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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Potential flood volume of Himalayan glacial lakes" *by* K. Fujita et al.

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We thank the reviewer for his valuable comments. Our replies are denoted by a header [Reply].

- In line with an earlier review I would like to emphasise that the authors do not present a risk assessment. The risk would include the downstream effect. This risk is affected by the potential flood volume but depends on many other factors like the characteristics of the downstream area and the affected land and infrastructure. Please revise the terminology and change "risk" to "hazard", "outburst probability" or similar based on the context throughout the manuscript. In addition, the authors present important aspects of a hazard assessment but not a full one (e.g. as correctly mentioned in the introduction many factors are important to consider if a glacial lake can be dangerous not only the steepness of the lake front and the lake volume). This should also be more

emphasized in the text.

[Reply] We have already declared that our approach did not fully cover the risk assessment in the text. Potential flood volume we evaluated is an initial condition for the following risk assessment. In another word, no risk assessment is possible without the PFV. Although previous studies have used lake water volume for such kind of assessment, our PFV is more realistic one. In this regards, we suppose that our approach is an initial part of risk assessment. Although we are not fully convinced by the reviewers' assertions, in which our approach is not the risk assessment, anyhow, we replaced "risk" by "hazard" or "outburst probability" in the revised manuscript. "risk" in Introduction remained because of its general overview.

- The quality of the utilized DEM is of high importance for the results. Both ASTER and Hexagon data have limitations for the generation of DEMs. The authors should also use the same resolution for the comparison of the DEMs or deriving parameters from them as the resolution has an impact. Please provide an accuracy assessment and show an example of the generated DEMs.

[Reply] We will add descriptions about DEM accuracy of ASTER and Hexagon KH-9 in the revised manuscript though we did not evaluate them in this study. The accuracy of ASTER DEM around the Himalayan glaciers was evaluated by comparing with DGPS data (Fujita et al., 2008; Nuimura et al., 2012). This was already described in the manuscript. The accuracy of Hexagon DEM was thought to be equivalent to that of Corona DEM, which was generated in the same procedure and evaluated by comparing with ALOS DEM (Lamsal et al., 2011; Sawagaki et al., 2012). Resolutions of ASTER and Hexagon KH-9 are 15 m and 10 m, respectively. This difference will not affect the results because we do not superimpose nor compare those DEMs but obtain distribution of depression angle from the lakeshore on the individual images.

- I appreciate that the authors include an error assessment. However, it should include all aspects of the data generation (e.g. lake mapping accuracy, uncertainty in the

volume estimate, co-registration error) and should be part of the methods section as the uncertainty has impacts on the results.

[Reply] We will evaluate error due to the lake delineation and the area-depth relation. The lake delineation error will be estimated by an approach which we have used for the Imja lake (peripheral length multiplied by half of resolution (7.5 m)) (Fujita et al., 2009). We will use variability of mean depth measurements from the approximation curve shown as "mean estimate" in Fig. 2 (one sigma = 7.35 m). Then we can evaluate error of PFV. We have no "measurement errors" in the individual observations. We do not perform co-registration for the images.

- The supplementary material presents interesting information which could be included in the main text or presented as an appendix as NHESS has no length restrictions.

[Reply] Fig. S1 (18 high PFV lakes), Table S1 (ICIMOD's PDGLs), Table S2 (list of 49 high PFV lakes) may be moved to the main manuscript, and Table S3 (sensitivity of number of lakes along the PFV at the different threshold angles) can be merged with Table 2 (number of lakes along the PFV at the threshold of 10 degree). Although we will add a table for ASTER data used in the analysis according to the comment by H. Frey, this may be remained in the supplement because of too many lines. Figure S2 (development of SLA at the Nagma lake) may also be remained in the supplement because of its repeated appearance. We will consult with the editor which figures and tables are moved to the main text or remained in the supplement.

Abstract The abstract presents mainly the methodology. However, some more results should be included as well.

[Reply] We suppose that PFV is the sufficient result. We are afraid that additional information makes the main conclusion unclear.

1. Introduction You may consider the following additional references here and also for the discussion: Huggel et al. (2002), Bolch et al. (2011), Mergili and Schneider (2011).

You may think about to include information about the location of the dams with respect to the probability of the existence permafrost (cf. Bolch et al. 2011) if existing. There is at least some information available for the Khumbu area (Fukui et al. 2007). Mergili and Schneider (2011) and Bolch et al. (2011) use also declassified imagery (Corona imagery) for the information about the earlier lake areas.

[Reply] We will cite these references. We will mention that distribution and degradation of permafrost have to be considered in further assessment with respect to dam stability. However, we will not incorporate permafrost distribution into our analysis because little information is available covering the same region we evaluated. We do not discuss lake expansion in this study so that we will cite the studies not for the utilization of declassified imageries, but for the risk assessment.

2. Methods Formula 1: The authors should clearly state that obtaining the depth from the surface area only is subject to high uncertainties and consider these uncertainties in the results and discussion.

[Reply] This will be stated in the error evaluation replied above.

P 19, l. 24ff: Generation of DEMs based on Hexagon data is not straight forward and requires the collection of many GCPs and postprocessing. This is a very tedious and time consuming task. Please provide also some more information also about the co-registration procedure of the DEMs and Hexagon images.

[Reply] We identified 4-7 GCPs around each target lake from the Google Earth. Because we did not superimpose the ASTER and Hexagon DEMs, no co-registration was made. We will add the descriptions.

P. 20, l. 7ff: Identification of lakes using the NDWI is in general suitable. However, the authors should clearly state the limitations and provide the information how they dealt with them: e.g. turbid lakes, frozen lakes, lakes in shadow, misclassified shadowed areas. How did you identify supraglacial lakes? Visually or automatically using a glacier

inventory (if so which one?)

[Reply] We referred the NDWI but manually delineated the lake boundary by considering the issues the reviewer commented above. We will add more detailed methodology about this.

3. Results The results are quite short. The authors have a huge and very interesting data set and I think more results can be obtained from them, e.g. distribution of lakes, more specific analysis with respect to the location of the lakes with potentially large flood volume (elevation, climatic patterns, debris-covered glaciers etc.). Not all must be done but some more information would increase the significance of the study.

[Reply] At this moment, we do not find more significant relationship among the PFVs against altitude or geographic locations (as climatic regime) than that we mentioned in the original manuscript (high PFV lakes are found in east) (see Fig. 1 in this reply). We are afraid that our main target (PFV) becomes equivocal among additional information such as distributions of area or altitude. Anyhow, we will make some more analysis or attempt combinations of multiple indices including PFV, and then determine whether put more information or not.

5. Discussion Please include a more in depth discussion about the strengths and limitations of the approach.

[Reply] We suppose that we can reply to this comment by moving the first paragraph of conclusions to discussion part. We suppose that advantages and limitations were already described in the original manuscript (but unclear due to bad structure). We are afraid that too much negative descriptions may give the readers impression that the PFV is a just trial index and useless for the practical assessment.

I also suggest a more in depth discussion with respect to the literature. Imja is a nice example (The authors may also consider also Watanabe et al. 2009) but not the only one and I am sure that the authors are well aware about the other investigated lakes

as partly mentioned in the “Conclusions” section but should be discussed here.

[Reply] Watanabe et al. (2009) is a problematic paper. They concluded that the Imja lake was much safer than the first author previously asserted (Watanabe et al., 1994, 1995) because the lake water level has continuously lowered. However, our study showed that little change of the lake water level between 1992 and 2003 (Fujita et al., 2009). We do not deny the lowered lake water level. Lamsal et al. (2011) evidently showed the lowered lake water level for the last four decades by comparing two DEMs generated from Corona and ALOS, for which co-registration and error evaluation were carefully performed. On the other hand, Watanabe et al. (2009) did not show how the water levels were measured at all while ours declared the benchmark positions, instruments and its measurement errors. What instruments were used? Where are the benchmarks located? These are totally unclear in Watanabe et al. (2009). We do not want to use the space to argue methodology and reliability of the other study. Therefore, we do not cite this paper though the changing water level may alter the SLA and PFV. The first paragraph of conclusions will be moved to discussion according to the earlier comments from the other reviewers.

6. Conclusions The first lines and several other parts are not a conclusion of the study but should rather be moved to the discussion. The first lines are very important statements and here the authors mention implicitly that risk (including the downstream effect) and hazard is not the same. Countermeasures are required if a lake has a high risk but not necessarily if it is highly dangerous.

[Reply] The first paragraph of conclusions will be moved to discussion according to the earlier comments from the other reviewers. We will emphasize that our analysis is useful as "the first screening" for the succeeding comprehensive risk assessments.

Figures Fig. 1: The caption is quite long and describes the methodology and should be stated in the main text. From my point a view a caption should not repeat what was stated in the main text Fig. 2: I would also suggest to shorten the caption and

to provide the important information about the constraints of the lake depth only in the main text.

[Reply] We prepare the figure captions for the readers to understand what we did even if they saw only the figures. We will add the explanations in the methods but will keep the original captions.

Fig. 7a: You may think about presenting lake area in log scale.

[Reply] We will change the x-axis (see Fig. 2 in this reply).

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 15, 2013.

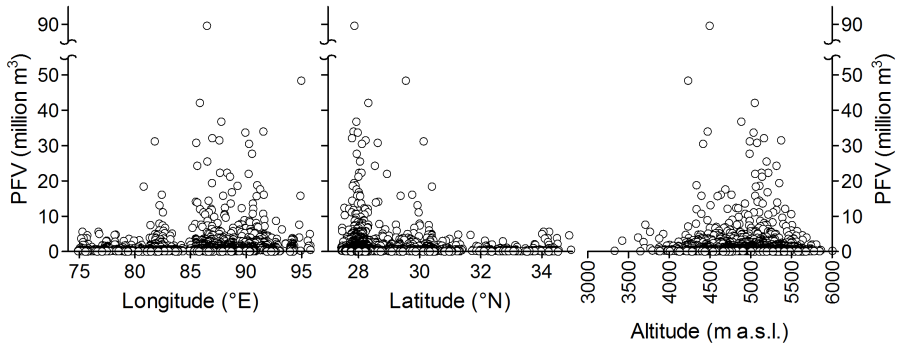

Fig. 1. PFV against lake locations

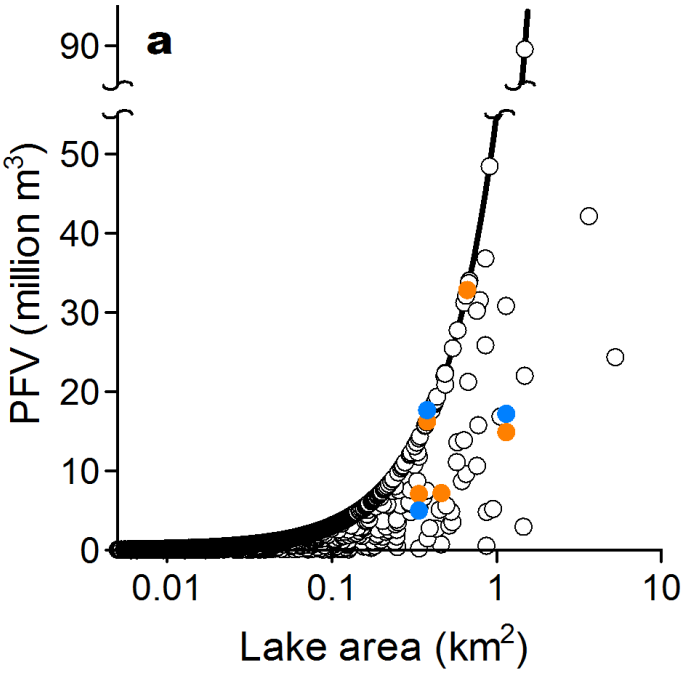

Fig. 2. PFV against lake area in log scale