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

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The manuscript by Beeson and McCoy explores the role of tilting in affecting bedrock channel profiles, a worthwhile topic, via numerical modeling and GIS analysis. Unfortunately, the main conclusions concerning the geologic history of the Sierra Nevada are refuted by previously published field evidence. The present study concludes that tilting of the northern Sierra Nevada drove incision of the American River system beginning 5 mya (or 1.9 mya); however, the presence of older sediments deep within the canyons of the American River (and other major Sierran rivers) unambiguously falsifies this conclusion (Gabet, 2014): a canyon cannot be younger than the sediments found within it. This study also concludes that the southern Sierra Nevada experienced 2.3° of tilt over the past few million years, a result contradicted by the slope of a lava flow that

proves that there could not have been more than ∼1.4° of tilt since the mid-Miocene (Huber, 1981). I describe these problems in more detail below, along with other issues; for example, the authors appear to have been confused about which river they were analyzing. I also provide suggestions for how the numerical analyses and the modeling might be improved. (I apologize if this 'short comment' seems long-winded, I've just written 2 papers on this subject and these issues are fresh in my mind.)

NORTHERN SIERRA NEVADA Although the authors refer to the Middle Fork of the American River throughout, the long profile shown in Figure 10a appears to be for the Rubicon River, which is a tributary to the Middle Fork, so I wasn't sure which river they actually analyzed. My comments below apply equally well in either case.

The approach used here is based on presumed incision depths below a series of late Cenozoic volcanic rocks found along the interfluves. This analysis is entirely dependent on the assumption that the present elevation of these volcanic rocks are faithful recorders of the pre-uplift topography. However, this assumption was falsified in Gabet (2014). In that paper, I provide examples of volcanic rocks and Eocene-Oligocene gravels that reach deep down into the canyon of the South Fork American River (note, the age of the volcanic rocks in Fig. 15b in that paper is late Miocene, not 3 my). In addition to those examples, 24 myo volcanic rocks can be found ∼200 above the bed of the South Fork at an elevation where the ms claims that there has been ∼500 m of post-5 my (or 1.9 my) incision on the Middle Fork, and 6.5 myo rocks can be found just 70 m above the bed of the South Fork at an elevation where the authors are claiming ∼300 m of incision on the Middle Fork (Gabet and Miggins, in review). Although these examples come from the South Fork, it is part of the same drainage system as the Middle Fork and the Rubicon and subject to the same forcings, and, therefore, should have a similar history of incision. On the Middle Fork, at a bed elevation of 1050 m, Lindgren (1911) found Eocene-Oligocene gravels at 1236 m, thereby constraining the net bedrock incision over the past ∼40-50 my to only 186 m (Gabet and Miggins, in review); the present ms, in contrast, suggests that there has been ∼400 m of incision at this elevation on the Rubicon (?) just in the late Cenozoic (Figure 10). I present other examples in Gabet and Miggins (in review), and they all demonstrate that there have been minimal amounts of basement incision in the late Cenozoic. To make this point more succinctly, unequivocal field evidence shows that the valleys of the Middle Fork and the South Fork of the American had already been incised down to ∼70% of their modern depths by the early Oligocene. Therefore the assumption underpinning the present manuscript's incision analysis is not valid – the position of the late Cenozoic volcanic rocks do not faithfully record the region's paleotopography. Moreover, the field evidence demonstrates that the beds of the Middle Fork and South Fork rivers were already near their present elevations by the early Oligocene, a conclusion incompatible with the results presented here (see also House et al, 1998). This conclusion applies equally well to the Rubicon because it's a tributary of the South Fork. The fact that uplift-related incision could not have occurred after deposition of the volcanic rocks has been known since the earliest days of California geology (a key point not appreciated in the Wakabayashi papers). For example, in 1880, after an exhaustive survey of the Tertiary gravels, Whitney concluded, with respect to uplift, that "There is also abundant evidence the volcanic epoch was not inaugurated in the Sierra until the range had approximately its present form." Also, since these canyons have been deep for a long time, knickpoints along the profiles of their tributaries cannot be used in the manner adopted here to provide evidence of recent uplift-driven incision (i.e. Section 6.2). (Note to the AE, I can send you a figure from my 'in review' paper showing evidence for the antiquity of these canyons).

I spent some time trying to understand why the results from the numerical analysis diverge from the actual field evidence to see how it might be improved. One issue is that the streampower framework begins with the assumption that there is uplift to be detected (egg, U in Eqn 5 cannot be zero). In other words, this technique cannot be used to determine whether or not there was uplift because it already assumes that U is not zero. Note that, although several years of measurements have found evidence for present uplift (Hammond et al., 2012), this is likely due to groundwater depletion (Amos et al., 2014); moreover, there would be little justification for applying several years of modern data to the past several million years.

Another critical issue with the streampower analysis is the assumption that the erodibility along the river is uniform (Section 6.1). Rivers in the American River watershed flow across a wide variety of rock types with a range of erodibilities. In Gabet (2019), I demonstrate that lithology is the primary control on the channel steepness index; therefore, any attempt to extract tectonic information from channel profiles must first account for bedrock erodibility. I calculated the normalized channel steepness for 36 different rock units in the northern Sierra; I also determined an average Ksn for the four main lithological categories. If the authors (or the AE) would like, I would be happy to provide a pre-print of this paper – it is an unfortunate bit of timing that it was not published sooner. The authors could use my results to account for differences in erodibility among the different rock types and then re-run their analyses. On a related note, the authors should be aware that the GIS lithology layer (Ludington et al, 2005, 2007) is not accurate (eg, contacts are sometimes not where they are supposed to be) and can't be trusted for detailed analyses.

With respect to how the model was parameterized, the river incision rate used (0.06 mm/yr; Section 6.1) did not appear to be well-justified. The study used a value from Stock et al (2004), which is an incision rate through limestone from a site ∼100 km away. Aside from the issue of distance between the two sites, the differences in lithology between the two is critical: the American River system cuts through very resistant rocks like granite, metavolcanics, and quartzite (note, the 1:250000 map shows sandstone but Hietanen (1973) clearly describes this as quartzite). As shown in Sklar et al (2001), these rocks are 1-2 orders of magnitude more resistant to fluvial abrasion than limestone (see also Hack, 1957). A more appropriate estimate for an incision rate can be determined with data from Gabet and Miggins (in review) in which we dated a volcanic deposit that is just 70 m above of the South Fork of the American River. At this site, the river is cutting through granitic rock, which is similar in erodibility to quartzite and metavolcanics (Gabet, 2019; Sklar and Dietrich, 2001). The age of the deposit is 6.5 Ma, yielding an incision rate of 0.01 mm/y over the past several million years; unfortunately, the data for this calculation are in a paper that is under review and, therefore, can't be cited until it is published but it highlights the issue with the rate used in the present ms. This incision rate also comes with an important caveat: in the latter half of the Cenozoic, the Sierras were buried by volcanic rocks from 2 major eruptive episodes, the first beginning about 28 mya and the second ending about 5 mya. Therefore, any incision rate based on the elevation of a volcanic deposit only constrains the maximum rate. For the example I just gave above, it is possible that portions of flow descended all the down to the river's present elevation but were eroded away, meaning that a bedrock incision rate of zero over the last several million years is fully consistent with the field evidence.

SOUTHERN SIERRA NEVADA For the southern Sierras, this ms analyzes the steepness index of the modern rivers according to their azimuth and concludes that there has been 2.3° of tilting along an azimuth of 237°. This conclusion is based on a resemblance between a DEM analysis of terrain south of the San Joaquin River and the results of a landscape evolution model in which instantaneous tilt was imposed as a boundary condition. However, the 2.3° of predicted tilt in the late Cenozoic in the southern Sierra is refuted by the field evidence. Along the San Joaquin River (in close proximity to where the authors made their slope measurements), a 10 myo trachyandesite flow has created a set of table mountains at the southern edge of Millerton Lake (Huber, 1981); from the lower table mountain to Squaw Leap, the upper, uneroded surface of this flow has a slope of 1.37° at an azimuth of 226°. Therefore, even if the upper surface of the lava flow had a slope of 0° when it cooled, it limits the total amount of possible tilt since 10 mya to only 1.39° (at an azimuth of 237°) because any more tilting would mean that the lava had flowed out of the Central Valley and into the Sierras, which would be nonsensical since the source of the flow was in the mountains. Furthermore, the mid-Miocene fluvial deposits underneath the lava flow can also be used as a tilt-meter (albeit with a bit more uncertainty). A paleo-slope analysis based

on the size of the gravels shows that they were deposited on a slope of ∼0.15°; their present slope is now at 1.26°, meaning that the maximum amount of tilt experienced by the region since the mid-Miocene cannot exceed ∼1.1° (Gabet, 2014). Therefore, two sets of field evidence refute the conclusion that there has been 2.3° of tilt at any time since the mid-Miocene in the southern Sierras. Similarly, Mix et al (2019) finds no evidence for significant late Cenozoic uplift in the southern Sierras.

MODELING I was concerned by the tectonic boundary conditions imposed in the model, specifically the instantaneous tilt of 1000 m, the instantaneous baselevel drop of 1000 m, and the instantaneous 1000-m tilt with uniform background uplift. It is obvious, of course, that none of these scenarios are realistic (i.e. mountain ranges don't instantaneously increase their elevations by 1000m); but that's not necessarily a problem. I understand the value in whacking a model and seeing what happens. The problem arises, however, when the results from these types of modeling scenarios are used to interpret real landscapes. For example, results from a model run with instantaneous tilt (Fig. 7c) are compared to a DEM analysis of terrain near the San Joaquin River (Fig. 12c), and a similarity between the two are used as evidence for tilting of the southern Sierra. Because the key element (tilting) differs in a fundamental way between the two (instantaneous in the model vs gradual in the real world), a comparison between the two seems like an apples-and-oranges situation. This may explain why the manuscript's estimate of tilt magnitude in the region is contradicted by the field evidence. Nevertheless, I think this technique could have value (in fact, I once considered trying something similar, albeit not as sophisticated) if the modeling adopted more realistic boundary conditions. I provided, in an earlier comment, some calculations that could be used to place constraints on a tilt rate over the past ∼10 my.

The rigid-block assumption over a distance of 180-km is also problematic because real crust will bend, which means that tilting is not distributed uniformly across the landscape (Martel et al., 2014). To demonstrate this, I did some calculations using the standard elastic half-space beam flexural model with the 'broken plate' assumption (appropriate because of the range-front faults along the eastern Sierras), regional flexural rigidity parameters (Martel et al., 2014), and the assumption that tilting is due to loading at the lower end of the block (i.e., the eastern edge of the Central Valley, at x=0 km). The results indicate that 2° of tilt at x=0 km decays rapidly with distance such that at x=100 km the block is only tilted by 0.7°, and the end of block (i.e., x=180 km) experiences essentially no tilt. Of course, this point loading scenario is a simplification and represents an end-member scenario; I use it here because it has an analytical solution. It nevertheless emphasizes the point that crustal flexure can't be ignored at these spatial scales, especially if one is trying to use slopes to detect tectonic signals. I would recommend looking at Martel et al.; they explore other uplift scenarios for the Southern Sierra which also show that tilting decays rapidly with distance. I think that incorporating crustal flexure into a tilting landscape evolution model would be a neat and interesting contribution. It may also help damp tilting in the model that led to the overestimation of tilt in the southern Sierra.

CONNECTIONS WITH PREVIOUS WORK The final issue was that the incorporation and analysis of previous work on the geological history of the Sierra Nevada could be more balanced. All but one of the geomorphic papers (Stock, 2004) marshaled to support late Cenozoic tilting are known to be flawed in one respect or another, or they are not appropriate in the context of this manuscript. In contrast, important recent papers challenging the hypothesis of recent uplift were dismissed a bit too easily or were missing from the discussion. With respect to the former group, I explore, in detail, their problems in Gabet (2014) but provide a brief synopsis here (I've uploaded a copy of this paper). I present these in the same order (more or less) as the list on page 23, line 20 and only focus on those presenting field evidence (pure modeling papers always limited by their assumptions).

1) Christensen (1966) concluded that the northern Sierra had been tilted by comparing the gradients of Lindgren's Tertiary paleochannels to modern analogs. However, the modern counterparts were small, ephemeral sand-bedded streams in semi-arid land-

scapes and, thus, were inappropriate and irrelevant analogs for the large, subtropical rivers capable of transporting meter-sized boulders that were draining the Sierra during the Eocene.

2) Clark et al (2005) analyzed river profiles in the southern Sierra and concluded that uplift-driven incision began carving the Kern River canyon 3.5 mya. However, this study was refuted by noting the presence of 3.5-my volcanic rocks deep within Kern Canyon.

3) The results from Huber (1981) can only constrain tilting of the southern SN, and they can only constrain tilting since the mid-Miocene. In other words, Huber (1981) cannot be used to support significant tilting over only the past 5 my, as demonstrated above.

4) Lindgren (1911) based his tilt estimate with the assumption that he correctly reconstructed the Tertiary paleochannels. Both Gabet (2014) and Cassel (2012b) demonstrated that his reconstructions were fundamentally flawed, either because they imply that water flows uphill or because he was linking channel segments that were unrelated in time and space. Moreover, Gabet (2019) demonstrates that, even if Lindgren had correctly reconstructed the channels, the differences in their gradients that he attributed to tilting can be wholly explained by differences in bedrock erodibility.

5) Jones et al (2004) based their tilt estimates on Lindgren's faulty reconstructions (see above) and are, therefore, inconclusive. This is evident by noticing that Jones et al's untilted South Yuba River has 4-5 reaches that defy gravity (i.e. water flows uphill in their Figure 3C). Note, also, that the good gradient-azimuth relationship in their Figure 3A is only possible because ~1/3 of the data points were ignored. Finally, like Lindgren, the authors did not account for bedrock erodibility.

6) Unruh (1991) based his tilt estimate of 1.4° on the gradient of Central Valley sediments. By using simple geometry, one can demonstrate that a consequence of this result is the prediction that, at some point in the mid-Cenozoic, Tertiary gravels at an elevation of ~700 m along the Yuba river were once ~500 m below sea level. In other words, untilting the northern Sierra by 1.4° places sections of the mountain range deep

underwater sometime in the past 30-50 my. This study is refuted, therefore, by the absence of deep Cenozoic marine sediments in the Sierra.

7) The Wakabayashi papers mostly base their analyses on incision depths determined from volcanic rocks, which are contradicted by the presence of these rocks deep within the canyons. The Wakabayashi papers present some other analyses which are also refuted (or challenged) by the field evidence.

8) Yeend's (1974) tilt estimate is based on Lindgren's paleochannels and suffers from the same problems as Jones et al (2004): (1) using the gradients of channels that never existed as continuous features in time or space and (2) not accounting for the role of lithology.

Therefore, the present manuscript leans on some discredited or inconclusive papers to support its results. In contrast, the published evidence for tectonic quiescence of the range throughout the Cenozoic is somewhat elided. Admittedly, the isotopic data from the leeward side of the Sierra may be confounded by the issue of terrain-blocking and, thus, citing Molnar (2010) is a reasonable counterpoint to some of these studies; however, several recent papers have avoided this problem. Mix et al. (2019) specifically addresses the terrain-blocking issue and finds no evidence for recent uplift of the southern Sierra (to be fair, this paper was just published so the authors may not have seen it). Cassel et al (2009) and Mix et al (2015) present isotopic data from the windward side of the northern Sierra, thereby avoiding the terrain-blocking problem, and also find no evidence for recent significant uplift. Cassel and Graham (2011) present a shear-stress-based paleo-slope analysis that also supports tectonic quiescence since at least the early Oligocene. Of course, there are uncertainties associated with the results from these 4 papers, however, it seems unlikely that, by coincidence, they would have all found the same result despite using 4 different techniques. In addition, recent papers describing the paleotopography of the region also merit consideration. Cassel et al (2014) and Cassel et al (2012a) provide evidence that, in the Oligocene, the Sierra Nevada was the western ramp of a high-elevation plateau, the Nevadaplano, that has

been gradually collapsing. Thus, according to these studies, it is not the Sierras that have been recently uplifted, it's the land to the east that's been sinking.

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Please also note the supplement to this comment:
https://www.earth-surf-dynam-discuss.net/esurf-2019-24/esurf-2019-24-SC1-supplement.pdf

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