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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