

## ***Interactive comment on “Dealing with deep uncertainties in landslide modelling for disaster risk reduction under climate change” by Susana Almeida et al.***

### **Anonymous Referee #1**

Received and published: 12 December 2016

#### General comments:

This paper demonstrates a useful application of a “bottom up” approach to understanding drivers of landslide behavior. I caveat that I am reviewing mostly in my capacity as one familiar with analytic methods to support decision making under deep uncertainty, and have minimal landslide modeling expertise. Therefore, my comments are mostly restricted to this realm, and I trust other landslide modeling experts will provide review on that front.

On this subject, my impression is that, for the scope the paper has limited itself too, I believe the treatment given to the subject matter is excellent. This includes the blending of distributions and deep uncertainty, and utilization of the full feature-set of CART, and

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introduction of an auxiliary variable to simplify the tree structure with minimal loss of performance.

In general, I believe the paper should be revised to more clearly discuss the limited nature of the findings, or better yet, explore a few other key parameters that would greatly enhance the ability to speak to issues of generalizability – the most clear being, slope, site, cell size, and time step. (To be clear, I don't necessarily think all of these things need to be explored for the paper to be publishable, but they should be spoken to. For many, the authors could just explain why they are confident the qualitative nature of the findings will not change.) It would also be useful to more clearly orient the reader early-on to how the Monte Carlo sampling mixes with the deep uncertainty sampling.

Specific comments:

P2:

L19 – 21: I understand how censored or biased availability of data limits statistical models relative to physically-based models, though would be nice (but not required) to speak to the issue of whether physically based models have large domains of applicability if their development and testing is often limited to the same regimes of data availability. Put more plainly: Yes, you can run physically based models in the domains where you might not have good enough data to build a statistical model, but might you still be tenuously extrapolating?

P4:

Research question 2 seems overly vague and of ambiguous relevance without sharpening it a bit. The discussion does that better, but maybe you could reverse engineer that question a little to be more precise.

L8: “has a track-record. . .” – causes the reader to expect citations here (I see some are provided farther below).

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P5:

L4-8: Would be strengthened by providing some context on what “good” classification rates are in this arena, both from a scientific perspective and a policy-relevance perspective. It would be great to also include at least one more validation exercise if such exists.

L12: “matric suction” – is technical. If fine for assumed audience, that’s fine, just flagging.

L26: Minor: Strengthen by putting a number before “uncertain input factors.”

P6:

L5-7: Since this is couched as bottom up and deep uncertainty oriented, might orient reader to this use of distributions and how that does or does not integrate with the deep uncertainty mode of analysis – which often (but not necessarily) operates without distributional assumptions.

L7-8: For reproducibility, would be good to describe where the “checks undertaken” are explicitly listed.

P11:

L11: This statement suggests there is a significant interaction effect between climate and other source of uncertainty, or that one conducting the sensitivity analysis independently would not think to add the two elementary effects of climate and other variables.

Figure 6: Seeing this visually makes me think the authors might also want to consider linear discriminate analysis — not at all a required change, just for consideration.

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