Authors' Response to Reviews of

"Controls on the formation of potential landslide dams and dammed lakes in the Austrian Alps"

Anne-Laure Argentin et al.
Natural Hazards and Earth System Sciences, https://doi.org/10.5194/nhess-2020-326

RC: ***Reviewers' Comment***, AR: Authors' Response

1. Anonymous Referee #1, Received and published: 23 February 2021

RC: ***Anne-Laure Argentin et al. entitled "Controls on the formation of potential landslide dams and dammed lakes in the Austrian Alps", present a process-based modeling approach to envision susceptibility of landslide damming and lake formation by individually simulating the process chain from the initiation probability of landslides, land-slide runout, river obstruction and damming. The concept and the methods employed by the authors are thought-provoking and progressive and this manuscript would be significant for the engineering geological and natural hazard community. The idea to conceptualize the landslide dam hazard chain through a process-based modeling is appreciable. Nevertheless, the study may still need some additional elements or factors that can be considered, warranting a minor to moderate revisions to the manuscript paper to be accepted and this reviewer see the following suggestions shall be followed.***

AR: We thank the reviewer for the thorough review of our manuscript and the pertinent comments formulated. We are very glad that the reviewer finds our article a significant addition to the scientific community and are confident that we can address the issues raised in a revised version of the article. In this response we reply line by line to the suggestions made:

1.1. Major comment #1: Modeling the landslide release areas

RC: ***The authors adopted a slope-based criterion following Hergarten et al. 2012 to determine the probability of landslide release areas. The authors do mention the reason for their choice "The approach proposed by Hergarten (2012) still seems to be the only model in this context which is able to predict the observed power-law distribution of rockfall and rockslide volumes". However, the performance of the model in a terrain with lithological variations need to be questioned. Different rocks would have different thresholds with regards to slope angle and stability.***

AR: The first method used from Hergarten (2012) is an empirical method, which is not physically process-based. Although it seems intuitive that different rocks would have different thresholds with regards to slope angle and stability, as formulated for soil-mantled slopes and bedrock slopes (Montgomery, 2001), it has seldom been tested with bedrock slopes (Goudie, 2016) and does not explain some "over-steepened" slopes (Fernández et al., 2008). Furthermore, rock mass strength is a variable that is controlled by a large set of parameters (e.g. lithology, structural discontinuities etc.), and, to our knowledge, no thresholds are available based on rock mass strength for our study area.

RC: ***In addition, rockfalls possess strong sensitivity towards discontinuities. I would request the authors to perform a validation of their analysis of landslide probability.***

RC: ***Is it possible to compare the landslide probability estimated by the Hergarten et al. 2012 to actual events of landslides within different geological units of the study area? It would be nice to see the performance of the model for past cases at first and then use it to predict the future.***

AR: The reviewer is right to mention the influence of lithology and discontinuities on landslide triggering. Our method uses a statistical approach that only holds for extended regions, does not take into account lithological variations and structural discontinuities, and thus cannot be applied to reproduce case studies. Moreover, we assume that the different stability thresholds lead to an equafinality of results. Taking the same thresholds for the whole mountain range allows for a simple model. The topography is the main control of landsliding here. Furthermore, no temporal constraints are applied to our model, and we do not investigate any triggering return periods or landslide frequency. We thus call "density" and not "frequency" the number of landslide simulated per km^2. This density is closer to a landsliding potential, with high densities where not much landsliding has already occurred (i.e. in steep terrains).

We validated our model by visually comparing the landslides created with a landslide database over the Austrian Alps. However, to provide a better overview of the difference between landslide densities per tectonic unit, created a histogram of the landsliding frequency in each lithological unit for both our model and the database:

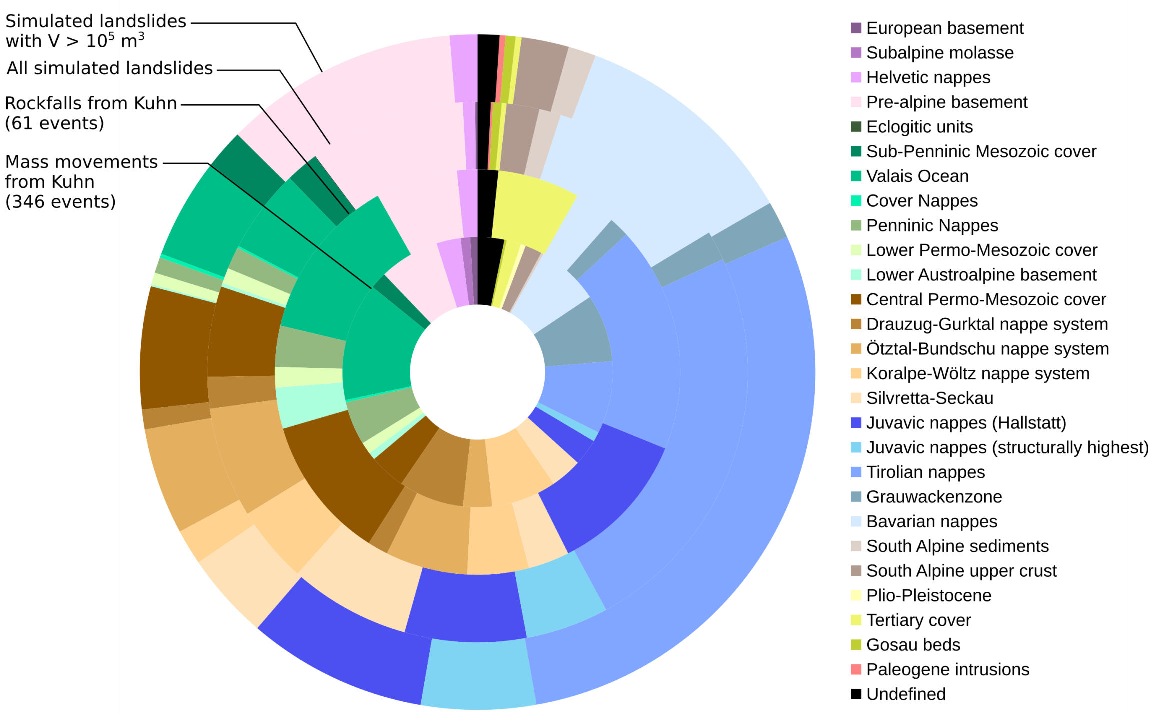

Figure 1: Comparison of landslide density depending on the Alpine tectonic unit. Each ring corresponds to a different dataset and the arcs length represents the landslide frequency for every tectonic unit.

The Tirolian nappes show a higher density of landslides in our model than in the datasets from Kuhn (visited 2020.07.27) (Fig. 1), which is logical since the time range investigated by the database is restrained. We notice that 1) the (computationally-driven) decision to choose landslides with $V > 10^5$ m^3 exacerbates differences with landslide database densities, 2) landslide densities are overestimated in steep terrains (e.g. calcareous nappes).

However, this model recreates the typical power-law scaling of landslides (Fig. 2, Tebbens, 2020), and changing the thresholds do not change the size distribution of landslides by much (Hergarten, 2012).

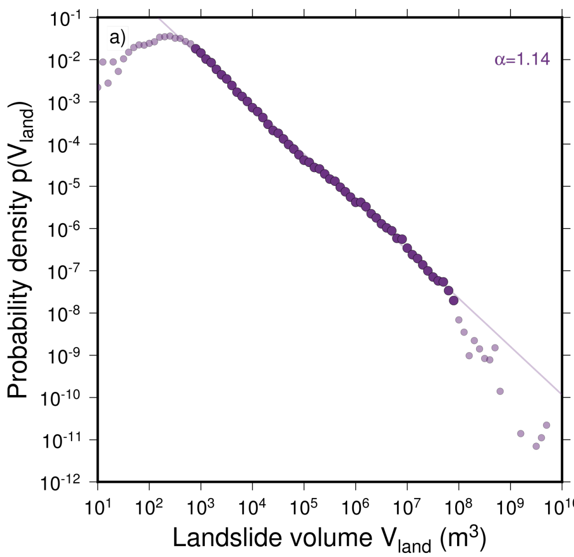

Figure 2: The landslide scaling of the landslides simulated in the model.

We will clarify the aforementioned points in the discussion. We will expand our paragraph to fully cover the limits of the method the reviewer signaled.

RC: ***In addition, the overall work, stressing on the importance of the chain of hazards from landslide occurrence, runout, damming and lake formation seems a bit incomplete. The authors do quantify the probability of failure of each potentially unstable rock mass but, not provide a probabilistic assessment of the conditions that might trigger such instabilities (e.g., a return period of a triggering rainfall, a return period of a triggering earthquake). I suggest the authors to refer Fan et al. (2019) and add a line of discussion regarding the limitations of the landslide simulations and the validity of the assumption adopted in this study.***

AR: We will add a paragraph on the absence of a timescale in our model, which prevents us from discussing the return period of triggering events. As a result, we talk about landslide "densities" for the number of landslides per area. This will be investigated in following work.

1.2. Major comment #2: Landslide runout simulation and #3: Estimation of landslide dam geometry

RC: ***The authors adopted Voellmy rheology to model the landslide runout with variables*** $\xi = 150 m.s^{-2}$ ***and*** $\mu = 0.12$***. It is common to use such constant values for different lithologies within a large area of a***

numerical model. However, the same need to be justified. In general, these values are obtained through back calculation of landslide runouts using known case examples. Regarding the calibration of the models, and in particular of Gerris, the authors need to discuss the choice of their parameters. Also, the authors should discuss why they think the parameters should be the same for all the subsequent events all over the study area (e.g., why should the acceleration remain the same? and the friction?).

RC: ***It is appreciable that the author attempted to simulate the landslide dam geometry at a larger scale. Their explanation of the calculation of landslide dam volume and geometry seems simple cutting down different realities but still acceptable considering the scale of the numerical simulation. However, the limitation of the approach used in this study need to be clearly mentioned. Please refer to Hungr (2011) for more insights on a comparative study on the use of landslide runout models to predict landslide dam geometries.***

AR: Two very good points, indeed. We chose to discuss and take into account the comments #2 and #3 together since they are strongly linked.

Rheology determination is important and especially tricky for landslide runout modeling. Rheology matters because its choice controls the landslide dam geometry (Hungr, 2011), as shown by the consistent impact our Voellmy parameters have on dam height (Supplementary Fig. A1). However, rheology determination is not an easy task, and usually needs the back analysis of a case study. Since rheology is linked to lithology, different landslides will present different runout rheologies. Rheology can also vary spatially in a single landslide event, when two different rock types are involved, or temporally, when a change in physical conditions happens during the landslide runout (Hungr and Evans, 2004).

However, we have no way of knowing how the rheology might vary over the study area, let alone temporally or spatially in a single case study. Using the same rheology over the whole Austrian Alps enables an easier analysis of the results based on the topography. If we were to add too much complexity, we would not be able to infer which effect controls which result.

Furthermore, landslide dam geometry influences the lake geometry in a complex manner. We show that although the Voellmy parameters have a consistent impact on dam height, this does not translate to consistent changes in lake depth or lake volume (Supplementary Fig. A1). Thus we assume that different rheologies would not necessarily lead to a statistically significant change in lake volumes.

For those two reasons, we decided to use the same rheological coefficients for all events in our study. We assume that all landslides are similar and exhibit the same rheology. Furthermore, using homogeneous parameters ensures we can easily compare the resulting dam geometries. Thus, we relied on the back analysis of landslide runout from Sanne (2015) on the Val Pola event to set our Voellmy rheology parameters.

We will add a paragraph on rheology and the use of landslide runouts to predict landslide dam geometries.

1.3. Major specific comment #4

RC: ***Landslide dam characterization: In addition to the height ratio-based characterisation of the simulated landslide dams, is it possible to identify the type of dams according to Costa and Schuster (1988); Fan et al. (2020); Hermanns et al.(2011)? The authors do mention the type of landslide dams in lines 150 using simplified planform geometry. There are also other predominant types of landslide damming based on morphology though not specific to rockfall/rockslide formed landslide dams.***

AR: In the current state of our work, a planform characterization of the simulated landslide dams would be difficult: it implies an automatic recognition of shapes in planform view. Our dams sometimes exhibit complex shapes, with landslide deposits spread across valley flanks and valley floors. The planform geometries would require we make the distinction between deposits that sedimented on the valley flanks and those that actively contribute to valley damming. Furthermore, some landslides separated in two valleys during their runouts, and thus form two distinct deposit areas. This distinction would require another non-trivial algorithm. The reviewer comment is however a very good idea for further work.

RC: ***I would like to see some discussion regarding the preciseness of the geomorphometric parameters identified and used in this study (Table 1).***

AR: We will add a few sentences which explains the difference between preciseness and uncertainty for our geomorphometric parameter, and thus what are its limits and how accurate it is deemed.

1.4. Major specific comment #5

RC: ***Dam formation and stability indices: The authors mentioned that their model cannot predict the stability of landslide dams. It is okay that the authors predict only the occurrence of landslide dam and lake formation and not the dam-breach or breach-induced flooding. However, the most significant part of this study on a hazard point of view is also to envision the relative stability of longevity of a landslide dam in the future if such events occur. On a true sense, the dam-breach and the outburst flood caused is the most threatening hazard than the landslide and damming itself. In a similar study by Fan et al. (2019), the actual dam-breach and flooding was simulated for different scenarios and the same has been compared with different empirical stability indices. I suggest the authors to refer and add some lines of expressions.***

AR: We will add a few lines on the necessity to model the dam-breach and simulate the flood to assess the hazards coming from landslide-dam failures.

RC: ***I also suggest the authors to add more lines of discussion regarding the performance of stability indices. The authors do mention BI,II, DBI, Is, Ia and HDSI are inconclusive in the Eastern Alps. This also depends on the availability of data as mentioned in a previous study by Fan et al. (2020).***

AR: We will reformulate this part. The reviewer's concerns about the availability of data (Fan et al., 2020) apply to the study of Dufresne et al. (2018), which found the BI, II, DBI, Is, Ia and HDSI inconclusive in the Eastern Alps based on a handful of case studies. However, our study presents a total of 1057 events, and those events do not present consistent stability assessments across indices. Although we cannot infer from this study which indice and which thresholds are best suited for the Eastern Alps, we can conclude that they do not agree with each other.

1.5. Minor comment #1

RC: ***The authors performed a well throughout study and I appreciate their efforts. I feel the English language presentation need improvement though myself neither a language expert or a native English speaker.***

AR: We noted the reviewer comment and will ask for English feedback from colleagues.

1.6. Minor comment #2

RC: ***Since the introduction part I felt many sentences are not connected to form a nice story. The authors shall imagine the geological processes in sequence and start from the conditions of landsliding and go on write about the events until for the formation of landslide dam and lakes. This will help the readers to understand the authors are focusing on an important large-scale geological hazard chain.***

AR: We will re-write more clearly the article and highlight its storyline by relying on transitional words and coherence methods. We will reformulate the article based on recommendations from a science-based guide to writing.

References cited by the reviewer:

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2. Anonymous Referee #2, Received and published: 8 March 2021

RC: ***I have revised the article "Controls on the formation of potential landslide dams and dammed lakes in the Austrian Alps" submitted by Anne-Laure Argentin and co-authors. The article discusses modeled landslides, landslide dams and dammed lakes, introducing an approach that combines a probabilistic approach to determine landslide release areas and a fluid dynamic model to compute runouts. The article is interesting but requires revisions before publications. A short introduction of the study area and available data should be added in chapter 2. It's not very clear what type of failures you model (debris flow, slide). Several comments are reported throughout the text. The manuscript should be revised by an English speaking person before publication.***

AR: We thank the reviewer for the thorough review of our manuscript and the pertinent comments formulated. We are pleased that the reviewer found our article interesting. Some of the raised issues originate from a lack of clarity on our part and we are convinced we can overcome those problems with some restructuring and a better wording. We discuss here the revisions suggested by the reviewer line by line.

2.1. Comments

RC: ***l. 2: the entire territory of the Austrian Alps?***

AR: Yes.

RC: ***l. 3: it's not very clear which type of landslides? & l. 4: debris flow? & l. 72: Describe better which type of landslides you are considering in your modelling. Rockfall and rockslide? Debris flow? & l. 78-79: (About slope thresholds) this is quite different from rockfall or rockslide & l. 96: (About runout simulation) Now you are not considering the failures as rock fall (line 74). Correct? & l. 114-115: (About runout simulation) You have selected "The approach proposed by Hergarten (2012) still seems to be the only model in this context which is able to predict the observed power-law distribution of rockfall and rockslide volumes" and now rockfall are not evaluated. Explain better.***

AR: Good questions. We do not specify the type of landslide, as this model (triggering + runout simulation) can be applied to any landslide type with high volume. The triggering model from Hergarten has been defined to reproduce the statistic distribution of rockfalls (Hergarten, 2012). Since slides also follow the same distributions (Brunetti et al., 2009) we can use the same algorithm. Runout simulations are indeed type-specific (Hungr et al., 2001), but most of rockfalls with $V > 10^5\ \mathrm{m}^3$ would have a long runout and be termed "rock avalanche" (Dorren, 2003). Rock avalanche runouts can be simulated accurately if the correct rheology is used (e.g. Val Pola Sanne, 2015). We chose to use the same rheology for all events to keep a simple dataset.

We will discuss this landslide type question more in detail in the discussion.

RC: ***l. 7: "small landslides damming large lakes" is the opposite of "lake volume increases linearly with landslide volume" that you say in the same sentence & l. 9-11: what do you mean with more efficient?***

AR: We meant to define what we call "efficient damming", but we will reformulate the text since it is not clear.

RC: ***l. 55: Add a short description of the study area and the available data, including the inventories you mention in the text.***

AR: We will add the requested information to the manuscript.

RC: ***l. 62: which is the resoluton?***

AR: The ASTER DEM has a 1 arc second resolution. We will add this information to the article.

RC: ***l. 65-66: this semtence here has no real meaning***

AR: We will reformulate the sentence.

RC: ***l. 68: moving window***

AR: We will change the wording.

RC: ***l. 73-74: About "The approach proposed by Hergarten (2012) still seems to be the only model in this context which is able to predict the observed power-law distribution of rockfall and rockslide volumes." -> Can you justify better this choice? Can you justify better the context of your application that justify this choice? & l. 88-89: (On the dependency of slope thresholds on lithology) In your analysis you have assumed that this statement it's acceptable. Is this reasonable in the test area?***

AR: Yes, we will add a paragraph at the end of the discussion to talk about the use and limitations of this method. The context of this study is the requirement to use a computationally efficient algorithm. We will reformulate this part.

Very good question. The model we use for this simulation is empirical and reproduces the size frequency distribution of landslides at the mountain range scale (Fig. 2). We answered this question in more detail in the reply to the Major comment #1 of Referee #1.

RC: ***l. 75: explain better***

AR: We will add some explanation.

RC: ***l. 79: landslide area or volume?***

AR: Well, it's both at the same time. The source area is expanding when the slope of the neighboring pixels is not stable, thus also increasing the landslide volume.

RC: ***l. 83: how do you evaluate the thickness?***

AR: With the algorithm mentioned above, we remove material until a stable slope condition is reached. The height of removed material per pixel is the evaluated thickness of the landslide at the pixel location.

RC: ***l. 86: Removed typo.***

AR: Thank you.

RC: ***l. 92: (About memory issues) which is the extent of your study area?***

AR: The study area is the whole of Austria, and the DEM weights more than 7.4 Go. Some computations do not support such a massive input area.

RC: ***l. 93: this means that the tiles are overlapping? what is the dimension of the buffer?***

AR: Exactly, the tiles are overlapping because we need space to simulate the landslide runouts. However, those overlapping areas are only taken once into account for triggering landslides. The buffer dimension is 10.25 km.

RC: ***l. 107-108: It's not clear how did you model the lake volume starting from the runout of the landslides. Cases shown in fig. A1, are real cases?***

AR: I see, I guess we mention this too early on, since we have not yet explained how we obtained the lake volumes. We will restructure the subsection.

No, the cases shown in A1 are simulated cases. We will modify the figure caption.

RC: ***l. 123-124: not clear***

AR: Just a technical detail. We need to cut the DEMs again in smaller parts for the landslide simulations. We will delete this sentence which seems to distract the readers from the process.

RC: ***l. 161: can you explain better?***

AR: We will reformulate the sentence.

RC: ***l. 162: In most of the indices you have Volumes. Explain how did you compute them***

AR: The landslide volumes are computed using the algorithm from Hergarten as described previously: the total of all removed material on top of each pixel in the source area constitutes the landslide volume. The lake volume is computed by filling the DEM (with simulated landslide) with a common GIS algorithm and making the difference with the topography before filling. This is explained in the previous sections.

RC: ***l. 165 & 168: explain***

AR: A_b is the catchment area upstream of the landslide dam (l. 138 & l. 141). We will modify the text to remind the reader of the meaning of A_b so they do not have to scroll back up.

RC: ***l. 191: 1) how did you selected the seed pixel? 2) this the procedure proposed by Hargarten. Correct?***

AR: 1) The seed pixel is randomly chosen. 2) Yes, exactly.

RC: ***l. 192: did you select all the Austrial Alps?***

AR: Yes, we launched the simulation on all the Austrian Alps.

RC: ***l. 193: 1) Is this an available dataset? if this is the case it should be described before (in the material) 2) can you add this data and its use in the workflow ?***

AR: No this is not an available dataset, this is the result of the simulation on the Austrian Alps. To avoid any confusion, we will add a priming sentence to the result sections, and we will modify the title of the first section.

RC: ***l. 197-198: not clear***

AR: We will reformulate.

RC: ***l. 198-199: high slope angle?***

AR: Yes, definitely. If the local slope is high, the landslide density will be higher. We will reformulate.

RC: ***l. 200: what do you mean?***

AR: We will change the formulation.

RC: ***l. 202-203 & 204-205: it is very difficult to find the formations in the map becouse colours are too similar***

AR: We will try to improve the readability of the map by numbering the tectonic units on the map and in the legend.

RC: ***l. 206: add reference***

AR: Ok, we will add a reference.

RC: ***l. 211: Did you mapped fluvial and glacial valleys in the entire Austria? In table 3 what is the area? How did you compute the area of density? km2 of what? glacial and fluvial valleys? I would expect larger volumes in glacial valley but smaller density.***

AR: No, we used the extent of the Last Glacial Maximum to deduce the extent of glacially imprinted landscapes. The area in Table 3 is the glacially imprinted area (glacial) and non-glacially imprinted one (fluvial). We computed the densities by dividing the number of landslides happening in the glacially imprinted landscapes (resp. fluvially imprinted) by the glacial area (resp. fluvial area). The unit is thus in "landslide.km^{-2}".

Yes, very good point. We indeed have larger landslide volumes in glacially imprinted areas. However, glacial valleys are prone to high landslide frequencies following glacial recession (Hartmeyer et al., 2020). Nonetheless, the "density" we are talking about in this section is not a "frequency", since we do not include any temporal constraints in our model. We will add an explanatory paragraph in the discussion about this.

RC: ***l. 263-264: Can you add in the figure the real data as done before?***

AR: Yes, we will try to add the real data to the Figure 6 as done for the Figure 5.

RC: ***l. 263-264: Have you tried to plot in different graphs fluvial and glacial landslide dams? & l. 291: Have you tried to plot separately glacial and fluvial data?***

AR: Yes, we plotted in Supplementary D1 the fluvial and glacial landslide dams separately.

RC: ***l. 264: can you explain why there are many undefined?***

AR: Yes, a range was said to be "Undefined" when the events found in it exhibited variable behaviors (Partial obstruction, Complete Obstruction, Stable, Unstable). The "Undefined" ranges are particularly wide since no consistent trend was found in it (Korup, 2004; Tacconi Stefanelli et al., 2016). We will change the formulation.

RC: ***l. 296: English is not always very clear: it should be revised by an English speaking person***

AR: We will ask one of our colleagues to revise the manuscript. We will also modify the manuscript to make the storyline easier to follow.

RC: ***l. 311: ?***

AR: "in" We will correct our typographical error.

RC: ***l. 313 & 314: not clear***

AR: We will restructure this paragraph.

RC: ***l. 320: I do not see the valley geometry in the discussion below (chapter 4.1)***

AR: We are indeed not using any valley geometry metrics in this section, but we think that the valley topography partly explains the relations highlighted here. We will change the wording of this sentence.

RC: ***l. 346: It's a little boit confusing what is is real and what has been modelled.***

AR: All this section discusses simulation results. We will add some precision to the subsection titles.

RC: ***l. 348: Do you mean Alpine regions?***

AR: Yes, we mean the Alpine regions, more precisely the Alpine tectonic units. We will change "regions" to "tectonic units".

RC: ***l. 371: where is the Ab in the graph?***

AR: A_b and $V_{landslide}$ are not directly plotted in the graph. We compute $V_{p\ lake}$ from A_b and $V_{landslide}$ and then plot $V_{p\ lake}$ against V_{lake}. We will modify the graph caption and the equation and corresponding paragraph to avoid any confusion.

RC: ***l. 387: which one?***

AR: Climate and tectonics are two of those big differences between mountain ranges. Climate and tectonics include among other parameters precipitation rates and earthquakes. Those parameters are two of the main triggers for landsliding. Precipitation rates, in particular, influence the rheology of the landslides (Chen and Lee, 2003; Wang and Sassa, 2003) and thus the geometry of the formed landslide dams (Hungr, 2011). Thus rain-triggered landslides exhibit a different shape than earthquake-triggered ones (Chen et al., 2014). We will reformulate the sentence.

RC: ***l.392-393: do you agree?***

AR: Yes, we agree with Korup and think uncertainties in the estimation of dam heights are partly responsible for differences between field and model results. We will modify the sentence.

RC: ***l. 417: there not much evaluations on the drainage system***

AR: We will modify "drainage system" to "drainage area".

RC: ***l. 426 & 427: write the complete name***

AR: Ok, we will add "Impoundment Index" and "Dimensionless Blockage Index" to the abbreviations.

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