Comment on nhess-2021-330

R. S. Purves (Referee)

Referee comment on "Automated snow avalanche release area delineation in data-sparse, remote, and forested regions" by John Sykes et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-330-RC2, 2022

I enjoyed reading this paper which aims to apply a method developed in Switzerland to a Canadian test site. The underlying aim of the method is to identify potential release areas (PRAs), using a combination of terrain and vegetation data. PRAs are an important and useful tool in hazard mapping, and the paper describes a complete workflow for their derivation. In practice this means that the authors not only calculate PRAs, but they also generate a terrain model and a forest mask (which is non-binary). Overall, the paper seems to me to make a useful contribution and I think it could be published in NHESS.

However, I have some reservations about the paper in its present state. At the moment the approach taken is to my mind a very linear one, and although some experiments are made, it is difficult to see how the approach could be generalised in practice. Furthermore, in places the manuscript is overly wordy, which I think obscures the message, while in others the methods are described in insufficient detail to allow their replication. The authors have chosen to incorporate results and discussion in one chapter which makes teasing out the implications for the wider field unnecessarily hard, and leaves the impression of a discussion of the specifics of this case study rather than the purported much more general aim claimed by the authors. I have several specific suggestions for the authors.

- The results reported compare, if I understand correctly, an optimised version of the PRA model including forest cover with the original Bühler et al. model. However, the original model uses the parameters derived in Davos. Given that the improvement is in any case rather small, wouldn't it be informative to do the grid search on the data from the Canadian test site and compare performance. In general, when we optimise models and compare, we should (in my view), always optimise our baseline too.
- You argue throughout the paper that the higher resolution data are needed. But you could easily show this by also running your model with the 18m DEM and 30m landcover dataset. It would be interesting to see how different the results really are.
- You argue regularly throughout the manuscript that your approach is transferable because you use open source software. But you bought data, and in fact you apply the

method to a very small region in the end. I think more transparency about data costs is important, since this is likely a show stopper in many regions.

- The methods are in places very wordy, and though I like the idea of introductory sections describing the overall approach, which is then broken down into more detail, you are often very repetitive. Furthermore, lots of tables contain information which is then repeated verbatim in the text. I would suggest:
  - Thinking about a graphical summary of your methodological approach as a whole and linking this to sections
  - Removing or reducing obvious repetition. For example, I think it would be more useful to give parameter values in tables and cite these (rather than duplicate) in the text.
  - The grid search (for which you simply cite Bühler (N.B. There are multiple 2018 Bühler papers in your references, you need to tidy up the citation)) is not described in sufficient detail to allow replication. I also couldn't find a clear description of this in the Bühler NHESS paper, but I am not sure if I looked in the right place. Since this is central here, I suggest describing the method more completely. By cutting other unnecessary text this shouldn't make the paper longer, but it will be much more useful to the reader as a standalone piece.
- The validation approach taken is interesting, but not really scalable in my view, which also limits the generalisability. Canada is rather special in having guides who repeatably descend the same routes winter after winter, and discuss these in comprehensive guides' meetings, often on a daily basis. How about (at least in the discussion) discussing how other data could be used, for example in a Canadian context those derived from InfoEx?
- I would strongly suggest splitting results and discussion. I also note that the papers' objectives are rather implicit. If you more explicitly stated these in the introduction, then you could write a more standalone discussion. I'd be particularly interested in more comment on how realistic it is to extend your method to areas in other countries and some discussion of the likely costs.

Minor comments

- In many places you use multiple references for rather simple points. Often 1-2 references would suffice.
- The validation polygons are all concentrated in a rather small area. What are the implications of this?
- On line 164 you say surface hoar is amongst the most common weak layers associated with avalanche problems. Later (L321) you say areas dominated by such problems were minimised in the validation dataset. Why?
- You don't comment on the avalanche problem in Davos, though you claim the parameters are rather universal (L452). This depends to some extent on avalanche problems, so wouldn't it make sense to discuss these.
- I don't understand how the statement on L519 where you state that your method is correct for 57.5% of the polygons and then say this means it is within +/- 12.5% of the area specified by the guides. Table 3 doesn't help me here…
- Figure 13 is really nice. But the very specific discussion of individual runs doesn't speak to a very generalisable method.