

## ***Interactive comment on “Holocene erosion triggered by climate change on the central Loess Plateau of China” by Gang Liu et al.***

### **Anonymous Referee #2**

Received and published: 19 December 2016

The manuscript by Liu et al. is puzzling. It presents a good problem: how erosion on the Loess Plateau, a prodigious sediment sink and source, is influenced by climate. It takes a logical approach by applying a Langbein-Schumm curve to tackle the erosion-climate relationship. However, once the erosion histories are available it fails to properly interpret them. As a result the paper is not publishable as it stands. Here are a few suggestions to improve it:

1. The authors should really reconsider where they send this paper for publication. EGU has a dedicated soil journal, a dedicated climate journal as well as a dedicated surface processes journal. At either one more suitable reviewers will provide more thorough reviews. I fail to see how this manuscript fits a Solid Earth journal.
2. Where are these soil profiles taken from? Are they from locations undisturbed by

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human activities like ploughing, herding, deforestation, etc.? The age models look pretty good suggesting that there is no homogenization by ploughing at the top and minimal erosion, but the location and process of selecting these profiles should be described.

3. the database used to reconstruct temperature, precipitation and erosion is not openly available. For T and P the paper the authors cite a paper that does not have a proper database either. For the erosion data the authors cite Chinese reports that are not openly available for scrutiny. I wonder if the policy of the journal allows publication of a paper where such basic conditions are not fulfilled. I suggest that authors include a table showing data that lead to figure 5 providing for each point the location (lat, long), local name of watershed, primary data (magnetic susceptibility, 137CS, etc.), derived data (T, P, etc.)

4. the writing is relatively OK until the Discussion section where the text becomes very hard to read. I suggest the authors use an editorial service to correct the many mistakes peppering the text.

5. not only the language breaks down in discussion but also the analysis of data. If I look at the data presented in Fig. 4 I see that erosion was stronger during stronger aridity at one site, which contrasts with other site where erosion is minimal during the most arid interval. This does not correspond at all with the extremely brief contradictory statements of the authors: "the estimated erosion intensity during the Holocene can show a principal trend of erosion caused by precipitation" (in Discussion) and "Holocene erosion intensity changed with fluctuation of mean annual precipitation, and these changes were similar in both sites". The paper thus fails to interpret their results in my opinion providing no lesson to learn for the reader although it would be very interesting to learn why the two sites behave differently (not similarly as the authors claim).

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Interactive comment on Solid Earth Discuss., doi:10.5194/se-2016-142, 2016.