

Interactive comment on “Brief communication: Rapid mapping of event landslides: the 3 December 2013 Montescaglioso landslide (Italy)” by A. Manconi et al.

Anonymous Referee #1

Received and published: 18 March 2014

Review of the manuscript “RAPID MAPPING OF EVENT LANDSLIDES: THE 3 DECEMBER 2013 MONTESCAGLIOSO LANDSLIDE (ITALY)” by A. Manconi et al. MS No.: nhess-2014-41

The manuscript by Manconi and coauthor describes a slope failure in Italy that occurred on 3 December 2013. The authors use a cross-correlation approach (if my guess is correct) applied to satellite radar data acquired by the Italian satellite CosmoSkymed. The Montescaglioso landslide was shown to move by as much as 30 m horizontally, with significant hazards associated. The authors discuss whether the landslide was associated with intense rainfall.

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The data analysis done on the radar data is certainly excellent, showing a spectacular event that occurred few months ago. The figures are high quality and all necessary (in fact also the figures in appendix are relevant). The writing, however, is rather poor and to my opinion misleading. Given the sound scientific analysis of the data and relevance of this particular landslide event, I can recommend the publication of the manuscript. Major major major rewriting has to be done, however, as detailed below.

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Major points

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1. Why the authors emphasize the “rapid” mapping and “rapid” analysis? This in fact was absolutely not clear. In the abstract alone, the word “rapid” can be found four times. Is a satellite imaging system that is acquiring every 16 days (the normal CosmoSkymed revisit period) so rapid? Other geophysical contributions dealing with rapid assessments after earthquakes and other hazards deal with timelines on the order of seconds or minutes. Here an analysis done weeks after a landslide hazard is not rapid. Please consider focusing on the geoscience contribution rather focusing on the “rapid” technique.

2. The literature review in the introductions is flawed and needs to be rewritten. For instance, in line 20 and following: Obviously the authors are not aware of the current alternative methods applied to satellite and ground imaging data, such as image cross-correlation, DEM differencing, and others. Please also consult the numerous publications that have emerged following the earthquake induced landslides in Japan following the Tohoku disaster, many of the studies are published and referred to in a book (Earthquake-Induced Landslides: Proceedings of the International Symposium on ... by Keizo Ugai et al, Springer 2013). Also consider ground based InSAR systems (Corsini et al., 2006 and Jaboyedoff et al., 2010), determination of the average spatial shift by a cross-correlation function image pairs (White et al., 2003), Target Detection

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and Tracking (Veeraraghavan et al., 2006) and others. A recent paper published by Gance and coauthors (Engineering Geology Volume 172, 8 April 2014, Pages 26–40) will allow to get some overview on the current photogrammetric methods.

3. Method is unclear. A section on methods is needed. Neither the PO method is detailed, nor the identification approach of fractures. How was the InSAR data processed. Detail the PO processing, which correlation term was used? Was it processed in the frequency domain? At which window and padding size? And so on.

4. Please add a chapter on the geologic interpretation. How rainfall and the landslide are related (if any)? Common concepts have to be discussed, associated to shear stress and pore pressure increase.

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Minor points

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1. Chapter 2, line 8: please add detail, how steep are the slopes. Facing which side? Is the configuration suited for your approach?

2. Chapter 2, line 10: please add a reference of the geological map authors/publisher

3. Chapter 3, 1st paragraph: This is interesting but hard to read. Please restructure by first describing the earlier events. First discuss the October rain, second the December rain, third the landslide. Please also compare the rainfall to the seasonal rainfall, for instance: “in just ~2 days a quarter of the annual rainfall was recorded”

4. Chapter 4, 1st paragraph. The analysis of the historical imagery would be worth to show in the paper or in the appendix. Figure S1 is only an interpretation of a data set that is not shown in the manuscript, analysed with a method that is not explained in the manuscript. At least the authors could create a composite map, using different images in different channels, to show the changes in a figure.

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5. Figure S1 and S2 could go into the maintext, including descriptions. The figure S2 shows the structural summary, that would be important to be compared to the displacement maps of figs 2 and 3. Are any of the structures mapped active? Are any structures active, or does the PO method not have the resolution and sensitivity to localize such?

6. Chapter 4, 2nd paragraph: photographs taken during helicopter flights: these are not provided, neither in the manuscript nor in the appendix. Figure S2 is not the one referred to in the text.

7. Chapter 4, 2nd paragraph: How are the “geomorphological features” identified?

8. Chapter 5, 2nd paragraph: How was the dinsar data processed?

9. Chapter 5, 3rd paragraph: How was the PO processing set up? FFT? Window size? Oversampling? Masking? Correlation function? Multi pass? No words about these!

10. Chapter 6, 1st paragraph: Completely change the scope. Omit the “rapid” discussion and focus more on the geoscientific results provided.

11. Chapter 6, 2nd paragraph: the sentence that “optical data can usually provide qualitative information only” is simply wrong, outdated and shows that the existing literature was not considered.

Summary. I am aware that my comments are very critical. But the manuscript has many very valuable contents that are worth publishing. I hope my criticism help to reflect and improve this early stage manuscript.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 1465, 2014.

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