

Interactive comment on “The December 2012 Mayo River debris flow triggered by Super Typhoon Bopha in Mindanao, Philippines: Lessons learned and questions raised” by K. S. Rodolfo et al.

Anonymous Referee #1

Received and published: 12 May 2016

GENERAL COMMENT

I've reviewed the paper “The December 2012 Mayo River debris flow triggered by Super Typhoon Bopha in Mindanao, Philippines: Lessons learned and questions raised”, by Kelvin S. Rodolfo and co-authors. The paper presents several information about the Typhoon Bopha that struck southeastern Asia on December 2012 and on all the related ground phenomena, and damage.

The description of the typhoon is accurate, also from a meteorological point of view. Detailed information about typhoon and related phenomena are provided, together with

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some personal communication from eye-witnesses. The paper is clear, understandable, well-written, and with a distinct structure, although it is too long in some parts.

Nevertheless, the main drawback of this paper, in my opinion, is that it is not a scientific research paper. According to the definition reported in the NHES journal website, a research paper should report “substantial and original scientific results”. This is not the case of this work, which does not propose any original scientific result, any new method, but only a complete description of the phenomena and several considerations about the past a future evolution.

The paper could be classified as a (very interesting and well-compiled) technical report. In fact, nothing about new methods or insights to deal with typhoons and related triggered debris-flows is presented or proposed in the paper.

I suggest to reduce a lot the text, in order to fit the directions of NHES journal for remaining into 4 journal pages, and thus, to re-submit the paper in the form of “Brief communication”. The topic fits perfectly the (c) point of the “Brief communications” descriptions, given that it “disseminate information and data on topical events of significant scientific and/or social interest within the scope of the journal”.

SPECIFIC COMMENTS

Regarding the classification of the debris flows, I agree with the Authors when they state that debris flows could not be classified as floods. Nevertheless, at page 1, rows 25-27, Authors state that: “Some textbooks classify debris flows as a type of landslide, but that term, when used as a synonym for “debris flow”, makes most people mistakenly think of rock masses detaching off a cliff and accumulating near its base.” First, this sentence seems quite unclear to me. Second, Authors should acknowledge that, in addition to textbooks, the most known classifications of landslide types (i.e., Varnes, 1978; Cruden and Varnes, 1996), recently updated by Hungr et al. (2013), do consider debris flows as a type of landslide.

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Concerning the description of the debris flow and of the other triggered phenomena, Authors state that they “mapped out the extent of the debris flow deposits using high-resolution optical satellite imagery acquired through Sentinel Asia” and “in the field, analyzed the new deposits, ascertained that they were indeed left by a debris flow, and found evidence. . .”. Nevertheless, what I can see from the figures and the results is only a contour of the deposit of the triggered debris flow at a very coarse scale, in Figure 2, and some photos of the ground phenomena.

Lines from 5 to 25, in page 4, could be reduced or removed. Section 5 is very long and not so useful, except for some parts, to the discussion proposed in the paper. I suggest revising this section and reducing it. Some historical notes (from page 6, line 27 to page 6, line 13) could be removed from the text.

The description of the event, in section 6, is very detailed and interesting. Perfect, in my opinion, for a good brief-communication on the event. At page 7, line 16, Authors refer to an “Amateur video footage”; it could be included in the supplementary materials, or, if it is published somewhere on internet, the related link could be reported.

The consideration about flow velocity (page 7, lines 19-22) are quite simple and not precise, as also stated by the Authors. I suggest removing it.

Section 7 is very long, with a lot of historical information; they are interesting, but not so useful to the discussion in a scientific paper. I suggest a huge revision and a huge reduction of this section. As an example, lines from 4 to 33 at page 8 could be reduced or removed.

Sections 8.1, 8.2, and 8.3 are interesting but very long. They could be reduced. I suggest to join sections 7 and 8 in a new section, namely a “Discussion” section, reducing them and keeping only the necessary parts. Finally, I suggest removing section 8.6.

Concerning the relation among typhoons, climate change and ground phenomena in tropical areas I suggest considering the work made by Chiang and Chang (2011). They

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stated that in the western North Pacific, typhoons and rainfall rates are predicted to intensify because of climate change. As a result, they predict and increase in shallow landslides.

TECHNICAL CORRECTIONS

Everywhere in the text, I suggest using abbreviations for measure units, e.g., “km” instead of “kilometers”, “m” instead of “meters”, etc.

Page 3, Line 26: Check references. Some commas are missing and there are two closing parenthesis.

Figures: I would suggest using colours for the photos.

REFERENCES

Chang, S.-H., Chiang, K.-T., 2011. The potential impact of climate change on typhoon-triggered landslides in Taiwan, 2010–2099. *Geomorphology* 133, 143–151, doi:10.1016/j.geomorph.2010.12.028.

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Hungr, O., Leroueil, S., Picarelli, L., 2013. The Varnes classification of landslide types, an update. *Landslides* 11(2), 167-194, doi: 10.1007/s10346-013-0436-y.

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., doi:10.5194/nhess-2016-102, 2016.

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