Nat. Hazards Earth Syst. Sci. Discuss.,
doi:10.5194/nhess-2016-317-SC1, 2017

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Anomalous winter snow amplified earthquake induced disaster of the 2015 Langtang avalanche in Nepal" *by* Koji Fujita et al.

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Received and published: 17 January 2017

The submitted manuscript by Fujita et al. documents the extent of the earthquake-induced Alpine mass motion (snow/ice avalanches and rock falls), which destroyed and covered the village of Langtang (Nepal) as a result of the 2015 Gorkha earthquake. Combining differential GPS measurements after the rock fall/avalanche events with digital elevation models derived from satellite and airborne (helicopter and UAV) measurements, the authors estimate a volume of the debris cover and its evolution over time. This constitutes one of the two central messages, which this study provides. For the second message, the authors use meteorological data and further imagery to suggest source constraints, namely that the primary avalanche damage was triggered by glacier collapse and unusual pre-event winter precipitation magnified the extent of the destruction.

The paper is clearly written and presents a valuable assessment of a tragic natural disaster. The provided geographic constraint on debris cover seems somewhat detached from the discussion of the avalanche source. However, since the Gorkha earthquake and its damage are still relatively recent events, it should be OK to present such different aspects in a single paper.

My main criticism deals with several assumptions, which the authors seem to make but do not fully justify. Most of all, did the increased pre-event winter snowfall really make a significant difference? On Page 4, Line 13 the authors state that for the region of concern, only 20-30% of the annual totals in snow accumulation are attributed to winter snow fall. Does this mean that similar or much larger avalanches are typically to be expected after the monsoon season? Do the authors assume that winter snow cover is particularly vulnerable to earthquake shaking?

Additional explanations and/or specifications of assumption are necessary for the subtracting of DEM's, the discussion of the hanging glacier collapse and calculation of avalanche source volume. Revisions should not require additional analysis. However, in its current state, the manuscript is not entirely clear on how the conclusions are reached. Result presentation and figure quality could also be improved, but these are likely more minor points.

MAIN REMARKS

Extreme snow fall: As mentioned above, it is not clear why the winter snowfall, even though it was particularly strong, amplified the avalanche devastation significantly. This seems to imply that either snow avalanche risk in in spring 2015 was larger than during the rest of the year and/or the earthquake was more likely to happen after winter than after summer (when most of the snowfall occurs). Neither of these points is argued for. As a first step, the time series in Figures 9a-c should be extended to a full year to show if solid winter precipitation dominated solid summer precipitation. Next, a discussion about earthquake-triggered avalanches would be helpful. Podolskiy et al. (2010 in

Journal of Glaciology) conclude that during earthquake shaking, avalanche failure occurs primarily along weak layers or the base of snow samples. Is such triggering more likely during certain seasons? Alternatively, if the trigger was entirely due to glacier collapse, is this more likely during winter? Finally, it would help to see an estimate of how much avalanche-prone snow volume is usually available for these kinds of events, excluding extreme snowfalls such as documented in the manuscript.

DEM Generation: This discussion inevitably involves lots of numbers. Unfortunately, the authors sometimes round numbers, and sometimes they do not. Also, standard deviation is not always indicated. This makes it very hard and tedious for the reader to flip between Tables 2,3 and the text. I suggest more consistency. Furthermore, I could not follow the reasoning behind the "initial deposit" (Line 15 on Page 5). I thought the off-debris area was deposit-free? I guess that fresh debris deposits would have been noticed during the GPS survey. Is it reasonable to believe the large bias given the similarly large standard deviation? I am not an expert on DEM comparison, but I expect that some statistical argument is needed here.

Projectile motion of boulder: I did not understand how the initial launch speed of the boulder relates to the avalanche. The authors seem to argue that the air blast (I suggest clearly defining this term) ahead of the avalanche launched the boulder. However, on Line 19 of Page 6 they compare boulder speed with "speed of muddy flow". In any case, there should be some explanation on moment transfer between avalanche debris or air and boulder. Is there really a simple explanation why the different speeds should be the same?

Glacier trigger: The authors provide evidence for a glacier collapse that could have triggered the avalanche. They should specify if this is the only collapse that occurred in the time window of interest or not. In addition, one of the conclusions of this study seems to be that glacier ice did not exceed a few percent of the avalanche volume. In contrast, on Line 38 of Page 6 they say that their ice cliff samples may not be representative. Generally, I suggest being clearer on the glacier collapse trigger, because

this is a poorly understood topic in glaciology. To my understanding, it is not generally clear how glacier and underlying bedrock failure play a part in large-scale avalanches.

Presentation of geographic and timing information: The manuscript would benefit from more altitude information. All maps/photos should include altitudes of mountain peaks, villages and perhaps other sites such as glacier tongues. Currently, it is hard to put the glacier collapse in context without knowing its altitude with respect to the rest of the avalanche source area. Moreover, what are the approximate glacier volumes? In section 4.2, the authors state that the collapsed glacier cliff has a similar volume to all of Yala Glacier. This seems very small. I furthermore suggest specifying the earthquake date whenever it is referenced to and the dates of the SPOT 6/7 images. The reader would appreciate a simple figure indicating the timeline of mass motion events and measurements.

Precipitation analysis: Even though it takes a prominent part in the study conclusions, no details of the statistical analysis showing that the pre-event winter snow fall was outstandingly large, are given.

SPECIFIC COMMENTS

How many dGPS points were taken?

Abstract, last sentence: This statement sounds as if the 2015 Ghorka earthquake disaster can be mostly attributed to the avalanche destruction. Please rewrite.

Page 4, Line 3: Is Table 1 correct reference?

Page 4, Lines 6-7: Quantify "high elevation".

Page 4, Lines 32-33: When were the precipitation ratios observed?

Some minor typos are present.

Sometimes, the name "Yala" is followed by "Glacier", sometimes it is not. Is this a place or only a glacier? Please clarify.

Page 5, Line 1: "we corrected traditions": unclear. Quantify "huge earthquake" (magnitude and epicenter).

Section 3.1: Please make sure that the numbers exactly agree with the tables. "same magnitude" should be specified. It is sometimes not clear, which surface and initial deposit is being discussed.

Page 5, Line 26: Is "significant" appropriate in a statistical sense?

Page 5, Line 37: "7 May" –> 10 May? Also, which area is assumed in the calculation of the number given in this sentence?

Page 5, Lines 39-40: "hard to melt" is weak statement. It would help to see a number on melt reduction with respect to uncovered ice.

Page 6, Line 12: "weight" –> mass. Why is this information needed? I assume that the trajectory analysis neglected air resistance and therefore did not require the projectile mass.

Page 6, Lines 26-27: Some information on tree age measurements is needed.

Page 6, Line 28: Quantify "large".

Page 6, Line 36: "1 m" large rocks are substantial. Where does this threshold come from?

Page 6, Line 38: Rewrite "condition as of ice cliff exposure".

Page 7, Lines 19-21: Details/numbers of this calculation would be helpful.

Page 7, Lines 23-24: Please show this temperature drop.

Page 7, Line 34: I suggest deleting "obviously".

Page 8, Points 1 and 2: Here it is not clear what the estimates on ice volume refer to. Glacier ice? If so, this seems contradictory, because the authors had previously said how uncertain the estimates of glacier ice volumes are.

Page 8, Equation 1: It seems that this assumes temperate ice. For such high altitudes I expect cold ice. Does this make a substantial difference in view of available melt energy?

Page 8, Lines 23-24: This sentences needs a reference or justification.

Page 8, Line 25: How was the energy released?

Page 8, Line 29: Whose initial speed is this point referring to? The boulder's?

Page 8, Point 8: Here it seems necessary to specify uncertainties of the given numbers.

Page 8, Point 10: Where do the 2.0 m surface lowering and 1 to 2 m debris thickness come from?

Page 9, Point 10: Here it would help to state HOW the debris-covered area was reduced during the monsoon.

Page 9, 11: I suggest given numbers for the initial and lost volumes, so that the reader can confirm their origin.

Section 4.2, first paragraph: The authors discuss glacier dynamics as a potential error source. This seems unrealistic or at least unintuitive, because depending on the glacier's size, glacier dynamic reaction to seasonal mass balance changes should be negligible.

Section 4.2, third paragraph: Here the authors should better justify their assumptions, otherwise the reader has the impression that they played with numbers until they matched. Why is only the no-winter-melt prone source area taken into account? Can snow also be mobilized outside such an area? What is the reason for choosing exactly 1/2*900 kg mˆ-3 for snow density?

FIGURES

Include summit elevations in all photos/maps.

Figure 2: Parts of the legend and the GPS location are difficult to read/find.

Figure 3: What are do the thick contour lines represent?

Figure 5: There are various regions of elevation change, which are not commented on (e.g., far Eastern and far Western regions).

Figure 7: I cannot confirm the "rock existence".

Figure 8: Is this figure really needed? How does this help to determine tree age?

Figure 9: It is hard to distinguish the thin, thick and blue lines in Panel d.

REFERENCES

Podolskiy, E. A., Nishimura, K., Abe, O., & Chernous, P. A. (2010). Earthquake-induced snow avalanches: II. Experimental study. Journal of Glaciology, 56(197), 447-458.

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