# Response to Reviewer 2

Review of manuscript: *The importance of erosion for debris flow runout modelling from applications to the Swiss Alps*

The authors incorporate an empirical model for debris flow entrainment within the RAMMS debris flow model to investigate the importance of erosion and changes in debris flow volume on debris flow runout. While the model presented in this study is location-specific, the results are more generally useful for understanding a problem that has important implications for hazard assessment. This contribution is appropriate for *Natural Hazards and Earth System Sciences* and should be of general interest to its readers. However, I had a lot difficulty following key portions of the manuscript and think additional detail in those areas is necessary (see comments below).

Reviewer: Anonymous Referee

Response to review by F. Frank and co-authors:

- Answers to and corrections proposed based on comments from this reviewer
- Answers to and corrections proposed based on comments from both reviewers
- **Some major changes proposed are bold.**

We thank the reviewer for the comprehensive and detailed review of our paper. The reviewer has pointed out many legitimate minor issues. We mostly agreed with his suggestions for corrections, which are described below. He also identified some moderate issues – e.g. especially about the potential changes in bed topography between the modeling of the first and second surge which were not in the original manuscript. **Because Reviewer 1 also identified this issue, please see that "Response to Reviewer" document for our proposed solution. Basically, we agree that this is an issue and we re-did our calculations. The changes do not change the major conclusions of the paper, however.**

## General Comments:

It should be made clear at an early point in the manuscript whether or not this is a study that focuses on changes in bed topography (over the course of multiple events or one event) or changes in debris flow volume caused by entrainment throughout the course of a single event. I had difficulty following the modeling results section because I wasn't sure exactly what simulations were being performed and how they were being initialized (changes in initial topography if multiple debris flows were being modeled, assumptions about flow volume change).

We addressed this below at several locations within the "specific comments".

## Specific Comments:

Title: Reading the paper, I was confused about whether or not this study was going to focus on how changes to the bed influenced debris flow runout or focus on how changes in debris flow volume influenced runout. From reading the abstract and title, I thought the main focus of the article would be on understanding how temporal changes to the channel bed impacted debris flow runout. My suggestion would be to use the term 'entrainment' rather than 'erosion' in the title if it is really changes to the flow volume that are the focus here.

Yes, the study indeed addresses the importance of entrainment and bulking on runout modelling i.e. flow properties and runout pattern. Nevertheless, the potential erosion depths and erosion rates recorded in the field are the data basis for evaluating the model. Although, we don't account for live channel-bed changes within the simulation itself, the erosion depths are also a result of the entrainment modelling. **Due to these comments, we re-did our erosion modeling: We now subtract the erosion of the first surge(s) modeled (07/23/2010) from the basic DTM (April 2010) to account for the bed level changes prior to the second surge (08/12/2010). This has been done for all combinations of friction coefficents ξ and μ. The results (Fig. 4 and 7; available at the end of this file) are slightly different and require slight revisions of the text in the result and discussion section. However, the main conclusions remain unchanged.**

Despite that new approach, we agree that we should use entrainment (and bulking) when talking about the influence on flow properties and runout pattern. We propose to generally change the terms from erosion to 'entrainment (model)' through-out the entire paper. On the other hand, we keep the term 'erosion' where we refer to the erosion field data (Fig. 3 and Eq. 6) and the calculated erosion depths and volumes (Fig. 4, 7).

Abstract - Line 4-5: The terms 'debris flow' and 'granular flow' often refer to very different phenomena so it is confusing to say that the 'RAMMS debris-flow model' solves the equations for 'granular flow.' Do these equations approximate equations commonly used for debris flow modeling or are you using the terms 'granular flow' and 'debris flow' interchangably?

The statement is that the 'RAMMS debris-flow model, which solves the 2D shallow-water equations for granular flows'. In other words, we use the term 'granular flows' to clarify that the RAMMS debris-flow model uses the Voellmy friction relation (parameters $\xi$ and $\mu$) to modify the 2D shallow-water equations to be used for the more granular type of flow, the 'debris flow'. So, the terms 'granular flows' and 'debris flows' are not used as completely 'interchangeable' – but to distinguish from the original intent of the application of the 2D shallow-water equations for fluid flows.

Abstract Line 5-6: What relationship? I'm guessing this is a relationship between maximum shear stress and erosion at your study site, but stating it this way makes it sound like there is a more general relationship between these variables.

To emphasize and clarify that we use an empirical relationship based on field data, we added "empirical" in this sentence:

*"In the erosion model, an empirical relationship between calculated maximum shear stress and measured channel-bed erosion is used to determine the maximum potential erosion depth."*

Abstract Line 11-12: I had trouble following some of the results/discussion section (see comments below), but If I'm interpreting the results correctly then it seems like this study addresses the problem of debris flow entrainment and changes in debris flow volume (bulking). The model presented here can't address how changes in channel bed erosion influence runout (at least not for a single flow) since dynamic changes in topography are not accounted for.

**We hope that the changes in the manuscript described above will address these concerns of the reviewer.**

Abstract Line 13: Same as above comment: 'channel bed erosion.' This makes it sound like dynamic changes in bed topography are important, but they are not included here.

**We addressed this point already in the comments above.**

**Short statement**

Just to clarify and to offer you quicker readability: This paragraph actually starts at location 2381 – Line 28; so minus 1 page – at least compared to the latest version of the discussion paper we have.

> 2382 - Line 28 : 'Recent debris flow research...' Consider moving this paragraph to start before the paragraph beginning with 'Runout models are increasingly....'

We agree. Hence, we moved this paragraph to the location you've proposed.

> 2382 - Line 22: This paragraph could potentially be removed. I wasn't sure how it fit in with the rest of the introduction. Are these difficulties the reason for not using a process-based debris flow entrainment model?

This paragraph is important to the paper and the authors think that it best fits in the Introduction section. It describes the conclusions made in the literature regarding laboratory erosion vs. field erosion studies. These studies mostly concluded that debris flow erosion process observed in the field can hardly be reproduced on the laboratory scale. That is the main reason why we are focusing on a field data-based instead of a laboratory-based approach for developing a debris flow entrainment model. To help clarify this, <u>we propose adding one sentence at the end to summarize our decision to use a field data-based empirical approach:</u> "*Therefore we focus on a field data-based entrainment approach in our study*."

> 2383 - Line 10-15: Previous models have included terms for debris flow mass change and investigated the effect of this term on flow runout. This study is different from those because of the strong connection to field data. Could emphasize the novel aspects of this study here.

We agree that the "strong connection to field data" is important in our approach. We think this is now provided by the sentence above and some additional text at 2383 – Line 12.

> 2383 - Line 26: What is 'steep'? Please clarify.

We propose to add *"(> 30°)"* which represents the steeper slope angles in the catchment of the Illgraben.

**Short statement**

From now on there seems to be a consistent shift of +1 page in the locations you depicted – at least compared to the latest version of the discussion paper we have. Just to clarify that.

2383 - Line 6: Was there any significant flooding during this time? Were significant water-dominated runoff events associated with the debris flow events?

Yes, there are some floods or debris floods at the Illgraben. However, the erosion sensor study (Berger et al., 2011) showed that such subsequent water-dominated runoff didn't cause significant bed erosion, which is mainly caused by debris flows. Additionally, the sediment transfer cycles described by Bennet et al. (2012, 2013) can be mostly attributed to a large but unknown number of small debris flows (maximum volumes up to a few 10 to 100 $m^3$) which normally don't reach the the Illgraben fan. Observations at the Illgraben (unpublished) suggest that significant erosion only takes place after relatively large debris flows, and that deposition is mostly due to small debris flows. However this is already accounted for largely because Schürch et al. (2011) measured the bed elevation quite often – he did not report significant fluvial erosion in his paper.

2385 - Line 11: Is 'granular flow' the appropriate term?

Please also see the answer above. We use a fairly standard description for the application of the shallow water equations to debris flows which is similar to the description in the RAMMS handbook and publications related to other debris-flow models (but not Flo2D which is limited to muddy flows). These equations are also used for flowing snow avalanches (which are also clods or granules of snow) and for dry rock avalanches, so in our opinion the term "granular" is justified here. In more detail, the equations include the active-passive longitudinal straining (earth pressure) formulations, which is apparently necessary for describing the frictional behavior of granular flows. If the reviewer has a better suggestion, we would be grateful to read about it and incorporate it, if appropriate.

2385 - Line 15: It is confusing to use subscripts to denote derivatives as well as direction (for flow velocity).

We prefer this method because it is consistent with literature we cite, written by model programmers (Christen et al., 2010).

2386 - Line 11: '...dominates deceleration behavior.' This contradicts the next sentence. Do you mean it dominates when the flow is moving slowly?

Yes, your observation is correct and we have changed the text accordingly.

2386 - Line 27: 'The RAMMS debris flow model....' This was already stated and could be removed.

We agree and therefore we moved the two references (e.g. Scheuner et al., 2011; e.g. Hussin et al., 2012) to the first paragraph in Section 4 and we will delete this sentence at page 2387 – Line 27.

2387 - Line 23: 'erosion algorithm is defined by'. Consider 'defined using' or 'a function of'

We agree and use "defined using" now.

2388 Line 18 - 2389 Line 16: Most of this sounds like it would fit better in the discussion section.

The first paragraph of that part discusses the critical shear stress and why we choose a different value (1 kPa) for the model from the assessment by Schürch et al. (2011) (2-3 kPa). Additionally (in the second paragraph "*The probability of erosion…*") the general reliability of the shear stress-erosion approach is shortly discussed by evaluating its potential limitations and impacts of the simplifications made. That brief discussion is necessary to explain the reasoning behind the construction of the debris-flow entrainment model (section 4.2) and to explain what the model implemented is doing. We therefore suggest leaving that part in the debris-flow erosion model chapter (section 4.2).

2389 - Line 24: 'at a specified rate'

We agree with this suggestion.

2390 - Line 9: 'main goal is to investigate the importance of erosion....' Over a single event? Multiple events? I've assumed up until this point that it would be for one event but if channel bed erosion isn't dynamic and only happens after the event then how is this possible? I assume (based on Lines 15-18 on page 2395) that changes in flow volume occur dynamically and maybe this is how erosion/entrainment become important in this model for a single event, but there is no source term for flow material in the continuity equation.

We reformulated that sentence based on a similar comment by Reviewer M. Mergili, thereby changing the focus of this reasoning. It now says:

"*However because we lack of additional erosion data from the model application study site – which could justify further adjustments of the erosion model coefficients for the Spreitgraben case – we leave these values at their default settings.*"

2391 - Line 1: I don't think this an adequate test for the model. Is it possible to predict values outside of that range given the limits imposed on erosion rate? Does the model predict negligible erosion for the smaller debris flow events as was observed?

Your comment raised a valuable point. It should be called "erosion range" (as it is named and shown in Table 2, units of meters) instead of "erosion rates" (2392 – Line 2). Your second question needs additional explanation. There was a problem of a potential misunderstanding regarding the maximum erosion rate shown in Eq. 6, which has now been corrected (see the comments to the other reviewer). The erosion model actually uses an erosion rate of 0.025 ms$^{-1}$ , and the amount of erosion (erosion range of 0.04 - 0.28 m shown in Table 2) describes the amount of erosion. Yes, in our experience the model predicts negligible erosion depths for small debris flows (because the predicted shear stress is small), the work herein indicates that the model produces plausible erosion depths for shear stresses well outside of the range of the data used to develop the erosion model.

2392 - Line 19: Are these choices (4 points, chosen time intervals) arbitrary? How do these choices for the initial condition influence the model-predicted erosion?

The shape and duration of the 4-point hydrograph is a simplified estimation based on field observations (Geotest, 2010). We added some additional text to clarify this issue: "*…as observed at Spreitgraben (Geotest, 2010) and Illgraben (Berger et al. 2011)…*"
The max. discharge $Q_p$ (determined based Eq. 7) at the same channel location (same cross-sectional area) is proportional to the max. flow height (> max. shear stress > the maximum potential erosion depth $e_m$).

2393 - Line 13: 'diameter' should be 'deviation'

We agree with this suggestion.

2393 - Line 1 and Line 25: One set of parameters is the best for the 'most realistic erosion result' and another set of parameters gives the 'most realistic spatial erosion pattern.' Please clarify.

The formulation at Line 1 should be called '*the most realistic model result incorporating erosion modeling*'. It refers to what is the best-fit regarding the front travel time which was observed to be ≈ 2 minutes and 30 seconds between upper fan and gallery of the main road – not the erosion pattern." Line 25 is an appropriate description, therefore we leave it that way. So it should be more clear and consistent, now.

> 2394 - Section 5.2: There were 2 flows mentioned earlier (2392 Line 17), but this section makes it sound like only one flow was modeled. Is this the case? Figures 4,5,6, and 7 also make it look as if only 1 flow was modeled. This is very confusing since changes in bed topography don't occur dynamically and changes in debris flow volume may/may not occur dynamically (I think they might but there is no source term for this in the flow equations). Please clarify exactly what event/events are being modeled and if changes in bed topography are accounted for between events (if multiple events were modeled).

The other reviewer identified some similar issues with this section, so **we re-ran the model (for all friction-coefficient scenarios)** to describe the approach more clearly:

*"The first event (23 July 2010, Table 3) was modeled on the digital elevation model available (April 2010). An updated elevation model for the second debris flow (10 August 2010, Table 3) modeling was created based on the erosion modeled for each parameter combination in the first model run (23 July 2010, Table 3) to account for the bed level changes caused by the first debris flow. Finally, the modeled erosion values of both separate model runs were summarized to get the total modeled erosion depths (Fig. 4a and 7), the cumulative erosion volumes (Fig. 4b) and the erosion pattern (Fig. 4 and 7) for the events of 2010."*

Further clarification is provided in the figure descriptions where we indicate if the two surges of the year 2010 (Figures 4 and 7) or the one surge of 08/12/2010 (Figures 5 and 6) were modeled.

> 2395 - Line 19: Does this mean that entrained material is added dynamically to the flow? If this is the case, it should be stated earlier. If changes in flow volume are not simulated, then how reasonable is it to start with a smaller flow volume initially (relative to the final flow volume)? Please Clarify.

Yes, the entrained material is added dynamically to the flow. We think this is clearly stated by this sentence in Section 4.2 (2396 - Line 22):

*As a model run progresses, the potential erosion depth (as a function of shear stress) is used to set the maximum erosion depth for each grid point in the model, and the sediment in the channel bed is entrained at the specified rate until the potential erosion depth is reached.*

Therefore, it is also necessary to start with a smaller flow volume initially. To ensure that it is quite clear we add an reference to the figure at the end of this sentence (2396- Line 20):

*The resulting maximum flow heights as well as the hydrograph using the erosion modelling approach are similar to the no-erosion modelling and consistent with observed peak flow heights of about 5 to 7 m (Fig. 5 and 6c).*

> 2395 - Line 27: The term 'hydrograph' is used many times before this so if it is defined in the text it should come directly after the first time it is used.

We moved this explanation of the hydrograph to its first mention on page 2391, in the third paragraph of section 4.3.

2397 - Line 20: 'assess determine'

We choose 'assess' and delete 'determine'.

2398 Line 1: But it does require a pre-defined maximum erosion rate.

Yes, that's correct. It does require a pre-defined erosion rate (see comments and changes above referring to Eq. 6). But <u>the statement itself is still valid</u>. The approach conducted by Hussin et al. (2012) e.g. is only feasible for conducting a <u>back-calculation of a documented erosive event</u> by pre-defining layers of erosion. Our empirical erosion model does not require input of the spatial variability of erosion depths.

2398 Line 14 - 2399 Line 2: This material belongs in the results section.

We agree and we moved this paragraph at the end of the results section "5.2 Erosion model application results".

2401 - Line 6: What was the most important factor in this case? Was it channel bed erosion or debris flow volume growth?

Bank collapse was likely the most important factor in this case because it preferentially removes sediment from the channel margins, where the shear stress predicted by the model is relatively low in comparison to the shear stress at the channel centerline. The sediment in the channel due to bank collapse is most likely not as tightly packed as the sediment in the channel bed, so it seems reasonable to assume that it might be easier to erode that sediment. However this is a secondary detail and it is only summarized here from the earlier discussion. We are not sure what the reviewer means with the second question because in this paper, channel bed erosion contributes to the growth of the debris flow, which is the basis of the model. In this sentence we summarize an earlier discussion and we do not actually know if bank collapse is truly the most important factor because we do not have any direct measurements of that process. In any case, we think that the clarifications provided through the revision of the manuscript will help to minimize the potential for mis-understanding.

**Figures appendix (new figures: 4/7 and corrected figure 3 are suggested):**

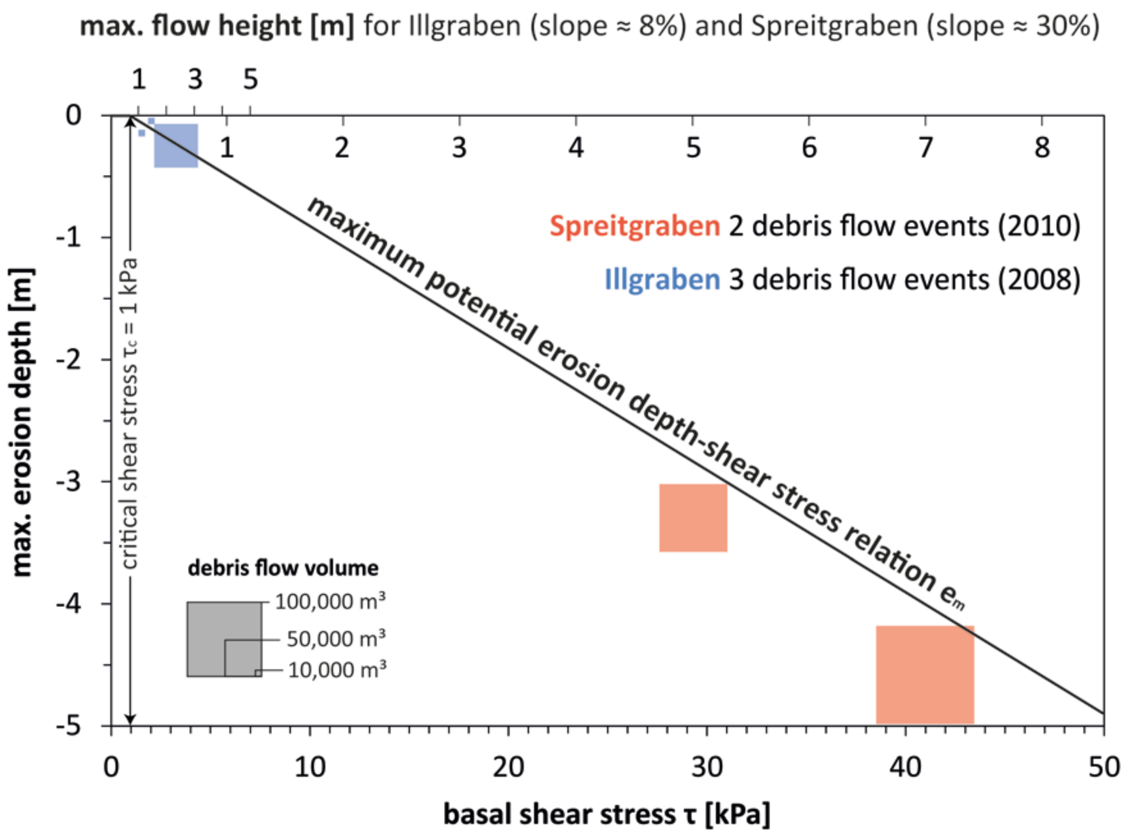


**Figure 3.** A linear relationship for maximum erosion depth as a function of basal shear stress forms the basis of the model. The size of the boxes is proportional to the estimated event volume at the Illgraben (3 debris-flow events, Berger et al., 2010) and Spreitgraben (2 events, Geotest AG, 2010). The upper axis indicates the flow height at the Illgraben (8% channel slope) with the numbers above the axis, and at the Spreitgraben (30% slope) with the flow depth values placed below the axis; the corresponding shear stresses (Eq. 5) are plotted at the bottom of the figure.

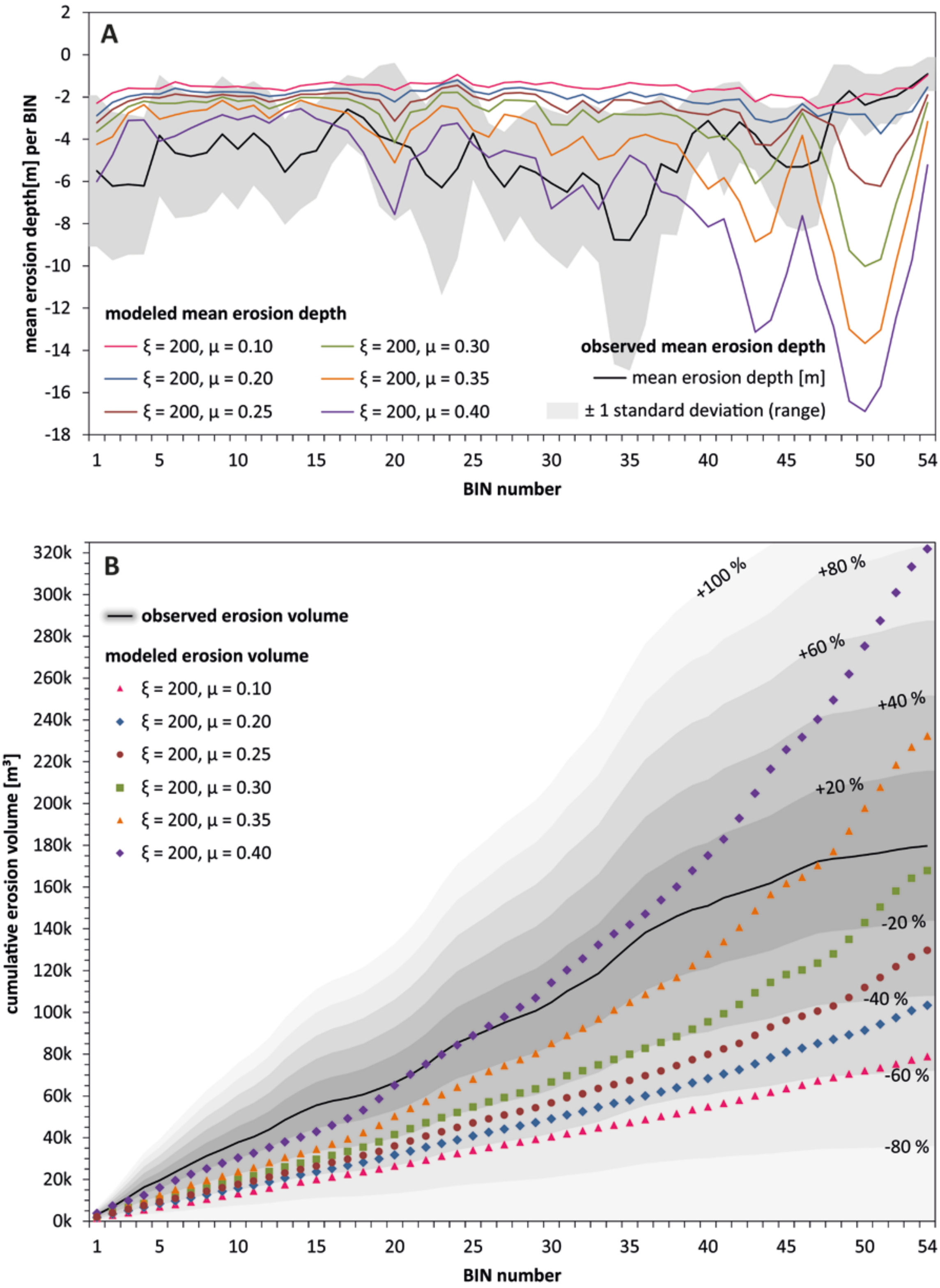


**Figure 4A**. Range of modelled compared to observed mean erosion depths for the two events of 2010 (Table 3). **B**. Modelled cumulative erosion volumes compared to observed cumulative erosion volumes using the bin-based systematic analysis. The gray shaded areas depict the ranges of percental volume difference compared with the observed erosion volume.

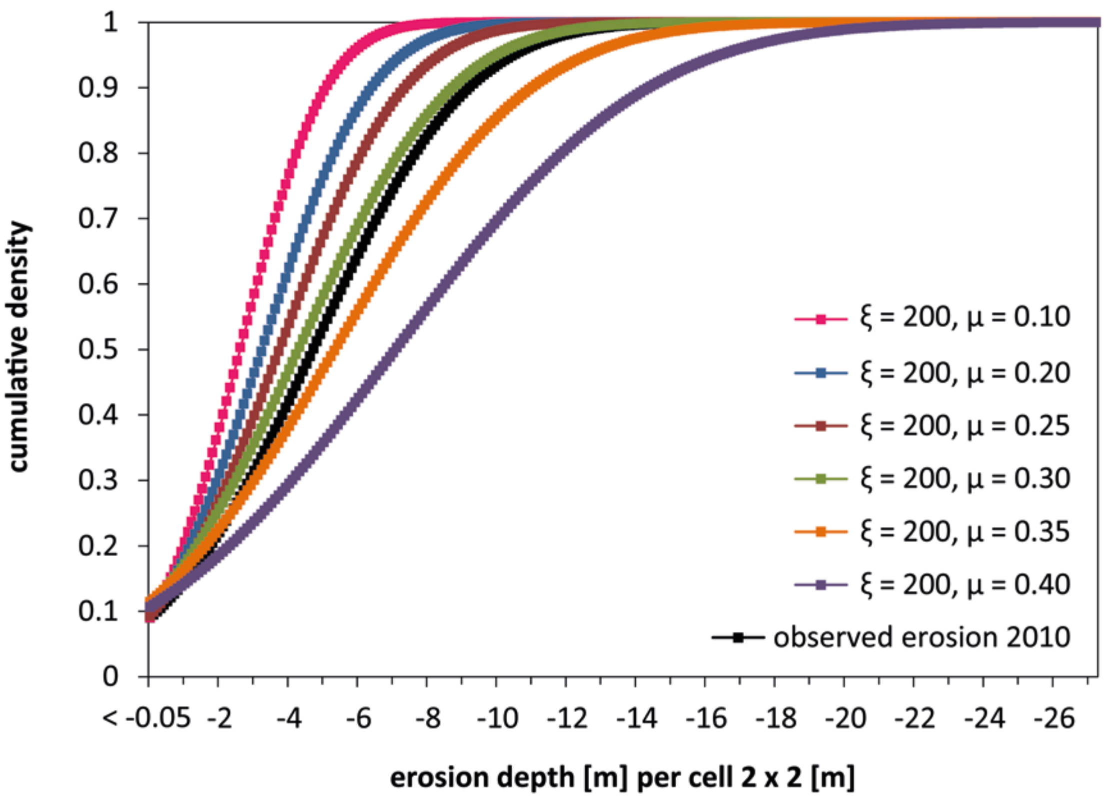


**Figure 7.** Cumulative density of modelled and observed erosion depths for the two events of 2010 (Table 3) based on a grid resolution of 2 m by 2 m in bins 1 to 54, for a total of 12,621 cells, using the DTMs of April 2010 and August 2010 **to calculate the observed erosion 2010. To model the second event (08/12/2010), the DTM of April 2010 was updated based on the erosion modeled in the first event (07/23/2010).** Erosion is represented on the x-axis (< -0.05 m) while no erosion cells and cells with deposition are not included.