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Reply on RC2

Yi Victor Wang and Antonia Sebastian

Author comment on "Equivalent hazard magnitude scale" by Yi Victor Wang and Antonia Sebastian, Nat. Hazards Earth Syst. Sci. Discuss.,
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October 22, 2021

Reviewer

Natural Hazards and Earth System Sciences (NHES)

Subject: Final response for article "Equivalent Hazard Magnitude Scale" with manuscript number nhess-2021-87.

Dear Referee,

Thank you so much for your careful review of our manuscript (nhess-2021-87) entitled "Equivalent Hazard Magnitude Scale". To address your comments, we attach below our responses in a point-by-point style.

We look forward to hearing back from you regarding our response.

Sincerely,

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Anonymous Referee

We thank you very much for your constructive comments and insightful suggestions. In the following, we copy your comments and follow with our response.

Comment: The paper introduces a new magnitude scale (the Gardoni scale) to describe the impact of different types of natural events and to facilitate the comparison.

Response: Thank you very much for your summary. However, the objective of the paper is not to describe the impact of events. Instead, we propose the Gardoni Scale to directly compare the hazard strengths of events across hazard types. The hazard strength of an event is not the same as the impact of the event, as the impact is associated with not only the hazard strength but also the values exposed to the hazard strength and the vulnerability of the exposed entity to impact. By using a large sample size of data with a good quality, we can assume that the factors of exposed value and vulnerability have been controlled for such that we can use the observed impact to calibrate a regression model to quantify the hazard strength as the expected impact given average exposed value and vulnerability. Once such regression models are established for each hazard type, they can be used to derive the equivalent hazard strengths for direct comparisons across hazard types.

Comment: Although I do agree with the main idea of the paper, i.e., hazards cannot be compared but we can compare their effects, I have several doubts about this paper.

Response: Thank you very much for your encouragement and comment. The comparison of hazards often involves the computation of the expected frequency of or the expected exceedance frequency of hazard strength for a given hazard type. In this paper, we aim to derive an estimate of hazard strength independent of hazard type. The main idea of the paper is that hazard strengths can be compared. First, hazard strengths can be compared within each hazard type. Agentially, for example, an M7 earthquake on the moment magnitude scale is larger than an M5 earthquake in terms of hazard strength, while the effects of the M5 earthquake can be much more severe than the effects of the M7 earthquake. Locationally, as another example, a community surrounded by a water depth of 2 meters is experiencing a much larger hazard strength of flood than another community facing a water depth of 0.5 meters. However, due to different pre-event mitigation efforts, the community with a 2-meter water depth may be impacted much less than the community with a 0.5-meter water depth. On the other hand, comparison of hazard strengths across different hazard types is difficult with the mainstream methodologies used in the existing multi-hazard studies. In light of this, the main academic contribution of this paper is that we propose a scale that enables cross-hazard comparison of hazard strengths in an agential and durational manner. We have also demonstrated how to compare the agential-durational hazard strengths across hazard types in this paper.

Comment: I will describe below only the most important ones (omitting other minor points), with the hope that they can be of some usefulness for the authors.

Response: Thank you very much for your encouraging comment.

Comment: 1. As just said, hazards cannot be compared but we can compare their effects; this is exactly what the risk analysis is meant to do. There is an extensive scientific literature on the comparison of the risks caused by different events (e.g., comparing the individual risk of death caused by different events), or comparing the risk with the acceptable risk that has been defined by decision makers. It is not clear why the authors dismiss completely all these efforts, which have eventually their same goal. Why do they think that their method is more effective than the classical risk and multirisk assessment?

Response: Thank you very much for your comment and question. As mentioned previously, the focus of the paper is on quantification of the equivalent hazard strength. As such, the objective is not to perform a risk analysis. Instead, we are deriving a new indicator: equivalent hazard strength, that is derived from impacts given average exposure and vulnerability based on a robust record of historical impacts. While we agree that it is important to understand how different factors contribute to overall risk, this is not the goal of this paper. Our proposed scale has applicational significance providing benchmark measures of hazard strength for vulnerability and resilience analyses. In addition, the derived equivalency of hazard strengths can be used to create multi-hazard hazard maps to show the distribution of exceedance probability of hazard strength across different hazard types.

Comment: 2. The authors based their analysis on a 120-year-long database. I think that the length of this database is clearly too short to get a realistic estimation of the impact of some natural threats, which have a longer average inter-event times (for instance super-eruptions with VEI7 or 8). That's important because the effect of one of such events can largely overcome the cumulative effects of all other events. As a matter of fact, for some of the hazard considered in this paper, the most impacting events at worldwide scale have a return time that is much higher than 100 years. This is also the reason for what the risk is almost never empirically calculated using databases of this time length, at least for the most damaging events.

Response: Thank you very much for your comment. As mentioned previously, the objective of the paper is not to quantify the effects of events but to demonstrate the computation of the equivalency of hazard strengths. Having said this, we do agree that the length of the database used for our study may not be long enough for comparison across hazard types that occur infrequently. However, as the purpose of the paper is to propose an empirical method to derive the equivalent hazard strength across hazard types, the EM-DAT database is sufficiently robust for demonstration of the proposed method. When other databases with higher quality and longer length become available, we intend to use them to improve our model results. In terms of the issue associated with the return period, the return period is always positively correlated with the hazard strength independent of the hazard type. Analysis of return periods for small to medium hazard strengths provides evidence to extrapolate the return period for large hazard strengths that are rarely experienced. When dealing with large hazard strengths, it is common even for singular hazard strength scales to have trouble in revealing the hazard

strengths. For example, all the earthquake magnitudes are known to have saturation issues, more or less to some degree, for large magnitudes. Therefore, it is expected that the derivation of equivalency of hazard strength for large hazard strengths may be less reliable than for smaller hazard strengths. Since most of the hazard damages communities experience are caused by events with small to medium hazard strengths, we believe that it is useful and significant to derive hazard equivalency even for small to medium hazard strengths. In addition, to what extent the computation of equivalent hazard strength becomes unreliable given a certain return period is beyond the scope of the current study. Future work should explore the effect of long return period on the estimation of hazard equivalency.

Comment: 3. I think that the exposure and vulnerability are strongly changing through time. Conversely, the authors are assuming that these quantities remain constant in the past 120 years. This assumption may introduce a significant bias in the ranking of the events; for instance, it may be argued that the same tsunami in 2004 would have caused much less casualties if it happened in 1904 (by the way, to my knowledge the number of casualties caused by the 2004 tsunami is much less than 2 millions as reported by the authors). Not less important, as also acknowledged by the authors, some of the data may be severely incomplete; incompleteness has to be carefully checked because it can introduce an important additional source of bias in the analysis.

Response: Thank you very much for your comment. We agree that exposure and vulnerability are changing through time. However, if such changes occur consistently across different hazard types, these changes are unlikely to affect the computation of equivalency of hazard strengths. By using the data of 120 years throughout the entire world, we assume that each hazard type has a similar temporal distribution of exposure and vulnerability, as is common in multi-hazard disaster research. Future work could examine whether this assumption holds true. Meanwhile, we also agree that there are some issues in the data from the EM-DAT database. In particular, there are many hazard types, such as volcanic hazards, that do not have measures of hazard strengths in the database. In addition, as also highlighted by the referee, there may be inaccurate records of disaster damages. In this study, we performed a quality control to exclude some obviously impossible values. We also excluded data points without hazard strength indicator values and performed a principal component analysis to support the derivation of the impact metric for data points with missing values for some, but not all, of the impact variables. However, we would like to point out that EM-DAT is a world-renowned database for hazard events and is one of the few open-source databases readily available to researchers and is often used for hazard analyses.

Comment: 4. I am puzzled by the inclusion of synthetic data to fill the "missing" data. This may be very dangerous, because the 'new' data have been generated assuming that the model used to generate them is correct. To sum, I do not understand the need to generate synthetic data and not using only the ones available. (but I may be missing something here)

Response: Thank you very much for your very insightful comment. We agree that using synthetic data always requires extreme caution. Therefore, we only included data points without missing values of impact variables and data points with missing values of one or two, but not all three, impact variables. There are no synthetic data points involved in the study. The purpose of filling the missing values of the partially incomplete data points is not to generate synthetic data points, but rather to form a mathematical mapping between the one or two impact variables with recorded values to the impact metric that is

the principal component of the three impact variables derived with the complete data points.

Comment: 5. The results of the correlation between impact metric and hazard (Figures 3 and 4) are largely not statistically significant (maybe except in a very few cases case, but we need also to take into account that the statistical significance has to take into account also the multiple tests). It is difficult for me to understand how we could use these relationship to rank the hazards in a meaningful way.

Response: Thank you very much for your comment. We agree that many of the estimates of model parameters are not statistically significant after the calibration of the regression models. However, the purpose of the regression modeling is not to provide statistical inference between hazard magnitude indicators and impact metric, nor is it to make predictions of impact metric with hazard magnitude indicators. Instead, the purpose is to provide a computational tool to map the hazard magnitude indicators to the equivalent hazard magnitude, which is correlated with the expected value of impact metric. In this sense, the lack of statistical significance or the wide spread of data points is not a problem for the computational methodology for deriving the equivalency of hazard strengths. In the paper, we present the statistics of model parameters mainly for reproduction of research results.

Comment: Figure 4 shows that, on average, the smaller the event the lesser the impact. This is already very well known but the large scatter of the logarithmic quantities implies that, for example, a large earthquake can cause no victims whereas a smaller one can cause a huge number of casualties. It depends on where the earthquake occur. For example, on average about 20 earthquakes of magnitude 7 or above occur worldwide per year, but only a very few of them in the last century caused more than 100,000 casualties, whereas most of them do not produce any casualty, or very few; the scatter in terms of casualties spans about 5 orders of magnitude for such a kind of events. This is a consequence of using an 'agential' approach, whereas the risk is intrinsically 'locational' (de facto, the exposure is strongly spatially clustered over the earth).

Response: Thank you very much for your careful observation and insightful comment. We do agree that events with large hazard strengths may result in small impact, whereas events with small hazard strengths may lead to large impact. However, this is not necessarily a consequence of using an agential approach. When looking at hazard strengths locationally, for example peak ground acceleration, we can still find many cases where large hazard strengths associated with trivial or no damage due to low exposed value or high resilience of communities experiencing the large hazard strengths. Nevertheless, it is important to continue research in hazard equivalency both agentially and locationally.

Comment: 6. The example reported in the discussion highlights the problem with this method. The authors say that the cold wave in Oklahoma city in 2021 has the same hazard magnitude in the Gardoni scale as an earthquake of magnitude 7.5. I do believe that a magnitude 7.5 in Oklahoma city would have caused an impact that is several orders of magnitude larger than the impact caused by the cold wave; and no impact (or very limited) if the same earthquake occurred in a remote area.

Response: Thank you very much for your comment. As mentioned previously, the

objective of the paper is to quantify the equivalent hazard strength in an agential and durational way and not to compare the actual impact locationally. The cold wave event mentioned in the paper not only affected Oklahoma City but also many other parts of the Southern United States. The impact of the cold wave event within the entire spatial range of the event is recorded to be equivalent to the impact of Hurricane Harvey and the expected impact of a magnitude 7.5 earthquake. Regarding earthquake, it is very likely that a magnitude 7.5 earthquake in Oklahoma City would only affect a relatively small area around Oklahoma City. That is why, agentially, the 2021 cold wave event is equivalent in hazard magnitude to a magnitude 7.5 earthquake, but locationally, the hazard intensity of the 2021 cold wave event may not be equivalent to the hazard intensity of a magnitude 7.5 earthquake. The issue raised by the referee here is not a problem at all. Locationally, a magnitude 7.5 earthquake in Oklahoma City would have caused an impact that is several orders of magnitude larger than the impact caused by the cold wave in Oklahoma City. Agentially, however, the expected impact of a magnitude 7.5 earthquake, with its epicenter in Oklahoma City, within its spatial entirety would be equivalent to the expected impact of the 2021 cold wave event within its spatial entirety, i.e., the entire Southern United States.