UNITED STATES DEPARTMENT OF INTERIOR

GEOLOGICAL SURVEY

A Discussion on the Assessment of the Mineral Resources in the Sherbrooke and Lewiston 1° x 2° Quadrangles, Maine, New Hampshire, and Vermont

Edited by Richard B. McCammon, with contributions by Robert H. Moench, Eugene L. Boudette, Frank C. Canney, Gary A. Nowlan, Leslie J. Cox, Wallace A. Bothner, John F. Slack, Stephen D. Ludington, Robert A. Ayuso, Carl Koteff, John P. Schafer, Wayne C. Shanks, III, and Howard A. Pohn

Open File Report 86-567

This report is preliminary and has not been reviewed for conformity with the U.S. Geological Survey editorial standards and stratigraphic nonmenclature.

1986

CONTENTS

INTRODUCTION

During the week of May 15-18, 1984, a group of U.S. Geological Survey geologists and other geologists from Universities in New England met in Reston, Virginia, to discuss the mineral resources in the Sherbrooke and Lewiston 1° x 2° Quadrangles, Maine, New Hampshire, and Vermont as part of the Conterminous United States Mineral Assessment Program (CUSMAP). The edited transcript of those discussions has been reproduced in this report.

The principal investigators responsible for the resource assessment were Robert H. Moench, Frank C. Canney, and Gary A. Nowlan from Denver, CO; Wallace A. Bothner from the University of New Hampshire, Durham, NH; Leslie J. Cox and Richard B. McCammon from Reston, VA. Other geologists from Reston who participated in the discussions were John F. Slack, Wayne C. Shanks, III, Norman L. Hatch, Jr., Jacob E. Gair, Frank G. Lesure, William F. Cannon, Michael P. Foose, Steven D. Ludington, Robert A. Ayuso, Carl Koteff, John D. Schafer, Howard A. Pohn, and Walter J. Bawiec.

The discussions centered on the major geologic and tectonic terranes identified within the quadrangles. The terranes identified were: 1) Cambro-Ordovician metavolcanic, 2) Ordovician-Carboniferous plutonic, 3) Jurassic volcanic-plutonic, 4) Paleozoic metasedimentary and 5) Holocene surficial sediments. Within each terrane, the ore deposit types most likely to occur were discussed, and for each deposit type, a set of recognition criteria based upon the available geologic, geochemical, geophysical, and remote sensing data were developed. These criteria were combined with the available field evidence to estimate the likelihood of occurrence of one or more deposits of each type within each selected area. The estimates were categorized according to degree of certainty which was expressed as probable, possible, and speculative. The probable and possible categories were based on a set of rules defined for each deposit type. The rules were incorporated in an expert system to aid in the mineral resource assessment of the area (McCammon, Richard B. and others, 1984).

Ore deposits having a speculative likelihood aof occurrence do not satisfy the rules defined for the probable or possible categories. However, it can be demonstrated that their geologic settings are analogous to settings elsewhere that contain ore deposits of the type being considered. Occurrence was defined as a minerals deposit above some specified minimum size and average grade above a specified depth.

The reader is referred to U. S. Geological Survey Open-File Report 84-650, map of the Sherbrooke-Lewiston area, Maine, New Hampshire, and Vermont (Robert H. Moench, ed.) for the location of areas mentioned in the discussions that follow.

The purpose here is to communicate to others engaged in regional mineral resource assessments about the process of regional mineral resource assessment. Of particular interest is the level of knowledge about the geology, the nature of the dialogue among geologists, geochemists and geophysicists, interpretation of data, and the means by which a consensus about the potential mineral resources of an area is achieved. The assessment of the potential mineral resources of an area is a complex process in which there are no fixed rules; rather, value judgements are made based on the available data and the current knowledge about mineral deposits types.

In the transcript, speakers are identified by their last name. Participants are referred to by their first name.

Editor's notes have been inserted in the transcript to provide continuity of the discussions.

CHRONOLOGY OF MEETINGS

TRANSCRIPT OF MEETINGS

Monday May 15, 1984

8:30 - 11 30 a.m. Discussion by Bob Moench and John Slack on geologic environments favorable for volcanogenic-associated massive sulfide deposits

Moench: We now have nearly all the data that we are going to get, so this is the time to apply what we know about the geology, geochemistry, geophysics, and remote sensing of the area to arrive at estimates of the potential mineral resources. This morning, we can begin with the resources in metavolcanic terranes. I will start off with a description of the extent and age relationships of the major metavolcanic units, and this will lead into a discussion by John Slack of stratabound massive sulfide deposits of volcanic association, ore deposit models and the characteristics of rock assemblages that constitute hosts for known districts in the northern Appalachians and perhaps elsewhere.

Since the beginning of the project, I have focused mainly on the volcanics in the Milan and Old Speck Mountain quadrangles, north to Canada and southwest to the Littleton-Moosilauke area. In an attempt to get some grasp of the potential of volcanic assemblages for occurrences of massive sulfide deposits of different kinds, during the mapping I tried to keep a few broad basic attributes in mind; namely those features and rock assemblages, based on the current thinking, that would provide the best environments for the development of massive sulfides. This includes, first of all, thick originally permeable assemblages of predominately volcanic rocks, such as pillow basalts and tuffaceous volcanic pyroclastics or mixtures of the two; thick enough to host a hydrothermal circulation system. Second, you need some kind of heat engine to drive the circulation system and a fair depth of water in order to precipitate minerals on the seafloor, rather than in the conduits. In the Kuroko District in Japan, calculated depths of water are on the order of three and one half kilometers. It does not necessarily have to be that deep however. Third, there needs to be evidence in the volcanic assemblages of hiatuses, that is, breaks evidenced by sedimentary layers or evidence of very slow deposition that would provide enough time for a deposit to form on the seafloor. Fourth, an overall extensional tectonic environment is necessary in order to maintain an open fracture system that could host a hydrothermal circulatory system. Those are just some of the things I kept in mind in the course of mapping. Superimposed on that is evidence of mineralization in the form of hydrothermal alteration preserved in the metamorphic rocks; in basalts, for example, evidence of depletion of calcium and sodium shown by the presence of gedrite and other low-Ca, alkali-absent amphiboles;

and in felsic rocks, expressed as pyritic quartz-muscovite schist, with some phlogopite or, if you happen to run across it, which is not often the case in this area, a zone or pipe of intense hydrothermal alteration. Based on these features, a belt of rock could be categorized as probable, possible or speculative potential for the occurrence of massive sulfide deposits. In the process of arriving at these potential estimates, we need first of all to develop a set of attributes starting off with what John Slack has to say about massive sulfide deposits, and then deciding how we can apply them. We can add the geophysics and geochemistry and then go from area to area assigning to each area a potential resource category with respect to the occurrence of massive sulfide deposits. Later on in the week, we will go into the plutonic terranes including the White Mountain plutonic rocks of Jurassic-Cretaceous age. Why don't we now turn the floor over to John Slack.

Slack: I have provided you with a short paper or summary that Bob Earhart prepared for the open-file mineral occurrence models booklet that the Survey put out a couple of years ago. (Erickson, R.L., 1981). Several other people contributed models, but his was the most general in terms of field recognition and characteristics without getting into details of chemistry or specific deposits. I have a number of other things that I would add myself and that is what I have summarized on this sheet which you have before you. As briefly as I can, I will go through what I call recognition criteria, supplementing them with some of the other handouts which are diagrams taken from various papers that illustrate the points, mainly rock associations, alteration patterns, and so forth.
The material has been gleaned from the literature and from people's talks, some of which are unpublished. You will not find a lot of this in any one place. It is important to note the association of some of the unusual lithologies that are commonly interpreted as metamorphosed alteration packages, and whether they are discordant or concordant. I will go into this shortly. As Bob mentioned, the main criterion is a thick sequence or pile of submarine volcanic rocks with minor to abundant amounts of associated meta-sedimentary rocks. In most cases, these are epiclastic sediments and in other cases, minor amounts of calcareous or dolomitic sediments. The carbonate component is generally minor.

McCammon: How thick is thick?

Slack: I think on the order of half a kilometer, 500 to 1,000 meters, something like that.

Moench: I would say that is the minimum.

Slack: Right. In other cases, there is a substantially thicker pile. In a lot of these belts that are tectonically affected one way or another, it is hard to put together a true thickness of those kinds of sequences because of folding and thrusting. No sulfide deposits are known in subaerial volcanic rocks so another principal recognition criteria is establishing a submarine environment. That is not always easy. In some cases, one can find pillow structures or something that suggests submarine deposition but mostly it is an inference. Another criterion many people use is evidence of pyroclastic activity as opposed to general extrusion of lavas. Pyroclastic material may range in composition from mafic to felsic or silicic and includes tuffs and tuff breccias, agglomerates, or very coarse pyroclastic rocks. There may also be associated fragmental felsic domes that are probably largely subvolcanic but may have been partly extrusive in their upper parts. Because of deformation, these may range from fairly pristine angular fragments in a felsic matrix to more or less fine-grained quartz porphyries in more metamorphosed terranes. It is not always easy to identify these and discriminate them from, for example, felsic tuffs or felsic flows. We can look at the papers in the handout which is from Plimer (1978). There are two figures, the bottom of which is a ternary copper-lead-zinc diagram. This is a broad generalization of proximal and distal massive sulfide deposits. It has some problems as it relates to proximal sediment-hosted deposits but it is generally applicable, especially in volcanic-associated deposits, in showing the relationship of proximal copper-rich ores, in some cases having lots of gold, with abundant or more abundant volcanic material generally near a submarine volcanic vent, as opposed to more distal ores which tend to be of a different composition, in many cases more lead-zinc-rich with silver, barite, various manganese minerals, and also associated with more abundant clastic sediments.

Moench: Do you think that applies to the Pecos mine where you have a good domal setting and right on the flank of that dome is lead-zinc ore which would indicate a proximal setting?

Slack: That is why I was alluding to the fact there are problems that many people now recognize that it is not necessary, nor it is not always true that a feeder zone, if present, is coincident with a volcanic center. Recent studias in the Kuroko district in Japan have revealed that the rhyolite domes there did not precede the generation of the massive sulfide deposits but came later so the association is not accidental, although they may have used the same conduits. The domes were more or less in place after the massive sulfides were extruded so that the association of the domes with the massive sulfides is almost the opposite of what one would expect in terms of a cause-and-effect relationship. Also, the recognition in the Kuroko district that virtually all, if not all, the ores there are transported in someway or another so that the association of copper-rich ores with the feeder zone may be an artifact of tectonic transport or instability on the seafloor. The general model is broadly applicable, but I want to stress, and Bob did also, the many exceptions to that, especially in sediment-hosted terranes. The bottom ternary diagram show some general metal relationships with those proximal and distal

deposits and it is true that gold tends to be more abundant in these cupriferous deposits which, in many cases, are thought to be proximal. Let's move to a more detailed figure, which is an idealized cross section of a Kuroko-type deposit showing the relationship of the massive sulfide ores to the edges of a brecciated rhyolite dome. This was done before the recent Kuroko research so it predates this new recognition of time relationships between the domes and the massive sulfides, but it does show these discordant feeder zones in the dome and the associated barite and gypsum, that is, sulfate-facies of the ores. A lot of people in the past have gone into a volcanic terrane looking for domes and thinking that that was the main area to be targeted, and yet there are many exceptions where there are no domes known and some fairly large deposits. This is particularly true in the Proterozoic of the Canadian Shield. Another is Bald Mountain in Maine. As far as I am aware, people have told me there is no subvolcanic felsic dome recognized there yet chloritic feeder zones are present in a calc-alkaline pyroclastic sequence. Here, then, is a good example of a large massive sulfide deposit in a proximal volcanic setting without associated rhyolite or felsic domes. So there are exceptions to that model. Another is the Noranda district in Canada. If you look at the next figure, it is an idealized reconstruction of the Vauze sulfide deposit in Noranda. It shows a rhyolite dome with massive sulfide lenses on the flank of the dome.
There are quite a few deposits in the district that are spatially related to these felsic domes. You can also see at least a suggestion there of a constrast which is common in many of these volcanic deposits, for not only the domes but the massive sulfides to be along lithologic contacts commonly between felsic and mafic volcanic rocks and in some cases with sediments. This is a fairly persistent feature so that a good generalization is that the massive sulfides tend to occur along lithologic breaks in the volcanism between dominately felsic and dominately mafic volcanism. In most cases, the hangingwall sequence is mafic, whereas the footwall sequence is felsic, commonly pyroclastic with tuffs and tuff breccias. Turn now to another figure. This is a section that shows a number of deposits including the Millenbach that occur at several stratigraphic horizons in the Noranda district. Several of these have been mined in the past. Millenbach has been mined out recently. You can see the discordant stringer zones or alteration zones that extend into the footwall of these deposits. In some cases, for instance, from the Amulet Lower A to Upper A deposits, the stringer zones continue into higher stratigraphic levels in spite of deposition obviously of overlying volcanic rocks.

Pohn: In this figure and the previous one, there are features that are parallel to the present surface.

Slack: You mean the plan of the present surface?

Pohn: Yes, there are levels that seem close to being parallel and a fairly large angle to where you would expect deposition to take place or enrichment to take place, and the same thing is true here on the figure you are talking about. There are at least some dikes that seem to be coming through that parallel to the present surface. I'm trying to determine if the whole complex has been tilted.

Slack: I think I see what you mean.

Pohn: If this was the original surface, this is the way the units would be aranged and now you got these dikes coming through that are parallel to the present surface.

Slack: It has been tilted though. Anyway, people have gone with the notion that the place to look for massive sulfides is the same stratigraphic horizon as the other occurrences or deposits in one area, and yet the Noranda district is a striking example of where this has not proved to be the case because just in the last 10 years or so, a number of economic deposits have been found in other stratigraphic levels in the district, namely at lower and higher levels where these alterations zones developed and then were reactivated at a later time at the same focus. You see an example of that between the Lower Amulet and the Upper Amulet deposits.

Moench: That is a good example of an ore guide you go with until you can unravel the stratigraphy.

Slack: Right. It is the same as the rhyolite dome concept where you need to have a rhyolite dome to indicate a volcanic center and, therefore, the deposits have to be near the rhyolite dome. In metamorphosed terranes, I would rather use other criteria that I will discuss in a minute that are equally important. I do not want to dismiss rhyolite domes--they are important--but they are not consistent targets on a worldwide basis. Also, they are hard to recognize. There are other field criteria that are more easily recognized. Going back to the main recognition criteria, another principal criterion is the evidence of chemical sedimentation. These are fairly distinctive rock units that most people can generally recognize in the field. They are not always laterally persistent but they clearly indicate some syngenetic chemical sedimentation. At least that is how I interpret them. In many districts, they are interpreted to be time-stratigraphic facies of the massive sulfide ores at the same stratigraphic horizon, or broadly so. In most cases also in the hangingwall, although they are not perfect stratigraphic analogs, they are in the immediate stratigraphic hangingwall of deposits. Examples include cherts (called banded tuffites) coticules, iron information and tourmalinites. Some of these may be easy to miss in the field. A lot of cherts look like quartzite or sugary quartzite when they recrystallize and these are not always obviously hydrothermal cherts at least in field identification. Some of these banded tuffites may have chemical compositions similar to keratophyre. Norm Hatch has seen one at the Davis mine. They may be partly mixtures of exhalative and volcanic components, but when not recrystallized they tend to be fairly cherty and fine-grained. Coticule and iron formation as well as tourmalinite can occur in sequences that do not have massive sulfide deposits. Clearly, they are not one-to-one guides, but they do indicate chemical sedimentation which is thought to originate from hot springs on the seafloor, so that they are broad guides to hydrothermal activity in a volcanic sequence. An illustration of this can be seen on the geologic map showing the Hawley mineral belt in northwestern Massachusetts. This map shows a number of occurrences of massive sulfide deposits associated with other types of chemical sediments, especially iron formation, at various stratigraphic levels. There are also a number of coticules. The best example in the sense of manganese-rich, largely carbonate sediments, is that of the Betts mine in the southern part of the belt. I interpret these based on mapping by Hatch to be several stratigraphic levels of chemical sedimentation that extend from the upper Moretown through the Hawley Formations. The masses of these chemical sediments occur where the map pattern of the Hawley Formation is the thickest. One interpretation of course is that this is tectonic thickening. There is a good chance, however, that it is a primary volcanic thickening related to a felsic volcanic center.

<u>Hatch:</u> There are a lot of coticules to the south.

<u>Slack:</u> Sure, but I'm saying we should restrict it more to the sulfides and the iron formations, and also to the north as indicated by that one prospect in Vermont. In the central part of the Hawley belt, there are some very coarse volcanic agglomerates. There is ore preserved in less sheared and less deformed areas between the two major massive sulfide deposits, Davis and Hawks (Mount Peak), so I think there is a potential there in spite of the deformation. There is a correlation between the distribution of massive sufides and iron formations with an original volcanic edifice. That may be one thing to look at. Also, there may be more of a felsic component to the volcanism in that part of the belt than there is to the north and south. Would you agree to that, Norm?

<u>Hatch:</u> In the thick part, sure.

<u>Slack:</u> Some people have suggested one way to look for that sort of feature is to actually construct isopach maps of thicknesses of felsic volcanics in various belts as a possible guide looking at the areas closest to the volcanic centers. That is one possible approach.

Monday, May 15, 1984

1:00 - 4:00 p m. Continuation of Monday morning's discussion

Slack: Moving on to the next main recognition criterion is evidence of hydrothermal activity. This will depend on a variety of features or parameters which include the protolith, whether it is felsic or mafic or calcareous or dolomitic or siliceous. Obviously the greater the metamorphism, and also in some cases whether it is contact or original metamorphism, so that the mineralogical affects one sees are not evident in the same volcanic sequence as a function of varying degrees of metamorphism and the lithologic nature of the protoliths. Nevertheless, there are a number of distinctive mineralogical associations and rock types that are reliable guides to hydrothermal alteration. These stand out in many districts. It has been demonstrated that the alteration associated with the generation and placement of massive sulfides are discordant and interpreted as alteration pipes stratigraphicly beneath the ores. Others are broadly conformable or semiconformable and may extend for several kilometers lateraly away from the deposits. In most cases these are in the stratigraphic footwall of the deposits. I do not think this is widely recognized. However, Jim Franklin, from the Geological Survey of Canada, gave a talk here several weeks ago and he described a number of these lithologies which are traceable for kilometers at the same horizon in the footwall away from the deposits. Such features provide bigger target areas in terms either of knowing where to look or for evaluating mineral potential for massive sulfides in a given area. Let's consider some of these features. Taking these in no particular order, first there is chlorite and talc shist. One doesn't always see the talc but chlorite-rich rocks are very common because of the magnesium metasomatism associated with the seafloor alteration, commonly as discordant bodies in the footwall of the deposits, in some cases with a fair amount of talc.

The second is cordierite-anthophyllite rocks or cordierite-anthophyllite-gedrite rocks. These are evidence of premetamorphic magnesium metasomatism but in contrast to the chlorite-rich schists that may be or commonly are in discordant footwall alteration zones, cordierite- anthophyllite rocks or gedrite rocks are in many areas laterally persistent for quite a distance--say hundreds to thousands of meters--away from the deposits. Naturally those rocks with cordierite would occur at higher metamorphic grades; you would not see cordierite in a greenschist-facies sequence.

Next are staurolite-rich rocks. These are not commonly recognized. They may be identified in the field but not recognized as possible hydrothermally altered rocks. The same is true for chloritoid-rich rocks. One example is the Mattabi deposit where there is a semi-conformable chloritoid-carbonate rock that occurs 2 or 3 kilometers on either side of the main deposit in the stratigraphic footwall. This is a distinct lithology attributed to hydrothermal alteration. These rocks are chloritoid-bearing in composition but the chloritoid and staurolite are iron-rich silicates indicative of iron metasomatism prior to metamorphism. Also, Al-rich silicates are indicative of (residual) Al-enrichment. That is the present interpretation. In one of the handouts, there is a diagram illustrating the footwall types of alteration in the Mattabi mine which Franklin worked on. These show varying types of alteration from sericite immediately underlying the massive sulfides and chlorite underlying it, as well as to zones of siderite and dolomite farther out. That is not to say the rocks are entirely siderite or dolomite. It is interpreted to be a series of alteration minerals superimposed on the underlying lithologies which, in that case, are felsic tuffs. Beyond this there is quite a bit of chloritoid and dolomite. More recently, it has been shown that there is abundant chloritoid underneath the sericite zone in the immediate footwall. Moench mentioned sericite and that is another commonly recognized key indicator. Another that is rarely or not widely known is plagioclase-rich rocks. These appear to be more common in sediment-hosted deposits, the ones, for instance, Jack Gair and I have looked at in Ducktown and the Gossan Lead, and the Sullivan in British Colombia. There are a number of volcanic-hosted deposits, particularly in Scandinavia, that have in most cases footwall zones which are composed mainly of plagioclase. They look superficially like a felsic volcanic or rhyolite or graywacke even, yet they are largely sodic plagioclase. These are attributed to seawater alteration of volcanic rocks in the sequence. They may be discordant as veins or lenses in the footwall. The Joma deposit near the Norwegian-Swedish border has a lense of plagioclase rock in the stratigraphic footwall. These are important rocks that tend to be extremely close to the massive sulfides spatially, and yet they may be very easy to miss in the field. Below that, I mention kyanite-sillimanite-rich rocks. These are also in many cases very close to the massive sulfide deposits in the immediate wall rocks, as a function of alteration forming clays that have later been metamorphosed to kyanite or sillimanite at varying metamorphic grades. There is a good documentation of these kinds of lithologies in many of the pre-Cambrian massive sulfide deposits in Canada. It is interesting that Nancy Pearre (Lesure), and Jim Calkins in their New Hampshire map, and also in their Vermont map but (especially in New Hampshire), note several kyanite and sillimanite prospects not associated with plutons in volcanic belts in western New Hampshire. I do not recall exactly where those are, but I think they are significant and someone should look into their distribution.

Moench: How much kyanite or sillimanite are you talking about, 50 percent?

Slack: I think more than 10 or 20 percent, probably 30 or 50 percent, something like that. If they were prospects they probably would have at least 50 per cent. They are also good targets for gold. Many of the kyanite-andalusite-sillimanite deposits in the southern Appalachian of the Piedmont have quite a bit of gold associated with them. In the pyrite there is a lot of gold. This was not even known until about 5 years ago. Sulfide-rich rocks in that column of course are also pretty obvious, disseminated sulfides or veinlets of sulfides as clues to proximity to deposits. Another one only beginning to be recognized is bleaching or silicification of mafic rocks. Unfortunately I do not have illustrations from Noranda. I have an example, though, on on the same figure as the Amulet deposits. Notice the feeder zones coming up through the Amulet rhyolite. It is really not rhyolite at all, but a basalt. This was not recognized as such until just a few years ago. The unit is so silicified that the chemical analyses generally come up to rhyolite. Yet they have been able to demonstrate just in the last few years that away from the massive sulfide deposits this is actually a basaltic andesite or basalt. It's been strongly silicified both where it is a flow, and in some cases where it is a tuff or tuff breccia. There are varying degrees of silicification of matrix and fragments to near complete alteration in that conformable zone not only in that conformable zone but peripheral to it. One example outside the mine area is to look for mafic or intermediate-composition volcanics in which there is silicification or bleaching of fragments as an indication of alteration. I will finish here with a few indicator minerals that are fairly easy to identify in the field and also are diagnostic in pan concentrates as guides to favorable massive sulfides terranes. These are gahnite, spessartine, tourmaline, barite, magnetite, and apatite. You can, of course, get magnetite in almost any pan concentrate. Getting a whole abundance where you don't expect it, for example mostly in a felsic volcanic sequence, you might wonder whether this is accessory magnetite that one might expect from metamorphosed amphibolite. Barite usually breaks down and does not show up much in pan concentrates as a residual mineral. It should be fairly easy to identify in the field, however, because of its specific gravity. Gahnite is distinctive with its green color. Spessartine is a good indicator of massive sulfide deposits, for example, being abundant at Broken Hill, although it is not really associated with volcanics. Tourmaline is another, especially when they are magnesium-rich. I mentioned apatite, but fluorapatite is another good indicator. Fluorine is becoming recognized as an anomalous element in many alteration zones of massive sulfide deposits. It generally resides in apatite so fluorine-rich apatite or an abundance of fluorine-rich apatite may be important.
Now I would like to show you some slides.
Most of these are from the Noranda district, eastern Ontario, near the Quebec border.

(First slide) This is one of the felsic tuff breccias very near the massive sulfide deposits in the general zone of favorable stratigraphy. The texture is monolithic; clearly, an indication of coarse pyroclastic felsic volcanism.

(Next slide) Here is what I was aluding to earlier, another felsic fragmental rock, but there is silicification. Actually I'm not so sure, this might be an example of a more intermediate composition, but there is silicification around the rims of these fragments. When we were there, they were mentioning this bleaching or silicification of fragments as a hydrothermal effect, so there is something to look for if things are not too badly deformed. Some of you may have heard of mill rock. This is the mill rock, Sangster's mill rock.

(Next slide) This is a fragmental felsic dome which is peripheral to one of the biggest massive sulfides in the whole district, perhaps the biggest massive sulfide, the Horn deposit which is now largely mined out. You can see the angular nature of these felsic fragments. I think it is largely rhyolitic in composition. That is the view from that outcrop across the lake to the Horn mine and mill.

(Next slide) Here are some outcrops in the district very near the massive sulfides that crop out at the surface, showing pillows in which the rinds have been replaced, partly by disseminated sulfides. To the left of that coin is disseminated pyrite and chalcopyrite outlining the pillows pointing to the left. They are standing pretty steep. That is another feature, one does see in the footwall, not only discordant veins and veinlets of sulfide, particularly copper-rich chalcopyrite, but also sufides that may occur along the rims of pillows. This is another distinctive rock, dalmatianite, which occurs in the footwall of the alteration zone of those deposits. It is a spotted rock. Unfortunately, it is limited to contact aureoles in which younger granitic plutons or stocks have provided a thermal aureole to heat up the magnesium rich-rocks to produce the spotted dalmatianite, which are magnesium-rich cordierite rocks, probably originally chlorite schists which have been thermally metamorphosed.

(Next slide) These are some underground shots of Millenbach which is one of the major deposits found along that same principal horizon which you saw in an earlier figure. These are views underground of some of the massive sulfide, banded sulfides. That is a lens cap there for scale. It is a typical feeder zone directly underlying the deposits developed in fragmental basalt. You can clearly see the anastomosing veinlets of pyrite. There is a lot of chalcopyrite as well.

(Next slide) Here is a pyritic, laminated stratigraphic unit that directly overlies the main Millenbach deposit. Lateral to that at the same stratigraphic horizon, sulfides disappear. You get this very cherty silicous banded rock. They call this key tuffite. It persists stratigraphically for hundreds of meters, more than a kilometer at the same horizon. They are good stratigraphic guides to where you are in the section and favorable targets for massive sulfides.

(Next slide) A little closer to home is this view of the Harborside mine in coastal Maine, just to show you an example of the distribution of some of these alteration zones relative to massive sulfides. It shows an unusual sequence of rocks, talc-chlorite, talc-carbonate rocks, and massive talc developed in a volcanic sequence composed of pillowed andesite or basaltic andesite. There are rhyolites and rhyolite breccias and various tuffaceous felsic volcanic rocks present. There is an amazing amount of carbonate and maganesium-rich minerals here, as evidenced by the chlorite and talc, in spite of the fact that you are largely in a felsic volcanic sequence. There had to be a large amount of carbonate and magnesium introduced into that system, particularly because locally there is very little mafic rock.

Moench: That is interesting, on a regional basis, because one of the criteria for alteration in metamorphosed rock assemblages is minerals that are depleted in calcium and sodium. Perhaps this is where some of the gold might occur.

Slack: Right, Jim Franklin was saying it is certainly widely recognized in the volcanic-hosted deposits, speaking strictly of volcanic deposits, the footwall zones, both discordant pipes and concordant units, are depleted in sodium, but now it is recognized in Canada that stratigraphically beneath, maybe the next unit, only a few hundred meters below, there may be huge amounts of sodium, as well as chloritoid-rich units. People just have not looked much below the immeditate footwall unit. Now they are starting to do mineralogical and chemical studies at lower levels. I think this will tell us where other things came from and where other things went.

(Next slide) This is a view of the rhyolite dome at Haborside which is directly adjacent to the massive sulfide, showing some spectacular bleaching and silicification of fragments in the vicinity of the deposits.

Moench: Do any of the carbonate rocks contain scheelite?

Slack: There, I have no idea. It is a possibility.

McCammon: Do these hydrothermally altered rocks tend to be mappable units here or are these local units that you find in and around deposits?

Slack: I think some are mappable and some are not. I would say that the first two are definitely mappable. Based on what Franklin said, they are mapping the chloritoid-carbonate and sericite-rich rocks up to kilometers in length. The others are probably not mappable except on very detailed scales. Probably the best mappable units would be the first four, excluding the staurolite-rich rocks, although that is a possibility.

Moench: How about a staurolite-garnet-chlorite unit?

Slack: How much staruolite is in it?

Moench: About 20 percent.

Slack: Really! How far is it mappable?

Moench: Staurolite actually comes and goes. It is more of a garnet-chlorite rock. It is a rock without alkalies or calcium, lots of iron, and magnesium.

Slack: Is it in the footwall?

Moench: It would be a host. It is the ore-bearing zone.

McCammon: Is there at least one of these hydrothermally altered rocks in the vicinity?

Slack: Yes. I think the most common ones are probably chlorite and sericite. There are a lot of chlorite- and sericite-rich rocks that are not known to be associated with massive sulfide deposits in a lot of terranes. Whereas these cordierite-anthophyllite rocks show a good correlation especially in Proterozoic deposits of the Canadian Shield with massive sulfide deposits. There is one deposit in the Canadian Shield that has a regional cordierite-anthophyllite rock which you can see and in tracing out in complex folds, that can be more or less be traced into the massive sulfide zone. In fact, Doug Rumble was telling me last week that he and Frank Spear were working on chlorite-anthophyllite and gedrite rocks right around a deposit in the Mt. Cube quadrangle. They mapped out quite a ways away. Very interesting boundaries. I don't know whether the metamorphic grades will go higher there.

(Ed. Note: This concluded the general discussion about recognition criteria for volcanogenic massive sulfide deposits. The discussion now turns to geologic settings of specific areas in the Sherbrooke and Lewiston 1° x 2° Quadrangles)

Tuesday, May 16, 1984

8:30 - 11:30 a.m. Discussion of geologic settings in Sherbrooke-Lewiston area favorable for the occurrence of volcanogenic-associated massive sulfide deposits

Slack: Cambro-Ordovician volcanic belts in the Sherbrooke-Lewiston areas and in Glens Falls as well, are favorable as opposed to Gile Mountain-Waits River types in eastern Vermont, which are sediment-hosted. You are right in terms of different classifications of Kuroko-type, Cyprus-type, and Besshi-type. People talk about Noranda-type and all these others, some of which are not really applicable although the Cyprus-type may be analogous to Chickwolnepy in some way. It did not get that far up, did it?

Moench: The Chickwolnepy is a gabbro-tonalite-sheeted diabase complex. It is subvolcanic to either the Ammonoosuc or to the next units up in the Silurian. I am not sure. Based on the chemistry, it looks as though it might be younger than the Ammonoosuc. That is still to be worked out. It is not big enough anyway to constitute a major source.

McCammon: It takes on an importance as to whether or not you want to devise a set of criteria that includes this type of deposit or whether you want to separate out early on and develop a set of criteria for more than one deposit type.

Moench: This is going on in my mind too as to whether we want to subdivide the areas on the basis of individual deposits types of volcanic association.

Slack: What are the criteria that are applicable to sediment-hosted type deposits such as the chemical sedimentation of the hydrothermal altered rocks? You find these in sediment-hosted environments, whereas thickness of the volcanic pile and evidence of pyroclastics are volcanic-associated features.

Moench: Certainly, pyroclastic activity is a positive indication whether deposits are going to occur right at the volcanic center or somewhere off the periphery. If you have the coarse pyroclastics, you are in a proximal environment.

Slack: With the exception of Cyprus-type deposits or ophiolite-hosted deposits, I do not know of any massive sulfides of any significance that occur in purely mafic lavas without any evidence of pyroclastic activity, mafic or felsic. Either there was a development of pyroclastic mafic activity, or, more commonly, a felsic component. A general geologic environment is not applicable to ophiolite-hosted deposits. You can only rely on pyroclastic material as a criterion.

Moench: There is evidence of chemical sedimentation. This would be the hiatus that permits enough time for a deposit to form plus the evidence of hydrothermal activity along the seafloor.

Slack: I am inclined to assign chert and iron information as major criteria and the others as minor criteria.

Moench: Sometimes the chert and coticules occur together. We have lots of banded white cherts that are very coticule rich. Or there can be siliceous maganiferous iron formation that can be cherty magnetite rock or magnetite bearing chert. For instance, you have the Franconia iron mine.

McCammon: Is that in the Lewiston Sheet?

Moench: Yes.

Slack: Bob, didn't you also map numerous localized iron formations?

Moench: Yes, magnetite-bearing iron formations.

Slack: That is important.

Moench: I have doubts about the Ammonoosuc Volcanics of Lisbon belt because there is such a vast thickness of massive,pyroclastic tuffs; this might be an area of high potential for lots of little deposits, but there wasn't enough time interval between the individual tuffs to produce a large deposit. On the other hand, in the same sequence there are places where there is a cutoff and you go up-section into rusty, cherty rocks. Here you would have a combination of enough thickness to host a circulation system, plus evidence of a volcanic hiatus and chemical sedimentation. You need evidence of a hiatus.

McCammon: Is that like at Bald Mountain or is there any evidence of a hiatus there?

Slack: There is a red jasper overlying the unoxidized portion of the orebody.

Moench: Anything immediately underlying it that would indicate a hiatus?

Slack: I don't know. The enclosing wall rocks are coarse, mostly felsic pyroclastic tuffs and tuff breccias. It is immediately overlain by an altered basalt, an alkaline basalt.

Moench: Well, we have coticule all over, many different kinds of it; in the Perry Mountain, for instance.

Slack: I would put that as a minor category unless it is sulfidic. If you had a sulfidic coticule, most all of the ones I have ever seen in New England with the exception of those near the Elizabeth mine and a few other places, have no sulfides whatsoever. I do not know if Norm Hatch would agree with that.

Hatch: Once in awhile, they were associated with black shales.

Slack: I mean actually in the coticule.

Hatch: Oh! in the coticule itself.

Moench: Tourmaline is something I have not really seen.

Slack: They tend to be more associated with sediment-hosted deposits.

Moench: But certainly we have the iron information, the cherts, the bulk of which may be manganiferous and garnetiferous. So we have these three criteria so far. Evidence of hydrothermal alteration is certainly a major criterion. Its recognition has many shapes and forms, such as broad areas of pyritic quartz muscovite schists which evidently were parts of hydrothermal systems.

McCammon: Bob, will this be included as part of your description of the geologic units?

Moench: Yes.

McCammon: As opposed to being mapped as units themselves?

Moench: No, with one exception over here (Milan quadrangle), a little unit of garnet chlorite rock that is the host rock of the Hampshire Hills prospect; there's not enough to map anywhere else. You certainly see evidence of hydrothermal alteration. My intention is to prepare an overlay showing zones of hydrothenmal alteration superposed on the lithologic units.

McCammon: That would be the way to handle it.

Moench: Again, indicator minerals (tourmaline, barite, etc.) might be something that would be picked up in the heavy mineral concentrates. It is not something that anyone was really looking for.

Canney: We have been able to do relatively little in the examination of the pan concentrates. The data simply are not available.

McCammon: How about barium?

Moench: If we were starting out the project now, we might, with all these varied surveys, we'd know what to look for and maybe we'd come up with a whole lot more, but without this information in hand right new we can hardly use it unless barium shows up in the chemistry.

Boudette: Actually, industry has been turning to barite as an ore guide.

McCammon: Is that right? Is it one of the pathfinders?

Boudette: Yes. Barite is a very powerful indicator of favorability.

McCammon: Was there barite around Ledge Ridge?

Canney: Gary Nowlan has a plot of barium from the stream sediment data.

Boudette: It should be at Ledge Ridge.

McCammon: Is barite at Mount Chase?

Boudette: I do not know the details about Mount Chase.

Slack: I am scheduled to go there in August so maybe I will find out. The only bedded barite I know of anywhere in New England is in the Hawley belt which is at the Hawley iron mine. On the dumps you can find coarse layered barite that is 90 percent barite granules.

Gair: Have they been examining the heavy minerals for tourmaline and spessartine?

Canney: No, we only have the chemical analyses.

Moench: Again, we have to stick with the criteria that we think we have. Barium in association with a volcanic belt could be an indication of at least a minor criteria. Anything else we want to add to the majors?

McCammon: Remember when you started out this morning, Bob, you ran through what you thought were the major criteria.

Moench: Yes, two include a thick pile, sufficiently permeable to host a circulation system, and a heat source possibly indicated by proximal pyroclastics, or maybe some intrusive at depth that could generate a hydrothermal system. I havn't recognized much in the way of contemporaneous intrusions, possibly excepting one one on the crest of the Bronson Hill anticline; this is a laminated rock probably of tonalitic composition, possibly a sheet of tonalite that was deformed and caught by a cleavage-producing mechanism (pressure-solution). There is no particular pluton that I could point to as being the source of driving a hydrothermal system. With the evidence of a hiatus, and the evidence of an mineralizing process in the form of hydrothermal alteration, ground is covered except for the overall extensional environment that permits the system to operate.

Canney: Would you want to do anything about the geochemical major, for example, does it has a high concentration of base metal values from the sediments?

Slack: How high do you recall, roughly?

Canney: The thing of it is on geochemistry for these massive sulfides. I am amazed really how low some of these base metal values are. For example, at Ledge Ridge only 2 of the streams actually are anomalous; I think they have a high 63 ppm copper and also zinc values of around 2 - 300 ppm. These levels are anomalous. This is why I have Boise-Cascade's permission to put out a short paper suggesting that when you employ drainage, geochemistry in the search for massive sulfides, don't neglect small anomalies. At least give them a second look. Lead, for example, hardly moved at Ledge Ridge. 40 ppm lead in stream sediment is anomalous even though you got .9 percent lead in the orebody. It seems to have only moved a few hundred feet from the orezone; yet here we have several million tons of massive sulfide sitting on top of a mountain (Gair, J.E., and Slack, J.F., 1979). You are going to have to look at the overall picture. Gary will be showing some maps a little later for the Ledge Ridge area where he plotted the 90th percentiles. Where you have a concentration of several of these in a area higher values may be an indication that the area has some potential.

McCammon: What you are saying is that the values are important if they are high relative to neighboring values, regardless of their absolute values?

Canney: Sure.

Moench: If they occur in the right geologic settings, then you have a positive indication.

Canney: Look at Ledge Ridge. The 60 ppm copper that was picked up in 1966 by Ed Post's geochemical mapping in the Maine project could have resulted in the discovery of the orebody if this value had been classified as being anomalous.

Moench: Don Grybeck said the Arctic deposit in Alaska was found on the basis of one 70 ppm copper analysis. They went up the ridge and drilled a hole into one of the largest massive sulfide deposits that has been found. No indication at the surface.

Canney: For some reason, these types of deposits are not ozidizing. We did a number of water surveys at Ledge Ridge and you would think several million tons of sulfide are on top of the ridge would be shedding ore metals or sulfate ion all over. Yet our data revealed mostly background values, just a very few and possibly anomalous values for zinc. You get only 5 to 6 ppm sulfate which is strictly background. The pH is in the normal range from 6.4 up to about 7.3.

Gair: Is there a lot of nonsignificant 60 ppm copper too?

Canney: This again was based on statistics specifically from areas of volcanic terrane where you might expect to find massive sulfides. The mean copper value is around 16 ppm. Alan Heyl told me he was in the Bathurst district New Brunswick when they first stripped the overburden from one of the Brunswick orebodies. He said the surface was like a mirror, the glistening massive sulfide had apparently not oxidized at all. I saw the same thing on a mass of pyrrhotite in the Moxie gabbro where I bulldozed a nickel-cobalt zone. There were tons of brighten oxidized sulfides right at the surface. Why this is, I am not sure. In many areas, these glacialed soils are overlain with a lot of organic matter, so it's possible that most of the oxygen in the rain water is being consumed by the organic matter with rain percolates through the soil. Consequently, you have reducing or at least nonoxidizing condition from the time the groundwater encounters the massive sulfide.

McCammon: Wouldn't that make these types of deposits good buried conductors if they were unoxidized?

Canney: Yes.

McCammon: Do the companys use geophysics in these terranes?

Boudette: They had a geophysical target you wouldn't believe.

McCammon: Where was this?

Boudette: Bald Mountain.

Canney: But the absence of geochemistry is not a negative factor.

Moench: That applies to everything except the volcanics. If you do not have the volcanics, this would preclude the occurrence of massive sulfides.

McCammon: You are saying that is a requirement?

Moench: Certainly thickness is, maybe a little less than 500 feet minimum. I am not sure how you would weight the other major criteria.

Slack: I would tend to weight the hydrothermally altered rocks fairly high because cherts and iron formations can be at similiar stratigraphic levels but a long distance away. Most people would say that rocks interpreted to be metamorphic alteration zones tend to be fairly close, meaning within a kilometer, as opposed to several kilometers. For iron formations and cherts, I would weight the iron formations higher because of their proximity to known deposits.

Moench: Let's look at the geophysics to see how it bears on the assessment. We have aeromagnetics and gravity.

Bothner: Gravity won't help us very much unless there is very substantial volumnes of very dense mafic rocks. It cannot help much in distinguishing felsic volcanics from the bulk of the metasediments or from the granite bodies. Magnetics will be helpful providing hydrothermal alteration has not destroyed the magnetite and ilmenite. The right kind of volcanics will show up as linear anomalies.

McCammon: Wally, how about distinguishing thick verus thin volcanics?

Bothner: We gain information on volume measured principally by the anomaly shape.

McCammon: Could you produce a map of thickness volcanics?

Bothner: Possibly, if you used the character of the anomaly made on an estimate the kind of prism that would account for what we observe.

Moench: It seems that the geophysics we have doesn't bear on the massive sulfides.

Canney: How about iron formation?

Moench: If we had closely spaced aeromagnetics for the area, maybe we would pick up iron formations.

Slack: Was it at a half mile, Wally?

Bothner: Yes. It would take a substantial amount of iron formation. The best we can do is to characterize the volcanic belts and the major intrusuve centers. Bodies that are a kilometer in diameter or kilometer wide we could characteristize.

Moench: Anything to add at this point?

McCammon: Does Howard Pohn's lineament map tell us anything about the permeability or general activities of hydrothenmal systems?

Moench: In the massive sulfide volcanic belts?

Pohn: I have two generations of maps here. These are the maps generated from the radar images. The other images were generated from the satellite images. You can use them if you want. My interpretation should fit both sheets. I have marked in yellow wax pencil the strong northwest lineament patterns that we see. There are a couple of different symbols on here, one of which is a little blue dot in the middle of the lineament. These directions are close to the glaciation direction as seen by striae taken off New Hampshire and New England maps. The purple dotted lineaments are zones where the lineaments are coincident with parts of mapped faults. The Landset map is on a much coarser scale because we are looking at a much poorer resolution from the satellite image. We have some broad brush strokes as you can see, regular lineaments on the Landset map. We also have some very broad lineament zones or strong trends.

Bothner: What are those broad brush bands?

Pohn: These are ones where we were seeing poorly defined lineaments. We don't have sharply defined edges of outcrops or sharp edges of ridges. The zone runs through the entire image. I noticed in looking at the radar data that there were areas in the north where we mapped a flurrie of lineaments. These flurries coincided with what I think are the larger mapped faults. Some areas might be a map fault that stopped. If you extended the fault, it coincides with additional lineaments. The lineaments and faults are connected. I don't know what. We may see fractures that are parallel to or removed from the faults. The fault patterns are mapped as Acadian or Alleghenian. Most of the lineaments we mapped coincide with compression faults.

Moench: We have brittle faults of different ages. These represent weakness produced by brittle faulting, slip cleaving, one thing or another, or some other kind of brecciation.

Hatch: A premetamorphic brittle fault would get healed in metamorphism and no longer retain its selvage. These are presumably brittle features.

Moench: To address the question on lineaments and how they apply to the stratabound massive sulfide deposits, sediment or volcanic-hosted, it doesn't; for lineaments, it applies to the evaluation of deposits related to brittle fractures, such as tin and uranium in the White Mountain Batholith area.

Pohn: In the White Mountains, it is essentially lineament-free. We looked thoroughly. I felt remiss because I was not finding enough lineaments that satisfy the overall pattern.

Bothner: Are there many that emanate from the centers?

Pohn: No.

McCammon: Howard, are you prepared to make derivative maps, like density of lineaments or something like that?

Pohn: No, we thought about using Gary Raines' lineament analysis program. We just don't have enough lineament intersections to do a reasonable job on that. You need about 3 to 4 times the number we have here.

McCammon: So, this is as far as you are going to interpret?

Pohn: The statistics are proving out what you can see anyway. There are definite areas of high concentrations. Those are the ones which would be logical locations, particularly if they fit other criteria for mineral deposits. If these are Triassic, we have to assure ourselves that they weren't initiated in the Ordovician or pre-Ordovician and reactivated in the Triassic.

Moench: I remember listening to a talk about the Kuroko massive sulfide associations (1983 National GSA). There was somebody who drew a correlation between regional lineaments and the occurrence of the districts. The districts correlated with lineaments that extend across Japan.

Pohn: It is a statistical game.

Slack: A lot of people think that those basement faults maybe reactivated and actually localized the volcanic center or distribution of the centers characteristically at the intersection of the lineaments.

Pohn: If I can digress just for a moment, this is one of the things that I am finding in the central Appalachians that, over and over again, we find the areas where there are ramps and where the decollements change level are over basement faults. I have seen enough proprietary seismic data to know this is the case. Reactivation of old fault systems localizes mineral deposits.

Slack: You do not see circular structures in volcanic belts, do you?

Pohn: No. But there is some. The radar does not lend itself to a good rendition of circular structures except for Mount Monadnock which shows up beautifully. There are arcuate features. I am not sure.

Moench: We are at the stage of sitting down and looking at the maps and perhaps attaching a priority listing on the major criteria. As far as I am concerned, thickness of volcanics and evidence of hydrothermal alteration stand out in front.

McCammon: Let me first ask, which of these, if absent, would preclude the presence of a deposit?

Boudette: Thickness of volcanics certainly.

McCammon: It is exclusive. If you do not have thickness, you do not have a potential mineral resource.

Slack: There is another model for Elizabeth however.

Moench: How about evidence of hydrothermal activity? If you do not recognize it, it is a question of extent and thickness. In an area of poor exposure, you may never find any evidence.

Slack: True, you are more likely to observe pyroclastics.

Canney: Still, it is a requirement for mineralization.

McCammon: In terms of probable or possible, to me probable means that it is prospective. It means that you would expect industry to look at it if they have not looked at it already. It satisfies the criteria for the occurrence of a particular deposit type. Do we mean that to be considered prospective requires that all five criteria be recognized or does it mean that volcanic thickness plus evidence of hydrothermal activity plus alteration or what?

Boudette: Industry would prospect without evidence of hydrothenmal activity being known. They will not rule it if they have no information. However, it is a requirement for mineralization.

McCammon: You could make this a requirement. If it does not meet this requirement, it drops to the next lower category.

Boudette: Having both then increase the likelihood of occurrence.

McCammon: In effect, these two plus maybe any other criterion are sufficient. You are saying three majors with volcanic thickness and being evidence of hydrothermal activity required.

Slack: If you had a kilometer of anthopyllite, it might not be very attrative for anyone.

McCammon: So you might want four majors, at least four out of those five.

Slack: No, I am thinking three I guess. The geochem is marginally effective. I would say three majors.

McCammon: Any three, assuming these were two out of the three would be considered prospective.

Boudette: You probably would not have much chert and iron formation without the pyroclastics.

Slack: I would say any three as long as volcanic thickness was one of them.

Boudette: You have to have thickness of volcanics. We do not want to have someone spend their exploration money on 500 meters of greenstone.

McCammon: Oh, so you want to make two requirements.

Boudette: There are pyroclastics and greenstones too.

Slack: True. If you have fine grained mafic tuff that has been metamorphosed to greenstone.

Boudette: Jim Pond is thick, with thick units of pyroclastics, so that alone does not do it. So we need pyroclastics. Should we say felsic pyroclastics?

Slack: Probably, fine grained felsic pyroclastics, and then make that a requirement.

Boudette: Two requirements and then 2 or 3 majors.

McCammon: That means you need them all, any one of those 5. You have flexibility. You can choose from amongst this list and specify some within that list that have to be there. Others may or may not be there. If you want to have the absence of any preclude an occurrence, the factor requirement does that the way we have set it up here.

Boudette: 3 majors and then you have the 2 requirements and 2 of the other 3 left over.

McCammon: You still want to leave this as a requirement, felsic pyroclastics, thick pile and hydrothermal alteration?

Slack: No, I would not make that a requirement.

Boudette: Just leave it as a major. Evidence of chemical sedimentation, hydrothenmal activity, and indicator minerals are the remaining majors.

McCammon: So a thick pile and felsic pyroclastics plus any one of these 3 you are saying?

Bawiec: Where do sulfides come in? The presence of sulfides should be a requirement.

Moench: That would be for a deposit.

McCammon: It is an identified resource at that point.

Moench: So 2 are required and 3 majors.

McCammon: For prospective or however you want to name it, for this category. How about geophysics?

Bothner: Most of the known deposits have some geophysical, principally aeromagnetic anomaly, associated with them, at Milan and Hamphire Hills and we see extensions of those same anomalies along the belt.

Slack: Are they not largely pyrites? They still have the magnetic anomalies?

Bothner: Perhaps it is geochemical and geophysical anomalies. Just lump them together as a major?

McCammon: Or would you want to consider this contributory towards a possible, in other words, lacking in some of the majors but making up for it with indirect evidence. What I am saying is,if this is a probable, then a possible is that you reduce to, let us say to 2 majors, with perhaps, one of these a requirement and then have a combination of minors. I am thinking that when you add the geochemistry, you come up with geochemical signatures that are combinations of elements. You mentioned zinc, lead, and copper. You could have degrees of certainty based on their signature. Some signatures are more suggestive than others and if you could express that in a way where we could write these out as major or minor criteria, then you could treat these as factors defined as different combinations of elements.

Canney: I think geochemistry and geophysics should be treated separately.

McCammon: If you had 2 majors and 2 minors and one requirement, you would be less certain but still encouraged to the point of going out and trying to upgrade it to a prospective category. So we have 2 minors and 2 majors. What would you want to be required in order for it to be still considered likely enough to be pursued given further information?

Moench: If we had the 2 majors?

McCammon: I am saying if we had 2 majors, and you have both minors, what would you stipulate as being required? If you have 2 majors, would that be sufficient for you to consider that to be a possible?

Bothner: We have two majors, two minors, but one requirement for possible and that is that there be a thick pile of felsic volcanic rocks.

McCammon: Here, requirement number one. You must have that at least.

Bothner: Even if it were 500 meters of amphibolites, one might take a look at it if it had a geophysical anomaly.

Boudette: That is the story of Bald Mountain; following the trail of greenstones.

Moench: The greenstone changed probably to something else.

McCammon: What is B zone sampling?

Canney: Soil sampling. John Cummings calls it phase I. He takes the whole zone, something like this, is how he discovered Ledge Ridge. He lays out a soil grid. If there are stream sediments there, he also grabs them. This is his phase I geochemistry. This was the discovery technique for both Ledge Ridge and Bald Mountain. Afterwards he zeroes in on the basis of volcanic terrane and so forth.

McCammon: What happens to those data?

Canney: I have the Ledge Ridge data in my files. I have never seen the Bald Mountain data. Very few people have.

McCammon: How about speculative? Do we need a category for this deposit type at all?

Canney: You have a requirement, you have a thick sequence but nothing else.

McCammon: That should be a fairly large area. Settings for which you have no known deposits, but for which elsewhere, there are known deposits. Speculative is just a thick pile, not really anything else. But there might not be very many areas like that.

Moench: There are quite a lot. It depends on how finely we want to subdivide. In the Lisbon belt, for example, I do not see much evidence of hydrothermal alteration. The intermediate-to-felsic volcanics there are roughly a kilometer thick. I am hard pressed to find a good hiatus. Not much in the way of pyritization, quartz-muscovites schists. I have not seen much either in the way of iron formation or cherts.

Slack: No deposits or occurrences?

Moench: No known occurrences. In the lower part, there is evidence of mineralization but it is a very thin section of volcanics. There is the thin lower member of Ammonoosuc, basalts plus sediments, overlain by Partridge (black schist) where you begin to pick up these sugary-cherts and in fact, the lower member does have an occasional quartz porphyry dome-like feature, and some evidence of mineralization, in the base of the Partridge. The thick upper felsic unit doesn't exhibit many favorable characteristics. We need detailed maps of the area to show the subdivisions we are able to recognize. This unit I am talking about, the lower member of the Ammonoosuc, which is mostly metabasalt or greenstone plus feldspathic biotite schists and the like, reworked volcanics, mafic volcanics, and occasional quartz porphyry with big quartz-eyes and right at the contact of the Partridge black schists, you find the sugary quartzite and some evidence of mineralization. In the upper unit, mapped as Ammonoosuc II, are pyroclastics--a thick section of felsic and possibly andesitic pyroclastics and basalts, mostly pyroclastic. Not much evidence of alteration. So the Lisbon belt has the thickness and it has felsic pyroclastics, but lacks everything else unless there is geochemistry from the middle of the belt that we would tie it to. Also, at the town of Lisbon, some of the blocks of rock along the Ammonoosuc River that are used to keep ice from being shoved onto the shore in springtime are from nearby, probably from the Ammonoosuc I; these have bands of massive sulfide, almost all pyrrhotite, but a little copper. We have a high potential for a lot of little deposits, at the top of the lower member, and a speculative potential for deposits associated with fewer larger deposits associated with the upper member.

McCammon: Why would you say a few large ones?

Moench: Bigger hydrothermal systems.

McCammon: As evidenced by the signs of alteration?

Moench: Evidenced by thicker massive felsic volcanics, and pyroclastics. But again, this is where geochemistry will be important in order to see whether or not there are anomalous values showing up in that valley and, if so, if we can pinpoint where they are coming from.

McCammon: What we need then is for you and Gene to apply these criteria to selected areas. You two are the most familiar with the geologic setting.

Moench: This whole belt starting with Owl's Head Dome, the Warren mine is right down here (near Warren, NH). It is a thick pile, I do not know how thick, somewhere in the middle of Ammonoosuc II. A lot of Ammonoosuc has been eroded away, and more that lies below. We have found neither the overlying Quimby nor the underlying Partridge black schists.

Slack: It is the same unit that has the Franconia Iron formation?

Moench: Yes. There are very thick pyroclastic flow deposits, either pyroclastic flows, either monomictic or polymictic. There are basaltic agglomerates along the Wild Ammonoosuc River; and rusty cherts, that would suggest the necessary hiatus and chemical sedimentation. Up here, near Franconia, I found convincing evidence of hydrothermal alteration; a nice little outcrop below a mansion, showing pipe-like features, in a pavement outcrop, maybe as high as this room of layered pyrite-chlorite-quartz schist overlain by felsic quartz feldspar gneiss but with pipes running discordantly through the gneiss. Additional felsic gneiss lies above the pipe-bearing rocks. I think the gneiss is metamorphosed post-ore tuff. Sugar Hill could probably be mined. I think the whole belt from the Warren mine to Sugar Hill is probable. Do we have to think about how big?

McCammon: With respect to that, what I propose we consider as an occurrence within a given tract is one or more deposits in which we specify what the minimum size is of the deposit, the minimum average grade, and at what maximum depth. If you want, we can go further and make estimates as to how many deposits of different sizes. In the first instance, we say that within this tract, it is likely there is one or more of deposits of the type we are considering as big as x, as rich as y, and as shallow as z. You do not say how many are within a given area, only if there is one or more. If you already have one like at Sugar Hill, that is sufficient.

Moench: Actually, I don't know if it is a deposit or whether it is on strike to a deposit. The evidence of mineralization is conclusive. I have not seen any additional evidence of chemical sedimentation along that boundary, so I do not know if there was enough of a hiatus to permit a large deposit to have formed.

McCammon: We do not want to say how big the deposits are going to have to be. We just have to say there is one or more above some minimum size and grade and depth.

Moench: Well, then you are saying how big it is.

McCammon: No, this is the minimum size it could be. If we do not put a lower limit on the size of the deposit, we are going to have them everywhere. Do we consider the minimum size as 50,000 tons of ore, the size of Milan deposit?

Moench: Milan is closer to 500,000 tons.

McCammon: That is large. 50,000 tons of ore would be minimal.

Moench: 500,000 tons is minimal.

If we classify this resource as prospective, we are saying it is prospective for one or more deposits of the type we are considering where the minimum size we are talking about is given by some figure. I do not know if 50,000 tons of ore is too small to be considered as the lower limit of anything that might conceivably be economic sometime in the future.

Canney: I think that is too small, don't you Gene?

Boudette: Unless you have many of these close by, you can't do it with one mill. Obviously, sometime in the future, this is probably going to be the history of mining. Finding that there are a lot of little ones that are left behind. That is how uranium in peat is going. You look for a lot of closely grouped bogs and build a central mill.

McCammon: I think we should put a lower limit because otherwise every area will be prospective.

Canney: Right now, for example, Ledge Ridge with a few million tons is next to it.

McCammon: Do we want to consider Ledge Ridge, then, as the minimum size?

Canney: No, I think it should be a little smaller than that.

Boudette: Yes, smaller than that. It is considered a small deposit by industry now.

McCammon: How large is the Clinton mine?

Boudette: About 2 to 3 million tons (Cheve, S., 1978).

Moench: I do not know that we need to really consider size. We are looking for districts which are going to contain, many small deposits ranging from a ton to maybe a super giant.

Boudette: What Dick is trying to get us to say is the likelihood that we have an undiscovered Bald Mountain in one of these terranes. John Cummings has said there are 2 more undiscovered Bald Mountains. People are listening to him. We have to accommodate information like that.

Moench: I think in the Warren to Sugar Hill belt, we have the potential for Bald Mountain.

Boudette: I agree with that.

McCammon: You are concerned about the upper limit, I am concerned about the lower limit.

Boudette: What you will have to predict is whether or not there is a Bald Mountain out there. This is what we are going to do.

McCammon: That is the part I see some resistance in trying to come to grips with.

Boudette: It would be useful to know what the probability is that there is a Bald Mountain in that much of the belt.

McCammon: Bob or Gene could speculate as to the probability, but I think you can be a little more than speculative with respect to smaller-sized deposits. You can be more certain about a smaller size deposit than you can be about whether another Bald Mountain sits there or not.

Moench: We can be quite certain about the question of whether or not deposits occur of that type.

McCammon: All I am asking is, for that type, not necessarily that size, what minimum size do you think is reasonable to consider as potentially economic.

Bothner: A million or a million and a half is just barely economic in 1984. If demand becomes greater by 1994, I would think a body half that size would interest people. So, 500,000 tons is not bad.

Canney: A lot would depend on whether you have a gossan situation like you do at Bald Mountain. There is a lot that is unpredictable.

Boudette: Gold will change everything very quickly. Suddenly a 500,000 thousand ton deposit could get very interesting.

McCammon: Well, how about grade?

Boudette: Let us start with total metals.

McCammon: Copper, zinc, and lead, 3 or 4 percent?

Boudette: 4 to 6 percent is where it picks up. There are going to be trace metals, but that is something else to be dealt with. Copper, lead, zinc is major in various porportions.

McCammon: All 3 of those are major.

Boudette: I would say so.

McCammon: How about minor, anything on silver or gold?

Boudette: Then the list gets quite long.

McCammon: How long?

Boudette: Take Bathurst for instance. They are taking out approximately 12 to 13 elements. It is a fairly impressive list in some areas. In Bathurst, they are also making sulfuric acid and pig iron.

McCammon: We want to think about the physical deposits certainly.

Boudette: Silver and gold would be on the top of the list.

McCammon: So there will be major and minor elements. How about depth you want to consider? About a 1,000 feet or 300 meters?

Canney: Yes, that would be about right.

Boudette: It is unlikely they would mine this type of ore at depths much greater than 1,000 feet. An open pit would allow the latitude of a lower grade. Caribou is underground. I do not think they plan to do much more than 1,000 feet.

Moench: Gary, do you have geochemistry that can tie in directly with the belt?

Nowlan: There is geochemistry down in the southwest corner. If you want me to pull it out, I will.

Moench: I think we can assign on the basis of geologic observation, a probable for the whole belt. Geochemistry will add to that. If it isn't there, it probably won't subtract from it because it is very possible to miss these small targets. Geochemistry might have an important thing to say about this belt.

Tuesday, May 16, 1984

1:00 - 4:00 p.m. Discussion of interpretation of geochemical data in Sherbrooke-Lewistion area as it relates to undiscovedred mineral deposit types

McCammon: This is zinc.

Canney: Do you show all the sample sites here?

Nowlan: No. These are above the 75th percentile, the 75th to 90th percentile, the 90th to 95th percentile, and greater than the 95th percentile. The large ones are upper 5 percent.

Moench: That is where you see the cherts. This drainage here comes up and across. You could be looking at an anomaly in there. Now, to go to the next belt.

McCammon: There are not as many over there in that second belt. Which values correlate with the major criteria?

Moench: How about other elements? Lead. Lead is situated around the dome.

McCammon: Looks like lead and zinc correlate.

Moench: This drainage could be coming from, it crosses both belts, could be coming from volcanics.

Canney: How about copper?

Moench: Some copper there, seems to have a slightly different pattern. That is Devonian copper.

McCammon: So it is really from another geologic unit. We do not need to consider the copper.

Moench: That is the area of Gardner Mountain. Not entirely, it is coming from the next belt over.

McCammon: Any chance of barium in there? How about gold and silver?

Nolan: There is barium in stream sediment. Barium is mostly scavenged by manganese oxides.

Boudette: Try uranium.

Moench: This terrain is not well suited for swamps. The two-mica granites form topographic basins.

Nowlan: These are the concentrates for gold and silver.

McCammon: Let us see what copper does here.

Nowlan: They go to fairly high values.

McCammon: Bob, are these tungsten values out of the volcanics?

Moench: This one is Coppermine Brook. We are thinking about the possibility of some tungsten associated with Devonian volcanics here. This anomaly came from downstream from the Coppermine Brook. Not much of a mine. I sampled in the Kinsman just up stream from the volcanics, and a couple of other samples much further up the stream.

Canney: We were thinking about the Austrian type, stratabound tungsten scheelite deposits. Looks like a gray gneiss. You put a light on and it just gleams yellow.

Moench: Is this your maximum, to 3,000 ppm?

Nowlan: Yes. That is the maximum value.

Moench: You do have some tungsten showing up here, not much over here though. Some associated with this dome, some within the pluton in here, and there could be some accessory scheelite. We don't have Attean, but we do have the Adamstown. Those tungstens, though, up to 3,000 ppm don't hold a candle to the kind of tungsten values we obtained down in the Pecos area, New Mexico. These were the sort of levels that were associated with Pecos greenstone belt, where there is good massive sulfide potential, but an order of magnitude higher came from the next belt over which is a younger one with more fractionated volcanics.

Nowlan: This is tin.

Canney: Where is the tin that is not related to the Jurassic?

McCammon: Bob, how about this tin by the sheeted dikes? What does that mean?

Moench: This is two-mica granite here. Rusty schists, quartzites here. You did not sample over that way, did you?

Nowlan: Yes, we did. This is one of the things that bothers me. It looks like a change right here.

Moench: Does that hold true with the sediment samples, too?

Canney: I am interested here about the fact that this break is about at the border of the Jurassic pluton.

Boudette: That is without doubt the two-mica granite.

Nowlan: Do you not expect tin there?

Boudette: We would expect tin in a good two-mica granite. I do not know if it is a matter of level or what. In France, one would be getting tin. It is concentrated in France in very late, crosscutting differentiates of the two-mica granite sheet.

Nowlan: I may have to break this out and see what kind of pattern will show up.

Boudette: That looks like a good correspondence there.

Moench: I would be interested if you get the same pattern; contrast with the sediment samples too.

Canney: You expect lead in here with the tin. Based on the setting in Nigeria, this is where your going to find the tin in the sediments.

Moench: This would be in the Jefferson Dome.

McCammon: It would be useful if we could define geochemical signatures for these terranes, that is, a combinations of elements that constitute an anomaly. If one element is high, for instance, which others tend to be high also?

Moench: Could you define total metal or something?

Canney: You could produce a map that way.

McCammon: Are you leaning more towards a possible here?

(Ed. Note: The conversation returns to a discussion about massive sulfides.)

Moench: Right. The general concept is that you need a thick section to produce an economic deposit. There may be many deposits along this contact; there's evidence of mineralization, but not enough of the background to make a deposit.

McCammon: We do say only that it is possible.

Moench: Yes. Then, we have this area to consider.

McCammon: How about hydrothermal alteration?

Moench: There is more evidence of hydrothermal alteration down in this area (Partridge Lake belt). I know less about it because we spent less time there.

McCammon: If you had to choose between those two areas, which one would you prefer?

Moench: This one is better (Partridge Lake). It has coarser pyroclastics. It is deeper down in the basin, beginning to grade westward to sedimentary facies of the volcanics. But I am not certain that the deposits are all volcanogenic.

McCammon: It might represent a different deposit type.

Moench: We have a fault zone here. Deposits on both sides of the contact look the same as the Paddock. It has some silicified material. It also has veins of quartz, pyrite, chalcopyrite, and possibly gold. Geochemistry shows a lot of evidence of mineralization, but we may be looking at a different model, a different deposit type.

McCammon: You mean sediment-hosted?

Moench: There is more sediment. I am thinking about the actual fracture systems related to the development of the fault zone. The listric fault involved was probably Silurian in age. The environment may not have been a sea bottom. If the fault is an extensional normal fault, we may be looking at fracture-controlled hydrothermal activity on both sides of the fault zone. There is good correlation of geochemistry with this fault boundary; one might make a suggestion that there is a big vein complex or stockwork deposit. That should be looked into. Perhaps the geophysics will help us. At this stage, it is speculative. The people that have worked the Gardner Mountain area suspect that it is massive sulfide country, including the Paddock and the Stevens mines. I am not convinced that the whole setting is one of massive sulfides, however.

Boudette: The emplacement is certainly different.

McCammon: I see no reason, Bob, why you could not delineate that as a separate deposit type with its own characteristics not necessarily using the present criteria but other criteria. It will be classified with respect to a different deposit type than the two belts we have considered.

Moench: What is the production of the Paddock? Is that listed?

McCammon: It was not much.

Moench: 1,000 - 50,000 tons. That is small. They should analyze for gold.

Boudette: We do have major gold exploration in progress there.

McCammon: The Ammonoosuc gold district?

Boudette: Link Page has reported activity by two companies.

(Ed. Note: The discussion now turns to the Lisbon Belt.)

Moench: In the Lisbon belt we have a thickness of greater than 500 meters of volcanics. We can separate it into two parts. The lower member has the cherty rocks associated with it, but has less than 500 meters of volcanics. It does not have the pyroclastics. It does not show much evidence of hydrothermal alteration. The upper member has the necessary thickness but not much evidence of hydrothermal alteration, not much chert or chemical sediments and not much in the way of geochemical anomalies. That makes it a possible.

Let us move into the next belt. We have been through the assessment of the Partridge Lake and Lisbon belts of volcanic rocks. This is what I call the Bronson Hill Belt. It extends along the lines of the domes from Massachusetts to the area of Milan, New Hampshire. The characteristics of this belt are that it has thick pyroclastic section, greater than 500 meters, that shows evidence of chemical sedimentation here and there, and it shows evidence of alteration. We showed a strong geochemical association with this belt, in contrast with the Lisbon belt, which also lacks obvious hydrothermal alteration, and contains no known deposits, mines, or prospects.

Slack: Are you confident that the eastern belt you talked about before is the same that could cover the domes to the south?

Moench: Yes.

Slack: There is Croydon Ore Hill and Neal and other occurrences around the Croydon and Owls Head domes.

Moench: Right

Slack: Those are in the same package that is correlated as it continues to the south.

Moench: The Lisbon belt doesn't even though it has a big section of pyroclastic rocks . It doesn't show a geochemical association, and doesn't have the alteration or the cherty or chemical sediments. So, we can have a look at the Partridge Lake Belt. It is essentially the type area of the Partridge Formation. Black slates of the Partridge here form anticlines; very complex, doubly plunging, anticlines. There are a lot of pyroclastics rocks. It is a thick section of felsic and intermediate mixed volcanics, not a lot of intermediates. There is one unit high in this section of basaltic rocks and tuffaceous andesites. We mapped it as Ammonoosuc III.

Slack: Bob, what is the scale?

Moench: This is 1:48,000. My plan is to put together a map at this scale (Moosilauke quadrangle southern half of Littleton quad, 15°) scale. We have the topo graphic base showing the reinterpretation and remapping of the Ammonoosuc plus Billing's delineation of the younger rocks, Clough, Fitch and Litteton Formations plus modification of more volcanics in that unit.

Nowlan: Bob, do you want some geochemistry for that scale also? Would it be worthwhile?

Moench: It might well be worthwhile.

Bothner: Would you want a magnetic overlay for that area too? The gravity coverage is sparse.

Moench: It would make a useful illustration of the different associations. This section of uppermost Ammonoosuc volcanics is getting more distal as it grades westward to a unit of greenish grayish phyllites with ashy beds and occasionally mafic volcanics; this appears to be a western distal facies of the mafic volcanics that are high up in the section. The Ammonoosuc in this area is in fault contact with the older assemblages of rocks to the west along the Gardner Mountain anticline. There are mineral deposits; quite a number of prospects up and down in this region near the fault. The largest is the Paddock mine. It has produced copper. I have been up past the Stevens mine although I have not been in the Stevens mine. The association of the Stevens is not like the association down here (Paddock). No thick sections of pyroclastic rocks or chemical sediments at the Paddock; these deposits are associated with bulbous silicified tuffites, massive and very fine grained quartz that occur within otherwise stratified rock. Some of the stratified rocks are tuffaceous, but a lot are pelitic sediments. It's not a good massive sulfide association. The deposits that are exposed look like veins that have been deformed; pre-cleavage veins. The model I am thinking of in this belt, at least across the fault zone, is one of veining, possibly contemporaneous with the fault. We have no way of dating that except that it is premetamorphic.I suspect we are looking at an extensional fracture system that opened up the belt to hydrothermal mineralization which then was metamorphosed and deformed. So we have the question of whether the massive sulfide models apply here or do we need two models, one for massive volcanigenic sulfides, the other, veiningwhich could be subvolcanic.

Slack: It could be a feeder zone. Any suggestion that those might have been deposited in very shallow water depths?

Moench: The sediments associated there are aquagene tuffs and they are also melange units. There is prospecting going on in good schist and quartzite of the Albee Formation. There was some production. Some of the rocks are dark phyllite with ashy beds; in places it is melange with bedding that's been torn apart, flazer-deformed, lenticular, and scaly cleavage in the rock. Most of the rock is fairly well bedded, however, but extremely deformed. In any case, the volcanics are not proximal. They are distal.

Hatch: Are the volcanics mapped separately?

Moench: They are not mapped separately. The sediments are probably deep water. We could be looking at a fracture associated assemblage of deposits on both sides of the fault that separates the Albee and Ammonoosuc and a possible potential for massive sulfide in this area, in the Ammonoosuc southeast of the fault. If we just talk about the Ammonoosuc, we have the thickness, probably greater than a kilometer of felsic and mixed volcanics, good pyroclastics. I do not recall much in the way of chemical sediments. Some thin-bedded rocks which would indicate at least slow deposition. These rocks on the west are very fine-grained; these on the east are coarser. With respect to the massive sulfide potential of this belt (Lisbon?), we are looking at a possible. There is no evidence of hydrothermal alteration. We are satisfying most of the criteria (Partridge Lake belt): evidence of hydrothermal alteration, pyritic quartz-sericite schists, especially down in this area. You really find it all over. I think there is certainly evidence of hydrothermal circulation. We were looking at the geochemistry yesterday.

McCammon: You certain!y have the zinc, copper, and lead.

Moench: Perhaps we can grade probables into high probable and low or medium probable.

McCammon: If you go that far, you might as well estimate the number of deposits.

Moench: Right.

Slack: Do you want to correlate those three volcanic belts? They have similar chemical and alteration signatures. Are they lateral facies of one another or do you think they really might be different in time?

Moench: The stratigraphy would suggest they are lateral facies. We don't have any isotopic dates.

Slack: One other question. Does the Bronson Hill belt have extremely coarse felsic fragmental rocks? I know you said the middle belt did and the northwestern belt for the most part did not.

Moench: The northwestern belt does and does not. It has fine-grained tuffs on the west side. It also has some conglomeratic phases, most of it matrix supported.

Slack: I was wondering whether there was a trend towards some volcanic edifice to the southeast which would be coincident with the greatest probability in distribution of alteration zones, or greater distribution of coarse pyroclastics?

Moench: There is a lot right up in here; coarse mafic conglomerates and some pyroclastics. I do not think we are in the position to say that there is a continuous gradation.

Nowlan: There is strong copper here.

Moench: That shows up in the types of deposits too. This is the Bronson Hill. These deposits (Paddock) are almost exclusively copper as far as I know from the production and what you see in the outcrop. The Warren mine down here is lead and zinc rich, and some copper.

Slack: How about the Stevens and Royce? Those are lead-zinc-silver deposits.

Moench: They are, Stevens is mapped as Albee.

Slack: Royce, especially, has massive sphalerite and galena, just a little bit of chalcopyrite, and a lot of silver.

Moench: I am not sure where the Royce is.

Slack: I think it has several names. I will have to look. Yes, that is an old name. It is on an old minerals map.

Moench: Again, you have a northwest trend of geochemical patterns. There seems to be some chromium. It is hard to say because the northwest-trending geochemical belt crosses the Clinton River volcanic belt; the Clinton River belt is the source of the lead.

(Ed. Note: The discussion now turns to the possible effects of glaciation on the dispersal of geochemical elements.)

Boudette: That is where the ice went too. The Boundary Mountains dammed up the ice. The ice then piled up against the Boundary Mountains and then went southeastward. Sampling technique could enhance an effect of pre-concentration. Ice is not what we are looking at yet, because it looks like it comes up to here and spreads out.

Nowlan: Which way is the ice going here?

Boudette: Well, it would back up and break through valleys in the Boundary Mountains, then stream south and probably make local excursions, probably not important, but maybe just enough to do it.

Moench: Then there was reverse flow, northward from the Boundary Mountains.

Boudette: There is someone working on that problem up around Priestly Mountain. He is using Preistly boulder trains to extablish that the last ice cap in Maine that fed the ice streams the other way.

McCammon: So you had glacial advance to the North.

Boudette: It was a plateau ice cap in Maine that could have caused it to flow down the St. Lawrence river. To go to Antarctica is to realize how complex ice movement patterns are. The beheading of ice streams is an important process. I was first exposed to ice flow regimes when I went down there. The Beardmore Glacier is capturing all of the minor ice streams near it. It is beheading ice streams that went down dry valleys. The first thing you see there are Grand Canyon-like features where ice streams used to be. But the ice is diverted flowing to join the Beardmore or perhaps some other dominant glacier.

Nowlan: These series of small faults here are also trending northwest.

Boudette: Some of those faults have been more accentuated since the Holocene by streams. A lot of them even have been etched out before Pleistocene time. Every cross fault for example; there are probably many more cross faults then we have shown. There are no less than you see there. Some places you can put your hand on those faults.

Canney: Speaking of ice streams, you would think if there really was an ice stream, you would get more of a fan shaped pattern.

Boudette: Because we know what the source of the chromium is, we should be able to test the hypothesis. Serpentine boulders are conspicuous to the south-east across Maine, but their distribution is not mapped--a few is possible.

Canney: It could be. It is a hypothesis that has to be considered.

Boudette: Considering the results of the pan concentrate analyses. You may be amplifying a low order effect.

Moench: There is some difference in the geochemical signatures between these two belts (Kearsarge and Connecticut Valley).

McCammon: You might want to put that in the same resource classification.

Moench: We might, we know less about it, we have some geochemical signature.

McCammon: What is it, a thick pile still? Pyroclastics?

Moench: I do not know.

Canney: How about bismuth?

McCammon: Bismuth, why are you interested?

Canney: It goes along with tungsten.

Moench: I am curious about the metalliferous character that is showing up in those sediments in the northwest.

McCammon: Actually, it lines up with the lineament, that one to the south. Let us look at that because before, we were off strike.

Nowlan: This chromium trend goes through here. There is a hint of lineation, but less mafic elements down this way.

Canney: How about the serpentine getting into the till?

Nowlan: It is possible.

Canney: I would like to see the Canadian data.

Hatch: Do you think the serpentine is from the Canadian asbestos area?

Moench: Judging from the ice direction, you would think it would be dispersed. Jim Pond, as a rule, is not coming up with very much; a slight zinc association. Thrasher Peaks, some of the metals train comes from there; copper, zinc. Then the Littleton volcanics in the southwest, showing up with heavy mineral concentrates for tungsten, zinc, lead, and copper. We could assign a probable. It has some geochemistry. We know there is mineralization. It has all the associations. Mainly a difference between large bodies of ore verus a dispersed source and lots of little ones.

(Ed. Note: The discussion returns to the consideration of the potential for volcanogenic massive sulfide deposits.

Canney: The geochemical anomaly is not necessarily related to the size of the deposit.

Moench: Or the potential of the district. What we are looking for is the geology plus some geochemical indication. We have concluded that, for this area, we have a potential for massive sulfides, maybe 500 thousand tons equivalent to the potential of this area; but then also, a large potential for lots of small deposits that nobody would be interested in.

Canney: Yes, the geochemistry is stronger astride that fault zone.

Hatch: Does that seem to argue against it being distal phase?

Moench: We have a possibility of two different types of deposits. I do not think it is distal. But a disperse source, lots of little veinlets, will give you broad geochemical anomalies.

McCammon: Lots of exposed mineralized areas.

Hatch: Would this be synvolcanic or perhaps something significantly later?

Moench: I suspect it was later.

Hatch: If that is the case, then the volcanics themselves could be related, the later phenomenon like the western belt, the Bronson Hill Belt. You are thinking in terms of an early Silurian tensional regime as the time of enplacement of the disseminated sulfides?

Moench: Yes, little deposits occuring here and there.

Hatch: Post-Quimby or post-Greenvale Cove, pre-Clough? That should mean that you should not find in Clough or younger rocks, so the mineralization you see in Clough or younger rocks should be in different and totally unrelated in geography and origin and everything else.

Moench: It has been suggested, I do not know if anybody has actually found any gold in the Clough.

Hatch: You are talking about two unrelated events. There is not much of a geochemical association. Although we have no deposits, the geology is favorable for deposits. We could say then, a probable, to this listing all of the geological criteria, a plus of sorts for the geochemistry, the geophysics indicates a good volume.

McCammon: A thick pile of clastic rock.

Moench: Yes. Then for Clinton River, probably thicker in the north, the major criterion.

McCammon: You have the chemical sediments.

Moench: Yes.

McCammon: Any evidence of hydrothermal alteration?

Moench: I have seen some really good pyroclastic, coarse bombi-clastic conglomerates just to the north.

Boudette: Right at the Clinton River mine.

Moench: Right, Clinton River and also just north of Ledge Ridge. There are coarse pyroclastics. North of about somewhere in here (Moose Bog), there is iron formation.

McCammon: How about hydrothermal alteration?

Boudette: Not much exposed.

McCammon: Barium occurs at the Clinton River mine. Unfortunately, it does not show up on these geochemical maps. How about geophysics?

Bothner: Very trackable. It is a continuous anomaly that tracks the volcanics well. Especially the magnetic greenstone. But they tend to be thin where we see them. Is that a possible or less than possible?

Moench: For the belt?

Bothner: For the southern part of the belt?

Moench: For the northern part, it is probable for additional deposits.

McCammon: There you don't have the hydrothermal alteration or the geochemistry. What about the chemical sediments?

Moench: You see a lot of sediments.

McCammon: Ironstone in the most southern part?

Hatch: Yes, there is iron formation.

McCammon: So it has the chemical sediments.

Hatch: Yes.

Moench: It has thickness, but thinner than in the north. I do not know how thick because it is complexly deformed.

Bothner: It might barely make 500 meters. It is not going to be much thicker than that.

Moench: Second category, pyroclastics. I do not recall any really good conglomerates.

Boudette: There is one exposure I have seen.

Moench: Sparse exposures.

McCammon: The lack of pyroclastics will take it out of the top category.

Boudette: There is at least one exposure of volcaniclastics I remember.

McCammon: We are downgrading this area on the lack of geochemistry more than anything else.

Moench: Well, thinner, it is a little thinner.

McCammon: That will do it.

Boudette: Volcaniclastics are minor. They are very fine textured.

Moench: Some of the sediments and volcanics are pyritic. How about geophysics?

Bothner: Very distinct, very sharp, very linear anomaly.

Moench: Assignment then is possible.

McCammon: Something that is unexplained is the tungsten. It might be skarn-type deposits related to the intrusives. No calcareous type rocks in that area? Bob, to me, that is speculative. You have something here, something going on. You can describe it in terms of the anomaly.

Moench: Yes, speculate as to what the association might be.

McCammon: You are saying that an anomaly is indicated by the data.

Moench: These are moderately thick section of mafic volcanics (Percy-Errol quads), very little felsic at the top, overlain by dark schists, graphitic, sulfitic, lots of chemical sediments associated with it, lots of chert and coticule. So by those criteria, we have the chemical sediments in abundance.

Hatch: Not going to give 500 meters, right?

Moench: Not going to give us 500 meters. Even the basalts are lenses. They are pods and the host is pretty much the black schists. It does show copper staining, all of it does, very sulfitic,

McCammon: It would be difficult to get that much above speculative.

Moench: That is right, the areas are so small, that it is hard to make sure that what you see in a sediment sample isn't coming from outside the area of volcanics. Phil Schafer and Karl Kotef said they will look at these anomalies. They are quite intriqued. They feel they have more to learn from us then we will learn from them. Most of you have seen the section of Jim Pond in the Stratton quad. As you go up-section, you pick up jasper usually filling in the voids between pillows and things like that.

Canney: Was the outcrop pillow basalts?

Boudette: The best outcrop is on a rebuilt part of State Highway 27 near Poison place called Posion Pond. There are excellent exposures along the eastern shore of Tea Pond. If you look hard, you can find chert near the bottom, but it seems to be increasing in volume toward the top. The volcanics are part of a homoclinal sequence. As you go up through pillow basalts there is the complication of trondhemite cutting pillow basalts. At the top, you pick up more chert, which sometimes is in continuous lenses, but a lot of filling occurs between pillows. Finally you go up into the first of the felsic volcanics which have been called keratophyre, dacite, or rhyolite.

Moench: How thick is the pillow sequence?

Boudette: At least a kilometer. The whole ophiolite is up between three and four kilometers. The top is about one kilometer. On Route 27, you do not see any felsic volcanics. There is a ridge to the west where are thicker and are interbedded with pillow basalt. To the east, on Chase Pond Mountain, there is a thick welt of felsic volcanics in which there are lots of pyroclastics and some of the rocks looks like the pictures that were shown by John Slack.

Moench: Angular?

Boudette: Very angular. If anything, there is more felsic breccia than there is relatively massive quartz-eye types like that seen to the west. Rather distinct thick beds of banded iron formation overlie the felsic volcanics. Toward the east, the iron formation tends to be blood-red. If you start working west, you find rather continuous beds. One bed at Boil Mountain that is blood-red laminated iron formation on the east end, and within a kilometer near flank of Boil mountain it becomes almost snow white, changing imperceptively. One can walk on it all almost the time. There is no question it is the same bed. I thought that it was rather a nice demonstration of progressive change in chemistry.

McCammon: Were you speculating about Bronson Hill?

Moench: Yes, whereas the Warren mine produced about 100 thousand tons, there is no way that any of these deposits is going to produce that much.

McCammon: So it is a different class of deposit.

Moench: Yes.

McCammon: Different size class and it is probably a different setting that makes it that way.

Moench: Unless you have a western type district where you have a vein system, where you can predict where that fault is changing strike and you can predict the rake of the orebody, then you can get big tonnage.

McCammon: In fact, from what you are describing, the likelihood of the large one does not seem very prospective at this point.

Mo nch: If we had the Silurian volcanics sitting up there at just a slightly higher level, we might have some possibilities.

McCammon: The Milan produced 500 thousand tons of pyritic copper ore.

Moench: It had lead and zinc also.

McCammon: It has 2 percent copper, 5 percent zinc, and 1 percent lead for the data I compiled.

Moench: We will call this the Milan Belt. We are going to have to compare the geochemical signatures of the different belts. The Milan belt winds all the way through here. We might check and see if there is any geochemical association with these little patches of volcanics (between Milan and Littleton). You have the Milan mine. There has been a lot of prospecting around

Milan in this territory. Hampshire Hills in there. We have stratigraphy that is analogous of what we have down here (south of Littleton). We are trying to get at the setting of the Milan belt volcanics through here. The lower unit is mainly volcanics but in places there is sedimentary material; in other places, a whole lot of sediment. I break out the two major members, the lower member, mafic volcanics with or without sediments, and the upper member of pyroclastics, felsic volcanics, and some mixed mafic volcanics, not much in the way of andesites. The sediments associated with the lower member are tuffaceous, nonrusty on the north side, nonrusty felspathic sandstone tuffaceous beds plus the pelites on the south side and over in this area, dark schists, feldspathic schists, and felsic or tuffaceous beds. In the upper member, all the way through you have the pyroclastics; some good agglomerates in this general area, probably close to a volcanic center, pyroclastic, mixed agglomerate next to the Chickwolnepy intrusion, which is a mafic sheeted dike complex with gabbro tonalite. The volcanics appear to be unconformably overlain by Quimby graywacke and black schists, at the base of that, is a layer of calc-silicate rock. The mineral deposits include the Milan mine, primarily. I think it is a copper-zinc deposit with minor lead.

McCammon: Yes, 5 percent zinc, 1 percent lead (Eric, J.H., 1979).

Moench: The Hampshire Hills, which is strictly copper, has almost no zinc; almost nothing else. If you make the analogy with Japan, the Milan is more like a Kuroko deposit and the Hampshire Hills probably a Besshi type of deposit, essentially Graywacke(?) association, volcaniclastic, epiclastic plus silicate-facies iron formation of garnet, chlorite, staurolite and magnetite. The ore is in the middle of it. There are places where south of West Milan you go across a section of altered quartz-pyrite-muscovite schists, succeeded by rusty gossan and then overlain very abruptly by unaltered amphibolite. Spectrographic data show a strong indication of mineralization. The prospects I am aware of in the area appear to occur at about the same stratigraphic horizon, maybe a couple of hundred meters above the top of the lower mafic member.

McCammon: How many holes did they drill there in Hampshire Hills?

Moench: Lots of holes.

McCammon: Enough to come up with an estimate of its size.

Moench: I suspect they did.

McCammon: Are we talking about something that is bigger than Milan. A hundred thousand?

Moench: I would not be surprised.

McCammon: But less than a million.

Moench: There is no way of knowing at the present time.

McCammon: This is a situation where we could assign a location as being a definite potential resource without it being an identified resource. It has a higher degree of certainty than any other locality in the belt. Maybe we can think about a definite category above the probable for areas such as Hampshire Hills.

Boudette: Do we have any information on the core?

Moench: I know there is core. We don't know what is in the core.

McCammon: So we do not know the grade.

Moench: No idea of grade.

McCammon: So we do not know much about it as an identified resource.

Moench: No.

McCammon: But it is there.

Moench: It is an occurrence in the same way that the Milan mine is an occurrence.

McCammon: But with the Milan mine, you can put crosses above the hammer on the map. You can not do that for Hampshire Hills.

Moench: No.

McCammon: That is why I say we need a category for occurrences like this. We want to think about that.

Moench: Without knowing size and grade we have no idea whether or not it is a resource. We have evidence of hydrothermal alteration and chemical sediments in the form of magnetite banded iron formation. I am curious to see how the geochemistry comes out.

McCammon: With respect to the majors, it is a probable.

Bothner: Not much on gravity, very strong magnetic anomalies.

McCammon: Very strong, meaning what, do you think, that you have a thick pile?

Bothner: You have a pretty thick pile, thats the easiest way to put it. A lot of little spikes.

Moench: The silver is related to argentiferous veins like the Mascot and Shelburne.

Wednesday, May 17, 1984

8:30 - 11:30 a.m. Continuation of Tuesday afternoon's discussion

Moench: How about Boil Mountain?

Boudette: Bimodal volcanics, pyroclastics, and in the core all I saw was mélange. It was a rusty black pelite, with clastic materials such as chert, and bimodal volcanic fragments. There are also banded sulfides, some clasts are obviously folded clastic sediments.

Moench: The geochémistry we have does not show much.

Boudette: The occurrence would seem to have just about everything you would look for with the possible exception of hydrothermal alteration. Unless you want to make a case for the chert, you cannot say there is any hydrothermal alteration. The question to ask: Is it a detracting factor that the setting is in mélange? If you look at the volcanics, particulary the greenstone, you will find a sharp chemical contrast to the middle Ordovician, mainly in the titanium and iron. These are extremely low in titanium, and quite low in iron. If you look only at the titanium, there are at least two different greenstones to consider. The Jim Pond/Boil Mountain would not indicate genesis in an extensional environment. They are more easily related to an environment of collision. I favor an obduction model.

Canney: What was the grade of the sulfide?

Boudette: I never got any numbers. I gather it was not very high. The problem with the sulfides is that it was largely pyrite.

Moench: To describe it best we could say it has all the attributes for 1 thru 5 with the exception of hydrothermal alteration. It has a thick pillow sequence, a thick felsic unit, certainly coarse, pyroclastic, angular, proximal tuffs. You have the iron formation going from proximal to distal, becoming cherty as you go out. You have weak geochemistry based on our data.

Bothner: Geophysics is inconclusive over the volcanics. Gravity exhibits a gentle gradient, mostly coming off Spider Lake and Flagstaff, very little in gravity, quite a bit in magnetics.

Moench: So you might have an assignment of probable for the occurrence of many small deposits, torn apart.

Boudette: The point is that it ducks underneath the Siluro-Devonian and reappears again as mostly melange.

Moench: Could you delinate the area of probable?

Boudette: The area one would be looking intensely at would be a small area, a few square miles, where the melange thickens up. The actual drilling program would be carried out right up here.

Moench: Is this strictly copper?

Boudette: The only thing I ever saw was a little chalopyrite and a lot of pyrite.

Moench: What is the maximum size deposit?

Boudette: A few tons, perhaps 10 thousand tons. It is small.

Moench: If you drew a circle around the small area of probable, could you assign the rest of the belt to a possible?

Boudette: Yes, possible. It is probable there, the rest of the belt is chert. You have everything going for you, but the fact is, the area has been explored.

Canney: I know that John Cummings had a good geochemical anomaly there. Maybe you know about it up there along the main highway, is it 27 West?

Boudette: Same environment. There is other geochemistry indicating sulfides here and there. There are also occurrences of asbestos.

Canney: Good long fiber?

Boudette: Yes, never better. This is at Onion Hill. There should be some anomalies that follow through because projecting around the Hurricane Mountain Formation you find remnants of serpentinite.

Moench: How about an assignment? Perhaps you can delineate certain areas of probable for small deposits; copper in those areas, and possible for the rest of the belt. You have a good thick assemblege of both mafic and felsic and some iron formation.

Boudette: It is an occurrence. You could put the crossed hammers on the map.

Moench: It is the type of occurrence that should go on the mineral occurrence map. In terms of the resource analysis, we can categorized all the Devonian volcanics as being thin, lacking evidence of intensive alteration, and showing only local chemical anomalies like the one in the Littleton area. Not much in the way of iron formation or cherts. So they are not the place to go looking for large massive sulfide deposits. Probably the same is true in the lower Silurian, mafic, some felsic, no strong geochemical anomaly. Probably, the the water depths were just not adequate. Not deep enough to prevent boiling in the pipe or deep enough to permit percipitation on the sea floor; probably no major circulation system. We had a big nickel anomaly showing up. When we get to the sedimentary arrangement, we might address that question.

We just went through the Jim Pond and looked at the Devonian volcanics. The small parts of the Jim Pond have high potential for many of small deposits, copper, disconnected, broken-up, segmented.

Slack: Do you see any occurrences overlying Boil Mountain in the lavas?

Boudette: We do, but they are all broken up in trench material. I can show you core in which there are folded, banded sulfide segments.

Slack: Is everything dissegregated?

Boudette: Yes, redistributed into a large volume of melange.

Moench: We just finished.up the volcanics. We were thinking about getting into the possible sedimentary associations. Ready to go to the sediments? Bill Cannon said he would like to look at the possibility of sediment-hosted deposits.

Boudette: An interesting thing that I would like to point out is that Bob Marvinney found a Pavement, I do not know the exact location, but it is roughly back in here someplace. It is sheared Seboomook with blocks of Attean Quartz Monzonite; a fault breccia. The blocks are presumably dragged in from the southwest where the Attean is exposed. This would indicate right-lateral offset.

Moench: Unless the Attean underneath comes over one or the other.

Boudette: Apparently the regional rotational sense seen at the plunging southwest end of the Chain Lakes can be explained by the geochemistry of this fault. The drag on the Jim Pond, Hurricane Mountain, and Dead River suggests motion that was right lateral. There are other reasons to think that it is left lateral. It is a problem. Where you can actually see the fault plane, it appears to be just a normal fault.

Moench: I might mention the distributions along the Bronson Hill-Boundary Mountain Anticlinouium. We have considered the volcanics of the Cambrian and Ordovician and some Silurian and Devonian volcanics; Devonian in patches, and Silurian, outliers evidently overlying the pre-Silurian section. The Devonian and Silurian in this area was pretty much shallow deposits, these are shelf-shoreline facies, particularly in the lower part of the Silurian; very thin and passing up-section into fossiliferous, muddy sediments of the Seboumock and dirty sands of the Farratine and Tombegan, which has volcanics. As you go south- east, you go into thick and extremely varied basin sequences of the Siluro-Devonian section. On the other side (Connecticut Valley-Gaspe) where there are undated rocks, I suspect the bulk of them is Ordovician, rather than Silurian and Devonianin in age. We are waiting for dates from the Northeast Kingdom plutons.

Bothner: Completely unfossilerous. Yes, no fossils at all, but a thick stack here in the Rangeley area, essentially 5 kilometers of Silurian and another couple kilometers of Devonian in here, clastic section, very little carbonate, some carbonate but then as you pass down to the southeast you have again probably the equivalent thicknesses of Silurian clastics, plus a lot of carbonate rocks. No one really knows about the petrology of those carbonate rocks or whether there are flysch type deposits.

The sedimentary petrology of these rocks is not understood. There are no volcanics in that whole section in through here, some volcanics mafics, scattered, closed through the Connecticut Valley Gaspe synclinorium. The question is, what is the potential of these terranes for metalliferous deposits? You find a few indications here and there, little lenses of coticule, rusty copper-bearing rock. I have some rocks with 1500 ppm copper with chalcopyrite. It is a muddy sediment, great big massive beds of gray shale, schist showing subtle mud-silt turbidite type features. Some of these rocks in southern central New Hampshire showed some indications of metals.

Boudette: The other day Page Chamberlain told me about massive tourmaline, in a bed that he could follow for as long as he had stratigraphic control. John Lyons, thinks it is in the Rangely (or what may turn out to be Rangeiy) it is quite spectacular.

Moench: Some of the rocks of the Connecticut Valley-Gaspe sequence up in Canada at least show copper stain on the cleavage, not really intense, but it is there. Our wilderness data for this area (Mount Washington and Wild River) show a correlation of low level copper associated with Siluro-Devonian gneisses. It looks as though these are somewhat metalliferous sediments. We have to consider whether or not there are facies environments here that would be favorable.

Slack: Is there any silver or copper?

Moench: We need to find out now.

Canney: The stream sediment geochemistry had anomalous silver but it does not correlate with any other element. It does not correlate with any geologic unit and there have been problems with erroneous silver values with respect to sample preparation.

Slack: What about the pan concentrates?

Canney: I think pan concentrates would be all right. Bob, how much marine clay do we have in the southeastern corner of the Lewiston Sheet? How far inland does that marine section extend?

Moench: To elevations of 500 or 600 feet? It could get up into some of this area down here. Does that have some possible effect on the geochemistry? In other words, if the clay forms a blanket over bedrock, stream sediments are not going to pick up anything from the underlying bedrock.

(Ed. Note: Discussion fellows about nickeliferous pyrrhotite deposits intersections of gabbro and sulfide metasedimentary formations.)

Moench: Throughtout the whole area, especially the east side, there are graphitic-sulfitic schists. One of the most prominent units is the Small Falls Formation, averaging about 7 percent pyrrhotite. Pyrrhotite contains traces of chalcopyrite, sphalerite, and nickel. The rock as a whole, however, is not particularly iron-rich or metalliferrous compared with the overlying and and underlying formations.

Slack: Are there any mafic plutons that intrude the Smalls Falls?

(Ed. Note: Discussion on tungsten in heavy mineral concentrates)

Moench: That is a possibility. Based on my experience in the Pecos wilderness, the tungsten values in the heavy-mineral concentrates are very similar to the so-called Pecos greenstone belt (New Mexico), a billion or so years older. The maximum tungstens in the Pecos greenstone belt are 1-3 thousand ppm W, whereas in the next belt to the west (which is stratigraphically higher and more fractionated), they are on the order of 1 or 2 percent.

Slack: Do you do more extensive splitting now?

Moench: Same kind of separations.

Slack: I remember they were multiple non-magnetic separations there, like four or something. It took off the magnetite, then split the non-magnetic, and split it again at least 3 times.

Canney: What you normally do is pull out the magnetite, then you do a split into the magnetic fraction and the non-magnetic fraction.

Slack: So you do at least 3 fractions.

Moench: We did it with an intermediate fraction of half and half. Within that data set they were handled all the same. There was a major constrast between the tungsten and the greenstone belt versus the tungsten associated with an apparently younger volcanic rock.

Slack: In comparing 1 percent there with 2 or 3 thousand ppm here, they could be broadly comparable considering the separation that was done.

Moench: This locality is the Coppermine Brook mine, so called, and the good copper and tungsten values in the concentrates occur downstream from that, but just upstream from the mines above the contact with the Kinsman Quartz Monzonite, the data show no copper or tungsten. Whether the tungsten came from the mine or from some other association in the Devonian volcanics, it is probably restricted to this belt of volcanics.

Slack: Is that at Littleton?

Moench: There are volcanics in the Littleton. Mafic, not showing much alteration. There are pods of calc-silicates. I looked for scheelite but did not see any. I suspect an association with the Littleton volcanics. These high tungsten values show multiple associations. One association is with the Moody Ledge Granite. This high tungsten value is from the east side of Mount Clough, which is almost all Ammonoosuc.

Slack: Do you have any scheelite around Hampshire Hills?

Nowlan: I do not think so.

Moench: The thin calc-silicate unit between the Ammonoosuc and Quimby has some tungsten. We had scheelite in one of the samples. We could not reproduce the data, or find the scheelite in outcrop. The ground up samples had little specks of sheelite. I think there was one crystal ground up.

Slack: Do you think this fault between the Ammonoosuc and Albee could be a growth fault?

Moench: I interpret it as being pre-metamorphic, younger than the rocks I assigned to the Greenvale Cove Formation which could be either late Ordovician or early Silurian but older than the overall structure, probably older than the Clough or Fitch.

Slack: Did you get any gold in the stream sediments?

Nowlan: Some. These are actually 15-20, less than 20 where it was detected.

Moench: The gold is associated with the volcanics. There is gold in the Indian stream, a well known panning area.

Boudette: Also, the Canadians tell us a story of an old sequence of Wisconsin terraces never seen on the U.S. of the Boundary Mountains. The older terrace system carry gold values. A million and a half dollars (pre World War II) worth of production has been recorded along the Chaudiere River.

Moench: Swift river is a famous gold panning stream. One could get nuggets so big from there and also from a number of these streams in here (Phillips quad., Maine). How were the stream sediment data handled?

Nowlan: What I did was to take the percentile value, the smaller symbols represent the 75 to 90th percentile, the medium size symbols the the 90 to 95th and the larger symbols represent greater than the 95th percentile. We changed the sample preparation method for the heavy-mineral samples, and I treated them as 2 different sets. Triangles pointed down are one set, triangles pointed up are the other set. They might represent slightly different concentrations but the percentile values are the same. Tin showed a similar trend. Analytically we feel confident about this because these are several generations of samples. These were taken 20 years ago. These were taken in 1979. These were taken in 1982 and yet the trend continues.

Schafer: These are all stream sediments?

Nowlan: These are stream sediments at minus 50 mesh.

Schafer: I am reasonably confident the Chromium anomaly is not related to a local feature.

Moench: We do not really know. There are lineaments going across here, northwest trending lineaments parallel to the trend. There are faults that were mapped in that direction. The question is, is there a swarm of ultramafic dikes going through there?

(Ed. Note: A discussion of the glacial history of the area begins.)

Schafer: The glacial history of this part of Maine has been undergoing much reinterpretation beginning about 15 years ago. There are now excellent reasons to believe that at the beginning and climax of the last glaciation (Late Wisconsinan beginning 20,000 years or so ago) is the marginal sector of the main Laurentide ice sheet was centered off to the north-northwest or northwest somewhere up in northern or central Quebec or the Hudsons Bay area, and ice moved southeast across this area, but that regine later was replaced because of the very rapid evacuation of ice probably mostly by calving into sea water in the St. Lawrence estuary. That calving cut off this sector more rapidly than the ice here could melt. Calving is a much more efficient way of getting rid of ice than just melting on dry land, because the radiant heat flow will melt only x number of meters per year. The evidence is now quite clear (as the Canadians have been insisting) for areas along strike to the northeast on the Gaspé Peninsula and in parts of New Brunswick, that the last ice, probably a pretty sizable area up to here, was a local ice cap.

These general ideas have not settled down yet. They have been having trouble straightening out the stratigraphy of different tills into ones that are controlled by the history of the ice sheet and direction of movement versus ones that are simply different because they derive from different rock types. There is a late phase when there is an ice carapace (independent of the Laurentide ice sheet remnant) sitting up here somewhere; it includes the Appalachian highlands along the Quebec-Maine border and on strike northeast and west. There is a gap across the St. Lawrence, the Champlain Sea is already in there, and there are radiocarbon dates on the shelves in the Champlain sea sediments. There is probably some sort of quasi-radial pattern of ice movement in the carapace, or to it is a linear carapace, then it is sort of a banana-shaped thing moving off in both sides. But rates of flow, rates of rock erosion and transport of debris almost certainly are less in that late carapace phase than earlier during the main ice sheet phase.

It is possible there will be some survial of material through more than one glaciation. For instance, southeast of where that carapace persisted, there are occasional Canadian Shield erratics in the drift. There is good evidence of at least one and probably more than one pre-Late Wisconsinan glaciation; there is an older till that is widespread in Maine just as in southern New England. You find one chunk of some distinctive rock type in a gravel pit down in central or coastal Maine, and that could have leapfrogged down through several glacial stages. But I don't believe that sort of multi-stage transport could produce anything as strikeing as the linear Cr anomaly. I think at least some of the people working in Maine might doubt whether even the early phase of the Late Wisconsin ice (when it had its maximum extent with the maximum flow to the southeast directly across the strike of the main rock belts) could have persisted long enough to produce any major effects on distribution of materials in the drift. Those are the kinds of questions that could come up. Unfortunately, you are squarely in an area that is probably the most controversial on this particular subject in all of New England. Some of the Canadians would like to deny even that the last glaciation ever crossed the lower St. Lawrence; all the ice here had grown independently. They have done this based on the work in Nova Scotia and Newfoundland and extended their ideas along strike to the southwest. There is no doubt that there was a Laurentide ice sheet extending all the way to Long Island across all of New England west of Maine. So you are right here in this zone of transition in terms of ideas and their development. An idea of wide extent and long persistence of overall Laurentide overriding ice sheet contrasts with an idea of relatively brief invasion of that ice, followed by the development of local sheets or carapaces.

Moench: What we are concerned about here is this chromium anomaly in stream sediments, very fine grain material. Do we start looking for chromium deposits or could it have come from a hundred miles?

Slack: It is on strike as I recall with the northwest-southeast trend of chromite anomalies in pan concentrates that are interpreted to come all the way from the southeastern Quebec ophiolite sequence.

Schafer: In general this refers to problem of local sources of chromium versus main Quebec source; and to effects of systematic SE transport by main Laurentide ice sheet versus subradial transport by carapaces. Subradial movement would be short, slow and weak, and might produce little effect.

Koteff: Except for the distance involved, it (the ice) would be drawn down in a short time.

Schafer: At least during the phase when the ice sheet is being cannibalized by carapace development.

McCammon: Do you have source here?

Boudette: Yes, chromite. Do you have nickel?

Nowlan: Yes, I think so. Chromium is the most obvious. You could argue there is also a trend of nickel.

Schafer: Because if this is just half the length of this anomaly belt (with the rest in Quebec) it is even a lot more striking, and you are not even sure you have the down-stream end. It may well extend toward the coast in the Bangor 2° sheet. The very striking thing about this is that it has an extraordinary linearity. Just what we see on your map, let alone that the longer it extends, the harder it is to explain why no fanning happened.

Koteff: Unless it really is streaming in a linear way.

Schafer: Yes but, there is nothing topographically to explain that. This part of Maine is next to the coast, in the middle of the Gulf of Maine.

Koteff: This could be the reason why it is so linear.

Bothner: Would that accelerate the flow into a large embayment and promote a linear flow?

Koteff: It would steepen surface gradients which would control the rate of flow. (That is the ocean in the Gulf of Maine, in contact with the ice margin, is rapidly drawing down the ice sheet in Maine by the calving process)

Bothner: It would enhance the flow in this particular area that was not oriented toward the major estuary.

Schafer: If it could be shown as being drawn down here into the Gulf of Maine, then there would be much less chance for local variations in the slope of the ice sheet such as would produce a fan-shaped rather than linear anomaly. It would maintain a maximum slope of the ice surface and, therefore, maximum velocities. The greater efficiency of calving into the sea versus subaerial melting means you can produce reentrants in the ice. In the Gulf of Maine and backing into the broad edge of the ice sheet, you do not get a deep narrow reentrant such as one did in the Gulf of St. Lawrence.

Koteff: That calving and drawing down could still happen as long as the sea was in contact with the ice all along its margin in Maine.

Schafer: What is needed is some rationale of why the glacier should do this here and the one that Carl suggests is the only one that comes to mind. The topography out there in the deep Gulf of

Maine and the concentration of flow outward to the Northeast Channel is the rationale for the drawdown in the Gulf of Maine. Your map area is a piece of terrain which does not in itself have bed rock surface topography which would channel the flow like that in a linear way. But if drawdown into the Gulf of Maine can maintain the maximum surface slope of the ice, can control it within a fairly narrow range of azimuths through the period that the Laurentide ice is moving SE, before the zipper effect of calving up the St. Lawrence and the change to the carapace phase, then that should give one a rationale. The reason nobody will look for one up here in your map area is that there is not enough topography to channel the flow. Something like the Connecticut Valley in CT and Ma is that kind of topography. This anomaly does not necessarily represent an ice stream or a sector of higher velocity of ice that coincides with the anomaly. The system we are talking about would be to maintain a more than average narrowness of a range of azimuths of direction of flow, and that means that would keep the anomaly more narrowly linear downstream the source. The ice on both sides may have been flowing just as fast controlled by the same factors. The same factor is drawdown into the Gulf of Maine, you see, but the reason for the linearity of this is the relatively invariant direction of flow. You can't, you really shouldn't, get a differential stream within this ice in itself, because this drawdown effect is felt for quite a few miles along the coast of the Gulf of Maine, should be more or less uniformly. It isn't until you get way out to Northeast Channel that the whole thing funnels in between Nova Scotia and George's Bank; you certainly get a ice stream out there at maximum extent. But that drawdown should be acting more or less uniformly along quite a sector of the coast of the Gulf here, but with this persistence of control of the direction. You see if you don't have that kind of control by drawdown out there, then there are all sorts of other factors that can intervene to produce variations in direction of flow of ice, especially once it's past its climax, which are minor variations in the bedrock topography, variations in the addition of ice because not all the ice comes from back up in the center of Quebec. A lot of it grows in place out here in the marginal areas, especially since moisture-bringing air-mass systems tend to drop most of their precipitation as they begin to climb up on the edge of the ice sheet and a lot of them never penetrate; much of the ice is built out near the margin. That kind of thing can produce a lot of little local variations of the flow of the ice, especially as it's thinning, so what is needed is something to overwhelm by some other mechanism that ordinary tendency to veer in direction. We are suggesting here that persistent drawdown into the Gulf of Maine is that dominant factor.

<u>Koteff</u>: It had to happen early on too because by the time you find the ice back here (just off the present coast), the depth of the water isn't going to be so great, you're getting shallower and shallower. If the ice is grounded, even if there is sea water against its margin, if its grounded, rather than floating, the rate of calving is much reduced.

Koteff: This would only be affected when it is here somewhere (just off the present coast of Maine).

Schafer: That is the critical point up at the Columbia Glacier, the grounding line versus the margin.

Koteff: It was grounded further out than they think in Maine.

Moench: There would be no way of tracing it back.

Koteff: The ice is still on the coast.

Moench: Once you get out of the major belt of ice-streaming for example, we could see nice anomalies associated with specific plutons. We can see anomalies associated with this group of rocks over here and you can relate geochemistry to individual belts of volcanic rock. No real evidence of dispersion.

Nowlan: You do not have big tails.

Slack: But you have that tungsten trail.

Moench: You have the tungsten trail over here.

Slack: It could be coming from a tungsten skarn.

Moench: How wide is the ice stream likely to be?

Schafer: No answer to that question. It is as wide as the circumstances require.

Schafer: It is usually controlled by bed rock topography. There are certain limits of relief parameters below which you don't have effects; that is, a valley the size of the Potomac Gorge out here doesn't produce an ice stream. You have to get to bigger topographic dimensions than that. There wasn't actually a stream producing this linearity. Instead there was a more or less uniform motion of the whole sector. There would be an ice stream at the Northeast Channel at the mouth of Gulf of Maine at time of the maximum.

Canney: Over here where we have the real high country and the White Mountains there is not much chance of any streaming ice, is there?

Schafer: It was crossed by the last ice. Probably a shadow in the lee of it. Shilts shows that in the glacial lee of Little Megantic Mountain there is no chromium or gabbro cobbles or anything. It flows right around it. There has not been enough detailed work to unmistakably define the pattern of ice flow over the highest part of the White Mountains. There is no doubt they were crossed but just what effect they did have is not known. The big indicator fans that were mapped in New Hampshire years ago, the Ascutney fan, the Red Hill syenite fan, the Ossipee fan, they show the ice is moving over them. But they aren't exactly in the league that White Mountains are in, for topographic height and extent, so they don't seem to say anything particularly about this ice being diverted by the White Mountains. Ascutney is way to one side but Red Hill is due south.

We have excellent control of ice flow by the Connecticut Valley across Massachusetts and Connecticut, and by the lower Narragansett basin. But it is the linearity and the absence of that kind of local topographic control that is striking in your map area. It does suggest speed, because of the sheer distance involved and the volume of material.

You can probably convert these anomaly numbers into something like volume of source chromium-containing rock. Your numbers tell you something about what the actual absolute amount of chromium is. There are some studies that indicated that fines of glacial deposits may indicate greater average distance of transport then one figures out just from looking at identifiable fragments of the rocks. That's what this seems to be saying too. You have a striking linear anomaly which in my extremely limited experience is considerably better developed than one ordinarily expects from glacially distributed materials. Carl suggested this rationale which is surely quite a reasonable one for maintaining unidirectional flow, and then the zipper calving effect up the St. Lawrence cuts the whole thing off, probably fairly abruptly. Rapid drawdown into the Gulf of Maine producing steeper ice gradients inland can persist only as long as the ice is being fed actively from across the Gulf of St. Lawrence. Once the calving embayment begins to cut seriously back up the St. Lawrence estuary, then that cuts off that supply and that's when marginal retreat increases. The drawdown produced by the relatively rapid flow on the steeper gradients is not made good by supply from behind, and instead the high part of the ice surface is drawn down and produces lower gradients which can't be steepened again. So the whole system slows down, and any minor local variations in direction then happen under that retreatal system in the late phases. Such variations involve velocities low enough and time short enough that you don't distribute material very far, so you don't substantially impair the anomaly pattern that has been imposed earlier.

There isn't any real rationale for concentrating (funneling) ice flow. Couldn't do that without bedrock topography to channel it. When the drawdown is occurring uniformly along this front into the Gulf of Maine and when the overall general slope of Maine is not marked by large troughs, there just isn't any obvious way to do it.

Moench: Shall we look at the other elements? Heavy minerals, a little bit of zinc and lead and silver showing up in the Rangeley here. Nothing much in the Sangerville belt.

McCammon: For these deposit types then, we have only the geochemistry. The question is, what kind of deposit types are we likely to have, if any? We have been focusing on the massive sulfides and volcanic associations and to some extent tin in the Conway granite. The general feeling in New England area is the sedimentary tracts really are not where you go to look for big deposits. There is a good chance this is a mistake.

(Ed. Note: A discussion about sediment-hosted massive sulfide deposits begins.)

Shanks: I agree that the world-class massive sulfide deposits are in sedimentary rocks.

Moench: Blue Hill maybe? We have a central track in the Sherbrooke-Lewiston sheets of Cambrian or Ordivician flysch-type deposits and volcanic rocks. The volcanics are in rather tightly constained narrow belts, predominately of Ordovician age, but also some of Cambrian age that are ophiolitic. In here is the ophiolitic Jim Pond Formation and scattered isolated volcanics of Devonian age, strongly bimodal. Rather thin, probably shallow marine, they are aquagene, some pillow basalts but scattered, not much in the way of resource potential. Scattered occurrences of Silurian volcanics, just a few outliers still preserved of basaltic volcanics, but with some felsic. Nothing in the way of andesitic composition. The sedimentary assemblage of Silurian and Devonian age are shallow and thin in this tract, pretty much unconformably lying on top of the pre-Silurian here. These are Devonian shales (Seboomook), sandstones (Tarratine) and some volcanics (Kineo); shelly-fossiliferous, very likely a fairly shallow, shoaly setting.

The Silurian and Devonian to the southeast (Kearsarge-central Maine) is thick probably in the order of 7 or 8 kilometers and complexly deformed. Carbonate-rich on the southeast and entirely clastic to the northwest, with northwestern source areas in this general region here (Boundary Mountains). As you get up into the Devonian of the Kearsarge-central Maine these are really muddy sediments and largely southeast-derived from the coastal volcanic belt, but with some contributions from this region up here (Boundary Mountain). Again, no volcanics. The bulk of these sediments was probably of volcanic derivation but much changed, either in the source area or during transport place to the other. The site of deposition was an extensional basin.

As you go to the other side, Connecticut Valley Gaspe Synclinorium, traditionally thought to be Devonian, mostly Devonian, there is a suggestion that a large part of that is quite a bit older than that, probably as old as Ordovician, or older. The rocks are predominately fine-grained clastic rocks, but with some carbonates in Waits River Formation here. Thickness is unknown. Source, again unknown, just has not been enough work done in that belt of rocks. We are getting some suggestions that some of these sedimentary units are at least weakly metalliferous. We do not have a lot of data on the rock itself, but you see copper staining on some of the joint surfaces, cleavage surfaces.

Some of the Devonian of the Kearsarge-central Maine Belt is comprised of muddy turbidites, mud silt turbidites, they have lenses of ironstone, with a tenth of a percent or so of copper as chalcopyrite. Some of the geochemistry suggests an association with these units, copper and other metals too. What are the facies that one would look for, if one were interested in finding metal deposits of sedimentary association? Not so much what the deposits look like when you find them, because we do not even know if they exist. We have to use the information we have.

<u>Nowlan:</u> Are they Devonian or older?

<u>Moench:</u> I expect they are. There are Devonian fossils in Quebec. They are in Compton rocks, which are mud-silt turbidites like the

Seboomook and Carabassett in Maine. When we get some dates down through here (Connecticut Valley-Gaspé) it will change the age of the Gile Mountain and Waits River Formations. A lot of reasoning has gone into the Devonian assignment; but it is reasoning with no hard evidence that I know of in terms of crawling over contacts, on hands and knees making sure the tops are always in the same direction as you go across, and being confident that you are not looking at a fault.

May 17, 1984

1:00 - 4:00 p.m. Discussion by Pat Shanks on geologic environments favorable for sediment-hosted massive sulfide deposits followed by discussion of favorable geologic settings for these deposits in the Sherbrooke and Lewiston area

Shanks: To get back to your original question about host rocks and sedimentary deposits. My experience is in the Selwyn basin. I have worked on a prospect called the Vulcan and the Anvil Range deposits (Central Yukon) which are fairly well messed up by deformation, metamorphism and I have studied the literature on the deposits at Howard's Pass. These are all related to lead-zinc metallogenic province and lead-zinc deposits. The host rock is a fine-grained siltite often referred to erroneously as a black shale. We are getting carbon, but when you analyze it, there is not a tremendous amount of organic carbon, 1 or 2 percent or something to make the rock look dark, as opposed to 6 or 7 percent that makes it oil shale. Faulting that was active during sedimentation is important in some cases. One might infer from your description that you have something analogous. The Vulcan deposit which is a noneconomic, fairly small prospect, in the eastern Selwyn basin along the Yukon-Northwest Territory boundary is in one of these faults that was active during sedimentation. The Jackson and Beale (1967) theory is that the ore fluids for these lead/zinc deposits are the same as ore fluid for Mississippi Valley type deposits. Basinal brines are migrating out of the basin. At the edge of the basin, if it finds a nice porous carbonate unit, it makes a Mississippi Valley type. If it is exhaled on the seafloor, you get a Howards pass, MacArthur River type lead/zinc deposit. The faults are often referred to as growth faults that occur along the boundaries of the basin, especially where you have a transition carbonate clastic units or little coarse blocks with carbonates on them with clastics in between. These are considered as a good environment. Jason is another one. Jason and Tom at MacMillan Pass occur in generally fine grain clastics but some coarser grained breccia units very closely associated with penecontemporaneous faults. That is a favorable geological setting for lead/zinc derived from a basin environment.

McCammon: How do they prospect for these types of deposits?

Shanks: Geochemistry and geology for the most part. In the case of Howard's Pass, they sampled the ore in outcrop and did not realize it was ore. Sent it in for analysis. It was 10 percent lead plus zinc. Very fine grained. Like slate. Looks like a black shale. You would walk right by it. Whoever took that sample was not very alert because the density should of tipped them off. Many of these have stratiform barite units with them and those have been prospected pretty carefully in the Selwyn basin. Unfortunately, in the Selwyn basin there are stratiform barites all over the place that have no lead or zinc or very little lead/zinc mineralization associated. A lot of the lead/zinc deposits, (Meagan and elsewhere) have sulfate horizons stratigraphically above the sulfide horizon. Base metal geochemistry (soil or rock) has been something they have used.

Moench: Hosts are not necessarily turbidite?

Shanks: In some cases they are, but they are pretty fine grained. Not the typical turbiditic sequence with coarse sandstones. In the case of Jason, you get breccia which is related to talus coming off this active fault during sedimentation and it looks like they had a fine-grained mud slide that capped off and stopped the deposit. There is a lot of fine grained material there. I do not know if you can view these as distal turbidites. Sullivan is an example where there are good turbidites, the host rock there. They claim they can trace the individual rhythmic banded unit for 20 to 30 kilometers around the Sullivan deposit. The deposit has a lot of copper in it, a bit different setting than the Selwyn Basin, but turbidites would not be a bad place to find one of these. There is some carbonate, a certain percentage of sedimentary rock in most of them, but they are dominately clastic. In the Anvil Range, the deposits are lead-zinc deposits, highly pyritic, a little different than Howard's pass type. They occur at the transition of an underlying pelitic unit, a lower Cambrain pelitic unit to a Lower Cambrian carbonate unit. The carbonate unit has a lot of detrital material in it. There are around the Anvil Range a number of sort of stratiform graphitic horizons. They are much more abundant in the area of the massive sulfides. As you go a certain distance to the northeast, you no longer get these spotty stratiform graphitic units. They look like the normal host rocks except that there is enough graphite in them to make them look blackish and be logged or mapped differently. There are 5 periods of deformation. There may have been some kind of bounding fault and to the northeast of that fault you no longer pick up the graphitic units. Probably, it was the upthrown block although you cannot really map it in the field. Having some sort of fairly good size sub-basin that gets you these graphitic Pods at places is another thing that would be favorable.

Moench: In the Kearsarge-central Maine belt there is strong indication of a lot of deformation or contemporaneous faulting. A lot of the rock is turbiditic. There are different kinds of turbidites there, the younger ones in the Devonian are mud silt-type, low-density turbidites. Lots of graphitic units. The most prominant one is the so-called Small Falls Formation; about 700 meters thick. Its thickest part is along a narrow, belt longtitudal belt. Very graphitic and sulfidtic. Other graphitic-sulfidic units occur at different levels.

Shanks: One of the things that has come out of the Australian work on places like Mt. Isa and MacArthur River is that there are salt casts that indicate even though there is fine-grained siltites, the depositional environment was occasionally desicated and very close to the same horizon as the occurrences of mineralization. That has not been shown for several of the Canadian deposits. Inferences are fairly substantial, water depths, 200 or 300 to a thousand meters or something like that. But that would be someting to keep in mind also.

Boudette: Are there tourmaline belts?

Shanks: There is no tourmaline known in the Selwyn basin. Of course it could have been missed especially with a lot of graphitic rocks being mapped as black shales and graphitic schists. As far as I know, people have not been doing boron analyses.

Moench: Some of the Devonian in here (Carrabassett) from the data I have in this region, is richer in boron than the Silurian section. A couple hundred ppm boron. Consistently less in other parts of the section. I think from what you say that there is a lot to consider but at this stage we have to see what the geochemistry says and then make a resource assignment as being speculative.

Shanks: There are no evaporites or baritic horizons?

Moench: No, not recognized. Along the margins of the Connecticut Valley Gaspe Synclinorium further south, someone studied fluid inclusions in the quartz veins and found consistent presence of halite in quartz veins associated with a base of this section, in southern and western central Vermont, and from that data concluded that this assemblage had some evaporite, now mostly gone, preserved only in the fluid inclusions. I think that the so-called Greenvale Cove Formation has some odd chemistry that might express a hypersaline environment.

Shanks: If you had any evaporites in there, they would be gone.

Moench: Yes. Certainly, there are some metal associations as we were just getting into. This unit in here shows some consistently interesting associations (just north of the Gore Mountain plutons) . We would call it the Frontenac Formation.

Shanks: A lot of the deposits probably form in anoxic waters so you do not get ferruginous manganiferrous cherts capping the deposits like you do for many volcanigenic deposits. They probably form more slowly as they often sit in several horizons which have quite a bit of stratigraphy and so you are talking millions of years or at least a million years to form individual deposits in some cases. Under those kinds of conditions the manganese and iron, possibly some of the iron gets dispersed in the water column and can form a halo which has regional significance. I think Rammelsburg and Megan both have good manganese anomalies for quite a distance around the deposits.

Moench: Most of this would be anoxic, some places at times extremly so. Actually, there are evaporites; in Silurian of the Gaspé.

Shanks: The Blackhawk deposit does not have any barium associated with it, does it? It has that carbonate-talc alteration zone.

Moench: Just from the look at boron and the heavy mineral data associated with the Connecticut Valley Gaspe Synclinorium and the Frontenac-Chartier belt, so far we have seen the association with bismuth, lead, tungsten, precious metals, copper, and a little bit of silver.

McCammon: Is the Red Dog considered this type of deposit?

Shanks: Right.

McCammon: What type of host rock is it?

Shanks: It is in the Mississippian. Red Dog is an amazing deposit. They are reporting 77 million tons of proven resources at 22 percent combined lead, zinc cut off, plus high silver (Menzie, W.D. and Mosier, D.L., 1985). That is a very conservative estimate of tonnage. The deposit is in 4 thrust plates. They have taken the lowest cohesive unit and cut out a high grade ore. It is probably 250 million tons. Do you have just stream sediments geochemistry?

Moench: Also, heavy mineral geochemistry. Essentially no mineral occurrences until you get down into Vermont copper. Let us see now, Mt. Glines deposit is sitting somewhere in here. Not much of a deposit. Described by W.H. Emmons (1910). I think it is a vein deposit. Could even be one of the veins associated with the White Mountain batholith.

Cannon: What about the sodic alteration?

Shanks: In the Selwyn basin there are not many well defined alterations zones. Around the Anvil deposit, there is a white mica alteration. As far as we know, there is no paragonite. It is sericite and K-mica. It forms an envelope around the massive sulfide deposits. Some geologists believe that it is more pronounced in the foot wall of the deposits, but when you look at maps, it does not come through. It is in the hanging wall and it occurs sometimes as a doughnut around the deposit. It is impressive to see in the field because you can walk along an outcrop or inspect drill core and see these monotonous pelitic schists, brown-green color, all of a sudden, you might get into one of these graphitic lenses where it turns blackish and then right in a piece of core you see transition. Graphite completely goes away and you are into white mica plus quartz and not much else. Sometimes the contact will cross-cut the foliation of the rock. It is clearly alteration. There has been bleaching. The total extent is not much bigger than the deposits. The deposits are 500 meters in diameter in the Anvil Range. You might get a thousand meters in diameter with alteration. Other deposits like Howard's Pass, there is not a well pronounced alteration envelope. There may be something like dolomitic addition to the rocks or silicate addition or something like that but I do not think you can map it.

Moench: Especially with the sillimanite, might make a white schist.

Shanks: The Anvil Range deposits occur along a sort of curvilinear trend which is about 20 to 25 kilometers long and one of the deposits, the Faro deposit, which is the only one that has been mined, is in the contact zone with a Mesozoic Batholith. It has been metamorphosed to amphibolite facies and a white mica envelope is clearly preserved. Each district has its own characteristics. Many of the large sediment-hosted lead, zinc deposits and even copper-lead, do not show much alteration.

Moench: What would produce the alteration? Hot brine on the sea floor?

Shanks: That is what I think. I could be wrong. The fact that it often extends into the hanging wall indicates the hydrothenmal system continues beyond the formation of the sulfide either as a pool of brine that sits there for some period of time or as continued venting that cross-cuts the deposit and produces alteration in the hanging wall. It is possible to conceive of a heavy hot brine pool that is making alteration zones as detrital sediment accumulates in it.

Moench: As an exploration geologist, you would not be looking for alteration zones in the same way that you would be if you were looking for massive sulfides.

Shanks: Right. Not to the same extent as you would in the volcanogenic type deposit's because Howard's Pass, MacArthur River and probably Mt. Isa just are not there. You would find the Sullivan deposit by the time you recognize the alteration. Trying to define the sedimentary basins and looking in graphitic units along the margins of the basins and then looking through base metal anomalies would be the way to try.

Moench: That is about where we are. Indications are that may be what we have. We have the contemporaneous faults or at least precursors. These things would not make nice channel ways, they are tight surfaces, they look like semi-soft semi-lithified water-bearing shale against semi-lithified water-bearing shale; tight slip surface, very little disturbance on either side; now are cooked up to high metamorphoric grade; not likely to have been permeable originally. I think we have two different kinds of early faults. The semi-soft type faults are confined to the sediment column; may involve as much as 4 or 5 kilometers of section as one big slump or slide unit. But on the east side of Gardner Mountain, the fault that separates the Albee from the Ammonoosuc cut somewhat more brittle rocks. There are deposits in there, vein type deposits, coursing back and forth and a strong metals anomaly.

Shanks: The hydrologists never seem to like the idea of fluids migrating along the faults because in near surface environment, the faults often are impermeable rather than permeable. If you talk to Dick Henley about the geothermal environments, a lot of times, it is clear that the fluids migrate upward along the faults. I do not know if examination of a fault in the field 500 million years later could tell you whether it would have been a conduit or not.

Moench: It could have been an opening developing in the environment and then closed up again, which is what I think happened here anyway.

McCammon: The decision to make, Bob, is this, do you have enough to say that this is a geologic setting similar to the kind we have been hearing about. Basically, if you can say that, it is a speculative resources.

Moench: Yes, I do not think what we have here necessarily conflicts with anything that Pat has described.

McCammon: The source, I presume are the brines.

Shanks: The idea is that these are basinal brines.

Boudette: Red Sea type brines?

Shanks: Probably not Red Sea type brines. The Red sea brines are geothermal brines derived from sea water, modern sea water which circulated fairly rapidly into the subsurface, got the salt from the evaporites and then heated to probably 300 degrees in contact with rift zone intrusives and then buoyantly made its way to the surface.There could be something like that, but in the Red sea, the Atlantis II deposit sits on maybe a meter of detrital carbonate and then basalts, rift zone oceanic theolites. That would clearly be representing geologic presence of igneous rocks. In many of these sediment hosted districts, there is very little representation of volcanic material.

Moench: How about intrusives in the sense that we see a correlation between mafic intrusives and some of these semi-soft faults? The gabbros seem to have come in along these surfaces; this could provide a source of hot water.

Shanks: Well, there is the Red Sea system. There certainly are deposits that have the connection, with igneous heat sources and that would not be considered unfavorable. In places like Mt. Isa and MacArthur River and Howard's Pass you can demonstrate virtually no contemporaneous intrusive or extrusive activity. There are tuffs in the sequence in the Anvil Range, there are probable mafic tuffs at the same horizon as the mineralization there are chloritic shists that have not been studied carefully. Some of them look like tuffs and as you go up section above the horizon of mineralization in the Anvil Range, you go through the Vangorda Formation which is a carbonate unit and above that is the Menzie Creek Formation which is a good size Ordovician volcanic pile. Some form of volcanism started at about the time mineralization formed and gradually became more important upward above the horizon of mineralization.

Howard's Pass is at least 270 million tons of 8 or 9 percent lead/zinc (Menzie, W.D., and Mosier, D.L., 1985). There is no volcanic correlation at all there; that is where people say, it has to be basinal brine. It is compaction-driven out of the source beds; also the fact they are lead/zinc dominated. There is copper in (at least) mafic igneous rocks and in the case of Red Sea.You see copper represented in saline deposits. There is no reason to think there is not copper in something that might be here either because you have Sullivan which is copper, lead and zinc mostly lead/zinc. Of course, Mt. Isa has more metal value in its copper than in its lead and zinc. You do not have to have igneous activity to have a deposit or at least not expressed locally.

Canney: At Sally Mountain in the basal conglomerate, you get pyrite and copper. There are some other spots there where traces of mineralization exist in some of the basal arkosic Silurian, maybe Devonian, sediments.

Boudette: On East Sally Mountain there is a good anomaly; a good strong copper appears in stream sediments.

McCammon: I gather then, in general, units around these deposits tend to have higher than average background in many of the trace elements, copper, zinc.

Moench: One thing that showed up in the Wilderness studies was fairly high levels of copper.

McCammon: How high? Over 500?

Moench: Oh, no. It was copper; it showed consistent weakly anomalous copper values related to the lower Devonian schist and gneiss relative to everything around it.

Canney: Do you recall Bob, that high copper up there in the Littleton? In that apparent anomaly we might have a couple of populations merging to give it a apparently normal distribution maybe the high copper is only relative then when compared with the low copper values in some of the Jurassic plutons that were included, with the data set. I would want to investigate further before going so far as to say those sediments are slightly anomalous in copper.

Shanks: I have not worked much with geochemical exploration. I am not sure how big a halo of the deposit making base metals , copper, lead and zinc. They might be pretty local to the site of deposit formation. Manganese would be one that would be more widely dispersed. On the modern Red Sea type system, there are quite a few base metals that are enriched in the water column over the brine pool, including copper, lead, zinc, cadmium, mercury, manganese, iron. Presumably those are particulates that settle on some kind of nucleys in the brine. You do not have any good strong enrichments of lead or zinc.

Moench: Not really.

Canney: We have occassional scattered high values but they are based on the fact a lot of these maps are percentile values. Particularly in some of this terrane here, we have not considered completely the possibility that some of these might be due to contamination particularly in southeastern part of the Lewiston sheet.

Shanks: If you have places with good stream sediment anomalies, then you would follow those up with more systematic soil and rock sampling in the area. As far as I know, geophysical techniques have not been very successful in exploring for these types of deposits.

Moench: In the set of heavy mineral concentrates, in this belt (south of Littleton?) we see a very tight control of tungsten and the little data we have on gold and silver, copper, boron, and barium. Scattered data.

Shanks: These are from the concentrates?

Moench: We are also getting some association down in the Rangeley Formation; zinc, silver - a little bit. This is a very thick, very clastic section. End meta's associated with metasedimentary formations.

If you total up all of the varieties plutonic rocks indicated so far by map symbols, there are some 85 different map units. This separates porphyritic from non-porphyritic rocks and all the different streckeisen compositions; plus characteristic minerals such as biotite, or hornblende plus biotite, or two-micas, or maybe with augite; plus textures and structures. I've classified the rocks by map symbols derived from the triangular diagram modified from the new Maine Geological Survey And for those units that have some formational significance like the Conway granite, I insert a lower case letter between the J, for Jurassic, and 1, for granite. The resulting map symbol for the Conway is Jc1b.

(Ed. Note: The discussion returns to plutonic associations.)

For resource considerations, we have to consider particularly in terms of two different plutonic associations. One is the White Mountain rocks which are Jurassic in age and correlate with big geochemical tin anomalies, plus anomalies of a wide range of other elements. Then, of course, all the other plutons of Ordovician to Carboniferous age. The data show some very interesting relationships there too. But all we have is geochemical data; we haven't made any kind of systematic study of the plutons. Classification of the rocks is based on whatever we could get out of the literature, or from more recent work done in local areas. Some of the descriptions are poor, very few modal data particularly in plutons in the Northeast Kingdom of Vermont. Tough to tell from the descriptions what the rocks are.

Ayuso: What percentage of the blue units there are actually Silurian?

Moench: Actually dated are only 2 plutons. One is the Moody Ledge granite, one of the Oliverian domes, with an age of 423 million years; and then the East Inlet pluton up here, dated at about 430 million. Those are the only two. Much of the rock in the Jefferson Dome is probably Silurian. The rocks at the east end of the dome look very much like the Moody Ledge pluton; very light color, pinkish leucocratic biotite granites.

Ayuso: Has anyone looked at those for the possibility of mineralization? Catheart is going to turn out to be of that age without doubt.

Moench: I think Gene will want to separate out the Cathart and Sally Mountain from the Attean. The geochemistry of the Cathart is really interesting, but so is the whole Attean,not just the younger part of it. I assume the older date of 445 Ma for the Attean is correct.

Thursday, May 18, 1984

8:30 - 11:30 a.m. Discussion by Steve Ludington on geologic environments favorable for tin concentrations in granitoid bodies followed by discussion by Leslie Cox on the interpretation of the field data for undiscovered tin concentrations in the White Mountains area in New Hampshire

Ludington: From this meeting is coming a rough draft of the words of the resource appraisal. Is that what the goal is here?

Moench: Yes, the goal is to come up with a preliminary mineral resource potential map. We have gone through the system, the procedures of working over the volcanic terranes, we looked over the sedimentary terranes. At this stage, we are looking for broad scale attributes that we can use in deciding whether this pluton or that pluton has a potential for any type of deposit.

Ludington: I would like to talk a few minutes about this and then hear about the chemistry. The essential characteristic of the Nigerian rocks, which is I think everyone has to agree are the closest analog for this system, is the fact that an intermixture of the very much alkalic-calcic appearing with the very alkalic rocks; there are peralkaline and peraluminous plutons coming out of the same magma chamber or at least in place above the same round feature in the crust, and they are intermixed. I have not looked at the geology of those places in the detail necessary to say that they always occur in a certain sort of sequence but it is reminiscent of where I am working now in Questa, New Mexico, where we have a peralkaline, ash-flow sheet. Most of the ash-flow sheet is peralkaline and many of the intrusive rocks associated with the caldera are not. As far as deposition of tin, the big difference between a peralkaline and the nonperalkaline rock is that the peralkaline rock can dissolve and thus hold during crystallization, a larger amount of tin, fluorine, and a whole host of other metals. Zirconium is one of these. There is experimental work to demonstrate this. When you see a peralkaline granite, you will find the zirconium content is 800 or 1200 or 2000 ppm whereas the limit of solubility in a peraluminous granite melt is around a 100 ppm. There is reason to believe that a number of the other metals behave the same way so that the peralkaline granites that are truly peralkaline may have a tin content of 50 ppm or 25 ppm. By the same token, none of that tin ever got remobilized in a hydrothermal fluid; so the bodies are anomalous and not of much interest if what you are interested in is something approaching economic concentration. But peraluminous or two-mica granite magnesia probably produce mineral deposits, because all of the tin as well as a lot of other metals have been stripped off during exhalation of the vapor phase. For the peraluminous plutons the place where the heat is going to occur, as the Russians would say, is the exocontact zone, out away from the pluton. So, for those plutons that come now exposed, referring to the drawing on the board, the chances of preserved mineralization are very low. From what I see on the geologic map, I would say that most of the White Mountain batholith is excluded essentially from being really high potential, by the fact of its depth of erosion. The really high potential regions left, are the volcanic pendants, the pieces of volcanic rock. Just as a preliminary to wanting to look at geochemistry, when you take a concentrate, what you essentially have is a sample of mafic minerals which is 1 or 2 percent of the rock. In particular, if you further split that with a magnetic fraction, you get very fine distinctions on an element.

We can go to G's in rocks that only contain a few 10's of ppm or only 10 ppm. We are doing a very efficient job of finding it with our goechemical techniques. That's why in some sets they are actually more interested in some of the other elements in that whole area that is anomalous. We have seen this in the Southwest. You can sample a stream from a rhyolite that has 10 ppm tin and get a very high tin number for the panned concentrate (it is basically hi-silica rhyolite) because there is nothing else. It is more severe there than it is in these granites, where you at least have some amphibole and mica to dilute it with. But in the southwest you may have some ilmenite, and a trace of cassiterite as the only iron magnesium magnesium bearing phenocryst, the only thing that ends up in a heavy concentrate. So a very ordinary rock is going come out with a G 2 thousand all over the place. Tin as well as some other metals.

Moench: You feel this is why in Nigeria all the ore is coming out of the placer?

Ludington: Yes, they are very closely related subjects. Given the residence for tin and the mineralogy of the rock, the placer environment is a good concentrate.

Canney: In the Conway, where the tin content is much greater than 2,000 ppm, and we have also found cassiterite.

Ludington: The right kind of mineralization does occur.

Canney: The thing that makes me wonder and impressed me about Conway as a possible tin source is that we also get strong anomalies in standard stream sediments. In many of these granites out here in the west, you get 20 ppm; it is high, whereas we get values in standard stream sediments going up 700 ppm.

Ludington: Okay, that is the place to look.

Canney: This to me indicated Conway is a potential tin source.

Ludington: Is there accompanying zinc?

Canney: Yes, there is a match in zinc; and also lead.

Ludington: The other aspect to talk about is grade and tonnage. We have mentioned in passing that the production in Nigeria is from placers. I have not seen information about primary grades. I suspect there are in a range to 0.02-.03 to about .1 percent. I doubt you would get a body of rock much bigger than about this room that is above .1 percent tin.

Moench: In this type of setting, say in your peraluminous setting, these Conway Granites are peraluminous to metaluminous, the same as in Nigeria.

Ludington: Yes, the biotite granite, but are there not some alkaline variants too?

Moench: Yes, but they are slightly different. The question in my mind, is your ore body, in the peraluminous setting, are they well defined discrete, or do they have halos perhaps of lower grade?

Ludington: It is not well known. I was just hearing about Kougarok in Alaska. That has over an area a few square kilometers. The mineralization is away from the pluton. There is a large dike there. It is in several apparently discrete roughly planar bodies. These are bodies that are greater than .1 or 15 in a matrix of the whole ground surronding the pluton is .0X that sort of thing or .02 or .04. The ones I saw in Europe had actually been drilled out and there we knew the acutal distribution. Best example I guess is Bal Hemerdon Amax property which was was for tonnage now small, 60 million tons, 40 million tons, something like that of .15 and .2 in each tin and tungsten. That was pretty homogeneous. That is a different situation because that orebody is in the granites and is not so much of a stockwork. If it is like the occurrences we are finding in Saudi, its veins are 1 or 2 centimeters up to 20-30 centimeters wide spaced every few meters. At Mt. Pleasant, New Brunswick, tin in the porphyry ore body is very low. The large tonnage deposit there is essentially a tungsten-molybdenum-bismuth deposit. The tin has moved higher in the system, and is in some skarn-like bodies and breccia pipes, where it is pretty high grade; some of it has several percent tin. The cogent fact about Mt. Pleasant is that the tin is separated from the rest of the metal, it is further out from the center. I am not familiar with East Kempsville. That is also in the range of somewhat under a 100 million tons of around .2 to .3 tin. That type of deposit has a good chance of being able to be mined because it is nearer to a 100 million and because it is over .2. The other aspect that is problematic about developing tin deposits is the metallurgy. Mt. Pleasant is starting to produce now. They spent 15 years working on a mill circuit. It is a well known problem in Bolivia that they lose a third to a half of there values in the deposits. Our job in a situation like this is to consider potential resources not ore deposits.

Cannon: The fact there are two mines just across the border in Canada is encouraging.

Ludington: I think it is easy to miss them; these things are not as big as porphyry copper deposits. They are easier to miss. Just a 20-30 million ton ore body would be of interest.

Canney: What about Salt Mountain in Nova Scotia?

Ludington: That is East Kempsville. As far as what it is like, I do not know of anyone that has been there. I do know it is Carboniferous in age.

Canney: What about the idea of Fehn, U. and others (1978) in the Conway on the theory of the development of hydrothermal convection cells, you know quite late after cooling?

Ludington: My feeling is that this model could result in at least some normal looking hydrothermal veins. My question is, are there some veins that were exploited for lead and zinc at one time?

Moench: Several, like the Mascot and Shelburne lead-zinc-silver veins, and there was the Jackson prospect that was mined for tin. Most of these are outside the main tin anomaly.

Ludington: So those are indeed very high grade.

Moench: Silver, zinc, just again one little locality and not within the anomaly.

Ludington: How far outside the complex is that?

Moench: It is right at the edge here.

Ludington: It was interesting to see in Cornwall, that all the production has come from epithermal veins. In most cases kilometers distant from the pluton.

Moench: From what you have said, we have, generally deeply unroofed plutons with patches of volcanics, some quite basaltic. The volcanics are pretty much down-dropped; in fact, one has some little slivers and slices of Littleton Formation which are really samples of low-grade metamorphic rocks 3 or 4 kilometers higher up. The surrounding terrane is sillimanite gneiss, which represents a depth of 10 kilometers or so in this area. We would be looking at a much greater favorability overall if there were more volcanics associated with the White Mountain plutons, and not down-dropped.

Ludington: That means that there has been 3 or 4 kilometers of erosion since the emplacement of these bodies.

Moench: The question is, out and around from these plutons we have geochemical anomalies. High tin and other elements too, out and around, the question would be are there Conway granites at depth there?

Lundington: Also you want to ask, I see the bodies decreasing in size to the north. Do you have any kind of feeling for the depth of stripping on that progression. Is that constant?

Moench: Metamorphic grade drops up there.

Ludington: I mean, since the Cretaceous.

Moench: Yes, in that direction, depth of stripping definitely decreases. Near the Katahdin Batholith in the middle of Maine, you are looking at levels now that were only at depths of maybe one and a half or so kilometers in the Devonian, and no evidence of their ever having been deeper.

Bothner: Perhaps a little because there is only biotite grade at the northernmost end.

Cox: What is also important are texture and geologic relationships found at a given elevation. You find miarolitic cavities and pegmatitic veins and other things like that which reflect preservation of a high level within intrusions in the north, whereas if you go to a peak further south you will not find miarolitic cavities or textural evidence that indicate preservation of a high level. You can see textual evidence that helps to estimate how much has been eroded away.

Moench: How about the Monteregian Hills? There are no volcanics up there in Canada.

Cox: At all of these, (pointing to the map at the Moat Volcanics-Conway Granite contact), the pegmatites are trapped beneath the contact with the volcanics. You first see small cavities in the granite, then larger ones as you gain elevation until you get to the largest cavities at the contact.

Ludington: You cannot calculate depth from these cavities, but it is an interesting point.

Cox: There is one paper that talks about erosion in the White Mountains.

Ludington: I am looking at this as a system,looks like it is related to a north-south feature. Is it better looking at the north end or south end, and sounds like the north end is most favorable.

Ludington: Do you see anything in gravity?

Bothner: In terms of changes in body shape to the north?

Ludington: Do you have any prospects for unexposed White Mountain?

Bothner: Very few. There is one far south of us that looks like it might be in the Manchester quite a ways away from Lewiston.

Bawiec: What you are saying, is when they are exposed, their favorability decreases.

Ludington: Yes, that is right.

Canney: But you have some hidden, like a deposit that has not quite reached the surface.

Ludington: Well yes, but the overall odds are down only because of the depth of erosion.

Cox: **(pointing to map)** - Here is where I picked up the highest tin values and noticeable sulfide mineralization, which was sphalerite, pyrite, arsenopyrite, and molybdenite. There, (pointing to map), most of the mineralization coincided with the contact with the volcanics or the contact with older quartz syenite porphyry and also with a fracture system that coincides with lineaments that where drawn from Landsat photos. In one location below Mt. Hitchcock, there are a lot of aplite dikes and various granitic type dikes associated with a mineralized area containing quartz-greisen veins. That's one of the places where mineralization doesn't coincide with a contact. There are several dikes. Suddenly you encounter a silicified area within otherwise apprently unaltered granite. I don't know if that would mean there is something beneath the area that is not yet exposed. The dikes could be the physical evidence for a buried pluton.

McCammon: Wally, with the gravity for the White Mountains, can you strip off what we see there and have a peek at what is below?

Bothner: It is difficult to separate the Conway from two mica granites that are around there.

Ludington: It is hard to find differences if the density of one is 2.6 and another is 2.6.

Bothner: It is not 2.6, a lot of it is 2.61 and a lot 2.62 between the Conway and the two-mica granites of the Sebago. We have done very, very careful traverses across that contact and it is extremely difficult to model it in a meaningful way. It is part of a large gravity low that extends from the western margin of the batholith all the way across the Sebago. The depth estimates are uncertain.

Shanks: What is the possibility for placer tin deposits 1 or 2 km downstream here?

Canney: They are excellent. Placer deposits in some of those glacial valleys up there would be well worth looking for. We have not done any sampling but we have speculated about it.

Ludington: Maybe that is worth highlighting, all the surficial material that is within the larger area of favorability.

Moench: I would think that would be the most favorable. It might be worthwhile to summarize the kinds of deposits that constitute evidence of mineralization. Leslie, do you want to summarize the evidence?

Cox: I made an attempt at that; (pointing to map), the rocks we collected are in black. My field assistant was doing a thesis on the Waterville pluton. I've indicated from the data we got back, which was AA-tin and E-spec, the values that were the highest, none of which are high. The highest value is 200 ppm E-spec and 60 ppm tin AA. I have the statistics on both sets of data for the rocks collected. I put the red dots on the map based on the anomalous chemistry which coincided for the most part with obviously mineralized rocks; mineralized, meaning quartz veins with alterations selvages or disseminated sulfides or sulfides of some type which can be seen.

In the area which I mentioned earlier as having quartz greisen veins, the sulfides are pyrite, possibly chalcopyrite, magnetite, and also fluorite. Another area here (pointing to map) at the contact of Conway and syenite has molybdenite, pyrite, quartz veins, and a tungsten anomaly. Over here (pointing at map) at the contact with Osceola granite and Conway granite, is some sort of fractured zone and hydro-thermal system. There is pervasive feldspar alteration, manganese staining, and disseminated sphalerite throughout the granite.

There are 2 different sets of high values. I have a scatter diagram for all the rocks which show that. The high tin AA does not coincide with the high E-spec. The highest E-spec values coincide with 2 ppm AA or no tin at all. I did quantitative and qualitative x-ray analyses on some of the samples to compare to the E-spec values. We also sent in replicas to check both the tin AA and E-spec. I do not think there is a problem as far as replicates go. The X-ray values match the E-spec values. The E-spec was more useful than the tin AA.

Moench: The AA was supposed to tell us about the nonsilicate tin.

Cox: The highest AA value at 60 ppm coincided with a 100 ppm E-spec value, and that is from a pegmatite. The nonsilicate tin would probably be in an oxide and not a sulfide in this case. In general, AA values were much lower than E-spec, which reflected total tin.

Ludington: Large numbers of your rocks came up with 10-15-20, is that correct?

Cox: Yes, for E-spec values.

Ludington: Do you have a histogram?

Cox: Yes, I have histograms of several of the different suites. The data indicate that some areas are more mineralized than others. The criteria for calling an area mineralized was a combination of visibly altered rocks which also had high E-spec or high tin AA values. I selected 4 rocks with that combination and examined their polished thin sections. Instead of cassiterite I found 40 micron zircons. They have reflectivity similar to cassiterite.

Ludington: Did you look at the inclusions of the biotite?

Cox: Yes.

Ludington: Are these ilmenite bearing, a lot of tin could be in ilmenite.

Cox: I looked at a polished thin section of a magnetite vein associated with sphalerite-fluorite-quartz veins down here (pointing to map). It has several iron-titanium phases in it and has zoned phases within the magnetite, but not cassiterite.

Ludington: You could have a couple percent tin in ilmenite.

Cox: The ilmenites I looked at did not have the tin. The only time I got a tin peak was on a quartz grain. There is a zincian iron-carbonate with fluorite. In the Mexican rhyolite tin deposits, it is reported the average size of tin grains is 7 microns. I was not looking at any grain smaller than 20-40 microns. I picked the large ones. Maybe one of the smaller grains might be a cassiterite grain.

Curtin: I do not see how you could see it in a thin section or any other way. That's practicully nothing; if you have 1500 ppm you might be able to see something.

Ludington: It is just as concentrated as a cassiterite. Do we have someone looking for cassiterite?

Cox: Yes.

Ludington: I do not see any in the ilmenites. You can see tungsten but never tin. One of the characteristics for a lot of large tin deposits, on all scales, the grade distribution is very heterogenous. I am not surprised that you could take a thumb nail size surface, could have 4 times as much tin as another thumb nail surface a few centimeters away. That is found at all scales, that is another reason they are difficult to find.

Canney: Isn't it true, Steve, looking at the granite, one indication of a possible economic deposit is when you have a lot of tin values on the rock that are uniformly high or uniformly low. In other words, the relative deviation, is quite high.

Ludington: Yes, seeing these numbers have, and from what we have heard, it sounds like most of the tin got out of the rock. Some might well have collected somewhere and it is probably in those metals or underneath those metals.

Cox: It might be possible to summarize in terms of what you are trying to do for the volcanic terrane: assign major type, such anorogenic type tin which is taken from Taylor, and 3 subtypes for White Mountain batholith area. The first subtype would be greisen-bordered veins, which is taken from Hosking from Taylor; or quartz-cassiterite. The second subtype would be stanniferous pegmatites. The third subtype would be sulfide-cassiterite assemblages or zinc-rich tin province. There is potential for those 3 subtypes under one major anorogenic type. Then, there is placer tin.

Ludington: We have the porphyry possibilities. We have the pegmatites which really are not very interesting. And tin rich hydrothermal veins which are potentially attractive targets.

Canney: They are finding quite a bit of the zinc-rich sulfide in Nigeria.

Moench: There is another type of deposit that may occur in there: namely the phenacite-lead-zinc-silver-beryllium deposit at Iron Mountain. The Jackson tin mine is just outside Conway.

Ludington: What is of more interest is what is it like.

Cox: The Jackson tin mine is right on the edge of the Conway. Iron Mountain is also in Conway granite. I think the mineralization is within the granite. It has been described as replacement lenses within the granite.

Moench: It has been described as replacement lenses within the granite.

Ludington: Has anyone seen it?

Cox: I have been to the Jackson tin mine. There is nothing to see at the mine site.

Ludington: There was no reported cassiterite?

Moench: Cassiterite in veins.

Cox: Another interesting area was within the Osceola granite. We found one quartz vein that was fairly high in tin.

Boudette: The quartz was dark?

Cox: Yes.

Boudette: Did you check it for uranium?

Cox: No.

McCammon: How about the spatial distribution of these types, Leslie, do you have in mind how you would generate a map?

Cox: The difficulty is that we do not know how the tin occurs, whether as cassiterite in veins, disseminated, or pegmatites.

Ludington: It is important to acknowledge the results of your work. You demonstrated the existence of all these types of mineralization. On the other hand, the sample of what you saw is minuscule. To worry about distribution of the types you saw is premature. There is no information with which to assign relative favorability.

McCammon: I am thinking of what areas are going to be delineated.

Cox: Generally, it is the same area that was reported in the Wilderness report.

Ludington: The key is the geochemistry.

Moench: Copper outside, pretty much mutually exclusive with tin.

Ludington: That would be the way to go about it. Using niobium to normalize.

Cox: To normalize, why?

Ludington: Niobium has not moved much so that if there is a medium tin/niobium ratio, tin/niobium ratios in excess of that, are actual hydrothermally emplaced tin. You would have to look at the two elements. The main population is the magmatic, with high tins outside of that. Do you have the histograms along? It would be interesting to see the samples that are anomalous in tin and not in niobium and see if those show as a halo around the plutons.

Canney: There are also high values for uranium, lanthanum, and fluorine.

Ludington: But niobium will remain even when lanthanum and cerium are moving into the hydrothermal system.

Moench: Some of the niobium is coming from older pegmitites. In terms of assessing the suite as a whole, we should use the broadest, general terms. Because it is deeply eroded, it eliminates, for the most part, the possibility of deposits above and around the plutons.

Ludington: It does, but I do not think it does away from the plutons where there are anomalies. That is the most favorable with the quailifier that there is no direct evidence.

McCammon: You do have these veins here.

Moench: All the deposits except one that are related to the White Mountain plutons are outside the principal anomaly. They are the lead/silver veins mined long ago. The Jackson tin prospect is outside the intrusive rocks, and outside the principal anaomaly too.

Ludington: To me, the striking anomalies are outside. This is coming from the rock, we know there was primary tin in those magmas but there was not in these schists and whatever else, so the fact there are anomalies means it came from somewhere or from underneath.

Moench: In the concentrates, we got high values in the Mt. Moosilauke area.

Cox: The most interesting histograms are those for the Conway Granite. I was trying to determine if the Conway had higher values of tin than other rock types in the area. The data are limited. The Paleozoic rocks exhibited a wide scatter. The plutonic Paleozoics had less than 10 ppm. That included the pegmatites.

Ludington: There is more written about the granites than about the deposits.

Cox: Mostly because of the very small volume of pegmatites, they are underneath the volcanics and the volume of the volcanics is small.

Ludington: The point about the pegmatites is even if you knew where they were and they were all of economic grade, the tonnage is small. At this point, I don't see a way to distinguish between 1 and 3 as far as any difference, but I would draw them. I suggest you come in half a kilometer roughly from the edge of the anomaly, than move the boundary out to include all the big triangles there that look like they belong to that system. You need some sort of arbitrary cutoff on the east. That is about 10 miles out. Include something about like that.

Moench: As a moderate potential?

Ludington: Yes, with the present data, it is not possible to assign relative favorabilities.

McCammon: The decision whether it is speculative or possible.

Moench: We just have to speculate.

McCammon: You are not prepared then to go beyond the speculative.

Ludington: How are you handling the question of economic viability?

McCammon: We are setting a lower size limit in what we are defining as an occurrence. A deposit is defined by minimum size, grade, and depth.

Ludington: Do you want to define that now?

McCammon: What would be considered a deposit in an area like this?

Ludington: I think we need to do this before we draw lines.

McCammon: Yes, I do not know who has this information at hand. Because it applies to deposits elsewhere in the world, obviously you do not know what the minimum size deposit is going to be here, but for some minimum size, you would estimate some with the likelihood that at least one deposit of that size and grade occurs.

Ludington: We know about the deposits that are smaller and leaner. However, they have been drilled. Tonnage-grade distributions are misleading. They represent the economic deposits.

McCammon: We are trying to estimate the likelihood of occurrence of one or more deposits of certain minimum size of a particular type within this area. We do not care how many, but that there at least one or more. That is what it boils down too.

Ludington: So the question for tin is, how small do they have to be? From Leslie's description, there is a known griesen deposit of at least 50,000 tons because what the outcrop carries is as big as this room or more.

Cox: Smaller.

Ludington: 10,000 tons.

McCammon: The question is, if we set that limit, it is an occurrence.

Moench: That is in the middle of the batholith.

Ludington: Yes, but there is evidence for an underlying pluton.

Cox: Yes, there is evidence bigger than the area of this room. But the actual, obviously altered rock is 5 to 10 ft. wide across the seam.

Ludington: Maybe it should be on the map. In a general way the background is less favorable.

Moench: The exposed plutons are too deeply eroded.

Ludington: Except for plutons not exposed.

Moench: You could use two patterns, one pattern over the pluton and one pattern in that more favorable zone on the outside border. Point out that the pattern over the pluton depends on the possibility of a younger body below. In general, it would be less favorable. But still more favorable than far removed.

Moench: It would be interesting to know how far down-glacier or down stream this tin anomaly goes.

Canney: It goes down to the Maine coast.

Moench: We may have to come up with two types of assessment, one for tin in bed rock, one for tin in placers.

Ludington: We talked about this a few years ago looking at coastal and offshore placers in the southeast Appalachians. How much do you have to take to sample something like that, a few cubic yards? Is that what is necessary?

Moench: We can, in general terms, assign an area of potential.

Canney: Take the area of the stream sediments, use the higher values in some of the stream sediments, draw a line about them as possible or speculative.

Moench: We have deposits and evidence of mineralization. We draw that as a broad area, possible occurrence of 3 and a lower possibility of 1. At depth, not exposed currently.

Ludington: I am not sure, lower, that is the one we have the sample of. We have samples of 3 also.

McCammon: Well really, you have 1, and 2, and 3. Right?

Moench: Yes, but 2 does not amount to much.

Canney: Only trouble with 2 is it probably helps to influence some of the stream sediments.

Ludington: But if you can write a few sentences about that and not have to draw a diagram. My feeling on the 1, 3 thing is at this point, putting 1 of them above the other is only liable to be misleading.

Canney: Yes.

McCammon: Keep them as possible, try not to make a final distinction.

Moench: Then it is up to whoever does more work to identify plutons at depths if that is possible. We could have a map, then, with a broad area of possible potential, the evidence of mineralization both within and around the known mines and prospects related to the suite, and a description of kinds of deposits that might occur.

1:00 - 4:00 p m. Discussion by Bob Ayuso, Frank Canney, and Gene Boudette on geologic settings favorable for porphyry copper and vein uranium deposits in Sherbrooke-Lewiston area and a discussion on areas favorable for uraniferous peat accumulation

Moench: Let us move on to Attean types of porphyry, moly copper resources associated with the Ordovician-to-Devonian plutons.

Canney: What about the uranium in the Conway granite?

Moench: They are something else again. We do have to consider that because the tin anomaly is defined also by uranium as well as niobium, lead, zinc, and beryllium. There is the possibility of Sunapee-like associations, with secondary uranium along surfical fractures or near-surface fractures. One of the most interesting is Cornelia Cameron's peat, overlying essentially two-mica granites.

Boudette: The Flodell Creek deposit in Stevens County, Washington furnishes a model applicable to the east. The deposit is in a very narrow, deep probably pre-Pleistocene valley cut into grus overlying two-mica granite. The rocks of the White Mountain intrusive sequence may also provide suitable environments. If we ever get around to mine these deposits, they have the potential of major sources of uranium. There is 17 feet of peat in the Flodelle bog, some of it runs nearly 1 percent uranium. The average ore is between .13 and .2 percent and it is not very radioactive because the uranium is "young"--out of equilibrium (Joy Mining Company, 1983).

Moench: Well, maybe that kind of resource, perhaps something like uranium in veins, we have an anomaly here and there is also the Sebago and Moosilauke magnetic batholiths. The uranium is showing up in the sediment samples. We have to consider separately the potentials having to do with uranium in fracture systems as secondary uranium, and uranium in peat.

Boudette: We may want to rate the bogs on the alkalic White Mountain rocks different from the two-mica granite. I will tell you why. Uranium in two-mica granites is almost 100 percent labile (leachable). Rocks of the White Mountain sequence tend to keep their uranium tied up in refractory minerals, and is much less leachable. The biggest uranium deposit in the world (Ilimaussaq) in Greenland cannot be mined because of the extractive metallurgy problems involved in getting the uranium out of the refractory minerals. It is obvious, however, that there is some uranium moving because in rottenstone pits in the Conway Granite, there is autunite. It definitely moves around in grus. Grus may be a big factor in the process of leaching a granite of its uranium. Grus provides an attractive alternative to leaching.

Ludington: Most of it is probably in the zircon.

Boudette: In the White Mountain sequence rock definitely.

Moench: What about the other elements that constitute an anomaly down here like niobium, beryllium, lead, zinc, rare earths, and silver.

Ludington: Niobium and the rare earths require high grades. What are the highest rock contents? 1,000, to 1,500 ppm?

Canney: We do not know.

Ludington: How about yttrium?

Cox: Yttrium is 1,070, 15 to 200, 9 to 200.

Moench: On to the older plutons. A range of possibilites exists with respect to the Attean pluton, in particular, Catheart and Sally Mountain. The question is what do we know about the association up there, and does it apply elsewhere down here along the line. The Attean is a hornblende granite, but porphyry with no hornblende contains the deposit. Very close to rhyolite in texture and composition.

Ayuso: The samples I have from the so-called unmineralized part of the quartz monzonite are 60 to 67 percent S_iO_2, and they range mostly from granite and granodiorite to quartz monzonite but, if you get up around Attean where we mapped it, it is clearly a quartz monzonite.

Moench: The compositional range of the Ordovician plutons north of the domes is hornblende granite of the Attean, biotite granite to granodiorite with a little muscovite of the so-called Adamstown pluton (Ordovician), and the East Inlet (Silurian). Others are granodiorite to tonalite or diorite; some quartz monzodiorite.

Ayuso: What about the plutons to the northwest of Attean?

Boudette: It would be like northern Attean rocks; hornblende granite to granodiorite.

Moench: When you get down to the northern New Hampshire area, there is a pluton of possibly two-mica granite there, and quartz-porphyritic granodiorite to trondhjemite around the Milan area of New Hamphshire. A belt of biotite granite, locally porphyritic, typically containing muscovite. We do not know whether the muscovite is primary, secondary, or metamorphosed. A light-colored two-mica granite in here, a coarse grain pink granite there. We have syenites that were dated at 441 Ma by Foland and Loiselle. This is syenite and quartz syenite, and have more hornblende-bearing granites; and then biotite granite to diorite in the Loit Nation pluton, based on available descriptions. Quite a variety.

Ayuso: Were those plutons classified according to the classification scheme you mentioned before? When you mention muscovite, do you put it also in the name?

Moench: If it is mostly biotite, I was conservative about calling a rock a two-mica granite.

Ayuso: The reason I ask is because in light of other things that were mentioned as two-mica granites in the Northeast Kingdom, the muscovite is not primary for the most part but it occurs in old shear zones and fractures. It has nothing to do with the primary mineralogy.

Moench: I would now be inclined to call most and possibly all the muscovite-bearing rocks as biotite granite, etc. with possibly secondary muscovite.

Boudette: Probably it should be pointed out here that all granite with two-micas are not necessarily two-mica granite! Some two-mica granites have only one-mica. We should call them French granites maybe. When one uses the term two-mica granite, there is a semantic problem.

Ludington: The problem is the implications of all this classification are chemical, the classification must be chemical and much of the time we do not have that kind of data.

Moench: You do not see a lot of Ordivician mafic rock, excepting small bodies like the Chickwolnepy intrusions and lots of other mafic dikes, which may or may not be Ordovician.

Boudette: Probably it should be pointed out at this time also, Bob, that both the Attean and particularly Chain Lakes are intruded by Cathecart-type quartz porphyry; some of which contains chalcopyrite and other sulfides as well. They comprise an interesting problem in terms of exploration.

Ludington: Is that restricted to the area?

Boudette: Yes. Some are within it, but the Chain Lakes to the southwest also is intruded. Some occurrences are actually breccia pipes. Some massive porphyry occurs in the Skinner quadrangle. Their frequency increases toward the Attean pluton. A mineralized porphyry breccia occurs near Jim Pond, a good distance away from the Attean pluton, however.

Ludington: Is it well established that those rocks fall close on the main Pluton?

Boudette: Yes. You can walk right into it--it is intruded by these porphyries. Is that what you mean?

Ludington: Yes.

Moench: On the subject of Devonian plutons, in various shades of red, are lots of real two-mica granite scattered about in bodies of all sizes. Several bodies are essentially granodiorites unfoliated, with fairly euhedral muscovites and biotites. Other compositions are quartz monzonite, quartz diorite, quartz monzodiorite, (Umbagog pluton, for example). There are extensive bodies of darkish hornblende biotite, granodiorites to quartz diorite. In the southwest is Kinsman, the so called Kinsman Quartz Monzonite. That's its formal name, but it is a Streckeisen granite, typically with K-feldspar megacrysts. The Bethlehem gneiss is mainly biotite granodiorite with a little muscovite and occasional megacrysts of K-spar. Up in the Northeast Kingdom, descriptions are lacking; these seem to be biotite granite with some muscovite, some porphyritic, grading into more mafic rocks, monzonites, diorites, in Echo Pond complex.

Ayuso: Most of those plutons that are called granites are really very plagioclase-rich. The Willoughby is the only one that has, nice looking muscovite. Even that pluton has an area that is much more plagioclase-rich that you would have expected to be associated with that rock. There might be about 12 percent k-spar and 35 percent plagioclase. Nulhegan pluton has very little. It is a plagiclase-quartz rock, containing amphibole-biotite and a little k-spar (5 to 6 percent). Right now, our modes show that it is a quartz diorite.

Moench: There is one noteworthy body in the Newark pluton, I think, that is what it is called on the state map of Vermont. It has a very distinctive magnetic anomaly associated with it, stands out relative to all the others in here from the available descriptions. Magnetite-bearing biotite granite.

Ayuso: We have not looked at that one but we looked at the Maidstone pluton which is the next one over. In the Maidstone, the contact with country rock is full of inclusions, it is completely hydridized and it seems like the pluton is a soup. It has rafts of country rock the size of this room and it is still being called a granite according to the available maps. We have a problem with the way the contact was drawn for that particular pluton. The rock was so hybridized and contaminated we didn't even sample it. Nulhegan looks excellent. It has a rim of hornfels around it, forming a perfect circle. The country rock-granite contact we have seen looks sharp. The contact at Willough has been checked, and what we have seen looks good. We only looked at parts of the Averill Pluton. The problem we have right now would be with the Maidestone contacts.

Moench: Could we get together later and redraw contacts?

Ayuso: Yes.

Bothner: Which one is the Maidstone?

Moench: We have it tentatively as a two-mica granite. Based on the mode described in the report, it is a questionable two-mica granite.

Ayuso: I do not know what we would call it right now because even at the center of the pluton, it resembled a migmatitic zone, streaked out.

Moench: There is more gabbro associated with the Devonian: Pierce Pond, Sugarloaf, Flagstaff Lake, and many others; also the layered gabbro-ultramafic Plumbago pluton. Then in the Carboniferous, we have the Sebago batholith, yielding ages at about 325 Ma; it is all two-mica granite, quite homogeneous.

Boudette: Within which there has been two uranium prospects drilled.

Moench: The resources overall range from the kind of thing that is associated with the Attean, copper-moly porphyry type, and perhaps young uranium associated with two-mica granites as well as with the Conway granite of the Jurassic system.

Boudette: Extensive garnet deposits occur north of Rangeley in the Flagstaff Lake pluton.

Moench: Yes - extensive metamorphic garnet deposits near Rangeley; and maybe some asbestos associated with the Boil Mountain.

Probably goephysics has an importance here, having to do with shapes of plutons. Wally's modelling indicates that a lot of the plutons, both the Sebago and also the plutons of western Maine that occur in the high-grade rocks are flat lying sheets, some horizontal. This would apply also to the Sugarloaf gabbro, very thin though big body in surface area. Steeper walled bodies appear as you go to the north, into the lower grade, upper crustal levels. Most of the southwestern lakes were emplaced probably at considerable depths, whereas lesser depths prevailed south, northeast. I rather doubt that same rule applies to the Ordovician, which may be generally upper-crustal.

Bothner: We have only done one 2-dimensional model on Attean. We are surprised by depths on the order of 5 to 6 kilometers. There is a problem in extracting the local gravity field from the regional because you still have a tremendous effect from things like Flagstaff and Moxie so it may be less than that. Adamstown looks to be thin, a couple of kilometers. Moosilauke is about 2 1/2 kilometers thick. We have not modelled the Highlandcroft in the Littleton area. It is very small. There is no expression we can isolate. But we do not have closely spaced gravity stations there. The ones at Phillips and Redington are a little thicker, but they are getting close to the sillimanite margin about 3 kilometers or so vertically. Lexington goes down to perhaps 4 km just as Bob pointed out. Sebago is thin as a pancake, about a kilometer southeast of the map. As I pointed out earlier, we cannot distinguish margins between that body and the White Mountain batholith to the west, the density contrast is too small. Northeast Kingdom, we have only done Averill and got a model of that of about 4 kilometers for it. Nulhegan is in the mill and the gravity anomaly on that seems to be offset to the southwest. It should give us a value not unlike that of the Averill.

Ayuso: We made a couple of traverses and we saw distinctions in the Averill. I though it was so complicated, that we abandoned it last year because we had no time to go after it; we just took a couple of traverses from the Canadian border to the south and saw at least 3 different types of rocks there.

Bothner: Density variations would be very small?

Ayuso: I think you would have seen them, porphyritic phases and some other petrographic types that are really different showing very drastic changes in grain size and texture.

Bothner: That is the same problem we had trying to distinguish Conway from Osceola.

McCammon: What do you put below the body when you model?

Bothner: A lack of density contrast.

McCammon: Wouldn't that make a difference as to how close you could fit the anomalies?

Bothner: Modeling is based entirely on lateral density variation not vertical, so we are able to calculate a depth below which no density contrasts exist. You can put Gile Mountain rocks, or you can put Littleton schists, or something else and a dashed line that says this is where we think the bottom is, but any kind of geophysical modeling is ultimately ambiguous. Proof consists in drilling and so far the only hole I know of is the one Ken Stewart drilled into the Conway granite that only went down a kilometer.

Ludington: There is a kilometer deep hole in the Conway?

Bothner: Yes. In fact, there are 4 holes.

Ludington: Are they described?

Bothner: Yes.

Ludington: All in granite?

Bothner: No. It bottoms in porphyritic quartz syenite, passes through two phases of granite, the normal Conway which is biotite rich, then a green phase which may or not be Osceola through some diabase dikes and bottoms in the Albany.

Boudette: There is some country rocks up there too, aren't there?

Bothner: That is equivocal.

Ludington: These lateral units you mention are Paleozoic?

Bothner: They are all Jurassic.

Moench: One of the interesting things that Leslie found in her mapping of the south side of the Gore Mountain plutons, and that I found in the west side of the Connor Mountain stock is that some bodies of Conway Granite have flat bottoms. It looks as though these were subhorizontal sheets too.

Boudette: If you want to use an analog in your model, there is a circle west of Manchester, New Hampshire, that you can see in several remote sensing images which is just the size of the Ossipee ring dike complex. The Manchester circle could be a cryptic White Mountain caldera substructure.

Bothner: It is bigger.

Boudette: Only slightly bigger?

Bothner: It is about 3 times bigger. You can draw a circle from Ossipee to Red Hill and to Belnap that's one big circle. Then the one down around Manchester I think it's about the same size.

Boudette: It is the same size as Valles caldera in New Mexico. Carbonatite occurs on the border of the Manchester circle. Also, John Aleinikoff dated Albany quartz syenite at 170 Ma in two localities. The Manchester circle appears to be the bottom of a White Mountain sequence alkalic center. Whatever was above is gone in erosion.

Bothner: The average depth of the White Mountain batholith is about 4 kilometers except for one zone in the center in which we have got equivocal data that suggests maybe 7 but they may be feeders or something that came up.

Ludington: So the hypothsis would be overall those bodies are quartz syenite and the granite is the differentiate, in a smaller magma chamber which is emplaced more or less towards the top of the system. The chambers gets sparser and sparser as you go down until you get out of it, which must be very different conceptually from what the quartz diorite is like. Even if they are at shallow levels, they are up in the north. These are open space fillings and pressure things.

Ayuso: I have seen some mirolitic cavities in the Willoughby and parts of the Willoughby are fractured and do have some staining. The plutons were intruded probably very shallowly in Vermont.

Moench: In terms of resources, one of the most important things we have to consider is the possible occurrence of the Attean association elsewhere among Silurian and Ordivician plutons, and also the possibility of molybdenum elsewhere in the 2° area.

Ludington: Do you want to hear a formal presentation on Climax type molybdenum? I think there is only a ghost of a chance that you have one. They are 5 kilometers higher than your levels of exposure. They would be Mississippian age and you only have 1 pluton, right? And it is S-type.

Moench: The question in my mind is, what are we really looking at up here in terms of the copper moly porphry?

Canney: There is a moly prospect here in the Lexington batholith but no one knows what it is about, mainly based on surface geochemistry.

Ludington: I know about molybdenite there, the pluton near Tunk Lake (southeast Maine).

Canney: There are a couple of others over there that are pretty dry looking and little sparsely disseminated molybdenite in them.

Ludington: Those systems may well have been moly porphyries 5 kilometers higher. I do not know because we do not know what the real ones look like 5 kilometers lower.

Canney: Catheart of course is copper, about 22 million tons containing about .20 or .25 percent copper, .07 percent moly.

Ludington: Sounds like small British Colombia porphyry

Canney: Sally Mountain, West Sally has some disseminated moly.

McCammon: Frank, do you think it is just one of a kind sitting there?

Canney: I would say chances are there are more.

Ludington: Robert, you are suggesting the Northeast Kingdom plutons are at high levels. The whole north western part of the terranes permissive.

Canney: Yes, definitely permissive. The other pluton in Adamstown has some interesting copper-moly values associated. It has a geochemical anomaly. Down south, what is the pluton in Keene, New Hampshire, that was definitely explored?

Nowlan: Fitzwilliams.

Canney: It has copper moly.

Ayuso: I have seen the core.

Boudette: Then there is Corbins Park; the so called lead mine. The lead mine is a fissure vein, filled with some chalcopyrite, pyrite, and some traces of molybdenite.

Ayuso: The Northeast Kingdom granites are high, pretty high up. The difference is not only of different age. They are going to be about 380's versus Catheart which is about 40 million years older. Also the Northeast Kingdom granites are dry, really dry. You go there, and you might find few aplites, few pegmatites, and a few places where you might find miarolitic cavities. It is not the top of the chamber where you might see lots of quartz veins or aplites.

Moench: For a Climax type, you are really looking at a high level.

Ludington: But emplaced 2 to 4 kilometers from the surface. It can even be much shallower like at Mt. Pleasant. That body is less than a kilometer.

Moench: What is the role of the shape that a body assumes at a high level?

Ludington: Pine Grove is sitting there probably erupted its ejecta 4,000 feet above the ore. The same is true at Mt. Hope but there is stratigraphic control.

Bothner: So it is going to be something like Katahdin where you have the volcanics capping the granitic rocks.

Boudette: There is moly there.

Canney: Is that the one down near Tunk Pond?

Ayuso: Was it in a quartz vein also?

Boudette: What I saw was.

Moench: So the reason for the requirement for shallow emplacement has to do with the capacity to develop a hydrothermal ciculation system? The permability of the brittle fracturing in the wall rocks?

Ludington: It has to do with water, say votatile contents, how high the pluton get in the crust. I think the volatile content is significantly not just water so they can actually get higher, evolve large amounts of fluids. The magmas that make these things have no reason to stop. The ones that stop lower are not at a fault.

Ayuso: And also, down below you do not set up a convection system which is necessary in order in order to leach out and concentrate the metals.

Ludington: It is evident that the Climax type found in one event, but that the ore deposit fluid is not. They have been modified.

Ayuso: So the fluid was concentrated and enriched in the metals, fractured escaped, relaxed and the whole system might have been the result of several generations.

Ludington: The new work at Henderson suggests that the orebody is a result of six different ore-bearing intrusions most of which are only a few hundred meters in diameter and they are all overlapping but they can map veins belonging to each one of those now. Each one set of veins is above, never peripheral to each system. Right below here, one would come from unaltered rock into the source pluton and go through a zone as wide as this room where the entire alteration sequence is telescoped. There is no evidence of lateral movement.

Ayuso: For Bob's purpose, what we need to tell him is that he needs to find plutons that are very shallow, especially the ones that would have been emptied into their volcanic ejecta.

Moench: The closest we can come to that would be along the domes if one would assume some of the dome rocks were subvolcanic to the Ammonoosuc volcanic.

Ludington: If you think you have a candidate, then we can talk about the chemistry. It has to be more than 75 percent S_iO_2, 72 or 73 percent S_iO_2 is not a candidate. If the MgO content is more than 4 or 5 tenths, it is not a candidate. Likewise, if calcium is more than 5 tenths.

Ayuso: Steve, you are telling them about a Questa, whereas maybe granodiorite-molybdenite types may even have a better chance of appearing here.

Ludington: Yes, I'm talking about two distinct things. Those systems, the Theodore systems, are in my mind, there is nothing to distinguish them from porphry coppers other than the copper/molybdenum ratio.

Moench: For that you are still looking at very high levels.

Ludington: Well, not as high, I don't think you need to be as high. You've proven that because you do have Catheart. It could just as easily be .2 moly and .07 copper in the lithotectonics. That is a very distinct deposit type.

Moench: Everything since the Middle Ordovician on?

Ludington: All I hear is 64 percent, 68 percent silica cuts, that is the part missing, I mean less than 54 greater than 65.

Moench: Well, there is a big saddle right about 55, we have the compositions there but it is a bimodal with a lot towards the middle. Peaks off on the end more granite than anything else at 70 or 75 percent silica.

Ludington: That has been quoted as one of the differences for New England as a whole. The whole mode has shifted to a more silicic composition.

Ayuso: Bob wants to have a couple of 2, 3, or 4 different things that he can use to quickly catalog his granites and if you just tell him to restrict himself to high silica values, then he might overlook something that looks good with lower silica.

Ludington: You have to draw that distinction. If you want to call it granite or granodiorite. As I said, the criteria for these is exactly the same as porphry copper in the classic Lowell and Guilbert sense.

Ayuso: If you have high florine, you would be in good shape. For example, Thompson Creek has 600 ppm florine and higher.

Ludington: That is low.

Ayuso: Right, Climax has 2,400 ppm and higher.

Ludington: We have yet to try that kind of distinction for Climax type moly systems because, to my knowledge, there is not enough data on unmineralized high-silica granites in similar terranes to make a comparison. To rule these out, you have the level of exposure, you could say something about volcanics, the two are tied together.

McCammon: How close is Catheart to Mt. Tolman?

Ludington: In my mind, they belong in the same class. There are hundreds of deposits that are similar (22 million tons of .2 copper) in Arizona.

McCammon: How about here? Is there more than one?

Ludington: You want to hear what like Catheart Mountain is there on eastern quads?

McCammon: Right!

Ludington: It cannot be in here because this is all deeper, is that correct?

Moench: It cannot be in the Devonian, could be in the Ordovician because they have been uplifted.

Ludington: So that means, it is more likely up here?

McCammon: Too dry?

Ayuso: That is why I have been asking, Gene has done some work on those two plutons, the one up there just to the west. To me those look good. The Ordovician-Silurian plutons to the west and southwest of Catheart Mountain to me are the ones I would want to see data on. Especially since Gene was saying that there is a lot of pyrite in some samples.

Ludington: Chemically, porphry copper source rocks are between 55 and 73 percent S_iO_2. They are associated with active margins. That holds for all these rocks.

Ayuso: You did pick up an anomaly there?

Moench: Yes, there are geochemical anomalies associated with that, moly and copper. Adamstown pluton here. The Skinner here.

Ayuso: Are they cut up by quartz veins, Bob?

Moench: I do not know about this one Gene told us about.

Boudette: Skinner? I have never seen one there. I will say though that this quad is only known in reconnaissance.

Moench: As far as I know this one is not (Adamstown). It is a deformed granite, metamorphosed, but is a fairly homogenous coarsely porphyritic granite.

Ayuso: Those three plutons would be the ones that I would concentrate the search.

Ludington: I like that of placement.

Ayuso: You have the right age, the right setting, and if you have the anomaly, it would be the one.

Ayuso: There are the ones in the Northeast Kingdom. Those do not look like they like they have anything going. I am not familar with the rest of the Acadian.

Moench: These are two-mica granite, lots of pegmatites. Brick red colors on the map are hornblende biotite granites. The northern part of the Phillips is biotite granodiorite with muscovite, intruded by two-mica granite.

Boudette: Do you call that dry, Bob?

Moench: Not the northern part of it.

Boudette: Lexington, I would not call dry at all.

Moench: No, Lexington is wet.

Ludington: If you have a well developed copper system within a few hundred meters of the surface, you will see a good geochemical anomaly.

Canney: I was just going to say there is no question if we get one exposed, there would be a huge anomaly.

Ludington: Yes, if it is exposed, you would be telling us where it is. Thus, it seems that there are none exposed.

Canney: We have some of these soil and stream sediments, a thousand ppm copper and many hundreds of molybdenum.

Moench: What it reduces to then, is what the geochemists say about that pluton. The next step would be to take the plutonic map down and put the geochemistry on top.

Ayuso: Do you have rock geochemistry?

Moench: No, we do not have rock geochemistry on that body. Geochemistry is scattered all around. I have my own files that account for the plutons in here. Gene has data in his area. From there, you have to go to the literature and get some analyses but again not much in the way of minor elements.

Ludington: The experience of the (Newfoundland) people is that the Acadian plutons are relatively dead. 380's to 400 the Devonian Plutons. For them, the most promising targets for exploration are the Mississipian granites. Second, are even the older ones. Yes, the 450.

Boudette: Of course, there is moly in the Lebanon dome.

McCammon: Sounds like the highest you are going to go Bob, is possible here. You have the geochemistry and perhaps the right age, but you do not have shallow plutons or the volcanic material.

Boudette: A case where geochemistry works well.

Moench: We know there is something worth looking at and talking about.

Ayuso: I think the Attean is 445, that seems to be cast in concrete. I am talking about the porphyry that is unmineralized at Catheart, that is about 426 m.y.

Ludington: Well, it is clear those plutons have a signature.

McCammon: Have they drilled in this area, Gene?

Boudette: There was some drilling in Sally Mountain done by Andes exploration.

Canney: No, not to my knowledge.

Ludington: Other than the copper, the lead and zinc the whole pluton runs about the same, doesn't it?

Boudette: The thing to remember is that the Attean is shot full of quartz porphry veins (dikes) which may carry values.

Ayuso: What does the geophysics look like in here between this and that.

Bothner: Very little over Skinner, a little tiny deflection. I would guess based on the pattern the contact goes off in that direction because these are real, I do not think one would see anything nearer. Host granite contact. Some test holes were drilled.

McCammon: Certainly makes for a bigger target. You might want to use contours, Bob, to draw in the lines rather than what the map shows.

Moench: Contour lines?

McCammon: Thinking of this as more than what you see here with the What Wally is saying is it is bigger than that looking at it in third dimension in terms of what might be prospective.

Bothner: You said something about estimating how far down you would have to go.

McCammon: Yes

Bothner: We could make some generalizing at the Attean 10 kilometers or 5, we would have to drill 2 kilometers.

Moench: You could draw the boundary outside the pluton, just barely outside. You would get a dip on it essentially from your overall mags and project to maybe a couple hundred meters.

Bothner My guess is, if anyone is going in and look at it and look at the rocks and say here is a good place to drill and not base everything on what we draw.

Canney: Actually there has been exploration in the area of the Attean.

Moench: You can bet that a good many people have done their own geochemistry. The plutonic geology of the dome is really quite complex. This is the best we could put together from the information we had.

(Ed. Note: Discussion follows about stratabound cobalt deposits in metamorphosed rocks.)

Boudette: Could be rusty rocks here and there.

Canney: Did you talk to Mike Foose about this? Any chance it could be a Blackbird type deposit? Stratabound in graywackes.

Boudette: If you want to trace these rocks down to the next quadrangle south, there appears to be some sediment hosted copper here and there such as that in Gilmanton, New Hampshire.

Moench: I am not sure we can draw lines or areas of the map so potential for stratabounds copper but it should to be mentioned. We only have the data on the regional geochemistry. We have to go to larger scale maps to show alteration patterns, type of volcanics, and so forth.

Canney: Foose and Slack have seen the Blackbird in Idaho in the last year or two. It is disseminated cobalt in otherwise unmineralized-looking rocks.

Moench: This area tends to show up geochemically in other ways too. It was copper that showed up.

McCammon: Do we have enough to say speculative for this type of deposit, from the geochemistry?

Ayuso: How does the cobalt occur? Is it a separate mineral?

Cannon: It is a separate mineral.

Moench: I think we have gone over the plutonic rocks as far as the porphry type of associations. We never did make any decisions on the sediments. We are concerned with the metasedimentary stratigraphy. What we have we have, first of all, a big belt of Ordovician-Cambrian and pre-Cambrian rocks here with along with the Bronson Hill-Boundary Mountain anticlinorium, volcanics of Ordovician age, and some Silurian and Devonian volcanics. A thin Silurian section along the anticlinorium; thin shelf-shore facies and probably rather shallow Devonian up here especially the Moose River synclinorium. But as you drop off on both sides, you get into very thick clastic marine sections, 5 kilometers or 7 or 8 kilometers. The Silurian here in brown on the map, generally with source directions over here to the northwest and the Devonian in blue with multiple sources available. I would suggest a major Early Devonian source to the southeast. The question is the resource potential of these schists of metamorphic grade as high sillimanite in the southwest zone to greenschist-facies and lower the north and northeast. These (Rangeley) are essentially graywackes or turbidites, proximal turbidites with rusty pelitic schists. There're showing some chemical patterns that were unexpected. One thing we have not looked at in the project is potential or possibility of stratabound sediment hosted deposits.

Foose: The volcanics up here are both mafic and felsic? Have you seen volcanics?

Moench: These (the Rangeley) are remarkably free of volcanics. I think a large part of the volume of sediment there came from volcanic belts on both sides, much changed in composition during chemical weathering, erosion, and transport.

Foose: I gathered from Robert, you wanted my general impressions concerning the occurrence of cobalt in terranes such as this one here.

Moench: We were just looking at geochemical patterns and they tend to show cobalt here. The association would be in the Silurian here, and also as you go down here, cobalt is showing up in patterns that seem to correlate with the Rangeley Formation southwest to the White Mountains. The Lower Devonian rocks are essentially mud-silt turbitides, and the Silurian rocks, particularly the Rangeley, are more proximal turbidites, sandy, conglomerate, quartzose polymictric conglomerate. In this section, a thick subaqueous fan deposit.

Foose: Do you know enough about the Belt Series in Idaho to compare it to this?

Canney: No, I do not.

Foose: Cobalt deposits are generally found associated directly with mafic or ultramafic rocks, an association that would not speak well of this terrane. An exception, however, is the Blackbird district in Idaho. There Belt-series rock, in particular the Yellow Jacket Formation, makes a thick undescriptive appearing sand sequence that is presumably a fairly deep water deposit. This rapidly deposited, fairly deep water, turbiditic sandy sequence contains stratabound cobalt occurrences. The cobalt extends over 30 miles within the same stratigraphic package along strike. The mineralization is mostly in hydrothermal veins. Initially, it was thought that hydrothermal vein deposits were associated with the Idaho batholith.

The mineralization occurs in folded veins and is primarily copper sulfides and cobalt arsenides. I do not know and I do not believe anybody knows exactly what the paragenesis is. I suspect that the cobalt mineralization started out as stratabound cobalt-copper sulfide. Thus, copper should also be associated with cobalt, and the geochemistry should show a copper-cobalt association if this type of mineralization is present. Subsequently the stratabound copper-colbalt mineralization occurred either during metamorphism or during emplacement of nearby granitic intrusions. The copper and cobalt was reworked and reconcentrated as part of a hydrothermal arsenide vein systems.

Where the copper and cobalt metals came from is an enigma. These sediments generally do not have a lot of cobalt in them. Occasionally, there are small amounts of amphibolities within the Yellow Jacket sequence. The grade of the Yellow Jacket at the Blackbird mine site ranges up to sillimanite grade and these amphibolite areas are interpreted to be mafic tuffs. They are, however, a small fraction of the sedimentary pile and it is difficult to conceive that they provided all the cobalt now concentrated in the deposit. Probably, the mineralization is associated with deep seated sedimentary faults. If the current models are to be believed, basinal fluids moving up through the clastic sequence leached out cobalt from either these mafic tuffs or from an unknown source, and concentrated the fluids along the faults and spread the metals out along the stratigraphic units. In the Blackbird case, they were hydrothermal remobilized. That is a synopsis of my thinking about the Blackbird.

Moench: That is something to consider, we may want to have a speculative category for people to look into and think about.

Cannon: You do have an anomaly in these rocks? About the only analogue we know of that is cobalt.

McCammon: Bob, is there time to follow up on what Mike was suggesting, looking at those values a little more closely. Figure out where the highs are.

Moench: Not really. Perhaps Nowlan and Canney can take a closer look.

McCammon: How about the ultramafics?

Boudette: There are diorites in the bottom of Chain Lakes massif but not ultramafics. There is a lot of detrital gold in the Chain Lakes. There is lots of sulfide veins and quartz sulfide veins in Chain Lakes. Gold is reported to occur in pyrite cubes. The ophiolite is on top of the Chain Lakes and much younger. Will the geophysics allow for relatively dense (mafic) rock under the Chain Lakes?

Bothner: I do not think so, do not have any place to go. There are parallels to be drawn with Sudbury. But we would have gone through the hanging wall into the foot wall if our reconstruction is correct.

Moench: Did some of those gold values that were detected, did they come from Chain Lakes?

Boudette: Yes, there were a couple up there. Many people still pan.

Moench: I think we have run through the whole resource analysis, haven't we?

McCammon: Looks to me like the end.

Boudette: What should we do about uranium?

McCammon: You mean in peat?

Boudette: We also have the Conway, that is compared to the Nigerian granites. It is the most uraniferous granite in the world. In round numbers it averages 100 ppm thorium and 25 ppm uranium. Most Thorium and Uranium is in refractories but there is some secondary movement as we discussed earlier. Two-mica granites is where uranium interest centers. Holes were actually drilled in the Sebago batholith, I never recovered any information about these, however.

Moench: No question about the association there, the question is what kind of concentration; where are we likely to run across rock that's badly contaminated with uranium? Probably two major possibilities; one would be the association, fractures, joints, weathered ground, concentration of oxidized zones among the joints. If one were to find deposits there, they would likely to along that trend. Likewise the Sebago. We may want to arrive at a speculative category for secondary uranium and joints along trends associated with Conway granite, in and around but particularly in the principal bodies of two-mica granite.

Ayuso: Is there that type of mineralization in any other pluton? The one you see in Sunapee, perhaps in these rocks also? Uranium oxide in the joints?

Boudette: Sebago, but not at Mooslook Megantic.

Moench: The geochemical data suggests that.

Boudette: One could generate a French-type of deposit in two-mica granite but you would have to see zones of potash enrichment. You would have to find syenite columns, which have never been seen. I think we would have seen them if they were there. There is no geochemical signature known yet. If you want to compare them with, say Margnac, there is Sunapee, but where does that uranium come from? It apparently has a lot to do with hydrology. Subsurface damns provided by Trassic lamprophyre dikes apparently caused precipitation of the uranium and diverted the ground water. Weathering of the dikes and subtle pH changes could be a factor.

Moench: For the purpose of this summary analysis we can restrict it to the two-mica granites.

Boudette: In the case of Sebago, I might want to go for probable.

McCammon: Probable?

Moench: I think you need occurrences to do that.

Boudette: Unfortunately, what I know about the core drilling by Exxon and Kerr-McGee won't help us. There apparently was never any follow up. Do you think possible is the best way to go?

McCammon: Yes, possible, provided you have some diagnostic criteria that are demonstrable.

Moench: Cornelia has data for occurrence near Sunapee which shows some concentration of uranium in peat, but no ore.

Boudette: Again, if we use Sunapee as an analog, there is not too much help, because values still don't compare to Flodelle Creek. Alpine-type bogs which may exist in White Mountains areas remain to be tested--they could be important resources.

Moench: From the data we now have, we will have to go with speculative.

(Ed. Note: This concluded the meetings. The final results of the mineral resource assessment of the Sherbrooke-Lewiston area will appear in future Survey publications.)

References cited

Cheve, S., 1978, Region du sud-est des cantons de l'est, Internal report DP-613, Ministere des Richesses Naturelles, Gouvernement du Quebec, Quebec, Canada, 80 p.

Emmons, W. H., 1910, Some ore deposits in Maine and the Milan mine, New Hampshire, U.S. Geol. Survey Bulletin 432, 62 p.

Eric, J. H., 1979, Preliminary geologic report on the Milan mine, Milan, Coos County, New Hampshire, U.S. Geol. Survey Open-File Rept. 79-353, 12 p.

Erickson, R. L., ed., 1981, Characteristics of mineral deposit occurrences: U.S. Geological Survey Administrative Report, 227 p.

Fehn, U., Cathles, L. M., and Holland, H. D., 1978, Hydrothermal convection and uranium deposits in abnormally radioactive plutons, Economic Geology, v. 73, p. 1556-1566.

Gair, J. E., and Slack, J. F., 1979, Lithostratigraphic structural setting of stratabound (massive) sulfide deposits, U.S. Appalachians, U.S. Geological Survey Open-File Report 79-1517, 4 sheets.

Jackson, S. A., and Beales, F. W., 1967, An aspect of sedimentary basin evolution: The Concentration of Mississippi Valley-type ores during late stages of diagenesis, Bull. Can. Soc. Petrol. Geology, v. 15, p. 383-433.

Joy Mining Company, 1983, Environmental impact statement for the proposed Joy Mining Company uranium mine and mill: Washington State Department of Social Health Services, 112 p.

McCammon, R. B., Boudette, E. S., Cameron, E. C., L. J., and Moench, R. H., 1984, An expert system for mineral resource assessment in the Sherbrooke and Lewiston 1° x 2° quadrangles, Maine, New Hampshire and Vermont, U.S. Geological Survey Open-File Report 84-751, 37 p.

Menzie, W. D., and Mosier, D. L., 1985, Grade, tonnage, and lithologic data for sediment-hosted submarine exhalative Zn-Pb and sandstone-hosted Pb-Zn deposits, U.S. Geological Survey Open-File Report 85-206, 16 p.

Moench, R. H., ed., 1984 Geologic map of the Sherbrooke-Lewiston area, Maine, New Hampshire, and Vermont: U.S. Geological Survey Open-File Report 84-0650, 1 map sheet.

Plimer, I. R., 1978, Proximal and distal stratabound ore deposits: Mineral Deposita, v. 13, p. 345-353.