

Interactive comment on “Regional-scale analysis of high-mountain multi-hazard and risk in the Pamir (Tajikistan) with GRASS GIS” by F. E. Gruber and M. Mergili

Anonymous Referee #2

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The article deals with an interesting application of GIS techniques to high-mountain areas, for the evaluation of different types of geohazards. It is well structured and presented, but it has several unclear points that it will be necessary to adjust in order to make it acceptable for publication. I found several problems in the terminology used by the Authors, especially as regards the terms hazard and risk. As for the first one, I have to observe that time of occurrence of the phenomena is never dealt with, which is a significant drawback in the definition of hazard. Actually, no hazard assessment can be performed without considering the likely time range of occurrence. Thus, the Authors should clarify what they do define as hazard, and possibly use an

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internationally-accepted definition, instead of creating their own. The same problem occurs with the term risk: this is generally defined as the product of vulnerability and hazard, and requires socio-economic evaluation of the considered elements at risk. The first problem is that Authors only consider as elements at risk the villages, excluding the roads and communication routes. In my opinion, this is too simplistic, since communication routes are crucial in mountainous areas (both as regards normal link among villages, and emergency and rescue operations), and they should be taken into account, too. If not, Authors should clearly state the reasons why they decided not to consider this element at risk, and justify their choice. Then, no economic consideration is presented, with assessment of the value of the single elements at risk. This makes, at most, the risk evaluation a qualitative one. This, too, should be clearly stated, since it is a limit of the work. Overall, I have the feeling that very few "real" data on occurrence of the considered phenomena are available to the Authors, and most of their work consists essentially of GIS analysis. This makes in some way quite weak the article, and has as main consequence the poor results, as also observed by the Authors themselves. Eventually, figures appear to me excessive in numbers, and in some cases too complex to be followed by the readers. I suggest to reduce their numbers, by making a careful selection and choosing those essential for the article, and to simplify the more complex ones. For all the reasons outlined above, and also the other observations made in the accompanying pdf file, I ask for major revision, hoping that the above comments may be of help to the Authors to improve their work.

Please also note the supplement to this comment:

<http://www.nat-hazards-earth-syst-sci-discuss.net/1/C884/2013/nhessd-1-C884-2013-supplement.pdf>

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