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Discussion

Reply to the comments by Domingo Aerden on "Reference frame, angular momentum, and porphyroblast rotation"

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We address Aerden's points in the order that they are raised in the discussion.

1. Does the application of continuum mechanics to rocks ignore the fact that rocks are made of mineral grains of contrasting mechanical properties?

Continuum concepts are routinely used in material sciences of solid and fluid materials. No one would probably question the validity of 'the density of granite and its variation within a granite body'. In fact one can consider the density (ρ) as a field variable that can be expressed as a function of spatial coordinates [$\rho(x,y,z)$] within the rock body. This practice is a continuum approach. Why this approach makes perfect sense even though the granite is made of mineral grains of different densities and void pore spaces has been explained in numerous textbooks (e.g. Batchelor, 1967, fig. 1.2.1, p. 5; Means, 1976, p. 4; Ranalli, 1987, fig. 1.1, p. 6) and papers (cf. Paterson and Weiss, 1961; Lister and Williams, 1983; Twiss et al., 1993). The key lies in the use of an average over a 'critical volume' to represent the point-quantity of a physical variable. For instance, the density of granite at a point is the average density over a finite critical volume that contains that point (e.g. Batchelor, 1967, fig. 1.2.1, p. 5; Means, 1976, p. 4; Ranalli, 1987, fig. 1.1, p. 6). The size of the critical volume depends on the heterogeneity and other factors of the material as well as the question addressed. All other continuum concepts such as stress, strain, strain rate, and vorticity are based on averaging over a critical volume as well. For example, in a flowing quartzite, the vorticity of the flow at a point does not mean the vorticity of a single grain at that point, but the vorticity averaged over the many grains making up the critical volume, centered on the point. We measure hundreds of quartz grains to characterize the *c*-axis fabric in a quartzite sample. Hundreds of grains here constitute the critical volume. Rocks are heterogeneous at all scales and in kinematic studies we are mindful of the significance of deformation path partitioning in the definition and characterization of a continuum. Without the above understanding of the continuum approach, the simple-minded criticism of Aerden that the continuum approach "simplifies rocks to perfectly homogeneous and isotropic media …" is useless.

2. Is the non-rotation interpretation of porphyroblasts confirmed by Takeda (2001)?

Aerden claims that "the feasibility of" the non-rotation interpretation of porphyroblast inclusion trails "has been confirmed by numerical modeling of strain partitioning in heterogeneous media (Takeda, 2001)…". This is not true.

Takeda (2001) investigates the partitioning of vorticity in an ideal bi-phase material. Among other assumptions, the author Takeda (2001, pp. 1319, 1324) clearly stated that the two constituent phases are mixed uniformly, and each phase is *a continuum in the usual sense*.

First, there is no problem to apply the continuum

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approach to such a bi-phase material, but we find it hard to understand how both phases can themselves be assumed continuous media at the same time. Second, Takeda's bi-phase continuum cannot be used to represent porphyroblast-bearing metamorphic rocks. Porphyroblasts in metamorphic rocks do not form a continuous phase because they are dispersed in the matrix and, above all, their grain size is commonly at least an order of magnitude bigger than that of the matrix phase. It makes far more sense to treat porphyroblasts as mechanical 'inclusions' as done by Jeffery (1922) for rigid inclusions and by Eshelby (1957, 1959) for deformable inclusions. As pointed out in Jiang (2001), a garnet porphyroblast typically occupies a volume that would contain ∼1000 matrix grains, making it justifiable to use the matrix vorticity to represent the angular velocity of a spherical garnet. Jeffery's (1922) theory has been verified by countless experiments since Taylor (1923).

Third, how should one understand the concept of 'the vorticity of a phase' in a bi-phase continuum such as Takeda's? In the event, the phase constitutes a continuum (certainly not the case for porphyroblasts), the vorticity in that phase, *in the usual continuum sense*, means the vorticity averaged over a critical volume containing many grains of that phase. It is a mistake to regard 'the vorticity of a phase' as the vorticity of each and every grain of that phase. In a bi-phase continuum in Takeda's sense with one phase being rigid, Takeda shows that 'the vorticity of the rigid phase' can be significantly lower than the bulk vorticity. Aerden mistakes this as meaning that all rigid-phase grains have zero vorticity. He therefore regards Takeda's work as confirming 'the feasibility' of the non-rotation interpretation of garnet inclusion trails.

3. Stating the porphyroblast controversy

Both Schmidt's (1918) model and Schoneveld's (1979) analog capture the main features of the geometrical evolution of a snowball garnet. The key point is that relative rotation between strings and rings produces the inclusion trail geometry. How fast the rings rotate with respect to the strings is irrelevant. The Schmidt–Schoneveld model is a purely geometrical and kinematic model, having nothing to do with the mechanics of the process. In other words, the model demonstrates the consequence of, not the causes of, the relative rotation between the garnet and the foliation. Aerden brings up the irrelevant points of mechanical anisotropy, deformation mechanisms in this context and claims that it was the consideration of these factors that led to the 'non-rotation' reinterpretation. We regard the strain-partitioning model as purely a geometrical model as well which is based on no mechanical principles. On the contrary, it is a model that is fundamentally at odds with mechanical principles such as the balance of angular momentum.

4. Arguments against a 'non-rotation' origin of spirals

The first part of our original paper serves to demonstrate that the inclusion trails in ellipsoidal porphyroblasts are not interpretable because our numerical experiments (both for simple and general shear; see figs. 1 and 2 of Jiang and Williams, 2004) clearly show that the inclusion trails do not generally have monoclinic symmetry and, even for simple deformation histories, each porphyroblast is an initial value problem. To explain the inclusion trail of an ellipsoidal inclusion, one will have to know the initial orientation and shape of the inclusion to a great precision (because a slight variation in the initial state can lead to drastically different final geometries) as well as the details of the deformation history. Our conclusion itself explains why we did not study inclusion trails in elongate porphyroblasts in real rocks—it makes no sense to try to interpret these inclusion trails kinematically as one can never define the initial conditions for each and every ellipsoidal inclusion precisely. Aerden is right to state that we ignored the work he cited, we did so because we find the data to be misinterpreted in this work based on the dogma that garnets do not ever rotate.

Bell et al. (1992) and Johnson (1993b) have provided their explanation how continuous spirals can be formed without porphyroblast rotation with respect to Earth. Williams and Jiang (1999) refuted their explanation on two grounds. First, although the original strain partitioning model (Bell and Johnson, 1989) can be fine-tuned to produce smoothly looking spirals in the section both perpendicular to the spiral axis *and* passing through the center of the garnet, the inclusion patterns on all other sections predicted by the strain partitioning model are completely different from those predicted by the Schmidt–Schoneveld model. All available snowball garnet data including those presented by Johnson (1993a) are compatible with the Schmidt–Schoneveld model but not the strain-partitioning model (Williams and Jiang, 1999). Second, continuously rotating a foliation around an irrotational-to-earth garnet can produce snowball garnet, but requires a deformation path that is unlikely to occur in metamorphic rocks. This point was further explained in Jiang and Williams (2004). We concluded that whether the foliation is a single foliation or represents successive segments joined so remarkably smoothly that they resemble a single foliation, the deformation path must be one in which particles move in a circular path (Jiang and Williams, 2004, p. 2218).

Aerden apparently missed these key points of our argument, but instead repeated the irrelevant single vs multiple-foliation argument of Bell et al. (1992) and Johnson (1993b). To produce a group of snowball garnets such as fig. 6d of Jiang and Williams (2004) (adapted from Rosenfeld, 1968), the deformation path would have to be that of a vortex if the garnets are to be irrotational to the earth.

We maintain that snowball garnets can only occur in shear zone environments. Multiple crenulation cleavage formation can potentially produce spiral-like inclusion trails only on the section perpendicular to the crenulation axis and passing through the center of the garnet (Williams and Jiang, 1999); it cannot produce the 3D geometry observed in snowball garnets. In a shear zone environment, the foliation overgrown by the garnet could be any foliation that can develop in a shear zone or it could be an inherited foliation.

The statement by Aerden "Obviously, the net torque acting on an object is proportional to its angular velocity in a viscous medium" is obviously wrong. The torque is proportional to the acceleration in an angular velocity, not the angular velocity itself (eq. (6) of Jiang and Williams, 2004). We did not calculate the torque for a spherical particle surrounded by a homogeneous viscous fluid as Aerden thought; we calculated the angular speed at which the inclusion must rotate to eliminate the torque acting on it. We also stressed that the law of balance of angular momentum is universal—it must be obeyed by motion of any object. Therefore, our incomplete knowledge of rock rheology does not provide a reason for garnet motion not to be subjected to the law. There are abundant experimental (Kohlstedt et al., 1995) and theoretical evidence (Poirier, 1985) that rocks under metamorphic conditions deform like a power-law fluid with crystal plasticity as the deformation mechanism. Rocks may exhibit mixed rheology such as viscoelastic, elastoplastic, and so on under different deformation conditions, but we are unable to imagine any rheological behavior whereby mechanical laws are obeyed, and at the same time rigid inclusions in the deforming body remain generally irrotational to earth.

5. Evidence for a 'non-rotation' origin of spirals

Numerous papers have been published and more may continue to be published showing 'evidence' that rigid porphyroblasts do not rotate with respect to Earth. We admit that we have lost track of, and ignored, some of these papers. We have presented our arguments in Williams and Jiang (1999), Jiang (2001) and Jiang and Williams (2004). In the spirit of our original paper (Jiang and Williams, 2004, p. 2212): "We do not repeat arguments already presented in the literature". We shall add only a few remarks here.

Aerden missed the three key points of our paper, namely (1) the strain-partitioning model cannot produce the 3D geometry of the snowball inclusion (see Williams and Jiang, 1999, for details), (2) the required deformation path for snowball garnets to form irrotational to earth is unlikely to occur in nature, and (3) the notion of irrotational inclusions in a deforming matrix violates the principle of balance of angular momentum. Instead he listed 'evidence' supporting his belief that porphyroblasts do not rotate. We lack the imagination to agree with Aerden's interpretation of his fig. 4b–d in terms of straight-line segments and fail to see the 'subtle, yet distinctly orthogonal patterns' (fig. 4b–d). Further, we find it far-fetched to claim that the orthogonal patterns are either parallel or perpendicular to the earth's surface (fig. 4f). Some as interpreted are close, but the initial choice of straight-line segments is completely subjective. Further, the 3D geometry of these inclusions should be examined. In fig. 2 of Aerden, the garnet clearly has been advected for tens, if not hundreds, of kilometers from initial burial to eventual exhumation in an interpretive corner flow environment, yet the garnet crystal is believed to have never changed its orientation despite the extreme deformation of the surrounding rock. This proposal is an excellent example to show how absurd a conclusion that the garnet-never-rotates-relative-to-earth dogma can lead to.

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