

## ***Interactive comment on “Estimating the risk related to networks: a methodology and an application on a road network” by Jürgen Hackl et al.***

### **Anonymous Referee #2**

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The authors address the topic of network infrastructure exposed to natural hazard risk taking the road network around Chur, Switzerland, as an example, and as such, the topic is of relevance for the target journal. Nevertheless, the manuscript shows some weaknesses which are discussed below.

The manuscript takes a systemic viewpoint by introducing the individual steps of risk analysis for networks (which is not a new task, by the way). Nevertheless, a sound review is missing, which makes it difficult to judge whether or not the authors provide added value to the ongoing discussions of this community (some journals are particularly dedicated to network risk and transportation infrastructure, and it would be good

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to see the application of the authors in relation to other approaches coming from the hazard community).

With respect to the hazard type, the authors are not consistent in what process type they model (Debris flows? Landslides? Floods?). In one section, the study seems to be limited to one process type only, while the Figures clearly indicate different hazard types (“The purpose of the assessment itself was to quantify the risk of a complete chain of events over space and time, from source events to their consequences, considering: rainfall, runoff, flooding, mudflows, physical damages and functional losses to bridges and roads (road galleries, tunnels and other structures were not of primary concern), traffic changes and restoration works”). Depending on the different hazard characteristics (floods – spatial extent and duration, debris flows and landslides – local occurrence and interruption, maybe damage to the road infrastructure, mudflows – do they really occur in the Chur region?) it remains unclear what exactly the impact will be. Even if they use respective models (and maybe just take the results as an input for network interruption), this should be clarified. The same holds for the determination of direct and indirect effects. It would be better to restrict this study on a clearly named sample of effects so that the potential readers can easily follow the concept.

With respect to the traffic flow model it remains unclear what exactly the input data is. Moreover, I cannot accept a contributions stating that “The methodology is described in detail in Hackl et al. (2016)” simply because the potential reader should get an overview by reading through the current contributions. So at least the main steps have to be summarized here. As such the entire manuscript reads more like a report than a scientific paper – also because of the repetitions that are presented in multiple sections (Floods are a challenge throughout Europe..., just to provide one example).

Irrespectively of the models used, it would be good to only focus on the network reliability (because a blockage can be of any origin, even road closure due to the WEF in Davos, to give another example).

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The title of this contribution suggests that the focus is on network reliability, not on the modelling (and more or less proper representation) of hazards affecting the road network. This would also sole the issues addressed in the first part ("too many scenarios..."). So methodically, the focus should be placed on the following three items: (a) object-level functional losses leading to network functional loss, considering the topology of the network, (b) object-level physical damages and object-level functional losses leading to restoration works, considering specific restoration criteria, and (c) network functional losses leading to changes in traffic flow, supported by an origin-destination matrix.

Sentences such as "Simulation-based risk assessments require the coupling of multiple heterogeneous models, where a given model encapsulates the behavior and state of a part of the system" do not provide added value – if the entire manuscript could be re-structured using a classical scheme these statements can go to the introduction and have to be underpinned by appropriate references.

The underlying traffic model is tricky and should be described in more detail. What exactly is the data used, and what are the assumptions? As far as I know, Chur is an economic hub for the entire region; so how did the assumptions made mirror the real behavior of commuters? This is central since when the authors compute risk, these figures define us the exposure.

So my suggestion is to re-organise the manuscript in the following way: Introduction: sound review of transport network risk to natural hazards, then categorizing the different approaches in network representation and modelling, then presenting the added value based on a identified gap) of the chosen model. Method description of the model and the main assumptions (data used). Results Discussion: limitations, uncertainties, etc.

Additionally, references are outdated, e.g., Eidsvig, U. M. K., Kristensen, K., and Vangelsten, B. V.: Assessing the risk posed by natural hazards to infrastructures, *Nat.*

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*Hazards Earth Syst. Sci. Discuss.*, pp. 1–31, doi:10.5194/nhess-2016-89, 2016 has already been finally published.

Moreover, references to the transport infrastructure are biased towards the contributions of the authors, here we need a broader review and a clear statement of research gaps and needs, and niches to be filled by the current contribution.

English needs a sound proofread by a native speaker.

Given the shortcomings of the current version, I kindly would like to suggest a rejection and encourage the authors to re-submit once the storyline is clear and focused on what has been promised in the title. Alternatively, the authors may wish to choose an alternative journal, presumably from the network modelling community.

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Interactive comment on *Nat. Hazards Earth Syst. Sci. Discuss.*, <https://doi.org/10.5194/nhess-2017-446>, 2018.

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