Response to all comments:

Comments by Referee #1 (Antonio Castro :

This is an interesting paper that merits publication after some minor changes. It is implicit in the proposed model that melting of lower crust and detachments are related each other. This is the classical model of MCC. Transitions from magmatic state structures to ductile (rock) and brittle shear zones and faults are in support of the conceptual model. Melting induced by mantle magmas agrees with petrological constraints and experiments. However, these processes are not restricted to heat input; the supply of water to the lower crust is a necessary condition according to experiments (Castro 2020). Water is likely supplied by mafic magmas formed in a hydrated mantle (Pargasite- bearing). The described mafic rocks that associate to plutons have the characteristics of vaugnerites (sanukitoids) and can be the water donors to the crust. Thus, the process must start in the mantle under extension and decompression (pargasite breakdown has a positive clapeyron slope). Mafic (hydrous melts) can be stored at the base of the crust and some pulses may reach the upper crust at late stages after the partially molten zone is exhausted. This seems to be compatible with the sequence of magmas. The outcome from numerical simulations matches quite well the conceptual models built on the basis of the geological and structural study. In this way these models cannot be called conceptual but geological, as they are based on real data, and not on simple observations. The paper lacks of basic information on the chemistry of intrusive rocks. If available, it would be interesting to show basic geochemical diagrams (e.g. Peacock diagrams, aluminosity, etc.). These may reinforce the interpretations on lower crust melting and the arrival of mantle-derived magmas at the time of extension and dome formation. In my opinion the paper can be published after addressing these minor revisions.

We fundamentally agree with A. Castro's approach although the goal of our paper is not to discuss the origin of water in the melting process producing the plutons we have studied. The focus is rather on the interaction between the pluton and the detachment once the magma has been produced. We however did our best to answer this remark and added a portion of text in the chapter describing the numerical model to make clear a number of points:

"Castro (2020) pointed out that melting of the lower crust is enhanced by both heat and water supplied by mantle derived mafic magmas. In particular, partial melting of granulitic component triggered by adding water from a mafic, mantle-related, component (vaugnerites) can represent the potential origin of secondary I-type granites as demonstrated by the experimental approach of Castro (2020). Castro (2020) followed the concept of Chappell & Stephens (1988) whereby the possible dual origin of I-Type magma stems from primary I-type magmas issued from coeval subduction, while secondary I-Type magmas are more likely related to melting of old subduction-related rocks. In the Aegean and Tyrrhenian tectonic settings, there is no evidence so far for the presence in the outcropping migmatitized crust of mafic components such as sanukitoids issued from older subduction-related rocks in sufficient volume to be the main donors of water. In contrast, there are many evidences of mafic mantle-derived magmas, coeval with the I-Type granites s.l. described in our study. For example, at the root of the Serifos granodiorite (Aegean Sea), Rabillard et al. (2015) describe mafic dykes

disrupted into enclave swarms scattered throughout the whole magmatic body. Injection of mafic hydrous component took place during the whole emplacement period of the pluton that was crosscut by basaltic dykes while the granite was at near-solidus conditions. Closely similar observations can be done in the Tyrrhenian granitoids. For example, the main facies of the Monte Capanne pluton exhibits a constant, peraluminous, monzogranitic composition (Poli and Tommasini, 1991; Dini et al., 2002; Gagnevin et al., 2004) while the mafic microgranular enclaves (MME) varies from tonalitic-granodioritic to monzogranitic. The leucogranitic dykes are syenogranitic in composition (Gagnevin et al., 2004). Gagnevin et al. (2004) proposed a multiphase magmatic emplacement from peraluminous magmas issued from melting of a metasedimentary basement and hybridized with mantle-derived mafic magmas whose heat supply possibly enhanced wall-rock assimilation. In addition, injection of mantle-derived magma in the San't andreas facies would have triggered extensive fractionation and mixing of the basic magma with the resident monzogranitic mush (Poli and Tommasini, 1991).

We thus fully agree with the assumption of Castro (2020) pointing out that the supply of water to the lower crust is a necessary condition to produce I-type granites, but we believe from the previous petrological studies combined with our field observations that the mafic magmas derived from the coeval mantle are the main donors of water during the partial melting of the lower crust. Distinguishing the two I-Type granites in both Aegean and Tyrrhenian granitoids can be completed by an extensive geochemical study of major and trace elements as illustrated by the synthesis made by Castro (2020) for I-type granites emplaced in different tectonic settings. This approach is not in the scope of our study as the origin of the mafic component has no significant direct impact on the interaction between plutons and detachments faults. Nevertheless, we agree that it may be worth showing basic geochemical diagrams to reinforce the interpretations on lower crust melting and the arrival of mantle-derived magmas at the time of extension and dome formation". In order to illustrate the chemical evolution of I-Type granites in the Aegean and Tyrrhenian settings, a complementary figure is proposed in appendix A (figure A1) issued from a compilation of geochemical analyses in. This MgO vs SiO2 Harker diagram clearly shows the classical negative correlation found in I-type hornblende-biotite-bearing granites. The microgranular enclaves represents the mafic hydrous melts that reached the upper crust while they mixed/mingled with differentiated melts either during ascent (Fernández and Castro, 2018) or at the base of the magmatic chambers (as well illustrated in Serifos granodiorite by Rabillard et al., 2015). Mixing/migling processes between mafic mantle-derived melts and acid magmas produce composite batholiths (Poli and Tommasini, 1991) as illustrated by the case of the Elba Island magmatic complex shown for comparison (see Dini et al., 2002 for explanation)."

Comments by Referee #2 (Andrea Brogi):

The manuscript entitled "Interactions of plutons and detachments, comparison of Aegean and Tyrrhenian granitoids" by Jolivet et al. deals with the Neogene-Quaternary evolution of two areas in the Mediterranean that have been affected by magmatism strictly controlled by extensional tectonics. Authors mainly refer on the mechanism of emplacement and exhumation in relation with the Miocene – Pliocene tectonic evolution. Although the relationship between tectonics and magmatism is a "classical" topic, and several papers have focused on this precisely in the Tyrrhenian and Aegean areas, the authors offer a best opportunity to tackle a hot topic that in the last decade has been taking place overall on the evolution of the northern

Tyrrhenian Sea and the Northern Apennines. In this view, Elba Island play a key role in the debate and has been object of different interpretations.

Thanks a lot for the kind general appreciation of our paper. We also thank you for the numerous suggestions of improvements that we have implemented in the revised version.

The manuscript sounds like a review paper as it has been constructed mostly integrating literature data apart from the last part which is dedicated to modeling.

This is not entirely true as the description of the detailed structure on the Monte Capanne pluton near San't Andrea is our own work also. The Aegean part is shorter and indeed corresponds to a summary of our recent findings in the Aegean plutons

In this view, the manuscript is well organized and symmetrically divided for illustrating both northern Tyrrhenian (i.e. Elba) and Aegean areas. The last part illustrates and discuss the model designed for reconstructing mechanism of emplacement and exhumation of the magmatic bodies. Figures are clear, well done and well represent all data needed for the discussion.

Thanks !

Nevertheless, in my opinion, a first figure comprehending both the northern Tyrrhenian Sea and the Aegean Sea, illustrating the tectonic scenario of the Mediterranean area is missed. A new figure in that line could be very useful for all readers not so familiar with this part of our Planet.

Yes, this is an excellent suggestion. We thus added a series of 3 maps taken and modified from our recent paper Romagny et al. (2020) reconstructing the Mediterranean region with a special look on magmatic events. The position of Elba Island and the Cyclades are shown with their plutons at the time of intrusion. The geodynamic context of both regions is thus better illustrated and the two can be compared.

See new figure 1

Some text was added to introduce this new figure and explain the geodynamic situation in more detail.

"Figure 3 shows the present-day situation as well as two stages at 5 and 15 Ma when the Tyrrhenian and Aegean plutons were forming adapted the detailed reconstructions of from Romagny et al. (2020). Magmatic events are shown with grey triangles (volcanism) and black squares (plutons). The detailed tectonic evolution, the reconstruction method and the link between magmatism and tectonics are described in discussed in Romagny et al. (2020) and Menant et al. (2016). The progressive retreat of subduction zones and foreland fold-and-thrust belts and/or accretionary wedges is shown coeval with crustal thinning and exhumation of metamorphic core complexes."

General conclusions of the paper are in line with what the authors are presenting and most of data presented in literature. However, a more accurate discussion should be dedicated to better highlight additional points as specified here below.

I agree with the authors that the Tyrrhenian Sea and Tuscany were affected by extensional tectonics since Ealy Miocene and this can explain all the geological evidences that we can see and measure on the surface. However, the tectonic evolution of the Tyrrhenian Sea is object of an increasingly noisy debate on the geodynamic scenario that should be mentioned in the text. As authors probably known, some authors have recently published papers that are framing all the geological issues of the northern Tyrrhenian Sea and inner Apennines in an alternative view with respect to the extensional tectonics. Also, the emplacement and exhumation of magmatic bodies are framed in a compressional setting instead of related to extension (see for example Montanari et al., 2010, Tectonophysics). I agree that the alternative conclusions are difficult to share, but just for this reason authors should face the problem and at least mention the existence of the improbable conclusions of these papers. Discussion has been addressed in several papers that could be useful for the authors; I mention here the most representative ones that may help authors to get useful idea: Brogi et al., 2005 (JVGR); Brogi 2008 (Int.J.Earth.Sciences); Brogi and Liotta 2008 (Tectonics); Brogi 2011 (Tectonophysics); Liotta et al., 2015 (Tectonophysics); Brogi 2020 (J.Struct.Geol.); Brogi et al. 2021 (Geosciences Switzerland). In particular, I recommend the last paper for focusing on the different views on the emplacement and exhumation of the magmatic bodies in the Tyrrhenian area (i.e. views from the office-desk vs fieldwork and data collection).

Yes. This is in line with the comment posted by Papeschi et al. on the discussion website. We have thus added in the geodynamic setting a paragraph devoted to this debate with associated references. We are in complete disagreement with this alternative interpretation, which is not new actually, but we mention the debate as suggested.

The geodynamic setting of the Northern Tyrrhenian Sea and Tuscany is debated. Since the late 90's two opposite interpretations have been discussed. One school of thought considers a continuum of extension from the Oligocene to the present with an eastward migration of extension in the back-arc region of the retreating Apennine subduction (Keller and Pialli, 1990; Jolivet et al., 1994; Jolivet et al., 1998; Faccenna et al., 2001a; Faccenna et al., 2001b; Brogi et al., 2003; Brogi et al., 2005; Brogi, 2008; Brogi and Liotta, 2008; Brogi, 2020). Extension starts in the early Oligocene between Corsica and Provence and reaches the highest part of the Apennines in the recent period. Extensional basins, controlled by low-angle east-dipping normal faults migrate eastward following the migration of the magmatic arc. The Zuccale low-angle normal fault or an east-dipping ductile extensional shear zone bounding the Monte Capanne pluton, both observed in Elba Island, are part of this continuum of extension in the late Miocene and the Pliocene (Keller and Pialli, 1990; Daniel and Jolivet, 1995; Collettini and Holdsworth, 2004). This type of model is challenged by an alternative view where extension is only very recent, not before the Late Miocene or even later in the Tyrrhenian Sea and where several basins on the mainland of Italy are instead interpreted as compressional (Finetti et al., 2001; Bonini and Sani, 2002; Ryan et al., 2021). One of the main data set which is at the root of this debate is the CROP seismic profile crossing the Tyrrhenian, Tuscany and the Apennines (Finetti et al., 2001). Discussions of this alternative can be found more developed in several papers (Brogi et al., 2005; Brogi, 2008; Brogi and Liotta, 2008; Brogi, 2020). We consider that the compressional model cannot account for the first-order features off the northern Tyrrhenian Sea such as the crustal and lithospheric thickness and the geological evolution of Corsica, Elba, Giglio islands and we deliberately place our research in the framework of the migrating extension models."

At the same time, Authors refer the development of the Tyrrhenian Sea to a back-arc basin related to the roll-back and slab-pull, but it is not a common view; see for example the papers by Mantovani et al. 2001 (J. Virtual Explor), Viti et al. 2004 (Tectoncis); Mantovani et al., 2019 (Journal of the Geological Society) and others, which highlight a more complex geodynamic scenario compared to the classic model. Authors probably should add some lines also for highlighting these different points of view.

Yes, we are also aware of this debate. We now mention it in the revised version:

"This evolution of the Northern Tyrrhenian region as a back-arc basin within the overriding plate of the retreating Apennine subduction is not however entirely consensual and alternative models exist, which involve different mechanisms, including escape tectonics. The reader is referred to the papers of Mantovani et al. (2020) and Romagny et al. (2020) for alternative views."

Magmatism (e.g. magma formation, emplacement and exhumation) has been modeled through numerical experiments and the results have been discussed in last part of the paper. In my opinion this is the most critical part of the manuscript. Authors set the model on parameters that I cannot understand: for example, the temperature, the depth and the volume of bodies seem to be not consistent with what it is known for the magmatic bodies described at least for the northern Tyrrhenian area. I suggest to better show in a table which are the parameters authors used for the numerical simulation.

There is a wide literature from which authors should constrain the parameters to be fixed for the modelling (Caggianelli et al. 2014, Geol.Soc.Spec.Publ; Rochira et al. 2018, Geodynamica Acta; Spiess et al., 2021, J.Struct.Geol.). Evolution of the magmatic systems can be found in Dini et al., 2002, Dini et al. 2008; Westerman et al., 2004...

Additional information for the age of the Porto Azzurro pluton are in Gagnevin et al. (2011, EPSL) and Spiess et al. (2021, J.Struct.Geol).

Our study is not so much about the specific case of Elba and the Monte Capanne, but more on the geometry and kinematics of plutons intruding the crust in an extensional context where low-angle detachment form. Our observations in the Aegean have revealed a common scheme for all the plutons we examined and we find some striking similarities in the case of Elba, in terms of geometry of the internal organization of the pluton and its sheared border near the detachment. This is what we want to model and which has never been done before.

The modelling procedure we use is entirely different from that adopted in the various papers you cite. Instead of pure thermal and rheological models we have here a thermo-mechanical model where we do not prescribe the position of the detachment and we accommodate large strain during the imposed extension, which is not achieved in other models. This detachment instead forms in a self-consistent way with the density contrasts and imposed rheology of the host rock and magma. The magma is a bit resistant probably compared to a true pluton, but its resistance is negligible compared to that of the host rock like we expect for a magma and changing this rheology a little would not change the main outcome of the model, as long as a significant rheological contrast is preserved. In our models, we also take into account the lateral thermal diffusion, which is not the case in most of the cited models. All parameters use for the modelling are in a table in the supplementary materials.

These models are shown for comparison with the conceptual model we derived previously from the field observations of the Aegean plutons and the overall geometry and kinematics are very similar, suggesting that our model is viable. The emplacement of granites in the model is controlled by structures that form in a way consistent with the rheology, dictated to the first order by the thermal evolution and the rheological contrasts introduced in the stratification of the initial setup. The fact that the plutons start as balloons is similar to the models you cite but the cause for melting is different. Melting is not caused by decompression but by heating from below because the upper lithospheric mantle thins and is boudinaged because of extension.

Our models do not consider a two-phase flow with a magma percolating in a permeable medium. Such models do not exist yet, at least for long-term models running on geological time scales, unfortunately. Tectonics involved in our models lasts for millions of years while porosity waves would last only a few hundred years, which correspond to only one time step of our model. We thus cannot consider the successive batches of magmas involved in the formation of plutons. The balloons that form at the beginning of extension are a good approximation of several successive episodes of magma extraction along the channels formed in the imposed stress field. This is the best we can do at the moment.

We have added a short paragraph citing some of the proposed papers (several ones were already cited):

"Quantitative data on the depth of intrusion of the Monte Capanne pluton can be obtained through the analysis of the metamorphic parageneses in the contact aureole and also assessed by comparison with the nearby Porto Azzurro pluton or the active geothermal field of Larderello. The Porto Azzurro pluton, more recent, induced the formation of a high-temperature contact metamorphism in the Calamiti Schists cropping out underneath the Zuccale Fault. Estimations of the P-T conditions of this metamorphism suggest that the pluton was emplaced at a similar depth of about 6.5 km and the maximum temperature recorded in the schists is about 650°C fringing the muscovite breakdown reaction (Caggianelli et al., 2018). Analysis of the metamorphic aureole also reveals multiple hydrofracturing episode by boron-rich fluids which can be compared to the present-day fluid circulation at depth in the Larderello geothermal field (Dini et al., 2008). Thermal modelling of an intrusion rising in the upper crust (Rochira et al., 2018) allows constraining the size of the pluton to produce the observed thermal anomaly beneath Larderello but such model does not allow testing the interactions between the detachment and the rising and cooling pluton. Although evidence of the involvement of transfer faults have been described in the case of the Porto Azzurro pluton (Spiess et al., 2021) we do not address these in our modelling procedure as our model is kept 2-D for the moment."

and

"Magmatism is recorded in the Tuscan archipelago (Capraia, Elba, Giglio islands) from 8 to 5 Ma with plutons in Elba and Giglio and volcanism in Capraia and the mantle source of the magma appears highly contaminated by subduction-related and crustal-derived metasomatic fluids (Gagnevin et al., 2011)."

Concerning the structural control on the magma emplacement, authors should also discuss the role of the transfer zones that accommodated the extensional tectonics since Early Miocene and which contributed to channel the magmatic intrusion (see Dini et al., 2008 – Terra Nova; Liotta et al., 2015 - Tectonophysics; Gola et al., 2017 – Energy Procedia; Liotta and Brogi, 2020 - Geothermics; Brogi et al., 2021 – Geosciences Switzerland). This part should be better introduced as authors, in their models, figure out the emplacement of the magmatic bodies “like balloons” without any explicit connection with crustal structures.

These papers mostly deal with geothermal reservoirs in Tuscany (Larderello) and Iceland. It is most certainly true that transfer faults play an important role in the formation of geothermal reservoirs because they contribute to enhanced permeability at the junction with extensional detachments in this sort of context. This has been shown also in the Basin and Range and the Menderes Massif (Faulds et al., Roche et al.). This is not at all the main focus of our paper but we acknowledge this point and added the following text because there are connections between granite emplacement/cooling and geothermal reservoirs:

“The emplacement of plutons underneath extensional detachments may also be influenced by transfer faults accommodating along-strike variations of the rate of extension. This has been mainly discussed for geothermal reservoirs associated with plutons as the intersection of a detachment and a transfer fault leads to enhanced permeability and more efficient advection of fluids toward the Earth surface (Dini et al., 2008; Faulds et al., 2009; Liotta et al., 2015; Gola et al., 2017; Roche et al., 2018a; 2018b; Brogi et al., 2021; Liotta et al., 2021). In the case of the Tuscan Archipelago and Tuscany, this possibility has been documented by field studies in eastern Elba and the Gavorrano pluton (Liotta et al., 2015; 2021). The present paper is however mainly focused on the extension component of deformation and the interactions between low-angle detachments et the emplacement of plutons.”

Of course, their geometry at melt and solid-state was modified by the activity of unroofing faults continuing to thin the tectonic pile, as clearly understandable from the surface analysis, but no indications are from the permeability development triggering the melt rise toward the upper crustal levels.

We focus on the interactions between plutons and detachments, based on field observations and numerical modelling and we show that the emplacement of a pluton in an extension context can be associated with the formation of detachments with a recurrent formation scheme with migrating detachments and a precise organization of structures within the pluton during emplacement and cooling that we find both the in the Aegean and Elba. So it is not only that the pluton is deformed by the detachment, it is a general emplacement mechanism that we propose here.

So in my opinion, this manuscript provides important inputs but additional work is necessary for refining at least the numerical modelling. The aim of the manuscript, addressed to resume and compare the tectonic setting which provided to the development of this part of Mediterranean as well as emplacement and exhumation of the magmatic bodies, is perfectly achieved.

Thanks a lot. We hope that the modifications we implemented will be satisfactory.

Regards,

Andrea Brogi (University of Bari, Italy)

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.

1) "**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."

2) **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.

3) **"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.

4) **"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

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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 β values 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.