NHESS-2022-186 Seismogenic potential and tsunami threat of the strike-slip Carboneras Fault in the Western Mediterranean from physics-based earthquake simulations

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Comments on revised version by Luis Matias, University of Lisbon

Recommendation

It is my recommendation that the work is ready for publication with minor corrections that are detailed below. The major concern on the current version has to do with the tsunami modelling procedure. It was not clear for me on the first version that the modelling included inundation. This misunderstanding raised several questions that were not appropriate if inundation was computed. Inundation requires a proper DTM for the land mass and this source of information is still missing on the current version. As a broad comment I suggest the authors to clarify this issue.

Major comments

In the following I make additional comments (in blue) on top of the *original comments (in black and italic)* and the author's reply (in black). Only those comments that merit additional changes to the manuscript are mentioned.

Part 1:The physical model for earthquake generation

My major concern regarding this subject is the lack of relationship between observed seismicity and the Carboneras Fault. This can be inferred from Figure 1 in the paper but is made clear on figure REV-01 (not repeated here).

As the reviewer knows, in zones of low or very low tectonic activity, the correlation between

instrumental seismicity, of moderate and low magnitude, and the main faults is not direct. On the one hand, location uncertainties can be of several kilometres, and on the other, the epicenters of historical events suffer from a lack of direct observations of shallow fault ruptures. If we also take into account that the seismic cycles of these faults last thousands or tens of thousands of years, it is logical to expect that the instrumental seismicity of a few decades will not reflect the seismogenic behavior of large structures, hence the interest of physics-based models.

I agree with the author's comments, but I am in favour that this information should somehow appear in the manuscript to emphasize the use of the methodology to other slow deforming regions.

The paper mentions that some model parameters are tuned so that the final Gutenberg-Richter (GR) law has a b value equal to 1.0. The paper fails to give the support for this assumption and no information is provided on the a value that also characterizes the GR law. Assessing the ISC catalogue and selecting a generous area surrounding the CBF we obtain the GR law shown in figure REV-02, where the number of earthquakes is scaled to 1 Myr as in the paper.

We obtain a very high b-value, not common for convergent or transcurrent domains, showing that large magnitude events are much less frequent than found on average on the earth. This may be a feature due to the small number of events, but it deserves discussion. The thickness of the brittle layer assumed for the physical model deserves additional discussion in the light of information provided by the earthquake catalogues and deep structure studies in the area.

The calculation of a Gutenberg-Richter fit requires that the magnitudes of the events used be homogenized in order to be comparable, in addition, a completeness analysis must be done to filter the events by date and the fit should preferably be done with a maximum likelihood adjustment. Nor is it possible to extrapolate the seismicity of a few decades in a seismic cycle of thousands of years, to a behavior of hundreds of thousands or millions of years, for this reason the value of "a" of the Gutenberg Richter law, which depends on the seismic productivity is not used, but the "b" value is compared so that the distribution of the size of the events is similar to the real one. As explained in Herrero-Barbero et al. (2021), one of the criteria for choosing the best-fit model parameters is that the b-value be close to 1, considering always the same completitude magnitude between several synthetic catalogs. This b-value is justified by the estimations in the same seismogenic zone in previous works based on instrumental seismicity (García-Mayordomo, 2005; IGN-UPM, 2013; Villamor, 2002), and is also a reference value as assumption in numerous papers of synthetic seismicity modeling (e.g., Console et al., 2017; Shaw et al., 2018). These references have been included in the text (lines 151-154). The seismogenic crust thickness of the model is based on previous seismotectonic studies at Southeastern Spain (García-Mayordomo, 2005, Fernández-Ibañez & Soto, 2008; Mancilla et al., 2013, Grevemeyer et al., 2015). These references have been included in the text (lines 126-128).

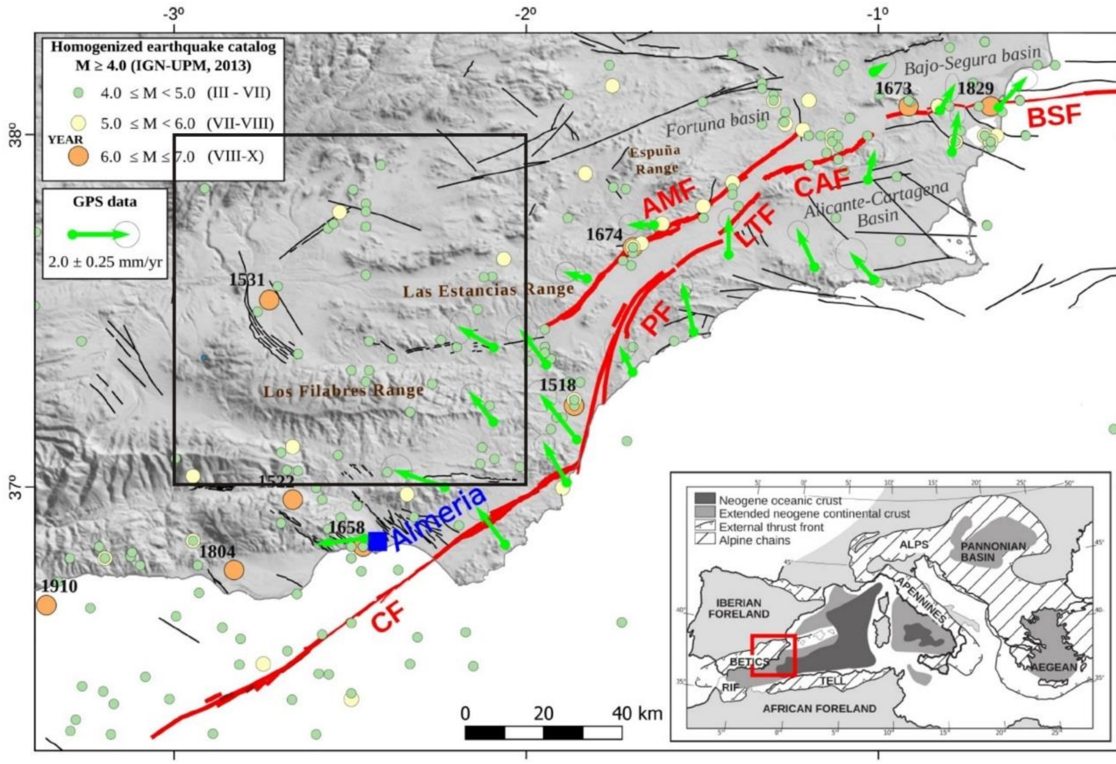
In my opinion, some short version of this information should appear on the manuscript, not for the benefit of the reviewer, but for the benefit of the reader.

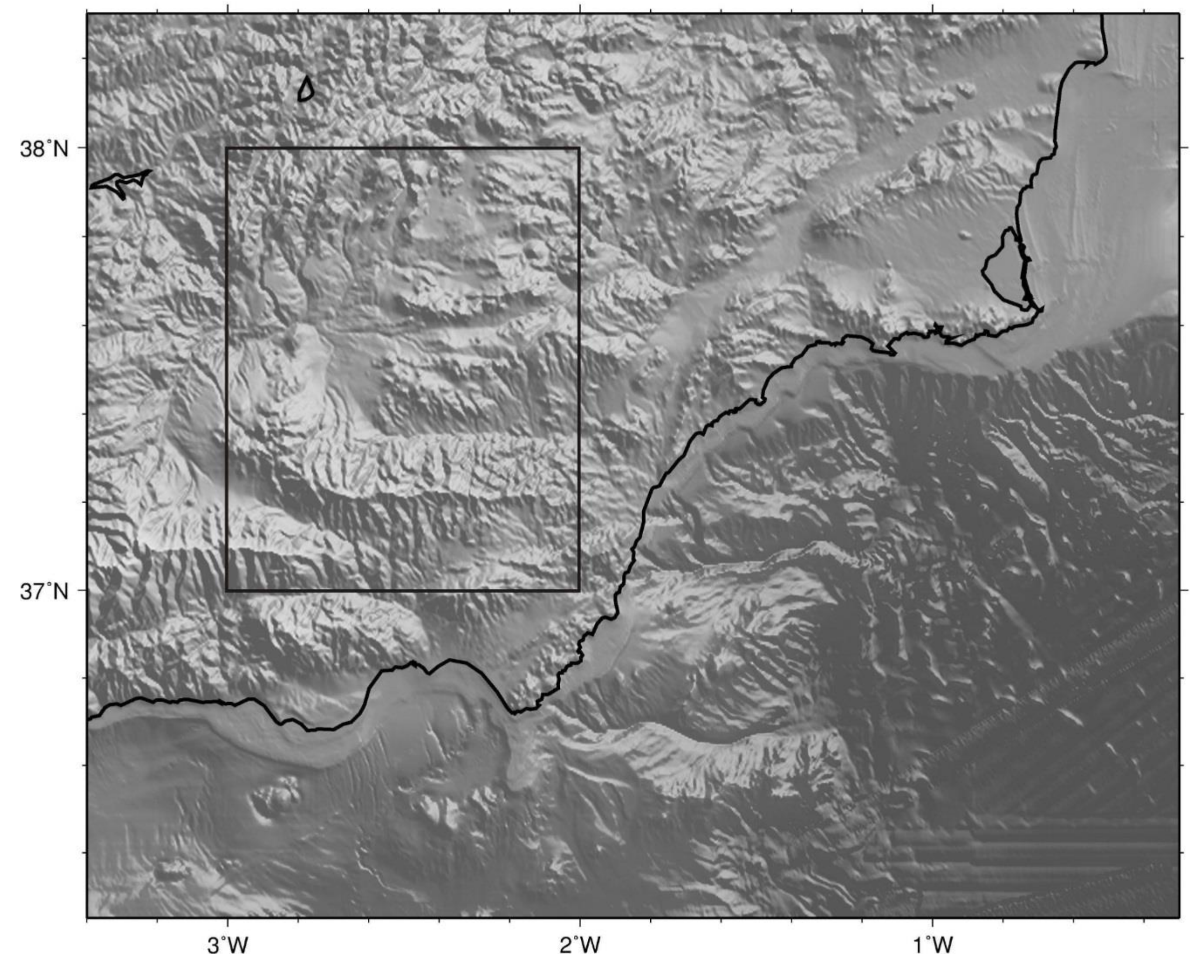
Additional comments

The geographic projection is "Plate Carrée" which is very unusual. Renders the comparison with other maps difficult. Why this projection was used?

The map shown in figure 1 has been generated with QGIS using a standard Mercator projection. Maybe it has been slightly modified by the vector drawing program used (Inkscape) but we think that for the purpose of the map (a location map) is precise enough.

The figure on the next page shows the geographical area of figure 1 as presented in the manuscript and as plot with Mercator projection by GMT. I added a rectangle 1º by 1º on both plots. This shows that the projection used in the manuscript is not Mercator as claimed. I do not suggest redoing the figure, just mention on the caption the geographical projection used, for the benefit of the reader.


Homogenized earthquake catalog
M ≥ 4.0 (IGN-UPM, 2013)
4.0 ≤ M < 5.0 (III - VII)
5.0 ≤ M < 6.0 (VII-VIII)
YEAR
6.0 ≤ M ≤ 7.0 (VIII-X)
GPS data
2.0 ± 0.25 mm/yr
-3º
-2º
-1º
38º
37º
Bajo-Segura basin
Fortuna basin
España Range
Alicante-Cartagena Basin
Las Estancias Range
Los Filabres Range
Almeria
AMF
CAF
LTF
PF
BSF
CF
1673
1829
1674
1531
1518
1522
1658
1804
1910
0 20 40 km
Neogene oceanic crust
Extended neogene continental crust
External thrust front
Alpine chains
ALPS
PANNONIAN BASIN
IBERIAN FORELAND
APENNINES
BETICS
RIF
TELL
AFRICAN FORELAND
AEGEAN


38˚N
37˚N
3˚W
2˚W
1˚W

Lines 154-155: As the sea-floor deformation generated by the earthquake is usually transferred instantly to the 155 elevation of the water free surface

This is not true in general, though it applies to the modelling of far source tsunamis. For locally generated tsunamis there are two effects that are not considered in the paper that deserve a comment: i) the finite compressibility acts as a filter when computing the sea surface deformation (e.g. Lotto & Dunham, 2015); ii) the horizontal movement of the sea bottom, in areas of relief, generate an initial velocity on the water that, in some circumstances, must be considered.

We have reworded the sentence (lines 198-199).

I see that the authors address (ii) above but not (i). It is a detail that is missed in many tsunami simulations but for this manuscript its relevance may be considered second order.

3. Tsunami modelling

5) How is the tsunami amplitude computed? It is recommended that the tsunami wave amplitude to be computed at cells with water depth no smaller than 50 m. The reason is explained in Kamigaichi (2011): "To represent the tsunami waveform correctly in a shallow sea area, very fine bathymetry data mesh is necessary (in a strict sense, 20 or more grid points are necessary within one wave-length [31]), and a vast time is required for the completion of such detailed calculations. To overcome this difficulty, the numerical simulation with the long-wave approximation is applied only to points which are a few to a few ten kilometers seaward from the coast ("forecast points") where sea depth is about 50m. Then, tsunami amplitude at the coast is calculated by using Green's law described in the next section."

We have modelled inundation at the coast and consequently there is no use of the green's law.

This is not completely satisfying. The figures presented in the manuscript show only "maximum elevation" but no inundation, generating the question in my original comment. Is it possible to show one inundation map as supplementary material?

Line 204: maximum wave elevation

The meaning of this parameter must be well explained. See my previous comment.

I think I don't fully understand the reviewer concern with the term. It is the widely used term to describe the maximum elevation reached by the free water surface at a point of the calculation grid on a propagation.

This comment is indeed true if the tsunami propagation extends to the inundation phase. The mentions of "inundation" in the manuscript are scarce:

Lines 232-233: … a Manning's roughness coefficient of 0.02 when computing the inundation.

Lines 250-251: and with relevant inundations, in the Almerian coast (Figure 9).

Figure 9 only shows maximum elevation, not inundation that we might see. At least it is not mentioned in the caption.

Line 253: and with relevant inundations, in the Almerian coast (Figure 9).

Line 303: and consequently the statistical distribution of maximum elevations and inundations.

I believe that the computation of inundation in tsunami modelling should be clarified or emphasized, given its relevance for the discussion. For inundation the authors need a detailed DTM for the land mass but no mention to it is found on the manuscript.

Line 211: relevant local inundations

It is not explained how "maximum elevation" is converted to inundation. See previous comments.

The inundation is computed by means of the COMCOT numerical model.

The only reference to COMCOT is found on line 224: In order to model the tsunami propagation we have resort to the highly used and validated code COMCOT.

In my opinion "tsunami propagation" is not equivalent to the computation of "tsunami inundation" which is more demanding computationally and requires new detailed datasets not mentioned in the manuscript.