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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 #2

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The presented research work concerns an area which is still poorly understood, though close to the "heart" of Europe, therefore this is, in principle, a very interesting paper. The manuscript contains two (2) main technical results: a) A focused relocation for two areas south and to the east of Sicily and, b) a revised stress tensor inversion, for the same areas, including the southern section of Sicily located between the two previously mentioned regions. These findings are employed to discuss the geotectonic setting of this area of interest and propose a revised seismo-tectonic model for the region.

Unfortunately, I found the manuscript to contain several technical problems and obscure points in the data processing, that really make the whole approach questionable:

The reader cannot trust the results, therefore why should he proceed to evaluate the interpretations. Moreover, it is evident that there is no evaluation on what are the new results that this manuscript contributes, and which are used for the geotectonic model interpretation. In other words, the paper has both technical and originality issues, which lead me to the suggestion that it cannot be published. Personally, I think that the authors need to completely revise the manuscript and their approach. They need to really provide new data processing and information, and only then tackle the geotectonic model interpretation, which is their primary goal. In the following, I am making some specific comments to explain the main problematic issues and provide some specific suggestions to the authors.

Language: The manuscript is not bad, but in some sections or sentences the English is poor and the reader struggles to understand what the authors are saying. In some cases, the authors use complicated (but not accurate language), making the whole text a puzzle, e.g. in lines 63-66: "...The potentialities of the methods of seismological analysis we have planned to use, in part of recent conception and in all cases already proven to be effective in the study region (see e.g. Neri et al., 2005; Billi et al., 2010; D'Amico et al., 2010; Presti et al., 2013), make us confident that the above declared goal of the investigation may be reached...", where they are essentially saying that their methods are working for the study area, so they expect to realize the manuscript targets, but in very difficult to read (and not correct in some cases) language (by the way, I would suggest to avoid such statements: Let the reader evaluate if they are doing a good job, on the basis of the data and methods, you do not need to advertise this in advance!).

I would strongly suggest that the manuscript is read by an English language expert and focus on the simplification of expressions and complicated statements. Please use smaller paragraphs (in section 4, one paragraph starts in page 11 and ends in page 13!) to facilitate the reader to follow your arguments.

Organization, Figures and Tables: The paper is organized in three (3) parts. The first presents the study area, existing models (for the broader area) and input data, and corresponds to Figures 1 to 5. The second section concerns the technical results (Figures 6, 7 and 8), while the last section and Figure 9 present the final model, proposed on the basis of the technical results. This is clearly not well-balanced. The authors spend 10 pages for the 1st part, 2.5 pages for the technical result presentation and 8.5 pages for Discussion-Conclusions. I agree with the 1st reviewer, the manuscript is lacking a proper data processing presentation. I am not talking about the data interpretation, but the actual commenting and evaluation of the result reliability (comparisons of relocations with initial catalogue, stress presentation on a map including comparison with previous findings by the same group such as Totaro et al. (2016) in a figure, and quantification of the new information that has been determined by the data processing, etc.).

The first part is simply too long: I found the discussion of Figure 3 interesting but tiring (for the reader) and in some places irrelevant. The authors present sequentially all models, though these models focus in different areas, e.g. Fig. 3g and h concern their region, most other models include the whole Adria and even northern Italy, where some of the discussed model differences have little to no implication for their area of interest. Because models are so different and are based on different datasets, the authors cannot explain in detail the models: For example, what exactly are we seeing in Fig.3g and 3h (not explained in the legend), what are the colored zones in 3e and why they have different colors (I suspect it is the faulting pattern, but why should the reader guess?)?

I understand that it is difficult to do this model comparison in a technical (and not a review paper). The authors should consider avoiding the discussion about the whole Italian, Adria, Tyrrhenian Sea, etc. territory and make smaller, focused model comparison only for the smaller area of interest. They do not need to discuss all models, just those ones that have fundamentally different implications for the geotectonic setting of the broader Sicily area. Since some models are evolutions of previous ones, stick to

the important ones and their main features. Presenting 3-4 models for the study area, where the main model features are discussed, instead of 10 models for a very large area, will make the manuscript smaller and better, more focused. I know that the authors want to present the “general” picture, but this belongs to a review article, not this one.

Data processing and Discussion sections: This is the main reason that I recommend rejection for the paper. My main reservations are explained in detail, and have to do with the reliability of the data processing, the input information, and the definition of the added value obtained from the relocations and stress inversion. More specifically:

A) The first technical target of the manuscript is to relocate seismicity in a relatively large area. The authors use the SIMULPS linearized code and the BayLoc non-linear locations, with a 3D velocity model (Orecchio_et_al., 2011). There are several problems here:

1) SIMULPS is not a location tool. Though you can optimize locations using a 3D model with SIMULPS, taking advantage of the linearization of the travel-time equation and solving only for locations (and this is what people have done sometimes), it does not have the capabilities built in standard linearized locations algorithms, even older ones (e.g. HYPOINVERSE), like azimuthal and distance weighting, outlier iterative rejection with progressing smaller misfit tolerance during location iterations, etc. It is correct that locations (with a large number of P and S arrivals) move spatially when using SIMULPS, responding to the improved velocity model, but at the same time they tend to disperse/defocus (especially events with a smaller number of phases), since SIMULPS is not made to handle these events. There are much better approaches for linearized inversions. A good choice for the authors would be to use TOMODD (Zhang and Thurber, 2003), which allows for the use of a 3D model, performs double-difference relocations (which really improve locations, like HypoDD) and has improved outlier rejection capabilities. SIMULPS is efficient, but simply not made to be a reference algorithm for linearized locations using 3D models, it is mainly a tomographic model

algorithm.

2) The authors claim to use a 3D P-velocity model (Orecchio_et_al., 2011), which improves the locations. However, this is clearly not the case. In the attached figure of this review (Fig. 1), I have superimposed the resolution area (red dashed line) of employed 3D model, as presented in Fig. 6 in Orecchio et_al. (2011) for the depth of 10km, which is the depth for which the resolved area shows the largest spatial extent (for all other depths e.g. 20km and 30km this area is smaller, therefore the actual 3D model resolution is worse than what I show here). The 3D model model stops at the depth 40km (see the plot in Fig 6, which is identical to the starting (a priori) model of Fig. 4 of Orecchio et_al. (2011)). In fact, the 3D model has section where the maximum resolved depth is at ∼20-30km (see section B-B' in Fig. 6 of the same work). Outside the red line there is no 3D model, just the starting (a priori) model that the authors have built from a limited number of seismic profiles, and which they present in Fig.4 of the Orecchio et_al. (2011).

From this figure several things are evident and several questions can be asked:

a) The Western Ionian (WI) area is outside the 3D model. Therefore, locations for this area of focus really do not have a 3D model determined from the inversions of Orecchio et_al. (2011), just an a priori model, which is clearly of much, much poorer accuracy.

b)The Sicily Channel (SC) area is even outside the a priori model of the Orecchio et_al. (2011), which the authors claim to use. I am wondering how did the authors actually perform any locations in this area where they focus their attention; did they expand the 3D model? How was this done?

c) What about locations with SIMULPS for the remaining area (black dashed line in the previous figure), which are clearly outside the 3D-model (red dashed line) or even the a priori, starting model (blue dashed line) of Orecchio et_al. (2011)? What did they do for these events? No information is provided in the manuscript.

d) The recovered Orecchio et_al. (2011) 3D model stops at 20-40km (depending the area). The a priori (starting) model stops at 40km. What model was used for locations down to 100km (as stated in Fig. 4d) or for the range 30-70km for Figures 6d and 7d?

e) What S model was used, since the Orecchio et_al. (2011) model is a P model? How was this constrained?

It is clear that there is no 3D tomographic model for either focus areas (WI or SC) and not even an educational guess model for the SC area (at least the authors do not describe one). This simply means there is no added accuracy in the recovered epicenters, and that the reported location errors (5km and 9km hypocenter errors for WI and SC) are simply super optimistic. It is well known that errors reported from any location algorithm as biased to low values when the models are wrong, especially when there is a large azimuthal gap, such as the case for WI and SC where the network is one-sided. Epicenters far from the coast may be of by >10 (or even tens) of km, especially if the crust is very different (e.g. fast crust in SC or WI areas, which are more similar to remnants of an old ocean, compared to the continental [slower velocities] of Sicily or southern Italy areas).

This is evident in the results: The locations in SC south of Lat∼36.5 show this near vertical depth distribution from 10 to 70+ km, appearing in the cross-sections (e.g. Fig. 7) as a vertical band of events, typical of very poor depth control. The locations for the depth range 30-70km (mantle range) look meaningless, and rather random, not providing some important seismotectonic information.

4) Even if the reader tries try to forget the previous issues, we still are missing the most critical point: The demonstration of the improved accuracy of new relocations. The authors present no comparison (e.g. initial [catalogue] distribution of events, revised relocations with SIMULPS, focused relocatios with BayLoc in the focus area). What, exactly, did they (and we, as readers) gained from this whole effort? What seismotectonic features are now seen with the relocations, that are not observed e.g. in the

catalogue data? The authors do not say, and the reader is left to wonder about the technical data reliability and contribution to this work.

5) A minor issue: It is not possible to make quantitative comments on seismicity with this dataset, e.g. lines 284-285 "…Also, a clear drop of activity can be noted in the Ionian offshore of Southern Calabria (Figs 4c and 5)…". The authors have not studied or presented the catalogue completeness, or its spatial variability. Therefore, seismicity changes may be apparent e.g. poorer detection and location of small events, as we move away from the Sicily coast, due to network detectability. First, they need to demonstrate the catalogue space-time completeness, then show only data that are within this space-time completeness intervals, and only then can they make quantitative assessments of seismicity.

B) The second technical target of the manuscript is to define a revised stress field for the study area, using an updated Fault Plane Solution (FPS) catalogue. Again, the reader is confused:

1) The authors use the famous GF method, and some rather old criteria for stress homogeneity, from >20 years ago. While the work of Wyss et al. (1992) and Gillard et al. (1996) is reliable, the GF method has received a lot of criticism, and several modifications (e.g. Michael, 1987, Lund & Slunga, 1999, Vavrycuk, 2014, 2015, Karagianni et al., 2016) have been proposed that overcome the GF problems, either by adopting a different solution stability check, or by using different criteria such as the instability criterion adopted initially by Lund and Slunga (1999). Why did the authors not compare their results with those obtained by other, more recent methods, in order to verify the robustness of the recovered stress field?

2) The zonation procedure and the presentation-discussion of the recovered stress field is completely obscure and definitively was performed in a different way than the approach described by the authors. The authors state (lines 300-311) that they subdivided the FPS in tens subcatalogues, using their spatial characteristics, and made

sure to have at least 20 FPS in each group. They then computed the stress and used the F value to assign stress homogeneity, based on some previously proposed cut-off values. The F-values were used to decide on spatial clustering.

However, it is completely unclear how this was done. How do you separate FPS in a semi-automatic manner, and finally end up proposing areas separated by a straight line (Figures 8b and 8c)? How was this separation performed and what is the significance of these lines? Are we to simply trust the authors that these boundaries somehow magically appeared in their systematic search? The authors do not show any other results (e.g. in an electronic appendix) but simply state that they decided to "…report…results that we consider more meaningful…". Meaningful in what sense? According to which criteria, or even subjective evaluation? For example, how was the RZ area actually defined? A minor issue: How was he earthquake magnitude used in the FPS grouping, as the authors state in line 302? To the best of my knowledge, the earthquake magnitude can be taken into account as a weight, e.g. in the inversion process, but not in the sub-area separation. Please clarify.

3) The results are really not presented in a clear manner. First, it is clear that the only consistent stress axis is the σ3, horizontal extension (see last column of Table 1) and that the separation that they perform leads to a σ1 instability for the East part (E in Fig8b, and RZ in Fig. 8c). In my opinion, this simply means that they are mixing FPS belonging to very different regimes. For example, Fig.6a shows that the depth distribution in WI is complicated. Have the authors separated seismicity with depth, or simply included all FPS from shallow crustal depths (0-20km) to upper mantle (50-70km) in the same stress inversion? Is this realistic? In a subduction area you often have transpresional deep events along the slab, with normal events at shallow depths (back-arc area); grouping events from all depths, upper crust to sub-Moho mantle, will certainly give you a very heterogeneous (and probably wrong) stress field.

The stress recovered for RZ and E are meaningless. The 2-sigma area for σ1 and σ2 is practically a vertical plane (also identified by the authors), suggesting that compression changes and that the dataset can not recover the changes, most probably due to the adopted spatial grouping.

4) Even without considering the previous problems, no discussion and visual comparison is made with previous results. Authors from the same group (Totaro et al., 2016) present results for the WI and part of the SC areas (see their Fig. 3). The stress recovered for area 6 of Totaro et al. (2016) is nearly identical to the one shown by the authors in Fig.8b for the East area (E). Visual comparison of the FPS datasets also confirms that the majority of FPS is identical, which explains the similar results. Therefore, what additional information did the additional FPS bring into the final results? What is the new information on the stress field of the WI area?

For the West (W in Fig 8) area, the authors have many more new FPS, but the obtained result for this area are very similar to the results obtained for the (smaller) area 7 by Totaro et al. (2016). Therefore, what is the new knowledge that we gain by the authors (very unclear) processing, that is used by the authors for their model and that was not present and discussed by themselves in Totaro et al. (2016), a few years ago?

5) Minor issue: The authors state that the stress in the west (W) is more consistent with the average from all data (ALL, Fig 8a). However, it is the East cluster (E) stress that is identical to the overall average (see Table 1). In fact, stress for W presents a $\sim$10 clockwise rotation with respect to ALL and E, which should be considered as significant, if the FPS errors are of the order of 10 degrees, as the authors state. In other words, the numbers tell a slightly different story, that the one presented by the authors.

I could provide additional comments on the Discussion section. Perhaps an important one is that, after an extensive introduction, spending nearly one page (p. 13) to present and discuss features already known (as the authors themselves state), seems like a waste of time. However, specific comments are simply meaningless, since the input information (seismicity relocations and stress inversion) are not reliable: The process-

ing has a lot of obscure points, even some that can be considered as partly flawed, weak presentation and no identification of the added value gained by new, additional information. This makes the last part of the manuscript practically irrelevant and hard to evaluate.

My last remark is a rather personal one: Reading the response of the authors to the comments of the first reviewer, I would strongly recommend to the authors to adopt a less aggressive attitude to reviewer comments. A reviewer that is not a native English speaker, can still understand that the English of the manuscript is rather poor, therefore there is no point to sarcastically address his criticism by mocking his own English fluency and accuracy. The same applies for the other scientific comments: I agree with the overall assessment of reviewer 1, that the manuscript appears overloaded in the Introduction and very weak in the technical section, as I also explained. I would strongly recommend that the authors take the advice proposed here, which is what two readers independently suggest, and use it to completely re-process data and re-write the paper, and not simply neglect these comments as negative criticism.

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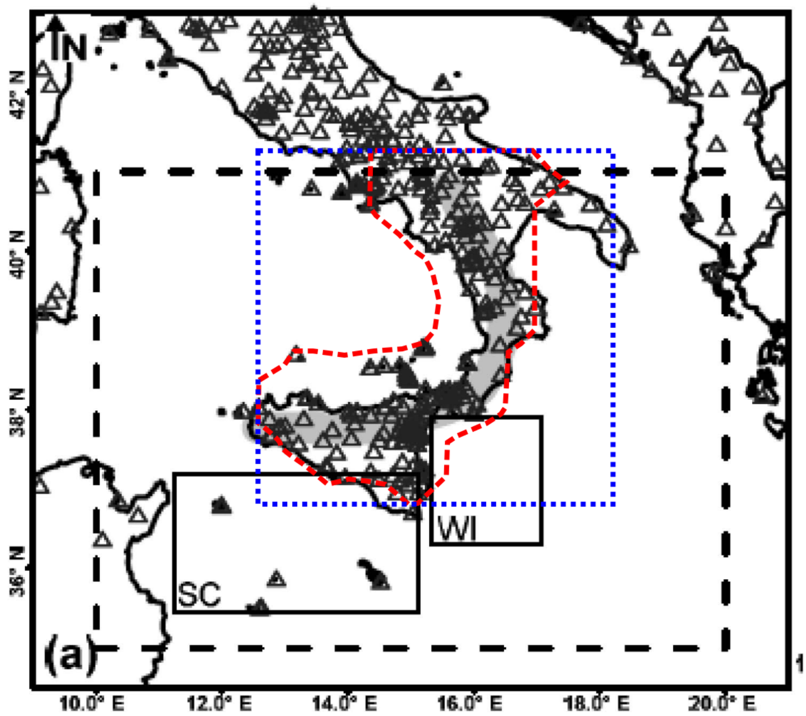

Fig. 1. Superposition of Fig.4a of the submitted ms with the 3D model area (red dashed line) and the starting model area (blue dashed line) from Orecchio et_al. (2011)