THE AMERICAN MINERALOGIST, VOL. 50, MAY–JUNE, 1965

REPLY TO DISCUSSION OF "EMPRESSITE AND STUETZITE REDEFINED" BY R. M. HONEA

Russell M. Honea, *Department of Geology and Mineralogy, University of Colorado, Boulder, Colorado.*

In reply to L. J. Cabri's comments regarding my recent paper (Honea, 1964), it would seem appropriate to retain the point by point order of discussion set forth by Cabri. This is as follows:

1) *Apparently incorrect statements regarding the assemblage stuetzite* ($Ag_{5-x}Te_3$)*-petzite and stuetzitz-petzite-hessite.* The assemblages given for the Golden Fleece mine, Lake City, Hinsdale County, Colorado, are *not* compatible with Markham's (1960) 300° C. isothermal section, and my thanks are due for pointing out this obvious error. The presence of the tie line between hessite and sylvanite precludes both associations, and as pointed out by Cabri such assemblages are possible according to Markham's data only above 315°±10° C.

Cabri's description of stability relations, polished section properties, and *x*-ray d-spacings of the "gamma-phase" found by Kiukkola and Wagner (1957) between $Ag_{5-x}Te_3$ and Ag_2Te (hessite) represents a new and welcome addition to mineralogic literature. The possibility of forming the three phase assemblage stuetzite ($Ag_{5-x}Te_3$)-petzite-hessite from breakdown of a gold-rich solid solution of the gamma-phase at surface temperatures is an intriguing one from both the standpoints of indicated original composition and possible use in geothermometry. I regret that I do not have textural evidence indicating this mode of origin for the Golden Fleece assemblage. My samples for the assemblages given are unfortunately specimens from our mineral collections, and the minerals noted are in relatively coarse aggregates. It was not possible to collect samples of the minerals concerned at the mine to supply geologic control.

2) *Misleading reference to Galbraith's telluride identifications* (*in Eckel, 1949*). This refers to my statement, "More complete data concerning the mineralogy of the telluride deposits in the La Plata district are given by F. W. Galbraith in Eckel (1949)." As pointed out by Cabri, I recognized the fact that the "hessite" previously reported from the district was probably stuetzite as redefined. I, along with any other investigator who has worked with tellurides, recognize the lack of reliability of identifications based solely on etch reactions and microchemical tests, but fail to see that I am misleading a reader by referring him to an earlier and more comprehensive account of the mineralogy of the several telluride deposits in a district by citing the only recently published account of the district.

There are indeed several inconsistencies in the associations reported by Galbraith when viewed in the light of more modern work, but I didn't feel it necessary to mention these specifically since the associations reported are ones observed by myself. My intent was to give due credit for the previous work, not to pass judgement on its validity.

3) *An interesting correlation between certain extra reflections in silver rich compositions of synthetic* $Ag_{5-x}Te_3$ *and some of the stronger reflections in the published AgTe pattern.* Cabri's reported failure to synthesize the pure phase AgTe (empressite) from runs of that composition repeats my own failure to do so—as was reported in the paper. I would have preferred to have been able produce the material synthetically, but failing to do so, felt that I provided more than sufficient evidence to substantiate the occurrence of the mineral. This evidence includes distinctive optical data; a new chemical analysis of material first shown by optical and *x*-ray techniques to be homogeneous (combined with four previous analyses from the literature); the *x*-ray powder (indexed in the original paper) and single crystal data proper; measured and calculated specific gravities in near perfect agreement; and finally, thermal data including a differential thermal analysis curve and *x*-ray identification of breakdown products of the natural mineral heated to various temperatures.

I find the appearance of extra lines corresponding to empressite in Cabri's synthetic runs corresponding to Ag-rich compositions of stuetzite ($Ag_{5-x}Te_3$) an interesting possible first indication of empressite as a laboratory product. Like Cabri, I can offer no ready explanation as to why the phase should be formed from the Ag-rich rather than the Ag-poor compositions where it would appear to be more likely. Neither can I offer a ready explanation of the coincidence of most lines of the powder patterns of empressite and stuetzite, but the production of sharp single crystal precession photographs without spurious reflections precludes the possibility of the coincidence being the result of a mixture. The answer should doubtless be sought in the area of structural characteristics, but there is unfortunately no such data available at the present. Regarding the "closer agreement" of cell dimensions and indexing of Cabri's data for synthetic $Ag_{5-x}Te_3$ with that for the natural material of Berry and Thompson (1962), a spot check of his data suggests an a closer to Berry and Thompson and a c closer to mine. A check of indexing against that in the original papers by both myself and Berry and Thompson (*hkl*'s and calculated d-values have been added to both in Cabri's table) indicates observed reflections for 29 planes not listed by Berry and Thompson and 21 planes not listed in my paper. I fail to see that this constitutes "closer agreement," or for that matter that it is significant at all since it is customary to omit calculated planes for which lines were not observed as a means of conserving space in journals. The reported variation in cell dimensions between my work and that of Berry and Thompson is on the order of 0.82 per cent as a maximum—a not surprising variation since the data were obtained using different camera diameters and from natural materials representing different localities, and no doubt varying to some extent in chemical composition, environment of deposition, thermal history, etc.

It seems to me that Cabri's last paragraph is the critical one. In the first sentence he says he does not wish to question the occurrence of AgTe (empressite as defined) as a mineral, then goes on in the second sentence with the premise that it is premature to redefine the name in its original meaning rather than continue the erroneous application to the phase $Ag_{5-x}Te_3$. As pointed out above I fail to recognize any "irregularities" in my data used to redefine either phase, but certainly would agree that the mineralogy of the silver tellurides is not completely understood. This is pointed out by Cabri's addition of new data on the gamma-phase and by his statements regarding some of his synthetic products. I do contend, however, that empressite (AgTe) as originally defined is a valid mineral, and my understanding of the rules of synonomy is that priority requires the name be applied to material of that composition. Further, application of the name stuetzite to the phase $Ag_{5-x}Te_3$ would seem to satisfy the rules of synonomy since undoubted type material was shown by Thompson *et al.* (1951) to be identical with synthetic material of that composition. It appears to me that the redefined names are eminently more desireable for two obviously different minerals than the "empressite" and "empressite (?)" that have been in the literature for the same length of time that empressite has been applied to the phase near Ag_5Te_3, or than the "empressite I" and "empressite II" that have appeared more recently.

REFERENCES

(Same as in listing following Cabri's discussion, p. 799, 801)