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COMMENT

Comment on “Spatial changes in inclusion band spacing as an indicator of temporal changes in slow slip and tremor recurrence intervals” by Nishiyama et al.

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Abstract

A recent paper by Nishiyama et al. (Earth, Planets, and Space 73:126) examined syntectonic quartz veins to constrain temporal variations in the recurrence intervals between slow slip and tremor events. The authors claim that by examining the liquid-volume fraction of syntectonic fluid inclusions in the veins, that they can accurately reconstruct pore-fluid pressures (and variations therein) that were operative during faulting at ~ 15 km depth in an exhumed subduction melange. From these observations, the authors infer that large (from lithostatic to hydrostatic) decreases in pore-fluid pressure occurred during faulting, and that these variations drove increases in supersaturation and rapid quartz precipitation over time scales consistent with the repeat times of seismologically observed slow slip and tremor events. Here, I show that Nishiyama et al.'s analysis neglects reasonable uncertainties in pore-fluid pressure reconstruction. When those uncertainties are included, the Nishiyama et al.'s results become ambiguous as to whether any variation in pore-fluid pressure during vein formation occurred at all, negating the validity of many of the subsequent conclusions.

Keyword: Quartz, Fluid inclusions, Pore-fluid pressure, Slow slip and tremor, Low-frequency earthquakes, Slickenfibers

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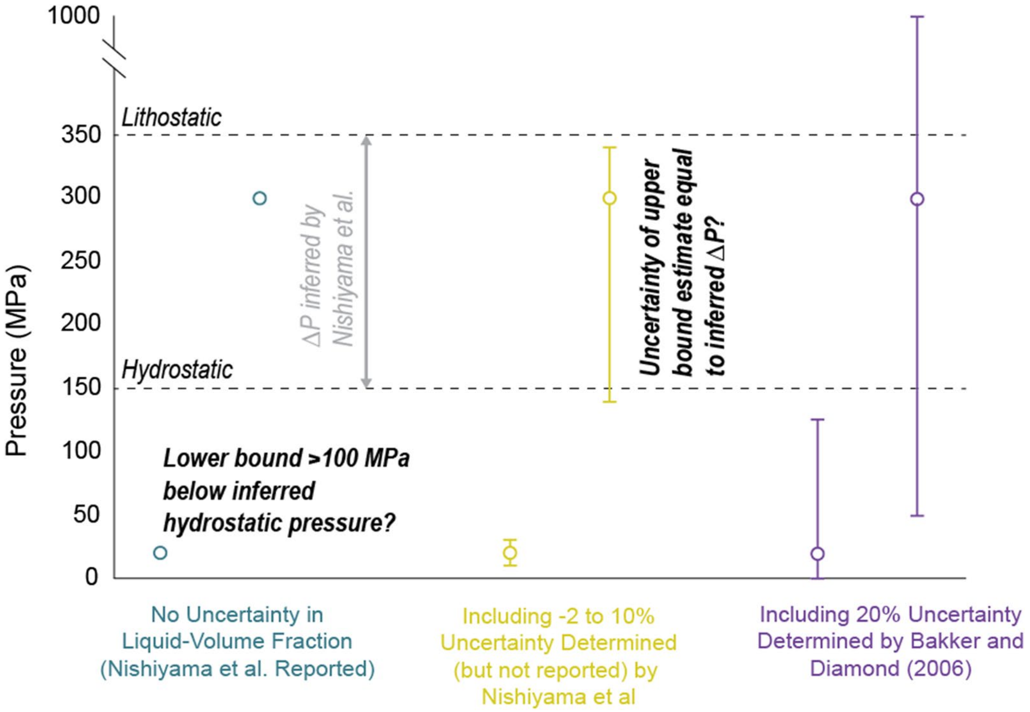
Graphical Abstract



Main

A recent paper (Nishiyama et al. 2021) published in *Earth, Planets, and Space* examined syntectonic quartz veins in an exhumed subduction melange to constrain temporal variations in the recurrence intervals between slow slip and tremor events. I read this paper with great interest, and I applaud the authors for taking a novel approach to a problem of great relevance to our community. Unfortunately, there are several major issues with the paper that cast significant doubt on its conclusions.

Nishiyama et al.'s results depend on estimating variations in the density of liquid water at the time of vein formation (from which variations in pore-fluid pressure may be extrapolated if precipitation temperature was constant and known independently). This is typically accomplished by heating two-phase, liquid–vapor inclusions until they homogenize using a heating and cooling stage. Nishiyama et al. state that this was not possible given the small size of the inclusions in their samples, and they instead opted for an alternative approach utilizing 2D-optical estimation of the liquid-volume fraction in individual inclusions. Critically, the uncertainty in liquid-volume fraction estimation was neglected in Nishiyama et al.'s analysis.

Equation 1 in Nishiyama et al. describes the theoretical relationship between the volume fraction of the liquid phase in individual inclusions at room temperature (V_r = [volume of inclusion—volume of vapor bubble]/volume of inclusion) and the density of the vein-forming fluid at the time of mineralization (ρ_h). Given that they assume fluid density at room temperature (ρ_r) and the liquid-volume fraction at the time of mineralization (V_h) are equal to 1, however, Eq. 1 can be written as

$$\rho_h = V_r$$

Thus, uncertainty associated with estimation of the liquid-volume fraction at room temperature in each inclusion is directly applicable to the estimated liquid density at the time of mineralization. Nishiyama et al., however, provide no estimate of the magnitude of this uncertainty, nor do they include any such consideration in subsequent calculations. A previous systematic study of error in 2D liquid-volume fraction estimates indicates that relative uncertainties of ~ 20% should be employed (Bakker and Diamond 2006). I raised this issue directly with Nishiyama et al. after reading their paper, and they indicated that unpublished, internal testing yielded uncertainties of a slightly smaller magnitude, specifically from − 2 to 10% relative to the estimated value in their samples. It is worth noting that Nishiyama et al.'s internal investigation of uncertainty in liquid-volume fraction relied upon comparing optically estimated values with traditionally measured homogenization temperatures (which they state in their paper could not be accomplished) but with a limited data set ($n = 10$). It is similarly noteworthy that Nishiyama et al. present homogenization temperature data for these same samples in a separate publication (wherein they conclude that the data preclude vein formation under a constant temperature; Nishiyama et al. 2020; more below).

For the sake of argument, I will adopt the uncertainty estimates derived from Nishiyama et al.'s unpublished analysis. Nishiyama et al.'s Fig. 4j, for example, shows minimum and maximum fluid density estimates of 0.91 and 0.68 g·cm^{-3}, which at an assumed precipitation temperature of 330 °C correspond to pore-fluid pressures of ~ 300 and 20 MPa, respectively (Wagner and Pruß 2002). Nishiyama et al. would have us consider these estimates at face value as defining a range of *real* pressure variations that occurred during faulting and vein formation, and they infer the data to record slip-induced decreases in pore-fluid pressure between from lithostatic to hydrostatic conditions ($\Delta P \cong 235$ MPa at 15 km depth). This estimate, however, does not include the uncertainty that Nishiyama et al. determined but did not report. Inclusion of that uncertainty yields fluid density estimates of ~ 0.82 to 0.93 and ~ 0.62 to 0.69 g·cm^{-3} for the upper and lower bound fluid densities, respectively. Assuming a constant precipitation temperature of 330 °C (questionable given the conclusions of Nishiyama et al. 2020), the inferred densities correspond to pore-fluid pressures of ~ 140 to 340 MPa and ~ 10 to 30 MPa for the upper and lower bound pressures, respectively (Fig. 1). The uncertainties are larger still if we adopt the %20 relative error proposed by the more systematic analysis of Bakker and Diamond (2006). It is noteworthy (and suspicious) that the lower-bound pressure Nishiyama et al. infer is more than 100 MPa less than the hydrostatic

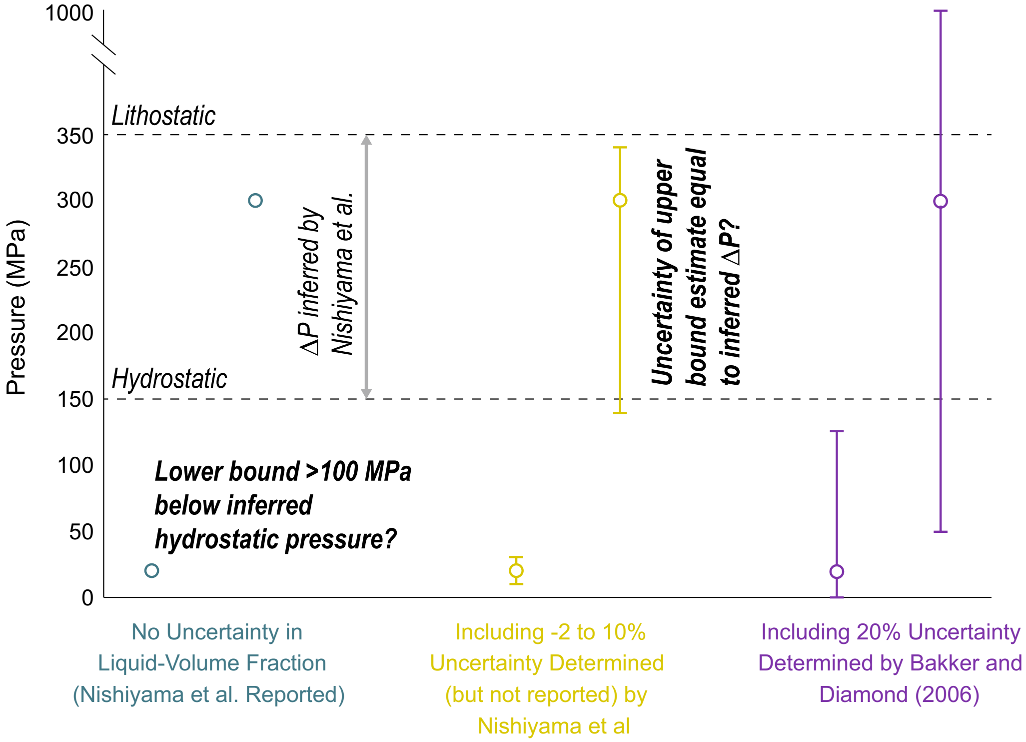


Fig. 1 Plot showing estimated pore-fluid pressures from Nishiyama et al.'s Fig. 4j without uncertainty as presented by Nishiyama et al. (left, green), with associated uncertainties determined (but not reported) by Nishiyama et al. (center, yellow), and with associated uncertainties suggested in a systematic analysis of liquid-volume fraction estimates by Bakker and Diamond (2006) (right, purple). Gray line shows the slip-induced decrease in pore-fluid pressure (ΔP) inferred by Nishiyama et al. based on the raw data. Note the discontinuous vertical axis above pressures of 350 MPa. When reasonable uncertainties in the estimated liquid-volume fractions are considered, the upper bound pressure estimate (which is inferred by Nishiyama et al. to record vein formation under lithostatic conditions) is indistinguishable from their estimate of the hydrostatic pressure at 15 km depth. Thus, when combined with an assumed minimum pressure consistent with hydrostatic conditions (as Nishiyama et al. argue), an interpretation of $\Delta P \approx 0$ is permissible

value of ~ 150 MPa they appear to adopt as a minimum in subsequent quartz growth rate estimations. More questionable still is the fact that the upper bound pressure estimate, which Nishiyama et al. infer to record precipitation under lithostatic pore-fluid pressures at 15 km depth, is indistinguishable from the hydrostatic estimate at that depth when the uncertainty is included. These eccentricities illustrate what has long been known in the fluid inclusion community: attempting to reconstruct temperature and pressure conditions on the basis of *individual* fluid inclusions generally produces dubious results.

In closing, Nishiyama et al. argue that Makemine vein samples record pore-fluid pressure decreases from lithostatic to hydrostatic values during faulting ($\Delta P \cong 235$ MPa). Even a generous interpretation of the uncertainty associated with their measurement technique, however, indicates the upper-bound pressure estimate spans the difference between lithostatic and hydrostatic conditions at the inferred depth. Thus, if we accept an approximate hydrostatic pressure as a reasonable system minimum (as Nishiyama et al. apparently did when estimating slip-induced changes in quartz solubility) a near zero decrease in pore-fluid pressure must be considered equally plausible. This implies that the 0.4–4 year fracture sealing time that Nishiyama et al. calculate, which is argued to reflect the recurrence interval of slow slip and tremor events that the veins are inferred to record, may just as reasonably be between a few decades and several thousand years (see their Fig. 5). These longer estimates are fundamentally incompatible with seismological observation of slow slip and tremor, contrary to one of Nishiyama et al.'s central conclusions. I make no argument here regarding which estimate is more likely. Rather, I argue that the issues outlined above raise significant questions as to the validity of Nishiyama et al.'s results, and any subsequent inferences on the systematics of slow slip and tremor events.

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Declarations

Competing interests
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