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Interactive comment on "Short Communication: Earth is (mostly) flat, but mountains dominate global denudation: apportionment of the continental mass flux over millennial time scales, revisited" *by* J. K. Willenbring et al.

Anonymous Referee #2

Received and published: 9 February 2014

I focus this review on the scientific merits of the Willenbring et al submission to Earth Surface Dynamics. Fundamentally, I agree with many of the detailed criticisms (in terms of the substantive science) presented by Referee #1, and I will not repeat these here. My own view is that this paper is not tenable for publication in its present form, without a much more carefully and cautiously written manuscript – that is to say, many of the conclusions in the paper are stated with more confidence than I think is warranted, given the range of issues in the global calculation. The authors do show an interesting first-order correlation between slope and denudation rate, and I think their basic effort to think about what this means for global denudation is a valuable exercise. But for me to view this work as publishable, the analysis needs strengthening and the paper needs to be written clearly for what it is, not a predictor of global denudation per se, given the many other controlling variables, but an exploration of how slope distributions influence denudation at the global scale – which is, as far as I understand, the question the paper wants to tackle.

In my view, a major problem with this paper is that Willenbring et al appear not to have done any robust uncertainty propagation, despite the obvious scatter (3 orders of magnitude!) in their calibration dataset. Because of the non-normal distributions, propagating uncertainties will not be a trivial task, but ignoring uncertainties leaves the overall analysis with a glaring gap. I am somewhat dumbfounded that Willenbring et al have managed to get this far with this work while plotting a single line with no uncertainty bounds in plots such as Figs 3 & 4. This is critical to their conclusions: what are the confidence intervals on the values they report, e.g. for denudation fluxes and proportions? Put simply, the authors need to consider uncertainties properly for me to see this work as publishable. I am curious what such an uncertainty analysis will end up showing, and how it might influence their conclusions. I did see that in this paper the authors weight the calibration fit based on the variance of the data within each bin they use for averaging, but this weighting is different from actually propagating uncertainty into the global calculations.

I am also curious about whether the results reported in this paper would be different if the authors were able to use a higher-resolution DEM for their analysis. This paper talks about the effects of a mismatch between the resolution of a calibration dataset and the global dataset used for extrapolation. It is clear that these resolutions must match for the analysis to be done correctly, and that a resolution mis-match was a mistake in the Willenbring et al 2013 Geology paper. So, in their revision, the authors do both the calibration and the global extrapolation with 250m data. They argue (Section 2.1) that this removes bias from topographic resolution. But what if they did both calibration and extrapolation with 10m data? Or 90m? Since apparent slopes are higher when calculated with a higher resolution DEM, and since the relationship in Fig 2 is non-linear, it seems to me that resolution might make a difference. Perhaps not, but this question needs some consideration and analysis by the authors. I realize that they cannot do a global calculation at a higher resolution, because there apparently are not global-extent elevation datasets that suit this purpose, but presumably they can play with some synthetic data, or even with a subset of areas using 90m SRTM, and such tests would help to tell us whether their use of 250m data is robust, or not.

Where does all of this leave the manuscript from my perspective? The main concluding message from Willenbring et al is that understanding lowland erosion is important. Although I can see merit in this argument in general terms, I am not sure that the authors' global calculations make this case strongly. Until they have considered uncertainties properly and addressed the comments from Referee #1, it is difficult for me to judge whether the "lowland erosion" message is well justified in the context of this paper, and so whether this aspect of the manuscript makes it scientifically worthy of publication.

In any case, the submitted paper does address some of the flawed aspects of the analysis that were incorrect in Willenbring et al (2013, Geology), in a far more specific and constructively illuminating manner than the comment and reply published in Geology (though I agree with Referee #1, in that the comment and reply should be clearly cited and discussed in this paper). Showing and clearly discussing the mistakes that were made in the 2013 paper is instructive, and I think makes for a useful contribution to the scientific literature. Consequently I diverge from Referee #1 in this respect, in that I can see the revisions described in this manuscript as being a good reason for the eventual publication of this paper. Whether such an emphasis is appropriate for publication in Earth Surface Dynamics is an editorial decision. I can see the reasons to advocate a correction published in Geology, although I doubt this would provide the scope for discussion that makes the present paper valuable in methodological terms. In any case, I would not recommend that this submission be accepted for Earth Surface Dynamics

ESurfD

2, C8–C11, 2014

Interactive Comment

without considerable revision. My guidance would be to encourage the authors to think about and address the range of substantive comments provided in this review, and in the review from Referee #1 (realizing that they may need to put on a rational filter to see through the destructively venomous rhetoric of that review), and if they choose to do so, to produce a modified version of their work for resubmission and re-review.

Interactive comment on Earth Surf. Dynam. Discuss., 2, 1, 2014.

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2, C8–C11, 2014

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