Review of Beeson and McCoy

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Summary

This manuscript consists of 3 parts: modeling, application of the model results to the northern Sierra Nevada, and application of the model to a site in Mexico. With respect to the first part, it's a standard streampower-based exploration of different scenarios, albeit with some odd initial conditions (eg, 1000 m of instantaneous uniform uplift). The novelty is that the authors are exploring the consequences of tilting and there is value in this exercise.

Problems arise in the second part and most of my comments below are focused on this section. The history of the Sierra Nevada is a topic with important consequences regarding our understanding of the geologic evolution of western North America. Because the stakes are high, published results have to be robust. However, many of the assumptions, interpretations, and results presented here are contradicted by the field evidence. For example, the authors base some analyses on the location of a knickpoint that they claim is a tectonically-driven migrating feature; however, all of the evidence indicates that it is a lithological/structural knickpoint. In another example, their reconstruction of the geological history of the Middle Fork American River canyon is refuted by the stratigraphic evidence.

With respect to the analysis of the Mexican site, I am not familiar with the region and was unable to procure a geologic map of the area. However, I am concerned that, although the analyses yield results with respect to tilt magnitude and timing, there doesn't appear to be any data with which to validate them. Usually, when new approaches are developed, their results are tested against known data – this provides confidence in the new approach so that it can be applied to new areas, and it gives us an idea of how accurate it is. In this case, a *new* approach is being applied to a *new* area and so an important step is being skipped. I would recommend applying these techniques first to a site where the tectonic history is well-known. Until that is done, we don't have any way of scientifically assessing the validity of this approach or the robustness of the assumptions (eg, uniform erodibility).

Comments according to section heading

Introduction (p. 3)

To motivate the analyses done in the Sierra, the authors cite several papers that they claim support the hypothesis of Late Cenozoic tilting; however, these studies have all been debunked. Below is a brief synopsis of the fatal flaws in each of them; more detailed explanations can be found in Gabet (2014). If the authors would like to cite these papers, they will need to explain why the analyses of these fundamental flaws are incorrect; otherwise, there's not much value in referring to discredited studies.

Lindgren (1911) based his tilt estimate with the assumption that he correctly reconstructed the Tertiary paleochannels. Both Gabet (2014) and Cassel (2012) 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.

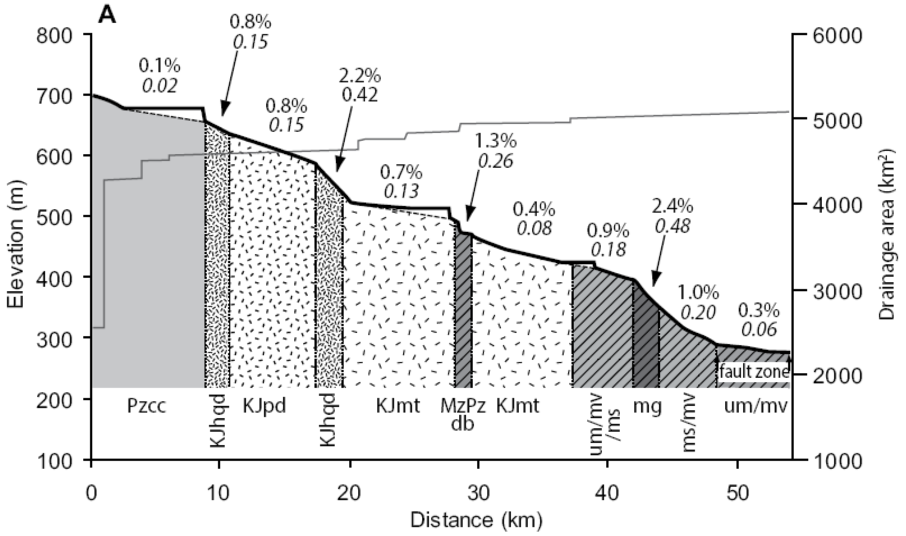
Jones et al (2004) based their analyses on Lindgren's reconstructions. Since those reconstructions are faulty, their analyses are meaningless. In fact, the reconstruction of the South Yuba River in Jones et al has reaches where water flows uphill (an absurd result). Moreover, like Lindgren, Jones et al ignores the role of erodibility in controlling channel slope.

Wakabayashi (2013) calculated bedrock incision depths and rates from Pliocene volcanic deposits along the rims of canyons without recognizing that older deposits can be found farther down the canyon walls.

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.

4 Modeling fluvial longitudinal profile response to perturbations (p. 7)

In this section, the authors describe the profiles from a series of numerical simulations that are then compared to the Middle Fork American River. Their model, based on the simple streampower formulation, is used to show that tilting leads to knickpoints that migrate up from the range-front. However, this result is obtained by making the extraordinary claim that rock erodibility is uniform throughout the area. In fact, rock erodibility is extremely *non-uniform* throughout the range and is the primary control on channel steepness (Gabet, 2019). Shown below is the profile of the North Fork Feather River demonstrating the important role of lithology and erodibility on profile shape. Note how nearly every knickpoint is associated with a lithological boundary and how the steepness index (the second number above each reach) varies according to rock unit, even from one granitic unit to the next.



4.9.2 Estimating tilt magnitude from rock-type knickzone geometry

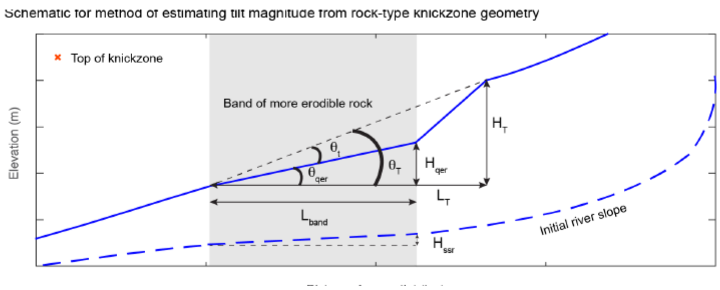
In this section, the authors estimate tilt based on the geometry of a river profile that flows across weak rock that is sandwiched between strong rock. There were a number of issues here.

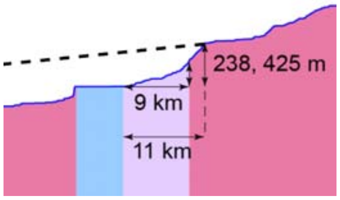
(1) To develop their model (Fig. 7), the authors are assuming *uniform* uplift in the northern Sierra Nevada (p. 23, l. 7,8). There is, however, *no* evidence for uniform uplift in the northern Sierra Nevada during the Cenozoic. The authors are the first to ever make such a claim and they do so without providing any evidence. Uniform uplift would create an obvious fault scarp hundreds of meters high along the entire western range-front, a feature which has not been observed.

(2) This technique is dependent on the assumption that the reach formed during the transient response to tilt is similar to the initial steady-state river slope. The authors, however, have not provided any evidence to support the claim that the pre-tilt river was at steady-state. Given the multiple episodes of aggradation, incision, and drainage reorganization experienced by these rivers due to repeated volcanic eruptions, the odds that this assumption is correct are vanishingly small.

(3) The authors are assuming specific erodibility values for the rock units at this site along the Rubicon River without any evidence that these values are accurate. They are using a *K* value determined from a different granitic bedrock unit in the southern Sierra Nevada and applying that to the Rubicon site without accounting for the fact that erodibility can vary greatly in granitic rocks (Gabet, 2019). For the Jurassic marine rocks, which the authors take as the “erodible”unit, they are assuming that it is 10x more erodible than the granitic rocks but, again, without any evidence. In fact, the Jurassic marine rocks include quartzite, which is as strong as granitic rocks, and greywacke, which is also very strong and certainly not 10x weaker than granitic bedrock (Gabet, 2019).

(4) Finally, the field site does not conform to their model. Below (left) is their figure illustrating their model and (right) the actual profile. Note how, in the model, the dashed line extends smoothly from the profile at the top of the knickzone down to the lower extent of the “erodible” rock. In the real river, extending the profile above the knickzone in a similar manner yields a completely different geometry.



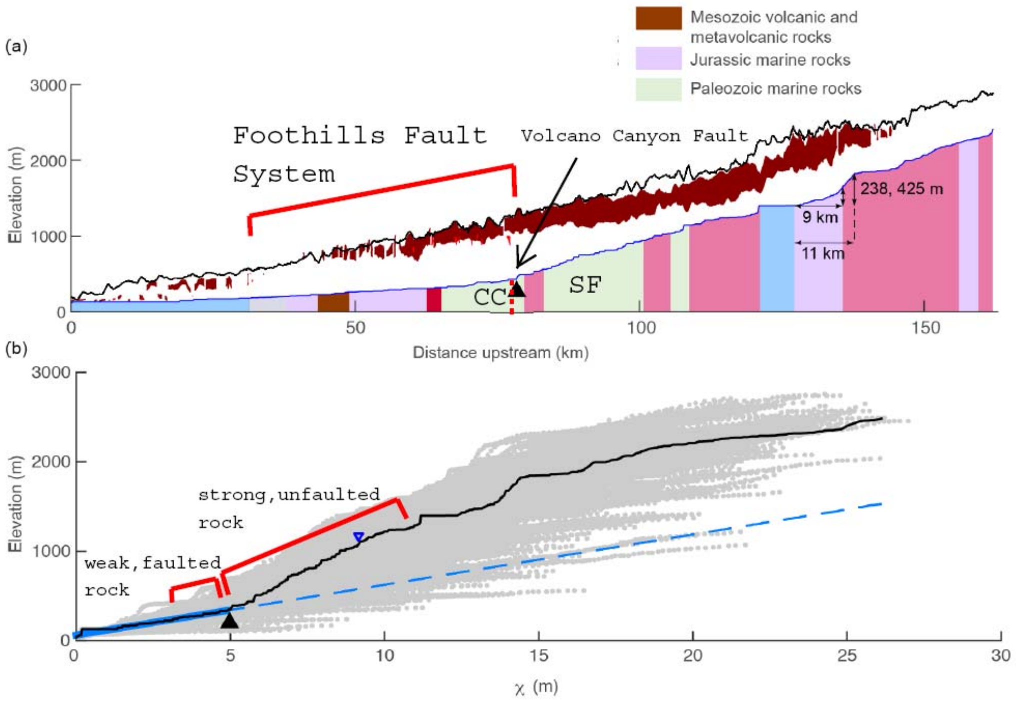


(5) To summarize, this tilt estimate is wholly dependent on the claim that the Sierras have experienced uniform uplift, which is demonstrably incorrect, as well as several assumptions which are unlikely to be all correct, and a geometric analysis that

does not apply to the field site. It would be difficult to conclude, then, that the tilt estimate from this analysis is scientifically sound.

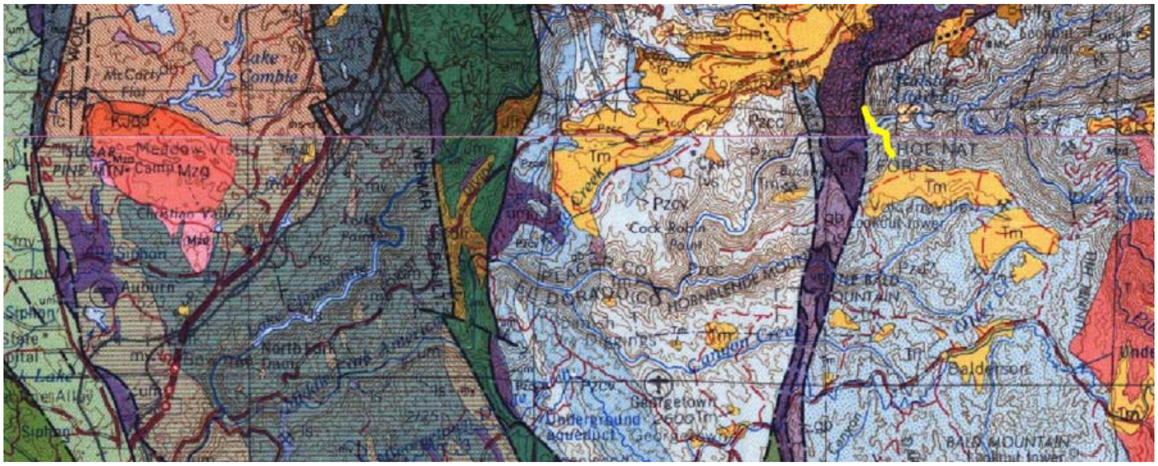
6.1 Disequilibrium form of the mainstem Middle Fork American (p. 31)

In this section, the authors identify a knickpoint in the Middle Fork American River and assume that it is a migrating knickpoint generated by uplift (black triangle in the profile below). This knickpoint, however, is associated with both a lithological and structural boundary.

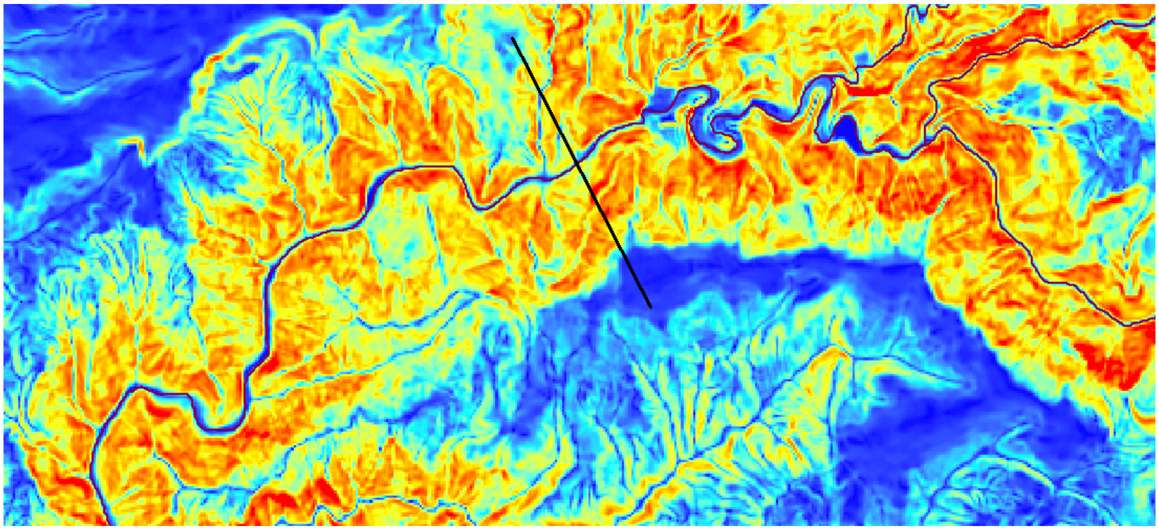


Although the authors have combined the metamorphic rocks (in green) directly above and below the knickpoint into a single unit, this obscures the fact that they are different formations with different lithologies (note that the pink band of granitic rocks that they show immediately above the knickpoint does not exist on any map that they cite). I have labelled these formations as CC and SF on their figure; I have also added a red dotted line to show the location of the Volcano Canyon Fault that forms the contact between the two (ignoring, as the authors have, smaller units at that site). The metamorphic unit on the upstream side of the knickpoint is the Paleozoic Shoe Fly Formation (SF), which is composed of resistant quartzite and metavolcanic rocks. The unit on the downstream side of the knickpoint is the Paleozoic Calaveras Complex (CC), a highly sheared subduction melange that includes weaker argillite and chert. From Gabet (2019), the steepness index (a measure of erodibility) of the Shoe Fly Formation is 0.13 while, for the Calaveras Complex, its average is 0.07 (note, these values are from other rivers and are, therefore, independent of the particular situation on the Middle Fork). The difference in the steepness index between the two units is strong evidence that this is a lithological knickpoint and, moreover, that the assumption of uniform erodibility is violated.

In addition, their profile does not show that the area downstream of the knickpoint is within the Foothills Fault System where many faults have made the bedrock more erodible (Gabet, 2019) – I have added a label and a bracket to their figure to show the extent of this fault zone. The figure below shows the lithological map of this area where the faults and the different units can be seen (a yellow line across the Middle Fork marks the knickpoint).

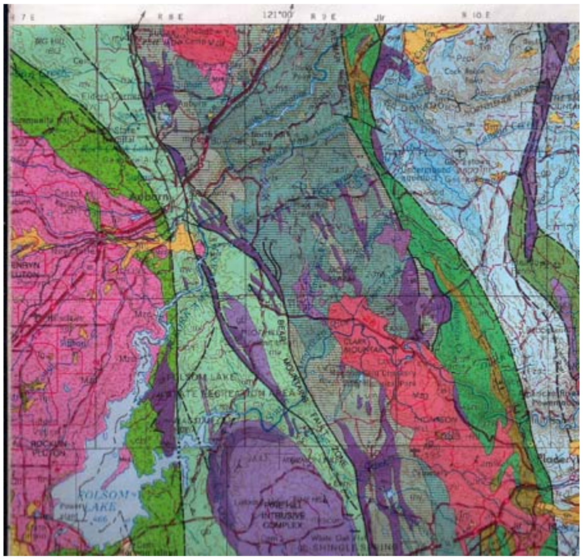


Another piece of evidence indicating that this knickpoint is not a migrating feature comes from the surrounding hillslopes. As demonstrated in Hurst et al. (2013), hillslopes just downstream of a knickpoint will be steeper than those upstream. However, in the slope map below, where the black line shows the location of the knickpoint and the river flows from right to left, the hillslopes are actually *steeper* upstream of the knickpoint (red is steep, blue is gentle). This slope map, therefore, not only contradicts the assumption that this is a migrating knickpoint but, instead, it supports the conclusion that the reach of river upstream of the knickpoint is steeper because the rocks are stronger.

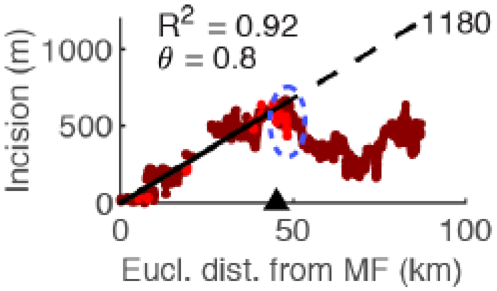


Thus, not only have the authors not provided any evidence that this is a migrating knickpoint, all of the available evidence indicates that it is a lithological knickpoint. As a result, the analyses based on the claim that this is a migrating knickpoint are fundamentally flawed unless the authors can demonstrate that this is, in fact, a migrating feature (ie. via field observations rather than model results); this includes the results regarding the magnitude and timing of uplift which I discuss below.

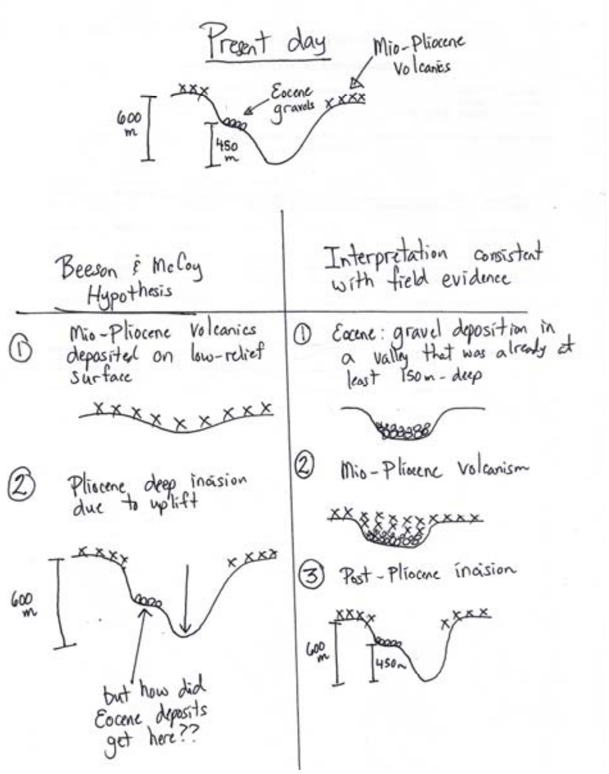
Based on the identification of the knickpoint as a migrating feature, the authors use its location to make estimates about the timing of uplift. To make this estimate, the authors assume that *erodibility is uniform* along the lower part of the profile that the knickpoint travelled across; moreover, they assume that *the erodibility of those rocks is the same as granitic rock* in the southern Sierra Nevada (p. 31, l. 25). Both of these assumptions are contradicted by the field evidence. In the map below, the section of the Middle Fork below the knickpoint begins at the top left corner and then flows diagonally down towards the northern part of Folsom Lake, through the lake and into the Central Valley. Along this path, it crosses a dozen different geological units, and only one of these is granitic while the rest are various types of metasedimentary and metavolcanic rocks, including a highly sheared subduction melange. As demonstrated in Gabet (2019), the erodibility of these rock types vary greatly; for example the steepness index of the subduction melange is about half that of granitic rock. Furthermore, the Middle Fork crosses six faults, which means that the rocks at those locations will be much weaker than at other spots (Gabet, 2019). Therefore, in addition to problem of assuming that the knickpoint is a migrating feature, the two other assumptions necessary for calculating the timing of uplift are violated



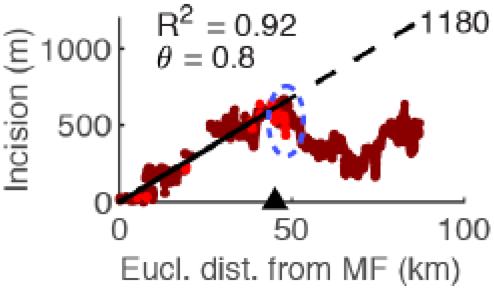
Finally, in this section, the authors estimate the magnitude of recent tilt based on alleged incision depths beneath Mio-Pliocene volcanic deposits (in brown below) and Eocene fluvial gravels (in red). The estimates of incision depths, however, are contradicted by the field evidence.

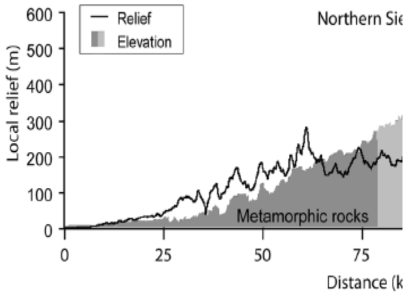


For example, at the site just above the black triangle on the plot above, the authors are claiming that there has been ~600 m of incision since the Miocene-Pliocene (see the regression line) and that this has been driven by recent uplift. However, the elevation of the Eocene deposits *proves* that there could *not* have been more than ~450 m of incision since the Eocene. In the illustration to the left, I show the present day profile across the Middle Fork at the top, Beeson and McCoy's interpretation of this profile (left column), and the standard interpretation to the right. The critical point is that the authors are claiming that the Mio-Pliocene volcanics represent a bedrock paleosurface and that all of the relief below the volcanics is due to Pliocene incision into basement rock. However, their interpretation cannot explain the presence of Eocene deposits *below* the Mio-Pliocene deposits. The only interpretation consistent with the field evidence is that the Eocene gravels represent the bedrock surface during the Eocene when this canyon was already at least 150 m deep. This landscape was then buried by volcanic deposits that are known to have been hundreds of meters thick. To put it bluntly, because 450 < 600 and because the Eocene is older than the Mio-Pliocene, the estimate for recent tilt based on the regression line in the incision plot is invalid. The only accurate statement that can be made is that there's been a maximum of ~450 m of incision since the Eocene – Early Oligocene.



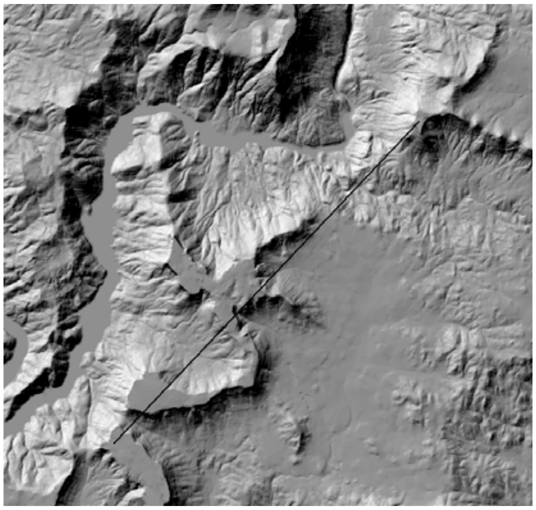
The use of the Mio-Pliocene volcanic deposits on the interfluves as an indicator of a paleosurface was promoted in papers by Wakabayashi (Wakabayashi and Sawyer, 2001; Wakabayashi, 2013) and this method has since been debunked as older sediments can be found far below them (Gabet, 2014). Because the volcanic rocks are remnant patches left on the ridges, the plot above is simply showing that relief increases from the range-front and then decreases as the crest is approached, as shown in (Gabet, 2014); compare the incision pattern of the volcanic rocks in the plot to the left with the plot of relief below (solid line).



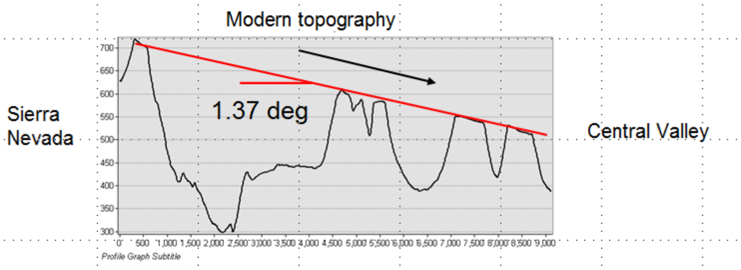


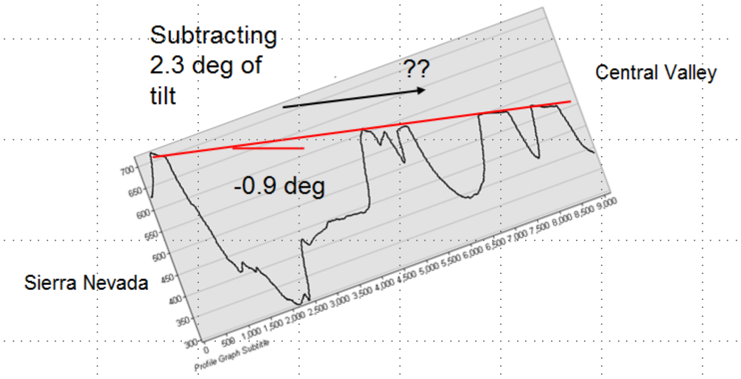
6.3 Tilt magnitude recorded in the stream network (p. 35)

In this section, the authors use a relationship between reach azimuth and gradient to look for signatures of tilt. Based on their analyses, the authors conclude that there has been 2.3° of recent tilt. Their technique can be tested using field observations. As noted by Huber (1981), the upper uneroded surface of a 10 Myo lava flow along the San Joaquin River (which is only 5-10 km north of where the authors did one of their analyses) forms a series of table mountains. The source of this flow was the Sierra Nevada (top-right of the map shown to the left) and it flowed down into the Central Valley (bottom-left of the map). The line in the figure to the left shows the transect plotted on the next figure.



The upper surface of the flow is at an angle of 1.37° (first figure below). If we subtract the 2.3° of recent tilt hypothesized by the authors, the upper surface of the flow is now at -0.9° (ie. 1.37 – 2.3 = -0.9; second figure below). This means that, if there had been 2.3° of recent tilting (as the authors claim), this lava would have flowed uphill 10 Mya. This result challenges the approach presented here.





The authors state that their results are a “maximum possible tilt magnitude (p. 36, l. 7)”. However, the table mountains data show that the maximum is actually ~1.4°, indicating that their technique is, in fact, unable to constrain the maximum amount of tilt. Moreover, without providing any bounds on the *minimum* possible tilt magnitude, their results are consistent with 0° of tilt. Therefore, this analysis isn’t providing any new information. At a minimum, a few other nearby sites should be analyzed because a sample size of 1 is too small to provide confidence in this technique. This would have the benefit of providing some measure of the potential error.

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