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Discussions

Interactive comment on "Exhumation of metamorphic rocks in N Aegean: the path from shortening to extension and extrusion" *by* R. Lacassin et al.

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Review of "Lacassin et al, Exhumation of Metamorphic rocks in N Aegean: the parth from shortening to extension and extrusion, eEarth 2, 1-35"

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General remarks

Lacassin et al. (2007) address a significant outstanding question in the Aegean geology, concerning the exhumation history of the narrow HP/LT metamorphic belt in the

Pelagonian zone of the Hellenides, from mt. Olympos to the Pelion. To this end, they review existing literature concerning structure and geochronology of the belt, and add new geochronology results from the Pelion peninsula. They propose a three-fold exhumation history, of synorogenic exhumation during underthrusting in or prior to the Eocene, early Neogene ductile-to-brittle extension and late Neogene brittle extension associated with the propagation of the North Anatolian Fault into the Aegean region around 5 Ma. Their work is novel in the sense that they argue for ductile extensional unroofing around 40 Ma, contemporaneous with the late stages of thrusting at the base of the HP/LT metamorphic unit. This interpretation and documentation adds to recent publications about the Aegean region, in which the importance of synorogenic extension in a 'subduction channel' (Jolivet et al., 2003) or 'extrusion wedge' (Ring et al., in press-a; b) is further documented and understood. Apart from typo's and small grammatical errors, I have 2 main points of concern:

1) The authors compare extension directions of 40 Ma with those from 4 Ma, but disregard a major phase of clockwise rotation between 15 and 8 Ma of the west Aegean region, which will certainly change their conclusions about the extension history.

2) The authors suggest based mainly on comparable ages of thrust- and normal shear related fabrics, that in the Eocene, until around 40 Ma, the Pelagonian basement exhumed by syn-orogenic extrusion. This interpretation seems reasonable, but their structural cross-sections, largely compiled from literature, are not in line with their interpretation. I am not a specialist in geochronology, and I cannot judge the validity of the presented work about this. I leave the review of that part to others. I will discuss these points in further details below, as well as some smaller points. I find the new radiometric age data in this paper certainly worth publishing, but their interpretation and discussion section require more detail, notably concerning the points mentioned above and discussed below. I would consider these revisions moderate (but since that is not an option in the review system, I mention 'major').

Specific comments.

In lines 10-18 of page 9, the authors discuss the evolution of the extension directions: ˜40 Ma old brittle-ductile shear zones give a top to the NE sense of shear. A joint set striking N140°E suggests also NE-SW extension and is filled with a 5.4 Ma old dike. This set is crosscut by an E-W striking joint set, suggesting youngest N-S extension. They postulate two scenarios: either the dike emplaced right after the formation of the joint set, and the authors suggest that that would indicate a constant extension direction between 40 and 4 Ma, or the joints are much older and formed at the end of ductile shear, and the dike utilised a pre-existing weakness zone. In both scenarios, they conclude a significant change in extension direction from NE-SW to N-S after 4 Ma. Many paleomagnetic studies have shown that the west Aegean region has undergone considerable clockwise rotation: in a recent paper (van Hinsbergen et al, 2005a), we have synthesised the available information and concluded a 50° clockwise rotation of western Greece between ˜15-13 and 8 Ma. It is very likely that the OOP range was included in that rotation: 50° of clockwise rotation was also reported from Evia (Kissel et al., 1986; Morris, 1995), Chalkidiki underwent 37°cw post-Eocene rotation (Kondopoulou and Westphal, 1986), and also from Skyros, approximately 30° of rotation was reported (Kissel et al., 1986). These rotations should be included in the discussion of Lacassin et al. (2007), and the directions inferred from their kinematic indicators should be backrotated in the discussion about the evolution of the extension direction. Correction for the rotation leads to an Eocene N-S extension direction for the OOP range, which is therefore parallel to the post-4 Ma extension. The two scenarios of Lacassin et al. (2007) propose become more important now: if their first scenario is valid, in which the dike emplacement approximately dates the formation of the joint set in which the dike intruded, the extension direction underwent two changes: from N-S in the Eocene to NE-SW around 5 Ma, and back to N-S after 4 Ma. If their second scenario is valid, extension has been N-S from 40 Ma onward. I would suggest the authors to more thoroughly discuss this issue, and compare these options in the wider Aegean context.

My second main point of concern focuses on the structural cross-sections of Figure

2 of Lacassin et al. (2007) and their interpretation of syn-orogenic exhumation and extrusion of the OOP basement. Syn-orogenic exhumation was already concluded by Lips et al. (1998), who in Mount Ossa reconstructed the structural and metamorphic history of the basal part of the Pelagonian metamorphics. There, they showed that a blueschist mylonite, with white mica ages of 54 Ma, was overprinted by thinner greenschist mylonites, the abundance of which increases downward, and which give white mica ages of 45 Ma. The base of this metamorphic sequence is - as also indicated by Lacassin et al. (2007), a brittle fault, which emplaces the metamorphics on Eocene, only anchimetamorphic flysch (Kisch, 1981). From this, Lips et al. (1998) concluded that exhumation to upper crustal levels of the Pelagonian basement occurred largely in the Eocene, during thrusting. The question remained how the exhumation was accommodated: by erosion, or an extensional structure at the top of the exhuming unit? Erosion is unlikely, why would it be so local only. The ages and extensional structures reported by Lacassin et al. (2007) may indeed solve this problem, and paint a picture of an extruding HP/LT block between a thrust at the base and an extensional detachment at the top. A comparable history was recently shown for the Cycladic Blueschist unit on Evia on top of the Almyropotamos unit (Ring et al., in press-a). So far, I agree with the interpretations of Lacassin et al. (2007). However, if that scenario is compared to the structural cross-section of Mount Olympos of their Figure 2, some inconsistencies arise. In the case of extrusion, one would expect sub-parallel foliations at the base and top of the extruding basement unit, with opposite sense of shear at the basal thrust and the top detachment, such as elegantly shown by Ring et al. (in press-a) on Evia. However, the cross-sections in Figure 2 of Lacassin et al. (2007) suggest something entirely different. In the case of mount Olympos, the extensional top to the NE shear zone is interpretes to cut off the thrust at an angle of approximately 60°. Moreover, the shearzone according to this cross-section developed in the anchimetamorphic Mesozoic carbonates in the core of Mount Ossa. This is strange, because these have never been to high temperatures. Moreover, Lacassin et al. (2007) suggest that this shear zone is a low-angle detachment, but given the present-day orientation of the thrusts,

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this block must have been tilted by approximately 50° or so to the SEĚthe presently low-angle shear zone would become high angle and very steep if these developed contemporaneously. The drawn shearzone in the Olympos case is thus probably much younger than the exhumation and clearly post-dates the thrusting (assuming this interpretation of the structures is valid). The top-to-the-NE shearzone of the Pelion and the top-to-the-SW thrust in mount Ossa could indeed represent the exhuming structures. However, I wonder on what basis the authors conclude the extensional nature of the structures (even though I find it likely that these structures are extensional). Simply the present-day orientation is not very convincing, because the tilting of the thrusts now suggests a normal motion. Emplacement of old metamorphic on young non-metamorphic convincingly proves thrusting. However, such criteria are not reported from the Pelion. In fact, the authors draw a thrusted unit above the Pelagonian of the Pelion. Is that the Ophiolite nappe? And if so, what is the nature of that contact? If these ophiolites have no record of Eocene metamorphism (and to my knowledge, that has not been reported), wouldn't this contact be the detachment then?

In summary, there is little conclusive evidence for the inferred extensional nature of the ductile extensional shear zone dated and documented on the Pelion. I agree with the authors that their interpretation that this zone represents the exhuming structure at the top of the Pelagonian basement block is likely, but there lies some danger for circular reasoning. If the authors have independent evidence for the extensional nature, I would like to urge them to include that (such as the nature of the contact with the overlying nappe). Otherwise, don't indicate 'extensional' in the text or the figures, but argue for that case in the discussion. The extensional structure at Mount Olympos must post-date the thrust-emplacement.

Below, I will list some smaller remarks, indicated by line and page number:

Title: I would suggest to change the titel into a more concise and conclusive one. For instance: Syn- and post-orogenic exhumation of the HP/LT Pelagonian basement„ N. Aegean or so. In fact, you conclude that at least part of the history is syn-orogenic.

Would you call that compressional or extensional?? You have thrusting and extensional detachment activity at the same timeĚ

p.2, l.5: ‘main exhumationĚoccurredĚat ca 43-39 Ma’, I would say ‘ended’ instead of ‘occurred’, as (Lips et al., 1998) made a case that exhumation started at or prior to 54 Ma.

p.2, l.7: ‘orogenic shortening in the close area’Ěin fact: coeval thrusting at the base of the Pelagonian unitĚ

p.2, l.10: ‘brittle normal faultingĚassociated with the onset of Aegean extension’: I would say ‘associated with Neogene Aegean extension’ or so, not necessarily the onset.

p.2, l.14: ‘Such a shift [in extension] is probably related to propagation of the NAF into the Aegean’. Around 5 Ma, many changes occur in the Aegean region, and the propagation of the NAF is just one of them. Other changes include the formation of the Kefallonia Fault Zone, the Pliny and Strabo trenches, rapid subsidence in the Pliocene volcanic arc, etc. I would suggest to add here ‘probably related to processes that led to the propagation of the NAF’, or so.

p.3, l.19: ‘culminations affecting thrust-nappes’. What do you mean? ‘culminations of thrust nappes?

p.3, l.22: ‘covering the passage from compression to extension’Ěit is difficult here to subdivide between compression and extension hereĚI would say ‘covering the Eocene exhumation period’ or so.

p.3, l.25: ‘..and show exposures of low angle ductile normal faults’. Now they are low-angle, but were they also when active? And now they are no longer ductileĚchange ductile normal fault to myloniteĚand see also my earlier notes about the ‘extensional’ interpretationĚyou don’t know for sure.

p.5, l.4: ‘earlier extensional ductile deformation’Ěagain: what is the independent evidence for extension?

p.5, l.7: ‘the thrust nappe edifice in the Ossa range’: For clarity, I would add the structural cross-section of Mount Ossa (Figure 2 of Lips et al., 1998) to your Figure 2.

p.7, l.28: typo: feldsdpars

p.8, l.10: search & replace in manuscript: gaz should be gas.

p.8, l.10: spectra should be spectrum

p.9, l.12: ‘the same than’ should be ‘the same as’. See further my comments on the extension direction and the west-Aegean rotations.

p.10, l.5: ‘The stacking of thrust nappes occurred after the deposition of the Ěturbu-ditesĚthus after 50 Ma’. No. Thrusting may have started earlier, emplacing the Pelagonian over lateral, and now further underthrusted equivalents of the lower nappe, while the Ypresian flysches were still in the foreland receiving flysch sediments. Apparently around 50 Ma, the lower unit was underthrusted and finally, around 40 Ma or so, the Pelagonian basement reached its present position with respect to the flysches.

p.10, l.10: ‘It could be due to upward wedge extrusionĚas in the Himalayas or Pamir’. I think you can make a stronger case than that, especially with the conclusions of Lips et al. (1998) at handĚand I would refer to the work of e.g. Jolivet et al. (2003) and Ring et al. (in press-a; b) for examples of this process in the direct vicinity, instead of the Himalayas. The Cycladic Blueschist exhumed from blueschist/eclogite conditions to greenschist conditions prior to the underthrusting of the Basal unit. (I compiled the supporting data for this recently, you can have a look in van Hinsbergen et al., 2005b).

p.10, l.14: ‘tenths of degrees’ĚI think you mean ‘tens’Ě

p.10, l.21: ‘and possibly of exhumation’Ěit has certainly enhanced further exhumation, but probably not much.

p.10, l.21: ‘was triggered by the propagation of the NAF’Ěagain I would say ‘was trig-

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gered by the processes that also led to the propagation of the NAF', or something alike. You don't know what is cause and consequence. Did NAF propagation induce Aegean extension, or did Aegean extension allow the NAF to propagate? Or was it collision of western Greece with Apulia? STEP faulting along the Kefallonia Fault Zone? Based on the presented data in this paper you have no ground to exclude other possibilities, so I would suggest to keep the options open.

p.11, l.4: 'exhumationĚmay have been synorogenic'Ěthe thrust evolution documented by Lips et al. (1998) shows that it was synorogenic. And your Pelion results have now pointed at a candidate for the other, extensional side of the extrusion wedge.

p. 11, l.9: 'OurĚdocumented large-scale changes in fault geometry and kinematics suggest that it has been Ě affected by Ě the NAF since ˜5 Ma'. Can you still draw this conclusion after you incorporated the rotation history? If your second scenario is valid (see my earlier comments), there is no change in extension direction, only a rotation of your older kinematic indicatorsĚ

1) Does the paper address relevant scientific questions within the scope of EE? YES

2) Does the paper present novel concepts, ideas, tools, or data? YES

3) Are substantial conclusions reached? NEEDS IMPROVEMENT

4) Are the scientific methods and assumptions valid and clearly outlined? YES, for geochronology, ask a specialist

5) Are the results sufficient to support the interpretations and conclusions? NEEDS IMPROVEMENT

6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? YES

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? NEEDS IMPROVEMENT, in fact, I think they can draw firmer

conclusions

8) Does the title clearly reflect the contents of the paper? NO

9) Does the abstract provide a concise and complete summary? YES

10) Is the overall presentation well structured and clear? YES, maybe add a conclusion section

11) Is the language fluent and precise? YES

12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? YES, but please add a key to Figure 2.

13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? NO

14) Are the number and quality of references appropriate? YES, plus see my remarks about the paleomagnetism and the references to comparable work in the Aegean

15) Is the amount and quality of supplementary material appropriate? I leave that for the geochronology specialists.

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