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DISCUSSION

DO MINERAL PARAGENESES REFLECT UNUSUALLY HIGH-PRESSURE CONDITIONS OF FRANCISCAN METAMORPHISM?

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Ernst (1971) pointed out that there currently exist several hypotheses for the genesis of blueschist facies rocks, including one proposed by me (Gresens, 1969). As a matter of fact, since the time that Ernst wrote his paper, a new hypothesis for the genesis of blueschists, based on gas overpressure, has emerged (Brothers, 1970), thus further adding to the controversy over the origin of these rocks. Ernst's paper attacked the various hypotheses proposed by other investigators and was written, naturally, so as to present his case in the best possible light. This prompts me to take issue with and enlarge on a number of topics on which he touched.

A major criticism of Ernst's paper is his tendency to downgrade the association of blueschists with ultramafic rocks. He admits (p. 93) that "On a global basis, it is true that glaucophane schists and ultramafics commonly are spatially associated," and he cites as examples localities such as "the Alps, the Caribbean, and New Caledonia, as well as the California Coast Ranges." To this list one might add Japan (numerous Japanese publications), Siberia (Yegorov and others, 1967; Dobretsov and Kuroda, 1970), Celebes (DeRoever, 1947), New Zealand (Coombs and Landis, 1966), Australia (Joplin, 1968), Venezuela (Shagam, 1960), Guatemala (Williams, McBirney, and Dengo, 1964, p. 4), Corsica (Brouwer and Egeler, 1952), Turkey (Cogulu, 1967), and Guinea (Davies, ms). It is a fact that where blueschists are found, there are also ultramafic rocks. This was recognized by Miyashiro (1961) in his classic paper on paired metamorphic belts, and it continues to be recognized in more recent papers (for example, Coleman, 1967; Dobretsov and Kuroda, 1970, p. 1401). But the association does not work the other way, that is, ultramafic rocks are not invariably accompanied by blueschists. (Because, according to my model, serpentinization could take place so as to fail to develop unusual chemical conditions as proposed in my hypothesis.) Rather than using the global association as a basis for discussion, Ernst has chosen a few areas where ultramafic rocks, *though present,* are quantitatively minor relative to blueschist rocks and/or where blueschists are not directly in contact with serpentinites. Specifically, he cites the North Cascades of Washington State and the Diablo antiform in California. The "near absence" of ultramafics is used to imply that ultramafic rocks are not required for the production of blueschists. The answer to that implication is presented in his own words (p. 99), that is, "the entire section is considerably disturbed". Tectonic separation of blueschists from genetically related serpentinites would be expected in the dynamic geologic setting in which blueschists form. The local occurrences of occasional blueschists with only small amounts of ultramafics now present are insignificant when compared to the global view.

Although Ernst refers to the "near absence of coeval serpentinized peridotites in the Shuksan belt of blue amphibole-bearing schists in Washington State", the fact is that in addition to many small ultramafic bodies emplaced in the Shuksan thrust, one of the largest single masses of ultramafic rock in North America, the Twin Sisters dunite, is probably part of the Shuksan thrust belt (Christensen, 1971). In regard to the Franciscan formation, other investigators (for example, Blake, Irwin, and Coleman, 1969; Bailey, Blake, and Jones, 1970, p. C77) believe that a thrust fault present in the Diablo antiform is part of the major "Coast Range thrust" along which both ultramafic rocks and "upside-down" blueschist metamorphic zones occur. In general, phrases such as "close spatial association" and "near absence" lack precision because the scale is not defined. Thus on the scale of the Franciscan formation as a whole, I would disagree with Ernst's statement that a close spatial correlation between ultramafic rocks and Franciscan metamorphic rocks does not exist. Moreover, if tectonic disruption is pervasive, such arguments are irrelevant.

Ernst concluded in regard to my hypothesis, that "although not impossible, available field relations, chemical and thermodynamic data do not seem to substantiate the metastable crystallization hypothesis proposed by Gresens." This statement could be completed by adding, "nor do they disprove it". The "available field relations" apparently refer to places such as the Diablo antiform rather than the global distribution, and this matter was discussed above. Thermodynamic data do not substantiate my hypothesis because none exist that are applicable to the possible kinetic factors (for example, interfacial energies) that were postulated. However, Ernst has a valid point in regard to chemical data, and this requires a more detailed answer.

I proposed that highly reducing, highly concentrated pore fluid may be generated around serpentinites. (The reducing nature seems well substantiated, see Thayer (1966, p. 698-700) and Chamberlin and others (1965).) Ernst posed the problem that if such fluids are responsible for blueschist metamorphism, shouldn't the rocks show a lower ferric/ferrous ratio and introduction of Na? (Some do, as discussed below.) He gives analyses of graywackes and metagraywackes to support his argument that such a chemical expression is lacking in these rocks. One may question, as did Essene (ms, p. 110-121), whether such comparisons are valid. One may ask how the selection of specimens is made. For example, I have in my possession a rock from the Franciscan formation that consists essentially of two minerals, albite and glaucophane. The identity of the parent rock is unknown. (Maybe it was a graywacke.) But clearly it would be discarded for a comparison of this type. Aside from such arguments, which verge on petulancy, I proposed that metastable crystallization, *with or without metasomatism,* may be responsible for blueschists. There are rocks (metacherts) that clearly show both introduction of sodium and reduction of ferric iron with progressive metamorphism (Coleman and Taylor, 1968, p. 1739; Gresens, 1969, p. 96, 98). There are examples of unusually Fe^{+2}-rich amphiboles and pyroxenes from blueschist rocks that the investigator attributed to the low oxygen fugacity of the metamorphic environment (Black, 1970a, b). There is a report of native iron in glaucophane-lawsonite schist (Quodling, 1964; Joplin, 1968, p. 100). I would argue that these are cases in which the genetic link to the reducing environment generated by serpentinites is established. However, Ernst cited valid examples of metagraywackes in which the ferric/ferrous ratio is virtually unchanged and no introduction of sodium is apparent. Such rocks are unquestionably a problem for my hypothesis. I can only state that I think that metastable recrystallization was produced essentially isochemically in these rocks by the influence of the pore fluid, and I speculated in my paper (1969, p. 99-105) that poorly understood kinetic factors were involved that led to the development of minerals with low amounts of tetrahedrally coordinated Al. With regard to this hypothetical effect of pore fluid chemistry on the structural position of Al, I am heartened by a recent publication by Martin (1969). He showed that the most important factor controlling Al–Si ordering of hydrothermally grown albite is the sodium concentration of the pore fluid (more important than P and T). This is an example of a thermodynamically unpredictable kinetic factor, involving the chemistry of the pore fluid, that can influence the structural position of Al in a silicate mineral. Another encouraging paper by Daniels and Skoultchi (1966) on pressure-induced phase changes in simple ionic compounds speculates that "the surface layers of the crystal have sufficiently different crystal binding compared to the interior, that they will transform at a lower value of the applied pressure than does the interior. . . . Thus there would be some pressure range below the 'true' equilibrium pressure in which the structure of the surface layers of the crystal was unstable with respect to that of the bulk . . . the question arises whether in these circumstances the surface layers will transform and if they do, whether the adjacent layers will become unstable, etc. whence the entire crystal would assume one structure in ambient conditions in which a surfaceless crystal would have a different structure."

With regard to the problem of how the ferric/ferrous ratio could remain virtually unchanged if a reducing pore fluid were present, the process clearly is not understood. But that it is possible is clear from the occurrence of authigenic blue amphiboles and sodic pyroxenes in the Green River formation. Milton and Eugster (1959) reported the presence of these minerals (which grew in a saline, reducing brine in the final evaporation of the ancient lake) and were puzzled by the oxidation-reduction relationships, noting (p. 141) that "The presence of authigenic acmite and an intermediate member of the magnesioriebeckite-riebeckite series in the Green River formation has been a surprise. . . . The oxygen pressures in the Green River formation are generally considered to have been low. . . . Yet both acmite and magnesioriebeckite-riebeckite contain essentially ferric iron only." They went on to say that Ernst (1957) had shown that magnesioriebeckite can exist at very low oxygen pressures.

Ernst's hypothetical dynamic model (p. 101-104) has problems of its own that should be pointed out. Ernst recognized, as do others (for example, Coleman, 1971, p. 1218-1219) that the relative motion on the Benioff zone is in the wrong sense to be able tectonically to juxtapose supposedly deeply metamorphosed Franciscan rocks against the shallower Great Valley sequence. Although Ernst postulated a series of imbricate thrust faults to mix blueschist rocks with weakly metamorphosed Franciscan rocks, the basic problem remains. If his dynamic model of deep tectonic burial is correct, then unless relative motion on the Benioff zone is reversed, there is no way to bring Franciscan blueschists into contact with Great Valley rocks. If buoyant forces brought Franciscan rocks to their present position, there should be some field structural evidence for the reversed relative motion.

Ernst argued (p. 100 and personal commun.) that the "obduction zones" (Coleman, 1971), where an ultramafic slab has been thrust over the continent (for example, New Caledonia), adds an instantaneous increment of pressure without raising temperature, thus producing the physical conditions presumably required to form blueschists. However, such ultramafic slabs are of different thicknesses (Coleman, 1971, p. 1214), and one may suppose that the thrust plane along which they are carried may intersect the continental sedimentary pile at variable levels (that is, variable intersections of the thrust surface with the ambient geothermal gradient). The added pressure increment would result in the necessary P/T conditions being attained in the sedimentary pile at some depth, *but variable depth,* relative to the thrust surface. But the belt of blueschist rocks in such obduction zones invariably lies directly below the ultramafic slab (as in New Caledonia, New Guinea, Turkey(?)) and passes downward into non-blueschist (commonly greenschist) rocks. Ernst tends to discredit these zones of inverted metamorphism, but others (Blake, Irwin, and Coleman, 1969; Coleman, 1971; Brothers, 1970) have emphasized them, and they appear to be a global feature.

A final comment is in regard to the recent work of Brothers (1970) on New Caledonia blueschists. Ernst (p. 100) cited Brothers as advocating tectonic overpressure for the production of these rocks. Actually, Brothers emphasized that there is a *genetic relationship between the blueschists and the ultramafic rocks.* He based this on the fact that the zonation of blueschist rocks under the ultramafic slab follows the trace of the ultramafic contact, while cutting across the regional structure in the underlying rocks. He considered this to be another case of "upside-down" metamorphism. Brothers ruled out deep tectonic burial on grounds of the passage downward into "lower pressure" rocks. He ruled out tectonic overpressure on grounds of textural evidence for static recrystallization. Brothers proposed a new hypothesis of gas overpressure by vapors trapped below an impermeable ultramafic caprock. But the relationships described by him are also compatible with my model of development of unusual chemical conditions in pore fluids near ultramafic bodies undergoing serpentinization.

In summary, the spatial association of blueschists with serpentinites is firmly established on a worldwide basis. In regard to the possible genetic significance of the association, I urge the interested reader to take neither my word nor Ernst's as the final authority. Rather, he should read the available literature and let the evidence speak for itself.

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REPLY

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Progressive, areally distinct, at least in part contemporaneous metamorphic sequences developed in *in situ* Franciscan rocks have been elucidated by many workers. (For documentation of this and other aspects of the problem, see references cited in Ernst, 1971, and Gresens, 1971.) In quartz and layer silicate-bearing metagraywackes, the most typical paragenesis seems to be: (A) laumontite + albite ± calcite; (B) pumpellyite + albite ± calcite; (C) lawsonite + albite ± calcite or aragonite; and (D) lawsonite + jadeitic pyroxene ± aragonite. Mafic metavolcanics exhibit a corresponding progression from feebly recrystallized greenstones to blueschists. The available thermochemical data and numerous experimental phase equilibrium studies are consistant with the observed mineralogic changes and suggest that relatively high pressures attended metamorphism. Combined with oxygen isotope geothermometry, it would appear that aragonite- and jadeitic pyroxene-bearing metagraywackes and associated metavolcanics must have crystallized at pressures exceeding 8 kb at temperatures on the order of 150 to 300°C. (Some investigators, while accepting these high pressures, have postulated substantial stress increment as the means whereby high pressures could have accompanied such low temperatures of metamorphism. However, because of insufficient rock strengths, high tectonic overpressures–including "gas overpressures"–seem to be unattainable under geologically reasonable conditions.) Provisionally accepting the pressures quoted above as lithostatic, how could such unusual P-T conditions have been generated?

The Franciscan tectonic model I have presented (Ernst, 1965, p. 905-910; 1970, p. 892-898; 1971, p. 103-104) involves two stages of motion: (1) active Late Mesozoic subduction of Franciscan units deposited on the Pacific lithospheric plate, thought to have been encroached upon and descending under the North American plate; then (2) buoyant upward rise of the decoupled ensimatic prism. Metamorphism was postulated to have attended chiefly the first stage and would have been of the high-pressure, low-temperature variety judging from geothermal calculations by several workers. When underflow ceased, or at least diminished, diapiric upwelling of the subducted, recrystallized mélange would be expected because of the marked density contrast between the Franciscan and the mantle material it had displaced; evidence of this inferred motion is displayed in the nearly vertical western limb of the asymmetric Great Valley synclinorium—the section of strata thought to have marked the Late Mesozoic margin of the North American plate.

Gresens (1969, p. 99-105; 1971) has proposed an alternative scheme for the production of blueschists which does not require the high pressures stated above. He postulates that during the serpentinization of peridotite, the generation of reducing fluids would allow the recrystallization of the country rock to produce the observed paragenesis metastably by an admittedly little understood process. Field evidence for a genetic association between glaucophane schists and serpentinites is lacking in the Franciscan terrane because of postmetamorphic structural disruption according to Gresens. Support for his petrogenetic hypothesis therefore seems to rest entirely on the well-known global association of blueschists with alpine-type ultramafics.

I too recognize this nearly world-wide association (Ernst, 1963, p. 1; 1965, p. 905, 1971, p. 83; Ernst and others, 1970, p. 13, 83, 225) but have interpreted it as a consequence of the tectonic setting of these metamorphic belts at old convergent lithospheric plate sutures. Blueschist terranes seem to be virtually confined to subducted slabs and to have been overridden at one stage by the upper lithospheric plates (crust + mantle). In California, Japan, and the Alps, for example, the progressive metamorphic sequences developed in the so-called high pressure terranes may testify to the direction of subduction. (Inasmuch as in general the highest grade portions of these belts are situated directly below the sutures with the overlying lithospheric plates, they have been described by some workers as "inverted" metamorphic zones; however, although complex in detail, including internal thrust contacts, what is evident from most published maps is an overall lateral gradation in metamorphic zonation.)

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Clearly, various interpretations can be made of the blueschist-serpentinite-plate junction association; therefore, it seems to me that Gresens' hypothesis for the production of blueschists, which appears to be based solely on the observed restriction of the glaucophane schist suite to alpine-type ultramafic belts, requires additional documentation.

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