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Earth Surface Dynamics Discussions

Interactive comment on "Numerical modelling of Glacial Lake Outburst Floods using physically based dam-breach models" *by* M. J. Westoby et al.

Anonymous Referee #1

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General comment: This paper presents the results of routine work on the breach of a dam using a shallow water flow model combined with some rules for erosion estimated with the bed shear stress derived from the model. The paper is poorly written and the descriptions of the statistical methods are not rigorous and sometimes wrong. The logic surrounding a concept defined as 'equifinality' is inconsistent with the entire analysis. Given these deficiencies, the paper should not be published. Some specific details are as listed below, though this is not complete. Not all errors are listed below.

Specific comments: 1) The concept of equifinality, used on line 19, page 481 before it is defined on page 482, is defined by the authors as "identical or similar end states in an open system are achieved from many different combinations of input parameters, initial conditions, and model structures" (lines 1-3, page 482). The logic here is inconsistent

with the authors later application of maximum likelihood (section 4.3, page 486), since maximum likelihood is based on the concept that differences between input parameters in a model produce differences in the output of the model. If they do not, then the parameters have no relevance to the data being modeled. As defined, the existence of 'equifinality' means that, if you are varying the input parameters over a large enough range, then you still get the same output. Physically, this means that you have the wrong model. With equifinality, there is no basis for statistical inference or maximum likelihood.

2) Lines 3-4, page 483. Is this paper about equifinality, or the null space for this model? Or is it about a dam breach study?

3) Section 4.2: There are no details of what HR BREACH does or how it works. Why are journal subscribers paying to advertise a proprietary model that is marketed for personal gain? There is no way to assess the efficacy of this model with the data presented here, since the data are from a field example of a breached moraine dam. The data from a lab experiment that was done is mentioned, and these results do not support the model as I discuss below. What is the benefit to the scientific community of a proprietary model advertisement? Why not just buy an ad for the journal?

4) Model dimensionality is mentioned, and this is an issue here. There are 4 free parameters in equation 1, at least 7 in equation 2 (assuming the weights are measured), an additional 2 in equation 3, and there are at least 4 parameters (including bed friction) in the shallow water flow vector in the code used to analyze the dam breach. This adds up to seventeen free parameters to constrain with the dam breach data, which here consists solely of the initial dam height and depth, and the change in the thalweg elevation of the breach with time. A standard Bayesian analysis would show that the evidence from these thalweg and height measurements is spread thinly over the model space – in other words that the data constraints on their model are weak.

5) Discussion of Monte Carlo sampling and clustering, pg 487-488: Use of Latin hy-

percube sampling would accelerate the search and eliminate the 'clustering'. This discussion is confused, since without additional criteria, all random walks will produce a 'cluster' of samples in the area where the search is started before wandering away from the starting point. The number of samples required to be representative depends upon the problem being studied and where the search is started, so the number reported here is irrelevant to other dam breach studies.

6) Lines 23-28, page 488: "ensembles were assigned positive likelihood values in the range 0-1". Assigned? Likelihood is calculated by comparing the model output to model input data, which if equifinality applied would give you nothing. And for Bayesian inference, which the authors later claim on line 11 page 490, the likelihood is modified by prior information and normalized by the integral over all possible posterior distributions of input parameters, sometimes called the evidence integral. Bayes equation and standard Bayes updates (Mackay, 2003, Information Theory, Inference, and Learning Algorithms, Cambridge Univ Press) bears no resemblance to what is presented here.

7) Equation 4, page 489: With equifinality, there would be no relevance to this equation, or reason to use it.

8) Line 23, page 489: What linear likelihood function? Not equation 4!

9) Lines 24-28, page 489: A dam breach is a three dimensional process, as demonstrated in all dam breach experiments. What is the justification of the two dimensional approximation? How is this useful to the reader?

10) Equations 5 and 6: For a Bayesian update, equation 6 should be an integral, as should the numerator quantities in equation 5. As written these equations are undefined and useless to the reader.

11) Section 4.3.4, page 491: Again, this likelihood construction is inconsistent with the authors overriding concept of 'equifinality'.

12) Line 19, page 491: "the modeled spillway measured 0.5m wide and 0.5 m deep".

The results from this experiment, introduced and briefly described here, are summarized on page 496, after interleaving a discussion of the field example modelling. This field discussion is a non-sequitur and very confusing. As for the comparison with the lab experiment, the model produces discharges an order of magnitude larger than were observed for the same boundary value problem. This is very poor model performance, and raises questions about the viability of the field simulation. I fully expect that there would be good agreement with the inundation in a field comparison if there were good surface control, since this depends primarily upon continuity. However the timing of any simulation would be very different from what actually happened, making the application of this model useless for hazards estimation. In this analysis described here, the word 'behavioral' means 'realistic'. Results deemed not 'realistic' were thrown out. Why? This is the antithesis of Bayesian inference, and to even suggest that this study is a Bayesian analysis is grossly misleading. The truly valuable result is that only Manning's 'n' is significant. Given how the authors have set up their problem, with equation 1 governing erosion, this result is expected.

13) page 495: The severe oscillatory behavior the authors observed in the ADI method could be due to carbuncles cropping up in the solution, when a shock is aligned with the grid. An ADI method would amplify these.

14) line 1, page 501: Maximum likelihood is effectively identical to soft K means clustering (Mackay, 2003, Information theory, Inference, and Learning Algorithms, Cambridge Univ Press, chapter 22), and should be referenced and discussed here.

15) Line 17-22, page 504: Mannings 'n' coefficient of .02 - .029 corresponds to an effective median grain size of between 0.03 m and 0.2 m (Henderson, 1966, Open Channel Flow), which fits with the authors bed observations. These values are not low (as the authors state), except in the application of poor models where high values of bed resistance are needed to stabilize the model calculations. And, as the authors state, the Dig Tsho moraine is composed of gravel, cobble, and boulder-sized material, whose median grain sizes do fit their model result. This is the only significant result

presented in this paper.

Interactive comment on Earth Surf. Dynam. Discuss., 2, 477, 2014.