

Please see our responses (in blue) to referee #1 between his comments below.

Formal manuscript rating and recommendation to the editor (non-public)

1) Scientific significance

Does the manuscript represent a substantial contribution to the understanding of natural hazards and their consequences (new concepts, ideas, methods, or data)?

Excellent

Good

Fair

Poor

2) Scientific quality

Are the scientific and/or technical approaches and the applied methods valid? Are the results discussed in an appropriate and balanced way (clarity of concepts and discussion, consideration of related work, including appropriate references)?

Excellent373

Good

Fair

Poor

3) Presentation quality

Are the scientific data, results and conclusions presented in a clear, concise, and well-structured way (number and quality of figures/tables, appropriate use of technical and English language, simplicity of the language)?

Excellent

Good

Fair

Poor

For final publication, the manuscript should be accepted as is.

accepted subject to technical corrections.

accepted subject to minor revisions.

reconsidered after major revisions:

I am willing to review the revised paper.

I am not willing to review the revised paper.

rejected.

Subject: Comment on nhess-2021-15

Catalogs that are compilations from previously published catalogs, assembled according the modern criteria, are important for both tectonic studies and seismic hazard studies. The catalog resulting from this paper will play an important role in future studies focussing on Iceland and vicinity. The paper comes across as sound from a scientific perspective, although I am asking for more detail on several points to confirm this impression. I also see the desirability for revision to address issues of style and exposition.

We thank the referee for his positive feedback. We shall do our best to address all the issues raised in his comments.

For discussion of prior cataloging of Iceland earthquakes, I would recommend explicit mention of the International Seismological Summary (ISS), the predecessor of the ISC. As is apparent from the results of the online search of the ISC (<http://www.isc.ac.uk/iscbulletin/search/bulletin/>), many of the early "ISC" locations are actually ISS locations. The ISS volumes have been scanned and put on-line by Italy's INGV (<http://storing.ingv.it/ISS/index.html>). Some of the ISS origins are actually those computed by IMO, but the ISS has, with these origins, associated arrival-times (actually, travel-times computed with respect to the published origin-times) from stations world-wide.

We shall make a mention of the ISS in the revised manuscript: **"Another global source for earthquakes in the first part of the 20th century is the International Seismological Summary (ISS), the predecessor of the**

ISC.” [line 58]. Our web search indicates that there are no magnitudes in the ISS bulletins. There are few earthquakes from the ISS period that we take from the ISC catalog, and most of those are marked “Gutenberg-Richter”.

Following are comments on particular sections or lines of the paper, referenced to the line number on the PDF file that was provided for review. I would acknowledge that some of these comments do not identify issues errors or issues that are important for understanding the paper, but just my personal stylistic preferences. I am assuming that the authors will recognize these cases of personal stylistic preference and judge for themselves whether or not to address them,

line 6 — “modified by some expert judgement” — this phrase should be revised to provide a better sense of the “expert judgement”. Presumably, in the context of the sentence in which it resides, the expert judgement is based on something besides technical reports, scientific publications, and newspaper articles. A possible example of modification by expert judgement, which should be possible to explain, would be the reinterpretation of some previously ambiguous data in terms of seismological or tectonic understanding that has been acquired since the data were initially interpreted

To clarify and simplify the description of the methodology used, we propose/intend to remove the reference to “expert judgement” from the abstract (viz: “...scientific publications, **and** newspaper articles, ~~and modified by some expert judgement~~. The catalogue contains...”). The expert judgement applies to events for which 6 of the authors (cf. section “Author contributions”) reappraised event locations at meetings. Sometimes the sources are explicitly vague about the location, and sometimes the sources’ epicenter is evidently wrong. Thus the example phrase given by the referee might adequately apply to both cases, and we intend to add a sentence at the end of the first paragraph of section 3, where “expert judgement” occurs again: “**One could say that we have reinterpreted the data with seismological and tectonic understanding that has been accumulating in recent years and decades.**”

line 8 — The authors citation of the largest magnitude of Mw 7.01 will seem naively precise to many readers, for reasons that are discussed at length in the body of the paper. I would recommend giving the “largest magnitude” to lower precision in this instance.

Indeed. We shall make the change.

lines 8-9 — I would suggest revising the description of use of local and teleseismic data, to account for the fact that listings in the ISC and ISS have made use of both local and tele seismic data.

We shall change “local and teleseismic data” to “**local data and teleseismic catalogues**” on line 8 in response to this suggestion. Just to clarify, we state earlier in the abstract that we are combining local epicenter information and global magnitude information, and the phrase “melting” refers to this. Few of the ISC magnitudes are based on local data, and we explicitly disregard Reykjavik and Akureyri as source agencies in our scripts.

line 9 — I suggest leaving out or modifying the second clause. Previous catalogs, such as those of the ISC and ISS, would not have contained epicenters that were obviously mislocated in ways different from the ways in which the epicenters of the ICEL-NMAR catalog are mislocated.

We are not suggesting that the nature of mislocation is different, but instead its magnitude. The accuracy of the locations in the new catalog is much better than in earlier maps, which sometimes (or often) provide locations that are wrong by tens of kilometers, and place earthquakes in locations where it is absolutely certain that they did not occur (such as directly under Reykjavík).

We intent to change “with no obviously mislocated events” to “**with much more accurate locations than earlier maps**” to try to be clearer.

line 11 — “computed with chi-squared — regression”. The proxy Mw values themselves are not computed with chi-squared regression, as is literally stated. The proxy Mw values are calculated from equations that were determined with chi-squared regression.

OK. We intend to change “with” to “**using**”

line 12 — “All the presented magnitudes have associated uncertainty estimates” I suggest revising, and augmenting, this sentence by citing typical, or example, values of uncertainty that are associated with a few classes of M_w .

We agree, this is a good idea if the length limits on the abstract would allow. The MW -uncertainty is not estimated when $MW_{proxy} < 4.5$, it is about 0.10 for moment-tensor modelled values, 0.15–0.20 when MW is computed from MS , and 0.25–0.35 when it is computed from mb . Accurate information on the estimates is already provided in section 4.4. Note that this means that the statement (“All the presented...”) is incorrect and we shall correct it to: **“Magnitudes $M \geq 4.5$ have associated uncertainty estimates”**.

lines 12-14 — The conclusion on the relationship of seismic moment to plate displacement should be summarized more precisely and informatively, so that the reader can better anticipate the reasoning you have use in the text to relate seismic moment to plate displacement. For example, as discussed in the text, there is relatively little seismic moment associated with the rift (i.e., non-transform fault) section of the plate-boundary.

To expand on the discussion of total seismic moment and plate displacement in the abstract we shall add the clause **“indicating that the seismic activity of the catalogue period might be typical for any 120 year timespan”** to the second last sentence in the abstract, drawing on the discussion at the beginning of section 5.4.

line 17 — I think that what the authors call the “North-America plate” is usually called the “North American plate” and their “Euro-Asia plate” is usually called the “Eurasian plate”. This was my impression, and I see it confirmed by the results of web-searches on the various alternatives.

Good point! Thanks for the web-search. We shall make the change.

line 26 — typo — “where recorded” should be “were recorded”.

ok

lines 29-30 — I would suggest specifically mentioning the Reykjavik Mainka seismograph(s) at this stage of the manuscript, and providing a citation. (somehow, my word processor occasionally changes “Mainka” to “Maniac”. If, in the version of this review that is transmitted to editor and authors, you see a reference to a “Maniac seismograph”, please read that as “Mainka seismograph”.) A suitable reference, although the year in which continuous operation resumed is given as 1926 instead of 1925, might be Charlier and van Gils (1953), which can be downloaded at <http://ds.iris.edu/seismo-archives/info/stations/Charlier1953.pdf>.

Much appreciated “research” by the referee, we shall follow his advice, and mention Mainka with a reference at the end of the first paragraph of the introduction: **“In 1909 a Mainka seismograph was installed in Reykjavik. It was operated until 1914, and again from 1925 when continuous operation was secured (IMO: Vedrattan (the Weather), 1924–2006)”**.

lines 31-37 — I would recommend, somewhere in this paragraph, stating that focal-depths are not given in the catalog, and explaining the reasons for this decision. This is currently discussed in lines 427-429, in which position it might come across as an afterthought.

ok. We shall end the paragraph with: **“Icelandic earthquakes are almost always less than 12 km deep, but the exact depth information is often not resolvable and therefore the catalogue does not include hypocentral depth.”**

lines 39-41 — You later (lines 273 - 279) discuss M_w (ZUR-RMT). I am thinking that M_w (ZUR-RMT) should also be mentioned at this point (around lines 39-41).

Here we are simply stating from which sources we obtain the data, not the original sources. We take all the ZUR-RMT that we use from the ISC online catalogue. Thus we do not intend to make changes to the manuscript in response to this comment.

lines 47-48 — This sentence does not do a satisfactory job of conveying why locations are the opposite of magnitudes. Most of the magnitudes in the catalog [particularly the M_w (GCMT)] are also based on teleseismic data, in contrast to the implication of the sentence. Moreover, errors in magnitude do not have the dimensions of distance that characterize location errors. So by what standard does one conclude that magnitudes are more accurate than

locations? I think the bottom line is that most of the uses for which the authors envision their catalog are more robust with respect to the likely catalog errors in Mw than to the likely catalog errors in location. But there are situations in which errors in magnitude can have more important consequences than errors in location. An example (although not pertaining to Iceland) would be the monitoring of nuclear-threshold treaties, in which anomalously high mb for a natural earthquake occurring within or near a national nuclear-test site may lead to suspicions that the nation that uses the test site is violating its signing of a treaty. In the case of the present ICEL-NMAR paper, I would recommend omitting discussion of relative accuracy of magnitudes and epicenters, and just focus on the reasons for using the Mw scale to express magnitudes of all earthquakes and on reasons for using local data to relocate the epicenters.

We think that the reason for this referee comment is that our sentence is not clear. What we are trying to convey is that locally determined epicenters are more accurate than teleseismically determined epicenters, whereas globally determined magnitudes are more accurate than locally determined ones.

We intend to amend the two paragraphs before the one being discussed: (a) State on line 36 that the magnitudes are MW (...reappraised **MW** magnitudes...) and (b) begin line 38 with: **“The most accurate magnitude information comes from international catalogs. Therefore the magnitudes are all copied...”**.

We also intend to rewrite the paragraph on lines 47–52: **“For the whole catalogue period local information is crucial for improving earthquake locations. Before 1955, and also for several subsequent events, written sources often provide valuable location information. Since 1955, when three seismometers were installed in Iceland covering the primary seismic zones, locally computed epicenters may be assumed to be more accurate than teleseismic epicenters in international catalogues, which are off by tens of km. One of the innovations in the new catalogue is therefore to use such local data. The primary local sources on epicenters are catalogues compiled at the Icelandic Meteorological Office (IMO), seismological bulletins, newsletters and reports published by the IMO and the University of Iceland Science Institute (UISI), journal articles with results of studies on Icelandic earthquakes, and contemporary accounts of earthquakes from newspapers.”** We hope this will be sufficient to clarify.

line 48 — There is an implication that the use of local arrival-times automatically leads to hypocenters that are superior to those based on teleseismic data. I would note that many of the earthquakes relocated in this study are at a distance from Reykjavik such that their first arrivals at REY will be Pn waves, whose computed arrival times (used in the location process to interpret the observed arrival times) will be sensitive to such characteristics of the model as assumed crustal-thickness, upper-mantle P velocity, and degree of anisotropy. For some of the early earthquakes, REY is the only station that does not lie to the east of the epicenters, and bias in theoretical Pn arrival-times at REY could lead to substantial bias in the epicenters. So the fact that a hypocenter is based on local arrival-times does not automatically make it superior to those based only on teleseismic data. That stated, I would agree that data from REY are an important supplement to teleseismic data, and that epicenters assigned by REY (IMO), determined with REY data, or somehow constrained by REY data, are prime candidates for preferred epicenters.

We hope to have addressed the concerns of the referee adequately by the proposed changes implied by our reply to the previous comment. We should emphasize that the article never *computes* locations from seismic measurements, but instead copies locations from other sources, sometimes complementing with judgement. This is made very clear in section 3.1. Also, before 1955, there are only 9 out of 109 events with the Icelandic Meteorological Office specified as “location source” in the catalog and we find it unlikely that the epicenters have been computed from REY together with only more easterly teleseismic stations.

lines 51-52 — Similar to my criticism of “expert judgement” in line 6, I think it is generally unnecessary to make a general statement that the authors have used their judgement in interpreting previously published data. However, for cases in which the authors’ judgements lead to seismotectonic inferences that are different than seismotectonic inferences previously made with the same or similar data, then the authors should indeed articulate, and take responsibility for, the specific judgements they have made in these cases.

We shall remove the sentence refereed to, namely: *“These sources are complemented by the author’s judgement”*. We also note that the catalogue itself clearly indicates locations where authors have relocated events (marked “New” under location source).

Figure 2 — The caption should give information on the catalogue(s) that are the source of the plotted epicenters and magnitudes (i.e., are these some set, which should be described, of previously accepted epicenters and magnitudes,

or are they the epicenters and magnitudes resulting from this study?). Comparing this figure with Figure 5, it appears that the epicenters in Figure 2 are consistent with those of Figure 5, but that the magnitudes are not.

The referee is absolutely correct, the caption is missing this info.. The locations and magnitudes are indeed those resulting from the current study. We shall add **“The displayed locations and magnitudes are those of the new catalogue.”** at the end of the caption. We shall also make sure that the magnitudes will be consistent between Fig. 2 and Fig. 5, by providing a new version of Fig. 2.

lines 74-75 — The assertion that the Grunthal and Wahlstrom (2003) magnitudes are systematically biased should be documented by citing a reference for this bias.

We shall rephrase the paragraph, emphasizing that we are comparing Grunthal and Wahlstrom’s magnitudes with those of our manuscript. As we point out in the ms their data comes originally from the IMO (with a URL of the source given in the ms), and not from ISC or other international catalogs. Therefore there is no reference that can be cited apart from those already provided. We shall however add some details on the magnitude differences for three period bins: **the average difference is 0.41 before 1970, 0.37 between 1970 and 1980, and 0.27 after 1980; 3rd quartiles 0.59, 0.47 and 0.36 respectively.**

lines 80-81 — This sentence should convey the reason why the paper of Woessner et al. (2015) is evidence that previously discussed hazard maps for Iceland overestimate the hazard. Does the paper document a consensus among hazard mappers that the the earlier estimates were greatly overestimated for Iceland, or is it simply that inspection of the hazard map of Woessner et al. shows lower hazard than shown in the earlier maps. Also, discussion of bias in the hazard maps that used the catalog of Grunthal and Wahlstrom (2003) should convey whether the bias is due entirely to magnitudes being biased, or is some of the bias is due to some other assumptions used in preparation of the earlier hazard maps, such as assumptions concerning site response.

We are sorry that we were not very clear. The Woessner paper is reporting on the SHARE results, with an estimated PGA for a 10% exceedance probability in 50 years as 0.4–0.5 g in Reykjavik, and we deem this as being a big overestimate, and support this by referring to four publications which estimate a corresponding PGA of 0.1–0.2 g. Yes, some of the bias is probably due to other assumptions. We shall rephrase the whole paragraph to read:

“For the Iceland region, all these projects adopted the original 2003 catalogue, adding data (locations and local magnitudes) after 1990 from IMO’s catalogue. Among the products of these studies was the “SHARE” hazard map for Europe, where the hazard was greatly overestimated in some places in Iceland, among them in the Reykjavík capital area, where the estimated PGA for a 10% exceedance probability in 50 years is given as 0.4–0.5 g (Woessner et al., 2015). Several recent local studies estimate 10% 50 year PGA as 0.1–0.2 g in the Reykjavik area (Sólnes et al., 2004; SCI, 2010; Sólnes et al., 2013; D’Amico et al., 2016). The reason for the presumed overestimation is likely a combination of errors in the underlying catalogs and differences in modelling.”

Section 2.1 — The catalog of Ambraseys and Sigbjornsson would not be an international catalog, if it only covers the region of Figure 1. It should be discussed in the next session.

We propose to change the title of section 2.1 to read **“Teleseismic catalogs”**, in order to describe our catalog classification better. We shall also amend the text in one or two other places to reflect this change.

Section 2.1 The Mw (ZUR-RMT) catalog should be mentioned in this section.

We actually consider the ZUR-RMT tensors not to be a separate catalog, as they come from the ISC catalog, but we have added a mention of them under ISC in section 2.1.1: **“Among other important agencies is the Swiss Seismological Service, providing the ZUR-RMT (Zurich Moment Tensors)”**. Instead we do not need to explain the abbreviation in section 4.1.1.

line 122 — The ISC did not exist before 1950. Its predecessor, the ISS, did exist. See <http://www.isc.ac.uk/about/>.

Yes, the statement is a little inaccurate. We shall change it to emphasize that it were the earthquakes that happened before 1950, but the reporting of them happened later: **“epicentres of events before 1950 reported by the ISC”**. Now it reflects better what the cited book says.

line 126 — Contrary to what is implied by this sentence, the USGS usually computes several magnitude types per earthquake, and these are given in some of the the output -formats of the USGS earthquake catalog-search that the authors cite. Also, the ISC on-line catalogs commonly attribute multiple magnitude types to the NEIC, which corresponds to the USGS. However, some output formats of the USGS/NEIC catalog-search do provide only one type of magnitude per earthquake. Also, for purposes of communicating with the media and the public, the USGS/NEIC does select a single magnitude value, so that the media do not get stirred up by the apparent “inconsistencies” of USGS magnitudes. Finally, there was a time when the predecessor to the USGS (NOAA) computed only mb values.

Yes, we agree, our description was confusing. We shall rephrase with the following paragraph: **“A simple online search in the USGS catalogue (2020) provides one magnitude value per earthquake (MW, MS or mb), although several magnitude types are often computed. The remaining values are in the ISC database, labelled USGS. Corresponding magnitudes from the two sources are in almost all cases identical. However the locations in the USGS catalog are different from those in the ISC catalog, the difference frequently amounting to a few tens of kilometers.”**

lines 132 — There is no mention of a Mainka seismograph in the current version of the Introduction. I have suggested (above discussion of lines 29-30) specifically mentioning the instrumentation in the Introduction.

Will be done!

line 225 — change “upto” to “up to” .

OK

line 254 — “and therefore the waveforms fit better” — this explanation for the reliability of teleseismic magnitudes would not apply to most magnitudes computed during most of the period covered by the catalog, computed from amplitudes and periods, but not based on waveform modeling.

We are not referring to source function modelling but simply the normal computing of MS and mb from the seismograms. To reduce the chance of misunderstanding we now say: **“The dominant periods at teleseismic distances are longer and the structure is smoother due to attenuation of the higher frequencies”**

line 257 — Similar to my comment on p. 11. It appears to me that, consistent with most studies that use the equivalent of a proxy Mw, the regression-determined equations that relate mb and Ms to Mw are determined from relatively recent earthquakes for which both the other magnitudes and Mw are independently available, and then these relations are used to determine the proxy Mw of the earlier events from the events’ mb or Ms. The authors description of their methodology implies that the mb and Ms of the earlier earthquakes are somehow included in the process by which the regression-determined equations are obtained.

We think we understand the reason for this comment: We say on line 258, that we use a “larger collection of earthquakes” for the modelling. We did not imply the use of mb/MS from earlier earthquakes, but instead from a larger region. To clarify we have changed: *“a larger collection of earthquakes than is really needed in the Iceland context is used to construct...”* to **“earthquakes from the whole NMAR region are used to construct...”**

line 275-279 How are the ZUR-RMT determined? The use of “RMT” to describe these moment tensors suggests to me that they are determined with regional, rather than global, data. The methodology and the data used for the ZUR-RMT should be briefly summarized in the paper, with a reference provided to the source of the ZUR-RMT.

We shall add a reference to “Braunmiller et al. 2002” which seems to be the “original” ZUR-RMT article at the end of section 2.1.1 where we also intend to mention it and the Swiss Seismological Service (see above comment on Section 2.1). The R in RMT does indeed seem to indicate *regional*. We obtain these data from ISC, and thus it is may be misleading to refer to them as a catalogue on line 275; we shall therefore change *“are listed in both the GCMT and the ZUR-RMT catalogues”* to **“have both a GCMT value and a ZUR-RMT value”**.

Figure 3 — caption, “improve visual appearance of the graphs” — I would recommend revising this reason to be more like that of the caption of Figure 5, which conveys the purpose of the jitter is to avoid superimposing different events (data points).

OK, shall be done.

line 384 —I would recommend changing "...events in the NMAR region, of these 933 are in the ICEL..." to "...events in the NMAR region, of which 933 are in the ICEL..." or "...events in the NMAR region: 933 of these are in the ICEL...". This is a stylistic quibble. The Reader (as did I) will know what you are trying to say.

OK, fine.

line 385 — Similar recommendation as that immediately preceding, for the current phrase "...2954 events in NMAR, of these 379 are in ICEL."

OK, shall do.

line 390 — I would recommend changing "and there" to "and that there".

OK

Figure 5 — It would be desirable to plot in this figure the tectonic features and some of the geographic that are shown in Figure 1, to make it easier for the reader to assess the spatial relationship between the epicenters of the new catalog and the tectonic/geographic features.

We shall add both the boundaries of the seismic zones (the TFZ and the SISZ), as well as some of the main tectonic features from Fig 1 (the ridge and fault lines).

line 400 — I would suggest changing "5" to "equation (5)"

We change it to Eq. (5) which is the journal standard style

lines 427-429 —"available information on hypocentral depth is very inconsistent" —the meaning of this phrase is not clear. The available information on hypocentral depths are consistent in implying that Icelandic earthquakes occur in the uppermost tens of kilometers of the earth's crust, as is implied by the next sentence of the paper. What the available information cannot do is to generally resolve the depth distribution within the uppermost crust. I do think that it is very important to state, as these lines do, that the catalog does not give estimates of focal-depth and that it is important to provide an explanation for not listing the focal depth. See also discussion of lines 31-37.

We shall remove the unclear phrase. Note that we shall also add a little discussion on line 37 (see our reply to lines 31–37) .

lines 478-480 — It would be desirable to explicitly state that you are assuming that relative plate motion at depths above 10 km is accommodated entirely by seismogenic slip (rather than aseismic slip), in addition to stating the assumption that most of the slip is occurring in the transform sections of the plate boundary.

We shall indeed add a small explanation after "10 km": "**below which the slip is assumed to be aseismic**"

lines 486-504 — These conclusions demand the labeling of tectonic and geographic features in Figure 5, as recommended earlier.

We shall indeed improve Figure

line 505 — "parallel" is mis-spelled.

OK

References — are the cited IMO publications now scanned and available on-line? If so, the on-line address from which they might be downloaded should be given in the citations to the publications

"The Weather" (Veðráttan) is online and shall add the URL of that to the references. Unfortunately the other two IMO publications are not (yet) on line.

line 623 —The Reference of Stefansson et al (1993) should include the title of the paper.

OK, well spotted

Comments on the "supporting-info.txt" file:

The citation (14) to the present paper in the "supporting-info.txt" file reflects an out-of-date plan for the publication of the paper.

Shall be corrected.

I would suggest introducing a paragraph entitled something like "ON THE FOCAL-DEPTHS OF EARTHQUAKES IN THE ICEL-NMAR CATALOG," in which you state that estimates of focal-depth are not given in the catalog, conveying that available evidence points to these events occurring at depths shallower than ** km (whatever is your best judgement), and providing the reason for the omission of focal-depth estimates.

--> Páll

In the DESCRIPTION OF CATALOG ENTRIES, MS should be defined as "Surface-wave magnitude" instead of "Surface magnitude".

OK. We shall also do a similar correction in two places in the main manuscript

In the DESCRIPTION OF CATALOG ENTRIES, mb should be defined as "body-wave magnitude" instead of "body magnitude".

Shall do. This time the main manuscript is ok

In the DESCRIPTION OF CATALOG ENTRIES, "loc-src", "epicenter location" should be shortened to simply "epicenter", since the additional descriptor "location" is redundant with respect to "epicenter".

OK.

In the MAGNITUDE AND TIME SOURCES, it is not clear to me what is represented by "ISCoother". For at least some of the events for which ISCoother is listed as the source of a type of magnitude, there are many estimates of that magnitude-type listed in the on-line ISC catalog. Is the ISCoother magnitude a mean or median of the different estimates of the magnitude-type?

It is a technical difference explained on the ISC home page. We shall include a reference to that page in the supporting-info file and also rephrase the line in question and say:

ISC	The ISC online catalog [9], reviewed magnitudes
ISCoother	The ISC online catalog [9], not reviewed magnitudes
	See http://www.isc.ac.uk/iscbulletin/search/bulletin/

The Internet address of USGS online catalog should be given somewhere in "supporting-info.txt".

We shall add that, and also the URL for the GCMT catalogue.