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MENTE ET MALLEO ATQUE CATINO*

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INTRODUCTION

The title of this address has not been chosen with the purpose of creating the illusion of classical erudition. Two-thirds of it was found ready-made, as most of you will recognize, on the seal of the organization for concerted effort in geology, The International Geological Congress; the other third I have added in order to emphasize my conviction that the motto, expressive of an all too common attitude, suggests but inadequately the desirable activities of geologic science. It ignores experimental attack upon the problems of the science. To observe and to attempt to reach conclusions based on the observations is not enough; we must also, insofar as may be possible, devise experiments which will test the conclusions. It may indeed be regarded as somewhat surprising that the adopted motto should be thus unbalanced, for the first paper presented before the first session of the Congress, Paris 1878, was entitled "Etudes expérimentales sur les déformations et les cassures de l'écorce terrestre" and represented only one of the diverse investigations of the brilliant genius of experimental geology and petrology, Daubrée. Nor did he labor alone in these fields. However, a motto is not a compendium and it is unquestionably true and will probably ever remain true that geology as a whole is dominantly a science of observation and inference. In those branches of geology with which we of this Society are principally concerned, namely mineralogy and petrology, there is an increasing shift of emphasis. Experiments devised to throw light on the conditions of origin of those mineral aggregates which we know as rocks and mineral deposits, are growing in number and scope. The position may be maintained that, for these branches of geology, experiment has come to be quite as important as field and laboratory observation in evaluating conditions of genesis, and it is this that is unquestionably the fundamental problem of mineral science. Experiment is a necessary check upon inference from observation, whenever experiment is possible, and it is

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an activity that should be more generally pursued, wherefore I say *atque catino*.

If one based his judgment upon present-day emphasis he would unhesitatingly conclude that the fundamental problem of minerals is that of their fine structure. The success of the *x*-ray spectrograph in revealing crystal structure is astounding and the activities of its devotees are altogether admirable, but the *x*-ray spectrograph, and, I may add, the petrographic microscope, the chemical balance, and other tools of the mineral investigator, are to be regarded as highly developed hammers. They enable us to observe more closely, and from these more intimate observations more reliable conclusions concerning the origin of minerals may be reached, but they are not primarily a part of that system which would seek to check conclusions by appropriate experiments. It is possible to suppose that, with progress in our knowledge, we may ultimately be able to read in its atomic arrangement and the electronic configuration of its atoms all the conditions that attended the formation of a crystal and also its subsequent history. We would then have a hammer so effective that the crucible becomes superfluous. The attainment of this blissful state is devoutly to be wished and efforts in that direction should be encouraged in every way. In the field of intermetallic compounds, which are, however, fundamentally different from ionic compounds as represented by the most important of minerals, the silicates, it has proved possible to explain the general form of the phase-equilibrium diagrams of some alloys in terms of the number of electrons in the electron shells, and a certain amount of successful prediction may be expected. Recent attempts to correlate the order of crystallization with the crystal-lattice energy of silicates may be steps in the same direction. The reduction of all knowledge in any science, indeed in all science, to a few unifying formulae is a goal ever to be striven for, but the possibility that such formulae may some day be available does not warrant an attitude of watchful waiting in the hope that laborious experiment may be rendered superfluous. Such aspirations inevitably remind me of a condition which prevailed when I first joined the staff of the Geophysical Laboratory. There were then enthusiastic thermodynamicians who wagered that within five years such advance would occur in thermodynamic theory and in the measurement of fundamental thermodynamic quantities for silicates that it would no longer be necessary to determine silicate equilibrium relations experimentally; instead they would be determined by a few simple calculations. Five times five years have passed since those sanguine days yet it is still necessary to determine silicate equilibrium diagrams by means of experiment. Perhaps no more rapid

success will be attained in the prediction of mineral equilibria from atomic and electronic considerations, and it is a knowledge of equilibrium relations that is essential to the determination of the conditions of genesis of minerals, their fundamental problem.

COOPERATION

Thus we are led to view the two great activities of mineral science, apart from interpretation; on the one hand, field and laboratory observation—wielding of the hammer; on the other hand, studies of phase equilibrium—testing in the crucible. Their coordination is a vital necessity to progress in the science, and should be the aim of all earnest investigators. Specialization is nowadays so great that a student must emphasize the one or the other. An individual will naturally seek to enhance the prestige of his chosen branch. His plain course here is to do so through the excellence of his work and not, as is unfortunately done by a few workers in each branch, through the disparagement of activities in the other branch.

Coordination of effort ordinarily takes a form somewhat as follows. An observer has examined certain mineral groupings in field and laboratory, and from his observations upon end-products has reached conclusions as to processes. He then approaches the student of phase equilibria, the tester, and urges upon him the desirability of a study of the relations between the mineral phases involved, the observer thus setting the problem. The chosen investigator may find relations that tend to confirm the conclusions of the observer and then all is merry as a marriage bell. On the other hand he may find relations that throw some doubt upon the observer's deductions and that even suggest another process. Then arises, for each, the temptation to disparage the other's work. Divorce is imminent, but not inevitable, for with the more thoughtful this situation will be only a spur to further effort. Each will reexamine his results to see where they might be improved in quality or extended in scope and will act upon his findings. Thus may eventuate an agreement of conclusions or a closer approach in views. If on the contrary they reach an impasse, much gain will nevertheless result. Each will have a more lively appreciation of the factors involved in the problem. Each will vigilantly seek a more penetrating method of breaking the impasse.

STUDIES IN MINERAL DEPOSITS

As an admirable example of a formal cooperative effort involving these two activities, I may mention the "Secondary Enrichment Investigation," in which a group of geologists combined to study the natural

relations of copper ores by both field and laboratory methods, and a group of chemists combined to carry out coordinate studies of the chemical factors involved in the formation of these ores and especially in their secondary enrichment. It had been concluded by geologists that certain copper ores had suffered enrichment through the agency of acid descending waters formed under surface and near-surface conditions. In the joint investigation the group of geologists reexamined the evidence, greatly extended existing knowledge of the ores, and were able to make additional suggestions as to the probable character of the processes whereby secondary enrichment is accomplished. The group of chemists studied the relations of the sulphides to each other, established the variable chemical composition of chalcocite, the black sulphide which is frequently formed during secondary enrichment, and determined the effect of composition upon the inversion temperature from the orthorhombic to the higher-temperature, isometric form. This latter study especially revealed that chalcocite with more than 8 per cent excess sulphur above the formula Cu_2S , experienced no inversion but persisted in the isometric form upon cooling. A chalcocite with orthorhombic outward form and with less than 8 per cent excess sulphur must therefore have formed below the inversion temperature of 91° . Although chalcocite of isometric outward form has not been found in nature, some massive examples of the mineral show an octahedral etch pattern which, if the excess sulphur is less than 8 per cent, proves their formation above 91° . These indications of temperature of formation are of some assistance to the geologist in reaching a decision as to the hypogene or supergene origin of certain chalcocites.

Of the reactions involved in enrichment itself it was found that copper sulphate solutions change pyrite to covellite and chalcocite, pyrrhotite to chalcopyrite and probably bornite, chalcopyrite to covellite and chalcocite, bornite and covellite to chalcocite, and chalcocite under some conditions to metallic copper, in which reactions a definite stability series will be recognized which accords on the whole with observations on the natural occurrences and confirms the general conclusions reached from them.

The general field of deposition of ore and gangue minerals from solutions is wide open and much can be accomplished in the determination of genetic conditions by investigations of phase equilibrium.

STUDIES IN SEDIMENTARY ROCKS

An outstanding example of the application of laboratory methods to the solution of geological problems lies in the field of sedimentary pe-

tology. It is the classic study of Van't Hoff and his colleagues upon the deposits of oceanic salts. Experiment has also guided interpretation of the factors controlling mechanical deposition and has thrown light on the modification of these factors by chemical effects, as in the peptization of suspensions by electrolytes, though here there is room for much further work.

Such problems as that of the origin of authigenic feldspars in limestones can hardly be brought to final solution elsewhere than in the laboratory. In spite of much investigation, the question of the genesis of dolomite is still susceptible of alternative interpretations and further studies of phase equilibrium are needed, though this may be found to merge with the problems of the metamorphic rocks.

STUDIES IN METAMORPHIC ROCKS

Investigations designed to throw light on the conditions under which various metamorphic rocks acquired their distinctive characters have not been carried far. The mineral associations observed in certain types of hornfels are found to comply with the requirements of phase equilibrium as determined in the laboratory under conditions of direct crystallization from the melt and therefore under conditions of high temperature and low pressure. On the other hand, other hornfels types have contradictory assemblages, showing minerals that are mutually incompatible under the mentioned conditions. Whether this is ordinarily to be referred to failure of equilibrium, or is sometimes, at least, due to a shift of equilibrium under lower temperature and higher pressure conditions are questions that have not yet been solved by experiment. This failure is largely the result of the exasperating slowness of reactions in the solid state and the difficulty of identifying microscopically any new phases formed. With the powerful tool now available, *x*-ray powder photography, it should be possible to overcome some of these disabilities. Undoubtedly also the attainment of equilibrium, the growth of crystals and the formation of phases not developed in the dry melt can be accomplished by adding small quantities of fluxes, including volatile fluxes, and progress may be hoped for through the application of this method.

Experimental studies of the factors controlling crystal orientation are also desirable. The need is stressed by the much greater prevalence of crystal orientation than had formerly been realized, a condition revealed by the advent of petrofabric analysis. Steps are already being taken by a number of investigators to meet this need and from them we may hope to have a valuable check on the conclusions reached by the observer as to the factors controlling and the processes effecting orientation.

Thus may the crucible, sometimes in the guise of the autoclave or the hydraulic press, aid in the solution of problems raised by the observer of metamorphic rocks.

STUDIES IN IGNEOUS ROCKS

An example of the actual interplay of the two activities is afforded, in the realm of igneous geology, by studies of monomineralic and anchimonomineralic rocks. Here there is no formal cooperative effort of organized groups, but coordinate attack by the two distinct methods is none the less real. The observer finds in the field gabbroic rocks consisting mainly of pyroxene and plagioclase with a little olivine, and frequently associated with them, rocks in which one or another of these minerals is increased in amount, in some instances to such an extent that it is substantially the only mineral present. Since the gabbroic rocks are obviously the product of direct consolidation from a melt of their own composition, it was but natural that the observer should regard all the related rocks as having been formed in like manner and should speak freely of anorthositic or dunitic magmas, without realizing that they present a special problem. But the student of phase equilibrium, working with mixtures of related composition, finds that when his mixtures approach pure plagioclase, especially more calcic plagioclase, or when they approach magnesian olivine, the temperatures of melting became very high. Calcic plagioclases melt around $1,500^{\circ}$, magnesian olivine around $1,800^{\circ}$. He points this out to the observer and asks him what the evidence is as to the temperature of formation of such rocks as anorthosite or dunite. Contrary to the usual case, the mineral synthesist here visualizes and sets the problem. The observer reexamines his rocks and finds little if anything to indicate unusually high temperatures. The synthesist then suggests that perhaps these rocks were not molten as such, perhaps they are just crystal accumulations from gabbroid magma. This suggestion violates all the observer's previous concepts; at first he refuses to consider such a possibility, but when he is reminded of his own observation that there are no lavas of anorthositic or dunitic composition he consents to examine the rocks again. Thus are the suggestions of the thermal investigator tried in the crucible of observation and it is found that many of the examples of these rocks have characters not inconsistent with the concept of their origin as crystal accumulations, but the examination reveals variants of the rocks for which the observer is unable to visualize any method of emplacement except as a liquid.

At this stage informal coordinated investigation of ultrabasic rocks now stands, to use the term ultrabasic in the loose sense signifying anchimonomineralic and related types. It has been and is being carried

on in a most healthy and delightful spirit of cooperation. Geologists have been most assiduous in field and laboratory study of these rocks. They have examined a great many occurrences and have found that in several complexes the principal anorthositic and peridotitic facies are susceptible of interpretation as crystal accumulations. Certain peridotite dikes which, on first acquaintance, seemed to render peridotitic liquid a necessary assumption were found on closer acquaintance to have basaltic selvages, suggesting the possibility that the flow of a crystal mush of peridotitic character was lubricated by basaltic magma. Geologists have also found anorthositic injection gneisses which they are inclined to regard as indicative of the presence of anorthositic liquid, but there remains the possibility that they were formed by reaction between the material of a banded rock and a liquid permeating certain bands of the rock, which liquid may not have been of anorthositic composition. Such an origin of anorthosite is not inimical to the view that there are no anorthositic magmas, though it indicates that the suggestion of origin by crystal accumulation, made as a result of laboratory studies, does not exhaust the possibilities.

However, difficulties accumulate for him who questions the existence of anchimonomineralic magmas. Certain dunitic pipes are found which geologists believe were injected as liquid magmas and they adduce some evidence that the magmas carried at least a little water. Geologists turn also to a study of the serpentines, rocks which are usually secondary after peridotites, and they conclude that some of them are the direct product of consolidation from a highly aqueous magnesia-rich magma.

The older concept of the existence in nature of highly magnesian liquid magmas is thus extraordinarily tenacious of life. Its vitality may be the vitality of truth itself. On the other hand it may be the result of a natural reluctance on the part of the observer to abandon opinions once held. In any case the problem is handed back to the student of phase equilibrium who is asked to determine equilibrium relations of such highly aqueous magnesian magmas, from which determinations it is hoped that suggestions may arise as to how such magmas can come into being. For the very existence of such magmas, if indeed they do exist, presents the special problem of visualizing a process which brings together in the same liquid the most refractory of the common rock constituents, viz., magnesian silicates, and the most common hyperfusible constituent, viz., water, and these to the practical exclusion of all other constituents. The investigator of equilibrium may or may not be able to accomplish the task set before him. He too may suffer from a reluctance to abandon opinions once held and may suggest, before undertaking his task, that serpentine, as a result of its peculiar physical charac-

ters, is probably unusually capable of intrusion in the solid, plastic state especially in *nappe* structures, and that in the meantime the observer examine his intrusive serpentines with this possible mode of intrusion in mind. The condition envisaged would explain the commonly observed lack of contact effects, thermal and hydrothermal, upon adjacent rocks, though it is possible that such effects are not always lacking, if the glaucophane schists associated with certain serpentines are, as has been claimed, of the nature of contact aureoles.

Serpentines with such aureoles are, in any case, highly specialized examples, but it is plain enough from the foregoing discussion that in spite of much investigation the problems of these ultrabasic rocks are far from final solution. Yet it can hardly be doubted that a large body of facts regarding them has been ascertained as a result of the close scrutiny to which they have been subjected, following the suggestion that they may never have been molten as such, but may be crystal accumulations. So progresses our knowledge of these rocks and so must progress mineral science as a whole—*mente et malleo et catino et mente*.