

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

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### General reply to reviewer 1

We thank the reviewer for the very careful examination of our work and helpful comments that have greatly improved the quality of our paper. All the comments from the reviewer have been carefully considered and point-by-point replies have been provided in this letter. The original comments from the reviewer are in black color and our replies are in blue color. The revised manuscript has been uploaded.

### Comments from Reviewer 1

Presented manuscript discusses pressure variations around inclusions in the homogeneous rock matrix and their implications for the accuracy of Raman-thermobarometry

C1

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 visco-plastic 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 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.

This is a very helpful comment. Indeed, our previous structure mix the two main parts of the model. We have re-structured the entire manuscript to clearly separate the two different models and corresponding results/discussions. We hope our revised structure will be clearer for readers.

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”. I would call these processes rather “stress release”. Besides, authors consider visco-elasto-plastic model for stress relaxation where plasticity contributes simultaneously with viscosity, i.e. purely plastic (or elastoplastic) stress release is not considered.

We agree with the reviewer’s comment and we have followed the reviewer’s suggestion to change “stress relaxation” in plastic yield situation and proximity to surface situation into “stress release”. We also added new paragraphs (in “Introduction” and “Residual pressure affected by viscous/plastic flow” sections) to make clarification that in our case, we refer to viscous relaxation as a time-dependent process and plastic yield as

C2

time-independent process that limits the deviatoric stress in the host, thus also limits the residual inclusion pressure. Both of them are irreversible.

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 micro-cracks) 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.

We agree that in Dabrowski et al. 2015, plastic yielding was not studied. But in Zhang 1998, he showed the effect of plastic yield on the stress distribution (Fig. 2B). Due to the limited differential stress in the host upon plastic yield, the stress state in the inclusion will be changed. In his section 4.3, Zhang (1998) focused on the radial cracks formed during decompression. Although he did not quantitatively show the amount of pressure release but he did mention about the fractures formed around various mineral inclusions depending on the bulk modulus of the inclusion and host.

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).

This part is now clarified. We thank the reviewer for pointing it out.

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].

Reference is updated, we added Kachanov (1971) as suggested.

Lines 85 – 89 and throughout the text. Parameter C in the Tresca yield condition is not 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

C3

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 (Eq. 5). There are various deformation mechanism maps for viscosity can be found in the literature.

This is a valid point and we agree with the reviewer. We have updated the reference based on the reviewer’s suggestion and carefully checked the entire manuscript to adjust “cohesion” into “yield strength”. We have also removed the parts for “fracturing” and replace them by plastic yield.

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.

This sentence has been adjusted as suggested.

Equation (9). Explain parameter delta.

Clarified as suggested.

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.

This is correct. We thank the reviewer for this point. We have corrected this sentence.

C4

The parameters we used to nondimensionalize the system are not independent, which has been noted in the revised manuscript by deleting “independent”.

Line 109. It is not entirely clear why  $P_{exp}$  is chosen as a scale. Please explain this parameter, where it comes from.

This is a good point. We missed the explanation for  $P_{exp}$ . It comes from the paper of Zhang (1998) which described the amount of residual pressure assuming constant bulk modulus and thermal expansivity. Using it as a pressure scale is advantageous as the inclusion pressure will vary from 0 to 1. Now this is clarified in the text.

Also check Eq. 11. It is not logical to scale yield function  $F$  and cohesion to different scales.

We thank the reviewer for this point. We have changed the scale for  $F$  with the yield strength of the host. This does not influence the other equations as  $F$  does not appear in other equations.

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 different mineral may also vary by many several factors, potentially orders of magnitude”.

We have removed all “cohesion” words following the suggestion. We have removed the statement of “several 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 visco-elasto-plastic numerical scheme or on the elastic numerical solution for half-space in the following sections.

We agree with the reviewer that the numerical approach is relatively short compared to

C5

other parts. We have added some new descriptions and references on this part. We would not say the numerical approach we presented as original, where we use finite difference method (staggered grid stencil) and iteratively solve for the three unknowns from three equations. The numerical stencil (staggered grid finite difference, mostly in 2D/3D) has been used widely (e.g. Gerya 2010, now cited in the text) and the iterative solver (Picard iteration) is also relatively common in numerical modelling.

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?

This is a very valid comment. A similar point has also been raised by the second reviewer. We have now reduced the focus of the “half-space” problem by moving all the derivations into appendix and we only left the final analytical formula for pressure distribution in the main text (which is a new result). We have added a new section discussing the application of the presented formula together with an updated figure (Fig. 3 in the revised version) to show the results of the analytical formula for pressure distribution of inclusion in half-space. We agree that the application for numerical benchmark is limited by the fact that the presented formula is only applicable to homogeneous inclusion. However, due to the simplicity (compared to the analytical formulas of e.g. Mindlin, and Seo Mura) we argue that it is still useful. This we have discussed it in a new paragraph

C6

for a benchmarking purpose in the appendix.

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.

Pexp is the initial overpressure that is already under force equilibrium in infinite host. As the inclusion approaches the thin-section surface, the inclusion pressure gets smaller than Pexp. This is now explained at the beginning of the new section of “Inclusion pressure modification due to proximity to thin-section surface”.

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.

We have restructured the whole manuscript and clearly separate the results of the presented two models. Now this section directly continue from the visco-elasto-plastic 1D model so there should be no confusion on which model we refer to. We also added a short paragraph before it to clarify the simulation setup.

Line 183. “This diagram may assist petrological investigations because  $D_e$  and  $C^*$  can be evaluated based on experimental rock deformation data for different minerals...” Please discuss how  $D_e$  and  $C^*$  can be evaluated based on experimental data. Which data is available?

We have added new sentences in the following text to clarify it, e.g. using flow law to evaluate  $D_e$  and using microhardness test data to evaluate  $C^*$ .

Line 196. Awkward phrase: “...and  $D_e$  is located above the plastic onset...” Please reformulate.

Done. We have changed it to  $D_e$  above one to describe the plastic dominant regime.

Section 3.2. Describe problem setup, boundary and initial conditions. Given that you

C7

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?

This is a valid point. We have restructured the whole manuscript to split the two models. Also we have added new text here to elaborate the model setup. This we have clarified in new section 4.2 (place has been changed due to restructuring).

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.

This has been pointed out before by the reviewer and we completely agree. We have checked the word “relaxation” in the entire manuscript to carefully split it with “release” due to different rheology.

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

As this is the results part, we do not discuss any implications for sample preparation. But we did follow the reviewer’s suggestion by adding new text in Section 5.1 (Discussion) about sample preparation and inclusion picking procedure to avoid pressure release due to proximity to thin section surface.

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 microfracture later.

Done.

Line 227. “The flow law of garnet from Wang and Ji (1999) is applied”. Please describe

C8

briefly this law.

This is a good point that we have previously missed. Now a new equation has been added for Wang and Ji's flow law with numbers. (Eq. 21)

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 visco-plastic flow instead of plastic yield.

Done. We thank the reviewer for pointing that out.

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

This point has been raised by the reviewer before and we agree. We have adjusted the terminology used here. We now refer C as the plastic yield strength. We have checked the entire manuscript to change cohesion into yield strength.

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) prefractures/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

C9

Griffith or Mohr-Coulomb, where cohesion and tensile strength play major role. Solutions for plasticity onset and failure pattern in elastoplastic and visco-plastic rocks with these failure criteria are available in the literature. They would give other estimations for pressure necessary to induce fractures.

We agree with the reviewer on this point. Here, we have deleted the words related to cracks. The point is that we may still have localized stress due to the following reason stated above, hence localized dislocation around a non-spherical, anisotropic inclusion. Therefore, a statement on localized plastic yield around natural mineral inclusion is valid, just that we do not involve cracks in our discussion.

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.

We added "first" and "then" before the two sentences for the two models to separate them.

"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 re-derived after original paper by Seo and Mura [1979].

We have rephrased this sentence so that it is clear that the solution was not original. The derivations were moved to the appendix.

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

This is a helpful comment. We have added some text in "Conclusions" about the petrological implications of our model and summarize what we have found about the determination of entrapment pressure of quartz inclusion and the application of garnet overstepping model. In the end, we again thank the reviewer for all the detailed comments that greatly improved the quality and technical correctness of the manuscript.

C10

