

Interactive comment on “The catastrophic landfill flowslide at Hongao dumpsite on December 20, 2015 in Shenzhen, China” by Qiang Xu et al.

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Authors would like to express gratitude to the insightful comments from the anonymous reviewer. Please see the responses to the comments.

4. Are the scientific methods and assumptions valid and outlined clearly? Terminology flowslide is confusing. All observations seem to prove that liquefaction was the primary process, followed by progressive backward mobilisation of the dumped mass due to loss of toe support. A sliding plane has not been detected.

The term 'flowslide' was used here on the basis of its definition given by Hungr et al. 2013, in which flowslide involves 'very rapid to extremely rapid flow of sorted or unsorted saturated granular material on moderate slopes, involving excess pore-pressure or liquefaction of material originating from the landslide source...' Considering the sat-

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urated materials in the landfill and the evidence of excess pore-pressure that may have resulted in the slope failure, we believe that the term 'flowslide' is appropriate for this instance.

Authors agree with the interpretation provided by the reviewer, as all observations indeed indicated static liquefaction and phenomena of a more diffuse failure (e.g. no evident master sliding surface). Section 7 presents the numerical back-analyses, and it was used to show the complexity of the failure mechanism as well as the challenges of using back-analyses for liquefaction-induced failures. Such erroneous back-calculated mobilized strength with a measured final sliding surface provides misleading results.

In Section 7, we set the FoS to unity to back-calculate the mobilized strength at failure which yielded an unreasonably low friction angle of 9.4° (as compared to the experimental finding of 31.9°). This is due to the inappropriate use of the static pre-shearing pore-water pressures with post-liquefaction failure surface. And the results of the numerical analysis demonstrated such inapplicability of using the conventional limit equilibrium method to analysis the propagation of diffuse failure. The text in Section 7 has been revised to better illustrate our goals, and it has been combined with section 6 and 5.3 in the previous manuscript.

5. Are the results sufficient to support the interpretations and the conclusions? The concept of liquefaction as main cause should be discussed in more depth.

Section 7 and 8 have been revised to reinforce the discussion regarding the liquefaction that possibly occurred near the base of the slope.

11. Are mathematical formulae, symbols, abbreviations and units correctly defined and used? If the formulae, symbols or abbreviations are numerous, are there tables or appendixes listing them? The accuracy of the data is in many cases a calculation accuracy rather than a real accuracy (see for instance lines 284 – 300). The whole paper should be reviewed to correct for this.

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The dataset which reflects the characteristics of movement, strength, and failure propagation of a landslide is typically back-calculated based on experimental findings in the absence of detailed field monitoring before and after the failure, and thus the accuracy of data would be expected to reflect the calculation accuracy rather than real accuracy.

12. Is the size, quality and readability of each figure adequate to the type and quantity of data presented? Some of the figures are not essential and can be removed (2, 3b, 3c, 3d, 7a, 7c, 7d, 7e, 10a, 10b, 15 or 16, 17a, 17c, 17d, 19a, 19c, 20, 22a, 22b, 22c) some others (7b, 7f, 17b, 19b, 19d, 22d) could be presented at a larger scale to be useful.

Figures have been re-organized and reduced to make the article more concise. Per reviewer's comments: Figure (2, 3c, 3d, 7a, 7d, 10a, 10b, 16, 17a, 17c, 17d, 19a, 19c, 22a, 22b, 22c, 24, 25) have been deleted. Figure (14, 18, 21) were re-arranged and re-grouped with other figures. Figure 3b was not deleted because it shows the relative size of rock fragments in the waste filling. Figure 7c was not deleted as it shows an erosion channel formed by surface run-off near the crown of the flowslide. Figure 7e was not deleted as it shows stagnated water in the flowslide source area. Figure 20 was not deleted as it provides a nice overview of the flowslide deposit. Various minor modification and revision were made.

18. Is there any part of the paper (title, abstract, main text, formulae, symbols, figures and their captions, tables, list of references, appendixes) that needs to be clarified, reduced, added, combined, or eliminated? In the introduction mention is made of a large number of waste disposal site accidents, but from the title of the references it seems that in many cases these accidents have happened in municipal waste dump sites instead of dump sites with construction waste material consisting of silty soil, clay, rock, and gravel which is the case in this paper. The references concerning municipal waste dumps are better removed from the paper. Furthermore in the introduction references to the Hongao dumpsite should be treated separately and not mixed through the references on other dump sites. Chapters 3 and 4 are better placed in the introduction or at

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least before the chapter on Methodology. The introduction should preferably conclude with some general conclusions derived from earlier publications on construction waste dump sites and a list of still open questions which remain to be solved by the research. The introduction should be followed by a special chapter on methodology in which is described which data had to be collected and which methods of analysis were used to solve the research questions defined in the introduction. This chapter can then be followed by chapters on data collected and on results and conclusions. The list of references seems to be unnecessary long, only such papers should be referenced that are used for the solution of the problem, not to show how many papers one has read but were not used. This is a research paper, not a bibliography. Chapters 5.3 and 7 can probably better be taken out completely.

The discussion of MSW landslide is relevant because construction and demolition waste in the article does not contain hazardous industrial waste, and therefore it belongs to the MSW management based on EPA standards (<https://www.epa.gov/landfills/industrial-and-construction-and-demolition-cd-landfills#CandD>), and many states in the US, such as Florida (<http://www.dep.state.fl.us/waste/categories/recycling/cd/canddmain.htm>), treat non-hazardous construction and demolition debris as part of the MSW management.

The brief discussion of the Hongao dumpsite failure gives an overview of the incident, and it may be useful if it is included in the here. The text and figures in this section were revised. Figure 1 is no longer included here and placed into chapter 3 to improve readability.

Shear strength (as well as the interplay between it and other variants) is the essential parameter that is of direct interest of the previous study concerning MSW landslide - which was the reason we mention it in Section 1 and analyzed in depth later in the article. Authors want to prioritize the discussion on the Hongao dumpsite failure other than giving review summary of the past incidents.

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Section 2 provides a rather detailed train of thoughts and methods, and it facilitates the logic flow of the entire paper. This section discuss the data which had been collected and the methods that were applied for the analysis, and thus coincides with reviewer's proposal. . .This section has been revised: section 2.1 and 2.2 are related to flowslide characteristics and landforms, whereas section 2.3 is related to the discussion on the failure mechanism.

Section 5.3, 6, and 7 are combined and named Section 6, wherein Section 6.1 discuss the flowslide movement characteristics by applying empirical correlation to back-calculate the velocity profile to verify the high-speed long-runout failure. The Section 6.2 combines the laboratory test and numerical back-analyses to discuss the complexity of the progress mechanism.

The reference list has been updated as well.

19. Is the technical language precise and understandable by fellow scientists? After a thorough review by a native English speaker with geotechnical background knowledge this should not be problem 20. Is the English language of good quality, fluent, simple and easy to read and understand by a wide and diversified audience? English can be understood, but should be revised by native speaker to improve its quality

The text of the manuscript has been revised.

Please also note the supplement to this comment:

<http://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2016-196/nhess-2016-196-AC1-supplement.pdf>

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., doi:10.5194/nhess-2016-196, 2016.

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