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Solid Earth
Discussions

Interactive comment on "Application of titanium-in-quartz thermobarometry to greenschist facies veins and recrystallized quartzites in the Hsüehshan range, Taiwan" *by* S. Kidder et al.

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Review of ms: Application of titaniumin-quartz thermobarometry to greenschist facies veins and recrystallized quartzites in the Hsuehshan range, Tiawan

By: S. Kidder, J-P Avouac, Y-C Chan

Reviewed by: Frank Spear

General comments

This is a well-presented manuscript about the applicability of Ti-in-quartz thermobarometry in low-grade quartzites. I thoroughly enjoyed reading the text and am pleased to say that I learned quite a bit in doing so.

I've made numerous comments keyed to lines in the text that the authors might wish to consider. My one major concern is the use in this manuscript of an "average geothermal gradient" to establish a context for interpretation of the TitaniQ results. As I understand the logic, independent temperature estimates are inferred largely from the deformation characteristics of the quartz (this is fine), and then the geothermal gradient is used to establish a pressure from this temperature so that this pressure can be used in the TitaniQ thermobarometry. I guess I find this a bit circular, and even more important, it might be missing some of the most important potential of this work. My thinking is this: the rocks have undergone a period of shortening and thickening (rates possibly as high as 9 cm/year), which will undoubtedly perturb the geotherm. I would suspect that this shortening was also accompanied by strain which might be responsible for the recrystallization of the quartz. So why not use the pressure dependence of TitaniQ to get an estimate of the pressure of this deformation and recrystallization and compare these with the thermal modeling? I think this might be far more interesting than a comparison of the two calibrations (also since the H&A calibration appears to be thermodynamically unreasonable.)

However, this is not a requirement of revision of the manuscript, but rather a suggestion for the authors to consider. The manuscript (with some minor corrections listed in the pages below) is well-worthy of publication.

Specific comments (keyed to lines in text)

Page 1, lines 23-24. This is an odd way of phrasing. The Thomas et al calib. doesn't make any predictions. It is a correlation between Ti concentration and T,P. Perhaps you could say something like: "Using lithostatic pressure and Ti concentrations measured by SIMS, the Thomas calibration yields temperatures consistent with other estimates

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(specify) of recrystallization temperature."

Page 2, Lines 12-13. There is evidence in Spear and Wark (2009) that detrital quartz does recrystallize at low temperatures. Specifically, the uniformity of Ti concentrations in schists of the garnet grade. Interestingly, these quartz grains yield TitaniQ temperatures less than the garnet isograd, suggesting that quartz recrystallizes more or less completely around 350-400 C then doesn't change unless affected by fabric evolution or metamorphic reaction.

Page 3, line 6. Temperature-time (not Pressure-Temperature) in Fig. 4

Page 3, lines 13-16. Why the switch in conditions? Any conclusions as to which are better?

Page 4, lines 33-34. I question whether the assumption of a constant, linear, steady-state geothermal gradient is warranted in a terrane undergoing convergence at 9 cm/year. This magnitude of tectonic velocity will perturb the geotherm, and quartz may be recrystallizing during this perturbation or during subsequent thermal relaxation.

Page 5, line 8. I would think the largest uncertainty is (a) the local Ti activity (i.e. is it consistently 1.0 along every grain boundary?) and (b) uncertainty in the instantaneous geotherm.

Page 5, lines 20-21. What is the wavelength sensitivity of this detector? It is well established that only CL in the blue (ca 415 nm) correlates with Ti concentration.

Page 5, line 29. Could you add a sentence describing these criteria used by Tillman?. Also "before or after collision" requires substantial interpretation. Is there a more observation-based criteria you could cite?

Page 6, line 33 and Fig 2c. Shouldn't you put error bars on the grey diamonds? ±50C is substantial and would reveal overlap with much of the TitaniQ data.

Pages 6-8 (Section 4.2 about temperature constraints). Are there any independent estimates of pressure for any of these rocks? It's too bad if not, because the discussion gets a bit circular otherwise (i.e. choose a temperature and a geothermal gradient to get the pressure, then see if TitaniQ matches the original temperature at this pressure.)

It might be quite interesting to simply assume that the recrystallized quartz gave accurate equilibration temperatures/pressures and use these to get the pressure (depth). I wonder if this would show anything about the thermal perturbation during collision?

Page 8 lines 5-7. Regarding strain rates and shear heating. Using bulk strain assumes uniformity of strain distribution. More likely the strain is partitioned and not uniform, so it's difficult to interpret what you say here.

Page 8 lines 15-17. The uniformity of Ti concentrations is a very important and significant observation.

Page 9 Lines 13-15. Yes - I agree that your data strongly indicate an approach to equilibrium!

Page 10, lines 4-5. I agree that bulk diffusion is not evident, but isn't the important process grain boundary diffusion? We have no quantification of this at all, but your data strongly suggest that grain boundary diffusion was sufficiently fast to allow equilibration with rutile at some distance from the recrystallizing quartz.

Page 10, lines 30-35 regarding H&A calibration. Of course, the H&A calibration also predicts a very unlikely curvature to the isopleths, which makes little thermodynamic sense (it would require a radical P-T dependence of the molar volume of the TiO2 component in quartz, which seems highly improbably for a trace element in the Henry's Law limit.

Page 11, Line 8… "there is no reliable procedure for distinguishing between inclusions and high impurity concentration…".

You might be able to distinguish if you analyzed many more elements be able to distinguish quartz from impurities.

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Page 11, line 31-32. Why do you say "compressional deformation began". Biotite in pressure shadows only constrains deformation to be ongoing at peak T. How about "...indicating that compressional deformation was operative (or ongoing) while temperatures were at or near peak conditions." Page 11, line 38. re “onset of collision”. I guess I just don’t like this term because it requires significant interpretation of observations that aren’t really a a part of this paper. How about using instead the term "onset of deformation".

Page 12, lines 9-10. Why do you say "lack of resetting?" The quartz veins have 0.2-1 ppm Ti. Can’t this be the equilibrium Ti content at the conditions the veins formed - i.e. there would be no driving force for resetting.

Page 12, line 16, line 20, line 30 and elsewhere. Some odd typos e.g. “te16” (possibly just a font issue with Adobe pdf.)

Page 13, lines 1-2. I think we know the sensitivity of TitaniQ. The question is, what pressure do the quartz and rutile feel? For matrix quartz and rutile, I would suspect it must be lithostatic, since the phases are under a column of rock. For veins it is not clear since the quartz veins are likely precipitated from aqueous fluids. But this is a good and important question.

Page 13, lines 4-8. Your paper really isn’t about the deformation per say - you only use it to establish a temperature framework. Although this statement is true, I wouldn’t say it is a "conclusion" of your study.

Page 13. Conclusion section. I don’t think this conclusion section is really necessary, since the abstract says everything you mention here quite well.

Figure 2 caption. At what pressure were Thomas et al temperatures calculated? Also, assuming what activity of TiO2? Also, Figure shows both blue and purple are TitaniQ temperatures, but last line of caption is ambiguous. Do you mean that the blue are TitaniQ temperatures for which there are also dynamic recrystallization temperatures?

Figure 3. You mention conjugate veins. Conjugate veins should have intersection angles of less than 60 degrees. I don't see this - what I do see are angles more like 90 degrees. Do you really mean "contemporaneous (with folding) veins"?

Figure 9 caption. Do you mean “Photomicrographs” (rather than microphotographs)?

Figure 10 and 11. CL images. You need to add some text about the wavelength of light that the CL is detecting. Only in the blue region is CL sensitive to Ti concentration.

Figure 12 caption. I understand this caption after reading the text, but it would be better to rewrite it so it can stand on its own. Specifically the last 3 lines don't make much sense. Here are the things I pondered when I first read the caption. . .. (1) How can temperatures based on Ti calibrations be based on independent T constraints? (2) How can you base a Ti temperature on a geothermal gradient? (3) How can you base a temperature scale on a Ti calibration and then show the blue bar not exactly coincident with the histogram bars?

Figure 6. The P-T path is worthy of additional consideration. (1) The path starts at 9 Ma. but doesn't show how the rocks got to the depth of 20 km. This can only be the result of thickening due to thrusting and shortening. If so, the prograde PT path would be much steeper than the 25 C/km used here (I suspect that the Simoes et al model has these data - it would be worth looking at). And isn't it likely that deformation started then - i.e. during shortening? You could be crossing the TitaniQ isopleths for 0.2 to 1 ppm in this part of the prograde path when the detrital quartz is recrystallizing.

Interactive comment on Solid Earth Discuss., 4, 663, 2012.