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THE CLASSIC HIGH-*T* – LOW-*P* METAMORPHISM OF WEST-CENTRAL MAINE: IS IT POST-TECTONIC OR SYNTECTONIC? EVIDENCE FROM PORPHYROBLAST – MATRIX RELATIONS: REPLY

GARY S. SOLAR* AND MICHAEL BROWN§

Laboratory for Crustal Petrology, Department of Geology, University of Maryland, College Park, Maryland 20742, U.S.A.

INTRODUCTION

We thank the Editor for the opportunity to discuss further the evidence bearing on the timing of mineral growth in metamorphic rocks of the Rangeley stratigraphic sequence in the type area of Maine. In his discussion of our paper (hereafter S & B), Guidotti (hereafter G) has commented critically on various aspects of our work, but his criticism is based largely on misrepresentations and unfounded assertions. In this reply, we elucidate issues that in our opinion are misrepresented in G, and we refute the unfounded assertions. We do not follow a "model-driven" approach to research, which is clear from the layout of evidence and argument in both S & B and this reply.

G presents a number of interpretations that have led him and others to allege that regional metamorphism in west-central Maine was post-tectonic, a contention that apparently carries with it the presumption that growth of metamorphic minerals was "static" (see also De Yoreo *et al*. 1989a, p. 174). This posit contradicts the conclusion of S & B, based on a detailed study of the microstructural evidence in the rocks, particularly as recorded by the porphyroblast–matrix relations, that mineral growth was syntectonic. Unfortunately, G has focused his discussion largely on the porphyroblasts, essentially in isolation of their matrix, using illustrations from field outcrops only. In comparison, the original paper by S & B reported field outcrop and microstructural information from both porphyroblast and matrix minerals, and documented supporting evidence from thin sections oriented with respect to rock fabrics. S & B used these data to develop a model for the metamorphism and its relation to the deformation.

Another significant difference in our views concerns the importance of heat advected with crustally derived granitic magma. G believes that the regional metamorphism was driven by advection of heat associated with granite plutons, whereas in our view the geological evidence indicates that heat advected with crustally derived granitic magma is only a second-order phenomenon clearly spatially related to individual plutons (Brown & Solar 1999). In our opinion, the first-order phenomenon is manifestly the thermal anomaly associated with the Acadian orogeny that enabled the crustal rocks to melt in the first place.

The controversy occurs because S & B questioned the dogma that plutons are presumed post-tectonic because emplacement is inferred to have occurred after the stratigraphic sequence was deformed into map-scale folds. When linked to the unfounded assertion that the regional metamorphism was driven by advected heat from plutons, this presumption forces the erroneous conclusion that growth of regional metamorphic minerals must have been post-tectonic, despite microstructures that indicate otherwise. As we stated in the introduction to our paper (S & B, p. 312), how the relationship between deformation and granite emplacement is perceived is dependent on the level of erosion and the scale of observation, because discordant contacts locally at outcrop scale or regionally at map scale do not preclude syntectonic ascent and emplacement when viewed in three dimensions at the crustal scale (Brown & Solar 1999). Furthermore, we need to move past a sequence approach to deformation, metamorphism and granite ascent and emplacement now that we are aware of the progressive nature of these processes and the coupling between them during orogeny. The same unblinkered approach is necessary for the correct interpretation of syntectonic veins at the outcrop scale, which may be discordant to the rock fabric, but which nonetheless predate the final increments of strain during an episode of progressive deformation.

In contrast to the view set forth in G, we argue that metamorphism accompanied folding of the succession and the development of a tectonite fabric as the orogen

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[*] *Present address*: Department of Earth Sciences, SUNY College at Buffalo, 1300 Elmwood Avenue, Buffalo, N.Y. 14222, U.S.A. *E-mail address*: solargs@bscmail.buffalostate.edu

[§] *E-mail address*: mbrown@geol.umd.edu was thickened. Contact metamorphism around plutons in this area is limited to discrete aureoles, and is not regionally developed as argued by G and others ("regional contact metamorphism", *e.g.,* De Yoreo *et al.* 1989a, Guidotti 1989b). In truth, the metamorphism in west-central Maine is typical of that in thickened orogens, where an enhanced thermal gradient is augmented by the intracrustal ascent and emplacement of granitic magma during deformation (Brown & Solar 1999). In this context, interpretations regarding the timing of mineral growth must take into account several additional factors, such as the partitioning of deformation at all scales, the effects of composition and rheology of the protolith on fabric development, and the episodic and diachronous nature of deformation and mineral growth (S & B, p. 314).

In response to the allegation by G, as far as we can determine by reading all the pertinent literature again, S & B did not misreport the work of anyone, and we regret that G perceives our paper as having "... misrepresented much of (his) work and that of others." Also, the false charge made by G that we have been casual in our work is unfounded. In 1994, Guidotti introduced us to the geology of west-central Maine, and we have enjoyed numerous discussions with him during the past five years, including those relating to his membership of Solar's Ph.D. committee (Solar 1999). Contrary to what might be expected, we find the scepticism of our interpretations expressed in G and in our discussions with him to be a constant stimulation that motivates us to test our hypotheses and models to check if they may be flawed (*e.g.,* Solar *et al.* 1998). In our reply to G, we first address general issues, and then we provide additional comments on the conclusions of S & B to clarify the evidence that underpins our interpretation.

GENERAL ISSUES RAISED IN THE DISCUSSION BY GUIDOTTI

The classic high-T – low-P metamorphism of west-central Maine

Classic areas or papers achieve that standing because of their exemplary status or well-above-average number of citations. On both counts, the high-*T* – low-*P* metamorphism of Maine and the papers by Lux *et al.* (1986) and De Yoreo *et al.* (1989a, b, 1991) are classics. Indeed, this area in west-central Maine became the model for metamorphism driven by a plutonic source, essentially displacing the Abukuma belt in Japan (Shido 1958, Miyashiro 1958, 1961), because Lux *et al.* (1986) used it as their example, together with the results of 1-D thermal modeling, to develop an alternative to rifting for worldwide high-*T* – low-*P* metamorphism, as stated in De Yoreo *et al.* (1989a, p. 171). The model of "regional contact metamorphism" was developed in part because the *P–T* conditions recorded by mineral assemblages in high-*T* – low-*P* metamorphic belts could not be reproduced using the 1-D thermal models widely available in the 1980s. However, the more complex thermal-mechanical numerical modeling of the dynamic evolution of orogens developed during the 1990s has shown that advection of heat with migrating magma is not required to generate the thermal conditions necessary for low-*P* metamorphism (*e.g.,* Jamieson *et al.* 1998, Huerta *et al.* 1996, 1998, 1999). Ironically, the high-grade metamorphic zones of the Abukuma belt are now recognized as the result of contact metamorphism superimposed on a regional high-*T* metamorphism that preceded pluton emplacement (Tagiri *et al.* 1993) and may have been caused by ridge subduction (Hiroi *et al.* 1998, Brown 1998).

Mapping

We began our study in 1994 with mapping by Solar to provide the foundation on which to build an understanding of the structural development and thermal evolution of west-central Maine. The work reported by S & B represents part of the Ph.D. dissertation of Solar (1999) that was based on approximately 12 months of field mapping during a five-year period. We were particularly interested in the spatial distribution, overprinting relations and regional variation in orientation of rock fabrics defined by metamorphic minerals, and this work is presented in detail elsewhere (Solar & Brown 2000). In this context, the relation of these fabrics to the growth of porphyroblast minerals may be interpreted with confidence, as demonstrated by S & B, and metamorphic zones can be mapped accurately. The lower sillimanite zone shown in Figure 4 of S & B is based upon previous work by G (referenced in G), and we do not understand the argument that if there is a wider metamorphic zone found elsewhere, it should be as wide everywhere. With respect to the migmatites, the migmatite-in isograd, the "margin of migmatite domain" drawn in Figure 4 of S & B, represents the first appearance of leucosome in outcrop and is shown as mapped without regard for preconceptions about the "usual" map-width of metamorphic zones.

It is claimed in G that there is " ... essentially no outcrop..." in the area of the Weld anatectic domain (WAD on Fig. 2 in S & B), based on the report by Pressley (1997) that she found only two outcrops [of granite] in the area of the WAD, which had previously been mapped as granite by Pankiwskyj (1978). In fact, Pressley (1997) did not comment in detail on the WAD, which was outside of the area of study for her thesis work. Mapping by Solar (1999) shows thirty-eight outcrops in the WAD that are well spread over the area, and are dense in some places, particularly in the higher elevations in the town of Weld, Maine (*e.g.,* Gammon Ridge), and in extensive road-cuts to the west of Dixfield town center (along US route 2 and state route 108). Thus, the assertion by G that "… there is virtually no information on the metamorphic history [of the

WAD] whatsoever…" is incorrect because it does not consider new information as a result of our mapping in that portion of the area. This information was available to Guidotti prior to submission of his discussion of our paper, since he was a member of Solar's Ph.D. committee (Solar 1999, title page), so we must assume he chose to ignore the facts. The majority of these outcrops are migmatite, but some include granite. We agree that the area within the WAD mapped as "inhomogeneous migmatite" in Figure 2 of S & B has the lowest abundance of exposed rocks in our area of study, but it is not without outcrop, particularly in the southern and eastern part (see Solar 1999, volume II, p. 97). The distribution of outcrop in the WAD is enough on which to base an interpretation, and our preferred interpretation is that this area is underlain by migmatite and not granite as previously mapped.

Periods of metamorphism

An important part of the discussion by G concerns the complex metamorphic story he has concocted around alleged multiple discrete metamorphic episodes in Maine, *viz.* "M1", "M2" and "M3", which it is suggested we may not understand. For the record, we note that De Yoreo *et al.* (1989a, p. 174) wrote " ... studies have revealed a polymetamorphic history for the region ... M1 was syntectonic and resulted in widespread greenschist facies metamorphism ... the rocks involved in M2 are characterized by assemblages bearing staurolite, staurolite-andalusite, andalusite, and occasionally sillimanite." In that paper, "M2" is given equal weight with "M3", both being defined on the basis of porphyroblast phases without regard to their relations with matrix phases, and both metamorphisms are described as "static events". We did not realize that this last statement was no more than an unsupported assertion without "... detailed documentation ... with regard to textures and their implications for the relative timing of deformation and metamorphism" (G), because we read in De Yoreo *et al.* (1989a, p. 173-4) that their assertion was based upon " ... a number of recent reviews (Holdaway *et al*. 1982, Guidotti *et al.* 1983, Guidotti 1988, Holdaway *et al*. 1987)." [Readers should be aware that the reference in De Yoreo *et al.* to Guidotti (1988) should be to Guidotti (1989b), and the reference to Holdaway *et al.* (1987) should be to Holdaway *et al.* (1988)].

We emphasize that S & B did not use the term "static" in reference to the metamorphism associated with the Mooselookmeguntic pluton ("M3" in the terminology of G), as misstated by G, although we did suggest that some textures are "consistent" with contact metamorphism. In our opinion, the term "static" is an unfortunate choice to describe mineral growth in the solid state. That the metamorphism is presumed "static" is based upon the predication that "... porphyroblasts are random[ly oriented] ..." (*e.g.*, Guidotti 1968, 1970a, b), a hypothesis that has gone untested for decades and has become dogma as a result (*e.g.*, Guidotti 1989b). In his discussion of S & B, G concedes that some porphyroblasts of staurolite are not randomly oriented, since they follow a cleavage that is presumed "axial-planar" to m-scale folds, and that some pseudomorphs after andalusite are randomly oriented in the plane of foliation only (*i.e.*, they form a preferred planar fabric). This is the first instance in which G has made such statements, following the observations first made by us in our paper, and we are pleased that he now recognizes these fabrics. In contrast, there is nothing unfortunate about the choice of title for our paper, which was carefully selected deliberately to highlight an important issue in metamorphic petrology – the relation between growth of metamorphic minerals and deformation in an area that has become a classic.

Regarding the several aspects of "M3" that are alleged by G to be crucial for the discussion of our work, we respond as follows. The U–Pb age data (by Smith & Barreiro 1990) on metamorphic monazite in rocks in the area of Figure 2 in S & B show only that there are two groups of ages for monazite growth, which Smith & Barreiro (1990) interpreted to record two separate periods of metamorphism. We have interpreted the first group of data (Solar *et al.* 1998) to record the age of the syntectonic regional metamorphism at 405–399 ± 2 Ma, whereas the second group of data records metamorphism at 369–363 ± 2 Ma in samples from close to the contact with the Mooselookmeguntic pluton, which we presume to be the age of the "M3" metamorphism using the terminology in G. We agree that this contact metamorphism spatially associated with the Mooselookmeguntic pluton is superimposed upon the regional metamorphism, although we suspect that the "M2" and "M1" episodes of metamorphism of G are manifestations of the same regionally developed event. In this context, we note that contact metamorphism is also spatially associated with the Redington and Lexington plutons, which plutons have crystallization ages indistinguishable from the age of the syntectonic regional metamorphism (Solar *et al*. 1998). Thus, it is essential to separate the progressive regional metamorphism due to the Acadian orogeny, involving prograde and retrograde segments of a clockwise P–T path, from the local effects of multiple episodes of pluton-driven metamorphism.

We favor the use of petrogenetic grids (*cf.* Pattison & Tracy 1991), and we are sympathetic to using the bathograd approach (Carmichael 1978). Our application of these methods in west-central Maine indicates clearly, as we would expect from both common sense and modeling, that our area of study has been one of dynamic equilibrium in respect of depth during the period of the Acadian orogeny. In our experience with these rocks, andalusite cannot be discounted in the simple manner suggested in G (as noted by Carmichael 1978) because it is variably replaced in common with staurolite. This replacement event was not necessarily coeval or synchronous across the whole area, which suggests that metastability also may be variable. G claims that "...there is no evidence whatsoever of pseudomorph formation due to the M2 event," but we cannot evaluate this claim because he presents no data to support his statement. There is no geological or geophysical evidence to suggest that the Mooselookmeguntic pluton extends in the subsurface to the northeast as G contends (see Brown & Solar 1998b). Thus, his assertion that retrogression of "M2" porphyroblasts of staurolite and andalusite is due to "M3" becomes difficult to reconcile with the clear spatial limitation of the real effects of the Mooselookmeguntic contact metamorphism. If "M2" andalusite did not re-equilibrate during "M3", then how closely rocks approached chemical equilibrium during "M3" becomes a matter of opinion (and distance from the contact with the Mooselookmeguntic pluton).

We have re-read the papers and field guides referenced by G to respond to his unfounded claim that either we did not read them before or we did not understand their content. Reading these papers again has raised several issues that need to be addressed. The summary of the timing of metamorphic events given by G in his discussion of S & B contradicts information given in the work he cites in support. This is particularly true of the doctrine surrounding the "M2" and "M3" events. For example, Guidotti & Holdaway (1993) and Guidotti *et al.* (1996) show isograds purportedly related to the "M3" event wrapping around plutons that are different in age by *ca.* 35 m.y. according to the precise ages of Solar *et al.* (1998). In fact, the granite forming the main body of the Mooselookmeguntic pluton has a crystallization age of *ca.* 370 Ma (and a satellite body has an age of *ca*. 363 Ma according to the U–Pb monazite data of Smith & Barreiro 1990); in comparison, the Phillips pluton has a crystallization age of *ca.* 404 Ma (Solar *et al.* 1998). Unfortunately, the "M3" isograds of Guidotti & Holdaway (1993) and Guidotti *et al.* (1996) are defined by assemblages of metamorphic minerals spatially associated with both of these plutons. By the reasoning of G in his discussion of S & B, "M3" should be limited to the area surrounding the Mooselookmeguntic pluton, and the effects of "M3" should not extend as far from its contact as is shown by those field guides. G now seems to recognize this possibility, and he uses the discussion of our paper as an opportunity to correct his previous mistake, although he makes no explicit reference to the new ages reported by Solar *et al.* (1998; see also age data reported in Bradley *et al.* 1998). If we adopt the "M2" and "M3" usage of G, then "M2" is regional and "M3" is local to the younger Mooselookmeguntic pluton in the western part of Figures 2 and 4 of S & B, as shown by the pattern of metamorphic zones in their Figure 4. Of course, once this point is conceded, "M2" cannot be distinguished from "M1", and the whole story becomes much simpler, in line with the structural history. In our opinion, the thermal evolution is better described as a progressive regional metamorphism that developed synchronously with the Acadian progressive deformation. Superimposed on this regional metamorphism are the contact-metamorphic aureoles around granites of various ages, including the Redington pluton in the north (*ca*. 408 Ma, Solar *et al*. 1998), the Lexington pluton in the east (*ca*. 404 Ma, Solar *et al*. 1998), the Mooselookmeguntic pluton in the west (*ca*. 370 Ma, Solar *et al*. 1998), and the Sebago batholith to the south of our area of study (*ca*. 293 Ma, Tomascak *et al*. 1996).

With regard to the allegations made by G that "… for the northern half of the TAD … there is little or no control on either the number or ages of the metamorphic events …" and that the central-southern part of the area of Figure 2 in S & B is "poorly understood" in respect of the metamorphism, we presume that he again chose to ignore our paper reporting precise ages from this area (Solar *et al*. 1998); we interpret his statements to mean only that the metamorphism is poorly understood by him, and, as with the WAD rocks, he has chosen to ignore our new work. In fact, the southeastern half of our area of study is the focus of a paper concerning the petrogenesis of the migmatites (Solar & Brown 2001), information that was available to G as part of the Ph.D. dissertation of Solar (1999). We take this opportunity to emphasize that the major regional structures continue through the area of the migmatites, as evidenced by the correspondence between the fabric types in the migmatites and the projection of the regional structures from the non-migmatitic rocks across the migmatite-in isograd. Thus, stromatic migmatites with planar leucosomes exhibit flattening strain and define the suprasolidus extension of the high-strain zones (HSZ) of S & B, whereas inhomogeneous migmatites have irregular leucosomes oriented along the mineral lineation and exhibit constrictional strain consistent with occurrence in the low-strain zones (LSZ) of S & B (see also Solar & Brown 2000, 2001). The obsession expressed in G with the imagined effects of "M3" in the central-southern part of our area of study suggests that he may believe the migmatites are related to the emplacement of the Mooselookmeguntic pluton. However, we have shown elsewhere (Solar *et al*. 1998) that precise age data preclude such an interpretation, and common sense dictates that this belt of migmatites, which shows no spatial relation to the Mooselookmeguntic pluton and stretches into eastern New Hampshire (Chamberlain & Sonder 1990, Allen 1996), cannot be a local phenomenon related to a contact aureole.

Porphyroblasts and matrix fabrics

In the discussion of S & B, G makes unsubstantiated arguments about the spatial arrangement of porphyroblast minerals while ignoring the microstructure of the matrix phases. In contrast, using microstructural evidence from rocks at all metamorphic grades that was documented in S & B, we argued that the fabrics are penetrative and defined by the preferred orientation and shape of the matrix minerals. Further, the rock fabrics defined by the metamorphic minerals record the geometry of the finite strain ellipsoid. The microstructures we described are regionally developed, but vary systematically in relation to map-scale structure and proximity to plutons. This variation is to be expected in circumstances where the deformation is partitioned (Solar & Brown 2000) and the plutons supplied heat locally to produce discrete contact-aureoles. Porphyroblasts wrapped by the matrix foliation have trails of preferentially oriented mineral inclusions and exhibit pressure shadow tails, features that are agreed to be present according to the "new observations" (*sic*) by G, evidence that to us precludes post-tectonic growth of porphyroblast or matrix minerals. Indeed, these microstructures were clearly explained nearly forty years ago by Zwart (1962, p. 42) who wrote " ... where a schistosity curves around a porphyroblast, this must be the result of later flattening, so that deformation outlasts crystallization." Although microstructural relations between any individual porphyroblast and the matrix may suggest pre- or syntectonic growth of porphyroblast minerals (Zwart 1962, p. 41), or intertectonic growth in the sense of Passchier & Trouw (1996), growth of regionally developed porphyroblast minerals in our area of study was clearly followed by accumulation of additional penetrative strain. Unfortunately, the assertion by G that "… high strain of significance … would have occurred at high *T* and so would have annealed out quickly" cannot be substantiated, and is clearly in error since we see microstructures that record the effects of high-strain deformation in the rocks today. In our opinion, the interpretation of porphyroblast–matrix relations generally is a well-developed practice. The observations and petrofabric data documented in S & B unambiguously support the logical conclusion that the regionally distributed metamorphism in this classic area was syntectonic.

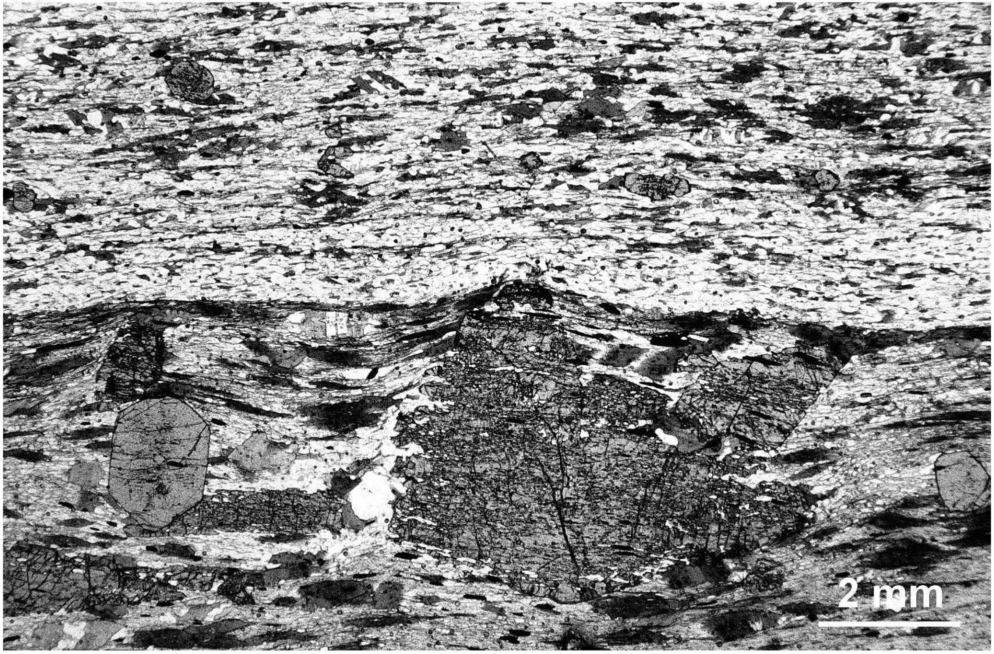
COMMENTS ON THE CONCLUSIONS IN S & B

High strain

A major point of debate in G concerns whether metasedimentary rocks in the area of Figures 2 and 4 of S & B record high strain. This was not a major focus of our paper, but is an important point considered elsewhere (Brown & Solar 1998a, b, 1999, Solar *et al.* 1998) and discussed in detail by Solar & Brown (2000). However, we did state (S & B, p. 316) that regional data relating the shape, orientation and intensity of the rock fabric as defined by the metamorphic minerals, the regional orientation of cm-scale compositional layers, and the shapes of folds and their hinge-line orientations, together suggest that "... all metasedimentary rocks ... are highly strained." The "sedimentary laminae" and "delicate cross beds" noted by G are not preserved or observed in pelite layers, only in the psammite layers, which have not developed Q and P domains. The psammite layers that form part of the sequence outcropping at Coos Canyon are not the "fairly pure quartzites" described in G, since they have ~15 vol.% biotite. It is local cross-bedding, preserved in the psammite layers (see G, Fig. 1a), that provides the only reliable way-up indicator. We are puzzled that G does not accept that some of the "delicate" sedimentary structures could have been erased by deformation. Structures that are interpreted by G as "graded bedding" are not "ubiquitous," but are locally preserved or formed (we cannot evaluate which is correct). It cannot be demonstrated in any layer that grain sizes reflect sedimentary sorting instead of recrystallization. Further, the assumption that the more pelitic portion of compositionally variable layers represents the top is dangerous in circumstances where metamorphic differentiation into Q and P domains demonstrably has occurred. The fold shown in Figure 2 of G shows a view that is perpendicular to lineation. Compare this view to Figures 1b and 3b in this reply. These figures are subparallel with respect to the fabric in the rocks. The apparent sedimentary structures in Figure 2 of G are elongate in the plane perpendicular to both the foliation and the view in the photograph. We find it difficult to interpret such structures without considering the 3-D geometry (*e.g.*, Figs. 1, 2 and 3 of this reply). Of critical importance to this debate are the penetrative grain-shape fabrics in these rocks (Figs. 1 and 2 of this reply), which cannot be sedimentary features even if the minerals defining the fabrics grew mimetically.

S & B stated that there are steep zones in which rocks "... show nearly complete transposition of structures... to record qualitatively higher strain …" that are separated by intervening zones where transposition is not as complete in zones which "... record qualitatively lower strain." In the steep zones, the dip of both mineral fabrics and compositional layers (alternating pelite–psammite layers) is subvertical, and planar structures have a narrow variation in attitude, in contrast to moderate dips and a larger variation in attitude of the planar structures in the intervening zones. S & B refer to these two types of zone as "higher-strain zones (HSZ)" and "lower-strain zones (LSZ)," respectively, based on the qualitative difference in apparent amount of strain recorded. Subsequently, Solar & Brown (2000) chose to refer to these two types of zone as "zones of apparent flattening strain" and "zones of apparent constrictional strain," respectively, referring to the correlation between the shape of the fabric ellipsoid and the tightness of folds in each zone. Thus, oblate fabrics (Fig. 1) and tight folds characterize the HSZ, whereas prolate fabrics (Fig. 2) and open folds characterize the LSZ. Because matrix mineral fabrics are of two types, apparent flattening and apparent constriction that record qualitatively higher and lower strain, they define two types of unit that can be

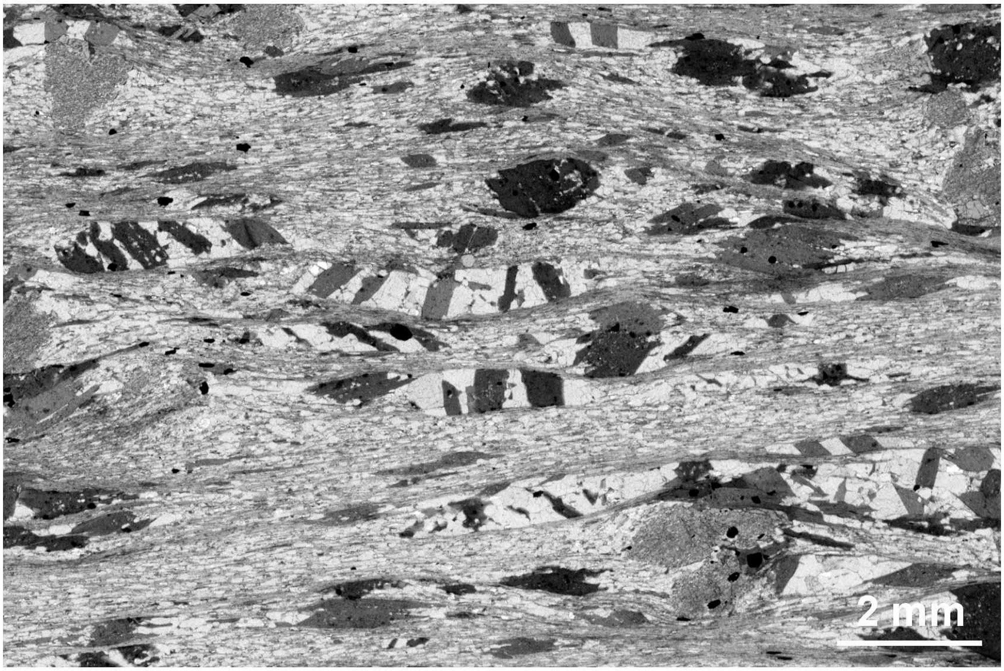
(a) lineation-parallel section



(b) lineation-perpendicular section

FIG. 1. Pair of transmitted plane-polarized light photomicrographs of thin sections cut orthogonally from a single oriented rock sample according to the grain-shape fabric as indicated. The sections are from a staurolite–garnet schist collected from the central HSZ (Perry Mountain Formation, Swift River, north of Coos Canyon, Byron, Maine). The strong blade-shape of micas is made apparent by comparing (a) and (b). Both views show the matrix fabric apparently flattened around the staurolite porphyroblast at bottom center, and both views show the staurolite to have inclusion trails that are not parallel to matrix foliation.

(a) lineation-parallel section



(b) lineation-perpendicular section

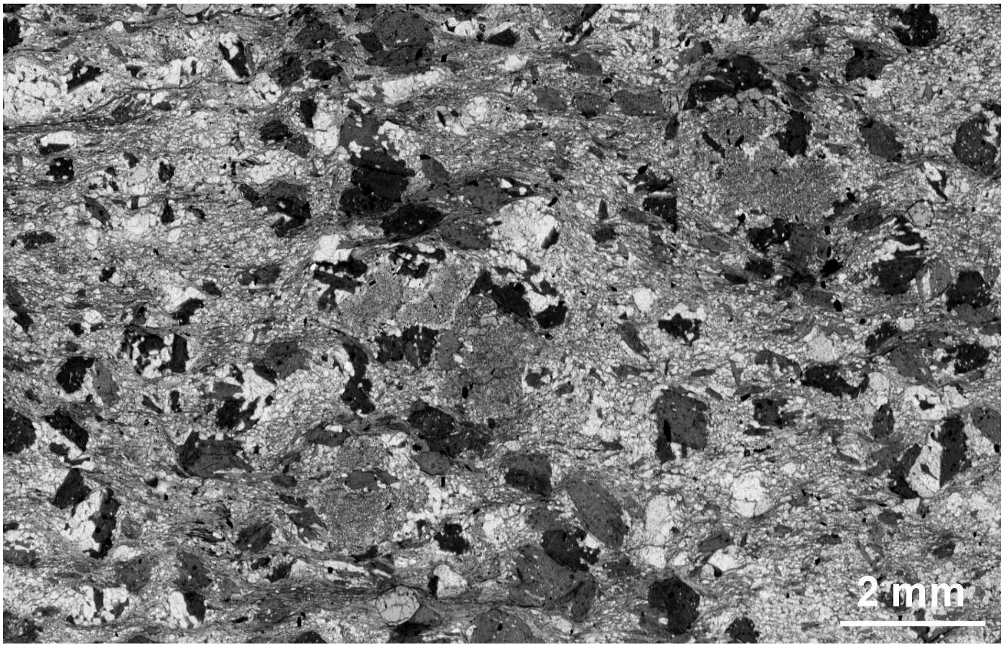
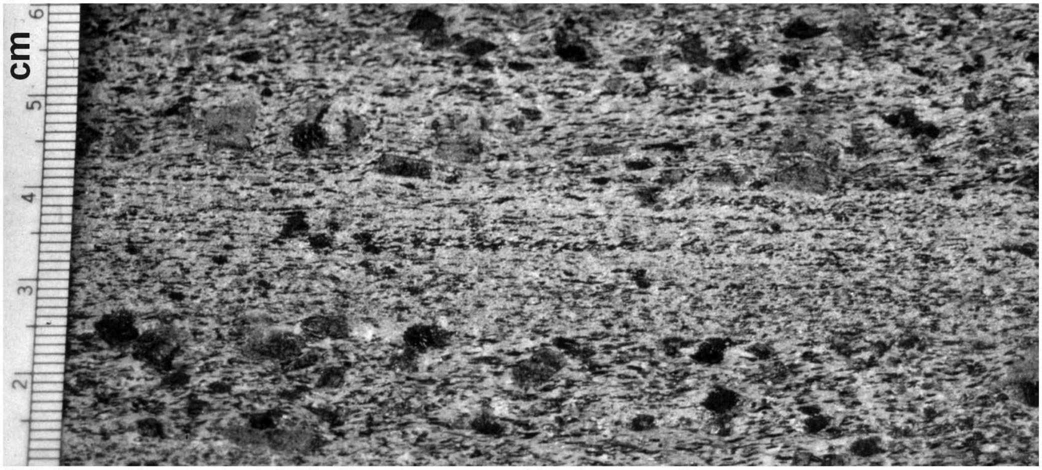
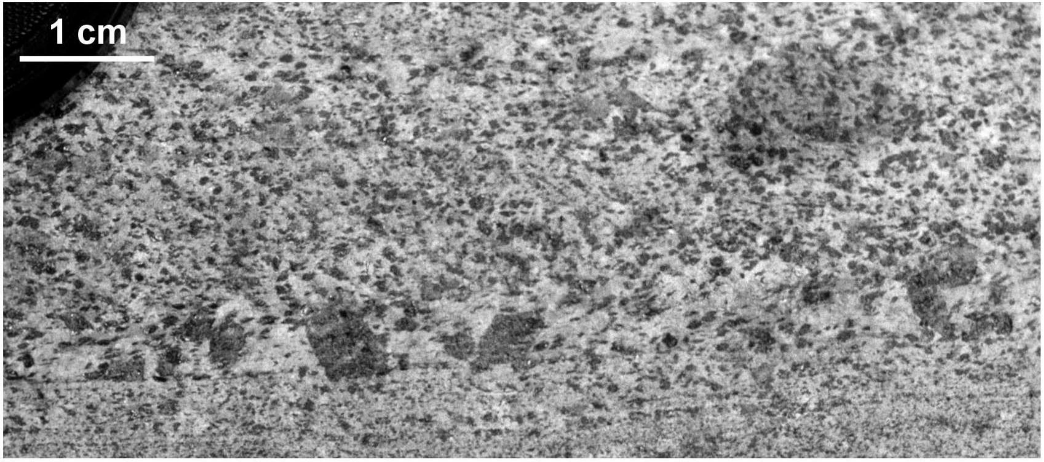


FIG. 2. Pair of transmitted plane-polarized light photomicrographs of thin sections cut orthogonally from a single oriented rock sample according to the grain-shape fabric as indicated. The sections are from a staurolite–garnet schist collected from the LSZ to the northwest of the central HSZ (Rangeley Formation, Swift River, north of Coos Canyon, Byron, Maine). The strong linear fabric is made apparent by comparing (a) and (b). Biotite is apparently pulled-apart in (a) with quartz infilled between the separated plates; pressure-shadow tails are seen in this view. These biotite "pull-aparts" are seen down-plunge in (b).

(a) lineation-parallel, foliation-perpendicular surface



(b) lineation- and foliation-perpendicular surface



(c) lineation- and foliation-perpendicular surface

FIG. 3. In Coos Canyon, Byron, Maine, megascopic views of the rock fabrics at outcrop. The view in (a) is parallel to the penetrative mineral lineation, whereas the views in (b) and (c) are perpendicular to this lineation. All three views are oriented perpendicular to foliation. The strongly elongate fabric is apparent by comparing (a) with (b), which are orthogonal surfaces at the same location in the outcrop. Partial pseudomorphs after andalusite in (c) are folded to suggest flattening normal to the compositional layers. The psammite–pelite contact above these pseudomorphs is also folded, which indicates that flattening was accommodated within these rocks during or after porphyroblast growth and not before.

mapped. These are the two types of unit we mapped as HSZ and LSZ in Figures 2 and 4 of S & B.

We used this qualitative difference in strain as recorded by the rock fabrics as a frame of reference in which to consider porphyroblast–matrix textures. Photomicrographs published in S & B as evidence were the best examples available at the time of submission, and these may have included a high proportion from the area in and surrounding Coos Canyon. However, observations by S & B were based on careful petrographic observation of several hundred thin sections from more than 150 oriented rock samples from all of the metasedimentary units, and not just from samples of the Perry Mountain Formation. The insinuation in G that the HSZ of S & B were defined by observations from the Perry Mountain Formation, and the LSZ of S & B were defined by observations from the Rangeley Formation, is false. S & B did not state that HSZ formed in particular stratigraphic units. Inspection of Figure 2 in S & B shows that boundaries between structural zones cross-cut stratigraphic contacts, an observation that precludes a stratigraphic control on the partitioning of deformation. What S & B did postulate is that HSZ may have been developed because of rheological differences between formations. However, this hypothesis is not supported by the present-day similarity in thickness of compositional layers between the Rangeley and Perry Mountain formations. These issues are discussed further in Solar & Brown (2000).

In contrast, G argues that rocks of the study area do not record high strain, although he presents no quantitative data to support his statement. Unsupported assertions such as "… by the time that staurolite and andalusite were crystallizing, there was little or no strain occurring in these rocks" and "… staurolite-rich veins cross-cutting bedding (*sic*) … clearly formed at or near the peak of metamorphism" are unscientific, apart from creating further confusion about exactly what G might mean by "… the peak of metamorphism …" in the context of his "M1", "M2" and "M3" episodes of metamorphism. We do not understand why G uses the word "prototype" for HSZ in relation to the outcrop at Coos Canyon. We did not define this specific outcrop as a type locality, although the central HSZ is well exposed at Coos Canyon, and this presumptive use by G of "prototype" is clearly inappropriate. G discusses the state of strain with reference to this one locality, but he has not compared the fabrics in each zone as mapped by S & B. Thus, the very limited statements G makes based on that one locality do not qualify him to evaluate the division into structural zones or provide a sufficient basis for him to proclaim that the structural zones described by us do not exist (see also Solar & Brown 2000). Further, the misrepresentation by G that S & B based their interpretation of the area shown in their Figure 2 upon the one outcrop at Coos Canyon is disingenuous at the very least.

Syntectonic metamorphism

In responding to many of the points of detail listed by G concerning the relative timing of porphyroblast growth and deformation, we emphasize the following observations and interpretations, as numbered in S & B (starting on p. 327), that again lead us to the logical conclusion that metamorphism in west-central Maine was syntectonic.

(*i*) *Regardless of structural zone, matrix textures have pervasively developed preferred grain-shape fabrics that record a tectonic strain ellipsoid*

Flow of rocks produces deformation, components of which are the accumulated (or finite) strain, rotation and displacement. A strain state is described by the strain ellipsoid, which has three principal axes of strain. During deformation below the brittle–viscous transition, if the rock fabric records the accumulated strain, the linear and planar elements of the fabric define an apparent state of strain. Lineations are subparallel across our area of study in west-central Maine, regardless of structural zone. At similar metamorphic grades, the same minerals define the rock fabric in both types of structural zone, and no discernable overprinting textures are observed. S & B used these two facts to conclude that the fabrics developed essentially during the same event of progressive deformation in both types of zone, and they interpreted these fabrics to record strain in the form of grain-size reduction during matrix recrystallization. Indeed, the simple relationship between rock fabric and apparent state of strain has been well understood since the pioneering works of Sorby (1853, 1856), Harker (1886) and Cloos (1947), all of whom were well aware of the importance of the orientation of the surface being examined in relation to the cleavage and the strain ellipsoid. Thus, it is somewhat galling that G would hide behind a statement that his rocks "… were collected long before it became "fashionable" to collect oriented samples …", particularly since the concepts of rock fabric and fabric symmetry extend back to Sander (1911, 1930) and Weiss (Paterson & Weiss 1961, Turner & Weiss 1963).

The invidious charge by G that S & B either did not cut thin sections properly oriented in relation to the rock fabrics or cannot cut oriented thin sections competently is not substantiated and is false. The fatuous assertion by G that using thin sections cut from the first four oriented samples he has ever collected has enabled him to "… more accurately locate(d) …" the principal axes of strain is unjustified. Further, the biotite lineation to which G refers is subparallel to the bladed muscovite that defines the penetrative lineation in these rocks, and it is likely that both sets of thin sections, those of S & B as well as those from the "new samples" of G, are oriented adequately for the purpose for which they have been used regardless of how "rigorously oriented" were the samples from which they were cut. However, the notion that four samples from one outcrop, however rigorously oriented, are adequate to investigate the timing relations between porphyroblast growth and deformation within a study area of more than 2000 km^2 is naive. In spite of this naivety, we are pleased that our work has prompted G to collect oriented samples, and to cut them relative to mineral fabrics.

To avoid misunderstanding, we make clear our method of thin sectioning. In order to cut thin sections accurately oriented relative to the rock fabrics, rock samples that were oriented in the field were returned to the laboratory to be cut in multiple directions perpendicular to foliation to properly identify the *x*, *y* and *z* directions of the fabric. This method renders clearly observable the orientation of the grain-shape fabric in all rocks for which thin sections were made, and does not rely on one mesoscopic fabric element only (*e.g.*, the biotite lineation noted by G). For HSZ rocks, mineral textures and microstructures were investigated by examination of a minimum of one pair of thin sections oriented according to rock fabric in each sample studied in detail. Both sections were cut perpendicular to foliation, but one section was cut parallel to and the other section was cut perpendicular to the mineral-elongation lineation. In addition, some foliation-parallel sections were cut to examine inclusion trails in porphyroblasts in three dimensions. In LSZ samples, mineral textures and microstructures were also investigated by examination of pairs of thin sections oriented according to rock fabrics. Again, both sections were cut perpendicular to foliation (where present), but one section was cut parallel to and the other section was cut perpendicular to the penetrative lineation. For all samples from both types of structural zone, the lineation-perpendicular section was viewed in the down-plunge-of-lineation direction.

In steep zones (the HSZ of S & B), planar and linear elements of the rock fabric are penetrative regardless of layer composition, and they show a high degree of parallelism across the breadth and along the length of the zones. The same planar minerals and mineral aggregates define both the planar and linear elements of the fabric. This fabric is illustrated in Figure 5 and parts of Figures 6, 8, 10 and 11 of S & B, and Figure 1 of this reply. The fabric is defined by several different minerals and mineral aggregates that are all arranged subparallel to one another. They are: (1) bladed muscovite, (2) elongate asymmetric "fish" of biotite, (3) elongate polycrystalline ribbons of quartz, and (4) tails around porphyroblasts of biotite, garnet, staurolite and, locally, andalusite. With regard to *c*-axis fabrics in quartz (S & B, Figs. 6a, b), the suggestion by G that "... no crystallographic orientation is apparent … [and that] a seemingly stronger crystallographic orientation in … P domain[s] … is … due to … fine-grained muscovite" is deliberate misrepresentation. There is also an intersection lineation defined by the main fabric and a more weakly spaced biotite foliation as seen in Figure 8d of S & B, and in the pelite layer at the bottom of Figure 1b of this reply. All of these lineations are subparallel at all scales of observation in the HSZ. Thus, it is obvious on the outcrop that biotite is not "… the only unambiguous megascopically visible … lineation"; the statement by G that "… many exposures … do not show any well-defined megascopic lineation" is fallacious.

In the intervening zones between the steep zones (the LSZ of S & B), only the linear element of the rock fabric is penetrative. The planar element of the fabric is less intense relative to the fabric in rocks of the steep zones. This fabric is illustrated in Figure 7 and parts of Figures 6, 8, 10 and 11 of S & B, and Figure 2 of this reply. An important observation is that the same lineations exist in the LSZ as in the HSZ, with the exception of biotite "fish", which are exclusive to the HSZ. Biotite in LSZ rocks instead is observed within rod-shaped "biotite pull-aparts" that have a preferred orientation to define a penetrative lineation that is subparallel to the bladed mica and polycrystalline ribbons of quartz (see Fig. 7 and Figs. 6e *versus* 6f, and 8e *versus* 8f of S & B, and Fig. 2 of this reply).

Biotite is not a "stretching" lineation in rocks at Coos Canyon as G claims. In fact, S & B (1999) did not refer to lineations in these rocks as "stretching" lineations. The only "stretching" lineation we observe within the area of study is defined by the "biotite pull-aparts" found in LSZ rocks (see Fig. 2a of this reply). In regard to the other lineations, the word "stretching" as used by G is misleading. Stretching is but one mechanism to produce a lineation. For example, a mineral or mineral-aggregate long-axis lineation, such as those described by S & B and summarized above, can be formed by rotation during any type of deformation, by any combination of coaxial and non-coaxial strain. Therefore, such a lineation is not necessarily a stretching lineation, but just a mineral lineation. The "biotite pull-aparts" appear to have no other explanation than stretching for their parallel attitudes and apparent separation of biotite plates. Accordingly, S & B interpreted them as a stretching lineation that is parallel to the mineral lineation (bladed muscovite). In our experience, "biotite pull-aparts" are not common in HSZ rocks, much less at Coos Canyon. In places, we have seen "domino-style" biotite crystals that have formed "fish" variably, but not "biotite pull-aparts" comparable to those in the LSZ. Regardless, if the assertion made by G is correct, the occurrence of "biotite pull-aparts" in the HSZ would be consistent with an encroachment of oblate fabrics into the LSZ, as discussed in Solar & Brown (2000). However, we cannot evaluate these "biotite pull-aparts" referenced by G in his discussion, because we are not given any evidence such as photomicrographs.

Coos Canyon is an excellent exposure in which to see fabrics characteristic of HSZ rocks at one outcrop, as there are plenty of opportunities to view three-dimensional surfaces. Megascopically, one can see the elongate oblate grain-shape fabric that is defined by the matrix minerals by inspection of mutually perpendicular surfaces (see Fig. 3 of this reply). The lack of experience in such highly strained rocks, evident from the discussion by G, may have led him to confuse elongate biotite "fish" and an intersection lineation defined by a secondary biotite foliation (weaker) that crosses the main foliation (*cf.* Figs. 1a and 1b of this reply). Further, we are uncertain what is meant by statements such as "… it is particularly revealing that in Figure 5b, the biotite lineation is abruptly truncated by a large ... andalusite crystal...", because the porphyroblast–matrix interface must cut a fabric for the porphyroblast to overgrow the fabric. Regardless, this "truncation" is only apparent, and the biotite lineation emphasized by G is not really truncated by the porphyroblasts. In any case, neither the biotite "fish" lineation nor the biotite-intersection lineation is a "stretching" lineation.

(*ii*) *Pressure shadow tails around biotite "fish" and porphyroblast phases are well-established indicators of strain, and their existence records dynamic matrix strain during regional penetrative deformation*

Our interpretation of microstructures is based on decades of research into deciphering the timing of mineral growth relative to deformation, and into understanding deformation in the metamorphic realm. This work was stimulated by Henk Zwart (*e.g.*, Zwart 1962), and developed subsequently by many others, including Simpson & Schmid (1983), Bell (1986), Bell & Johnson (1989), Reinhardt & Rubenach (1989), Vernon (1989), Bell *et al.* (1992), Passchier *et al.* (1992) and Spiess & Bell (1996), culminating in the book by Passchier & Trouw (1996). An important development in the study of microtectonics is the recognition and critical assessment of asymmetric structures that record the vorticity of the deformation, and that are considered in appropriate circumstances to be kinematic indicators. A commonly studied indicator is the occurrence of pressure or strain shadow tails around porphyroblasts (*e.g.*, Passchier & Trouw 1996). S & B concluded that the existence of tails on porphyroblasts, some of which are asymmetric, and that are elongate in the direction of the mineral lineation, records perturbations in the flow, which precludes post-tectonic growth of these porphyroblasts (see Figs. 5, 6c, 6d, 7, 8, 10a, 10b, 11a and 11b of S & B; see also Fig. 4 of this reply). We take this opportunity to point out that Zwart (1962, p. 53, Fig. 8) published an excellent example of biotite "fish" in schistosity, and in his description he is perfectly clear about the syntectonic nature of this microstructure.

(*iii*) *Inclusion foliations in garnet and staurolite porphyroblasts are found to be both parallel and non-parallel to matrix foliation*

Obliquity between inclusion trails and matrix foliation is explained in S & B by the growth of porphyroblast phases prior to final recrystallization of matrix minerals. Many of the papers cited above address the interpretation of inclusion trails in porphyroblasts [see Johnson (1999) for a review], and each supports an interpretation that such microstructures indicate at least interkinematic growth of the porphyroblast minerals during dynamic recrystallization of matrix. Most inclusion foliations preserved in porphyroblast phases in our area of study (S & B, Fig. 2) are not continuous with the external foliation, and they show a consistent sense of obliquity of inclusion foliation relative to matrix foliation in the same thin section among biotite, garnet and staurolite crystals (see Figs. 5, 10c, d and e; see also Figs. 5, 6, 7 and 8 of this reply). S & B stated that angles between S_i and S_e vary upward from 0°, and that S_i may be parallel to S_e. In his descriptions of S_i and S_e relative to garnet, G does not mention the overgrowth rims on garnet. We continue to interpret these features to have grown during "M3", using the terminology in G for the metamorphism around the Mooselookmeguntic pluton.

We do not see successive overprinting of crenulation cleavages, or development of penetrative crenulations as postulated by G in his imaginative discussion, nor do we see overprinting of crenulation cleavages recorded by inclusion trails in porphyroblasts (*cf.* Bell & Rubenach 1983, Bell *et al.* 1986). Thus, we do not find evidence to support the postulate in G that there are two schistosities that developed sequentially (his S1 and S2), and no convincing evidence is provided in his discussion. We have examined carefully the texture in Figure 4 of Guidotti (1970b), which is the only evidence offered by G in support of a regionally pervasive S2 foliation. Unfortunately, the orientation of the field of view of this photomicrograph in relation to the rock fabric is not specified, and we find the weakly curved internal trails of quartz inclusions in xenoblastic staurolite unconvincing evidence for the existence of this S2 crenulation cleavage. We prefer to explain variations in fabric orientation in relation to folds by progressive deformation. For example, folds like those shown in G (Figs. 1, 2) form initially at perturbations in the flow field, perhaps owing to the heterogeneous rheology of the deforming rocks suggested by the presence of alternating pelite–psammite layers. The folds tighten progressively as the deformation continues, and folds formed earlier in the deformation history are tightened to a greater degree than folds that formed subsequently (see the model in Fig. 12 of S & B). Fabrics that form by syntectonic recrystallization during progressive flattening of folds must adjust as the fold tightens or as the axial plane rotates. If axial planes of folds rotate, so too

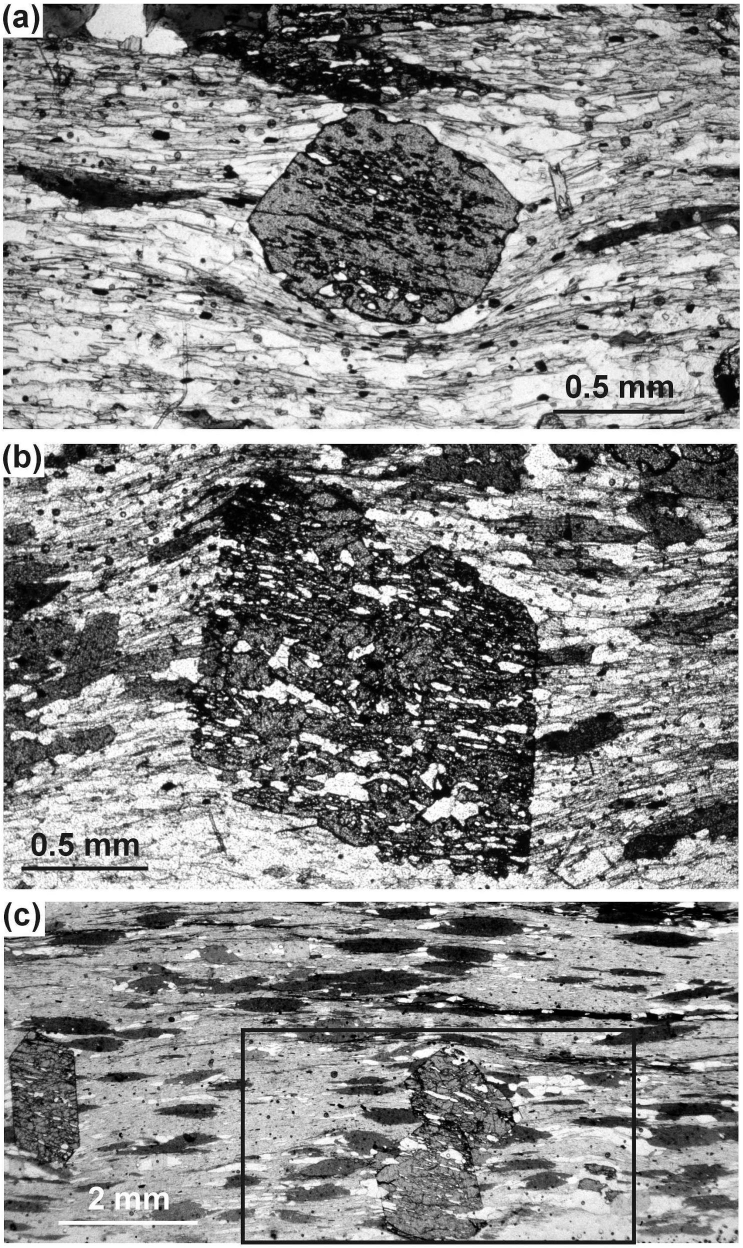


FIG. 4. Examples of asymmetric microstructures in transmitted plane-polarized light photomicrographs of oriented rock samples from a HSZ. All examples are from lineation-parallel thin sections of staurolite–garnet schist. Photographs (a) and (c) are of samples of the Perry Mountain Formation at Coos Canyon, Byron, Maine, and (b) is from the Rangeley Formation to the east of Coos Canyon, Byron, Maine. The garnet porphyroblast in (a) has inclusion trails that are not parallel to the matrix foliation, and pressure-shadow tails that are elongate along the lineation. The matrix fabric is wrapped around the garnet, and terminates at the up- and down-plunge-of-the-lineation sides of the garnet. (b) This staurolite porphyroblast has sigmoidal inclusion-trails that are apparently continuous with the weakly flattened matrix foliation. We interpret this microstructure to record a progressive change in orientation of the foliation relative to the staurolite crystal as the porphyroblast grew. (c) Staurolite porphyroblasts show variable amount of obliquity between inclusion trails and matrix foliation; the matrix foliation is composed of a dense mica fabric that wraps around biotite "fish"; the outline shows the area expanded in Figure 5.

FIG. 5. Detailed view of Figure 4c shows the obliquity between the fabric defined by the trails of included minerals in the staurolite porphyroblast and the matrix foliation defined by mica. Notice the asymmetric pressure-shadow tails associated with the biotite "fish" in the matrix.

must an axial-planar foliation. This may be accommodated by passive rotation of the fabric, or by further recrystallization of matrix minerals to form in a new orientation.

The model in S & B is based upon the porphyroblast–matrix relations as documented in that paper. We find the arguments in G concerning what constitutes "S1" *versus* "S2" confusing. We are not certain if he assumes that any planar foliation is "S1" and any not-exactly-planar foliation is "S2". This interpretation would ignore the established model of Bell & Rubenach (1983) and Bell *et al.* (1986) concerning the formation of crenulation cleavage. Some of the textures to which we believe G refers are presented in Figure 4 of this reply. The "lower-strain lens-shaped area" of S & B that occurs south of Coos Canyon possesses a crenulated foliation (S & B, Fig. 9). The crenulation foliation is locally developed in this m-scale structure. The fabric in rocks that surround the low-strain lens-shaped area wraps it as foliation may wrap around a porphyroblast. The long axes of staurolite were measured inside this m-scale structure and are presented in Figure 4d of S & B. Further, we do not observe "open folds on the earlier isoclinal folds", and we submit that the crenulation developed simultaneously with the non-crenulated foliation during one progressive deformation. G proposes that inclusion trails in porphyroblasts are formed by growth of porphyroblasts over a postulated S2 crenulation cleavage. This is inconsistent with his view that S2 developed during "M3", which is spatially restricted to the immediate vicinity of the Mooselookmeguntic pluton. If garnet or staurolite overgrew "weakly developed S2 crenulation cleavage" as G claims, what happened to this crenulation cleavage in the matrix? The foliation is not pervasively crenulated now outside the m-scale low-strain structures discussed above (see Figs. 4 and 6 of this reply). Regardless, we interpret these porphyroblast–inclusion textures to show that deformation outlasted porphyroblast growth, which in the model proposed by G would include decrenulation of the matrix (*e.g.*, Bell 1986). This model was rejected by S & B because the observations we reported in that paper do not fit such an interpretation (S & B, p. 329).

(*iv*) *Syntectonic growth of minerals is illustrated by the textural zones present inside garnet and staurolite porphyroblasts*

Porphyroblast phases that have multiple textural zones characterized by differently oriented trails of mineral inclusions within them record periods of porphyroblast growth separated by periods of matrix fabric reorientation. Some porphyroblasts, like those shown in Figures 6c and 6d in S & B and Figure 6c and 7 of this reply, show up to three textural zones. In these porphyroblasts, a progressive reorientation of matrix foliation after periods of porphyroblast growth commonly is recorded by a change in obliquity of inclusion fabrics from the inner to outer zone, from higher to lower angle of obliquity with the matrix fabric, respectively. The successive zones with differently oriented trails of mineral inclusions, none of which are parallel to the matrix fabric, illustrate the punctuated syntectonic or interkinematic growth of porphyroblast phases and preclude post-tectonic growth of porphyroblasts. Other porphyroblasts show only one period of growth, recorded by one inclusion-trail geometry (see Figs. 10c, d, e, and 11a, b of S & B). These apparently single-stage porphyroblasts also commonly show overgrowth rims that separate the inclusion trails from matrix minerals. This texture becomes progressively dominant along

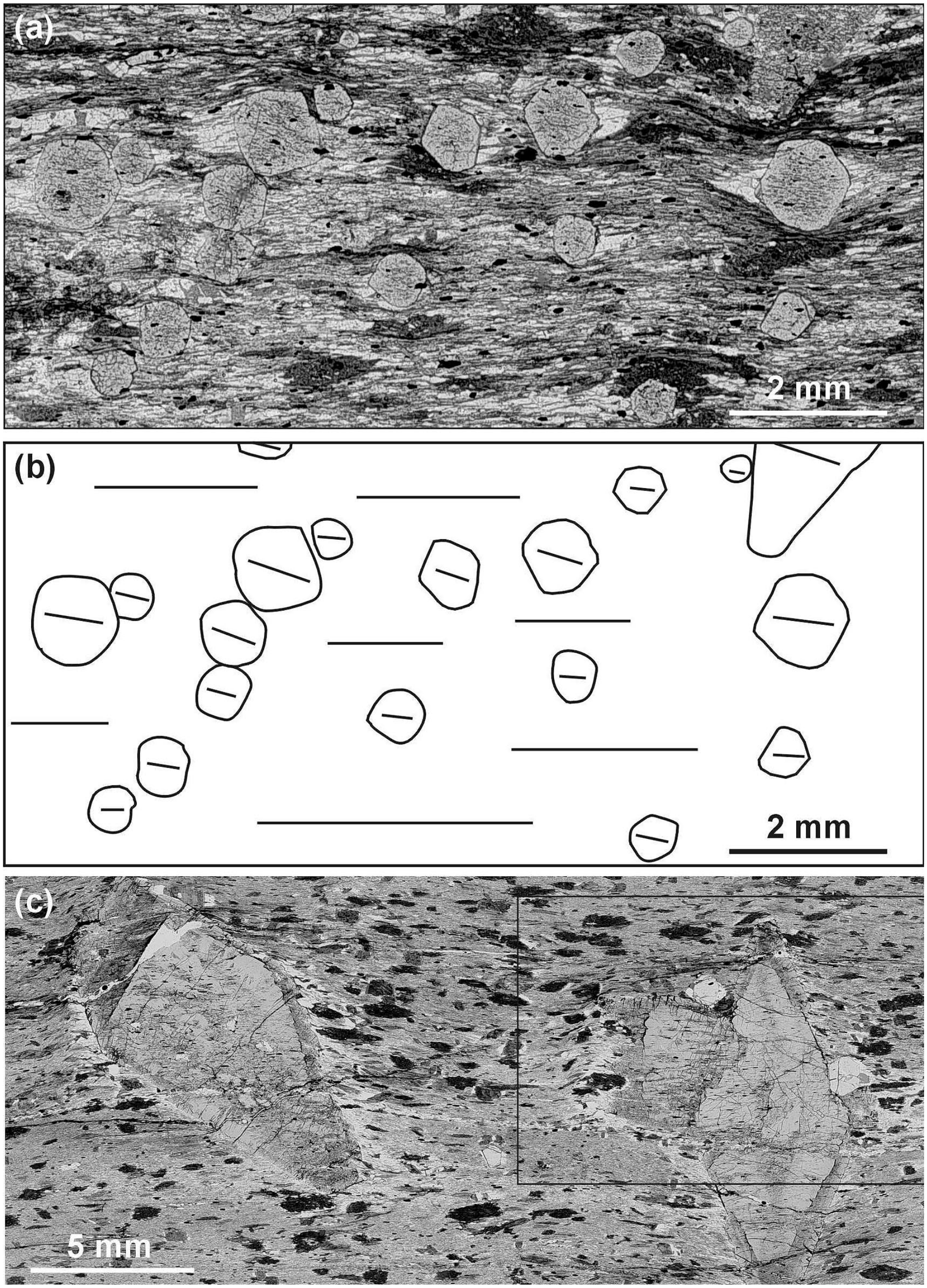


FIG. 6. Transmitted plane-polarized light photomicrographs illustrate the obliquity between inclusion trails in porphyroblasts and matrix foliation. Photos (a) and (c) show garnet–staurolite schist from the Rangeley and Perry Mountain Formations, respectively, taken from the area of Coos Canyon, Byron, Maine. Each view is parallel to the penetrative mineral lineation, and perpendicular to foliation. Outlines of the porphyroblasts in (a) are shown in (b). In (b), the lines inside the porphyroblast outlines represent the general apparent dip of inclusion trails. The long lines in (b) outside the outlines represent the intersection of matrix foliation with the field of view. Staurolite crystals in (c) have textural zones defined by differently oriented inclusion trails; the outline shows the area expanded in Figure 7. Notice the pressure-shadow tails associated with the left-hand staurolite porphyroblast in particular.

FIG. 7. Detailed view of Figure 6c shows the arrangement of growth zones in staurolite and the variation in orientation of the internal foliation defined by the mineral inclusions in relation to the matrix foliation (foliation orientation is emphasized by the superimposed black lines).

traverses toward the Mooselookmeguntic pluton (S & B, Fig. 2), until younger porphyroblasts of garnet ("M3" of G?) are seen cross-cutting the penetrative rock fabrics (see Fig. 10f in S & B) within tens of meters of the contact.

Although we did not discriminate the opaque phases in S & B, since the main thrust of the paper concerned the relative timing of deformation and metamorphism, careful petrographic observation of the Perry Mountain Formation over the area of study leads us to disagree with the statement by G that graphite is not common in that formation. For the record, we are well aware of reports that graphite inhibits recrystallization and grain coarsening, most likely by pinning grain boundaries, and promotes idioblastic crystal form, perhaps because it adsorbs to crystal faces of porphyroblast minerals. The imputation that S & B do not have the petrographic experience necessary to recognize the effect of graphite on textural development is not sufficient scientific basis to dismiss the pressure-shadow tails around garnet and staurolite as "ambiguous". Features such as depletion halos (*e.g.*, Rubenach & Bell 1988) are easily distinguished from pressure-shadow tails of the kind illustrated by S & B in Figure 11a, because of the marked difference in internal microstructure left by mineral dissolution and depletion in comparison with synkinematic recrystallization, as evidenced when the field of view in Figure 11a is viewed with the first-order plate, as shown in S & B, Figure 11b. However, we do agree with the general conclusion of Rubenach & Bell (1988) that the presence of graphite probably helps to concentrate shear strain in graphite-rich laminae, most likely because it orients so that the single slip plane can preferentially accommodate the shear strain. Thus, the record of strain in graphite-rich layers may be different to that in graphite-poor layers (see also Bell & Brothers 1985). Nonetheless, the variable modal abundance of graphite in different layers and formations will not change the timing of textural development relative to deformation recorded by the rock sequence as a whole, and there is no reason to believe that the well-developed practice of microstructural interpretation of porphyroblast–matrix relations is negated by the presence of graphite (*e.g.*, Bell & Brothers 1985).

(*v*) *Porphyroblasts of staurolite are statistically aligned with the matrix foliation*

We are amazed that G would argue against measured orientations of the long dimension of porphyroblasts with the words "simple visual inspection ... reveals no obvious alignment ...", and that he would suggest that we proposed "a staurolite lineation" when we did not. As a test, we invite "simple visual inspection" of Figure 8 of this reply. S & B demonstrated that staurolite grains in both types of structural zone are aligned preferentially, using statistically valid datasets from multiple localities. It is clear that porphyroblasts are neither "randomly oriented" nor "unoriented", as G has written in his field guides. S & B did not state that porphyroblasts of staurolite are "elongate", only that they are inequant in shape. We discussed the significance of the alignment of the porphyroblasts and did not use it singularly as evidence of syntectonic growth. Cruciform twinning of staurolite porphyroblasts is not "almost universal" (see, for example, Figs. 5 & 6 in G, and Fig. 8 of this reply). This is obvious at outcrop, and we did not ignore these twins as G claims.

The assertion that andalusite is "... typical[ly] no[t] align[ed] ...", despite the data in Figure 4c of S & B, is not supported by any new data in G and is plainly incorrect. Indeed, many of the andalusite porphyroblasts in Figure 7 of G appear to be aligned subparallel to the bottom edge of the photograph. We do not agree with G that these are randomly oriented, in particular with respect to matrix foliation, although they commonly seem random within the plane of matrix foliation. This was found at the locality measured by S & B (Fig. 4f). S & B did not refer to any "… lineation of andalusite crys-

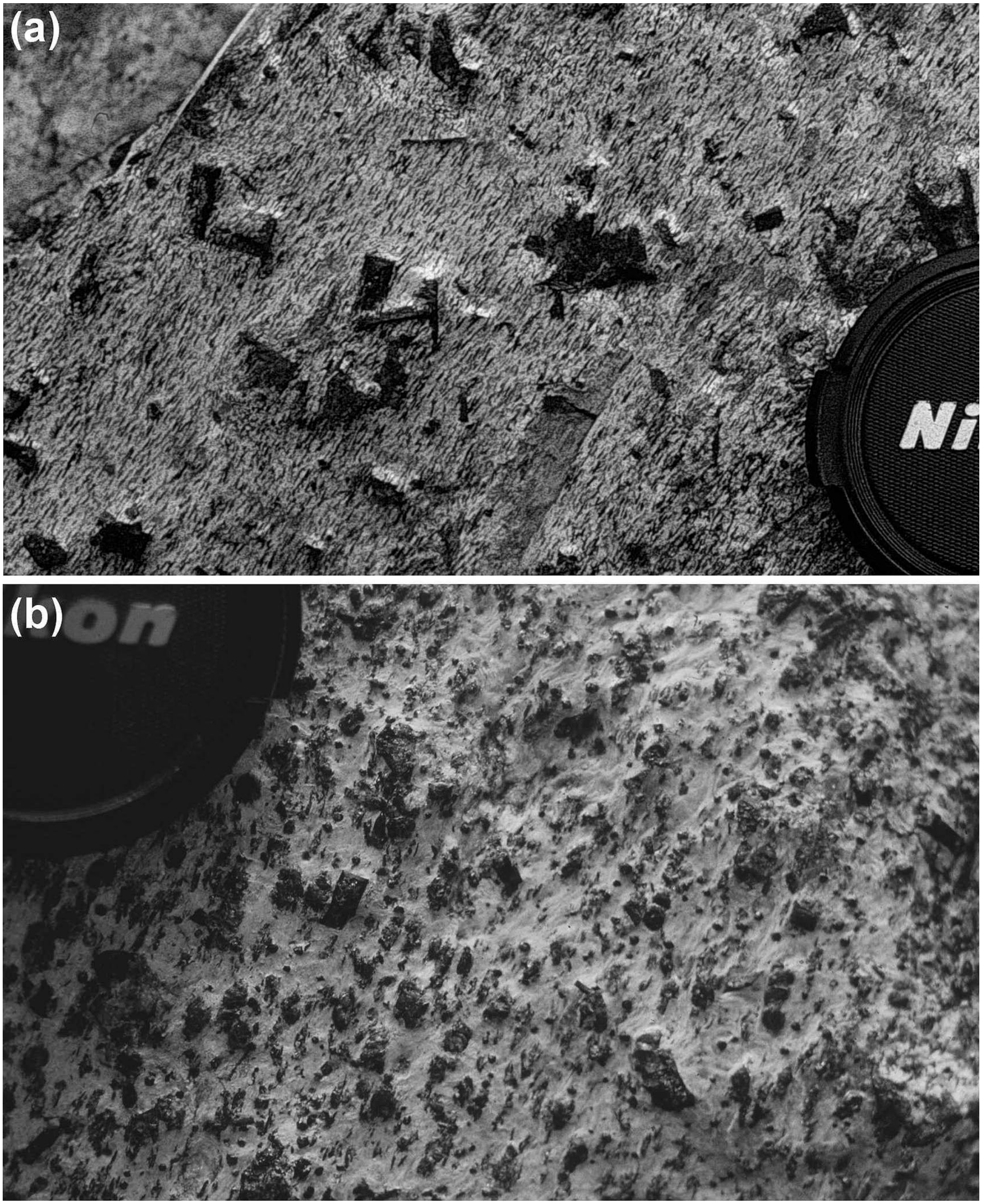


FIG. 8. Views parallel to both foliation and lineation from two localities at Coos Canyon, Byron, Maine. Both (a) and (b) are steep surfaces with northeast to the left. Matrix fabric in (a), illustrated by the acicular biotite lineation, is apparently wrapped around porphyroblasts, particularly the staurolite crystals at left of center. The partly replaced porphyroblast after andalusite at bottom center is oriented subparallel to the matrix fabric. The view in (a) is a closer view from Figure 3b in S & B. The view in (b) shows an apparent preferred alignment of staurolite porphyroblasts subparallel to the matrix lineation in (a).

tals", whatever G may insinuate. We are uncertain why the andalusite veins shown in Figure 8 of G are problematic. We interpret these as annealed fractures where andalusite nucleated late in the history. Regardless, we ask why the andalusite in these veins is oriented exclusively in the plane of foliation? If metamorphism was a "static recrystallization event" as G claims, this preferred orientation is problematic. Further, at one locality in Coos Canyon, pseudomorphs after andalusite are apparently folded within a pelite layer, and the surface between the pelite and psammite compositional layers is apparently flattened around them (Fig. 3c of this reply). To us, such data render problematic the assertion by G that andalusite growth was post-tectonic.

We leave it to readers to decide whether or not porphyroblasts are randomly oriented in west-central Maine (see Fig. 4 in S & B). The statistical alignment of long axes of staurolite and andalusite porphyroblasts demonstrated by the data presented in S & B were interpreted in light of the inclusion foliations within these crystals, which we examined in multiple sets of three mutually perpendicular thin sections cut with reference to the tectonite fabric, as described in S & B, and the presence of quartz–mica pressure-shadow tails elongate in the lineation around these crystals (*e.g.*, Figs. 11a, b of S & B; see also Fig. 4 of this reply). Using all available data, S & B concluded that the statistical alignment of porphyroblasts by whatever means (rotation or flattening, or both) was related to the accumulation of strain in the rocks. However, as discussed by S & B, without additional microstructural information, the preferred orientation alone cannot be used to indicate syntectonic metamorphism.

(*vi*) *Pseudomorphic replacement of staurolite and andalusite porphyroblasts.*

White mica and chlorite that replace porphyroblasts locally are parallel to matrix fabrics, suggesting that retrograde metamorphism occurred during the waning stage of the deformation. In addition, we note that elongate crystals of poikilitic muscovite and minor chlorite occur parallel to foliation and lineation throughout migmatites of the TAD and WAD. Given the steep orientation of these fabrics, we interpret the retrograde growth of muscovite and chlorite to record buoyancy-driven fluid flow parallel to the fabrics in the rocks. This fluid is likely to have been derived from crystallizing melts within the migmatites, and we postulate that this is the principal cause of the regional retrogression of staurolite and andalusite.

CONCLUDING STATEMENT

The principal aim of S & B was to document evidence pertaining to the timing of mineral growth in metamorphic rocks of the Rangeley stratigraphic sequence in the type area of Maine. In our opinion, the evidence we presented in that paper is neither undermined by the unwarranted aspersions cast on us by G in relation to scientific methodology nor challenged by any of the "new observations" presented by G. Thus, the conclusion that regional metamorphism in west-central Maine was syntectonic is not disproved by G is his discussion. The claim by G that "...observations and data [in S & B] ... are sufficiently flawed and questionable that their general conclusions or assertions should be accepted only with reservation" is not supported by any new evidence and is false. We take exception to the use of "conclusions or assertions" by G since the conclusions presented by S & B all were supported by evidence presented in that paper. Indeed, G presents no new data to support his criticism of our work or to refute any of the conclusions we have presented in S & B or in our other papers. Argument by authority rather than supported by observations is not uncommon in geology, but it has no place in scientific debate (Vernon 1996).

Although we agree that readers should examine critically the conclusions of any scientific work as part of the normal methodology of science, as we hope we have done in all our papers, the unfounded assertion that our observations are fit to a particular model concerning the timing and emplacement of granite plutons is false. In fact, our interpretations have been developed based on extensive mapping and integration of data from structural geology, petrology and geochemistry, all of which data are either published in the peer-reviewed literature or are to appear in the peer-reviewed literature (*e.g.*, Brown & Solar 1998a, b, 1999, Solar & Brown 1999, 2000, 2001, Solar *et al.* 1998). The same standard of peer review has applied to the work by others in our research group, including the work of Pressley (1997) for her MS thesis (Brown & Pressley 1999, Pressley & Brown 1999). Indeed, the regular publication of our work in peer-reviewed periodicals confirms the probity of our science, and our ongoing research in west-central Maine will include testing further the robustness of our interpretations.

We have difficulty understanding why G continues to contend that porphyroblasts are randomly oriented despite the orientation data presented in S & B. Although these data do not preclude a model of post-tectonic metamorphism, they do add to the preponderance of evidence from microstructural observations of porphyroblast–matrix relations that led S & B to conclude that metamorphism in west-central Maine was syntectonic. Our re-evaluation of the evidence and arguments in the light of the provocative discussion by G does not lead us to any different conclusion in this reply.

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