DISCUSSIONS

BARITE FLUID INCLUSION GEOTHERMOMETRY, CARTERSVILLE MINING DISTRICT, NORTHWEST GEORGIA

Sir: I would like to comment on the paper on this title by D. L. Rife (1971), since I believe that the paper as published may give an erroneous impression of the significance and validity of inclusion studies in general. I do not mean to debate Rife’s conclusions as to the origin of these deposits, as I have no evidence to the contrary, but I do not believe the internally contradictory evidence presented supports the conclusions reached.

In essence, Rife’s study consists of the following six steps: (1) Selection of primary inclusions; (2) Evaluation of their possibility of change since trapping, as by necking down; (3) Determination of their homogenization temperatures; (4) Estimation of their compositions; (5) Calculation of their pressure corrections and pressure of formation from (3), (4), and the field evidence for depth of burial at the time of trapping; and (6) Consideration of possible origins for the fluids—and the ores—on the basis of these data.

(1) Selection of primary inclusions. This is a difficult matter at best, and is seldom free of ambiguity. But the value of the data obtained, and the significance of the conclusions reached, rest squarely on the validity of this assignment of primary origin. The criteria that were used here in the selection of primary inclusions are stated as follows (p. 1164):

“The basis for selection of a primary inclusion was the presence of random clusters of semifaceted, thin, tabular, dendritic vacuoles (0.01–0.1 mm) in clear, or light brown mottled zones (Fig. 2).” I suggest that from this statement, only the word “random” implies primary origin; the other words of description used are either neutral in character or even strongly suggestive of a secondary origin (e.g., “thin, tabular, dendritic”). This is particularly true for inclusions in a soft cleavable mineral such as barite. The two sketches in Figure 2 are similarly non-definitive (left sketch), or strongly suggestive of secondary origin (right sketch). This latter sketch shows an apparently thin, tabular, dendritic inclusion (parallel to a cleavage direction?) which cuts a dark, heavily pigmented band (i.e., a crystal growth band?). Most fluid inclusion workers would consider such evidence to be fairly good proof of secondary origin. Perhaps these particular samples, in common with many, simply do not have any inclusions that can be proven to be primary.

(2) Evaluation of the possibility of change. It has been shown elsewhere that inclusions in barite are particularly susceptible to necking down. The statement is made here, however (p. 1164), that “Recrystallization (necking down) of the inclusions is uncommon and is believed to have occurred during inclusion entrapment. This is substantiated by the close agreement of filling temperatures for these inclusions with temperature data for the normal primary inclusions.” Specific documentation for this close agreement is not given. But since the homogenization data (Rife’s Fig. 3) are widely scattered over a 171°C range (from 126°C to 297°C), and they “…show no variation in temperature with respect to primary growth zones or crystallographic position” (p. 1167), it is difficult to see how such otherwise random data could show “close agreement.” To this writer, the stated facts (an extremely wide range in homogenization temperatures and no correlation with growth zones) are very difficult to explain if the inclusions are primary and unchanged, but very expectable if they are secondary and (or) have necked down.

(3) Determination of homogenization temperatures. Two points are particularly significant. First, the histogram (Rife’s Fig. 3) shows a beautifully random group of data, from 126°C to 297°C, with an average frequency of values for each 10°C sampling interval (about 8 determinations) throughout the central 100°C bracket that is only about three times as large as that at each extreme range (2 at 125°C and 3 at 295°C). What significance can one attach to such data? This question is particularly pertinent in view of the second point, which is that the histogram does not include the entire range of inclusions. On p. 1164, single-phase primary inclusions are described. These effectively extend the histogram of homogenization temperatures on down to room temperature. If they are truly primary and unchanged, the barite surrounding them crystallized at or near room temperature (this is a valid possibility for some barite), and the high-temperature determinations seem invalid. If they are secondary or have necked down (also a valid possibility), then the same may be true of the high-temperature inclusions.
(4) Estimation of the composition. The statement is made (p. 1167) that the "...lack of daughter salts in the inclusions is suggestive of liquids relatively low in salinity." The most common salt in solution in inclusions is NaCl, and it can and does form very high-salinity fluids without reaching saturation—up to 26 weight percent. If other salts are present as well, even higher concentrations are possible, and even probable, without reaching saturation at room temperature. This point is more than academic, in that it can lead to serious errors in the pressure correction (e.g., almost 100° high at 300° C and 40° low at 250° C; Roedder, 1971, p. 111).

(5) Calculation of the pressure corrections. In addition to the problems inherent in the lack of salinity data, Rife also states (p. 1164) that liquid CO₂ was found in one inclusion. This information is of great importance to the section on the calculation of pressure corrections and pressures of formation but is not mentioned there. A single such inclusion is just as pertinent in this respect as a thousand. Another statement concerns boiling (p. 1167): "Boiling apparently did not occur because all the inclusions are nearly full of liquid and the liquid-vapor ratio in all the inclusions is approximately the same." It is difficult for the writer to understand how inclusions homogenizing at 126° C and at 297° C can have approximately the same liquid-vapor ratio. For pure water inclusions, these should have 7 and 28 percent vapor, respectively. If the bubble volumes do not vary this much there must be gross differences in the salinity (which affects the coefficient of expansion of the liquid). Still another statement is made (p. 1167) concerning the depth of formation: "The lowest filling temperatures are well above those expected at 5,000 to 8,000 feet along a normal geothermal gradient suggesting formation of barite under anomalous gradient conditions." This statement conflicts with the occurrence of single-phase primary inclusions.

(6) Consideration of possible origin of the fluids and ores. In view of the above points, I believe that the major conclusion of the paper—that the inclusion data support a magmatic-hydrothermal origin for the deposits—is unwarranted. The deposit may well have originated in this manner, but such a conclusion should not be reached on the basis of conflicting and ambiguous data, from inclusions or otherwise.

EDWIN ROEDDER

U. S. GEOLOGICAL SURVEY,
WASHINGTON, D. C. 20242,
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REFERENCES


BARITE FLUID INCLUSION GEOTHERMOMETRY, CARTERSVILLE MINING DISTRICT, NORTHWEST GEORGIA—A REPLY

Sirs: Dr. Roedder kindly provided me with the opportunity to read and prepare comments on his very thorough discussion of my paper (Rife, 1971) prior to publication. The opportunity for immediate reply is welcomed.

(1) Selection of primary inclusions. Roedder misinterprets my statement (p. 1164) concerning "the basis for selection of a primary inclusion..." and the words "semifaceted, thin, tabular, dendritic" stress shape. Ermkov and others (1965), moreover, used the term "semifaceted" to describe the shape of primary inclusions. Furthermore, the terms "feathery" and "dendritic" were used by Roedder himself (1967, p. 519) in describing primary inclusion growth. The term "random" and the occurrence of the vacuoles "...in clear, or light brown mottled zones..." imply primary origin. In my study, inclusions having random three-dimensional distribution within clear bands of zoned crystals were considered to be primary. This is consistent with Roedder's definition (1967, p. 520). Inclusions microscopically connected by capillary tubes were rejected, as were groups of inclusions near cleavages or fractures.

Figure 2 (p. 1166) illustrates large, primary inclusions, occurring along growth zones. This occurrence is typical of the inclusions investigated during the study. The righthand sketch in Figure 2 shows an inclusion (definitely not parallel to a cleavage plane) that propagated itself from right to left across a minor dark pigmented band. It should be emphasized that inclusions can "cut" such zones only when they are controlled by fractures or cleavages and are therefore secondary or pseudosecondary. The lefthand sketch in Figure 2 shows an elongate inclusion which is parallel to growth zones and appears to have grown vertically.