

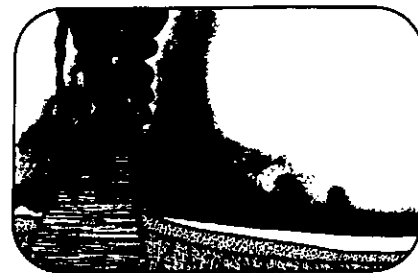
the biota must be assumed to have occurred more than once over wide regions of the crust, nevertheless, there was every reason to believe that few if any of the events suggested can have been truly world-wide, thus allowing continuity in, at least, some forms of life. One paper examined the effective area necessary to cause extinction on a world-wide scale, and concluded that the world biota was remarkably resistant. If a catastrophe was to extinguish a high proportion of the marine biota, then it would have to be almost totally world-wide in its effect. Such an event may have arisen only on a very few occasions during the existence of life on earth. The bulk of the discussion on the biological record was confined to the K-T boundary changes, and this is unquestionably the best studied horizon from the point of view of determining the likelihood of a large body impact as a cause. Nevertheless, it was pointed out repeatedly that the Late Cretaceous was a time of broad environmental deterioration and of graded extinction throughout the last several stages. It was claimed that the total extinction of the widespread rudistid reefs took place before the end of the Maestrichtian, and pre-dated the "boundary clay" by some one and a half million years. The massive extinctions in marine plankton, however, are firmly linked to the boundary event. Consensus, if any, appeared to favour the idea that there was certainly some very sudden event at the close of the Maestrichtian and that some form of extraterrestrial triggering mechanism might well have been responsible. Coupled with the fact that the "boundary clay" is of a composition that suggests a greatly enhanced supply of extra-terrestrial material, including the iridium anomaly, and is accurately associated with the severe marine plankton extinctions, the hypothesis is certainly well supported.

An eight man panel led a discussion on the last afternoon. Many of the points made in this brief review were covered. Highlights include the importance of finding "signatures of interaction". The problem is to find geological evidence, in the broadest sense, for extraterrestrial intervention in terrestrial evolution. It is right to tackle the problem from both ends - that is to try to work out the effects of impacts that are known to occur, and at the same time to try and find evidence in addition to the already impressive list of craters that have been demonstrated, many of which date from the relatively recent geological past. We must accept the frequency and size distribution, but the geological evidence remains baffling - with the exception of the iridium anomalies. Not all signatures will be the same

in every case. The driving force of biological evolution is environmental change, and during the last 30 years we have become increasingly aware of the dynamic nature of the earth and the influences acting upon it both internally and externally. The new uniformitarianism may have to absorb yet one more major mechanism that has evidently been interfering with the "normal" earth processes that we have accepted until now. Biostratigraphy may prove to be the most important correlation tool available but must be based on many faunal and floral groups. Within such a scheme all apparent rapid extinctions of unrelated forms must be suspect and will require exhaustive examination both in regard to taxonomic and biomass changes. A result of the Snowbird Conference is that those who attended it will take each other more seriously in future. From a geological point of view we must be prepared to accept the accidents that our planetary colleagues are pressing on us; just as they must accept the geological evidence in regard to the scale of the effects.

Abstracts of the papers were printed and distributed before the meeting as Lunar and Planetary Institute Contributions No. 449 with Supplement. Arrangements have been made to publish most papers as a Special Paper by the Geological Society of America.

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A Critical Appraisal of the Applicability of Recently Developed Data and Theories to the Search for Volcanogenic Massive Sulphide Deposits

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New Exploration Guides for Kuroko-Type Massive Sulphide Deposits

The full-day symposium entitled "New Exploration Guides for Kuroko-Type Massive Sulphide Deposits", sponsored by the Mineral Deposits Division of GAC at the May, 1981 annual meetings in Calgary presented the practical results of a three-year "U.S.-Japan-Canada Cooperative Research Project on the Genesis of Volcanogenic Massive Sulphide Deposits". An overview of the project was presented by its organizer, Hiroshi Ohmoto (Pennsylvania State University), and was followed by ten papers on a variety of topics pertaining to exploration for Kuroko-type deposits in Japan and elsewhere. The research was funded by the National Science Foundation (U.S.), Japan Society for the Promotion of Science (Japan) and Natural Sciences and Engineering Research Council (Canada). National leaders of the project are Ohmoto (U.S.), Ei Horikoshi (Toyama University, Japan) and Steven Scott (University of Toronto, Canada).

The symposium attracted an audience of about 400 most of whom were from the exploration and mining industry. Scott and Ohmoto organized and chaired the sessions. Financial support for participation by the Japanese speakers was provided by NSF, GAC (Robinson Foundation Fund) and Dowa Mining Company. The following is a slightly

revised text of the paper presented at the conclusion of the symposium by G.W. Mannard as a summation and critical appraisal.

Editor

Introduction

The volcanogenic massive sulphide deposits have always fascinated economic geologists. The literature is full of lyrical phrases describing the fineness of their laminations, the beauty of their breccias, the symmetry of their zoning. At their best, they represent truly remarkable concentrations of base and precious metals. This characteristic makes the Kuroko-type deposits attractive to the miner, as well as to the geologist, although this has not always been so. Prior to the 1930s, many massive sulphide deposits could not be exploited profitably because of inability to separate the intergrown copper, lead and zinc minerals they contained. The development of the technique of differential flotation by Asarco and others solved this problem, and the volcanogenic massive sulphide deposits have been an attractive exploration target ever since. Throughout this presentation, I use the terms "Kuroko-type" and "volcanogenic massive sulphide" interchangeably. I have never accepted that there is any substantial difference between the Miocene Kuroko deposits and their older cousins in the Archean.

Compared to most other types of mineral deposits, many of the volcanogenic massive sulphide deposits have a high metal content and a large number of potential products - copper, lead, zinc, cadmium, tin, precious metals, iron and sulphuric acid. These properties give them a relatively high unit value. Mining and milling of small quantities of ore can yield large profits - a highly desirable characteristic in these times of increasing energy and labour costs. A high-grade massive sulphide deposit, which lies in such a way that it can be mined by the open pit method, can yield a remarkably high return on investment.

Current Exploration Situation

In the glaciated regions of the world, most outcropping volcanogenic massive sulphide deposits have been discovered. In unglaciated areas, discovery has been delayed by difficulty in recognizing the surface expression of deeply weathered sulphides. This problem is now being resolved, and several deposits have been found recently in Australia and Southern Africa. It seems likely that within the next 10 years, all outcropping deposits will have been found, as well as a large

proportion of those which lie within 100 metres of the surface and are readily detectable by geophysical methods.

All of this means that, in the very near future, we will be called upon to explore at depth, where our main weapons will be geological projection and rock geochemistry. Recent discoveries by Dowa, deep in the Hokuroko basin, and Falconbridge, at 4500' in the Noranda camp, show that we already have some measure of capability. As the search becomes more and more difficult and costly, the exploration geologist will come under increasing pressure to provide reliable guidance for deep exploration drilling, in situations where drilling costs \$100 to \$150 per metre, or say, \$200,000 per hole! He will need parameters to help him recognize prospective geological environments, and to aid him to locate specific targets within the complex mineralized systems once they have been found. Who will provide these parameters? In order to answer this question, it is necessary to review the development of current theories concerning the volcanogenic massive sulphides and to discuss the "state of the art" of geoscience research on these deposits.

Historical Review

Prior to the 1950s, there was no widespread understanding of the origin and nature of volcanogenic massive sulphide deposits. During the 1940s or even earlier, isolated individual geologists in Scandinavia, Central Europe, Japan, Canada, and Australia began to question the dogma of Lindgren hydrothermalism. It is my contention (disputed by some) that most of these dissidents were field-oriented geologists employed by mining organizations. In the early 1950s, the ideas of those pioneers began to be expressed in what was then considered to be a novel approach to exploration. In this approach, granitic plutons were disregarded, structure was relegated to a secondary role, and the emphasis was placed upon volcanic stratigraphy. Success followed in such areas as Noranda, Bathurst, and ultimately, in Timmins. The new movement spread to the government surveys and the universities, where it found convincing advocates. Slowly, the attention of the research community began to focus on the volcanogenic massive sulphide deposits.

Their first problem was to convince themselves that the minerals found in the volcanogenic massive sulphides could indeed form under sea-floor conditions, and that the massive sulphides were truly coeval with the enclosing rocks. Once this had been accomplished, investiga-

tions were aimed at elucidating the temperatures and pressures which prevailed during deposition, and the source and nature of the mineralizing fluids. As researchers moved more and more deeply into the physical chemistry of the ore-forming processes, they moved farther and farther away from the application of their findings to the solution of practical exploration problems. A community of laboratory geologists developed. Many of these scientists had little field experience, and it is not surprising that they became alienated from the exploration geologists.

Today, as we approach the period in which our ability to find deeply hidden Kuroko-type deposits will depend increasingly on close collaboration between exploration geologists and the research sector, it is obvious that we must close the gap which hinders effective communication between two communities. The problem has been recognized by several organizations, and efforts are being made to solve it. Symposia such as this one on new concepts in exploration for Kuroko-type deposits make a strong contribution in this direction.

The Current Situation - What is Needed?

Until recently, much of the academic research devoted to the volcanogenic massive sulphides has been too introverted and curiosity-oriented to be of much use in exploration. This rather bald statement is supported by two very recent publications. In a talk delivered at the prospectors and developers convention in March, R. W. Hutchinson (University of Western Ontario) described the situation very effectively, as follows:

"For decades, since its initial distinction as a sub-discipline within geological science, economic or mineral deposits geology has been self-centred, introspective and consequently perhaps, too narrow. Detailed studies of ore deposits dealing with their size and grade, mineral economics, mineralogy, textures, structural control, geothermometry, geobarometry, major and minor element content, isotopic geochemistry, etc., have been the rule. Unquestionably, all these have contributed to the knowledge of ore deposits and their significance is in no sense here discounted. Yet it is no longer adequate to investigate the many parameters of an ore deposit itself. Geological technology must be broadened in order to relate the orebody in proper perspective to its surrounding geological setting or environment. Ore must be treated as simply another, albeit economically unique, rock type. Its lithological setting, whether in igneous, sedimentary or metamorphic rocks must be considered. Its

stratigraphic relationships to its host rocks, both locally and regionally, must be evaluated. The tectonic environment under which both ore and host rocks were formed, and possibly deformed, must be interpreted. In short, a broader, geologically comprehensive or metallogenic approach to ore deposits geology must supplement the earlier, narrower, detailed studies" (Hutchinson, 1981).

A similar set of conclusions can be drawn from the preliminary analysis of responses to a questionnaire circulated by the Society of Economic Geologists, as reported by J. Eidel at the 1980 GSA Meeting (Eidel, 1980). Of the 250 respondents, 65 percent represent the mining industry, and 30 percent the academic community. The questionnaire, which was quite detailed, essentially asked, "What do we need the most?" The requests were overwhelmingly for empirical data. Among the items considered most needed were: alteration and zoning studies of the tops, bottoms, and lateral extensions or ore deposits (with emphasis on field studies and mapping); multi-disciplinary field studies of ore districts; and several other similar items. At the end of the long list, relegated to the category of "additional subjects", were such items as high altitude imagery, fluid inclusion studies, geochronology and petrophysics.

These two sources are sending us a strong and uniform message as to the type of research which is needed. I can add little to their statements, except for a few pragmatic suggestions about the constraints which should be kept in mind when the results of the research are converted into new guides for use in the search for volcanogenic massive sulphide deposits. Geological criteria are needed for the selection of generally favourable large areas which are the starting points for regional reconnaissance, for the identification of prospective stratigraphic units within these areas, and for the recognition of mineralized systems within these units. Once a mineralized system has been recognized, it must be relatable to a conceptual model which the exploration geologist can utilize to direct his efforts towards finding the maximum concentrations of metals.

To be most useful, exploration guides should be applicable in geological terranes which may exhibit wide ranges of age, structural deformation and metamorphism. Ideally, a guide should be broadly applicable, rather than being specific to a single district. Even more ideally, the guide should involve techniques which can be applied at reasonable expense. Methods involving highly

sophisticated analytical or interpretive techniques, which can be applied by only one lab or individual, quickly result in frustrating bottlenecks if they become widely applied.

Having made all of these generalizations, it is appropriate now to examine the exploration guides put forward in this symposium.

Appraisal of Exploration Guides

The first exploration guides presented derive from the failed rift hypothesis put forward by Cathles, Dudas, and Lenagh. The authors directed our attention to longitudinally rifted volcanic belts. This is exactly the sort of guide which, if valid, can be useful in the selection of broad areas for reconnaissance. Carried further, it can help us take a second step by confining exploration to the part of the stratigraphic assemblage which was deposited at the time of rifting. Together, these ideas constitute a useable sequence of guides. They are *new*, in the sense of being applied specifically to Kuroko-type deposits. Similar guides have been in use for at least a few years in the search for buried Keeweenaw-type and Zambian-type copper deposits and carbonatites.

Although Takahashi and Sato disagree with Guber and Ohmoto concerning the water depth during deposition of the Kuroko ores (1000m vs. 3500m), both sets of authors directed our attention to the dacitic cauldrons which appear to localize Kuroko deposits. Takahashi focussed particularly on the periphery of a single major cauldron, whereas Guber recognized several smaller ones. Location of such cauldrons by stratigraphic and structural studies, supplemented by geophysical observations, is obviously an important step in the investigation of a volcanic basin. Similar guides have been used successfully in older terranes such as the Noranda district; they are of limited use in highly deformed volcanic belts. Gruber's excellent illustrations of lithologic distributions documented the history of the basin. Takahashi and Sato also offered, but did not pursue, the intriguing suggestion that pre- and post-ore dacites may be distinguishable by having different H₂O and CaO contents. This difference, if established firmly and proven to be widely applicable, could also constitute a useful guideline.

Date, Watanabe and Saeki have provided us with a detailed and quantitative three-dimensional picture of the alteration zones surrounding the Fukazawa Kuroko deposit. They have also described the technique of preparing sets of "Halo Maps" for magnetic susceptibility and major

element rock chemistry which can be most useful in local situations where adequate data are available. The applicability of these guides outside the Hokuroku basin is open to question.

The papers by Urabe, Scott and Hattori, Green and Ohmoto, and Kalogeropoulos and Scott, described mineralogical, chemical and isotopic signatures which are diagnostic of the presence of various Kuroko-type ores. Most of these indicators are slightly to moderately more broadly distributed than the orebodies, and therefore can serve to enlarge the targets in situations where their spatial relationships to the orebodies are understood. Of the various indicators cited, some have been long recognized (e.g., chlorite chemistry) and others, such as oxygen isotope zoning, appear to be inconsistent and inadequately understood to be reliable guides at this time. However, used in combination, these indicators can be effective. Strong evidence was presented that at least some of them are valid in both Tertiary and Archean terranes. Particularly promising is the evidence that ¹⁸O depletions persists in the post-ore rocks above certain deposits, and the suggestion that this form of alteration can survive metamorphism intense enough to obscure mineralogical alteration patterns.

Similarly, the paper by Doe, Fehn, Farrell and Sato offered the hope that widespread haloes defined by element concentrations and isotope compositions of lead and strontium at the Fukazawa deposit may be broadly applicable as guides. Rubidium was also mentioned as having potential. The authors concluded that further work will be needed to establish the validity of these guides.

I must confess that I am incapable of judging the merit of Cathles' paper on the use of hydrothermal circulation models in exploration. My instinctive feeling is that the approach involves too many basic assumptions which may be unjustified in our present state of knowledge although Cathles' modelling does help to explain quantitatively the various geochemical haloes.

The paper by Cambell, Gorton, Scott, Franklin and Thurston described one of the least proven, but potentially most exciting, guidelines mentioned in this symposium. If preliminary findings that rare earth element geochemistry can distinguish productive from barren acid volcanics can be substantiated, a powerful exploration weapon will be added to our armoury. Again, a great deal of work will be needed to confirm the validity of the method.

Summary and Conclusions

In order to provide future supplies of raw materials which will enable our descendants to enjoy a standard of living at least equal to our own, an increasing pressure will be exerted on those who are at the leading edge of the search for resources. For mineral deposits researchers, this will require a shift of emphasis towards identifying and elucidating the factors controlling mineralization, and developing a better understanding of those peripheral effects of mineralization which can constitute evidence that ore may be found nearby.

The papers presented in this symposium are clear evidence of a welcome new trend in which mineral deposits research has shifted away from "science for the sake of science" towards "science in the service of mankind." We have been given a veritable feast of exploration guidelines, some proven, others whose validity remains to be confirmed. Equally important is the demonstration of what can be achieved by international collaboration in attacking geological problems. As a member of the exploration industry, I can assure you that these guidelines will be put to good use.

References

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Flow and Transport in Fractured Rocks

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On April 23, 1981, a one day invitational workshop on "Current Laboratory Studies of Flow and Transport in Fractured Rocks" was held at the University of Waterloo. This workshop was followed, on April 24, 1981, by a one day conference on "Flow and Transport in Fractured Rocks", which was sponsored by the Natural Sciences and Engineering Research Council of Canada. The workshop and conference were organized by W. Brace (Mass. Instit. Technology), J.E. Gale (Chairman of the organizing committee, University of Waterloo) and P.A. Witherspoon (University of California at Berkeley).

The purpose of the workshop was to bring together about 15 laboratory oriented researchers who are currently studying the flow and transport properties of fractures in order that they could compare experimental techniques, discuss their latest experimental results, and share their views on the adequacy, completeness and direction of current research programs. The workshop participants, in addition to the three members of the organizing committee, included T. Engelder (Department of Geological Sciences, Lamont Doherty University, New York) whose work includes studies of the effect of fracture roughness on flow through artificial fractures, H. Heard (Lawrence Livermore Laboratory, Livermore, California) whose work includes the measurement of key geophysical parameters and the permeability of both intact granitic rocks and fractured samples, R. Kry (Esso Research, Calgary) whose detailed field studies of the effects of viscosity changes on flow, around a borehole packer, through a single fracture indicates the importance that the oil and gas industry places on understanding the mechanisms

of flow in fractured reservoirs, J. Logan (Center for Tectonophysics, Texas A & M University) whose extensive work on both intact porous sandstones and samples of the same sandstone containing artificial (sawcut) fractures includes permeability testing under both hydrostatic and triaxial test conditions, W. Marine (Savannah River Laboratory, Aiken, South Carolina) who has been involved in an extensive series of field and laboratory studies of the hydrogeologic systems in the coastal plain sediments and underlying crystalline basement rocks and a series of low-permeability Triassic sedimentary rocks, P. Rissler (Ruhrtalsperrenverein, Essen, West Germany) well known for his careful laboratory studies on artificial fractures and detailed discussion of the basic laws governing flow in fractures, D. Snow (Colorado Springs, Colorado) well known to the hydrogeologic community in both North America and Europe for this theoretical contributions on fracture flow, R. Stesky (Erindale College, University of Toronto) whose research focuses on changes in geophysical parameters and the permeability of artificial fractures as a function of changes in confining stress up to 200 MPa, G. Thompson (University of Arizona) who has completed an exhaustive study of the characteristics of a wide range of nonradioactive tracers for solute transport studies, J. Walsh (Mass. Instit. Technology) well known for his theoretical contributions to the field of geophysics, D. Watkins (Lawrence Berkeley Laboratory, University of California) whose recent research deals with the laboratory study of the stress-permeability characteristics of natural fractures in cores ranging up to one meter in diameter, and M. Wigley (Battelle Memorial Institute) whose interest is in the potential of fractures in crystalline and sedimentary rocks to provide pathways for the migration of radionuclides from high level nuclear waste repositories.

Formal presentations at the workshop were kept to a minimum in order to ensure time for a reasonable amount of discussion on each topic. The workshop opened with a presentation by Peter Rissler on basic flow laws governing flow in fractures.

As pointed out by Rissler more care must be taken in conducting both field and laboratory flow tests. Especially important is the need to conduct an initial analysis of the raw data to determine which flow law or model is most applicable in its interpretation. The topic of laboratory testing of single