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Interactive comment on "Fluvial boulder transport controls valley blocking by earthflows in the California Coast Range, USA" *by* N. J. Finnegan et al.

Prancevic (Referee)

jeff.prancevic@gmail.com

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This manuscript addresses the interesting question of how different parts of the river system deal with earthflow-derived boulders and how this could influence hillslope-channel feedbacks and knickpoint propagation. The manuscript is well written and was a pleasure to read. However, I feel that the manuscript has several issues that should be addressed prior to publication.

In the manuscript, the authors present a mechanistic argument for why small rivers are more susceptible to blocking by earthflows than are large rivers. The argument hinges on the tendency for small rivers to have both narrower widths, making them more sus-

ceptible to boulder jamming, and shallower flow depths, leaving large boulders emergent from the flow during bankfull discharges. Both of these mechanisms should lead to boulders being more stable, locally inhibiting bedrock incision with measurable impacts on the river longitudinal profile: local steepening of the river where it crosses the toe of the earthflow, and widening of the river upstream. The manuscript argues that these topographic indicators of boulder stability are absent in the longitudinal profiles of large rivers because boulders are more easily transported away.

These arguments seem very reasonable to me, qualitatively, but I see some issues with the evidence used to verify these mechanisms in this manuscript:

Flow hydraulics and flow competence: My primary concern with the manuscript is the line of reasoning used to calculate the maximum boulder size that can be transported by the Eel River and Arroyo Hondo. First, as far as I understand, the analysis relies on hydraulic measurements (flow depths, velocities, and discharges) that were made at USGS gage stations that are 2 km (for Oak Ridge earthflow) and 25 km (Mile 201 slide) downstream of the earthflow deposits. Flow depths, velocities, and discharges at the gage sites are not the same as those at the earthflow deposits. In particular, flow depth and velocity are both sensitive to changes in bed sediment size and channel width. The presence of coarse sediment (such as that found in the earthflow deposits) tends to reduce flow velocities and increase flow depths (e.g., Rickenmann and Recking, 2011). This phenomenon is particularly concerning for this analysis because it may reduce the tendency for boulders to emerge from the flow, which the manuscript proposes as the primary mechanism to stabilize boulders. Second, I am not sure that it is necessary to introduce a new, untested calculation for boulder mobility when tested models already exist.

Simultaneously accounting for the effect of boulders on sediment mobility and flow resistance (for calculation of flow velocity and depth) is tricky and prone to error, but these difficulties can be circumvented by using a critical dimensionless stream power to calculate flow competence (Parker et al., 2011). This criterion has been shown to be a more reliable predictor of sediment mobility than the critical Shields stress in shallow, rough flows (Parker et al., 2011; Ferguson, 2012; Prancevic and Lamb, 2015), and it only requires information about discharge, width, slope, and grain size, all of which the authors have measured at the earthflow deposits.

In a quick calculation, I found that this method predicts that the largest mobile sediment during the bankfull flood is $\sim$20 cm for both sites. I may have missed something, but this would suggest that there is not a large difference in flow competence between the two sites. Moreover, these grain sizes seem much more reasonable to me than 2.4 m and 4.9 m. (Can a 4 m boulder really be transported by a 2-year flood in the Eel?) This estimate of the mobile size fraction is also consistent with the observation that there are very few boulders larger than 30 cm found in the river outside of earthflow deposits at both sites. __________________

Comparison of longitudinal profiles: I do think that the longitudinal profiles (Figs. 5-7) show different earthflow signatures between the study sites, but I think that the comparison would be more compelling if it were more even. The profile of the Eel River is 3x to 4x longer than the other two profiles but is squeezed into the same plot size, which makes it difficult to compare the topographic imprint of earthflows (which seem to not change substantially in size between the two study areas). Moreover, the slope measurements on the Eel River were made at a resolution 10x larger than at the other study sites, potentially smoothing over some of the variability in slope.

I think it would be more convincing to use the same spacing for slope measurements and to zoom in on a portion of the Eel River profile such that the profile examined a similar length to the others two. __________________

Boulder supply rate: The manuscript focuses on the relative size distributions of coarse sediment supplied to the river between the two study areas. However, it may also be more important to consider the supply rate of coarse boulders. If boulders are delivered

very slowly, then the river can rely on bigger, rarer flood events to remove the boulders. It may be outside of the scope of this paper to consider this effect quantitatively, but it should at least be discussed, especially because the Oak Ridge earthflow seems to be sliding 5x faster than the Mile 201 slide. This could be part of the explanation for why there are more big boulders in the river next to the Oak Ridge earthflow.

Also, if the supply rate is important, it is not only the distribution of coarse sediment delivered by the earthflow that matters, but also the proportion that is coarse sediment. What portion of earthflow-derived material is just fines that is just being easily washed away, and does this proportion vary between the two sites? Again, it may be outside of the scope of this paper to measure this, but a discussion is warranted. __________________

Grain size distributions: I had a difficult time understanding how the grain size distributions were characterized. Were the original distributions calculated using an area-by-number measurement and then transformed to a grid-by-number (or, equivalently, volume-by-weight) (e.g., following Bunte and Abt, 2001)? I'm just a bit confused about how the immobile fraction is transformed from 10% to 80% and 1% to 20% for the two study sites. Please be explicit about which distributions are being used, and perhaps consider using only the volume-by-weight equivalent to avoid confusion.

Also, the manuscript argues that the sediment sizes measured from aerial imagery are representative of the distribution of coarse sediment, but this is somewhat inconsistent with the rest of the analysis that argues that meter-scale boulders are at least partially mobile. This means that the deposits may also be winnowed with respect to boulders, and not just sediment finer than 30 cm. In other words, the river has likely moved more of the 1 m boulders than 2 m boulders since the boulders were deposited. This might be a small effect, though, especially if the mobile fraction is actually much finer than what is currently reported in the manuscript. __________________

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