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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "The 27 May 1937 catastrophic flow failure of gold tailings at Tlalpujahua, Michoacán, México" *by* J. L. Macías et al.

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Anonymous Referee #1

General comments: This is a very interesting paper devoted to reconstruction of a major catastrophic event, which could provide useful knowledge for current risk assessment of tailing collapse. The paper starts with valuable and very complete review of the state of the art, which gives the paper further significance. The event of 1937 catastrophic flow failure at Tlalpujahua has been described in some of the previous papers, but the present paper seems to give new and valuable results. However, the way

in which authors do this, is not always clearly readable from the manuscript structure.

We appreciate the comment made by the anonymous reviewer that we replied as carefully as possible in the following lines.

Much of the paper is written in a descriptive manner, perhaps as a recap of what is currently known about the causes (triggers) and impacts of the event. It is not clearly stated, what are the particular aims of the paper, or how exactly did the authors want to contribute to current understanding of the event. In the abstract, they mention that the major contribution is in confirming the former interpretation of three muddy phases of the flood caused by tailing breach (l. 13-15), but much of the paper is devoted to historical accounts.

The 1937 flood event at Tlalpujahua was up to a certain point well documented because the mining company was very important during the early decades of the XIX century. Most information was available through newspapers and brief descriptions of the event by several people that were inhabitants of the village or mine workers (see historic accounts of Uribe-Salas (1994; 2008; 2009; 2012). However, prior to this study, no one had performed a systematic study of the failure nor studied the deposit trying to link historic accounts with field and laboratory analysis including unpublished photographs belonging to the mining company that were facilitated for this study. Therefore, we will re-write the abstract because we did not meant to confirm that the flood had three pulses, in fact, when we started our study in the field, we did not have a clue that the flood had occurred through three pulses.

I suggest, these comments could be addressed by reorganizing the papers structure, first by stating what has been done and then by defining the particular aims (specific contribution) of this paper. Then, authors should continue with rigour description of methods used. In the current version of the manuscript, methods are mentioned only as brief introductions within the sections describing the results (sections 3-6).

To avoid any confusion with the methods used we include a new section called

“Methodology” to clearly stated the methods used.

The results section could summarize only the major findings of the previous works and should be rather devoted only to the new results achieved by authors in this particular paper (i.e. tailings flood deposits). Section 5 (Mineralogy and chemistry) also brings new data, but its relevance for the paper is not clear from its current version. Moreover, some of the results are currently presented in Discussion (e.g. flow velocities), which is not appropriate. To sum up, it is not clear, which of the sections 3-6 or which of their parts provide original data.

All parts in this manuscript represent new contributions to the knowledge of the flood event, but, section 5 (Mineralogy and chemistry) in which we summarized previous work (as clearly stated in the text) to emphasize that tailings have lower concentrations of Potentially Toxic Elements (PTE) and therefore that do not pose a threat to local population.

Therefore all other sections, but section 5, contain new information regarding the study case. Thus the manuscript certainly used previous data as it was necessary to make clear what is going to be explained or discussed.

Finally, the discussion should gradually and systematically address the reliability and value of new results. Beginning of the section 7.2 (l. 1-15) repeat what has been said already. The most important part of section 7.3, which should be given more detail. The conceptual model of the flood behaviour derived from this part that would extend our current knowledge would be worthy.

The anonymous reviewer is correct section 7.2 (1-15 lines) repeats previous information, thus, it was deleted from the text. We expand section 7.3 giving some more detail of the flood behavior.

Specific comments: Section 6.1, l. 23 – authors assume an average thickness of 1 m for flood deposit and (l. 21-23) give examples of thickness at some of the sections.

Please provide more accurate data (at least list of thickness at all sections). The ERT profiling would be helpful in providing more accurate estimation of the thickness.

We include a list of all sections with locations and details of thicknesses in new table 2. Previous table 2 became new table 3. In addition we included topographic profiles in figure 8.

Section 7.1, l. 10 an. – the recurrence frequency of extreme rainfall totals is not clearly explained. If I understood well, Martínez-Medina et al. (2012) analysed the period of 1972-2002 (continual measurement at the station), but then authors are talking about certainty of two extreme events within this period, one of them evidently occurring in 1937. At the same time, authors note that there are no precipitation data for 1937, thus its extremity is rather an assumption. Moreover, the recurrence interval of 29 years (Martínez-Medina et al. 2012) does not match to the interval between 1937 tailing collapse and extreme rainfall in 1986. In Fig. 13, authors note recurrence interval of 49 years, but again, this is based on their assumption as there are no data for 1937 and statistical analysis that would testify this conclusion is not presented. Please try to rewrite the paragraph to give more precise idea of the rainfall totals and recurrence frequencies as this is fundamental for possible risk modelling.

The anonymous reviewer is correct, we did not have information of the flood time nor from 1937 to 1972. Figure 13 was modified as well as the text.

Section 7.3 - The flow velocities are worthy, but their estimates can be inaccurate. Authors should try to combine more methods, e.g. to reconstruct the discharge as well, as this will give another comparative perspective to the results. They can use some of the existing empirical equations or GIT models (HEC-RAS, etc.). The archive sources are not listed in References. I suggest noting the major sources in Appendix or list of References. Figure 8 – should be at the first place as it provides location of study area.

We calculated the flow discharge at the dam breached, according to the available

information. In addition, some of the dynamic parameters observed in photographs and the field were included. Flow discharge and velocities were calculated (see nre version of manuscript)

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/2/C3087/2015/nhessd-2-C3087-2015-supplement.pdf

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 5361, 2014.

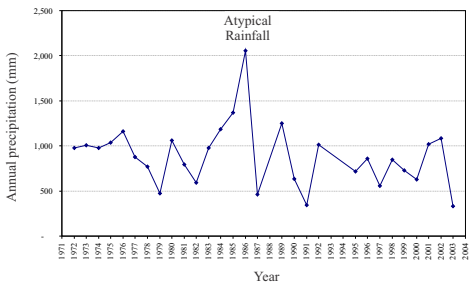

Fig. 1. New figure 13

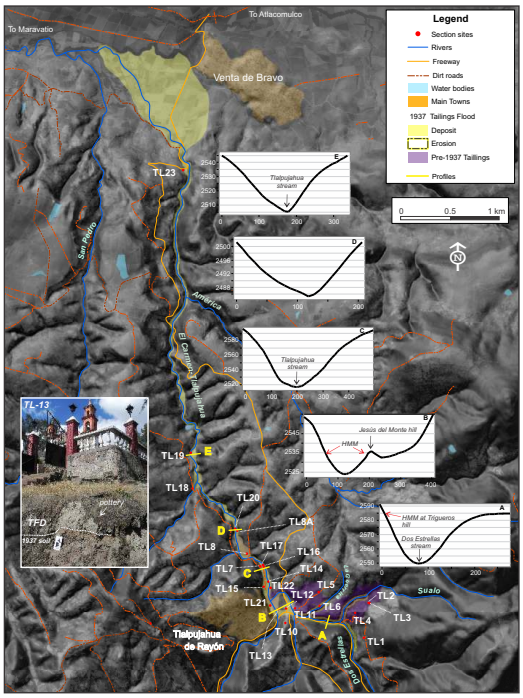

Figure 8

Fig. 2. New figure 8