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Interactive Comment

Interactive comment on "Landslide dynamics and coupling revealed by L-band InSAR in central Georgia" *by* E. Nikolaeva et al.

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Replies to comments by "Anonymous Referee #1"

The anonymous referee has read and assessed our manuscript in detail and provided numerous comments and suggestions that we found very helpful to further improve our work. Please, find below the reviewers points followed by our answer in the bold.

COMMENT 1: I don't think that the authors have justified the use of Okada dislocation model. Muller and Martel (2000), cited at pg 4934, line 26, use a boundary element method for modeling translational landslides - not Okada as implied. The observations of horst and graben structures and description of the landslide itself made up of several smaller sliding blocks suggest significant internal deformation, not all elastic. A

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stronger case is needed to justify the use of the Okada dislocation, and if used, also some discussion of how deviations from elastic behaviour would affect the depth found for the decollement.

REPLY 1: We appreciate this comment and added more explanations in the text, also improving the justification on the use of a dislocation model by inserting and considering more references. The corrected text reads as follows: ‘We followed previews kinematic landslide studies (Fruneau et al. 1996; Martel 2004) where models were used to describe landslide process. These elastic models consider a flat earth, and a linear elastic rheology. The main rupture plane of the landslide was simulated by a planar dislocation plane. We follow these works and also used a dislocation plane as a first-order approximation (Okada 1985). Although also our model is simplified in geometric and dynamic sense. The limitation of such a model was already detailed earlier, as it ignores non-elastic deformation (Martel 2004).‘

COMMENT 2: Title: I am not sure that the use of the “dynamics and coupling” in the title is appropriate, because a) the discussion of coupling with e.g. rainfall or earthquakes is inconclusive (page 4943, lines 13-15), b) the Okada modelling does not capture dynamics and c) the analysis of InSAR derived deformation over time is based on interferograms themselves rather than a time series, and does not properly account for noise in phase. This points should all be addressed and the title changed.

REPLY 2: We appreciate this comment and changed our title to: ‘Landslide observation and volume estimation in central Georgia based on L-band InSAR’

COMMENT 3: Although there are a couple of statements in the discussion and conclusions that refer to broader implications of this work for landslide hazard in Georgia, these ideas are not developed. This work could be strengthened by more detailed consideration of the history and potential future developments of this landslide, as well as a discussion of whether or not it is representative of landslide hazard in the region.

REPLY 3: We added in the discussion: ‘Our work suggests the landslide to have a decade long history, which is developing. From Landsat imagery we see that the man-made activity has significantly increased. Our own survey and questioning of residents further supported the fact that the mining activity has strongly increased in the past 8 years. One may speculate that the effect of man-made activity on landsliding may even more augment as unloading in the toe region continues. Moreover, as the landslide is hence further developing, also interacting processes, such as earthquake or rainfall triggering may alter with time. Therefore, a close observation and further work with a more regular data acquisition is needed, allowing to detect displacement rate changes at higher detail. Also a monitoring of the mining activity may help to clarify the impact man made actions do have on natural hazards. In this view, the Itskisi landslide may provide an excellent laboratory, where such interacting and cascading processes might be well studied'.

COMMENT: I have listed the other changes to be made before publication by line number below: Abstract, line 15: what are the "important implications that are applicable elsewhere"? Is this a reference to earthquake triggering? If so, say so explicitly – also in the Conclusions at 24-26

REPLY: We mean an earthquake triggering, mining activity and rainfall.

COMMENT: Page 4926, line 35. Not clear what "conditionally" wet means?

REPLY: We deleted "conditionally"

COMMENT: Page 4927: Why are landslides "in Georgia in particular" likely to accelerate?

REPLY: It was clear observed after the 1991 Racha earthquake (Jibson et al. 1991), or after rainfalls in south-west part of Georgia (Gracheva and Golyeva 2010).

COMMENT: Page 4929, line 1. Exponent missing in number.

REPLY: We changed.

COMMENT: Page 4930, line 16. Word choice – “good conditions”?

REPLY: We changed ‘good’ to ‘suitable’.

COMMENT: Page 4931, line 16. Missing word?

REPLY: We added: ‘images of’.

COMMENT: 3.2 InSAR. Were any tests done for DEM errors after topographic correction? E.g. at linear regression for baseline dependence? If there is significant topographic change due to long term sliding/mining activity, then current topography may differ from ASTER DEM, which will represent time-averaged topography since early in ASTER mission.

REPLY: The test was not done for DEM errors after topographic correction. Beyond the scope of this work and require new study.

COMMENT: A time series analysis of InSAR data would also help conclusions about coupling and triggering. Perhaps the authors did not attempt this due to the low number of images available – but this should be at least be addressed in the text.

REPLY: Accepted and changes made. We refer to the REPLY 3 above.

COMMENT: Page 4936. Some further information on the derivation of Equation 1 would make this easier to follow.

REPLY: The cross section of the ellipse with plane $z = h$ is an ellipse and its axis may be calculate by using equation 1. We added more explanation in the text.

COMMENT: Page 4937, lines 5-12. The observations about nonlinear rate would be aided by time series analysis if possible. If not, then at least some discussion of the noise level in the phase data is still needed. Is the maximum difference, 5 cm, likely to be larger than e.g. atmospheric noise?

REPLY: Unfortunately, data is not warranting a robust time series analysis. We calcu-

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lated noise level outside of the landslide (see supplement file).

COMMENT: Figure 5. I struggle to see correlation between antithetic faults and displacement map described in the text. Could the trace of the faults be marked on? The caption refers to a 'displacement map' derived from InSAR – derived how? Unit is cm/day, so shouldn't it be 'rate map'? Is this the average rate for all interferograms?

REPLY: We added in the text page 4937, line 26: ', best visible by the blue pixels on east side of fault '. We added 'rate' in the caption of Fig. 5. It is a displacement rate from 2009 09 04- 2009 10 20 interferogram.

COMMENT: Page 4939 lines 25-28. What is the noise level and how was it estimated?

REPLY: We added a chapter detailing the noise level and its estimation in the discussion. As pointed out, however, colored noise can hardly be estimated as long as an independent dataset from GPS or precise leveling is not available.

COMMENT: Page 4940 lines 20-24. How do these factors explain difference? Would they lead to under or overestimation in each method?

REPLY: The variation of exponential change to 1.2 or 1.4 will bring insignificant change in the result.

COMMENT: Page 4940 lines 17-20. The timeline is missing, and this is important information. When was the abandoned? When were houses damaged?

REPLY: We agree that it is an important piece of information. However, we have no precise information about it. The Itskisi village was on the topographic map 1974. We observed only several damaged houses during our field trip in 2011. Remains of buildings it was possible to see in the pond, which was not mapped in topographic map 1974. We ourself did ask a number of residents, but obtained conflicting answers that we decided not to present in this manuscript.

COMMENT: Page 4941 lines 7-12. On what basis were interferograms excluded from

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the analysis? Baseline threshold? Coherence threshold? This should be quantified and stated explicity.

REPLY: We added: 'We choose only interferograms with coherence higher than 0.4 after filtering of interferograms'.

COMMENT: Page 4941 lines 21-23. More detail is needed here and this statement should be also be referenced. Could it be attributed pers.comm. ? What constitutes a "dramatic increase in landslide hazards"? This is rather imprecise.

REPLY: We changed to 'increase in landslide hazards'.

COMMENT: The hazard context is not always very clear: how long has this landslide been taking place? Significant movement started since 1972 at least? How recent was onset? There are several statements in the Discussion that imply that it has been long-lived, but it would be useful if this question addressed directly at some point in the text.

REPLY: We entirely agree that this question would be useful. However, the private mining companies did not provide us this information. This is understandable, as significant claims might be expected, and we prefer not to go into this detail for the same reason. Also, we did not find archive information about this place except topographic map from 1974.

COMMENT: Page 4942, lines 19-22. I don't see this in Figure 9. Also not clear what is meant by interferograms "tend to occur" as they have fairly long duration relative to a minimum point.

REPLY: We changed color in Fig.9, so we hope it is clearer now. "tend to occur"= to exhibit a tendency

COMMENT: It would be good to have some discussion of how typical/atypical this area is relative to other landslides identified in Georgia with reference to volume, velocity or impact of mining?

REPLY: Accepted and changes made. In the revised manuscript we provide details based on our field trip and InSAR observations. Also we provide a comparison to what is known at landslides associated with the 1991 Racha earthquake. However, we are not able to provide a review on other landslides induced or affected by mining activity in Georgia. To answer the question we add a more general discussion on landslides affected by mining from other parts of the world.

COMMENT: Page 4943, lines 1-3 and 13-15. These statements seem to contradict each other. Is there increased velocity during periods with earthquakes? Figure 9 looks like it.

REPLY: Obviously our description was misunderstood. To avoid such apparent contradictions we clarified the text.

COMMENT: Page 4945, line 10 “conditionally faster”?

REPLY: We deleted ‘conditionally’ for ease of understanding.

COMMENT: Page 4945, line 25-26 “the location and type of landslides in Georgia appear to vary in time”. What does this mean? From where are conclusions drawn about Georgian landslides in general?

REPLY: The inventory data of landslide supplemented and varies with time in Georgia. For example, several landslides are stable nowadays and were not stable in 1991. Conclusions drawn based on our investigation, all available literature about landslides in Georgia (Jibson et al. 1994; Arefiev et al. 2006; Gracheva and Golyeva 2010; van Westen et al. 2012), from the department of natural resources (http://www.gadnr.org/) and seismic monitoring center (http://seismo.iliauni.edu.ge/) in Georgia.

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/1/C1722/2013/nhessd-1-C1722-2013-supplement.pdf

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 4925, 2013.

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