Solid Earth Discuss.,
https://doi.org/10.5194/se-2018-112-AC1, 2018

Solid Earth
Discussions

SED

Interactive comment

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

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Received and published: 19 December 2018

The Referee 1 shows to have read with a certain attention the article. The overall conclusions he draws and reports in the first paragraph of his review are, however, influenced by some wrong axioms or preconceptions he clearly explicits in the list of "main concerns" or "Main points" A to D. The clear description of the Referee's arguments in the Main points A to D brings us to organize our replies starting from the Referee's criticisms reported in these points A to D.

Reply to Referee's Criticisms reported at Point A

There is a wrong axiom or a preconception in this point A when the Referee says: "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". The Referee neglects that the application of a relative old (but of documented effectiveness) method may in many cases produce results which are able to improve the scientific knowledge. Although the efforts of the Referee to make "more complex steps of data analysis" can be fruitful and has to be appreciated, the advantages brought to Science by application of a relatively old method giving original information on the phenomena must not be refused. The Referee focuses only on the date of the stress inversion method used and does not consider the intrinsic complexity of the choice of the method of data analysis for a given study. We evidence that the literature shows that new stress inversion methods bring some advantages wrt older ones, but they may suffer of consequences of basic assumptions in many cases. For example, the relatively old method we used (GF84) is more conservative than new ones concerning the relative orientation of seismogenic stress and seismic dislocation surface, that is an important aspect in our study. We will furnish more information on this choice in the article.

There is another wrong axiom or preconception when the Referee says: "are the revised locations giving new fundamental information for the next stress inversion?" In this case the Referee assumes that the revised locations must contribute to stress inversion. He is probably used to follow this methodological path in his research and this is correct, but he neglegts that Science offers also other paths for improving knowledge. The information 'a' (earthquake locations) and 'b' (seismogenic stress inversion results) may contribute as "separate data" to the definition of a new model/theory. It is like in the case when 'a' is represented by gravimetric anomalies and 'b' by seismic data, i.e. two different types of information may concur to advance an hypothesis or to define a model (see, e.g., Neri et al., Int. J. Earth Sci, 2012). We are not absolutely obliged to state a close relationship between 'a' and 'b', or to use the first to obtain the second, also because of the different scales of the processes of interest in the respective cases. The Referee seems more concentrated on the relationship between ‘a’ and ‘b’ than on their capability to improve the knowledge available on the geodynamic processes under investigation.

The Referee fears to have misunderstood the process of data analysis, but we can say that he has not, i.e. he has – in general - understood the process of data analysis. Because the Referee says that he has spent “a lot of time to clearly define the author’s workflow” we are going to revise the text in order to solve this criticity.

The Referees writes: “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”. It would be trivial – in our opinion - to compare our non-linear locations to the original ones from the INGV-CNT database. The latter were obtained by a linear method with a 1D velocity model of all Italy. The linear locations of the INGV-CNT database show unavoidable approximations witnessed, for example, by hypocenter depth fixed at 10 km for many events, a circumnstance well known to occur when the normal location process with four unknowns tends to fail. Also, we need to say that non-linear location methods have been proven to furnish more realistic estimates of location errors than linear ones at least since 2000 (Lomax et al., 2000; Lomax and MIchelini, 2001; and many others). It is well known and unanimously shared that the linear methods do underestimate location errors in the great majority of cases. For these reasons, the epicenter maps of low-location-error earthquakes obtained by linear methods are less accurate than maps obtained by non-linear methods. Concise information to be reported in the article to fulfill the eventual curiosity of the reader can be the rms of hypocenter locations by Bayloc vs SimulPS. We are going to include this comparison in the article.

Reply to Referee’s Criticisms reported at Point B

We have limited for conciseness the information concerning the stress inversion runs, but we understand that the Referee can ask this information. We are working to report the information requested as Supplementary Material.

Reply to Referee's Criticisms reported at Point C

Since several years the area/topic of interest of the present study do not receive particular attention by geophysicists and our team is one of the few teams which have worked on the topic in argument. This explains why the percentage of self-citations is 30%. This also explains why the rate of advancement of knowledge on this topic in this area is so low and allows us to bring significant progress of knowledge by the basic analyses reported in the present study. On the other hand, the problem of very limited approaches and rough knowledge of microplate geometry and kinematics in our study area was already posed by Nocquet in 2012 (paper cited in our article).

Reply to Referee's Criticisms reported at Point D

The Referee writes: "The introduction and geodynamic background sections occupy 6.5 pages, while Data, methodology and results are described in 2.5 pages".

The slow evolution of knowledge mentioned in our reply to Point C includes several contrasting views and still open questions on the plate/microplate geometry/kinematics in the study area. In our paper, the description of these views and a detailed list of the open questions are basic for helping the reader to understand what is the real progress of knowledge coming from our study. For this reason 6.5 pages may, in our opinion, be appropriate to frame our original contribution and to address future efforts.

Also, 2.5 pages (plus five Figures and two Tables!) for description of Data, methods and results should in our opinion be nearly appropriate, because: - we do not apply new methods, therefore we may give a relatively short description of the methods used and cite bibliography; - we present concisely the results in the 2.5-pages Section, whereas in the Discussion we comment/discuss the results also describing (when necessary) the details. Among other things, this helps reducing repetitions.

In any case, on the basis of the Referee's observations and misinterpretation reported at the Minor Points 1 and 3 (sse our Replies below), we are going to enlarge the presentation of data in the Section "Data, methods of analysis and results" with particular reference to (i) the description of new data analyzed in the present study wrt previous papers, (ii) the quality of focal mechanisms used for stress inversion, and (iii) the whole process of data analysis.

Reply to Referee's Criticisms reported at Minor Point 1

The statement of the Referee is wrong. Additional seismic data for hypocenter locations and focal mechanisms for stress inversion have been used in the present study with respect to the previous ones. For example, hypocenter locations have been performed on a dataset updated to December 31, 2016, while the previous study in the same area was updated to May 31, 2011 (Orecchio et al., 2014). 1420 earthquakes not belonging to the previous dataset have entered into the new one. This update has been mentioned several times in the paper (starting from the first line of the Abstract), however we are going to furnish more details on this point in the text.

Reply to Referee's Criticisms reported at Minor Point 2

We have understood that the Referee is not an English native speaker from many, repeated elementary grammar mistakes. "The authors seems to relocated . . ." "the authors must illustrated how . . ." and so on. Anyway, because a request to improve our English style and grammar is always pertinent, we will work for this purpose.

Reply to Referee's Criticisms reported at Minor Point 3

Concerning the uncertainty of focal mechanisms used for stress inversion, the Referee writes: "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". The Referee's statement of un-realistic estimates of focal mechanism uncertainties for our off-shore earthquakes would have been correct if we had used focal mechanisms computed by inversion of P-onset polarities. As clearly explained in the article, we used only focal mechanisms estimated by waveform inversion, and the Referee's statement is wrong in this case. In this connection, we need to remark that the application of the CAP waveform inversion method to offshore earthquakes of the study area, together with the application of the non-linear procedure by Totaro et al. (2016) for fault parameter error estimates, show that uncertainties are of the order of 8-10 degrees, even less than the values of 10-15 degs we declared for our dataset. The declared values of 10-15 degs are conservative because we cannot control adequately the uncertainties of all the waveform inversion focal mechanisms taken from the official catalogs. Also, a main feature of the waveform inversion focal mechanisms (that the Reviewer seems to neglect) is that they are very stable when varying epicenter latitude and longitude and the focal depth in relatively large volumes around the hypocenter, this being widely demonstrated by a great number of tests published in previous papers (D'Amico et al., 2010, 2011; Totaro et al., 2016; Scolaro et al., 2017). We argue from the Referee's misinterpretation that we should enlarge this part of presentation of data and results in order to allow better understanding of the quality of our analysis. We are going to make this integration of text in the article.

Authors' concluding remarks

The Referee's axioms or preconceptions discussed in the above replies lead him to state that the manuscript should be rejected although he also requires at Main Points A and B additional clarifying information on the main results of earthquake locations and stress distributions. We remark that the real heart of the present work is that these results (i) contrast with the kinematic models of the study area presently available in the literature and (ii) answer some questions left open by the previous investigators.

In summary

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Discussion paper

- We do not know if the Referee is really convinced of his axioms.

- We know that he requires additional information on earthquake locations and stress distributions to verify the significance and usefulness of them.

- We are sure that he has not spent any word in his review on the main scientific problem, as posed by our work and also anticipated by Nocquet (2012) on the limitations of the approaches used for modeling geometry and kinematics of tectonic units in the Central Mediterranean.

We also tend to believe that a discussion with the Referee better focused on (i) improvements of the presentation of our results and (ii) comparison with the present geodynamic knowledge, would have been probably more fruitful than a discussion on the fundamentals of the Scientific Method.

With reference to what has been said above, we take the Referee's requests of

- (i) additional information on stress inversion runs and

- (ii) more detailed description of the contribution of Bayloc locations to our conclusions

as appropriate requests for improving the article and we are going to work on the paper for this purpose. Also, we will furnish more details in the text of the article on the structure/quality of the datasets used for hypocenter locations and stress inversion, in order to avoid misunderstandings and misinterpretations.

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