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Earth Surface Dynamics Discussions

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 #1

Received and published: 13 February 2014

As noted in the previous comment, the first review was removed from consideration for not adhering to the obligations expected of reviewers. While the editors' decision was disputed, it was decided that removal was necessary in attempt to remove "bad reviewer practice."

Yet, readers of the these reviews may become confused because Reviewer #2 agrees with the (removed) technical discussion of Reviewer #1, but the content of this technical review is no where to be found. Thus, in an attempt to present the more useful portions of the first review while adhering to the reviewer obligations, I copied the original first

review but removed all materials that may be considered "bad." I hope these materials meet the high standards expected of reviewers at eSurf:

. . . As a bit of background, the authors (Willenbring et al.) are writing a paper acknowledging two fundamental errors in their 2013 paper "The Earth is (mostly) flat. . .", which had the novel and highly publicized, but . . . erroneous, conclusion that the flat areas of the Earth contributed ∼90% of the sediment to the sea. A Comment letter was written in reply by Warrick et al. that disputed these results on multiple grounds (including a list of problems that they had no space to elaborate upon). In Reply the Willenbring et al. authors acknowledged that they had misrepresented some results, erred in their calculations, and made faulty conclusions.

The paper under consideration here . . . revive(s) the original analyses by correcting for two . . . errors – (i) mismatch of the model and DEM and (ii) correction for log-transform biases. . . . (B)oth errors were clearly acknowledged by the authors in their Reply as quoted below:

"After our paper was published, we became aware that our mass flux estimates were affected by log transformation bias (Ferguson, 1986) and by the systematic underestimation of topographic gradients in the 1 km Global 30-Arc-Second Elevation Data Set (GTOPO30) (Kirchner and Ferrier, 2013). Correcting for these biases yields global mass fluxes that are broadly consistent with previously published estimates."

Thus, one can safely conclude that this issue has been dealt with already. . . . With the new submission considered here then, we must ask whether the authors sufficiently improve the results by addressing the most significant problems in their analyses. The answer is . . . "no." In short, although the authors addressed two sources of bias, they have not addressed the most fundamental problems that exist in their . . . global model. More specifically, the authors have avoided many . . . issues:

(i) The conclusion that the revised rates of global denudation (∼23 Gt/yr) match the rates of sediment flux to the sea (∼20 Mt/yr) should not be a result to champion. Rather this is a sign that the model is vastly underestimating denudation. . . . Using simple assumptions, the authors of the Comment letter suggested that total global denudation would be at least 40-50 Gt/yr. . . .

(ii) Why should a global denudation model based on one simple parameter (landscape slope) work? These issues are never addressed and are central to the method and validity of their results.

a. For example, why should the denudation within two similar flat regions – one arid, the other humid tropical – be the same?

b. Or, for example, compare these arid and humid systems with a boreal system. Why should denudation be the same across these diverse systems owing to equal slopes?

(These diverse areas – deserts, boreal and tropical lowlands – make up the majority of "flat" landscapes of the world, thus they must have appropriate denudation estimates for the results to be valid).

c. Further (and much more importantly) why should the denudation of similarly "steep" landscapes in different settings - for example, one with resistant rocks and negligible uplift (e.g, Rocky Mtns.), the other weak rocks with high rates of uplift (e.g, Taiwan) – be the same magnitude as predicted by the model? (Studies show vastly different rates).

(iii) The model used by the authors includes no information from regions of the world with the highest rates of denudation. This was . . . pointed out in the Comment letter, and the authors agreed in their Reply stating that this happened,

"because measurements of basin-scale erosion have been acquired for a variety of purposes other than creating an unbiased data set of global denudation, and because areas of supposedly high denudation were largely avoided for concerns relating to limitations of the technique".

. . . (Thus), the authors acknowledge that the global data set is biased on the basis of location and denudation rate. Further, they note that the high denudation regions have

been "avoided" and are thus . . . underrepresented in their . . . database and model. This bias is not corrected for . . .

[**added note: The authors defended their model in the Reply by stating that new denudation data from a Taiwan watershed,

". . . do not deviate from the exponential pattern (of the model)"

And yet, the maximum denudation rate allowed by the model is ∼1 mm/yr, which is half the rate measured in the Taiwan watershed. Couple this with the fact that the integrated slope of this watershed is likely less than the highest slopes in the model, one is left with the impression that the model would underestimate denudation from this Taiwanese watershed by at least 2-4 times. I am not sure if this pattern of underestimation can be extrapolated across the high sediment yield region of southeast Asia, but it should give the authors concern with their results.]

(iv) The authors . . . misinform the readers. They state that the new "methodological improvements" alter the results of the original study. For example one of the three "surprising results" of the original paper was that denudation had no slope dependency beneath 200 m/km. However, the Comment letter showed that standard statistical methods showed slope dependencies in the original data. In their Reply, the authors disagreed on the grounds that,

". . . at specific narrow ranges of slope values (the flat ones), the various second-order controls on total denudation obscure the first-order control of topography . . ."

Now, in the manuscript considered here, the authors have found new "methodological improvements" which show these slope dependencies in data from the flattest landscapes. . . .

(v) Side note. . . If the authors were correct that,

". . . at specific narrow ranges of slope values (the flat ones), the various second-order controls on total denudation obscure the first-order control of topography . . ."

then shouldn't their model deal with these "second-order controls," . . . (such as) . . . climate, parent material, vegetation, etc., . . . (which) . . . are highly variable in the world's flatlands . . .?

(vi) A good model of global denudation would be compared to known denudation and/or river sediment flux rates, especially on a regional or basin-wide basis, to evaluate goodness of fit. For example, the Comment letter included a simple analysis of the Amazon River basin that . . . showed . . . the original results were . . . (incorrect). We should expect . . . (the same thing) . . . from the Willenbring et al. efforts, . . . Focus should naturally fall upon a diversity of settings.

(vii) Roughly half of the Earth's surface, representing the flattest of all landscapes, has no denudation data from which to build a model. Thus, the model and its results rely heavily upon extrapolation beyond the range of measured data (i.e., extrapolation from higher sloped regions to the unmeasured "flat" ones). Couple this problem with the fact that these "flat" landscapes occur in a diversity of settings (as noted above). . . .

(viii) . . . presentation of the background and reanalysis (should) include reference to and citation of the Comment and Reply process of the original paper, especially because the authors agreed with so much of the Comment.

Thus, it is this reviewer's opinion that:

(a) . . . (T)he issues covered in this paper have already been addressed in the Comment and Reply process at the journal Geology. No improvements were made to the underlying model, and thus, nothing new is presented here. If the authors need to further correct problems with their original paper, they can send a Correction statement to the journal Geology.

(b) . . . the authors . . . are encouraged to develop a . . . model that handles the true complexity and diversity of Earth systems (beyond slope) and the underlying biases and gaps in the global database.

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