Observational Criteria for the Classification of Mississippi Valley-Bleiberg-Silesia Type of Deposits

(An attempt at a brief summary)

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First, I would like to congratulate the organizing committee for promoting the discussion on mineral deposits in the Alps.

One of the groups of deposits are the Pb-Zn-fluorite deposits of the Mesozoic carbonate province. I thought, it might be of interest to point again to their great similarity with the Mississippi Valley deposits. Also, I feel an answer to the paper by Brown in the June 1970 issue of MINERALIUM DEPOSITA is needed. Yet, many papers so far presented at this meeting and many papers which appeared elsewhere (both, in the Journ. of Econ. Geol., and in MIN. DEP.), were actually direct answers to many negative points of Brown's paper.

At first, it may perhaps seem redundant to write another summary on the Mississippi Valley-Bleiberg-Silesia- or M. B. S. type of ores. Yet, a close look at the New York Symposium (Brown, ed. 1967), and especially at the recent attempt of a summary by J. S. Brown (1970) shows that many misunderstandings are hard to die, and a separation between facts and interpretation appears to be most problematic. Indeed, it would be easy to get discouraged in view of the fact that observations of very simple sedimentary features are ignored and suggested interpretations are said to be based on no observations even though they are printed or pictured on the same pages. As pointed out elsewhere, it is disappointing indeed to see how the genetic interpretation of ore deposits in some instances still rests on pre-Darwinian patterns of thought. These are of course entirely subconscious. The best way to get out of them is probably a continued discussion of observations which illustrate the logic of geological relations, and not to ignore them. Because of this situation and for the reasons just given, a new attempt is made to summarize the essential characteristics of the so-called M. B. S.-type of mineral deposits.

The term Mississippi Valley-Bleiberg-Silesia (M. B. S.) is preferred to the restrictive term Mississippi Valley for various reasons. First, the environment in which stratabound Pb-Zn-barite-fluorite-(Cu-Co-Ni)-deposits of sedimentary diagenetic traits are forming, are not restricted to continental platforms. Second, they are not restricted to one geological period or to one continent. Third, in certain environments, they grade into Kupferschiefer or red bed, or massive sulfide deposits. Fourth, they range all the way from absent to positive volcanic-exhalative affiliation. They are observed in undisturbed as well as in metamorphosed and folded terrains.

Naturally the type name could include many more locality names in many continents, but this would not be convenient. The three names proposed here represent perhaps a "happy medium". Also, it is probably not very useful to apply only geotectonic differences for subdivisions in a classification of ore deposits.

Brown suggested, at the beginning of his paper, a wide and unclassified variety of criteria for classifying a deposit as of the Mississippi Valley type. At the end of his paper, he turns around and suggests to apply Pb-isotope ratios only. He wants to exclude all deposits which do not have J-type lead.

With regard to the unclassified collection of criteria, I should like to suggest a simple, but systematic set of criteria. These criteria ought to be equally useful for exploration as for theoretical work on the genesis. I have described many of them previously and will therefore only repeat the most essential traits.

Despite the diversities between the Pb-Zn-deposits of the Alps and the Mississippi Valley deposit just mentioned, there are many common traits. These may be summarized as follows:

a) **Regional scale:** An essential characteristic of most M. B. S. deposits is their "omnipresence" in carbonate provinces, both on continental platforms and along geosynclinal belts. As again shown by some authors of the New York Symposium on these deposits, the carbonate province extending from Alaska through Canada and the Middle West down to Texas and Mexico, contains hundreds of economic and non-economic deposits of this type. On the North American continent, this Paleozoic sedimentary province extends also to the West, for example the North of Washington State (Metalline District) and to the East (Appalachian equivalents, such as the Tennessee deposits in the same Paleozoic sediments).

Identical descriptions from literally all sedimentary carbonate provinces can be given with comparable, though not always equal quantities of the same stratabound base metal sulfide deposits. Consequently, this coincidence in space and time with a "host-rock" may be called large scale congruence. In the French terminology, these sedimentation environments are the regional or continental metallotect for the M. B. S. type of deposits. One of the best known areas is the Pb-Zn-Belt of the Alps.

b) On the **local** i. e. mine and outcrop **scale,** the M. B. S. type deposits coincide with such paleogeographic features as coast lines, bottom highs ("haut fonds"), both with or without a clearly developed reef facies, or with other zones (displayed by rhythmic sedimentation).

Many of these congruencies have been listed and pictured in the summary of Nicolini (1970). These again may be called metallotects of a second type. Most papers on such deposits which pay more than a passing attention to the wall rock, described such metallotects in many details and show an almost perfect degree of congruency between ore features and the named sedimentary structures. Again, the book by Nicolini is specifically strong in regard to features on this scale. With regard to the Triassic of the Alps Maucher & Schneider (1967) have presented many important observations.

c) On the **handspecimen scale** a large number of typical sedimentary features have become known in M. B. S. ores during the past ten to fifteen years. Again, only some typical ones can be named, which are common to all or most of the deposits of the M. B. S. type.

The simplest ones are bedding features, such as simple layering, cross-bedding, ripple marks, associated stylolites, stratigraphic pinch-outs, swells, slumpages, intraformational breccias, load casts, etc. (compare the frontispiece of Larsen & Chilingar, 1967, and several other figures, e. g. figures 4, 6 and 7 in Amstutz et al., 1964, and figure 7 in Amstutz and Bubenicek, 1967).

More rarely, but statistically significant (because of a congruence of frequency of occurrences with and without ore), are such special features as sedimentary dykes with or without associated mud volcanoes, mud cracks, karst horizons, submarine erosion channels etc. (compare Park & Amstutz, 1968; Amstutz & Park, 1967; Maucher & Schneider, 1967; Park, 1969; Zimmermann, 1969a & b; Zimmermann & Amstutz, 1971). A very neat fossil mud volcanoe in Cambrian beds with sulfides in the Mississippi Valley province was described by this author for example in 1967 (figures 17 and 18).

Again, the congruencies on this level are astonishing and a most attractive field of research, which was neglected until about 12 years ago.

d) The **microscopic scale** has played a special role in the recent re-interpretations, especially within the Mississippi Valley itself (compare the papers by Amstutz et al., 1964, and Amstutz & Bubenicek, 1967, Park, 1969). On this scale the diagenetic crystallization and recrystallization sequences showed with perplexing clarity that even on this small scale and even down to very delicate details, perfect congruencies exist between the normal common mineralogy and the same sediments containing ore minerals. In this connection, the reference to the papers quoted should suffice (compare also Park and Amstutz, 1968; and Amstutz and Park, 1967 and 1970/71).

After this very brief summary of criteria which may be of use in the classification of M. B. S.-deposits, I should like to underline some prerequisits for a good hypothesis in ore genesis:

First, interpretations should not be made on evidence from one scale only; second, none of the congruencies are a priori more important than others; third, chemical or compositional evidence also works with congruencies (histograms, phase diagrams, etc.); fourth, the compositional (chemical, physicochemical) and the geometric (textural) evidence should be used "at par", i. e. none should be overrated; and finally, an interpretation or theory should be considered to be a working hypothesis, because our interpretations are all subjective, i. e. full of cultural, which means historical and geographical relativity; and we need the next younger generation or colleagues with a different cultural background to tear us loose from our own idiosyncraces, our own subconscious ties to stiff dogmas. These bonds have a different character and lie in different fields in each generation.

Now with regard to the suggestion of Brown at the end of his paper, to classify the Mississippi Valley type of deposits only according to Pb-isotopes: It appears somewhat peculiar, in principle, to use a property of unknown nature as a criterion for classification. Why not use the known properties? Also, one should not be satisfied with the result of the "J-type pattern", i. e. with words such as "undatable, futuristically anomalous lead" (Brown, 1970, p. 117). We want to know the reason for the anomalous nature. This reason must lie in so far unknown factors of isotopic fractionation. Here is an interesting open field for investigation. On the ground of a perfect match of the textural evidence on all four scales, we suspect that the fractionation factors are of diagenetic age. Whether the originally available lead was also different or not, in the three different areas, is equally unknown. Therefore, we are presently investigating the isotopic ratios of lead with different diagenetic histories and from different facies paleogeographic environments. Combined with fluid inclusion data, we may perhaps obtain an answer and eliminate the enigma of the J-type lead.

At many places of Brown's paper, one finds statements which, to an economic geologist trained before about 1960, sound like reasonable objections, but to a sedimentologist are not acceptable. This only illustrates the enormous gap in the traditional education of the so-called "economic geologists". (After presenting the first paper on sedimentary features in sulfide deposits at the Annual Meeting of the SEPM in Dallas in 1958, one of the leading sedimentologists came to me and said: "Finally an economic geologist is getting interested in sedimentary textures. Please, let us have your paper for publication"). But this gap still has to be closed in some universities, despite the fact that by far the largest proportion of today's base metal deposits occurs in sediments.

Some of these statements in Brown's paper are:

1. On replacement (p. 115—116): "... many European geologists simply cannot accept in the light of their conviction that no positive evidence has yet been developed to prove that the geopetal features on which so much stress is laid could not be merely pseudomorphs from later replacement, or results of solution and settlement in lithified rocks long after diagenesis". In view of such statements, it is hard to refrain from saying: "please look closer". An old Missouri slogan says: "I'll believe it when I see it", and if you look close enough, what do you see? You see details which in part have been published — but obviously not often enough: one sees first of all no textural evidence for postdiagenetic replacement. And if there is a replacement, it has been shown on the grounds of ample sedimentological evidence that it is of diagenetic age. Also, this replacement affects only specific crystal phases and not complete nodules. But

the positive evidence for a diagenetic age is abundant. In figures 4, 6, 7 and Plate II, and again in figure 9 in Amstutz et al. (1964) and figures 3, 4, 5, 7, 17, 18 and Plate II, and other figures from the Mississippi Valley or similar sedimentary provinces in Amstutz and Bubenicek (1967), the textures display an obvious diagenetic crystallization sequence. This is seen on the basis of many converging patterns, such as the decreasing idiomorphism of later ("overgrown") phase, the fracturing of earlier phases (pyrite-marcasite); the filling of the breaks by later phases (e. g. chalcopyrite or galena or late carbonates); the load cast formation by the agglomerations of early phases (marcasite-pyrite nodules), which are obviously denser than their surrounding matrix rich in clay minerals and carbonate ooze.

If this evidence is not positive enough, one wonders how J. S. Brown would like to define the term positive. It is certainly very negative for any epigenetic mimetic replacement theory. In addition, other logic approaches (observations and analogies, compare Amstutz, 1967/69) make an epigenetic interpretation most improbable if not impossible.

The results of the fluid inclusion work are used as a strong hold of the epigeneticist. There is hardly any justification to this, because of the following reasons: The temperatures known to exist during diagenetic stages certainly approximate those found by the inclusion work. The difference between the average of these temperatures is not essential enough to cast any serious doubt on a diagenetic age of the sulfide crystallization, inasmuch as the understanding of the process of inclusion formation is still somewhat incomplete. The methods of fluid inclusion work and the detailed observations have reached a remarkably high level; but it is naive to believe that sophistications of interpretations always stay in pace with that of the observations. To mention only some questionable steps in the interpretations given in some papers; it is highly improbable that the volume of fluid and gas is representative, if the concentrations differ so strongly from those known from pore solutions during the early and medium periods of diageneses. Consequently, the heating temperatures may require certain corrections. It is hoped that the weight of all the other criteria speaking for a diagenetic age of sulfide crystallization will prompt more work on the unknown factors involved in the inclusions of trapped fluids. A detail to be investigated more closely is the surface chemical effects of the trapping of fluids. Obviously the concentrations present near the surface of a crystal and in slowly shut off cavities may be different from those in pores. Also the time of formation of the cavities (e. g. during the diagenetic crystallization or the recrystallization process) is important. Consequently, the textural position (paragenetic generation) of the sample is important and any fluid inclusion data without accompanying petrographic information are not of much use.

Before closing, I should like to make a remark on the use of the terms syngenetic and epigenetic. Rather than to ban these terms, they ought to be used more carefully in the literature. Both terms are quite useful, if the reference in space and time is given. This means, these terms have to be given in relation to something else. Also, we have to realize that petroleum geologists and sedimentologists use the term in a different way; for them, diagenesis is already epigenetic.

I simply propose and repeat here that the use of the two terms is perfectly in order, if and when the space and time relation is given. Let me illustrate this briefly with two examples:

— The sulfidization of fossil plants is certainly epigenetic with regard to the tree growth; it may also be epigenetic with regard to the transportation and the beginning of the decay. However, it may be syngenetic in regard to the burial and diagenetic fossilization.

— The same is true with regard to the sulfidization of mafic minerals in crystal sands around fumarolic activity.

The determination of "syn" versus "epi" is normally one of congruence versus non-congruence, as pointed out many times elsewhere.

In conclusion, and also in answer to the lengthy discussion by Brown, I would like to suggest the following:

Regional, paleogeographic, and local concordances or congruencies between the ore and the country rock are not "minor facets of the evidence", as Brown states. They are rather the main criteria in exploration and even those which have made the new lead belt in Missouri many times more important than before 1958. These regional and textural criteria also served as the best exploration guides in the Alps, as we have seen in many interesting papers in this Symposium.

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SUMMARY

An attempt is made to give a systematic list of genetic criteria for a classification of Pb-Zn-Cu-(Ni-Co)-Ba-Deposits of the Mississippi Valley--Bleiberg-Silesia type of deposits.

Four classes of criteria are given starting with the regional scale and proceeding to the outcrop, handspecimen and microscopic scale.

DISCUSSION

Petrascheck: I'dont feel perfectly happy if the alpine lead-zinc deposits, alpine in the geographical sense, are parallelized with the epicontinental type of the Mississippi-Valley-Upper Silesia-Cévennes in Southern France. I think there is a fundamental difference. The alpine Pb and Zn deposits are Triassic and occur in a geosyncline. Maybe a miogeosyncline, but anyhow it is not epicontinental. We have a clear submarine volcanism, we have thick series, and so without any other conclusions I think it would be better to distinguish this paleogeographic environment.

Amstutz: Thank you for this interesting question. Professor Petrascheck has mentioned three terms. One, epicontinental versus geosynclinal, two, the facies and three, the volcanism. I agree with the regard to the first point, the geotectonical difference. About the third one, we are not sure yet. In the Mississippi Valley, as a matter of fact, in the center of the Mississippi Valley mineralization, there is in the same age, in the Cambrian, a volcanic explosion crater and volcanic tuffs. This fact has been kept secret for about ten years by the St. Joseph Lead Company, but we now know that volcanism occurred. So this is not a real difference. Now with the regard to the second item, the facies, I don't think that we find big enough differences to use it as a differentiation.

Uytenbogaardt: It does not completely belong to the lecture but can Prof. Amstutz tell us in a few minutes something about the lead and zinc of the MBS type of deposits?

Amstutz: The first part of the answer is something which I always emphasized in Missouri to my students: that I believe that this is not the first question to ask. It is much more important to look first at the deposit and to develop the geometric and geochemical criteria for a mode of deposition. This will help us then to answer the second question: Where did the metals come from? Now, in regard to Missouri, or rather all the Mississippi Valley type deposits, we don't find any channel-ways. So this already points to other possibilities and it is also very likely impossible to have it form in a way in which Brown suggested in his book on ore genesis. The question, where did the lead came from, has to be answered in the next few years. I don't believe it is such a terribly important question because if you make a geochemical balance over these huge provinces including, of course, the sandstones and the shales in them, the total Pb and Zn which you get is not higher than the average Pb and Zn which is reported as an average for sediments. Why is this so? The concentration of these elements follows similar lines as the concentration of Ca, Si and some other elements. The average sediment composition is much more differentiated, than that of the igneous rocks. If you take the average igneous rock, you always have SiO_2 between 35 and 70 %, or even narrower. But if you take the sediments, the concentration of the SiO_2 in the average limestone is very low, and in the average sandstones it is very high, and this we can say for practically all elements.

So I am not astonished that we also get very pure PbS beds, in the Lead Belt for example, or here in the Alps, or very pure ZnS beds. So I am not astonished at that, and I think we can derive in most places these metals as a product of erosion which travelled in suspension to the oceans. The next step we don't know yet, but there are five laboratories at least working on a solution for it. What we would like to know during the next 5 or 10 years are the factors which lead to a concentration of the dissolved or adsorbed metals in preferred areas. And as soon as we know them, we may also be able to say more about the origin of the elements. I think they can be of an exhalative or a purely erosional origin. To me this question is not so important as to some other colleagues.

Maucher: Zu Ihrer letzten Frage nach der Herkunft der Erze möchte ich mich auf den Standpunkt von Herrn Amstutz stellen. Es ist gar nicht so wesentlich, wo das Material herkommt, und ich glaube, daß man diese Frage gar nicht eindeutig beantworten kann. Es wird Lagerstätten geben im karbonatischen Milieu, bei denen die Metalle aus reinen Verwitterungslösungen kommen, und es wird Lagerstätten geben, bei denen sie aus salinaren Lösungen kommen, und es wird Lagerstätten geben, bei denen sie aus vulkanischen Lösungen kommen. Hier wird wahrscheinlich der wesentliche Unterschied zwischen den epikontinentalen und den geosynklinalen Lagerstätten liegen. Man muß also erwarten, daß die ganzen Untersuchungen über die Herkunft der Metalle sehr verschiedene Ergebnisse geben werden. Es wäre völlig falsch, sich darüber zu streiten. Das Wesentliche ist die Frage, was ich schon mehrfach gesagt habe, des Milieus, in dem die abgelagerten Erze angereichert werden. Dies ist die erste und wichtigste Frage. Die Frage, wo die Lösungen her sind, ist in Wirklichkeit die sekundäre Frage.

Wenn wir uns über eine Erzlagerstätte unterhalten und über ihre Genese, dann wollen wir ja wissen, durch welche Vorgänge die erhöhte

Stoffkonzentration an dieser Stelle entstanden ist. Der Vorgang der Konzentration ist das Wesentliche, nicht der Vorgang der Zufuhr, denn die Zufuhr allein bedeutet noch keine erhöhte Konzentration. Wenn ich von einer Lagerstätte als »syngenetisch« spreche, darf ich das nur tun, wenn der Konzentrationsvorgang syngenetisch, das heißt gleichzeitig und auf dieselbe genetische Art und Weise abgelaufen ist wie die Bildung des umgebenden Gesteins. Epigenetisch darf ich nur etwas nennen, dessen Konzentration nachträglich, nach der Entstehung des Umgebungsgesteins erfolgt ist. In der Diskussion werden »syngenetisch« und »epiginetisch« meistens auf ganz verschiedene Dinge bezogen und gar nicht mehr auf den Vorgang der Stoffanreicherung in der Lagerstätte. Daher kommen die großen Mißverständnisse. Wenn wir immer nur vom Konzentrationsvorgang sprechen würden, dann wäre der Fall der Syngenese oder Epigenese in seiner Definition sehr klar.