Natural Hazards and Earth System Sciences Discussions

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC2
https://doi.org/10.5194/nhess-2021-111-RC2, 2021

Comment on nhess-2021-111

Anonymous Referee #2

Referee comment on "A data-driven evaluation of post-fire landslide susceptibility" by Elsa S. Culler et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-111-RC2, 2021

This manuscript pulls in several interesting global datasets to try to add more data and a global perspective to the existing literature on wildfire and landslides. Currently, there are a few relatively large challenges for the manuscript that lead to a lack of clarity, generally. I will point out several of these challenges and potential solutions that might help the authors to refine their description to enhance clarity and ultimately usability of the results.

The first challenge is that the authors do not differentiate between landslides and debris flows following wildfire. This is problematic because there is a very large body of work that exists on post-wildfire debris flows, and a smaller, but important body of work on post-fire landsliding. I would highly encourage the authors to make this distinction using terminology such as the Varnes 1978 classification. The reason this is important is because the mechanisms that generate these different types of mass movement are very different and occur at very different times following wildfire. For example, post-wildfire debris flows typically happen in the first year after a fire and they are generated by distributed overland flow that coalesces into channels and mobilizes sediment (see for example McGuire 2017 and references therein). By contrast, shallow landsliding often happens decades after fire due to soil saturation and loss of root cohesion (e.g. Jackson and Roering, 2009 and references therein). These mechanisms are nearly polar opposite, in that the first is generated by very low infiltration after fire, the second is generated during a condition of very high infiltration after fire. Lumping these two types of mass movement together makes it extremely confusing for readers to put your precipitation analysis into the proper context. Even though debris flows and shallow landslides both move rock and sediment and involve some water, most of the erosion by debris flows happens in channels whereas most of the erosion from shallow landslides happens on hillslopes. This is sort of like saying that bread and dog biscuits are similar because they involve grain and baking, but functionally, they are extremely different. Consequently, if you could clarify what types of mass movement you are focusing on, that would go a very long way to improving the current manuscript.

The second challenge is the imprecision in the spatial location of your landslide database. Currently you are using a 10km buffer to see if there are burned areas near the landslide. In the case of shallow landslides, that can be extremely small (on the order of 10-100 m in cross-hillslope width if you are talking about true landslides and not debris flows). A buffer of 10km will often be much larger than a wildfire perimeter therefore it would be very easy to accidently confuse an unburned landslide with a burn area, resulting in spurious conclusions. Moreover, in many studies that focus on true landslides after fire, the rainstorms that trigger slides in burn areas also trigger slides in unburned areas (See for example: Meyer et al., 2001). I suggest that you carve out a small case-study to convince readers that you have a handle on the location or can quantify the uncertainty. If you can use a subset of the data with very well known locations and show the applicability at a known location with post-fire landsliding I think this will help people to trust the generalizations you make.

The third challenge is timing of the landslide database that you are using with respect to the wildfire. The issue of timing cross-cuts the first challenge. We know that, in general, shallow landslides happen several years after a wildfire and post-fire debris flows happen very soon after a wildfire, but you show the timing of the landsliding in any of your plots so it is very hard to analyze the how precipitation forcing should work based on the differences in those landslides with respect to time since fire. Consequently, explicitly analyzing time since fire will go a long way to helping readers to understand how to interpret your data.

A final general comment is that some of the precipitation analysis is very vague for readers unfamiliar with the type of data you are using. For example, you often refer to changes in percentiles, but often it isn't clear what the precipitation is a percentile of? Is it the percentile of the max 7 day rainfall, the max rainfall in a 38 year record, or something else. More detail in explaining your methods would really help readers. Similar comment for the figures. Many of the figures are missing axis labels or labeled tick marks like the inset figures in Figure 1, Figure 4 h-u, and Figure 5.

Below I will mention several line specific comments.

24: odd ref to Shakesy and Moody here as neither of those papers deal with landslides.

25: Do Kirshbaum and Stanley reference wildfire?

56: Ebel 2012 said that ash holds much more water, not that it reduces infiltration

67: There are many more up to date references you should add along with Cannon and Gartner, 2005. See refs in Moody et al., 2013; Santi and Rengers, 2020.

70: I'd add references to Pelletier and Orem, 2014

82: You are referencing papers about post-fire debris flows here, which are very different than landslides.

104: The Donnellan paper is about debris flows. To my knowledge there have not yet been landslides reported for the Thomas fire.

139: Please provide a more detailed definition of the precipitation depth percentile.

156: It isn't clear how you define those categories (e.g. what defines "rain" versus "downpour")

179: Previous studies say that debris flow susceptibility increases within six months of a fire, but landsliding can take many years to occur. See Benda and Dunne, 1997

194: You should acknowledge that severe wildfire is most common in semi arid regions. Humid regions can have fires, but the severity is limited and very few fires from humid regions result in landslides or debris flows because they don't reach very high burn severity.

196: Would CHIRPS even pick up a storm like the NCFR that hit Montecito, CA in January 2018?

215: Please provide a more detailed description of both CHIRPS and Daymet.

224: A 30-day rolling percentile of what?

236: Again the median percentile of what?

266: Note the wide literature that wildfire is more likely during droughts.

440: Be more specific in the length of time you are referring to when you say "a dry spell followed by a sharp uptick in precipitation" Are you talking about decadal drought, a few weeks, ?

446: Since you don't differentiate between debris flows and landslides, it is entirely unclear how to assess your conclusion that you think landslides are caused by isolated intense thunderstorms on dry soil. Wall et al., 2020 offers a really nice overview of literature in the Pacific Northwest about true post-fire landslides (not debris flows). Note that the authors referenced therein often saw landsliding after very wet periods many years after wildfire.

Figure 3: Not sure what you mean by "bold coloring" in the caption. What makes a color bold? Also there are 6 symbols in the legend, I only see four symbols in the plot.

Figure 5: I am very confused on what the y-axis is supposed to represent here, it is very hard to understand what this plot is showing with the current description.

Figure 6: In the legend are the first two lines supposed to be dashed?Also in the text can you explain what exactly you are doing with the kernel density. I'm unclear on the anslysis.

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