

COMMENTS

on the manuscript entitled:

“100+ years of recomputed surface wave magnitude of shallow earthquakes”

by D. Di Giacomo and D.A. Storchak

submitted for publication in “Earth System Science Data (ESSD)”

A. General Comment

Earthquake catalogs, extending over a wide period and covering the globe are useful tools for many studies. Two are the critical preconditions that must be fulfilled: accuracy in their focal parameters and homogeneity regarding the scale in which their magnitudes are expressed. Considering the fact that it is not suffering saturation but only at its large values, M_s is a suitable magnitude for such studies.

In this spirit, I believe that this work is very useful and it is my sense that its outcome (the catalog) is going to be extensively used in the future.

The paper is well written and its content corresponds to its title. There are some minor issues that I will describe below, which, if clarified, I believe will further improve the manuscript.

Concluding, it is my opinion that the manuscript can be accepted for publication after some minor revision.

Following are my comments in details.

B. Specific Comments

- 1) In the 1st paragraph of “Introduction” the basic pros of surface wave magnitude, M_s , are mentioned. I believe that the cons (e.g. inability of M_s estimation by using records of short period instruments and, therefore, of small local earthquakes, possible underestimation for very strong earthquakes) should be mentioned too.
- 2) Page 3: In the square root, the factor “2” must be out of the brackets:

$$\left(\frac{A}{T}\right)_H = \sqrt{2 \cdot \left(\frac{A}{T}\right)_{N/E}^2}$$

- 3) How have you estimated the final surface wave magnitude if more than one M_s values were available? Mean value? Weighted mean? Have you applied any filters to avoid contamination that could be caused by one or more potentially incorrect magnitude values that may deviate significantly from the majority of the rest?
- 4) The final catalog includes only events with recomputed M_s magnitudes, meaning that this catalog is not complete, as it possibly misses earthquakes which could be included in catalogs published by other authors, covering wide regions and extending over wide time periods, but with magnitudes consistent (not original) to the standard M_s , (e.g. Karnik 1996).

- 5) In lines 72-74 you mention that: *“The locations adopted in this work come from the ISC-GEM Catalogue (Bondár et al., 2015; Di Giacomo et al., 2018) between 1904 and 1963 and the rebuilt ISC Bulletin (Storchak et al., 2017, 2020) from 1964 onward”*. Looking in the ISC-GEM catalog I could not find some earthquakes included in your catalog. Indicatively I mention the following events: 1904-12-02, 02:19:12; 1904-12-11, 17:05:42; 1908-01-31, 04:49:15 etc. These events are also not included in the online ISC bulletins. Figure 3 clearly shows that data before ~1950 are coming exclusively from ISC. So, which is their origin?
- 6) Chapter 4 entitled “Catalogue Properties” gives a detailed and very useful analysis of the time-history of Ms scale. However, it looks there are four gaps regarding earthquakes of Ms<6.0 that can be observed in figure 10: one at ~1920, the second during 1940-1950, the third between 1960 and 1978 and the fourth between 1980 and 1984. The authors are right about the impact of World War 2, which justifies the second gap. According to the authors (see chapter 6), there is further work to be done that will possibly allow some of the above gaps to be covered. So, consider this as just a remark.
- 7) In the same chapter (4) and in lines 160-170 there is an analysis of the features of the formed catalog. It is mentioned there how the completeness magnitude, Mc, is distributed over time. There is a point here that, in my opinion, needs clarification. To proceed to a meaningful Mc estimation and to study its variation with time it is necessary to know first if there are earthquakes systematically missing from the data set. I mean, are there any earthquakes whose focal parameters are known but they are not included in the catalog because it was not possible to have Ms estimation for them? If yes, then I believe that the term “completeness magnitude” should be avoided as, at least literally, it has another meaning.
- 8) Figure 11 & Lines 180-190 (a follow up of the previous comment): The rates shown in figure 11 do not necessarily show variation of completeness magnitudes over time.
- 9) Figure 12: The Ms underestimation for very strong earthquakes (e.g. $M \geq 8.0$) has been already observed and noticed (e.g. Heaton et al., 1986). For the example of the magnitude of the Aleutian earthquake of April 11, 1946, the magnitude reports in the ISC bulletin are: Ms=7.3 (after Abe, “Phys. Earth planet. Interiors”, 1981); Ms=7.1 & Mw=8.0 (after Pacheco & Sykes, “Bull. Seism. Soc. Am.”, 1992); Mw=8.6 (after López & Okal, “Geophys. J. Int.”, 2006). These values are clearly showing (as the authors of this manuscript state) that Ms values underestimated the real magnitude of this great event. Therefore, although the data in figure 12 are not many, it can be stated that the “saturation” of Ms for values over ~8.0 is confirmed here and should therefore be considered as a fact.

C. Technical Corrections

I could not make it to locate in the text the following two references (included in the “References”):

- 1) Line 339: Bormann (2012)
- 2) Line 350: Di Giacomo and Storchak (2016)