Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2018-234-AC1, 2018

Natural Hazards and Earth System Sciences
Open Access
Discussions

Interactive comment on "Integrated risk assessment due to slope instabilities at the roadway network of Gipuzkoa, Basque Country" *by* O. Mavrouli et al.

O. Mavrouli et al.

o.c.mavrouli@utwente.nl

Received and published: 12 November 2018

Q1. The objective of this manuscript concerns the risk assessment characterizing a roadway network affected by different hazards in Spain. For this, a "hybrid" quali-quantitative method mainly expressed in monetary terms has been proposed (as a multiple of a cost unit amounting to 1,000 EUR). The failure probability is evaluated with reference to four types of geotechnical hazards (rockfalls, anchored wall failures, slow landslides, and sea wall failures), whereas the related risk level is based on an economic criterion expressed as the average annual repair cost, at the considered section. The annual ïňĄnal risk is given by the product of probability/frequency oc-

Printer-friendly version

Discussion paper

currences of events, in assigned magnitude classes, per the monetary value of the elements at risk. Even though the work is interesting, there are some questions which I would like to highlight.

A1.We would like to thank the Reviewer 1 for his/her comments and insights, which helped us a lot to improve the quality of the manuscript. We tried to address all comments and suggestions. Please find below the point-to-point response.

Q2. I don't understand why the Authors put together the rockfalls (natural hazard) with the failures of man-made structures such as anchored walls, road platforms and sea walls. It is clear that these failures can be caused by natural causes linked to the slope movements but, in such a case, I would have expected the analysis of failures of passive retaining structures (barriers, fences, etc.), due to rockfalls.

A2.The hazards that were addressed at this work are the hazards that are mostly encountered in the road network of the study area, and are related to the major intervention costs. The challenge in this case, and the motivation for this work was indeed to analyse all hazards at a common scale, so as to prioritize those road sections that need urgent interventions against other where the risk is lower. Putting together all these hazards and comparing the related risk has been the challenge due to the fact that the hazards generated by these processes follow different temporal patterns and intensities have different type and extend of consequences. To the author′s knowledge this is the first time that risk from different hazards is calculated in a quantitative way. This is extensively explained in the introduction.

Q3.Another big concern is for using heuristic methods in order to calculate the annual P/F occurrence for addressing the lack of adequate data. It seems to me that sometimes they are based on not adequately justiïňAęed or tested expert judgments. This is particularly true for rockfalls and slow landslides types, where these approaches are used in order to overcome the lack of complete rockfall and landslide catalogues. In this respect, the Authors extend the frequency/magnitude relationships, calculated for sites with adequate data, to slopes with similar geostructural characteristics, scars, heights and block sizes (see on page 9, line 14).

A3.The approaches followed, except for the risk related to sea walls, is not heuristic, instead it is based on analogs and proportionalities for calculating the probability of occurrence of the events of a given magnitude/intensity which is assessed based on quantitative data. This procedure is not heuristic. In any case, quantitative analysis can be based on expert judgment as well. Most of the procedures for risk assessment are based on analogs. As for example, the most well-known approach, which is the Rockfall Hazard Rating System follows the same logic, as it assigns the same level of risk to slopes showing similar geometric and geomechanical features. In our work, for the rockfall hazard, we followed such a logic, starting from the slopes with F-M available and assigning these values to slopes showing similar characteristics. This assumption is not restrictive but justified on the grounds of geomechanical characterization and its relation to the hazard, as for the RHRS mentioned above. A similar approach is followed for the slow-moving landslide, where we try to identify their pattern movements and their relation to the triggering factor (rainfall in this case), the latter with a known recurrence. This procedure is in the same line with the calculation of the occurrence of landslides, based on the recurrence of the triggering rainfalls, which is widely used.

Q4. With reference to rockfalls, the scoring assignment for the frequency index (IF) calculation involves ïňĄve frequency indicators both qualitatively and quantitatively deïňĄned. In my opinion, this is a hybrid approach linking qualitative terms to quantitative data. With reference to the Differential Erosion indicator please, clarify what is the used ïňĄgure for the score 2 (No, Yes, and...?). Then, the scores are summed up to calculate IF (see eq. 2), and a relationship between IF and the rockfall frequency, for enabling the assessment of thresholds, has been established. For this purpose, a calibration has been performed. I thought it would be interesting if the Authors would speak to us the calibration procedure and results.

A4.The index IF is not a frequency indicator but an indicator corresponding to the geomechanical characterization of the slope (please see also previous answers), thus is used to provide 5 different geomechanical characterizations, which after that, they are assigned with a frequency, considering that slopes in the same geomechanical class have common frequency and equal to the one available in some of the sections. Frequency is obtained quantitatively and not qualitatively. The authors recognize that in the way that the text was written, the term "calibration" is not representative of the procedure explained here, which is the followed procedure. For this we rephrased it in the text. On the contrary of what the reviewer mentions here, we do not link qualitative terms to quantitative data, but vice versa, we link quantitative data (frequency) to already defined qualitative descriptors (geomechanical class). We reference to the differential erosion, if it is present the score for the IF is 1 and if there is not it is 0, which means that it does not add to the IF. It is not the scores 0, 1,or 2 which are assigned to the indeces, but the indices get qualities that correspond to a score. In this sense the qualities of the index IE are no or yes, which correspond to the scores 0 or 1, and 2 is not relevant here, that is why there is not quality in the relevant. We added "not applicable" to avoid confusion.

Q5. Also with reference to the correction factors (Fr) assigned to different protection measures, it seems to me that this was done to ensure too high safety standards (See Table 6). I understand that, lacking in literature adequate tested values, the Authors adopted precautionary data but (e.g. see the tunnel case) assuming a range of values between 4 and 2, for magnitude classes A-D it seems to me too high!

A5. As also mentioned in our previous response, the correction factors were assigned to different protection measures based on the observed frequencies in the study area, and in particular at the sections that this data was available. Volkwein et al. (2011), report capacity for energy absoprtion about 3000 kJ, without cushion materials, and about 5000 with it. These capacities are sufficient to retain most volumes of the categories A and B, considering the potential energy of the rock, for falling heights of few tens of meters, as the heights of the rocky slopes in the area,. For magnitudes

C and D, given the fragmentation of the initial rock mass a percentage of the blocks is expected to be retained. Reference: Volkwein, A., Schellenberg, K., Labiouse, V., Agliardi, F., Berger, F., Bourrier, F., ... & Jaboyedoff, M. (2011). Rockfall characterisation and structural protection-a review. Natural Hazards and Earth System Sciences, 11, p-2617.

Q6. Similar considerations must be done for the failure of retaining structures, where the HI factor is only based on subjective evaluations and expert judgments. Hybrid data (qualitative and quantitative in nature) is also reported in Table 7. What means sound rock or mixture? For instance, is it adequate the use of the Schmidt hammer or pocket penetrometer for their characterization? As the Authors area well-known team working in the ïňĄeld of the hazard and risk assessment, I think that they must be very wary of suggesting not well-tested approaches because they have big authority in this very poorly explored research ïňĄeld.

A6. For assessing the probability of failure of retaining structures, we adopted the approach proposed by Silva et al. 2008, who argues that the probability of failure of structures can be assessed, considering besides the Safety of Factor, the quality of the construction. Similarly the HI factor is calculated based on an objective indicator model, including data from the load cells, and the specifications of the technical project. The approach is not based on expert judgement, but clear criteria. Sound rock or mixture refers to the ground where the structure is anchored, and is used as a factor to measure effectiveness and uncertainties for the support of the anchors. Schmidt hammer is not used. We are based on the geotechnical reports of the works.

Q7. Concerning the slow movements affecting the roads, might the slight/moderate damage on the road be due to shallow subsidence of the subgrade? (as it appears to me by some photos). Since the inclinometers are very often affected by installation problem, or malfunctioning causing lack of data have you been performing a reliability analysis?

A7. We would like to clarify that in all the cases analysed here, the road traverses well identified landslides, as such movements are associated to landslide activity. This part was added to the text. The methodology that we propose here for the evaluation of the probability of landslide reactivation based on the indications of the inclinometers can be applied given that the inclinometers function well and are reliable. Indeed, in the study area there are some inclinometers which are inoperative or not reliable. However, the analysis presented here in this paper, refers to those sections with good inclinometer data. Please not, that according to yours and other Reviewers's suggestion, we added up some text in the Discussion section, on the reliability of the inclinometers.

Q8. For the failure of sea walls, I think that in an oversimpliïňĄed way the procedure for the PIP index calculation is evaluated. As it is well know, the undercutting by waves is very important in causing the sea-cliff retreat or wall failure (mainly for toppling). Waves erode the cliff toe, undercutting and over steepening it. This destabilizes the overlying slope, causing it to collapse. Also with reference to the sea walls, the main failure mechanism is linked to erosion by waves. Consequently, the main factors affecting this failure process are the real dynamic pressure exerted by the water at the wall toe, the mechanical strength of concretes and design characteristics. As these quantities are very difïňĄcult to assess, generally the research approach uses aerial or satellite photos, topographic survey comparisons, LIDAR techniques, etc. Also the on-shore wave characteristics and meteorological observations in time and space are needed. With reference to eq. 7, and according to my opinion, the protection mass index (Mp) already should incorporate the correction factor (Fc) for the protection structure. What do you think? I suggest that you remove this hazard from the text.

A8. The reviewer is right, and we also on removing this hazard from the new version of the manuscript.

Q9. In conclusion, the suggested approach must be considered as a ïňĄrst attempt that cannot be extended to areas with different geotechnical and geomechanical characteristics, respect to the studied ones. The study conïňĄrms that a reliable quantitative risk analysis involving man-made structures can be performed using reliable and numerous data only. Otherwise and for wide areas, only heuristic approaches based on expert judgments can be used. But the question we have to ask is whether it is worth using complex procedures which incorporate not yet well-tested ratings.

A9. The authors would like to thank again the reviewer for the comments. For the reasons exposed in the introduction we think that quantitative risk analysis for roads, involving manmade structures is of wide interest. To the authors' knowledge, this is the first time that risk from different hazards is calculated in a quantitative way. Of course, several challenges exist which are also exposed in the introduction. It has to be mentioned that in the study area, there is an extensive and systematic collection of data, which has allowed the development of the proposed procedures, not always available in other study areas. Given the existence of this data, we think that the development of the approaches presented here, even if they leave a margin for refinement and validation is the added value if this work, permitting objectivity and reproducibility in the assessment. However, to avoid any misunderstanding we stress again in the conclusions that the procedures were development here given the local characteristics and available data in the area, and that direct transfer of the results to other study areas, without prior study of the local characteristics, is not recommended.

Q10. (see Table 6 – please, check the correct numbering of all tables and ïňĄgures; the ïňĄgure 4 is several times duplicated!)

A10.The numbering of Tables and Figures was checked and adapted to the changes in the text. In our version, we do not see Figure 4 duplicated.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2018-234/nhess-2018-234-AC1-supplement.pdf

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

2018-234, 2018.