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Comment on nhess-2022-198

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Referee comment on "Using machine learning algorithms to identify predictors of social vulnerability in the event of a hazard: Istanbul case study" by Oya Kalaycıoğlu et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2022-198-RC1, 2022

As an open reviewer of this manuscript, I first thank the handling editor of this special issue of NHESS, for giving me the opportunity to conduct the review. Next, let me introduce myself to demonstrate my qualifications for this review. My name is Yi Victor Wang (https://dryvw.com/). I am currently serving as a Postdoctoral Fellow at the Institute for Earth, Computing, Human and Observing (ECHO) at Chapman University, Orange, California, USA. I have been authentically studying and researching in the scholarly field of science, engineering, and management of hazards and disaster risks for over a decade. I have a bachelor's degree, Master's degree, and a Ph.D. degree in this field. One of my major academic contributions so far is the proposal of an ***empirical predictive modeling*** approach to quantifying disaster vulnerability with consideration of social factors (a.k.a., ***social vulnerability***). To facilitate communications regarding this review on the topics of social vulnerability to natural hazards, especially in the event of an earthquake, from my perspective, I recommend that the authors take a look at my first-authored peer-reviewed journal papers of Wang et al. 2019, 2020, and 2021 as well as Wang and Sebastian 2021 listed at the end of this review. In particular, Wang et al. 2021 is highly pertinent to what has been covered by the authors' manuscript.

General Comments

In terms of the authors' manuscript and research, I like the idea of applying machine learning (ML) methods to quantify social vulnerability and to identify predictors of social vulnerability. I also appreciate the technical prowess of the authors manifested in their statistical analyses. Having said these, however, I believe that the current version of the submission is far from the level of acceptance for publication. There are a number of major issues that render the manuscript highly dubious. The story line is also logically unsound and broken at several locations of the manuscript. The way the manuscript is laid out exposes the authors' lack of knowledge, confidence, and familiarity in topics related to disaster vulnerability and natural hazards. The authors have spent a disproportionately large amount of effort in showing the technical details of a few selected sections of their research that are actually not highly important regarding the purposes of their research. The motivations, results, and discussions in the manuscript around the topics, that are

supposed to be pertinent to the practices to improve earthquake disaster risk reductions, are presented in a highly superficial manner. In order to receive a green light from me, the authors need to solve the major and minor issues as listed below and conduct a thorough revision to their manuscript accordingly in the later stage of the review process.

Specific Comments

- L1: The uncountable noun of vulnerability in disaster research, especially for risk assessment for prediction of future loss, essentially means the propensity of an entity towards loss given a unit exposed value (such as life, economy, health, livelihood, infrastructural functionality, etc.) when the entity has experienced a certain level of hazard strength (such as ground shaking of an earthquake, wind gust of a tornado, inundation of a flood, etc.). In addition, vulnerability is usually also considered to be associated with the tendency towards a long-term suffering due to poor recovery by many, especially social scientists. To facilitate the management of disaster vulnerability before an unwanted event occurs, we may conceptualize disaster vulnerability as a combination of social vulnerability due to social factors, environmental vulnerability due to environmental factors, infrastructural vulnerability due to infrastructural factors, etc., as described in many classical literatures such as Cutter 1996 (https://doi.org/10.1177/030913259602000407). By the way, this Cutter 1996 is not the paper cited in the authors' manuscript. In the early days without big data on reliable and sufficient historical records of disaster losses, practitioners and scholars needed some method to quickly estimate disaster vulnerability. When it came to social vulnerability, professionals found that using social factors to construct a social vulnerability index (SVI) seemed to be a good approach for measuring social vulnerability. However, SVI itself is not social vulnerability. It is an indicator/predictor of social vulnerability at most. In the title, the authors claim that their research was to identify predictors of social vulnerability. But according to the body of the manuscript, it is clear that what the authors actually did was to identify predictors of an SVI. This is equivalent to building models to establish the relationships between a set of social variables and another set of social variables. What is the point for doing this when the authors could simply add these so-called predictors directly into their SVI?
- Then, regarding the SVI in the authors' research, I am not sure how the authors could resolve this second issue satisfactorily. As I have said in the previous comment, the original efforts to create SVIs were limited by a lack of sufficient historical records of event losses. Now, we are in year 2022 in the age of big data. We are having access to a gigantic amount of historical records of event losses to support empirical modeling of disaster vulnerability, socially, environmentally, infrastructurally, or in whatever manner. Why do we have to get stuck with the non-empirically derived SVIs to guide disaster risk reduction practices? For those SVIs that cannot be verified with historical data on losses, they are not reliable for offering any policy suggestions. For those SVIs that can potentially be verified with historical data on losses, it would be more appropriate to directly establish empirical models of disaster vulnerability with calibrations of models on the historical data on losses. Without empirical evidence that directly associates with the expected event losses or poor recovery processes, any SVI is merely a product of social construction based on amplified voices from a seemingly scholarly, but actually perhaps more political than academic, echo chamber that eventually result in the production of some form of emperor's new clothes more or less.
- L2: The title emphasizes "social vulnerability in the event of an earthquake". While reading the manuscript, however, I could hardly find anything to support the hypothesis that the manuscript is about vulnerability to an earthquake event. The input

variables of the ML models have nothing to do with earthquakes. The authors have also failed to show why the output variables of the ML models are for an earthquake event. It seems that the data of the research is based on a survey by the Directorate of Earthquake and Ground Research of Istanbul Metropolitan Municipality. Although the name of this organization involves earthquake, the variables of the survey used by the authors seem to be totally unrelated to earthquake events. There are no measures of hazard strengths of earthquake events, such as local magnitude, moment magnitude, peak ground acceleration, peak ground velocity, peak ground displacement, peak spectral acceleration, modified Mercalli intensity, etc. The authors need to justify why their work is for an earthquake event or for earthquake events.

- L27: The term of "social vulnerability risk" or "risk of social vulnerability" that also appears later in the manuscript is exceptionally confusing. As I have referred to the meaning of social vulnerability previously and the word "risk" also has its specific meanings, what is the meaning of this "social vulnerability risk"? For a summary of the meanings associated with the word "risk" in scholarly works, the authors may have a look at Möller 2012 (https://doi.org//10.1007/978-94-007-1433-5_3). It seems that, with their survey data, the authors created two categories, i.e., a high SVI and a low SVI, based on a cutoff score. So, why do the authors have to call these two categories "severe risk of social vulnerability" and "non-severe risk of social vulnerability", instead of "high SVI" and "low SVI"?
- L152-153: The authors need to introduce more regarding their SVI score, as it is unclear how readers may access an English version of IMM 2018 and MenteÅ□e et al. 2019 is just a conference abstract and presentation instead of a peer-reviewed journal publication or technical report. The authors need to transparently and concisely demonstrate why their SVI can effectively measure or indicate social vulnerability of a household in the event of an earthquake. Is their SVI related to an expected loss or loss ratio given a metric of earthquake hazard strength?
- L266-268: According to the title of the manuscript, the authors' main work was to use ML algorithms to identify predictors of social vulnerability. First, the initial feature selection of input variables of ML models has nothing to do with ML algorithms, as the authors claim clearly on L163 that the "predictors chosen have been selected following extensive literature reviews" and "discussions with experts". Then, it is still unclear what ML algorithms the authors have adopted for quantifying the importance of input variables in their predictive classification models. It seems that the main work of the authors was merely to calibrate some supervised ML classification models to map a set of already chosen input variables to their binary output variable of SVI score category. The authors need to at least explain more in a concise manner how they measured the importance of input variables of the ML models.
- Regarding the ML classification models, I am not convinced that the authors have the capability to properly compare the prediction results of the models that they have adopted. When dealing with statistical analysis, model validation, resampling, subsampling, etc., it seems that the authors have a lot to say. But when it comes to the ML models, there is almost nothing in the manuscript. For example, what is an SVM? What is an ANN? Are the authors using the multilayer perceptron, convolutional neural network, recurrent neural network, autoencoder network, or something else for their ANN modeling? What is the difference between a CART and an RF? Are the authors capable of explaining all the models they used in their study?
- The entire Introduction section needs to be thoroughly revised. The authors need to make sure that their introduction is concise, relevant to their research work, and following a story line that is logically sound. For example, on L34-35, the authors start their manuscript with a UN-qualified definition of disaster in terms of coping capacity. However, this definition is irrelevant to the vulnerability quantification at a household level.
- L3-37: The statement that the "evolution of an earthquake event into a disaster is typically studied through the lenses of geoscientists, civil engineers and earthquake engineers" is not true. There are many social scientists who have dedicated their

research works to studying earthquake risks and disaster vulnerability to earthquakes (see, e.g., Stallings 1995 https://www.routledge.com/Promoting-Risk-Constructing-the-Earthquake-Threat/Stallings/p/book/9780202305455; Bolin and Stanford 1998 https://doi.org/10.4324/9780203028070).

- L37-39: The statement that "it is often forgotten or ignored that the human consequences of disasters are in part derived from the composition of the population and society prior to the event" is false. There are plenty of works looking at the social factors of disaster vulnerability even for quantitative and engineering modeling purposes within the context of earthquake hazard (see, e.g., Peduzzi et al. 2009 https://doi.org/10.5194/nhess-9-1149-2009; Lin et al. 2015 https://doi.org/10.5194/nhess-15-2173-2015; Wang et al. 2019, 2020, 2021; Chen and Zhang 2022 https://doi.org/10.1016/j.ress.2022.108645).
- L49-65: This paragraph is totally unacceptable. Many sentences in this paragraph do not follow a logical flow. They read more like an awkward assemble of incompatible spare parts with fake "made in" labels on them. For example, on L55-58, the capacity of an entity to anticipate, cope with, resist, and recover from the impact of an earthquake actually includes the ability to reduce casualties due to collapse of buildings in an earthquake event.
- L59-61: Following the previous comment, I find it extremely difficult to understand why the authors have to talk about something called "social risks"? Also, I highly doubt that Prof. Susan Cutter has ever mentioned the term "social risks" in her 1996 paper. Can the authors provide the page number for where Cutter mentioned "social risk"?
- L66-83: This paragraph also reads awkward. It is unclear what the main point is for the authors to compile such a paragraph. On L68, the authors even cited the wrong Cutter 1996 paper.
- L86: The authors list logistic regression (LR) as a traditional data analysis tool. How could the authors justify their using LR as an ML method later?
- L92: The statement involving using "ML methodology over regression techniques" is confusing. Supervised ML methodology consists of two basic groups of methods. One is classification and the other is regression. ML regression methodology is part of ML methodology.
- L95-111: This paragraph needs to be rewritten to be concise and professional. It needs to serve the purpose of pointing out the motivation of and rationale for the proposed research. The authors need to read more technical papers published in hazard and disaster journals to get more familiar with the flavor of the introduction sections of papers that can be accepted for publication in this journal and rewrite their introduction accordingly.
- I am not at all convinced how the authors could justify the identification of risk of job loss in the event of an earthquake as a vulnerability factor/predictor. To reduce disaster risk is to reduce the expectation of event losses, which include the loss of livelihoods, or job loss. It is totally pointless to tell practitioners that, to reduce disaster risk including risk of job loss given an earthquake event, we need to reduce the risk of job loss given an earthquake event.
- L116-118: What is this "broad conceptual model"? What are the other models that the authors have compared their model to for demonstrating "a better understanding"? How is the authors' model better?
- L157-160: The listed three reasons for treating social vulnerability as a binary output of ML models are not convincing at all. First, with regression approaches with a numerical output variable, one can also identify vulnerability factors/predictors quantitatively and empirically. Second, the accuracy of predictions does not depend on whether using a classification or regression method. Third, it may be actually easier to interpret the regression results, especially when the regression models are linear or close to being linear.
- L497-498: Without historical data on event losses and recovery processes involved in their modeling efforts, how can the authors make such a bold statement that, based on their research, they "have found that socially, economically, and environmentally

vulnerable communities are more likely to suffer disproportionately from disasters"? Where are the actual evidences?

- L521-526: The authors claim that their research can support decision makers and local authorities to improve disaster risk reduction practices. However, I feel hardly confident to agree with this claim after reading the manuscript. ML methods are good at predicting output variable values based on the optimization of parameters of a mathematical model that empirically represents the relationship between the input and output variables based on the data for training. What the authors have achieved is using an index-based approach to create an SVI to indicate social vulnerability during their first phase. However, as I have mentioned previously, this indicator is not social vulnerability itself. It is an indicator of social vulnerability. Without consideration of empirical data on event losses, etc., this indicator itself is not a good indicator of disaster vulnerability. Then, in their second phase, which is what is mainly presented in the manuscript, the authors used ML methods to establish models of the relationships between a set of social variables as the input and their SVI as the output. With these models, the authors suggest that practitioners may identify pertinent social variables to improve disaster management. However, when targeting the identified social variables and changing their values, such an alteration of input variable values will only change the predicted model output value, while the changing of the model output value may have nothing to do with the actual reduction of social vulnerability. ML methodology does not identify causal relationships. I am simply wondering how, from the authors' perspective, their modeling results can actually benefit local management of seismic disaster risks. Can the authors explain it more in detail? In addition, what potential issues should the practitioners pay attention to when the practitioners are encouraged to apply the authors' models for guiding earthquake disaster risk reduction practices?

Technical Issues

- L24: Why are the words "Artificial", "Neural", and "Network" with their first letters capitalized while the ones on L21 are not? Plus, "(ANN)" should be following the "artificial neural network" on L21.
- L42-44: Why do the authors have to mention three return periods when the mentioning of 100-year return period alone would suffice in this sentence? Also, where is the evidence to support this statement?
- L53-54: What do the authors mean by the phrase "robust and concrete disaster risk reduction"? What does "robust" mean? What does "concrete" mean?
- Table 1: What is "Dept"?

List of My Publications Relevant to this Manuscript

Wang, Y. V., and Sebastian, A. (2021). Community flood vulnerability and risk assessment: An empirical predictive modeling approach. *Journal of Flood Risk Management 14*(3), e12739. https://doi.org/10.1111/jfr3.12739

Wang, Y. V., Gardoni, P., Murphy, C., and Guerrier, S. (2019). Predicting fatality rates

due to earthquakes accounting for community vulnerability. *Earthquake Spectra 35*(2), 513–536. https://doi.org/10.1193/022618EQS046M

Wang, Y. V., Gardoni, P., Murphy, C., and Guerrier, S. (2020). Worldwide predictions of earthquake casualty rates with seismic intensity measure and socioeconomic data: A fragility-based formulation. *Natural Hazards Review 21*(2), 04020001. https://doi.org/10.1061/(ASCE)NH.1527-6996.0000356

Wang, Y. V., Gardoni, P., Murphy, C., and Guerrier, S. (2021). Empirical predictive modeling approach to quantifying social vulnerability to natural hazards. *Annals of the American Association of Geographers 111*(5), 1559–1583. https://doi.org/10.1080/24694452.2020.1823807