Author's Response to the Reviewer's Comments #1 (PAPER: Complementing regional moment magnitudes to GCMT: a perspective from the rebuilt ISC Bulletin, https://doi.org/10.5194/essd-2020-371)

Domenico Di Giacomo James Harris Dmitry A. Storchak

March 16, 2021

Response to Specific comments

We thank the reviewer for his comments. Below we reply in detail to each point showing, in bold, the Reviewer Comment (RC) and, in italic, the Author Response (AR).

RC: The manuscript is generally well written and easy to follow. The authors are inclusive and consider all possible agencies that are reporting Mw. However, the text would be easier to read by moving text on data sources that provide too few Mw reports (and are not much used in the study) to supplementary material.

AR: We have removed the sentences regarding agency MOS. Most of the small reporters are already in the Appendix.

RC: While Mw is well covered in the literature, the authors could add a couple of sentences on the basic methodology used by GCMT to obtain the seismic moment, which would be useful for the non-seismologist. It also would be useful to mention limitation of Mw in not being sensitive to energy release.

AR: We think that this manuscript is already long enough and the reader would not benefit from us adding a summary of the GCMT methodology. We,

however, pointed out in the revised Introduction that Mw alone is not able to "fully characterize the energy release of an earthquake"

RC: The authors avoid providing details on how Mw is measured by the smaller agencies. I think it would be important to at least explain that the methodology may change when going from large to small earthquakes (where moment tensor inversion may no longer be the method that is used). Ideally, the manuscript would benefit to at least give an indication of what is done by the institutions at the regional scale, perhaps in form of a table, as this is needed to even partly understand some of the observations that are made when comparing global and regional Mw.

AR: We have added Table 1 to summarize what we know about the methodology used by some agency to obtain Mw. Unfortunately for several agency that information is not available to us. The addition of Table 1 in the revised manuscript allows us to address other comments.

RC: It is also possible that Mw is the outcome of a standard automatic procedure – in that case it would be useful if and how the results are revised.

AR: As in previous reply, this information is not fully available to us.

RC: The authors explain that NEIC reports Mw provided by other agencies and these are labelled. I am concerned if the other way around, regional agencies may also provide Mw that they took from NEIC/GCMT. There may be no issue at all, but if there was the comparison would not be meaningful for those agencies.

AR: As also explained in reply to another comment below, this may only happen if an agency appropriates under its name the solution from another agency. We are confident that this is not an issue in our case.

RC: The authors appear to be using MS and mb to compare to regional Mw, this may require better justification. It also could be an alternative not to include non-Mw magnitudes in this work.

AR: This comment is also in the annotated PDF at page 9, line 260. We have added a sentence to the text to clarify why we also use mb and MS ISC

in the comparisons with Mw from GCMT and regional agencies. We believe that such comparisons add significance to the paper.

Response to Technical suggestions

RC: More detailed comments on content and language are given in the attached PDF.

AR: We are grateful to the reviewer for the language and other stylistic suggestions. We have accepted all of them. We also included (or modified) the text according to some of the reviewer suggestions. In particular we have added sentences in response to the comments at page 1, lines 24 and 25 in the annotated PDF.

In response to the suggestion at page 3, line 66, we have added a sentence listing the number of earthquakes in the DH Mw List with Mw≥5.0, and listed total number of earthquakes in the ISC Bulletin 1964-2017 with ISC mb≥5.0. We used ISC mb rather than any magnitude type available in the ISC Bulletin since we believe that this way we give a more reliable measure of the fraction of the earthquakes without Mw.

At page 4 line 98 of the annotated PDF, the reviewer points out that we give too much detail to NEIC compared to GCMT. This is necessary for two reasons: 1) details of NEIC Mw computations are less known compared to GCMT methodology; 2) there are four different types of Mw from NEIC. In addition, we think that this manuscript is already long enough and the reader would not benefit from us adding a summary of the GCMT methodology. However, we moved part of a sentence from the Conclusions regarding GCMT in this section, in line with another reviewer comment.

At page 4 line 120 of the annotated PDF, the reviewer suggest to move the sentence to another Section. We believe that the sentence better serves the reader in this section.

At page 5 line 154 of the annotated PDF, the reviewer suggest to go more into detail about the procedures adopted by the regional agencies. For this purpose we added in Table 1 a column providing the information whether waveform inversion or spectral analyses are used. With a few exceptions, we cannot be sure when solutions are fully automatic or revised/approved by a seismologist. For several agencies, to the best of our knowledge, the procedures to obtain Mw are not documented.

At page 7 line 219 of the annotated PDF, the reviewer points out that

regional agencies may report third parties solutions. In our analyses we reduce this risk by requiring that the Mw author is the same of the agency, therefore the issue may only occur if an agency reports under its author name third parties solutions. In our experience this is an unlikely eventuality.

Regarding the comment in the Conclusions at page 11 line 333 of the annotated PDF, we have added the following sentence: "It would also be desirable that agencies document the procedures used over time and whether automatic or revised solutions are obtained."

Regarding the last comment in the Conclusions (page 12, line 353 of the annotated PDF), we cannot dictate to reporting agencies a change in their routines operation to include Mw computations (but we do encourage them). We assume that the reasons for which several regional agencies do not compute Mw vary a lot, but we are not in a position to discuss that in the paper.

Finally, we feel that a detailed explanation of the content of the DH Mw list is required in the Section 7.

Author's Response to the Reviewer's Comments #2 (PAPER: Complementing regional moment magnitudes to GCMT: a perspective from the rebuilt ISC Bulletin, https://doi.org/10.5194/essd-2020-371)

Domenico Di Giacomo James Harris Dmitry A. Storchak

March 16, 2021

Response to General comments

We thank the reviewer for his comments. Below we reply in detail to each point showing, in bold, the Reviewer Comment (RC) and, in italic, the Author Response (AR).

RC: It would be nice to have a description, even a short one, of the different methods used in the different agencies to get the Mw.

AR: We have added a Table to list the agency full names and whether they perform waveform inversions or spectral analyses. For several agencies, to the best of our knowledge, the procedures to obtain Mw are not documented. Text with references to waveform inversions and spectral analyses have been added.

Response to Technical suggestions

RC: 1) Some of figures have to be modified, mainly maps of events because those with lower seismicity are mostly not visible. Probably an inversion of the plotting order avoid that bigger symbols

overlap smaller ones.

AR: We inverted the plotting order for all maps, which increased the visibility of smaller earthquakes. The reference to GMT (Wessel et al., 2013) in every figure with a map was required by the journal.

RC: 2) It would be really interesting, when possible, to add in the M comparison plots also other relation curves (e.g. the Bormann et al. 2013 or Lolli et al. 2014 or else?)

AR: We think that this request would not serve the readers since the figures are already quite rich of information and, as stated in the text, we do not aim to provide yet another set of conversion relationships from MS—mb to Mw or compare such solutions with previous studies. Our aim is to show how MS and mb are useful at lower magnitudes to highlight differences between regional Mw and global Mw values. The regression models plotted in the figures are only to guide the reader and to summarize the differences between global and regional Mw values. In addition, we cover different areas and for some of them we would not be able to show a model published in the literature. Also, although we can presume the dataset we analyses overlaps to some degree with that used, e.g., by Lolli et al. (2014), there is not guarantee we would make a fair comparison.

RC: I recommend also a review of the language.

AR: We have accepted the language suggestions in the annotated PDF. Furthermore, the language has been checked again by a native English speaker.