Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2018-406-RC1, 2019

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "From examination of natural events a proposal for risk mitigation of lahars by a cellular automata methodology: a case study for Vascún valley, Ecuador" *by* Valeria Lupiano et al.

Anonymous Referee #1

Received and published: 13 July 2019

The paper describes the use of LLUNPIY cellular automata code for the simulation of lahars occurred in 2008 in Vascun valley (Ecuador). The presented case study and the approach are interesting, but many issues hamper the publication of this manuscript as it is. The most critical point is the language. A complete revision of the text done by an English native speaker is mandatory for adapting this work to an international standard. After a total revision of the language, many scientific issues can be better verified. At this stage, it is not easy to understand if there are several scientific questions and problems that should be solved or verified or if it is only the language that hampers a real

comprehension of authors approach and results. In the following, some comments and suggestions: Page 1 – Abstract: please, revise the language and adapt the abstract to the final version of the manuscript. Sentences like: "such that equilibrium conditions could lack far" are not easily understandable by readers. Page 1 first line of introduction: published works should support this sentence. Page 2, line 11: what kind of threshold? Page 3 line 1: the definition of "external influences" is not understandable Page 3, line 5: phenomenology or phenomenon? Page 3 line 6: again, the definition of "substate" is hard to be understood. Page 3 line 15: "are our top models". This is an autoreferential approach and not a scientific approach. Page 3 line 18: please check the difference between safety and security. I think that the authors wanted to describe safety issues and not security problems. Page 3 line 20 "safety measures can increase the disaster risk in several conditions" this sentence is hard to believe. If it is true, the authors should explain better when and why. Page 3 line 25: risk reduction structures have to be maintained to be efficient. It is clear that, without maintenance, risk reduction infrastructures can create or increase the level of risk. But the problem is not the presence of infrastructures, the problem, often, is the lack of management of them. Page 4, first paragraph: I am not sure that this part should be in the introduction Page 5 line 6: what kind of fresh material? Page 4 line 13: AFM and IGENP activities should be better described also using citations. Chapter 3: the description of LLUNPIY is too limited and it is hard to understand. Some citations are used by authors to describe the LLUNPIY but readers should be able to understand this article even without reading all other cited papers. Chapter 3.2: this part should be rewritten. It is quite impossible to understand this chapter. Can pyroclastic cover mobilization and effect of turbulence considered external influences? If yes, a list of external influences should be presented, and then every element can be described. Chapter 3.3: it not clear if authors considered and simulated 2005 and 2008 lahar event or only (as presented in the title) 2008. Page 10 line 26: considering 5 meters of pyroclastic stratum seems to be a strong approximation. Have the authors considered other thickness to evaluate the impact of this approximation over the final results? Page 11 figure 5: the Authors

describe the level of performance of their simulation, but there is not a real presentation of the difference between the real event and the simulated one. In a scientific manuscript, an evaluation of the performance of the code should be supported by data and not a simple opinion of the authors. Chapter 4 Lahar triggering and effects. This chapter introduces a FEM simulation for slope stability. Readers have to read twelve pages before knowing that authors can also present a FEM analysis. If authors want to use a FEM model, they have to describe the workflow of their activity and make a better description of their research processes and results. A general comment on chapter 4: I am not familiar with pyroclastic deposits, but I had seen many streams affected by debris flow. Slope failures that caused temporary dams are often irregular and heterogeneous deposits. For this reason, if authors want to make a simulation of the temporary dam, the use of a typical geometrical section of an artificial dam seems to be not appropriate. If they want to use this geometry, they have to present better this assumption using filed data and other information. Another important point is the evolution of temporary dams. Many times, the dam break is due to the flow of the water, which fill the small temporary basin and start to flow on the dam deposit. The erosion caused by this process can create an additional destabilizing process that in this simulation, with a static level of the water has not been considered. Chapter 4.2 authors made many hypotheses, but the quality of field data seems to be very limited. That means that many of the proposed hypothesis cannot be really supported by field data. This part is interesting, but authors have to consider the introduction of a validation procedure and a numerical (and objective) evaluation of simulation performances. There is not a real discussion on this manuscript, and conclusions should be rewritten according to the improved version of the text.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2018-406, 2019.