Solid Earth Discuss.,
https://doi.org/10.5194/se-2019-124-RC1, 2019

Solid Earth
Discussions

Interactive comment on "The relaxation of residual inclusion pressure and implications to Raman-thermobarometry" *by* Xin Zhong et al.

Viktoriya Yarushina (Referee)

viktoriya.yarushina@ife.no

Received and published: 24 September 2019

Presented manuscript discusses pressure variations around inclusions in the homogeneous rock matrix and their implications for the accuracy of Raman-thermobarometry measurements. Authors study two different processes that might lead to stress changes around a single inclusion: stress relaxation on a geological time scale due to viscoplastic stress relaxation and proximity of the free surface to the mineral inclusion during sample preparation in the lab. Authors show that both stress relaxation and presence of free surface might alter stresses inside inclusion and in the host matrix. Hence, they might lead to erroneous estimations during Raman-thermobarometry. While this is an interesting paper, its logic and presentation could be improved. Authors are using two different problem setups and switch in the text from one of them

Printer-friendly version

Discussion paper

to another without much mentioning of it. I would recommend revising the manuscript and clearly separate results related to sample preparation (i.e., setup with elastic solution for half-space) and results related to stress relaxation on a geological time scale. Some other points to consider:

- Throughout the manuscript. Use of term “relaxation” with respect to changes in stress due to the presence of free boundary as in sample preparation setup and due to plastic effects is incorrect and confusing. See e.g. original paper by Zhang (1998) where he states that “Plastic yielding does not relax the stress but does limit the deviatoric stress” (page 215). I would call these processes rather “stress release”. Besides, authors consider viscoelastoplastic model for stress relaxation where plasticity contributes simultaneously with viscosity, i.e. purely plastic (or elastoplastic) stress release is not considered.

- Lines 48-50. Authors state that “Mechanical models show that both viscous creep (dislocation or diffusion creep of host) and plastic yield (radial or tangential microcracks) can cause significant pressure relaxation (Dabrowski et al., 2015; Zhang, 1998).” While cited references indeed present viscous relaxation models, none of them presents mechanical model that shows plastic yield or radial and tangential microcracks. Care with references is needed.

- Section 2.1. Logic of this section can be improved. Equation (2) uses results of equation (4), which is further in the text. It is better to introduce plastic flow law (4) first and then give summary equation for total strain rate (2).

- Lines 84 and 86. The choice of references for classical Tresca yield criterion and associated plastic flow law is a bit odd. There are much older, standard and very good textbooks that introduced those, e.g. [Hill, 1950; Kachanov, 1971].

- Lines 85 – 89 and throughout the text. Parameter C in the Tresca yield condition is not a cohesion. It is a half of the yield limit for simple tension of the host matrix in the case of spherical inclusions [Hill, 1950; Kachanov, 1971]. This is important to note because later in the text (in the discussion section) authors make estimations of this parameter based on the experimental data for cohesion and make conclusions for Raman-thermobarometry. Also, I would want to mention that Tresca criterion represents yielding due to dislocation sliding in crystalline materials at high pressures and thus cannot be taken as a proxy for fracturing. You need a more thorough discussion on the deformation mechanisms in the host rock to justify the choice of yield function and viscosity (eqn (5)). There are various deformation mechanism maps for viscosity can be found in the literature.

- Line 83. Wrong statement: "lambda is the plastic multiplier (s-1) which guarantees that the plastic yield criterion is not exceeded". Plastic multiplier provides the amount of the plastic deformation. However, in the numerical codes it is indeed calculated from yield criterion and consistency conditions.

- Equation (9). Explain parameter delta.

- Lines 102-105. Authors write that "This is done by choosing the following independent scales: the inclusion radius, temperature change, time, viscosity pre-factor of host, plastic cohesion of host, and the expected pressure perturbation that is given as follows. . ." Again, be careful with your statements. These scales are not independent. You can have only one independent scale of pressure, time, temperature, length. Thus, viscosity factor and cohesion would be already dependent parameters.

- Line 109. It is not entirely clear why Pexp is chosen as a scale. Please explain this parameter, where it comes from. Also check eqn (11). It is not logical to scale yield function F and cohesion to different scales.

- Lines 123-127. Again, C is not cohesion. Please rewrite this para. Statements on the range of viscosity and yield limit must be supplemented with references and even better if with realistic numbers. I doubt that one can expect orders of magnitude variation in yield strength of minerals as it is stated by authors: "the cohesion of difference mineral may also vary by many several factors, potentially orders of magnitude".

- Section 2.3. Given that there is no reference for the numerical approach, I assume it is original. Or was it previously reported somewhere? In general, there is a different level of detail in the paper. E.g. there is too much focus on standard non-dimensional analysis and almost nothing on non-trivial viscoelastoplastic numerical scheme or on the elastic numerical solution for half-space in the following sections.

- Section 2.4. It is hard to see the value of this section in the paper. Authors present analytical solution previously derived by Seo and Mura [1979]. However, no meaningful analysis or conclusions for the topic of the paper (i.e. Raman-thermobarometry) were derived from this solution. Its use for benchmarking of numerical code is also limited as analytical and numerical results are different due to various assumptions about material properties of inclusion. On the other hand, other analytical solutions used for benchmarking in section 2.3 are not resented at all. If authors choose to keep this solution, I would recommend discussing carefully boundary, initial conditions and its relation to the Raman-thermobarometry. For example, is it an incremental solution and does it show changes in stress from a specific initial condition? Or does it show stress distribution in an inclusion-host system without initial pre-stress? What do we learn from this solution?

- Line 172. Why Pexp is referred to as "initial residual pressure"? As this solution is presented now, there is no process in it, only static force equilibrium.

- Section 3.1. Switch from one problem setup to another comes very abrupt here. Please document your simulation setup (geometry, boundary and initial conditions, properties of the host and inclusion) and state which problem you address (i.e. stress relaxation or sample preparation). The title of this section is inconsistent with the following sections.

- Line 183. "This diagram may assist petrological investigations because De and C* can be evaluated based on experimental rock deformation data for different minerals…" Please discuss how De and C* can be evaluated based on experimental data. Which data is available?

- Line 196. Awkward phrase: "…and De is located above the plastic onset…" Please reformulate.

- Section 3.2. Describe problem setup, boundary and initial conditions. Given that you have two different problem setups in the paper it is confusing. Governing equations and a little bit info about numerical implementation would also fit here rather than supplementary materials in the same way as you present another model. Without such descriptions, it is hard to see the relevance to sample preparation problem. Do you consider just an equilibrium stress state, or do you have an incremental formulation that considers initial condition? Check for use of word "relaxation" here and rather use "release". Check also for consistent use of "quartz-in-almandine" and "quartz-in-garnet" terms.

- Line 210-216. What are the implications for sample preparation, e.g. in terms of thickness, etc?

- Line 223. "Assuming that the thin-section surface is sufficiently far away from a quartz inclusion and no microcracks appear around quartz inclusion…" I recommend replacing "microcracks" with "yield" as your solution does not consider microcracks and there is a discussion on microfractures later.

- Line 227. "The flow law of garnet from Wang and Ji (1999) is applied…" Please describe briefly this law.

- Line 272. "The three mechanisms investigated here, i.e. viscous creep, plastic yield and proximity of inclusion…" Plastic yield was studied only together with creep, i.e. on a geological time scale. Plastic yield without creep as might occur e.g. during sample preparation was not studied. Thus, I think it is more appropriate here to use term viscoplastic flow instead of plastic yield.

- Section 4.1. C is not cohesion, please check relevant values and your estimations for

Ch.

- Lines 283-289. “This suggests that plastic yield does not occur in an idealized scenario of isotropic, spherical quartz inclusion entrapped in infinite garnet host. However, such an ideal scenario is highly improbable in natural samples. The observed cracks in garnet host may be formed due to potential reasons including: 1) elevated differential stress when the inclusion is close to thin-section surface (“ring” shaped pattern in Fig. 4a); 2) stress concentration at the corners of quartz inclusion (Whitney et al., 2000); 3) anisotropic elastic deformation of the quartz inclusion (e.g. Murri et al., 2018); 4) pre-fractures/weakness in garnet host before the entrapment of quartz inclusions.”

While I agree with the possibility of elevated differential stress and stress concentrations at the corners, I would like to emphasize that this statement is based on the solution for materials obeying Tresca criterion, which does not describe fracturing. To make conclusions about fractures around inclusion, one needs to consider other failure criteria such as Griffith or Mohr-Coulomb, where cohesion and tensile strength play major role. Solutions for plasticity onset and failure pattern in elastoplastic and viscoplastic rocks with these failure criteria are available in the literature. They would give other estimations for pressure necessary to induce fractures.

- Conclusions. “We presented a 1D visco-elasto-plastic model to study the inclusion-host system undergoing prograde/retrograde P-T path” There are at least two different models in this paper.

- “A simplified analytical solution for inclusion pressure (Eq. 32) close to stress-free thin-section surface is derived.” The solution presented by authors was not new, it was reroduced after original aer by Seo and Mura [1979].

- Please also make some statements on the implications for Raman-thermobarometry and how to use your results.

References:

Printer-friendly version

Discussion paper

Hill, R. (1950), The mathematical theory of plasticity, 356 pp., Clarendon Press, Oxford,. Kachanov, L. M. (1971), Foundations of the theory of plasticity, xiii, 482 p. with illus. pp., North-Holland Pub. Co., Amsterdam,.

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2019-124/se-2019-124-RC1-supplement.pdf

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2019-124, 2019.