Dear Editor,

According to your request I have completed my review of the manuscript "A seismic hazard analysis considering uncertainty during earthquake magnitude conversion", by J.P. Wang and X. Yun. The review includes general comments/impressions as well as some specific comments.

The paper states to perform seismic hazard analyses accounting for uncertainties in Mw-Ml conversion. The Authors analyze the seismicity located around a nuclear power plant in northern Taiwan, an area characterised by high seismic activity, and conclude that there is a 10% of probability that PGA at the site exceeds 0.28g in 50years.

However, both the method and the results are presented in a confusion way, and I cannot catch any novelty through the manuscript. Accounting and declaring for uncertainties in PSHA is the main task of many researchers in the last years, see for example the UCERF project in California, the studies in New Zealand and in Italy, but the Authors do not consider any of these papers.

I think the paper aims with an interesting topic, but it is not supported by an adequate analysis, or at least it is obscured. In my opinion the paper should be rejected, as it needs a complete reorganization before resubmitting. Moreover, even if I am not a mother tongue, I suggest English should be heavily revised. Below I listed some major weakness points need to be addressed.

General Comments:

- I suggest that the organization of the manuscript will revise to point out the main aim and the results. I have found really difficult to follow a rationale among title, abstract, analysis and conclusions. Now it is not clear if the main aim is to show how the uncertainty in Mw-Ml conversion affects the hazard or to perform an evaluation at a specific site.

If the aim is to account for the uncertainty (as the title suggest) I think that hazard curves should be presented along with ranges of uncertainties. If the aim is to characterize the seismic hazard at a site, a nuclear power plant, I don't think the PGA at 10% of exceedance in 50 years is the best value.

- I don't understand the use of a mean magnitude as computed in the paragraph 5. The histogram of seismicity seems to show a Gutenberg - Richter behaviour, so defining a mean magnitude is unusual. In the same manner, also the use of a mean source-to-site distance is not conventional.

At the end, it is not clear if the Authors have integrated knowledge about rates of occurrence of earthquakes, the possible magnitudes and distances of those earthquakes, and the distribution of ground shaking intensity due to those earthquakes, that is the basis for any PSH study.

-The core of the methodology is distributed in three paragraphs (3,4 and 6), but it will be good to have just one paragraph describing it. Also, the spreadsheet described in Fig. 6 is not clear.

Specific comments:

-Introduction states the aim of the paper, but the First-Order-Second-Moment (FOSM) analysis is introduced in a too short way. Please, clarify here what it is and give some references.

-The FOSM method should be described in a single chapter (methodology), so Authors could merge the chapters 3,4 and 6.

-Provide an adequate geological and seismotectonic background. I think readers could benefit from this description.

-In the chapter 2, and often through the manuscript, Authors describe differences between probabilistic and deterministic seismic hazard analysis, but they used an example that sounds very strange (why Authors need to cite soil's friction angles in chapter 2?).

-Probabilistic analysis allow using all possible earthquake events and resulting ground motions, along with their associated probabilities of occurrence, in order to find the level of ground motion intensity exceeded with some tolerably low rate, so why the Authors used a single value for magnitude and distance? I know they computed their standard deviations, but figure 3 shows that magnitude ranges from 6 to 8.2, so are earthquakes with Ml larger than 6.43(+0.46) ignored?

The mean, I suppose an arithmetic mean, is guided by the boundary the Authors have chosen but, what happen if the lower threshold for Ml is 6.5?

-The maximum (best will be a range of) distance and magnitude should be chosen according to the ground motion predictive equation.

-The Discussion section is lacking. Here the Authors should explore the significance of their results instead, in the chapter 7, the Authors refer to a paper of Chen et al., (2007) who computed a PGA at 10% of exceedance of about 0.3g, but the following discussion is not clear. What is the novelty of their work in respect to Chen? The Authors should do some efforts to discuss improvements of their work in respect to published data in an exhaustive way.

- I cannot understand what the Authors would discuss in the chapters 7.2, 7.3 and 7.4

- I agree that conclusions should be clear and concise, but here it is not clear if the main result is a new method or the seismic hazard at a site.

- My last comment is that the Authors make an effort to improve the references. Many countries currently use PSHA for national building code and nuclear power plant (California, New Zealand, Italy, France, Japan…) and researcher involved in this topic produced a vast literature that is now completely ignored in the manuscript.

To conclude, in my opinion the paper should be rejected in its present form, as it needs a huge effort by the Authors before a possible and deeper second round of revision.
Moreover, in the following section, I listed my opinion about the aspects required by NHESSD review procedure.

Does the paper address relevant scientific and/or technical questions within the scope of NHESS?	Yes
Does the paper present new data and/or novel concepts, ideas, tools, methods or results?	No
Are these up to international standards?	No
Are the scientific methods and assumptions valid and outlined clearly?	No
Are the results sufficient to support the interpretations and the conclusions?	No
Does the author reach substantial conclusions?	No
Is the description of the data used, the methods used, the experiments and calculations made, and the results obtained sufficiently complete and accurate to allow their reproduction by fellow scientists (traceability of results)?	No
Does the title clearly and unambiguously reflect the contents of the paper?	No
Does the abstract provide a concise, complete and unambiguous summary of the work done and the results obtained?	No
Are the title and the abstract pertinent, and easy to understand to a wide and diversified audience?	No
Are mathematical formulae, symbols, abbreviations and units correctly defined and used? If the formulae, symbols or abbreviations are numerous, are there tables or appendixes listing them?	Yes
Is the size, quality and readability of each figure adequate to the type and quantity of data presented?	No
Does the author give proper credit to previous and/or related work, and does he/she indicate clearly his/her own contribution?	No
Are the number and quality of the references appropriate?	No
Are the references accessible by fellow scientists?	Yes
Is the overall presentation well structured, clear and easy to understand by a wide and general audience?	No
Is the length of the paper adequate, too long or too short?	No, too short
Is there any part of the paper (title, abstract, main text, formulae, symbols, figures and their captions, tables, list of references, appendixes) that needs to be clarified, reduced, added, combined, or eliminated?	I suggest all the parts of the paper to be clarified. Moreover, Chapters 2 could be eliminated and Chapters 3,4 and 6 could be combined.
Is the technical language precise and understandable by fellow scientists?	No
Is the English language of good quality, fluent, simple and easy to read and understand by a wide and diversified audience?	No
Is the amount and quality of supplementary material (if any) appropriate?	No