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Interactive comment on "What seismicity offshore Sicily suggests about lithosphere dynamics and microplate fragmentation models in the Central Mediterranean" *by* Giancarlo Neri et al.

Anonymous Referee #1

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In this manuscript, the authors reported the analysis of an updated seismic catalogue for the central Mediterranean area, namely, the surrounding region of Sicily. The authors analysed the seismic parameters (focal mechanisms and stress inversion) for events occurred both in the off-shore of the Messina straits, east of Sicily, and Sicily channel, south-west of Sicily, and on-shore, southern half of Sicily. The authors defined a separation, running roughly NE-SW, between the seismicity occurred in the Messina straits area and the on-shore seismicity plus seismicity of the Sicily channel. Such separation is commented in the framework of local geodynamic processes (slab retreat, STEP fault propagation and so on).

Overall, I found this study of limited interest due to the lack of a relevant new analysis of the seismic data and other main issues listed below. The authors seems to relocated off-shore events using Bayloc, which is a interesting operation due to the poor azimuthal coverage for such events, but, then, they did not move the analysis further. All the considerations about the stress distribution (i.e. the "main findings" of the paper) are based on previously published focal mechanisms. In my opinion, the manuscript should be rejected. I list below my main concerns.

Main points.

A. Limited new data analysis.

First of all, I specify that the methodology is poorly described and, also, uniformly distributed along all the manuscript. This took me a lot of time to clearly define the author's workflow for the analysis of the seismic data (I will go back to this point later). So, I could have mis-understood the process of data analysis. Say so, the new steps of analysis seem to be confined in: (a) relocation using Bayloc; and (b) computation of stress inversion. I consider the first step as a good idea, given the general poor location for off-shore events. However, it is not clear if the new locations impact, in any sense, on the main findings of this manuscript. That is: are the revised locations giving new fundamental information for the next stress inversion? A careful reading of the manuscript suggests a negative answer. Moreover, the seismicity distribution presented (for an area of hundreds of square kilometers) should be compared to the original one, to highlight the improvement with respect to standard locations. The revised catalogue should be published on a regional journal. Finally, I consider the "stress inversion" a tool for routine data analysis (to be clear, the references reported by the authors span between 1984 and 1996). I also use it to investigate my data-set, but I consider the results as a preliminary support to other, more complex, steps of data analysis. I discuss the separation of the stress inversion in two sectors below.

B. Segmentation of the stress inversion solutions.

The author claimed at line 308-310 (L308-310, hereinafter) that they obtained “more meaningful” results dividing the focal mechanisms in two areas and computing two different stress inversions. The authors overlooked to explain the meaning of “more meaningful” results. This could be fine, but then, few lines below (L337-339) they claimed that the E-W separation of the focal mechanisms, which was originally introduced by themselves without any detailed explanation, approximately corresponds to the Alfeo-Etna Fault. And this is a fundamental point in the findings of this manuscript. So, the author must illustrated how they produced the E-W separation of the stress inversions and on which criteria they based their claims.

C. Self-citation

The authors cited 20 of their own studies, out of 64 total references. This could be considered citation stack. Many of those self-citations come along with other citations or can be replaced, so the number of self-citations can be easily reduced without any impact on the manuscript.

D. Manuscript structure.

I found the manuscript not well organized and too long in many sections. For example, the Introduction and geodynamic background sections occupy 6.5 pages, while Data, methodology and results are described in 2.5 pages. Methodology is partially described in the Discussion section (e.g. see L300-311, where the authors introduced the partition of the stress inversions). Results should be clearly separated from Discussion section, while the authors describe details of the results throughout all the Discussion section.

Minor points

1. No new data.

The authors did not seem to add any additional seismic data in terms of raw recordings, new seismic stations, new pickings of seismic phases and so on. This is not fundamental, but it must be clearly stated.

2. I’m not English native speaker, but I suggest to improve writing style. Also, writing style is somehow awkward (e.g. see L83-84, “below . . . cessation”, where the subject is the “slab”, but the slab is not close to cessation, is the slab retreat process that is close to cessation).

3. The authors used Bayloc to improve estimate of uncertainties in location parameters, which is a very good choice, from my point of view. But, then, they used, in the stress inversions, estimates of uncertainties on the focal mechanism parameters obtained from linearized inversions, which are, for such poorly located events (i.e. off-shore events) definitely un-realistic. Such estimates must be completely neglected. Focal parameters for off-shore events, with such poor azimuthal coverage, must have larger uncertainties than reported values.

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