if you exclude all tributaries above the glacial limit, does your tilt magnitude estimate change? With its glacial history and lithologic variability, the Sierra Nevada seems like a pretty complex test case to evaluate the real-world applicability of fluvial metrics of tilting identified in the modeling. The relative convergence of the results and consistency with previous interpretations is compelling, so perhaps the complexity proves the robustness and widespread potential for extracting tilt histories from river profile and network morphology. However, at present, I'm not sure the authors have fully addressed these confounding factors nor sufficiently quantified the sensitivity and associated uncertainty of the Sierra Nevada results to their assumptions and interpretations.

Other comments: (1) Pg. 2, Line 30 - What about documented response to tilting associated with dynamic topography? There are a number of papers (e.g., Braun et al. 2013, Ruetenik et al. 2016) that have addressed fluvial response to long wavelength dynamic uplift and subsidence, including continental-scale tilting. It seems like this should be mentioned or at least the distinction in underlying mechanism and/or the scale of tilting should be clarified.

(2) One major assumption underlying all model tests is the assumption of uniform concavity, which could very well be violated if the mechanisms of river incision change with channel slope or in different lithologies. There are multiple places (e.g, in presenting the first results in Figure 2 with vertical knickpoints and in presenting the results in Figure 4 with heterogeneous lithology) where this assumption should be restated and its limitation clarified. It's a little hard to tell in Figure 10 since lithology is not included in (b) (I suspect since what would ideally need to be displayed along the chi axis is integrated lithology), but it appears to me, looking at the elevation ranges, that river profile concavity could vary between the different lithologies.

(3) The assumption of a negligibly small/frequently exceeded threshold for bedrock river incision should be stated and the limitations of this assumption should be addressed, particularly since I imagine tilting could have a significantly enhanced impact of bedrock river incision if it increased channel gradients enough to change the frequency and magnitude of events exceeding a threshold for bedrock river incision.

(4) Pg. 9, Lines 10-14 and Line 33; Pg. 11, Lines 19-21; Pg. 13, Lines 7-10 - It's great that the authors performed these sensitivity tests for the slope exponent. I find the references to these tests at the end of each result subsection a bit disruptive to my understanding of the main results, particularly since many results in the n=2/3 and n=5/3 cases are similar to the n=1 case, per the authors' own assessment. I'd prefer to see these all aggregated into a supplementary material section with a single main text reference.

(5) Pg. 11, Line 20 - Should be noted that such 1-D modeling neglects area-loss feedback and full 2-D/network dynamics of drainage reorganization

(6) In the 1-D profile modeling, I suspect some of your quantitative results (e.g., estimated tilt from geometry of local rock-type related knickzones) may be somewhat sensitive to your forward differencing scheme and discretization. I suggest clarifying which forward differencing scheme you use. In the same vein, as mentioned above, in the 2-D LEM, the results are likely sensitive to the flow routing algorithm, which is not specified in the text.

(7) Pg. 17, Line 30 - The heterogeneous lithology tested is quite idealized, and I wonder if this is a really a reasonable proof-of-concept test in considering if one could apply this to a real landscape. What happens if you add noise (even much lower magnitude than the heterogeneous lithology K contrast) to your spatially variable K, as you might expect if there's spatial heterogeneity in rock erodibility even within the same lithology? Is it still possible to recover tilt timing and/or magnitude? Also, many landscapes where tilting is suspected to have modified the topography are landscapes with tilted sedimentary

rock units. Tilting of such tilted strata would effectively result in spatially and temporally variable erodibility within bedrock channels, as rivers incise vertically into a tilted layer, causing the exposure of that layer to migrate up/down stream depending on the river orientation and tilt magnitude and direction. It seems like this at least warrants mention.

(8) Pg. 19, Line 5 - Specify what type of curvature

(9) Figure 10 - I don't think the rock types should be extended all the way down to the x-axis, since this seems to imply that the rock units have vertical dips extending to depth. I suggest fading away the lithology shading just below the river profile.

(10) Pg. 27, Line 5 - Would be helpful, for reproducibility, to specify the threshold you use to determine if tributary knickzones collapse with the mainstem on the chi plot or not. Here and elsewhere, ditto for how exactly you define a knickzone (e.g., what threshold change in steepness do you use to identify the top and bottom?)

References: Braun, J., Robert, X., & SimonâĂŘLabric, T. (2013). Eroding dynamic topography. Geophysical Research Letters, 40(8), 1494-1499.

Granger, D. E., & Stock, G. M. (2004). Using cave deposits as geologic tiltmeters: Application to postglacial rebound of the Sierra Nevada, California. Geophysical Research Letters, 31(22).

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