Wind Energ. Sci. Discuss., referee comment RC1
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Comment on wes-2021-81

Anonymous Referee #1

Referee comment on "Seismic soil–structure interaction analysis of wind turbine support structures using augmented complex mode superposition response spectrum method" by Masaru Kitahara and Takeshi Ishihara, Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-81-RC1, 2022

The authors can find below the comments and severe reservations that the reviewer has regarding the specific manuscript. The authors are kindly asked to address sufficiently these comments.

C1. By reading the manuscript, the reviewer is wondering which exactly is the scientifically novel part of the study presented by the authors. If the reviewer is not mistaken, the authors applied an existing technique for RSM for the seismic analysis of wind turbine support structures. This method has been successfully applied for building type structures but not for wind turbines. So, one difference between the current study and the already published one can be seen in the structures (building vs wind turbine) that this method have been applied. The other difference is that the authors applied a threshold on some excessive values that can be derived for modal damping ratio. This correction is based on an empirical formula (Eq. 15) that does not necessarily come from the authors. Hence, in other words, one can say that the current manuscript reflects the application of an existing method in order to calculate the response of a wind turbine subjected to seismic forces while the use of existing formula is also adopted herein to fix some excessive damping ratio values. The reviewer has nothing to say against that this application and the results seem to be quite promising since a high similarity was found between the results from the currently applied method and the THA. However, the reviewer is bit reserved about the overall novelty of the current study. According to the reviewer's opinion, the current manuscript fits better to an application paper (or technical note) rather than an original research article. The authors are kindly asked to provide their point of view for this issue. However, it is also an issue that the Editor can have a word.

C2. Can the authors describe the origin of the empirical formula that they used to define this threshold for the modal damping ratio? Especially, the reviewer is interested in the 0.1 value that is included in the formula. Is this value based on engineering judgement?

C3.The damping ratio that was found for the 3rd mode and Soil Type was equal to 40.8.

Indeed, it is an excessive number. However, this manuscript describes a specific case, for which this high value was calculated. There is a chance that the application of the current method for another case (different soil type, different wind turbine supporting structure etc.) will lead to another value for the damping ratio, for example, 15%. So, what should someone do in this case? Which is the limit of the damping ratio over which the substitution should take place? In other words, the method that is presented by the authors should have somehow a more general validity and should not be highly case-specific and highly dependent on engineering judgment.

C4. The reviewer is a bit confused about the earthquake records that were artificially generated and used for the THA. Especially, Fig. 3 shows, among others, the response spectra of four recorded (natural) strong ground motions. So, did the authors used recorded ground motions for the THA or did they used artificial ones? Or both of them? And, why did the authors choose to show the response spectra of the existing ground motions and not of the artificially generated ones?

C5. By the beginning of chapter 3, the authors describe one wind turbine (2 MW) and two foundation solutions (gravity and piles). Then, rated power was varying – hence, different foundations (i.e., different footings) as well as different characteristics of the overall wind turbine structure were considered. However, it is not clear at all for which of the aforementioned cases the authors present results. For example, Fig. 5 provide results of shear forces and bending moments along the height. However, to which of the aforementioned cases do these results correspond? The same is valid for all the results that the authors present.