Response to Associate Editor comments

Westoby *et al.* '*Numerical modelling of Glacial Lake Outburst Floods using physically based dam-breach models'*

Associate Editor comments:

Westoby and co-authors address the important topic of dam breach scenario modelling, which is relevant to hazard assessment and understanding dam breach processes and flood hydraulics. Two referees have commented on the paper, and here I summarize my assessment based on the consistency of their reviews and my own reading of the paper. Both reviews raise major concerns about the framing of the article and the study design, including the appropriateness of the 2D model, the statistical methods, and logical inconsistencies between the emphasis on equifinality and use of Bayesian inference.
We thank the Associate Editor for their comments on the manuscript. We address the two reviewers' specific comments in detail in the following sections. However, we would like to take this opportunity to note that these two reviews offer markedly different reactions to our manuscript, with one suggesting direct rejection (reviewer 1), while the offer suggests publication following some moderate revisions (reviewer 2).

The basis for the stance of Reviewer 1 appears to rest on two central points. Firstly, a peculiar position over the use of a well-established numerical dam-breach model – HR BREACH. The principal objection seems to be that our research offers unwarranted advertising for this specific software package. Clearly, these comments could be taken as a principled position against any use of subscription software within the academic literature. However, similar objections are not raised with respect to our use of other commercial software within our approach – such as the 2D hydrodynamic model ISIS2D, and the geospatial toolbox, ArcGIS. Moreover, details of the model structure, parameterization and applications of HR-BREACH are clearly cited in our manuscript, there is no attempt to obfuscate the detail of this modelling scheme, and readers are offered peer-reviewed sources of further information to pursue. As such, we are left questioning whether such comments from an anonymous reviewer actually reflect personal a conflict of interest rather than a critical stance on methods we employ.

Their second concern lies in confusion over methods of uncertainty analysis we use to explore the range of the predictive outcomes generated by our model chain. In part, this reflects the need for greater clarify on our behalf, specifically over the definition of equifinality – from both a numerical and geomorphological perspective – and our use of the term 'maximum likelihood' – on page 497. This singular use was inappropriate, but was in no way intended to suggest that the numerical uncertainty method used (the well-established Generalized Likelihood Uncertainty Estimation method) involved the direct use of maximum likelihood estimation (MLE) methods. Nowhere else in the manuscript do we refer to MLE and provide consistent reference to methods used which we contend cannot be confused with MLE methods. The reviewer appears to take a literal view on this one reference to ML methods and fails to comment on the broader philosophy of model uncertainty that we discuss and at no point addresses the specific approach of Generalized Likelihood Uncertainty Estimation.

The second reviewer also recommends that we clarify the precise interpretation of equifinality, but unlike R1, finds no internal contradiction or errors in our approach. We agree that the theme of equifinality needs to be developed more fully at the outset and would welcome the opportunity to revise our manuscript accordingly.

Both referees also note that the choice of model (HR BREACH) is not justified. I agree with both referees that the paper is not always clearly written, and that discussion of the relevant literature on statistics and uncertainties is insufficient.
We believe that our decision to use HR BREACH over other models is justified implicitly in our description of its improved physical basis in section 4.2, which sets it apart from existing dam-breach codes that are typically based on simpler, empirically- or analytically-derived equations. We will make the decision for our choice of dam-breach model explicit in the revised manuscript. We agree with the Associate Editor and the reviewers, especially reviewer 2, that more could be done to make the research more accessible to non-specialists. The manuscript will be revised with this in mind, and will place particular emphasis on conveying the significance of the results in terms of its application to predictive GLOF modelling.

Referee #1 recommends rejection, and although Referee #2 recommends "moderate revisions and further clarification," Referee #2 echoes many of the same concerns of Referee #1 and does not make any positive statements about the scientific contribution of the paper except to say that if the 2D approximation was shown to be appropriate for this 3D modelling problem and if the approach was demonstrated to be applicable to other situations, then the paper would be of value.
We agree that the paper does not satisfactorily consider the application of the approach to other situations at present. This will be addressed in a revised discussion section, as there are a number of ways in which we believe our results are significant and could be demonstrated to be useful for informing the development of an adapted workflow that would be appropriate for predictive outburst flood modelling. However, we note that the predictive application of the workflow is beyond the scope of this study.

Unfortunately, Referee #1 argues convincingly that the 2D approximation and other aspects of the modelling approach are flawed and not applicable to other situations. The authors are free to address the referee comments in the public discussion at this time. According to journal policy, all comments have to be answered in the public discussion before a revised manuscript can be considered for final publication. However, given that the referee concerns about both the study design and its presentation are in my view major and that the scientific contribution of the work is unclear, I suggest the authors may instead wish to withdraw the paper at this stage. I would encourage a new submission of a similar study if the fundamental problems pointed out by the referees can be addressed and the scientific value of the work is clearly articulated.
We agree that many of the reviewer's concerns could be construed as major. We agree that the clarity of the paper needs improvement, and this will be addressed. Many of the problems that are highlighted by the reviewers are not, in our view, fundamental, and our reasons for this are discussed below. We hope that our responses to their comments and a significantly revised manuscript will satisfactorily address these concerns.