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Earth Surface Dynamics Discussions

Interactive comment on "Influence of topography and human activity on erosion in Yunnan, SW China" *by* Amanda H. Schmidt et al.

Anonymous Referee #1

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Schmidt et al. present an interesting study in which they aim to evaluate the effects of human activity on long-term apparent erosion rates calculated from measurements of in-situ 10Be collected from river sediments in a nested sampling scheme across three watersheds in Yunnan, China. The authors choose three watersheds differing in size across covarying gradients of elevation, precipitation, channel steepness and land use. The authors present the results of correlation and regression analyses of in-situ 10Be-derived long-term apparent erosion rates calculated using the CRONUS-Earth online calculator with topographic (basin average slope, normalized channel steepness and relief/elevation), climatic (mean annual precipitation), and land use/land cover variables.

Through this analysis, the authors present results which suggest that in-situ 10Be derived apparent erosion rates are related predominantly to topographic variables in two

of the three watersheds (Yongchun and Weiyuan) which are not dominated by human land-use), while apparent erosion rates are not correlated with topographic variables (and instead correlated with area in agricultural land use) for a third watershed (Nankai) which was heavily dominated by human land-use. Further regression analysis suggests that human land-use may play a secondary but not controlling role in the Weiyuan watershed (which lies in the middle of the land use gradient from most intensive human land-use (Nankai) to least intensive human land use (Yongchun).

The authors conclude that in-situ 10Be apparent erosion rates can be significantly affected by human land-use at the basin scale and suggest caution to researchers attempting to derive long term apparent erosion rates in watersheds which have undergone significant human land-use change, are not large enough to buffer the effects of land-use change on sediment composition, and are heavily disturbed.

This is a nicely packaged study that is worthy of publication. Although the results are not surprising (there are some watershed and basins in the midwestern and southeastern United States, for example, where a significant amount of the current surficial material is actually subsoil exposed due to the effect of highly erosive historical land use practices (Piedmont, Corn Belt Plains, Driftless Area)), they are important to consider for researchers who are considering constructing studies of long-term apparent erosion rates. However I do have a few suggestions which may assist the authors in improving the manuscript.

General comments: 1. In section 5.3 of the discussion, lines 11-15, the authors present anecdotal evidence from field observations that "much of the landscape mapped as grassland and shrubland is actually tea and rubber plantations...", and thus this land use category is actually reflective of agricultural land use. This is an absolutely critical observation which allows the reader to better interpret the data from the scatterplots presented in Figure 6, and it came to me as a surprise that it is buried in the discussion. I believe instead that this observation should be presented much earlier in the manuscript, perhaps in the methods section. Do the authors have any point data

or observations associated with this that they could include in the supplementary information? Have the authors attempted to sum fraction of agricultural land use and fraction of shrub/grassland (which would presumably represent "total" agricultural land use) and plot that against 10Be derived erosion rates? Is that relationship stronger or weaker than the relationships of erosion rates with agricultural land use and shrub alone? If stronger, it would suggest that the sum of agricultural land use (regardless of management practice) is important. If weaker it might suggest that the erosive effects of tea and rubber plantations on the uplands alone are extremely important in understanding the effects of these different land uses on apparent erosion rates. Additionally, although the relationship between %shrub and erosion rate in positive for Nankai, it seems to be negative for Yongchun. Does this imply that the land cover classified as shrub/grassland in Yongchun is truly shrub, whereas the land cover classified as shrub/grassland in Nankai is in fact tea and rubber plantations? Do the authors have any observations to assist in resolving this question? I believe any further interpretations that the authors can provide in this matter will greatly assist the readers in understanding the results presented, especially when attempting to link portions of the results/discussion to the Figures and Table. Table 1 should be presented with the results and moved out of the discussion.

2. I understand the authors' intent in Figure 7, however it took quite a bit of time to interpret. It may just be my own perspective, however I believe that interpretability may be improved by putting the erosion rate and slope data points into separate portions of the figure? Additionally, the text in P7, Lines 3-4 states that "...we observe a non-linear decrease in erosion rates as mean basin ksn increases..." (again, based on the figure, I believe the authors intend to write "median basin ksn"). However, in looking at Figure 7C, I would argue that (at least in my interpretation), erosion rates appear to show a peaked non-linear distribution as median basin ksn increases, not a non-linear decrease only.

3. The authors spend a significant amount of text discussing correlation numbers,

however I could not find text describing which correlation method was used (pearson, kendall, spearman, etc...). Also, it might be helpful (whether in the main manuscript or supplementary material) to include a table of correlations of the variables with eachother so that the reader could better put the patterns seen in the scatterplots of figure 6 in context.

4. In the manuscript text, the authors integrate regression statistics into the results and discussion to support some of their major conclusions. Some of these regressions (from the text) appear to have been multiple linear regressions, however in Table S5, the results that are displayed appear to be from simple linear regression. It would be helpful to include results and more details from multiple linear regressions in table S5. For example, were they conducted with or without interactions between variables?

Minor/Editorial comments: P1. Line 15 (Abstract): Perhaps should read; "In order to better understand..." instead of "In order to understand better..."? P1. Line 32: Missing names on first reference: "1995" P5. Line 15 (Eqn 1): I believe Eqn one should have a theta term included to represent concavity. P7. Line 4: The figure referenced in this sentence shows Median basin ksn, however the text reads mean basin ksn. P7. Line 32-33: Perhaps the authors might soften the assertion that "Thus, we conclude that precipitation is not a significant control on erosion in this landscape" with something like: "Thus, within the scale and scope of our study, we conclude that precipitation is not a significant control on erosion in this landscape". P8. Lines 16-25: These seem to belong in the results section and not the discussion section. P8. Line 30: Missing names on first reference: "1995". Figure 4. It appears that sample CH-01 and CH-23 which are included in supplementary table 1 are not included in Figure 4. A short explanation for why (or a footnote either in the supplementary tables or figure) would be helpful. Supplementary Tables: It would be helpful to include the basin name (instead of just the basin number) in the supplementary tables, as any reader who wishes to analyze or interpret the raw data will be familiar with the basin names only from the manuscript text.