We thank Dr. Ilaria Mosca for her review and comments. Below are Dr. Mosca's comments and line-by-line suggestions, in black. Our responses are in blue.

The aim of the paper NHESS-2023-213 "Probabilistic Seismic Hazard Assessment of Sweden" is to present seismic hazard estimates (hazard maps and hazard curves for selected sites) for Sweden using the probabilistic seismic hazard analysis (PSHA). This country is characterised by low levels of seismicity and therefore the time length of the earthquake observations, which span a few hundreds of years in the best case, is much shorter than the seismic cycle of large earthquakes, which is of the order of thousands of years in low seismicity regions (e.g. Stein et al. 2015). Using sparse and limited sets of data represents a challenge to fully capture the epistemic uncertainties in a national seismic hazard model. In this context, the aim of this paper is of primary importance for seismic hazard analysis. However, there are some inaccuracies in the manuscript (e.g., the description of the steps for PSHA) and more explanations to justify the decisions taken by the authors to develop the seismic hazard model for Sweden are required. Furthermore, the English language seems to be quite poor in some paragraphs. Although I provide below some editorial comments on wording and sentences, I would suggest a significant revision in terms of the language throughout the manuscript. Here I list the main technical and editorial points.

1- A discussion on the uncertainty in the parameters of the earthquake catalogue is not mentioned at all. What are the uncertainties in the epicentral locations and the magnitude? Are they accounted for in the estimation of the recurrence parameters?
Location and magnitude uncertainties have of course evolved over time, they are currently 2 -3 km and 0.1 – 0.2 on average in the Swedish seismic network. The historical, macroseismic data has significantly larger uncertainties. We do not consider the location accuracies to be a problem, concerning the size of the seismic source areas. The magnitude uncertainties for events in the older data does affect the recurrence calculations but are accounted for in the logic tree through the a- and b-value 2-sigma uncertainties. We have now added information on uncertainties to line 221 (in the original manuscript).

2- The authors do not mention at all the focal mechanisms of the earthquakes in Sweden and Fennoscandia. Are there any focal mechanisms known for earthquakes that occurred in the region? Similarly, what is the hypocentral depth, together with the associated uncertainty, of the earthquakes in the final catalogue built for this work?
We do refer to Gregersen et al. (2021) for a recent review of the debate surrounding the source of Fennoscandian seismicity, which includes focal mechanism studies and their interpretations. As for the depth distribution of earthquakes, Gregersen et al. (2021) says the following "Information on the focal depth distribution is not optimal due to the combination of sparse station density and large lateral variations in the crustal structure. Routine source depth estimates may contain significant uncertainties, and fixed depth estimates are frequently used by some seismic observatories." Earthquakes in Sweden are mostly strike-slip, and focal depths vary widely between near-surface and down to 35 km depth. We added more info on depth after line 87, and also refer to the depth implementation in OpenQuake discussed at line 441, and focal mechanisms info after line 95 (original manuscript).

3- The discussion on the magnitude homogenisation and assessment of the completeness thresholds (Section 4.1.3) in the catalogue is difficult to follow and lacks crucial information. Is the ML(HEL) used for all events in the final catalogue, including those from NORSAR, NNSN, and SNSN? If not, the description of how ML(HEL) was estimated is unnecessary. What are the equations used to convert ML into Mw? Are they applied to all the data in the final catalogue? For the assessment of the completeness threshold(s), from which year is the catalogue complete for Mc = 2 Mw? Furthermore, is a single Mc value used for the calculation of the recurrence parameters? Why did the authors not use the completeness thresholds for Fennoscandia estimated in ESHM20 or ESHM13?
There was an unfortunate error in the equations in line 270, as they should say Mw(HEL) =…
We have corrected this and added more text to the section, making it clear that all magnitudes are converted to Mw(HEL), which is what we later refer to as just Mw. Completeness vary in time and per zone, we have made individual assessments for different source zones and time periods, as noted on line 349. For the entire data set, completeness is around Mw 2 for onshore areas from around the mid-1970s. We did not use the completeness thresholds from ESHM20 as they have a magnitude cut-off of Mw 3.5 and therefore do not include many Swedish events.

4- The authors should explain better how they defined a Mmax distribution between 6.3 and 7.5 (I assume this is Mw, isn't it?). In analogue regions, there are no examples of 7.5 Mw earthquakes, so the authors should justify better the 7.5 Mw value.
As we write in section 4.3, the rupture of post-glacial faults in Northern-Sweden has been estimated to lead to earthquakes as large as Mw8 nearly 10,000 years ago and the most recent rupture of the post-glacial faults in Northern-Norway was estimated to be at M7.0. We therefore consider it reasonable to include Mmax=7.5 with a low weight of 0.05 on the logic tree.

5- If I have understood correctly, the authors have defined new TSZs and ASZs from the ESHM20. If this is the case, why did the authors use the TSZs and ASZs from ESHM20?
Yes, we do define new source zones compared to ESHM20 but no, we do not use the ESHM20 zones, we use the newly defined zones. This is described in section 4.2,

6- Is a single source model considered for the PSHA of Sweden? Alternative source models would account for different interpretations of the mapped tectonic structures, large-scale deformation, regional stress field, and observed seismicity in Sweden and Fennoscandia. It would ensure to capture the epistemic variability in the behaviour and location of seismogenic structures and their correlation with seismicity. Did the authors consider to use of the zoneless (zone-free, smoothed) models (see Beauval et al. 2006; Zechar and Jordan 2010 for more details) approach as an alternative sesmic model? This was included in the ESHM20 model and other national seismic hazard models, such as Germany (Grünthal et al. 2018) and France (Drouet et al. 2020).
Yes, we only use one source model. We have an ongoing project where we use seismicity smoothing as implemented by Frankel 1995, to estimate seismic hazard. However, we decided to not include that in this study owing to time-constraints for a PhD-student. We aim to include smoothed seismicity in the future as a part of a project on a joint Fennoscandian seismic hazard assessment.

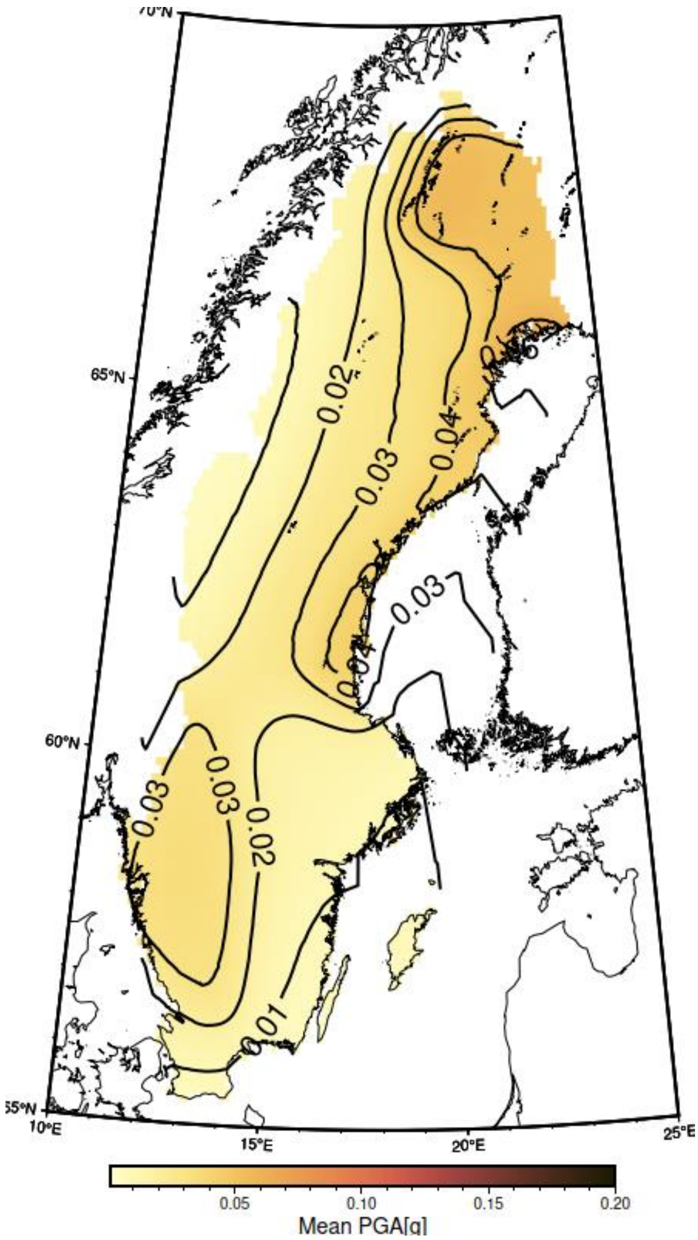
7- How were the weights in the ground motion logic tree decided? Are there any available ground motion recordings for instrumental earthquakes in Sweden and Fennoscandia? If so, it would be useful to compare them with the predictions from the selected ground motion models. This comparison can be used to assign the weights for the ground motion models in the logic tree, together with expert judgements due to the limited ground motion dataset in the region.

The ground motion logic tree includes the ground motion models included in ESHM20 and the regional ground motion model FennoG16, developed by our colleagues in Finland. The combined weights of the ESHM20 GMMs were scaled down to include the FennoG16. The actual weights were then decided based on spread of the uncertainties in the ground motion curves for Uppsala, as shown in figure 7, where we aim for weights that give us fractile curves wider than the FennoG16 but narrower than the ESHM20 logic tree.

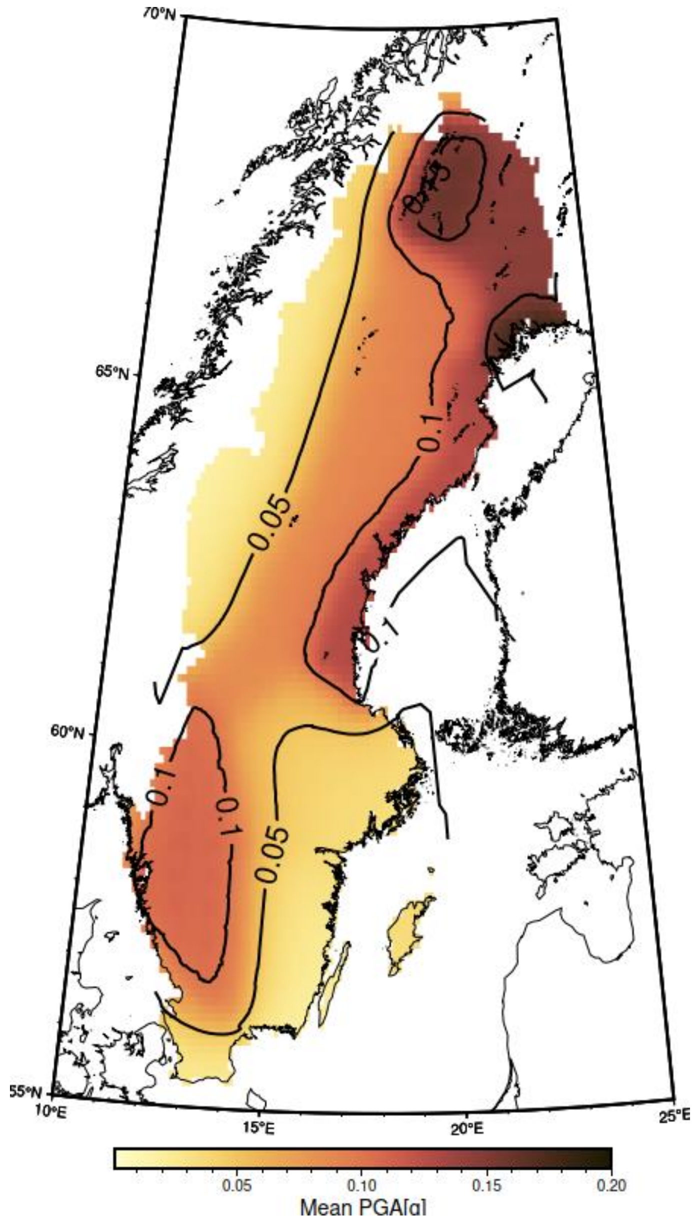
FennoG16 (Fülöp et al., 2020) used all available ground motion data in Fennoscandia for its development. There are no strong motion data from Fennoscandia, and very little data from events larger than M4, or close to events larger than M3.5. FennoG16 therefore also includes data from Eastern North America, as does the ESHM20 craton GMM (Weatherill & Cotton, 2020) who also compare the ESHM20 craton model to FennoG16. This is noted in section 4.6.

8- Why was a minimum magnitude of 4.5 Mw selected for the hazard calculations? The minimum magnitude (Mmin) in a hazard calculation is defined as the threshold for potentially damaging earthquakes (e.g. Bommer and Crowley 2017). This parameter is usually defined between 4 and 5 Mw for PSHA. In the PSHA for the UK, it was set to 4.0 Mw because it includes the probability that the impulsive nature of small earthquakes and their high-frequency content could be potentially causing damage (Mosca et al., 2022). I would think that due to the low levels of seismicity in Sweden, this may be appropriate also for this country.

We noted that it is common to use a minimum magnitude of 4 Mw when it comes to a seismic hazard assessment focussed on the civil infrastructure, while a minimum magnitude of 5 is commonly used when estimating seismic hazard for nuclear power installations. We therefore believed a minimum magnitude of Mw 4.5 was a reasonable compromise between the two cases. However, we have now calculated the seismic hazard for a minimum magnitude of Mw 4.0 and present the results below, as well as include them in the manuscript.



Seismic hazard map with a minimum magnitude of Mw 4 for a return period of 475 years.



Seismic hazard map with a minimum magnitude of Mw 4 for a return period of 2500 years.

9- Section discussion (Section 6 here) should not repeat what was already written previously. It should emphasize the main result, highlight the strengths and limitations of the study, provide the interpretation of the results in the context of regional hazard and eventually give future research directions. For example, Subsection 6.2 “Comparison with previous studies” should be part of Section Results.

We emphasise the main results in the Results section. Our intention with the discussion section was to contextualise the results in terms of the challenges we face to estimate recurrence parameters, other studies that have estimated the hazard in Sweden, and other studies that comment on the regional stress field and tectonics. We do agree that there is some degree of repetition in this section and modifications have been made to address this. We do not agree that Subsection 6.2 “Comparison with previous studies” should be part of Section Results as we want the results section to highlight our findings.

10- An acronym should be explained only when it is mentioned the first time in the manuscript. ML and Mw are not explained when they are used for the first time in Section 2.
Changes have been made to address this concern.

11- All the geographical names mentioned in the text should be indicated on a map because not all the readers are familiar with the geography, geology and tectonics of Sweden.
The text and figures have been appropriately modified.

Line 4: Include a comma before “which”. Replace “large number of events” with “ high number of events”.
Modified as per reviewer’s suggestion.
Line 5: Replace “5.9 to -1.4” with “-1.4 to 5.9”. What is the magnitude scale?
Modified as per reviewer’s suggestion.
Line 6: “less uncertainty” is in contradiction with the first line of the abstract, which states that the seismic hazard assessment in stable continental regions is challenging due to the limited amount of available data ”. Also, “recurrence parameters to be calculated for more source areas than in previous studies” is unclear and I would suggest re-phrasing it.
“less uncertainty” refers to a comparison to earlier studies and not to an overall challenge. We keep that and rephrase the “more source areas” part
Line 13: replace “highest PGAs” with “highest PGA values”.
Modified as per reviewer’s suggestion.
Line 19: What do the authors mean by “disaster development in the event”?
Modified and re-phrased.
Line 25: Replace the full stop before Occurrence with a comma.
Modified as per reviewer’s suggestion.
Line 26: Replace “as England and Jackson (2011) show, the risk” with “England and Jackson (2011) show that the risk”.
Modified as per reviewer’s suggestion.
Line 30: What is the magnitude scale in this case? How were these estimates (one event of magnitude 5 every 100 years and one event of magnitude 6 every 1000 years) computed? Replace “until 2005” with “before 2005”.
Added Mw, we refer to the reference for details of computations.
Line 34: How large are the “large earthquakes”? Provide an indication.
Modified as per reviewer’s suggestion.
Line 39: Provide references for “earlier estimates”.
As this is outlining what is to come in the paper we refrain from including the 16 references here. They are discussed in section 3.
Line 44: Replace “The hazard is calculated using the OpenQuake engine (Pagani et al., 2014) and we produce hazard maps…” with “We use the OpenQuake engine (Pagani et al., 2014) to develop hazard maps …”.
Modified as per reviewer’s suggestion.

Line 49: The first sentence of Section 2 is more appropriate for the introduction than for this section, which could start with the second sentence. It would be useful to mention which are these damaging earthquakes and which damages were produced.

We think the first sentence is appropriate in a section on Earthquake activity and keep it here. The damaging (large) earthquakes are discussed further down in the section, we added damage descriptions there.

Line 76: For which year does the completeness magnitude of 0.5 correspond? What is the magnitude scale? In Section 2, both ML and Mw are used. Probably it is better to use only one magnitude scale, preferably Mw.
Mc of 0.5 refers to the modified network after 2000, we added that and ML. As not all magnitudes in the section are available as Mw or ML, we use both but made it clear.

Line 82: How low is the magnitude? Provide an indication.
Modified as per reviewer's suggestion.

Figure 1: Besides reporting the geographical names in the text into Figure 1, it would be useful to show the distribution of the earthquakes in terms of magnitude highlighting those of magnitude 4 and above. Which magnitude scale is used in the figure, ML or Mw? Also, would it be possible to label the earthquakes mentioned in Section 2 into Figure 1, e.g. 1819 earthquake? Last, it is difficult to distinguish the Tornquist and Trans-European Suture zones from the earthquakes (they both are indicated by dots).
The figure uses the homogenized moment magnitude scale and we include the large earthquakes mentioned in Section2. Highlighting all events above M4 would clutter the Figure too much, instead we show those in Figure 2A. The Tornquist and Trans-European Suture Zones are better outlined.

Section 3: In general, the main components (i.e. catalogue, source model, ground motion model) of each study, together with the highest hazard computed by the studies, should be explained to facilitate the comparison between models, including the model presented in the manuscript. Probably, a table which summarises the various components of previous studies in Sweden and Fennoscandia may be helpful. I recognise that indication of the resulting hazard in the previous studies is done, but not all the components are briefly described. For example, the ground motion models used in Bath (1979), Wahlström and Grünthal (2001), Mäntyniemi et al. (1993, 2001), etc are not indicated explicitly. The model of GSHAP and ESHM13 are cited but no information about them is provided. It would be useful to see how they differ from ESHM20 in terms of individual components and hazard results. Please indicate the magnitude scale every time (see lines 124-126).
We choose to highlight our study and results first, followed by a comparison with what was obtained by ESHM20. We compare the methodological differences through out the paper with the intention of making it easier for the reader to follow the thread when it comes to understanding why our results are similar and different. We appreciate the reviewers suggestion but choose to only provide results from the previous studies and not further discuss the other components or choices made.

Line 108: It would be useful to indicate how much "highest" is the highest hazard in Bath (1979).
This has now been rephrased to indicate that Båth calculated earthquake risk according to the formula put forward by Lomnitz around 1950s-60s describing earthquake risk as"*the probability R(D/T) that a shock of mean return period T occurs during a design period D.*" There are broader similarities between Båth's estimates of seismic risk and our calculations.

Line 109: Replace “an ML ≥ 5 event” with “an event of 5 ML and above”.

Modified as per reviewer’s suggestion.

Lines 116: What does “various combinations of seismic source areas” mean? Also, replace “rate information” with “rate estimation”.

Modified as per reviewer’s suggestion. The text reads as follows:
FENCAT data until 1987 were used for the first Swedish probabilistic seismic hazard assessment (PSHA) work, directed at site-specific assessments for Sweden’s four nuclear power plants (SKI, 1992). Fennoscandian earthquakes south of latitude 61◦ were used to estimate the seismicity rate.

Line 120: Move “for a probability of exceedance of 10−5 per year and a damping of 5%” at the end of the sentence. Furthermore, the damping is for spectral acceleration, not PGA.

Modified as per reviewer’s suggestion. PGA in SKI92:3 is defined at 100 Hz and used also for spectral acceleration, which is why we used it here.

Line 130: Replace “Wahlström and Grünthal (2000) and follow-up Wahlström and Grünthal (2001)” with “Wahlström and Grünthal (2000, 2001)”.

Modified as per reviewer’s suggestion.

Line 151: Provide the references for “two large PSHA projects for the nuclear industry in Finland”.

The two are referenced in the next sentences. No change.

Lines 152-153: Replace “The first, the Fennovoima project, assembled seismologists and geologists from Finland and Sweden to perform a full site-specific PSHA” with “In the Fennovoima project, seismologists and geologists from Finland and Sweden perform a full site-specific PSHA…”.

No change, as the sentence follows on the previous.

Lines 169-170: Replace “events, from 1497 to 2014, with magnitudes 3.5 ≤ MW ≤ 5.8.” with “events with magnitudes 3.5 ≤ MW ≤ 5.8 from 1497 to 2014.”.

Stylistic, no change.

Lines 172-174: It is difficult to follow this sentence. I would suggest rephrasing it. ‘

The sentence is rephrased as follows

We split the sentence in two: Fennoscandia is assigned to a single maximum magnitude zone,
within which Mmax is uniform. It is divided into two “completeness zones” (CSZ) where reporting is assumed to be homogeneous through time such that the temporal variation in the magnitude of completeness, Mc , is the same all through the zone.

Line 173: Replace “In these zones,” with “In ASZs with more than 30 earthquakes,” and remove “for zones with more than 30 earthquakes” at the end of this sentence.

Sentences modified

Line 177: It is double (not doubly) truncated Gutenberg-Richter. Correct it throughout the manuscript. Also, replace “using an automatic maximum” with “and an automatic maximum”.
There seems to be different opinions about “doubly/double”. Early investigators, e.g. Cosentino et al. (1977) and Kijko & Sellevoll (1998) uses doubly, later investigators vary their use and Danciu et al. (2021) uses both. We stay with doubly. No change.

Line 179: Replace “is re-used” with “is assumed as a prior value”.
No change, as the TSZ b-values are used as-is for the ASZs with few events, not as a prior for further calculations.

Line 194: It should be mentioned that the GMM in Kotha et al. (2020) are for active shallow crustal regions in the ESHM20.
Modified as per reviewer’s suggestion.

Section 4: The description of PSHA is inaccurate. It consists of four steps (e.g., Reiter, 1990; Baker et al., 2021): 1- Definition of seismic sources based on knowledge of the tectonics, geology and seismicity of the study area. 2- Quantification of the rate of earthquake occurrence for each seismic source zone using the Gutenberg-Richter frequency-magnitude law. 3- Characterise the ‘earthquake effect’ expressed in terms of some instrumental ground motion measure, such as PGA, or seismic intensity. 4- Estimation of the hazard at the site(s) by analytically integrating over the source models for the location and size of potential future earthquakes (Steps 1 and 2) with expected values of the potential shaking intensity caused by these future earthquakes (Step 3), including the associated variability in each. The development of the earthquake catalogue is part of step 1.
There are a number of ways to divide up the PSHA methodology, Baker himself used 5 points in 2013. Our 3 points incorporate the important points, but we realize that the text is a bit too brief, considering we spend 7 subsections describing our work. The text has been modified to better reflect the subsections.

Subsections 4.1 and 4.2: They can be merged. Why few events from the ESHM20 catalogue are not included in the FENCAT catalogue? When did these events occur? and what was their magnitude? How small were the events in the SNSN catalogue that were included in the final catalogue? How were quarry, industrial or military blasts, rock bursts, mine collapses etc identified as nontectonic earthquakes? Did the authors remove also non-tectonic events offshore?
We think the reading is easier if the subsections are kept specific so as to not get too long. The ESHM20 catalogue was prepared by aggregating earthquakes from several catalogues across Europe whereas the FENCAT catalogue includes data specifically from Fennoscandia, which is a reason why not all events that exist in the catalogue used by ESHM20 exist in the FENCAT catalogue. We, therefore, include those events from ESHM20 that lie within our source zones and were missing from FENCAT. This includes a M3.81 earthquake that occurred in northern Germany in 1909, two earthquakes with magnitudes 4.57 and 4.73 from 2004 that occurred in Kaliningrad, four events in Latvia with magnitudes between 4 and5 that occurred between 1821 and 1910, another from Estonia that occurred in 1670 with a recorded magnitude of 4.7 etc.The smallest event from SNSN in the declustered catalogue is below Mw 0. Classification of events is a major undertaking at the

seismic networks in Fennoscandia, as the rate of detected blasts is 10 times higher than the earthquake rate. Analyst experience during manual review together with a recently developed ML system (Eggertsson et al., 2024) is used to classify events. The process is however out of scope for this paper. We added brief info on the ESHM20 events and classification to 4.1.1.

Line 231: Indicate the magnitude range for the 24,215 events in the final catalogue.
Modified as per reviewer's suggestion.

Section 4.1.2: How do the results of the declustering method (modified Gardner and Knopoff, 1974) compare with that from the method of Burkhard and Grünthal (2009) that was calibrated for the earthquake catalogue in Central Europe and was used in ESHM13 and ESHM20?
As the Uski et al. (2015) method worked well, we have not used the Burkhard & Grunthal (2009) approach.

Line 223: Replace "at our disposal spans the year 1375 until the end of" with "that we used spans between 1375 and the end of".
Stylistic, we keep our text.

Line 234: Provide a reference for the first sentence.
An appropriate reference has now been added

Lines 249-251: It is difficult to follow this sentence, so I would suggest rephrasing it.
The text has been rephrased

Line 257: Replace "a smaller fraction of dependent events of only 11%, a difference to our result which is likely due to the fact that" with "less dependent events than those in our study. The difference (11%) is probably because".
The sentence has been rephrased.

Line 249: How do the earthquakes in the FENCAT compare with those in the ESHM20 catalogue in terms of epicentral location and magnitude? In Figure 4 the earthquakes should be plotted in terms of magnitude to facilitate this comparison. Figure 2: I would suggest adding an extra figure to show the distribution of the seismic source model. Figure 2 should show only the final catalogue for this work where the distribution of earthquakes in terms of magnitude should be highlighted.

ESHM20 used the FENCAT catalogue for most of the northern Europe data. Epicentral locations should therefore be the same (as those in FENCAT when the ESHM20 data was assembled) but homogenized moment magnitudes may vary as the homogenisation schemes are different. We have not done an exhaustive comparison of the ESHM20 event catalogue. We modified Fig 4 such that our events with Mw >= 3.5 are identifiable. It is unclear to us what the reviewer refers to with "an extra figure to show the distribution of the seismic source model". In response to earlier comments we modify Fig 2A such that only shows events with Mw>=4 whereas Fig 2B will continue to show our full, declustered catalogue.

Line 312: Replace “is complicated by” with “is difficult due to”.
Stylistic, we keep our text.

Line 315: Delete “purposes”.
Modified as per reviewer’s suggestion.

Lines 320 and the following lines: Indicate the magnitude scale.
The magnitude scales are now indicated.

Table 2: Why aren’t the recurrence parameters of all ASZs reported in this table? The a and b-values for zones 1,4-8,10-12, etc are missing. For transparency, they all should be reported. Is the activity rate computed for 0 Mw? It would be also useful to indicate how many earthquakes within the completeness thresholds were used to estimate the recurrence parameters. As mentioned before, it is unclear which completeness thresholds were used for the estimation of the recurrence parameters. For many zones, the b-value seems to be quite low (< 0.9), what is the reason for this? How do the b-values compare with previous studies, in particular the ESHM20 for similar zones?
We chose to only include recurrence parameters for zones 2, 3, 4, 9, 13, 14, 15, 18, 23, 24, 30 and 31 as these are the only zones that lie within Sweden. The new table indicates the number of complete events used towards the recurrence parameter calculations. The activity rate is indeed calculated for 0 Mw. Our b-values are generally in line with the ESHM20 b-values, within the uncertainty limits. We have one b-value comparison in section 6.2, we added more comparisons.

Line 383: replace “construct” with “develop”.
Stylistic, no change.

Line 415: What is Model 5?
Good catch, that should be Model 4, also on lines 418 and 422. We had five models early on. Changed.

Line 433: Replace “yearly” with “annual”.
Changed

Lines 441-444: The hypocentral depths of the earthquakes in the catalogue have not been discussed at all in the manuscript to justify the depth distribution indicated here for the hazard calculations.
As indicated above, we have now improved the text to include a depth distribution discussion.

Section 4.7: Openquake requires also the definition of the faulting style for potential, future earthquakes, defined by rake, dip and strike. This set of parameters has not been discussed at all in the manuscript.
Openquake requires the definition of the strike, dip and rake of the nodal plane orientations. We have defined these nodal planes to be oriented parallel and perpendicular to the regional stress field, as defined in the ESHM20 calculations. The text has been updated to say the

following - “*Each seismic source area requires the definition of orientations and faulting styles of ruptures, quantified by the strike dip and rake of nodal planes. We use the strike and dip values adopted by ESHM20 in their calculations and choose a strike of 0 degrees, a dip of 90 degrees. We define two rake values of 0 or 180 degrees, each with a weight of 0.5 in the probability distribution.*

Line 456: In the revision of the Eurocode 8, the seismic hazard is described in terms of the 5% damped maximum spectral acceleration at a short period and 1.0 s period, and PGA is not mentioned anymore. Would it be useful to estimate national maps also for spectral acceleration for a representative short period (e.g. 0.2 s) and 1.0 s?
We agree and the new hazard maps with Mmin = 4 include results for the spectral acceleration calculations.

Figure 10: What is plotted in Figure 10 exactly? Is this the relative or absolute difference between the new map for Sweden and the ESHM20 maps? It would be helpful to produce such a map also for ESHM20 and the other previous maps discussed in Section 6.2.
Figure 10 shows the difference in mean PGA between our new model and the ESHM20, for the two return periods. Absolute in the sense that it is just our Model – ESHM20, which is why some areas are positive and some negative. As we have focused on a comparison to ESHM20, we choose not to include difference maps of other studies.