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eEarth Discussions

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Interactive Comment

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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General comments: The paper of Lacassin focuses on the younger history of part of the internal Hellenides. It is a relatively short communication based on a limited data set of 5 40Ar/39Ar incremental heating experiments. While the major part of the metamorphic history of the Olympos-Ossa-Pelion ranges and in its extension the Cyclades occurred during the Cretaceous and its waning stages largely during the Eocene, there is a clear message in its younger history. Obviously for its setting Lacassin et al. draw heavily on the previous work by Schermer and by Lips. Lacassin and co-workers spell out what is more or less implicitly assumed by Lips and by Schermer, namely that HP

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metamorphism had ended by the Eocene, and that there was a relatively quiet period until the internal Hellenides got themselves caught in the next orogenic phase, that of slab roll-back, extension and the initiated of the Northern Anatolian fault. The new data of Lacassin reinforce conclusions reached earlier by several workers (Wijbrans and McDougall in 1988, Schermer and co-workers in 1990, Lips and co-workers, 1998, 1999, Keay et al. 2001) that Miocene metamorphism in the Aegean domain was preceded by a quiet period for an extended part of the Tertiary. I welcome the publication of this work as it sheds some additional light on the interpretation of the thermal evolution of what is rapidly becoming one of the textbook examples of blueschist metamorphism.

Specific comments: Page 1; Line 21: The authors argue that 'However, the timing of inception of continental stretching, which is critical for constraining mechanical evolution models of the Aegean lithosphere, remains poorly constrained.' Actually there is a good date in Lips work dating thrusting under greenschist conditions at Mt Ossa at ca 42 Ma (as quoted by the authors on pag 6: line 21).

Page 6; line 11: Alternatively, the LT steps could be interpreted as the result of cooling of all samples below phengite closure temperature (˜350 oC) at that time. The rational for the argument that the ages recorded in the LT steps record the time cooling below the closure temperature of phengite is not entirely clear to me. Surely, any phengite plateau age should be interpreted as the time of cooling of the system below the closure temperature! In a more complex thermal history, after original cooling, when during a subsequent thermal pulse there was diffusion loss of argon from the crystal lattice, diffusion gradients may be produced. Wijbrans and McDougall in 1986 pointed out that you cannot easily constrain the temperature of overprinting from a diffusion gradient alone, as diffusion is a function of several parameters, temperature, duration of the thermal pulse, but also grain size.

Page 11; line 16: J factors were estimated by the use of duplicates of the Fish Canyon sanidine standard with an age of 28.48Ma (Schmitz et Bowring, 2001; Schmitz et al., 2003). There is some ongoing discussion as to the age of Fish Canyon Tuff sanidine, a commonly used standard / flux monitor of 40Ar/39Ar dating. The choice of Lacassin et al. to apply the zircon date published by the MIT group as the age of the extrusion of the magma and hence the true age for the sanidine, is almost certainly the wrong choice, given that there are reasons to assume that the zircons may have recorded some history since crystallization before extrusion. There may well be a small but true discrepancy between the zircon U/Pb age for Fish Canyon and its sanidine 40Ar/39Ar age. Until this discrepancy is fully resolved (cf. the work of Kuiper and co-workers, 2004, 2005), I would strongly advise the authors to follow the age calibration for Fish Canyon sanidine recommended by Renne and co-authors 1998). In the context of the discussion of metamorphic histories as loosely timed as in this paper and in the work of Schermer and Lips, the differences originating from this error are trivial. However, in the context of timescale studies, we'd be dealing fundamental choices. Therefore the authors are advised to choose wisely.

Appendix A2: I am a bit confused by the meaning of the statement on interpretation of age spectra, as expressed in Appendix A2. For me the plateau is still alive and well. Especially when working on volcanic systems, we commonly measure perfect plateaus! Even in metamorphic minerals I have come across many plateaus that are near perfect. Deviations from ideal preservation almost always can be attributed to imperfect preservation of the minerals due to complex thermal histories that have caused partial alteration of the minerals used for dating. Much of such alteration doesn't follow the ideal world of volume diffusion and as a consequence cannot be understood from diffusion modeling. Having made these statements, the reader will understand that I am a bit at a loss as to the interpretation of the K-feldspar data. I do accept that it is highly unlikely that these minerals were substantially overprinted after ca 40 Ma. However, are these experiments revealing plateaus or diffusion gradients? The question becomes more intriguing, as it is stated that these minerals are from mylonites. Therefore I can see three different scenarios: 1. perfect preservation, which may be understood in terms of a plateau age; 2. substantial (nearly 100%) diffusion loss from the smaller domains; and 3. mechanical grain size reduction perhaps in combination

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with recrystallization. Only for option 2. MDD diffusion theory is valid! It is a bit confusing to see tables with MDD data, and no mention of modeled histories. In the text only relatively qualitative statement concerning closure temperature ranges can be found. The case of option 1. (a plateau is preserved) allows the calculation of a Tc based on simple diffusion theory. If case 3. (mechanical grain size reduction) is true the use of diffusion theory (either simple Dodson type calculations, or the more involved Lovera type MDD calculations) should be avoided as the boundary conditions on which diffusion models are based (the mineral remains stable during the experiments) are not fulfilled. From the discussion of the authors, it seems to me that we are dealing at best with a mix of case 1 and 3, and as a consequence I suggest that the authors be cautious when citing closure temperatures for these K-feldspars.

In the text the white mica experiment is said to be carried out on muscovites, whereas in the table the minerals are described as sericites. What is the grain size of these micas. If they are true sericites, are the results valid in the light of potential recoil artifacts?

Brief comments:

Page 2; Line 1: 'encompass Greece highest summit' should read: 'encompass the highest summit of Greece' Line 2: 'the extremity of the North' should read: 'the western extremity of the North' Line 9: 'appears' to read 'appear'

Page 8, lines 10 and 11: 'gaz release', to read 'gas release'

Page 9; line 22/23: 'Our structural observation and Ar/Ar data yield us to conclude that final exhumation of the OOP range resulted from three successive tectonic stages' should be rephrased to read: 'Our structural observations and 40Ar/39Ar data lead us to conclude that final exhumation of the OOP range resulted from three successive tectonic stages'

Page 11: line 22: 'liquid air' more commonly 'liquid nitrogen' is used for this. Line 22:

’Zr-Al geters’ to read ’Zr-Al getters’

Evaluation points:

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, the paper presents new data on an issue that is in ongoing debate- 3) Are substantial conclusions reached? -YES- 4) Are the scientific methods and assumptions valid and clearly outlined? -MOSTLY, please note technical points raised in the above evaluation- 5) Are the results sufficient to support the interpretations and conclusions? -For a relatively small contribution, Yes, results sufficiently support conclusions reached.- 6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? - the presented data is sufficient to judge their quality, MDD data table can be omitted if the authors don’t discuss their MDD results- 7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? -On the whole YES, there is much more literature that could be cited in the context of this paper, e.g. stuff concerning opening and closing of isotope systems, that probably wouldn’t add to the clarity of the arguments.- 8) Does the title clearly reflect the contents of the paper? -I would call the studied region ‘Internal Hellenides’ not ‘N. Aegean’- 9) Does the abstract provide a concise and complete summary? -YES- 10) Is the overall presentation well structured and clear? -YES- 11) Is the language fluent and precise? -YES- 12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? -YES- 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, but please take above discussion into account- 15) Is the amount and quality of supplementary material appropriate? -YES, but please note remark on MDD data table-

Additional references cited (as copied from WoS):

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Keay S, Lister G, Buick I The timing of partial melting, Barrovian metamorphism and granite intrusion in the Naxos metamorphic core complex, Cyclades, Aegean Sea, Greece TECTONOPHYSICS 342 (3-4): 275-312 Sp. Iss. SI DEC 2001

Kuiper KF, Hilgen FJ, Steenbrink J, et al. Ar-40/Ar-39 ages of tephras intercalated in astronomically tuned Neogene sedimentary sequences in the eastern Mediterranean EARTH AND PLANETARY SCIENCE LETTERS 222 (2): 583-597 MAY 30 2004

Kuiper KF, Wijbrans JR, Hilgen FJ Radioisotopic dating of the Tortonian Global Stratotype Section and Point: implications for intercalibration of Ar-40/Ar-39 and astronomical dating methods TERRA NOVA 17 (4): 385-397 AUG 2005

Renne PR, Swisher CC, Deino AL, et al. Intercalibration of standards, absolute ages and uncertainties in Ar-40/Ar-39 dating (vol 145, pg 117, 1998) CHEMICAL GEOLOGY 149 (3-4): 259-259 JUL 31 1998

WIJBRANS JR, MCDOUGALL I METAMORPHIC EVOLUTION OF THE ATTIC CYCLADIC METAMORPHIC BELT ON NAXOS (CYCLADES, GREECE) UTILIZING AR-40/AR-39 AGE SPECTRUM MEASUREMENTS JOURNAL OF METAMORPHIC GEOLOGY 6 (5): 571-594 SEP 1988

WIJBRANS JR, MCDOUGALL I AR-40/AR-39 DATING OF WHITE MICAS FROM AN ALPINE HIGH-PRESSURE METAMORPHIC BELT ON NAXOS (GREECE) - THE RESETTING OF THE ARGON ISOTOPIC SYSTEM

Interactive comment on eEarth Discuss., 2, 1, 2007.