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Interactive comment on “Formation time and mean movement velocities of the 7 August Zhouqu debris flows extracted from broadband seismic records” by Z. Li et al.

Anonymous Referee #1

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This paper is not ready for publication for two reasons 1) structure and english usage and 2) lack of justification for and clarity of methods used. For these reasons, I have not done a detailed review, but instead summarize some areas that need the most attention.

The first major issue is that though technically the use of English grammar is not incorrect (except for many missing articles – a, an, the), there are many inappropriate word choices and strange phrases throughout that make it difficult to understand what the authors did and what they are trying to say. The intro and discussion sections in particular are full of strange phrasing devoid of any clear meaning. A related issue that

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Discussion Paper



is not just a function of English usage is that the authors fail to put their study in the context of what we already know and how this study expands on that. They simply list a large number of references of past studies on the same topic (p677 L23- p678 L2), but make no effort to say what this collective effort has uncovered so far, what past authors findings contribute to this study, and what this paper contributes to what we still need to know about the seismicity of surface flows.

The second major issue is that the scientific methods used and thus interpretations made from these methods are not fully justified, (though some of this could be from my misunderstanding of the methods because of the English usage):

1) The methods used to select a start time are arbitrary – like signals from most surface flows, the seismic signal from the debris flow emerges gradually from the noise, and the exact time it emerges depends on the noise level at the site, the energy of the event at the source, and how far away the source is from the station. At these high frequencies (1-10Hz), attenuation is quite rapid so distance is not a minor factor. The method they use to find the start time (STA/LTA) is just quantifying an arbitrary choice made using an arbitrary threshold. If the noise level at the site were lower, this method would undoubtedly find an earlier start time. If the station were further upstream, the start time would also be earlier. Despite this, the authors go on to overinterpret the meaning of different start times on different components – none of this is justified when it really is just all a function of noise level (unless they can show convincingly that it is not).

2) The authors use what they call a “time-by-time normalized spectrogram,” a concept they seem to have created, but it’s not clear. They use this spectrogram to say that the frequency content from before and after the start time they chose is distinct. They conclude means that the development stage of the debris flow is physically different in how it generates seismic waves – however this does not seem to be a real effect. Their normalization method basically just amplifies the noise when the signal is low, so before their selected start time the spectrogram is just amplifying the frequency

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content of the noise, these frequencies are diminished during the flow phase because the actual signal is so much higher, but this makes it look like the frequency content of the signal has actually changed.

3) What the authors actually did in section 3.2 to generate Figure 4 upon which many of their conclusions are based is so poorly explained that I have no idea if it is valid or not. I have many reservations about their methods for assigning segments of the path to specific time intervals of the seismic signal (p683 L5-26), but cannot make a proper judgment because I do not understand where Figure 4 comes from. Also, how do the authors know that some of the different “stages” they identify are not due to other factors like multiple surges, seismic waves from the debris flow in the other valley, material reaching a pinch point/waterfall or other energetic location and so on?

4) The authors make interpretations based on the assumption that the frequency change over time is because of the Doppler effect, however this is not likely because the debris flows are moving so much slower than the seismic wave speeds that the Doppler effect would not be detectable – but if they disagree and want to keep making these interpretations they need to make some calculations to try to justify their interpretation. In my opinion, the upward shift in the peak frequency band is just because the debris flow material is getting closer to the seismic stations so the higher frequencies are less attenuated and make up a larger portion of the overall signal – their normalizing method makes it seem like the lower frequencies are diminished, but really they are just a lower proportion of the total.

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