Nat. Hazards Earth Syst. Sci. Discuss., 2, C296–C298, 2014
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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Landslides triggered by the 12 January 2010 Mw 7.0 Port-au-Prince, Haiti, earthquake: visual interpretation, inventory compiling and spatial distribution statistical analysis" *by* C. Xu et al.

Anonymous Referee #1

Received and published: 1 April 2014

This paper presents an in-depth description and analysis of the landslides triggered by the Haiti Earthquake. The preparation of the inventory seems to have been done in an objective manner that is consistent with the best methods to detect and map the landslides triggered by the main event seismic shaking without actually travelling to the site and performing "on-the-ground" observation. The parameters chosen to compare with the landslide distribution reflect a slight naivete concerning seismically induced landslides. The parameters, elevation and slope aspect have seldom shown any significant influence on the landslide patterns in a seismic event. Elevation, in

itself, has no mechanistic effect on a landslide's location. It may play some part in topographic effects on shaking, but has no intrinsic effect itself. The azimuth in which the landslides occur generally has little significance on landslide location except where steep slopes are preferentially oriented due to fracture or joint orientations in the landscape. The general conclusion of the paper that slope and PGA (shaking) have the most influence on the landslide distribution is one that has long been known to scientists working on earthquake-triggered landslides and is not surprising or novel. In comparing the landslide "proxies" to different variables such as PGA, lithology, slope, etc., the use of letter or numbered categories instead of the variable quantities themselves is particularly confusing and hectic. It would be much easier for the reader to view a graph in which PGA was represented by 0.2g or 0.4g instead of "category 3" where you have to "decode" the category in every case. In comparing lithologies, use "ls" (limestone), or "ss" or the formation symbol instead of numerical categories that have to be repeatedly looked up. There is a lot of text and graphs in the figures and having the actual values and abbreviated descriptions in the text and on the graphs would be a lot easier for the reader to follow. There are 17 graphs, most with at least four variables plotted on the same axis which makes for pretty tedious and confusing reading. I wonder if some of those that don't show anything remarkable could be dropped and their relationships just mentioned. In their comparison of the landslide density to various seismological parameters, they compare the variations of landslide density to different segments of the Enriquillo-Plantain Garden Fault (EPGF) even though the greatest amount of energy release was on a blind thrust which dips to the north away from the trace of the EPGF. I fail to see the reasoning for concluding that different landslide densities along the trace of the EPGF were due to differences (not specified) along the trace when motion on the EPGF is not what is generating the slope failures. Also the speculation that a preponderance of the landslides are on east-facing slopes might be caused by eastward motion along the EPGF is not really supported by the actual fault slip model which is mainly oblique thrust motion. In addition any motion that decouples slope material from an east-facing slope would necessarily involve

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accelerations to the west. Eastward accelerations would put slope materials in compression (less favorable for failure on east-facing slopes) while westward acceleration would put slope materials in tension (more favorable for slope failure of east-facing slopes). These kinds of arguments are also too simplistic to explain slope failures that result from complicated interfering waves and prolonged shaking durations. I have a problem with the comparisons of the landslides with PGA. There were no seismic instruments in Haiti during the earthquake. The PGA distribution used by the authors comes from SHAKEMAP which estimates PGA from teleseismic and other indirect evidence. The PGA variation from the earthquake is not known with enough precision for the authors to be differentiating the shaking into different categories of 0.04g intervals. This is way too detailed. Meaningful differentiation of shaking might be known to intervals of 0.25g or maybe even coarser. So, I think that differences of the landslide distribution that are ascribed to the categories of PGA used by the authors are probably meaningless. With a significant rewrite of the manuscript to remove the naïve assumptions and speculation regarding the mechanics of landslide triggering and ground motion generated by the earthquake, the paper could be published. The inventory seems to be nearly a complete one limited by the resolution of the imagery used and would be a useful addition to landslide inventories triggered by earthquakes. It seems to be much more complete than the inventory produced by Gorum et al., (2013). Detailed comments and recommendations are inserted as sticky notes in the margins of the manuscript.

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/2/C296/2014/nhessd-2-C296-2014-supplement.pdf

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 1259, 2014.