Natural Hazards
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Sciences
Discussions

Interactive comment on "Significance of substrate soil moisture content for rockfall hazard assessment" *by* Louise M. Vick et al.

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This manuscript addresses how substrate soil moisture content affects the runout of rockfall boulders, with an emphasis on improving rockfall modeling and hazard assessment. This is the latest in a series of nice contributions utilizing data from rockfalls in the Port Hills, triggered by the 2010-2011 Canterbury Earthquake Sequence. Here the authors use field-scale testing and laboratory direct-shear testing to quantify how the strength of the local loessial soil changes with moisture content, and then evaluate those changes in cases of well-documented rockfalls. Their conclusion is that a certain soil will produce greater rockfall boulder runout distances when dry rather than when wet, because wet soil will dissipate more of a boulder's kinetic energy as the soil deforms. Although this finding not especially surprising, it is nice to see the re-

sults quantified in both field experiments and in model simulations. The results help to identify the maximum credible rockfall hazard, which occurs in dry soil conditions.

Overall the manuscript is well-written, balanced in its interpretations, and offers solid conclusions supported by data. I have a few suggestions for improvement below, but overall this is a valuable contribution to rockfall hazard assessment. My recommendation is to accept with minor revisions.

The amount of disparate data presented in the manuscript is impressive, and, at times, perhaps a tad overwhelming. These data include: (1) rockfall boulder runout from earthquake triggered rockfalls, in dry conditions (2) boulder runout data from experimental rockfalls, in wet conditions (3) measurements of boulder impact scars, (3) measurements of soil moisture content, (4) soil shear-test data, (5) models simulations of (1), and (6) model simulations of (2) in both wet and dry conditions. Because the manuscript addresses these data sources at two different study sites (Rapaki Bay and Mount Vernon), I had some trouble keeping track of the various conditions and results (e.g., remembering whether the rockfalls at Rapaki Bay happened under wet or dry soil conditions). I found it easiest to understand the results as a function of location, so I recommend that the authors make clear in each instance which site the results are from and what soil conditions that site represents. As an example, the caption for Figure 6 should state that these results are from the experimental rockfalls at Mount Vernon, which occurred under wet soil conditions. Ideally, it would have been nice to see additional experimental rockfall runouts from Mount Vernon in dry soil conditions to provide a direct comparison with the runouts in wet conditions, but I recognize that we cannot always perform science under ideal conditions!

The RAMMS rockfall model used in this study does not specifically address the conditions of the substrate that rockfall boulders impact, so the authors account for this by adjusting some of the RAMMS terrain parameters to reflect lowering of soil shear strength. The values that they adjust to account for wet conditions are reported in tables, but I found myself wanting more information on exactly how they derived the

adjusted values. Unless I missed it, the most information offered on this subject was the statement on Page 6, line 23: "Parameters were adjusted incrementally until satisfactory results were achieved." What does this mean, exactly? What defines "satisfactory"? The results may have been satisfactory, but the explanation of this methodology is not.

Along those lines, it might be useful if the authors discussed how other models using restitution coefficients to represent boulder impacts with the substrate could be modified to account for wet soil conditions; as is, the discussion is limited to the RAMMS model, which is only one of several rockfall runout models in use.

The sections on impact scarring somehow feel a little disconnected from the rest of the manuscript, even though they deal with a fundamental issue, namely how the effects of a wet or dry soil on rockfall impact are actually expressed in the field. Presumably the difference in the scar depth/length ratios at the two sites shown in Figure 3 is due to differences in soil conditions but this is not stated explicitly, either in the text or in the Figure 3 caption. This is relevant because Figure 3 provides evidence for the difference in the wet versus dry models results shown in Figure 6, where the dry conditions model significantly overestimated the actual rockfall runout. Perhaps the impact scarring data would feel more connected if the authors incorporated more discussion as to the usefulness of these measurements. As an aside: If soil conditions are not known at the time of a rockfall, could they be inferred from impact scar measurements, potentially offering a field-based method of soil characterization after the fact?

The authors tend to use passive voice (e.g., "samples were tested"), which leads to some ambiguity as to whether the authors performed certain analyses or whether they are referencing previous work. For example, on page 5, lines 10-13, it is unclear whether the authors inferred the moisture content of the soil at Rapaki Bay, or if this was done by Carey et al. (2014). Use of active voice (e.g., "we tested samples") can help to reduce ambiguity.

Regarding the rockfall experiment at Mount Vernon, the authors state that 20 boulders were triggered and mapped, yet figure the caption for Figure 6 indicates 70 experimental rockfall boulders. Why the discrepancy?

Figure 4 caption: The impact scars in "C" are representative of dry soil conditions (correct?), and thus only show examples of the schematic in panel "A". Are there similar photos of impact scars in the wet soil conditions at Mount Vernon? If so, it would be nice to show examples from both wet and dry conditions.