

## ***Interactive comment on “The large-scale assessment of avalanche risk for ski resort areas in Northern Caucasus region” by A. Y. Komarov et al.***

### **Anonymous Referee #2**

Received and published: 21 April 2016

Referee's report on nhess-2016-68, The large-scale assessment of avalanche risk for ski resort areas in Northern Caucasus region

The authors present a regional risk assessment for snow avalanches, focusing on the scale of ski resorts in the Northern Caucasus, Russian Federation. They compute the individual and collective fatality risk based on collected data, estimated amounts of elements at risk (skiers) and avalanche simulation performed with RAMMS. As such, the topic is of considerable interest to the readers of NHESS, and the topic should be considered for publication.

However, there are some shortcomings in the current version of the NHESSD paper which I will address below. These shortcomings should be considered by the authors

C1

before the manuscript may become acceptable for inclusion in NHESS.

The main point of criticism is related to the main message of the paper: if people on ski-runs only would drive fast enough downhill, their individual risk to die in a snow avalanche would decrease. I strongly believe that such messages are against ANY common sense, because this would mean that “if car drivers would only drive fast enough, they will never experience a car accident”.

Despite this rugged statement, the method applied seems to be reliable to compute individual and collective risk, but I have some concerns described below in a chronological order.

#### **Abstract**

Page 1, line 15: “. . . should take place during the engineering surveys of the object” -> please specify, this is not clear to me

#### **Introduction**

Page 2, line 2: Please start the introduction with more general statements funneling down later to the case study of the Northern Caucasus. IT may be worth to check available literature on tourism infrastructure and risk assessments, and then come to the case study. The statement of increasing visitors during the last years needs a Citation.

Page 2, line 11: “The special avalanche and snow observations are almost absent [in] Veduchi. . .” -> needs clarification, I do not really understand. Same with “That the formula test results should display some notable deviations for each of the regions as well.”

#### **Section 1.1**

Here I would like to suggest a map showing the readers the location of the test sites.

Table 1: From the information provided in relation to the cyclone frequency I cannot

C2

deduce the differences between the three case studies mentioned on page 2, line 19 – the frequency is between 34% and 37%.

### Section 1.2

This section on previous studies needs clarification, for details please see the attachment. The main points are:

- Where do the numbers for “appropriate”, “acceptable” and “unacceptable” risk have their origin?

- Page 4, lines 16-22: This information should go to the Methods section, and needs more specifications beyond “using certain correlation dependencies. . .”

### Section 2

Page 4, lines 24 ff: I would like to suggest a more proper reference to available literature on snow avalanche risk, including the “classical” risk equations (risk = hazard x vulnerability or risk = hazard x elements at risk x vulnerability) and how these approaches rooting in risk evaluation for either settlements or traffic infrastructure were modified in order to meet the requirements to compute individual fatality risk. Moreover, it seems debatable why the 1/100 year avalanche is used during computation – in the European Alps, the more frequent but smaller avalanches seem to be the challenge for individual risk of skiers.

Page 4, lines 28 f: From my point of view, vulnerability to snow avalanches is related to the impact pressure – and then secondary dependent on the characteristics of elements at risk. For skiers, however, the use of impact pressure would be misleading. As such it would be useful to compare the approach presented to a more empirical approach: Wilhelm (1997, p. 76), to give an example, shows the probability of dying in an avalanche by using data collected by the Swiss Federal Institute of Snow and Avalanche Research and he concludes that the probability of death is 32 %. It may also be worth to check the studies by Keiler, Fuchs, Sokratov and Shnyparkov related

C3

to the short-term avalanche risk (Fuchs et al. 2013; Keiler et al. 2005) – I just mention this because some of them are acknowledged in the end of this manuscript.

Equation (1) needs careful interpretation because (1) the avalanche hazard is variable over time and (2) during the summer months there may not be any risk due to the closure of the skiing areas. As such, the 365 days should be re-interpreted.

Equation (2) relies on the assumption that the number of people on a ski slope are homogenously distributed, which may not be the case due to the network of ski runs, the ski lift infrastructure as well as other infrastructure such as shelters and mountain restaurants. This could be addressed also in the discussion section.

Equation 3: The explanation on page 5, line 16 is not clear to me, and needs specification. Moreover, why did you use a 66 % change to die, where does this data come from? The statement on data made in lines 16-20 could be better supported by published material rather than by “analyzing the laboratory material”.

### Section 3

In general lots of material presented here is a repetition of the methods section. The authors could (and should) focus on the results instead. I am missing a sound description of the materials presented in Tables 3-7. My main concern about speeding the ski-run in order to survive was already introduced above.

Equations (3) and (4) were already presented in the methods section, moreover, Equation (4) in the results section makes use of different indices. The information given on page 7, lines 5-6 is inconsistent to page 5, line 26.

Page 7, line 10: primary and secondary measures needs clarification, as well as the sentence on “active influence on snow using different methods”.

### Section 4

The statement on page 7, lines 20-22 needs clarification; what do you mean when

C4

writing “the spatial distribution [of what] shows a good correlation with the training levels of sportsmen. Using the materials [which] and official statistics [which are. . .] (. . .)” the density of people was determined? Moreover, I kindly would like to suggest that the authors clearly address the limitations of their study, for example with respect to the homogenous distribution of people on the ski runs, the precision of RAMMS, the temporal aspect that during the weekend and during holidays there may be more people present, etc. It is also worth to think about the general small risk values for the individual snow avalanche risk (Table 4) in comparison to other risks such as traffic accidents in Russia or health risks. From Table 6 it can be concluded that we should prohibit skiing for beginners, intermediate skiers and even professional skiers due to the distribution of snow avalanche velocities – in almost two third of the area the snow avalanche speed exceeds the speed of professional skiers which means that finally 2 out of 3 will not be able to escape.

More remarks are given in the supplement to this review. Further the manuscript needs a proper proof-read by a native speaker, there are some classical errors from the Russian translation (e.g., the missing articles) but also the overall wording should be improved in order to enhance the readability.

#### References mentioned

Fuchs, S., Keiler, M., Sokratov, S. A., and Shnyparkov, A.: Spatiotemporal dynamics: the need for an innovative approach in mountain hazard risk management, *Natural Hazards*, 68, 1217-1241, 2013.

Keiler, M., Zischg, A., Fuchs, S., Hama, M., and Stötter, J.: Avalanche related damage potential – changes of persons and mobile values since the mid-twentieth century, case study Galtür, *Natural Hazards and Earth System Sciences*, 5, 49-58, 2005.

Wilhelm, C.: *Wirtschaftlichkeit im Lawinenschutz*, Mitteilungen des Eidgenössischen Instituts für Schnee- und Lawinenforschung, Davos, 309 pp., 1997.

C5

Please also note the supplement to this comment:

<http://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2016-68/nhess-2016-68-RC2-supplement.pdf>

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Interactive comment on *Nat. Hazards Earth Syst. Sci. Discuss.*, doi:10.5194/nhess-2016-68, 2016.

C6