

## ***Interactive comment on “New Global Characterization of Landslide Exposure” by Robert Emberson et al.***

### **Anonymous Referee #2**

Received and published: 28 May 2020

I enjoyed reading this paper and especially enjoyed looking at the maps and figures. It tackles an interesting and large problem of modeling relative landslide exposure worldwide. I think the topic the authors address is worthy of being published and the results will be of interest to many. However, the manuscript and results are not yet ready for publication. In particular, there is some potentially flawed logic and confusing methodological choices that need to be sorted out or clarified. I summarize the major points of confusion or issues that I feel need addressing below, and then provide line by line comments below.

1) The authors say they are modeling landslide exposure globally, and though they do not specify, from the context they give I assume they mean as it exists now or in general. However, I do not think that is actually what they are modeling. By using past

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weather data, they are modeling what landslide exposure WAS averaged over the time period of the IMERG data. There were likely some extreme weather events in those 19 years that hit some countries and caused elevated Popexp values in this analysis that will not happen again in the next 19 years, there will be new extreme events that did not happen in the last 19 years, and also all bets are off with climate change.

2) The second major issue I struggled with was conceptualizing the physical meaning of the various metrics they use as proxies for exposure. What is the physical meaning of population exposure, road exposure and infrastructure exposure, as currently computed? What am I supposed to make of “nowcast density” or the inclusion of the vague concept of nowcasts in the units? Nowcasts are a fuzzy concept in themselves and then the authors convert it to another confusing metric, a rate of unknown timescale and a density over, I assume, time(?), it doesn’t mean anything to me and is really hard to wrap one’s mind around. Why not at least do something more tangible like number of days per year of elevated landslide hazard? Or alternatively, the percentage of the time that a given cell has an elevated “nowcast”? Nowcasts represent relative landslide hazard for 3 hour time periods, so either of those seem easy to compute and would make a lot more sense. It would give the units with some physical meaning, albeit still somewhat vague: e.g., people-hours/year/km<sup>2</sup> exposed to elevated landslide hazard or percentage of the time landslide hazard is elevated/person/km<sup>2</sup>. Either would be much clearer in my opinion than the bizarre units and metrics currently used and also easier for other people to use or compare against in future studies.

3) The authors seem to conflate their modeled results with observations/ground truth and also conflate their modeled proxy for population exposure with actual population exposure. The authors need to always be clear that they are presenting modeled results and proxies, NOT data or observations. I have noted some of these instances in the line by line comments.

4) The writing is sometimes hard to follow. There are many run-on sentences and some confusing and/or convoluted logic, especially in the abstract and introductory sections.

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I have pointed some of these out in the line by line comments.

5) Critical details are missing about the LHASA model updating that was done, the validation of the updated model (against, presumably, the inventories that were previously discussed as being biased?) as well as the uncertainty estimation.

6) One point needs clarifying: the authors state that they normalized the areas by squared decimal degrees rather than km<sup>2</sup> for country-wide statistics. Do they mean they used the latitude and longitude grid? If so, that is going to skew the normalization pretty dramatically for countries away from the equator. That normalization needs to be done in units that preserve area for the proxy to be globally consistent and comparable from country to country.

Line by line:

Abstract: The logic of the abstract does not make sense to me. It introduces the problem of inventories being biased away from areas where human settlement or infrastructure are, but then it says in order to address this limitation, they are going to model global exposure to landslide hazard. . .but that isn't addressing the problem they raised in the previous sentence, they raised the problem of landslides in remote areas far from humans. It's also unclear what gaps in the inventory-based estimates (what estimates/estimates of what?) they are filling in. Then on line 17, they say they compare levels of landslide hazard "mitigation" between countries but they don't look at mitigation at all in the paper. Overall, the abstract is really confusing and could probably be rewritten.

L36 – though it is stated later, since the authors mentioned "near-real-time" here and that has many different meanings, I would suggest moving the info about how often it is updated and with what delay here.

L39 – why "near global"? what is missing?

L44-46 should be in the abstract

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L49 – I would just delete the part of this sentence after the semicolon, no one should be using a global map at 1 km resolution for evaluating landslide hazard except as a way of looking at overall trends or patterns on a global/continental scale.

L50 – what pixel resolution? And I didn't think LHASA modeled exposure, does it? Isn't that what this paper is about?

L51 – it does not provide a “clear picture”, that phrasing implies that it is reality. It is a model. An estimate. The authors should be clear about that here and throughout.

L54 – I find the concept of an “average rate of hazard Nowcast’ to be very convoluted. See my earlier comment about a suggested alternative.

L57 – what is a “landslide climatology”?

L69 – what are these thresholds based on? Also, is it the ARI that is modeled based on the susceptibility layer or the threshold that the ARI must exceed? Please clarify.

L74 – Forest loss since when?

L75-79 – I found this description very hard to follow, can it be clarified?

L79-80 – More info needed on the methodology (generally, what is it based on/how does it work/how is it validated) even if the readers can still look to the original paper for the nitty gritty details.

L89-90 – How is landslide activity anticipated? Need more details here about how these thresholds are chosen since that ultimately controls all the results of this paper. . .

L92 – I thought there were different levels of “nowcasts”, how are those dealt with in this averaging?

L108-109 – They happen to line up as well? Didn't you have to resample one to the other?

L117 – GRIP acronym needs to be defined above before its used.

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L124-125 – Don't some of these road network datasets often include small "roads" like footpaths and farm roads? It seems like some levels might be worth excluding since they are not likely mapped consistently across the world whereas higher levels would be more likely to be consistent.

L140 – I am very confused about how one multiplies infrastructure by a nowcast density...

L149 – Need to include some more info about how the model is trained in the first place in order to understand this part about assessing errors.

L175 – Applying what? What is "this"?

L176-178 – This sentence is confusing, can it be clarified?

L180-182 – I'm not following the logic here, if the infrastructure is not completely mapped in a country, then the normalized metric will be just as wrong as the raw numbers, wouldn't it? The ratio would be off.

L195 – The results of this study are not observations. They are modeled proxies. Please rephrase.

L197 – Given the wonky units of popexp, population exposure annually is not what is modeled and shown in Figure 1, but a proxy for population exposure.

L229 – Impact is not the right word here. The authors are not modeling impact at all, they are modeling exposure, and also, the wording here implies that their results represent impacts that actually occurred, but they do not.

L240 – "modeled" population exposure

L256-258 – This point would be better made if Figure 4 showed the relative world map and the overall map side by side.

L259-260 – Don't we have the same problem if a given small country did not happen

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to be hit by an extraordinary precipitation event in the 19-year IMERG dataset but was next year?

Figure 2C – it is hard to see the colors against the black background in this part of the figure.

Figure 5 – are the fatalities by country the total number ever, or per year, or ?

Figure 7 – what are the x-axis offsets within each continent? Just random for visibility?

Figure 8 – Here and in other similar plots, it might be useful to label some more key countries, especially the outliers.

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