Solid Earth Discussions

Solid Earth Discuss., author comment AC4
https://doi.org/10.5194/se-2021-11-AC4, 2021

Reply on CC2

Laurent Jolivet et al.

Author comment on "Interactions of plutons and detachments: a comparison of Aegean and Tyrrhenian granitoids" by Laurent Jolivet et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-11-AC4, 2021

Comments by Papeschi et al.

Dear authors,

We thank you for your interesting paper. We have mixed feelings on it and in the following we try to elaborate our view as to why the part of your study dealing with the Northern Tyrrhenan Sea and the Northern Apennines should be carefully reconsidered.

We believe the model proposed by Jolivet and coauthors to be fully consistent with the geology of the Aegean for the following reasons:

1) Extensional detachments (NCDS, WCDS, NPEF) do exist in the area and have been described, documented and characterized in great detail. They are invariably defined by a lower ductile shear zone (e.g., Livada detachment) and an upper brittle fault system (e.g., Mykonos detachment)

2) Footwall units are composed of middle crustal rocks and sequences (4-5 Kbar) exhibiting, on a regional scale, widespread evidence of HT/LP migmatization. These units were exhumed to higher structural levels during extensional shearing along regional detachments.

3) Hanging wall units are composed of upper crustal rocks and sequences and/or sedimentary successions that were deposited within extensional basins formed during the extensional phases accommodated by the detachments.

4) Sequences from the footwall and hanging wall units are starkly different from each other as they represent and are derived from crustal domains that were initially very far apart from each other, both horizontally and vertically.

5) There occurs regional-scale magmatism that is clearly synkinematic to the extensional detachments. Intrusions that can be ascribed to this magmatism are affected by the extensional detachments and exhibit common and widespread evidence of deformation ranging from magmatic to low-temperature solid state. Structures thereof are distributed over deformed sequences that can be up to several hundreds of meters thick. The granite

in Mykonos is a classic example: in the Livada detachment footwall there occurs a continuous shear zone several hundreds of m thick that deforms the granite into impressive mylonites to ultramylonites.

6) The Miocene age of (i) HT/LP metamorphism, (ii) magmatism (which, again, is fully synkinematic with respect to the extensional detachments) and (iii) of the supra-detachments sedimentary basins indicates a coeval and regional-scale evolution for the entire area.

The structural, metamorphic, magmatic and sedimentary features of the Aegean geodynamic domain mentioned above are fully consistent with the first-order features of extensional domains elsewhere within other orogenic belts, such as the metamorphic core complexes of the North American Cordillera, the South Tibetan Detachment, the Montagne Noire, Pilat and Velay domes in the French Variscan belt.

Therefore, we find the part of the paper dealing directly with the Aegean to be interesting and to add valuable constraints on the investigated processes that are typical of extensional back-arc settings.

Thanks for these positive comments on the Aegean part of the paper

However, for the reasons we discuss below and that have been documented in depth in the literature for the last twenty years, we find the submitted study neither relevant nor consistent with the geological framework of the Northern Apennines and the evolution of the northern Tyrrhenian Sea.

A general answer:

Dear colleagues

thanks for your comments and your positive appreciation of the Aegean part of our paper. As for your full disagreement with the Tyrrhenian part of it, which is actually the main focus of the paper, we face a difficult issue as we fully disagree with your understanding of the geology of that area. There has always been two schools of thought in this region. One considering that the Northern Tyrrhenian Sea results from the same back-arc extensional process that has been active since the Oligocene in the back-arc region of the Apennines subduction and one claiming a mostly compressional history until recent time. We deliberately place our study in the first league. Probably we should have extended the geological setting of the Elba MCC to better account for diverging ideas, which we do in the revised version. Then, we persist in considering that the context is mainly extensional and that two main extensional detachments have been active in Elba since the Late Miocene up to the Pliocene. The Zuccale Fault is a major low-angle detachment with a contrasting P-T evolution between the footwall and the hangingwall. The HP-LT parageneses recently described there are found only in the hangingwall and they are older than the motion along the detachment and the intrusion of the Porto Azzuro pluton, just like in the Aegean. This shows that the hangingwall units have not seen the high-temperature conditions and the effects of the plutons recorded by the footwall, hence the different evolutions. Then, the E-W general stretching and flow direction in the Monte Capanne pluton have been described a long time ago and we see no reason to come back on it. The contact metamorphism along the northeast contact, associated with the skarn deposit is clearly syn-kinematic of the E-W stretching and the top-east sense of shear, a situation exactly similar to that of the Aegean MCC, such as Serifos for instance. The progressive transition from syn-magmatic flow to sub-solidus mylonitic shearing is also very similar to what we observe in the Cyclades. We thus stick to our conclusions and will modify the paper to better inform the reader of the current and long-lasting debate about the evolution of the northern Tyrrhenian Sea.

>>>> See also the answer to the comments of A. Brogi above.

- "**No significant crustal exhumation**" in the area. The Zuccale Fault (Eastern Elba) is still considered as the main extensional detachment that would have controlled and steered the opening of the Northern Tyrrhenian Sea as a back-arc bain in the upper-middle Miocene. The Zuccale fault, although indeed subhorizontal, does not juxtapose different crustal domains. The same pile of tectonic units, with exactly the same structural and geometric relationships, occurs both in the footwall and in the hanging wall. This indicates that the footwall is not a deep crustal block that underwent significant exhumation in comparison to the hanging wall. The current geological set up is instead consistent with an almost complete lack of exhumation during faulting along the Zuccale fault. Moreover, existing PT data constrains show that the exhumation to high structural levels (< 2kbar) of the metamorphic units in the area affected by Zuccale Fault had ceased before faulting along the Zuccale Fault.

We understand your concern about the amount of exhumation accommodated by the Zuccale fault. It is probably not a detachment comparable to the North Cycladic Detachment System (NCDS) as a whole. It is probably more comparable to the Mykonos Detachment, which is the latest brittle increment of the NCDS. But the paper is mostly devoted to the exhumation of the Monte Capanne pluton accommodated by the Capanne extensional shear zone, not so much with the Zuccale fault. Then the migration of extension deformation eastward and the late formation of the Zuccale fault is in a sense similar to the progressive migration of the branches of the NCDS through time. This is another similarity that we had not enough emphasized. We thus added this new text to discuss the point further:

"At the scale of Elba Island, the sequential intrusion of the Capanne Pluton and the Porto Azzuro pluton associated with the sequential formation of the Capanne Shear Zone followed by the Zuccale Fault is reminiscent of the migration of detachments within the NCDS and the NCDS, the last increment of extension being accommodated by a low-angle brittle detachment, the Mykonos Detachment in the case of the NCDS and the Kavos Kyklopas Detachment in the case of the WCDS. This is another significant similarity between the Aegean and Tyrrhenian plutons."

- **No syn-extensional sedimentary basin**. No sedimentary sequences are documented from the Northern Tyrrhenian and in the Tuscan Archipelago as infill of supra-detachment basins. The only known Neogene deposits therefrom are upper Pliocene deposits from the Island of Pianosa. They are resting subhorizontally upon an angular unconformity over lower Miocen deposits. Also, Pianosa is known as a "positive structure" that underwent uplift in the lower Pliocene.

There is no supra-detachment basin on Elba Island, this is true. But it is true also for some of the Cycladic islands like Tinos, Andros or Serifos. So this is not a strong argument. It is quite clear that the migration of Miocene and Pliocene deposits in the Norther Tyrrhenian Sea and Tuscany is related to basin formation and to rifting. We lack detailed seismic profiles that would show the intimate geometry of basins. When you write that Pianosa island is known as a positive structure, this is your interpretation. The tilt of Miocene sediments could instead be the result of the activity of normal faults. The Miocene sediments I have seen on Pianosa are similar to those of eastern Corsica in the Aleria plain and the unconformable sediments on top a very shallow water marine sediments that do not call for a major uplift.

- **"Pliocene age of deformation"**. Faulting along the Zuccale Fault is constrained to the late Miocene - early Pliocene (< 5.9 Ma).

Yes, this is fine. It is roughly coeval with the emplacement and cooling of Porto Azzuro pluton.

- **"No synkinematic magmatism"**. Intrusions in the area such as (i) the Monte Capanne pluton, (ii) the Giglio pluton and (iii) the Montecristo pluton do not exhibit any evidence of syn-extensional deformation. The greatest volume of intrusive bodies is defined by an isotropic fabric or an only weakly developed magmatic foliation, which is oriented differently in all plutons. Only in the eastern part of Monte Capanne intrusion there are local and discontinuous decimetric to metric monzogranitic volumes containing a west dipping magmatic foliation, the attitude of which, however, is not consistent with eastward extension. Despite the common exposure of intrusions in the area, no evidence of extensional ductile and brittle shear zones can be recognized. Likewise, in the host rocks no regional extensional faults occur and HT-LP contact metamorphism-related recrystallization largely overprinted previous foliations and folds. The only evidence of syn- magmatic deformation are rare small-scale deformation structures such as asymmetric folds with centrifugal orientations around the intrusions that occur in the host rock right at the contact with the magmatic rocks.

This is not at all our experience of the field in Elba. The Monte Capanne pluton shows a conspicuous orientation almost everywhere and we observed a clear gradient of facies and fabric going eastward. The fabric we observe is very similar with that shown by the Aegean plutons. Then the deformation associated with skarnification is also consistent with top-to-the east shearing during the pluton emplacement and cooling. The brittle fault does not crop out clearly along the eastern margin of the Monte Capanne pluton and I am not even sure it exists precisely there although several papers propose the existence of an east-dipping fault. But the equivalent of the brittle detachment seen in the Aegean is clearly the Zuccale Fault. This late fault could be the equivalent of the late brittle detachments seen in the sequential development of the NCDS (see the answer to the reviewer's questions above).

In summary, these first-order structural, metamorphic, magmatic and sedimentary features indicate that Northern Tyrrhenian-Northern Apennines geodynamic system it is not consistent with a mature and long-lived back-arc basin tectonic environment. We thus believe that it is misleading and not founded on solid evidence to compare the Northern Tyrrhenian-Northern Apennines area of the central Mediterranean with the Aegean back-area area.

In conclusion, we believe that a comparison with the results of the submitted modeling cannot be accepted for the Italian case study and, as a matter of fact, seems to be irrelevant for the geodynamic boundary conditions listed above.

This is your interpretation of the geodynamic context of the Northern Tyrrhenian Sea, which is our opinion does not explain the current crustal and lithospheric structure along the transect running from Corsica to the Apennines that you have recently published in Tectonics. We have a different interpretation.

The Aegean sector, on the other hand, represents one of the typical examples of a back-arc basin with crustal extension and the submitted study is very interesting and relevant for that Greek scenario.

Lastly, we wish to point out that much of the geological literature from the last twenty years dealing with the geology (structural, magmatism, metamorphism) of Elba Island and, more in general, of the northern Tyrrhenian Sea, is neither reported nor discussed by the authors. If it were, the natural boundary conditions would clearly contradict those of the submitted paper, indeed making the Italian case study inadequate.

We have added recent references also to accommodate the reviewers' suggestions (see above) although there are not always directly related to the central topic of our paper. They surely give a better account of the current debates.

Interpretations must be based on robust analytical data and we think that much of the available geological record was ignored or downplayed. The model presented for the Northern Tyrrhenian and Elba island corresponds to the mainstream interpretation of the mid-nineties, which we believe is in great need of significant updating.

We have come a long way since and much analytical data and ideas have been developed.

This is a matter of interpretation: ours is entirely different from yours, and we do not mind you publishing yours.

Sincerely,
Samuele Papeschi, Giovanni Musumeci, Francesco Mazzarini, & Giulio Viola

Second comment by Papeschi et al:

You are right: your paper should describe better alternative views in the area but also other views that have been proposed through the years, as I firmly believe that models/ideas can be improved by confronting with different tectonic and geodynamic models, and I deeply believe that the manuscript would benefit from a better and improved discussion of them.

We hope that the corrections made to answer the reviewers' suggestions above contribute to filling this gap.

My major concern with your manuscript is that important data has been ignored. For example, the only paper documenting partial melting and deformation in the presence of melt – and to date the best constrain on the metamorphism of the Calamita (Papeschi et al., 2019. Lithos 350) has not been considered, the only available age constrain on the Zuccale Fault and syn-magmatic shear zones (K/Ar authigenic illite age Viola et al., 2018. Tectonics 39) is not discussed. There is also a plethora of works documenting coeval ductile deformation and pluton emplacement/contact metamorphism, and clearly documenting a transition from dominantly ductile to brittle deformation whose findings suggest top-to-the-E deformation on W-dipping shear zones (Musumeci & Vaselli, 2012. Geosphere. Musumeci et al., 2015. Tectonics. Papeschi et al., 2017. Tectonophysics. Papeschi et al., 2018. J. Struct. Geology. Mazzarini et al., 2011. J. Struct. Geology. Massa et al., 2017. Geol. Journal).

Once again, this paper is about the internal structure of the Monte Capanne pluton, not about the deformation of Calamiti peninsula. Very little has been done on the pluton itself, apart from very nice works on the successions of several magma batches with different facies by Farina et al, among others, and older papers by Bouillin et al. or Daniel and Jolivet. The main disagreement as the scale of the island, I think ,comes from different interpretations of the top-to-the east shear zones in the east above the Porto Azzuro

pluton. The papers you mention above provide a very detailed and nice account of the deformation there, which shows without doubt the reality of this top-east sense of shear. Then it is a matter of interpretation, this sense of shear is either related to back-tilted extensional shear zone associated with the Zuccale fault during the colling of Porto Azzuro pluton, or to west-dipping thrusts, which is your interpretation. It remains that the Zuccale fault cuts down section and cannot be a thrust and that the top-east shear zones can very well be some precursors of the Zuccale fault, back-tilted during extension. This is our interpretation. We have added some more text to better acknowledge this debate, as follows:

"The current debate about the tectonic context of the emplacement of plutons also stems from different interpretations of the observed top-the east shear zones in eastern Elba in the vicinity of the Zuccale Detachment. Detailed studies have documented the progressive deformation along these shear zones from brittle to ductile and the HT-LP conditions associated with the most ductile ones and they have been dated from the Pliocene (Mazzarini et al., 2011; Musumeci and Vaselli, 2012; Musumeci et al., 2015; Massa et al., 2017; Papeschi et al., 2017, 2018; Viola et al., 2018; Papeschi et al., 2019). Their interpretation can then be debated. They can either be west-dipping thrusts or back-tilted top-to-the east extensional ductile shear zones coeval with the progressive localization of the Zuccale Detachment, which is our interpretation following Daniel and Jolivet (1995)."

A good regional model has to explain and fit all the structures and evidence coming from field, metamorphic, and geochronological studies, otherwise it needs to be improved. Therefore, I kindly ask you to discuss these findings in the framework of your model.

We try to do it better in the revised version.

Regarding the Zuccale, actually there is no such different P-T evolution in the footwall and hanging wall, since the exact same units occur below and above the fault (in agreement with all papers from Keller & Coward, 1996 to Smith et al., 2011; Musumeci et al., 2015). Indeed, regional units (Rio Marina) can be found both above and below the fault and in the Ortano valley you have HT-LP parageneses above the fault. Indeed, the whole structure of the fault has a maximum throw of about 6 km, which makes challenging to link this structure to large scale exhumation. Therefore, irrespectively of the different interpretations, other structures are needed to explain the exhumation from mid-crustal depth that you propose.

As discussed above, the Zuccale Fault is only the last brittle increment of extensional deformation and it progressively localized with evidence of ductile-to-brittle evolution. The difference in maximum temperature between the footwall and hanging wall is significant with higher temperature underneath the set of top-east extensional shear zones. The total offset may not be very large but it is nevertheless, in our opinion, the continuation of a long process of extension accommodated by top-east detachments and coeval intrusions, just like in the Aegean.

Regarding the Zuccale, the recent papers by Moeller and co-workers raise an interesting question, as they document low βvalues and clearly state that extension is mostly on high-angle normal faults. I would be very interested to check if your model fits with the reconstructions provided in Moeller et al. or if you can offer a discussion of these papers (2013, 2014; J. Geoph. Research Solid Earth and G-cubed).

First of all, we wish to stress that the works with seismics of Moeller et al. (2013 and 2014) conclude in terms of extension and not compression, which is in line with our

interpretation. They then indeed discuss a beta-factor around 2.2. Moreover this is leading us quite far from the main issue discussed in our paper, i.e. the interactions between detachments and plutons. We added references to these two papers in the text:

"A part of this extension is also accommodated by higher-angle normal faults, most of them dipping eastward, leading to a stretching factor of about 2.2 (Moeller et al., 2013; 2014)."

Regarding the Monte Capanne pluton, I believe that it would be actually very important to come back on that structure, since other workers on the area have proposed completely different models (e.g. Farina et al., 2010; Pandeli et al., 2018), indicating that there is not agreement yet, about the structure of the pluton.

The internal structure of the pluton was studied from different points of view. Farina et al. studied the distribution of the petrographic facies they have identified in the Capanne pluton and we have used their results for our map and cross-sections in the paper. Pandeli et al., which was already also cited, show additional features such as the orientation of folds in the metamorphic aureole and their conclusions are also in line with ours with a syn-extension pluton. The main difference with our paper is that none of these papers explicitly mention the detachment on top of the Capanne pluton. So we see no fundamental disagreement with our interpretation.

It would be actually interesting for the readership to better document the structures and flow patterns of the pluton, since the numerical model is based on it. I also think the Capanne is really different from the Aegean, as there is no documentation of a continuous fabric from mylonite to cataclasite at the top of the pluton like in the Aegean. Moreover, on Elba, syntectonic deposits associated with low-angle normal faults are entirely missing. (only sedimentary deposit on Elba: pleistocene eolianites). The nearby Pianosa Island shows a major unconformity of Upper Pliocene over Miocene deposits, which is not explained.

We have already discussed these points above.

All the points raised above are, in my opinion, really major. I am personally genuinely interested to see a thoughtful discussion/confrontation with these data and models, as I believe an improved model and a better depiction of the geology of the area could arise from it. Moreover, it would give the readers a better understanding of the geology of the area, which is to date far from be resolved. As a concluding remark, I do believe that a good, regional, comprehensive model, has to explain all the tectonometamorphic features that are present in a given area.

Here we can agree with you that a thorough new synthesis of the geology of the larger northern Tyrrhenian Sea region would be useful, but this is not at all the central topic of our manuscript. We might do it in the near future.