Natural Hazards and Earth System Sciences Discussions

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Comment on nhess-2021-15

Jim Dewey (Referee)

Referee comment on "A harmonised instrumental earthquake catalogue for Iceland and the northern Mid-Atlantic Ridge" by Kristján Jónasson et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-15-RC1, 2021

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 in 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.

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.

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.

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.

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.

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.

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.

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 Mw.

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.

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.

line 26 — typo — "where recorded" should be "were recorded".

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 .

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.

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

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 Mw(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.

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.

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.

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.

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.

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.

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 discussed in the next session.

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

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

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.

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.

line 225 — change "upto" to "up to" .

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.

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.

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.

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).

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.

line 385 — Similar recommendation as that immediately preceding, for the current phrase

"…2954 events in NMAR, of these 379 are in ICEL."

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

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.

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

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.

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.

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

line 505 — "parallel" is mis-spelled.

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

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

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.

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.

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

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

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”.

In the MAGNITUDE AND TIME SOURCES, it is not clear to me what is represented by “ISCother". For at least some of the events for which ISCother 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 ISCother magnitude a mean or median of the different estimates of the magnitude-type?

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