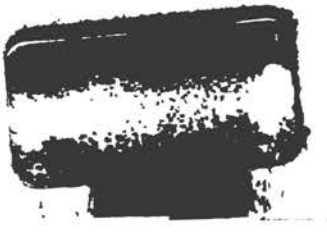
Suggestions Concerning Desirable Lines of Research in the Fields of Geology and Geography (1936)

Pages
88

Size
7 x 10

ISBN
0309347319

Bastin, Edson S.; Dunbar, Carl O.; Platt, Robert S.; Division of Geology and Geography; National Research Council



National research council. Division of Earth Sciences.

NATIONAL RESEARCH COUNCIL

DIVISION OF GEOLOGY AND GEOGRAPHY

WASHINGTON, D. C.

SUGGESTIONS CONCERNING DESIRABLE LINES OF RESEARCH IN THE FIELDS OF GEOLOGY AND GEOGRAPHY

1936

Edited by

Edson S. Bastin for Physical Geology

Carl O. Dunbar for Paleontology and Stratigraphy

and

Robert S. Platt for Geography

INTRODUCTION

During the year 1936 two surveys were initiated by the National Research Council which it is hoped will be helpful to research workers in the fields of geology and geography.

One of these concerns the borderland fields between geology, chemistry and physics and was initiated jointly by the three Divisions of the Research Council concerned with these sciences. This survey is in the hands of a committee of six geologists, three physicists and three chemists, with Thomas S. Lovering as chairman. The first meeting of the committee was held in Washington on June 5 and a second meeting is scheduled for December. A report will be forthcoming in due season.

It seemed desirable also to conduct by correspondence a survey of researches lying within the recognized confines of geology and geography. To this end the Divisional Chairman addressed a letter of enquiry late in April to some 300 research workers in these sciences. In this letter they were asked to indicate the lines of research that to them seemed especially needed at the present time and particularly worthy of encouragement.

The replies have been interesting in a variety of ways. A very few voiced disapproval of any attempt at stock-taking in research upon the grounds of futility or autocracy; some stressed merely their personal needs; some dealt largely in generalities. The great majority, however, presented carefully considered and in many cases very interesting and valuable suggestions. For all these candid opinions the Division Chairman is grateful. Many involved a large amount of thoughtful effort.

During the summer the Chairman has classified and edited the replies in the field of physical geology and Robert S. Platt, geographer and vice-chairman of the Division, has edited the replies in the field of geography.

Carl O. Dunbar kindly consented to edit the material on stratigraphy and paleontology.

The material is presented here largely as verbatum quotations with brief editorial interludes to give continuity or occasionally to amplify a topic to which the editor ventured to contribute. The most difficult task of the editors has been that of selection from the large amount of material submitted, largely with the purpose of avoiding duplication. For any sins of omission we crave the indulgence of our colleagues.

GENERAL GEOLOGIC SUGGESTIONS

A number of suggestions of a general nature or applying to several sub-fields may appropriately be presented before the classified material.

As bearing upon the general utility of enquiries of this sort and on the ways in which the National Research Council may function in the service of research the following statements are of interest.

C. E. Weaver[1] says:

"My feeling is that a very large amount of research is carried on at considerable expense and effort which would be far more effective in the advancement of geologic science if it fitted into some plan of co-ordination. The published results of many geologic investigations bear little relation to the more important problems which are under consideration by most geologists at the present time. If a larger number of the younger men could see the value of selecting problems whose solution would fill gaps in the different fields of geologic knowledge, it would aid not only in their professional advancement but also in that of the science."

In somewhat similar vein though less restrained are the comments of Paul Bartsch.[2]

"While all problems that make for advance in science are laudable, many or most of the efforts appear to result in heaving another brick on the already huge accumulation of other odds and ends of bricks that will probably remain on the waste pile forever.

---

1) University of Washington.
2) Smithsonian Institution.

"What we need is architects with vision, in the various sciences, who will design structures and furnish specifications to the various workers so that the bricks they are making may be used in the construction of the larger design. By so doing we should attain the goal towards which we are striving much more rapidly than we ever will with our 'fire at will' methods."

To the rugged individualists in science we commend the words of one of their most productive colleagues, Rudolf Ruedemann.[3]

"It is rather difficult for me to give an opinion as to the relative value of individual or institutional or committee efforts, as I have always been a lone worker who preferred to go unhampered on his way. While this method probably leads to the best results where the fields of research are as yet little worked and wide open for everyone who wants to work, it seems to me that too many divergent and scattered researches lead to a waste of effort when enough data have been accumulated to organize further research. That seems to me to be the case in various fields."

The functions of the editors are of course not "architectural" in the sense of Mr. Bartsch's letter and can hardly extend beyond the piling of a few "bricks" in orderly fashion. That these efforts might be followed by more effective activities is suggested by Kenneth C. Heald.[4]

"One of the things the Research Council can do, it seems to me, is to establish a committee to consider <u>what is needed</u> in the way of geologic research in the United States. I anticipate that your questionnaire will bring you a good deal of material of the type that should be considered by such a committee but I doubt that you will secure information which will permit you to appraise the relative importance to American geology of the various projects that may be submitted. I would like to see a real attempt to appraise the outstanding needs in the way of geologic research. There are a good many organizations with funds which they wish to devote to scientific advancement and it has been my experience that these organizations welcome suggestions which will permit them to put these funds to work intelligently and effectively. It does not seem to me that this is a project that can be handled hastily. It will require a good deal of thought, and there should be careful consideration and discussion by men who know research and who are familiar with the various types of geologic work that may be benefited by an attack adequately financed and directed.

"A second project, which is a corollary of the first, is consideration and compilation of data concerning effective research directors in the United States. I refer exclusively to geologic research. I do not believe there is any question by anyone, who has had to deal with research projects, that the effectiveness of any piece of work, regardless of the merit of the original idea, depends critically on the men who interest themselves in it and in particular on the research director. It is my own

---

3) New York State Museum.
4) Staff Geologist, Gulf Oil Corporation, Pittsburgh.

feeling that there are far too many so-called research projects in progress which are not adequately directed and because of this lack of direction, are doomed to failure or to mediocrity. When suggestions are made to organizations with funds, it is my own feeling that the suggestions should cover not only the problem that is proposed but also the men who may make the project a success."

That in all attempts to further productive research the great contrasts in talents and temperaments among research workers must be fully recognized and adequately provided for is forcefully emphasized in the following statement from George W. Bain.[5]

"An analysis of the present situation in geologic science would probably result in placing the emphasis upon the encouragement rather than the development of research. This can be considered under at least three headings. First, and foremost, is the type of workers available and the lines along which they may be encouraged to direct their energy when they are in doubt about the type of work to undertake; two persons equally qualified technically may vary greatly in productivity and attainment depending upon the type of work. The second question is the relative urgency of problems; areal problems are less apt to improve geological methods than the establishment of some principle or development of a new tool or line of thought, even though they may require equal time, talent, and funds. Under this heading might come adaptation of methods and knowledge from the sister sciences of physics, chemistry and biology. The third factor is the judicious distribution of research facilities among men and institutions in order to maintain the productivity of the workers and the goodwill of donors to research; it is conceivable that an institution might damage goodwill much more by accepting rather than refusing funds at a time when they could not be used effectively.

"Types of Researchers. At least two extremes of temperament can be recognized at a very early stage in the life of the men of science. The man at one extreme prefers to develop a thorough mastery of standard technique, methods, and literature; his life and work follows a course charted by the cases and experience of others. The other is content usually to have a working mastery of standard technique or methods. He spends much time exploring troublesome and generally avoided problems, and acquires thorough mastery of a technique and the literature only when they are essential to solution of a problem.

"It is easy to work with men of the first group because they are numerous, adequately industrious, and because they rarely disturb the regular course of events with embarrassing questions. They graduate with distinction and are a group to warm any instructor's heart; biennially they even add a good piece of regional geology to the literature. Their institutions are rarely embarrassed through controversy generated by the activities of men of this type.

---

5) Amherst College.

"The second group is turbulent from the beginning. As undergraduates they spoil the best pedagogical comparisons and subterfuges; as graduate students they become so involved in one phase of their studies that the work and attention to instruction declines in branches other than their main interest; and after graduate school they are centers of frequent controversies. Numerous contributions appear under their signature; some are none too well thought out or organized; but all activate work whether constructive or destructive in character. Their number is not great which may be fortunate for those who prefer tranquillity.

"Encouragement and direction of research interest. The form of encouragement to produce results may be expected to vary for the two extreme types. Many men of the first classification never become completely independent whereas it requires tact equal to that of the late Professors Chamberlin and Kemp to handle the second. It is even possible that this second type might thrive better if left alone.

"The more tranquil type does well in industrial research. A thorough mastery of technique and the literature enables them to carry an assigned problem to completion. Unless they have some of the restlessness and probing spirit of the second group, side relations of their problems and integration of their knowledge with the rest of their science may pass undone. They work best in committee where a head integrates their work and suggests the unexplored. This sort of person should be encouraged to enter group or cooperative projects for which they are admirably suited.

"Men of the second type usually work alone. They have too many problems of their own to take on any suggested outside and are more prone to appeal for help in fields over which they lack mastery than they are to give prolonged assistance to others. This behaviorism is illustrated by the spirit back of some of the major geologic controversies of this century such as the magmatic origin of mineral veins, contact zones in limestone, or even the origin of the earth. Reason and experience seems to indicate that this sort of person should not be drawn into a group project excepting on his own initiative.

"A worker who leans heavily on a senior, first for problems to study, and later for an advantageous sequence of studies to attain a result, cannot help but realize his dependence upon another for criticism of his results; his activity is rarely affected adversely by criticism. And such a worker merits criticism indeed, if that small amount of planning left for him to do, was done poorly. The second type of research worker is usually a more sensitive type and his work is almost certain to be retarded by careless or thoughtless criticism even when and where the criticism is merited. The critic who wishes to see geologic science advance would do well to keep this in mind, remember that the contribution is largely the thought and activity of a single person, and that it deserves serious consideration for at least two reasons; first, the contribution represents 'nonhybridized' thought and may have some really new suggestion; second, an inferior contribution by an independent worker usually represents greater personal effort than a better contribution by a dependent type of man. Criticism of work

and criticism of worker for presenting the conclusion should be sharply separated in all cases and graduated in degree for each of the two types of men if these two essential types are to be encouraged to continue their research activity.

"Urgent research problems. Geologic science is filled with problems suited to men of either group. Studies fall between two limiting extremes; at one end lies a large group of areal or descriptive works and at the other is a series of studies on principles. Areal or descriptive work rarely retards progress in geologic investigation; on the other hand, petrology was greatly handicapped until technique of the petrographic microscope had been developed and handling of it mastered; even today structural geology has received added impetus through the removal of a deficiency by petrofabric analysis. In general, problems involving principles would seem to merit major consideration, and areal work deserves only that encouragement which would assure use and test of such principles as may be developed.

"Distribution of research facilities. A stronger predisposition exists in state surveys, national surveys, and corporations developing natural resources to expend funds on areal studies than on development of principles to facilitate areal study and interpretation. This general condition indicates that the greatest need for funds lies in the field of research in principles and methods. Specifically the type of work done by the Geophysical Laboratory might be increased in range without changing the intensity in any present field of investigation.

"A certain part of available funds should be set aside to check principles developed by research in pure science against field performance. This is one very important lesson from industrial research; if a product of pure research fails to work in that field, each supposed fact must be checked to find out which is at fault and caused failure of the developed principle to predict working conditions. Too often, necessity that a principle shall function is not a requirement in pure science; apparent functioning in a few cases and desire that it will work in others is often sufficient to cause insistence on its validity rather than its potentiality.

"Uneconomical expenditure of funds when they are available because they are available or expenditure of funds when their productivity will be ridiculously low seems ill-advised because of future dissatisfaction and possible curtailment of income from that source. A personal feeling is that donors of real usefulness are impressed by the éclat of achievement made possible by their gift rather than by the distinction of being a donor. Judicious application seems essential to perpetuation of sources of revenue."

Turning from these general statements to specific problems of broad scope it becomes apparent from the suggestions that follow that the National Research Council may do much to further those researches that involve a large amount of patient detailed observation, all of which may contribute to the understanding of geologic processes of major importance.

The concept of rhythm or cycles in geologic processes lies at the very foundation of all attempts to subdivide or recognize discontinuity in the midst of a broad continuity. Whether such cycles be major or minor and whether the repetitions involved represent equal or unequal periods of time, the fact of repetition is of immense importance in foreshadowing at least some understanding of the forces operative in the varied history of the planet.

The cyclic deposition so clearly established by J. M. Weller and Harold A. Wanless in the Pennsylvanian and such rhythmic phenomena as the multiple glaciation of the Pleistocene are cases in point. The broader importance of the cyclic concept in geologic chronology is emphasized by Charles Schuchert[6] in the following words:

"As geological phenomena are cyclic in their nature, and as this periodicity is thought to be at the basis of our systems of rocks and periods of time, it is advisable to examine into all the periods to see how many are separated by universal breaks and which of these periods have transition formations into the next period. It may be advisable to place this matter in the hands of a special chairman, and to select the workers for each of the periods."

The following statement by F. F. Grout[7] is apropos at this point.

"There seems to be grave doubt whether or not the recurrent revolutions of geologic history are world-wide or much more local. Africa in particular seems to have had revolutions that alternated with those of northern countries. The assumption that diastrophism is world-wide and a safe basis for correlation needs check, and the problem is purely geological. It involves more detailed exploration of the continents, particularly those portions that are least known."

Of the problem of the permanence of continents and ocean basins Grout says: "A certain amount of foundering is recognized but the subsidence of ocean basins is of a different order of magnitude from the known graben. If the continents have remained positive blocks since Archean time it is no doubt attributable to the light rocks and adds the problem of how the granitic rocks were concentrated in the continental areas as distinct from the oceanic areas.

---

6) Yale University.
7) University of Minnesota.

"The problem of drifting continents should be attacked by rigorous methods, astronomic and geodetic."

A number of suggestions of a general nature but less fundamental are quoted below.

From E. M. Kindle:[8]

"In view of the unnatural political and economic boundary lines commonly imposed unavoidably on research work directed by official surveys and industrial organizations it should, I believe, be the aim of the National Research Council to give special consideration to research problems which need to overstep political boundaries or which do not promise predictable or early economic returns."

From Fred B. Plummer:[9]

"Establishment of Fellowships. One thing the National Research Council could very well do would be to bring to the attention of the commercial companies the value of establishing fellowships and research grants in the various universities which are equipped to carry on worth-while research but who, at present, are curtailed by lack of funds and time."

From Ira H. Cram:[10]

"Dictionary or Glossary of Geologic Terms. Many geologists see the need for a dictionary or glossary of geologic terms. Anyone who has written a paper and has attempted to use precise nomenclature has come in contact with the numerous definitions and interpretations of geologic terms. One can consult the various texts written by the so-called authorities and find many different definitions for the term in question."

It is obvious that a project of this sort involves a large amount of cooperative effort. The manner in which it might be accomplished is perhaps indicated by the critical analysis of the nomenclature of sediments recently completed by the Committee on Sedimentation of the National Research Council. In short, it would have to be accomplished by a number of groups competent in different subdivisions of the geologic field.

By far the greater part of the replies received relate to various subfields of the earth sciences and can therefore be separated into categories. In the choice of headings for this classification the editors have been guided more by considerations of convenience than of strict logic and consistency.

---

8) Geological Survey of Canada.
9) University of Texas.
10) Division Geologist, Puro Oil Company.

PETROLOGY AND PETROGRAPHY

Igneous Rocks

The most fundamental problems in the petrography of the igneous rocks are obviously those relating to the causes of magmatic differentiation. A. H. Koschmann[1] suggests an attack upon these problems by more systematic studies of the variations observable within intrusive bodies. He says:

"Variations within intrusive bodies include both mineral and textural variations of which the most common are basic borders, chilled margins, and vertical gradations in sills and laccoliths. The range in variations in such bodies, to my knowledge, has never been summarized. Variations within such bodies will undoubtedly give considerable evidence on the order and processes of differentiation and be a check on the determinations made on dry melts by the Geophysical Laboratory. Included in such a study should be a tabulation of the variations in the various rock types, gabbros, diorites, monzonites, syenites, and granites, which would throw light on the amount of differentiation in situ. Basic borders are most commonly regarded as chilled facies of the original intrusive magma, but this conclusion is doubtful in most cases.

"There are also needed (1) a thorough review and summary of the literature on intrusive bodies showing variations; (2) conclusions; (3) suggestions for further field observations and laboratory study. Such a study could well be made by a committee."

More restricted in scope but nevertheless constituting a formidable task are the studies in the variations in basalts proposed by A. C. Waters.[2] Although plutonists and neptunists no longer debate the sedimentary versus the igneous origin of basalts, these rocks still remain the focus of other geologic controversies. The following is quoted from Waters' letter.

"It seems to me that many of the most vital and difficult problems of Geology are related in some way to our common volcanic rock basalt. Many of our foremost petrologists believe that the majority of all the diverse kinds of igneous rocks are differentiates of a basaltic parent. The workers in tectonics are constantly speculating on the existence and characteristics of a substratum of basaltic composition - be it in the glassy, gabbroic, or eclogitic state. Many questions regarding other phases of geology and geophysics also depend for their solution upon an adequate understanding of the properties of this rock. Yet despite its importance none of the great basaltic lava fields such as the Deccan, Columbia River, or North Atlantic

---

1) U. S. Geological Survey.
2) Stanford University.

(Thulean Province), have received anything approaching monographic treatment. It is true that parts of the North Atlantic province have been carefully studied, but not the whole of it. The value of even these smaller studies is abundantly proved by the magnificient Mull Memoir of the Scottish Survey which has had such a marked influence on petrological thought in recent years.

"The sponsoring of a detailed monographic study of the Columbia River basalts would certainly result in the acquisition of much important material and information. Such a project would require the cooperation of several men, for the project is too large and too monotonous to be tackled by one man. Also some phases of the work would require the services of specialists. In the course of the investigation the following things should be done:

1. Geologic maps should be prepared of a number of critical areas where several kinds of flows are found, or where there is a good opportunity to get data on the mechanics of intrusion, differentiation, or other processes.

2. In the more monotonous and uniform districts several well-exposed canyon sections should be studied and specimens collected from each flow. The thickness, degree of vesiculation, flow units, and other characteristics of each flow should be noted. In this work particular attention should be paid to the collection of segregations or late crystallizing residues within flows, to the variation of composition within individual flows, to the occurrence of inclusions, etc.

3. Careful petrographic study of the materials collected should be made, and this work should be supplemented by numerous chemical analyses. Further chemical work, much of it probably of physical chemical nature instead of analytic, would be suggested as a result of these studies.

4. Helium-lead ratios of representative flows should be made, as well as determinations of the total radioactive content of the flows. This information would be of value not only for age determinations, but also in guiding such speculations as those of Joly and Holmes.

5. A careful study of the chemical and petrographic nature of the flows with respect to earth movements should be made. This is a particularly good region for this kind of study, for in the western part of the lava field we have the folded Cascade Mountains and in the southern part the block mountains of Southern Oregon. Preliminary studies that Dr. R. E. Fuller and I have made already appear to indicate that the flows increase in alumina and lime in the Cascade region, whereas they become richer in magnesium, iron, and the alkalies in Southeastern Oregon. The most uniform part of the volcanic field appears to lie in Eastern Washington where the flows approximate H. S. Washington's average 'plateau basalt' very closely in composition.

6. The specimens collected would also be available for experimental work at high pressures and temperatures, for obtaining data on the transmission of earthquake waves, or for determining other physical constants of use in geophysical investigations.

"The completion of a project of this sort would require several years and the cooperation of a number of specialists. Dr. R. E. Fuller and I have already done a considerable amount of work on the project, but the vast size of the area makes it improbable that we would be able to complete the work by ourselves."

It is interesting to note that suggestions by J. S. De Lury[3] follow somewhat similar lines, as indicated below:

"It is currently assumed that evidence of consanguinity in igneous rock series requires differentiation to produce diversity. Hence the common assumption of homogeneity of parental magma and the other requirements of the differentiation hypothesis - magma chambers, weak shell, passive intrusion, and the dictation as to the nature of the interior and the control of tectonics. All of these assumptions are open to serious question, so I venture to suggest:

"(1) Scrutiny of the evidence for "Homogeneous Magma," to be supplemented by -

(a) Chemical analyses of composite samples, each from a series of vertical sections of widespread units of extrusion or sill-like intrusion, which have been assumed to be homogeneous. Individual flows in Columbia River lavas or diabase sills at Cobalt, Ontario.

(b) Similar investigation of bodies which appear to have been differentiated in situ.

(c) Investigation (statistical and local) of plutonic complexes, e.g., Coast Range batholith, to see if consanguinity accompanies diversity. Lavas from a single vent commonly appear to be consanguineous, but they are relatively small in mass and decidedly local.

"(2) Scrutiny of the evidence of differentiation in situ, and review of the criteria of such differentiation, with a view to making theory and application agree. Sudbury supplies an extreme example: norite and granite are cited as differentiates, but do not accepted criteria decree otherwise?

"Even official geological literature is filled with the assumptions that diverse intrusives are co-magmatic and that they are to be referred to differentiation in a chamber of homogeneous magma 'below.' All of which is open to serious question."

Such studies as those suggested by Koschmann, Waters and De Lury should include not only comparisons of chemical analyses and of mineral composition, but also a study of the paragenesis or age relations of the rock minerals for recent investigations indicate that certain rocks that in the field have all the appearance of simple products of magmatic differentiation are in reality a product of later - sometimes much later - hydrothermal alterations of the normal igneous rocks.

The following suggestions from Geo. W. Bain[4] relate to fundamental problems of magmatic differentiation.

---

3) The University of Manitoba.
4) Amherst College.

"The current theory of diversity in igneous rocks has its root in differentiation by fractional crystallization. The writings and communications of Daly, Bowen, and all who specialize upon this problem give the original crystallizing solution a composition on the borderline between diabase and diorite. If all potash is concentrated in the 'sial' fraction, which is being too generous to the theory, the ratio of sial to basaltic products should be about 1 to 20. Field observations on intrusive masses indicate an even smaller proportion. The actual ratio of sial to basaltic layer in the earth calculated on the basis of fractional crystallization is altogether out of agreement with estimates based on the velocity of transmission of earthquake waves, with the composition studies of Washington, or with the work of Bowie and his associates on the force of gravity. It would seem that study of the origin of the alkalic feldspars in the rocks of the sial layer should rank at least on a par with the study of the equilibria of basalt minerals. Goranson approached one phase of this problem; Schaller worked on another; their work merited encouragement and it seems today that some additional study should be given to the origin of feldspars in granite."

The following recommendations of Frank F. Grout[5] also deal with fundamental problems of the igneous rocks.

"(1) The explanation should be sought of the general association of rock types with structures; basalts with geosynclines, granites with orogeny, and alkalic rocks with a perforation of the stable crust.

(2) The progress of petrology is delayed by a lack of accepted nomenclature. Every time a petrographer mentions a rock by name he has to define his terms or he is liable to misunderstanding. There should be a group to recommend good usage.

(3) The probability of assimilation by magmas should be studied where exposures of material seem to give most promise of information.

(4) Several problems of differentiation deserve study. The unmixing of magmas is maintained by some but not by all. Geochemical tests are needed. There is reference to anchieutectic magmas, but little experimental basis for the idea. The evidence for and against, from field and laboratory, should be made as clear as possible.

(5) The alkalic rocks and the anorthosites furnish specific problems in magma evolution.

(6) It is claimed as a fundamental principle in heavy accessory mineral studies that the amounts and ratios of accessories in a simple intrusive are nearly constant. This is in need of extensive check. The problem is complicated by composite intrusives and by differentiation within single intrusives, but the whole idea is open to question."

Another suggestion in the field of igneous petrography is that of A. F. Buddington[6] indicating the need as a contribution to the fundamental data of petrology of a very large number of systematic studies of specimens of igneous rocks, preferably related to each other as a series, for each of

---

5) University of Minnesota.
6) Princeton University.

which the bulk chemical analysis, the analysis of each variable constituent in so far as it may be separated, the quantitative mineralogic composition, and the geologic mode of occurrence are given.

In the borderland between petrology and ore deposits is the recommendation of Kenneth K. Landes[7] for more systematic studies of the genetic relations between pegmatites and hypothermal veins. "Naturally a research project of this type goes rather deeply into the realm of speculation. However, before the speculative stage is reached much can be done in studying field relationships between these types of deposits. Much information can be culled from the literature but field work in key localities is desirable."

Metamorphic Rocks

In the field of the petrology of metamorphic rocks certain rocks of mixed derivation or migmatites have proved particularly baffling. Florence Bascom's[8] outline of the "Problem of the Migmatites" is here quoted in full.

"Considerable areas of pre-Cambrian gneisses are proving to be neither orthogneisses (gneissose granites) nor paragneisses (coarsely crystalline schists), to use Rosenbusch's terms, but something more involved and with a history more difficult to decipher.

"We have criteria - chemical, petrographic, petrologic, and field criteria - for the discrimination of sedimentary and igneous rocks, but of a third class of rocks, mixed rocks or migmatites,[1] no criteria have been definitely determined.

"Migmatites - a schistose sediment (or a foliated eruptive) with (infused by) a silicic and a subsilicic eruptive - neither an eruptive rock proper nor a crystalline schist, offer peculiar difficulties of determination and call for special research in the field and laboratory.

"Chemical composition does not alone indicate a mixture of silicic and subsilicic igneous types but also the interpolation of a schist, which brings confusion into the calculation.

"Petrologic study reveals a certain confusion of textures: a partial obliteration of primary textures and retention of all stages of an interlocking crystalline texture combined with prevailing straight lines of a schistose texture, and yet no marked evidence of dynamic action.

---

1 J. J. Sederholm, Commission géologique de Finlande, Tome 5, No. 23, p. 110, 1911.

7) The University of Kansas.
8) U. S. Geological Survey.

"Petrographic study discovers a curious combination of silicic and subsilicic constituents: some myrmekite usually, feldspar showing primary form and arrangement, biotite and hornblende associated with quartz; a considerable range of constituents and mutual relations; mineralogical interaction rather than mechanical effects.

"Field observation may not disclose any obvious connection with independent intrusives, but possibly a transition through lit par lit migmatites into some subjacent igneous source.

"There is no recognizable basement and no supercrustal effects; veins not from without but due to the solution of invaded rock, possibly showing ptygmatic folding, demand explanation. The rock may be a polymigmatite.

"Such in general is the problem of the migmatites:
(1) the discovery of criteria for their certain determination
(2) the interpretation of their history by means of their chemistry, texture, mineralogy, and field relations.

"Their origin, under subcrustal conditions only, demands a geologic, chemical, and physical study of these conditions.

"This subject of research might advantageously be brought to the attention of petrographers. It is a problem primarily for study by individuals with the integration of results by a Committee.

"I might add that an associated subject is the investigation of mylonites."[11]

A. F. Buddington indicates the need for more systematic data on metamorphic rocks similar in character to those recommended by him on page 12 for igneous rocks.

G. M. Schwartz[9] thinks that in general little progress has been made in the broad aspects of metamorphism in the last twenty years and suggests that a systematic review and analysis of all that has been done should lead to a desirable clarification of the subject. To quote:

"The general problem of metamorphism or anamorphism stands out in my mind as greatly in need of organization and clarification. My own interest has been particularly in contact metamorphism and this phase alone needs clarification. What, for example, should be included in contact metamorphism? Practice varies and there is a gradation to other types, and yet it seems to me that contact metamorphism in a strict sense is

---

11 Charles Lapworth, The Highland Controversy in British Geology: Report of the British Association Adv. Sci., Part II, pp. 1025-1026. 1885.

9) University of Minnesota.

something rather definite and I believe cooperative work by a group interested in the subject would lead to a more precise definition of what should be included under the term. From this work progression into other aspects of metamorphism might lead to more precise definition of other types, eliminating terms not necessary and lead also to a discussion of processes and rock types. European geologists have done much, but many of their conceptions are at variance with some in current use in America."

A suggestion of F. F. Grout's is pertinent at this point.

"The great problem of metamorphism seems to be the crystallographic orientation known to occur in schists but not satisfactorily explained. The common statements refer to crystals 'in favorable position' or to crystallographic 'directions of easy growth.' These explain nothing until we find some physical basis for such terms."

Other problems in the metamorphic field are indicated by Geo. W. Bain as follows:

"The origin of porphyroblasts has become an even more acute problem since the structural petrologists, headed by Sander and Schmidt, have pointed out the inapplicability of the Riecke principle to a majority of the cases in which it has been used to explain crystal modification. Their work casts reasonable doubt on whether more than a very limited number of porphyroblasts can form by recrystallization, and then only where the rock composition is extremely favorable.

"Sander and Schmidt, with their new statistical approach to petrologic study, especially of porphyroblasts, strengthen the argument that introduced material forms an important part of these large crystals, a conclusion which reaches out not only into fields of petrology and mineral deposition but also into structural geology and the mechanism of tectonic movements as well. Perhaps no more important information is needed in geology than that bearing upon conditions causing growth of porphyroblasts and movements arising from their growth."

Donald C. Barton[10] suggests that evidence of flowage in sediments may be more widespread than has generally been recognized. He says:

"Nettleton's experimental report[1] in regard to the formation of salt domes taken together with our knowledge from other sources suggests more plastic flowage of rocks than geologists ordinarily envisage. It seems to me, therefore, a subject worthy of much further study in connection with the mechanics of all rock deformation."

---

1 L. L. Nettleton, Bull. Amer. Assn. Petrol. Geol., Vol. 18, pp. 1175-1204, 1934.

---

10) Humble Oil Company, Houston, Texas.

Sedimentary Rocks

The fields of sedimentology and sedimentary petrology though differing in their emphasis have so much of community and overlap that they may well be considered together. Among the broader suggestions in this field we may quote first those of W. B. Wilson.[11]

"Until petroleum geology made available, beginning some ten years ago, samples from the basin and geosynclinal areas, study of sedimentary rocks had necessarily been mainly from outcrops, and these outcrops tend to parallel the old uplifts. Occasionally geosynclines have been folded up in the mountains and exposed, but in general, students of sediments have mainly worked along the strike of regional structures and therefore parallel to the old shore lines. Now there are available data down the dip from the outcrops to the bottoms of the basins, and there is an opportunity to study facies changes of sediments of a kind and in a way that was not formerly available. In fact, detailed petrographic studies of sediments are still far behind similar studies of igneous rocks."

"There are two Basins in Oklahoma in which in the last two or three years very deep holes have been drilled, and in which additional and even deeper drilling may be expected in the future. One of these is the Anadarko Basin and the other the Ardmore Basin. In the Anadarko Basin tests have been drilled to depths somewhat exceeding 10,000 ft., one of the deepest of which is located in Section 34-10N-10W, Caddo County. This well did not reach the base of the Pennsylvanian. Eastward from this test there are other tests at not too great intervals to permit study of facies changes all the way to the outcrop of the base of the Pennsylvanian on the flanks of the Ozark Uplift. Among the interesting points that may be developed in a study of basin sedimentation would be an application of Wellers Cycles of Sedimentation in them with special reference to what happens to the cycles as one proceeds farther and farther off shore.

"In the Ardmore Basin wells are now being drilled to a depth of 8000 or 9000 ft. in Love County. Again, these tests do not necessarily reach the base of the Pennsylvanian. There are perhaps not enough of them as yet to make a very satisfactory cross section between the deeper parts of the Basin and the present outcrops, but I anticipate that such will be the case within the next year or two.

"In the Permian Basin of western Texas and southeast New Mexico there are now, or will shortly be available cycle data from tests drilled for petroleum which would throw light on what happens in the way of facies changes in a desiccating basin. However, due to lack of fossils and rapidly varying lithology of the section in that basin, I am not very hopeful that anything very definite could be worked out either now or in the very near future."

---

11) Gypsy Division, Gulf Oil Corporation.

Donald C. Barton urges the value of further studies of modern sediments as an aid in the interpretation of ancient sediments. He says:

"I have difficulty in reconciling some of the formations with which I am familiar with any type of sedimentation that is going on today. I am not sure whether the difficulty is real or imaginary. I have the impression that much of our reasoning in regard to the conditions under which specific formations have been laid down in the past is academic and that we have not as yet carried on sufficient field observation both on land and in the sea in regard to sedimentation. Specificially: (a) the question has always worried me why the Midway and Navarro each maintain so constant a lithology over hundreds of miles and why their lithology is much alike but yet maintains a constant difference; (b) another difficulty has been to envisage the conditions which would make the Wilcox so homogeneously heterogeneous over so wide an area; and (c) the conditions for the deposit of salt and anhydrite."

Charles E. Weaver[12] suggests a number of regions where the study of modern sediments is likely to throw much light on the origin of older sediments.

"Detailed investigations of present day sedimentation is desirable in certain embayments where the physical and environmental conditions are in part representative of those in existence during the past. Suggested areas of importance are:

1. The Gulf of California
2. The Gulf of St. Lawrence
3. The Gulf of Guayaquil in Ecuador

The Gulf of California is probably representative of conditions such as existed in the Coast Range of California during certain intervals of the Tertiary period. Continental deposits are accumulating in Imperial Valley. Immediately south an important delta is being built out into the head of the Gulf, and smaller deltas under conditions of aridity in the surrounding land areas are being formed farther south along the borders of the Gulf. Ocean currents are re-working and distributing these sediments. All of these factors jointly are contributing to the physical environment under which present-day marine fauna are living. Investigations of this type would be extremely valuable as an aid in interpreting the geological history of former embayments on the Pacific Coast of North America."

According to J. Marvin Weller[13] "Sedimentation studies of the Mississippian and Pennsylvanian system offer many research problems. There has been little done so almost any intelligently conducted work would yield important results. For example, insoluble residues of the Mississippian limestones have hardly been touched upon. Also, systematic petrographic studies

---

12) University of Washington, Seattle.
13) Illinois Geological Survey.

of the Pennsylvanian clastics are badly needed. They should throw light on the source areas of the sediments and climatic and depositional conditions."

The study of the finer grained sediments, both modern and ancient, presents peculiar difficulties but is not only of scientific but of great economic importance. Ralph E. Grim[14] has been doing pioneer work in this field and is particularly competent to present the possibilities it offers.

"The following suggestions on needed researches are limited to the field of sedimentary petrology, in which I am particularly interested.

"(1) Mineral composition of modern argillaceous sediments. A considerable amount of work has been done on the texture and various chemical properties of recent sediments but almost no research has been directed to their mineral composition. Soil investigators are rapidly providing important information on the mineral constitution of source materials for argillaceous sediments and a considerable body of information is being gathered on the constitution of clays and shales. Data on the makeup of recent sediments are vital to a satisfactory understanding of many features of clays and shales. The problem is one that would require a combination of optical, X-ray, and chemical methods in addition to some purification technique.

"(2) Texture of argillaceous sediments. A large amount of work has been done on the mechanical analysis of sediments but in so far as it concerns clays and shales, analysis usually measures the degree of disaggregation of the constituent particles rather than their size in the natural sediment. Because of mineralogical changes and mineral growth, a mechanical analysis may not yield information on the original particle size distribution. The subject of the significance of mechanical analysis in relation to natural particle size and original particle size is one that deserves research.

"(3) The synthesis and the transformation of clay mineral. A small amount of work has been done, particularly in Germany, in synthesizing clay minerals. So far as I am aware, practically nothing has been done in an attempt to alter in the laboratory various of the clay minerals. This is a vast field for research and one which should provide vital information for an adequate interpretation of the history of many clays and shales."

The characteristics and genesis of special types of sediments present numerous problems. J. E. Lamar[15] suggests the need for study of the factors governing the crystalline texture of dolomites and limestones and Chas. E. Decker[16] suggests that Oklahoma offers particularly favorable

---

14) Illinois Geological Survey.
15) Illinois Geological Survey.
16) University of Oklahoma.

opportunities for the studies of the origin of dolomites. He says:

"Bordering on geology and chemistry, a study in Oklahoma could be made on the origin of the dolomites of the Arbuckle limestone, as there are several thick ones in it. Also a study could be made of oolites which extend from the Henryhouse shale far down in the Arbuckle limestone."

W. A. Tarr[17] says: "My own experience has shown much need for the study of the carbonates of calcium and magnesium. Our simple statements regarding the deposition of these substances are far from presenting the true picture of their manifest varieties of origin. The complexity of their occurrence becomes evident upon analysis, but we have made only minor contributions to the subject in recent years."

A. O. Hayes[18] stresses the need for more thorough studies of glauconite - in part for its bearing on the genesis of sediments still richer in iron. He says: "A continuation of the study of the origin of glauconite should be carried on coincidently with stratigraphic investigations. The glauconite deposits of the Upper Cretaceous and Tertiary sediments of New Jersey offer an opportunity which has not been sufficiently improved."

Minor structures of sediments indicated by J. E. Lamar as in need of study are "a) the mechanics and chemistry of cone-in-cone formation and associated phenomena and b) the conditions governing the formation of concretions - quartzose, sideritic, pyritic and others."

W. H. Twenhofel[19] recommends "that more attention be given to the influence of micro-organisms on the deposition of sediments, the diagenetic changes after deposition, and rock decay. From what little is known, I am certain that research in this field will be productive of great results."

The influence of composition of the waters on sedimentation is a field almost untouched as yet except for the saline sediments but is of great importance particularly in its bearing on phosphate deposits and the sedimentary iron ores. M. G. Cheney[20] says:

"The effect of increasing pH value upon the deposition of clays and silts under conditions similar to those which prevail in shallow epieric seas should offer interesting results. Extensive areas of thin shale beds present physical problems regarding their deposition which may be answered by change from fresh to brackish and saline water which evidently occurred

---

17) University of Missouri.
18) Rutgers University.
19) University of Wisconsin.
20) Petroleum Geologist, Coleman, Texas.

repeatedly in the many cycles recorded in Carboniferous and other deposits."

The importance of the newer mechanical and statistical techniques in the study of sediments is stressed by W. C. Krumbein[21] who has devoted especial attention to such methodology.

"The study of sediments is a field which illustrates the close connection between geology and other physical sciences. It is no longer sufficient to analyze a single sample in the hope that from its histogram one may decide whether the deposit is wind- or water-laid. Modern sedimentary studies involve sets of related samples covering entire environments of deposition, and the analyses include both the physical and chemical nature of the deposit. Thus, size frequency analysis and shape analysis are among the physical approaches, and mineralogical analysis and organic content analysis furnish chemical data. The interpretation of these data involve primarily a sound statistical technique.

"At present much remains to be done in establishing techniques on a firm basis. Thus, in the size frequency analysis of sediments there must be considered the influence of the dispersing agent on the colloids, and size effects of diagenic changes must be evaluated in reaching any conclusion about the original composition of the sediment. Likewise in the analysis of the organic content (a point of considerable significance in petroleum research) such factors as the relation between organic matter and size is important, but the analysis of one attribute may influence the results obtained in the other. Trask has been a leader in this type of work. Throughout the field, then, there are overlaps between chemistry and physics, with the geologist in such a position that his own conclusions depend on the solution of purely chemical and physical problems. Likewise the statistical study of data requires sound theory to support it, and unfortunately it is necessary to use logarithmic probability curves in much of the work. Such curves are just beginning to receive attention from mathematical statisticians, and are nearly completely unknown to many workers in sediments. Mathematics also enters strongly in questions of the size and shape of particles, and it lies within the domain of pure mathematics to define the 'shape' of irregular solids. Wadell has made important contributions here.

"It should also be emphasized that at present the training of physical geologists does not enable them to cope with these borderline problems. Furthermore, even to use the findings of specialists in the other physical sciences, it is necessary for the geologist to be able to apply the techniques, and this application often requires mathematical, chemical and physical knowledge which at present is usually not a part of the geology student's equipment."

---

21) University of Chicago.

STRUCTURAL GEOLOGY

AND

STRUCTURAL PETROLOGY

Under this dual caption may be included tectonic features of all degrees of magnitude. A number of the replies received are general in their scope and these may appropriately be presented first. Paul Billingsley[1] offers some wholesome criticism of current methods in this field and a plan for a more thorough mapping technique.

"The branch of earth science in which I am particularly interested is concerned with structure of the earth's crust. I feel that there has been much premature theorizing in respect to structural processes, with an inadequate background of actual structural facts. Particularly in North America are we far behind in our mapping of mountain structures and of structure in the basement rocks.

"Adequate mapping would provide a necessary environment of reality, embracing the problems of mountain making, intrusions, ore deposition and so forth. Mapping of the necessary precision is as yet almost nonexistent in Western North America. Investigators skilled in Alpine structure and pre-Cambrian structures will find unlimited fields in the mountain ranges of the West.

"In general, I feel that the theoretical and laboratory phases of geology have far outstripped the factual background, which can only be learned by widespread, close field work. The field workers should not, as has generally been the case in the past, be specialized in oil structures or coal fields or structures of mining districts. They should be competent to observe, map and understand as a process all earth structures. Attacks could be made simultaneously on many crucial areas, in the general method adopted by the Princeton party in the Absaroka-Big Horn region. In these investigations seismology and the geophysical approaches to earth science should be closely tied to the actual, observable geological structures."

Pertinent at this point is the comment of Frank F. Grout[2] that "The technique of structural geologic work in field and laboratory is not well systematized. Much work is needed and it should be done with a terminology and with map symbols that will not be too confusing to readers."

---

1) Consulting Mining Geologist, Salt Lake City, Utah.
2) University of Minnesota.

Under the leadership of Grout the Committee on Batholiths of the National Research Council rendered singularly effective service in clarifying our knowledge of this class of intrusion and its associated dynamic effects. Grout's suggestion of further work needed in this field is therefore of particular weight. He says:

"The rise of batholiths is a major structural problem and at present the Cloos method seems to be contributing the most useful evidence. It should be taught and practiced much more widely. Field excursions should be planned at districts that promise conclusive results and full details obtained to make them convincing to all."

According to Ian Campbell[3] "Information on the third dimension of batholithic structures is badely needed. Geophysical technique and interpretation have now reached the point where investigations of the structures of the batholiths, especially as regards the shape of the outer walls of the chamber, should disclose accurate and valuable information."

Evans B. Mayo[4] in his letter of reply has gone to the pains of indicating at some length and very instructively the problems involved in the study of batholithic intrusions and his contribution is quoted in full below.

"Magma, whether or not it has reached the surface, must have existed for a time in the plastic or semi-solid state; therefore, unmetamorphosed igneous rocks are likely to be the most sensitive and faithful indicators of structural conditions in the crust at the time these rocks were molten. The story which these once-molten masses have to tell is recorded (a), in their internal structures (b), in the nature of their contacts with the invaded rocks (c), in related modifications of older structures in the wall rocks and (d), in the shapes and arrangement of the igneous bodies on a regional scale.

"Studies of the rôle of igneous activity in crustal mechanics afford a promising approach to an understanding of the structural history of the earth. Some of the problems confronting this line of approach are outlined briefly below, and some methods for their solution are suggested.

"(a) Studies of the internal structures of igneous intrusions are of significance in connection with problems of space and intrusion mechanics. Does the magma rise by overhead stoping, by melting or assimilating the wall rocks, or by forcefully displacing the invaded rocks? The elements that have been used in such studies may be classified roughly as follows:

---

3) California Institute of Technology.
4) Cornell University.

(1) Inclusions and early-formed crystals that existed as solid suspensions during the plastic stage, and by their parallel orientation indicate the principal direction in which the magma advanced. (2) Primary joints and faults, formed as the intrusion solidified.

"Structural surveys, using these elements, have already shown that many intrusions have a regular internal pattern that could not possibly have resulted if the magma had been emplaced by overhead stoping. The pattern of flow structures, together with the arrangement of primary joints and faults, indicates forceful emplacement of the magma. Many more intrusions will have to be studied, however, before statements of general application can be made. There is special need for more information concerning the internal patterns of flat, sill-like masses, since this type of intrusion has so far received comparatively little attention from the structural viewpoint. Every new survey of an intrusion will be a welcome addition to our knowledge of intrusive processes.

"The origin of the oriented, dark inclusions that occur so abundantly throughout certain granite masses has not yet been definitely settled. Some writers regard them as segregations from the magma, while others consider these inclusions to be reconstituted fragments of the wall rocks. Obviously, this question has an important bearing on the problem of the space now occupied by the magma. In some places it is possible to find 'frozen into' the igneous rock, huge masses of wall rock that show various stages of disintegration to smaller fragments. The fragments themselves may show successive stages in mineralogical reconstitution toward types which closely resemble the dark inclusions in question. Hurlbut (Amer.Min., Vol.XX (1935), pp.609-630) has recently made a valuable contribution to this problem. There is need for many more field observations, supported by petrographic studies, to determine how broadly Hurlbut's conclusions will apply.

"(b) Intrusive contacts indicate the manner in which the wall rocks have yielded to the igneous invasion. If the wall rock behaved as a brittle substance, the igneous contact will, in general, be discordant; if the invaded rock yielded plastically, concordant contacts should result. Discordant intrusions, whose walls have shattered rather than flowed, should offer the most favorable conditions for overhead stoping; indeed, the very existence of such discordant bodies has at times been taken as proof of such stoping. There is need for re-surveys of such intrusions to determine whether the internal structures and other geologic relations accord with the stoping hypothesis.

"The evidence of assimilation should be plainest near igneous contacts. The fact of assimilation has been clearly established, but there is room for much further research to determine the relative importance of this process as a factor in the emplacement of intrusions.

"(c) Structures in the wall rocks may help to complete the picture of intrusion mechanics. If it is known what type of structure existed in the wall rocks previous to intrusion, then it becomes possible to recognize modifications that have been superposed upon the original structure. Thus:

the initial regional trends may be deflected to fit the forms of intrusions; folds may be tilted up along intrusive margins; wall rocks may be differentially thrust away from the invaders, etc.

"A question may arise as to whether the later modifications are a result of forceful emplacement, or whether they result from the crushing of older structures against already solid intrusions by lateral compression. If exposures are favorable, this question can usually be answered one way or the other, and the place of the intrusions in the structural history of a region thereby determined. The study of wall rock structures related to igneous intrusion offers a fine field for structural research. In addition to furnishing further examples of forceful intrusion, it should throw more light on the time of intrusion in relation to mountain folding.

"(d) As detailed studies in a given region progress from locality to locality, they will lead up to more general problems, involving the broader relations of intrusions to the regional structure. In many regions it has been noted that the intrusions are elongated parallel to the general trend of folding. Where huge, elongated batholiths parallel the axes of mountain ranges, it seems logical to suppose that in some way melting of the crust has been localized directly beneath the folded geosyncline, and that the resulting magma has risen as a single huge unit along the axis of most intense deformation. Under these conditions, it has been suggested that the intrusion of a batholith is an attempt to restore the isostatic equilibrium that was disturbed as a result of folding, erosion of the resulting highlands, and the large-scale generation of magma.

"On the other hand, it is known that some supposedly giant batholiths are in reality composite groups of many small intrusions, differing somewhat in age. In a few cases it has been shown that whereas the group as a whole parallels the regional trend of folding, the axes of individual intrusions are arranged en echelon, and show the same barbed relation to the regional trend that gash veins show to faults. In such a case, it seems that the growth of the batholith has been a process of slow accretion, and that it has been controlled by local tension, developed as a result of horizontal motion.

"In which of these ways is a batholith formed; are some formed in one way, some in another, and some by a combination of both? We still await the answer, which must have an important bearing, not only on the mechanics of igneous intrusion, but also on broader problems of crustal motion.

"Long after a period of batholithic invasion, some areas have undergone great uplift, accompanied by block faulting and considerable volcanism. Detailed studies in such regions show zones of faulting and chains of volcanoes arranged en echelon and in barbed relation to the regional trend of folding. Such structural patterns again suggest horizontal motion, whereas the great uplift reveals an important vertical component. The study of such newly-uplifted regions probably yields the best information concerning what happens at the surface when a composite batholith is growing at depth. Such areas should be carefully searched for any evidence that would indicate that large intrusions ever destroy their roofs.

"Regions that show all of the features discussed above are not common, but any well exposed area of intrusive rocks is likely to yield information that will contribute to the general problem. What is needed is more data from many regions. These data can best be obtained in the field by individual workers, properly trained in igneous geology. The field worker should be familiar with the methods of granite tectonics, as developed by Prof. Hans Cloos, and he should be able to combine these methods with more familiar field practise to meet the demands of the particular problem. To obtain the greatest value from his work, the investigator should be able, where necessary, to confirm his deductions by petrographic methods.

"The ultimate solution of some of the broader problems may depend upon sources of information that are beyond the scope of the individual geologist. Wherever this is the case, the problems will sooner or later demand cooperation between the geologist and students of such border sciences as geodesy and geophysics."

The following recommendations from A. O. Hayes[5] indicate certain broad problems and suggest specific regions favorable for their attack.

"Further studies are needed wherever folded and faulted crustal conditions promise to add to our knowledge of plastic flow of rocks and the nature of the forces which have produced it. This is related, of course, to the question of limits of the continental drifts of overthrusting which have been revealed in the 'Nappes' of the Alps and the problem of the 'Zwichen Gebirge.' Mississippian and Pennsylvanian rocks of the Maritime Provinces in Canada, especially along the Bay of Fundy, Northumberland Strait and possibly in southwestern Newfoundland, offer an opportunity to study some five formations with unconformable relationships, each with a basal conglomerate, indicating rejuvenation after marine or estuarine accumulation. The deformation is accompanied locally, especially in the western part of Cape Breton Island, by flat-lying thrust planes, the limits of which have not been determined. The Joggins section in the Upper Bay of Fundy region and the Sydney, Cape Breton coal field region are relatively little disturbed areas situated along side the highly disturbed belts and thus add to the data for interpreting the structural history.

"Since this region lies nearest to Europe, it is the most promising locality for inferential interpretation of the Atlantic Ocean floor."

Relating also to major overthrusts as tectonic features are the suggestions of G. R. Mansfield.[6]

"The Bannock overthrust in Idaho and Utah, from areal geologic evidence, appears to be 300 miles or more long and to overspread folded and faulted formations to the east at least 36 miles. Windows in the upper thrust block 20 miles or more back of the trace appear to indicate that the thrust plane, though folded and faulted to a greater or less extent, is relatively flat lying for at least that distance. Interpretations of the

---

5) Rutgers University.
6) U. S. Geological Survey.

eastern margin of the upper thrust block suggest a sole for the main thrust plane and an imbricated structure of greater or less complexity associated with that sole. Nothing is yet known of the roots or place of origin of this overthrust. The plan here suggested would provide for a gravimetric and seismographic or possibly magnetic survey cross section along one or more lines normal to the front of the overthrust and carried back from it at least as far as the innermost recognized window (20 miles) and preferably far enough to solve or throw more light on the root problem. If several such cross sections could be made it might be possible to contour the thrust plane within the area controlled by the proposed survey. Such a study would reveal much of the topography of the thrust plane and furnish as an economic by-product a check on existing estimates of reserves of phosphate rock. The depth limit for estimating reserves, as imposed by existing regulations of the Department of the Interior, is 5,000 feet. In making the estimates referred to, the assumption has been generally made that the plane of the overthrust, which cuts off below the structures in the upper block, in which the phosphate deposits are generally contained, does not lie within 5,000 feet of the surface. That this is not true for the thrust plane as a whole is indicated by the windows mentioned above. If the proposed survey should show that the thrust plane is generally shallow and that beneath the apparently deep phosphate-bearing synclines it is less than 5,000 feet in depth, the validity of the existing estimates of reserves of phosphate would be seriously impaired. Many other problems connected with interpretations of overthrust phenomena might be greatly clarified by such a study."

The following problems in the tectonics of major folding are suggested by F. F. Grout:

"1) The relation of geosynclinal accumulation to earth movements; which is cause and which effect?

2) The relation of diastrophism to geosynclines. Do the trough and welt of the orogeny transform a wide basin of a geosyncline into a narrow trough and welt; or are the trough and welt linear features distinct from the basin?"

Integrating somewhat with Grout's suggestions is that of Donald C. Barton[7] in regard to the basements of the great sedimentary basins. "A large part of the area of many continents is occupied by large sedimentary basins. We may know much about the upper part of the sedimentation, but in general we know relatively little about the actual configuration of the basement on which the sediments lie. A knowledge of the topography of that basement would give us enormous increase in knowledge of the regional geologic framework."

According to F. H. Lahee[8] further study should be given to the dynamics of the Gulf Coastal Plain.

---

7) Humble Oil Company, Houston, Texas.
8) Sun Oil Company, Dallas, Texas.

"A great many fields have been found, most of them roughly paralleling strike, and evidently related to the gulfward settling and tilting of the Cretaceous and Tertiary prism portrayed in the Gulf of Mexico. In many cases these faults are paired, one having a drop on the regional down-dip side, and the other a drop on the regional up-dip side, with a narrow graben between them. It would seem that this is due to tilting, with a dropping of narrow wedges along certain fulcrum lines."

W. H. Twenhofel[9] makes the following comment concerning the importance of geological structures of lesser magnitude.

"Studies have been made of the larger structures and, as every geologist well knows, these are not fully understood. The minor structures which are found in such areas as the Appalachian Plateau, the Mid-Continental basins, and such arches as the Ozarks and the Cincinnati may contain the clue to much of that which is unknown. There are scores of minor structures in the Mid-Continental basins which are parallel to the general trend of the basins in which they lie and which are in relatively incompetent strata. Were these formed by pressure laterally applied?"

Similar ideas were perhaps in the mind of E. H. Sellards[10] when he wrote as follows:

"The problems of structural geology, it seems to me, are not receiving as much attention in some parts of the country as they deserve. This perhaps is not true of the mountain regions on which a great deal of structural work has been done. I am thinking more particularly of the more level-lying parts of the United States where structural features are more or less concealed, thus adding to the difficulties of the problem, and where the observations must be obtained in some instances from drilling records. We have recently completed for Texas a structural map with 500 foot contour interval. This map helps to emphasize the very profound structural features that are concealed under level-lying later formations. This is a problem that can be advanced only by cooperation of many workers."

The field of structural petrology or microtectonics is one in which there is a large amount of current interest and one that is being cultivated by a committee of the Research Council. In this field G. R. Mansfield offers the following comments:

"Microtectonic methods evolved in Europe are being introduced in this country to a greater or less extent. There seems to be a tendency on the part of some of their users possibly to attach too great importance to them and perhaps to attribute to them greater validity or potence than they may in fact merit. It would be desirable if feasible to have a group of three or more qualified specialists in stratigraphy, petrography and tectonics

---

9) University of Wisconsin.
10) University of Texas.

restudy some area already mapped in relatively recent times and see what checks, if any, can be devised to test the validity of interpretations reached by microtectonic methods. I have in mind an area such as the Gaffney and Kings Mountain quadrangles (North and South Carolina) which is covered by one of the later issued geologic folios of the U. S. Geological Survey. Here is an area mapped in detail with great care and skill by one of our ablest tectonic geologists. It is part of a region in which much geologic work remains to be done. Further detailed work in this region will require all the knowledge, skill, and improved methods that can be brought to bear upon it in order that durable results may be obtained. Doubtless such a study will show that some of the mapping or interpretations already made could profitably be revised. On the other hand, sound mapping and interpretations based thereon should tend to check microtectonic interpretations. If a fair approach to agreement is reached by the two methods, new work in the general region could be undertaken with a much greater degree of confidence than now seems attainable."

The following suggestions from Geo. W. Bain[11] relate in part to the use of minor textures in solving larger structural problems.

"The need for the determination of the direction and amount of movement, or kinematics, of a structural unit seems to be more pressing in structural geology than the cause or mechanics of earth movement. The need is apparent in certain major as well as minor microscopic examinations.

"The rebound concept of movements on an active fault plane has grown out of an investigation of the mechanics rather than the kinematics of earth movements. Assembly of all data of relative movements of structures and boundary lines on the San Andreas Rift during the earthquake of 1906 shows that north of San Andreas Lake the west side moved relatively northward but in the reverse direction south of that point. All movements should be in the same direction and of about the same amount to be in strict accord with the rebound concept. Had the picture been built up from the movements, or kinematics, concept rather than from a possible cause or mechanics idea, it would have founded a very different working hypothesis.

"The movement data compel recognition of an expansion outward from the epicentral region. If the movement was the expression of sudden expansion, then it necessarily follows that decrease in mass per unit volume and decrease in the force of gravity accompanied the movement. The few gravity measurements made in the last part of the past century indicate increasing value for the gravitational 'constant.' Measurements after the earthquake indicate a decrease to below the earliest value. The volume change gives a measure of the force. The combination of the force and the direction and the amount of movements furnish tangible figures for studying the mechanics of the action.

---

11) Amherst College.

"Inverting the order of the observations, and measuring gravity changes at a location should yield an accurate method of predicting the time and location of earth movements of this sort. Such work is very essential to the improvement of structural studies and the prestige of the geological fraternity; it extends their work from the past into the future. Efficient study of this sort of problem requires active cooperation of a large group with very diverse training ranging from pure geologists, through geodesists to pure physicists.

"Petrofabric analysis along the lines developed by Sander and Schmidt is an important tool in studying movements which took place in the distant past. This new aid to geologic research makes possible an evaluation of the relative importance of the partial movements throughout a rock mass and the great local movement along overthrust shears. Petrofabric analysis furnishes one of the most important methods of investigating movements in minor geanticlines such as the one between the Champlain and Boston basins. The larger structures suggest radial expansion in an east-west vertical plane but the only method of determining exactly the partial movements is a systematic petrofabric analysis of rocks in a cross section of this great arch."

THE GEOLOGY OF THE OCEAN BOTTOM

The study of the geologic phases of oceanography has recently undergone a notable revival of interest in this country, partly as a result of the work of F. P. Shepard in drawing attention to the surprising number and magnitude of submarine canyons and partly because of the successful application of geophysical instruments and of new sampling devices to a determination of the nature of the materials on the sea bottom. The perfecting of phonic sounding has been an added stimulus to such studies.

The results already obtained are sufficient to indicate that new lines of attack have been opened on some of the most fundamental of geologic problems, such, for example, as the degree of permanence of the ocean basins through long periods of geologic time. The advancement of these studies in the United States is proceeding in an effective manner under the guidance of the Committee of the Geophysical Union on the Geophysical and Geological Study of Oceanic Basins and Their Margins, of which Richard M. Field is chairman. This committee functions not only as a planning agency, but is integrating in a most effective way the work of the many cooperating organizations and individuals. Field summarized in a very interesting way the present status of this work in the symposium on "Recent Developments in Geophysics" at the 1936 Annual Meeting of the American Geophysical Union.

It is clearly evident, however, that this immensely promising field should be rapidly developed and that for such development more unified facilities are needed - particularly the facilities of a special vessel for geophysical and geological work at sea, especially in waters contiguous to the United States. The difficult techniques involved have now been

perfected to a degree that fully warrants such assistance. Francis P. Shepard[1] outlines as follows some of the researches to which such a vessel would be dedicated. They would "include careful study of the submarine canyons to determine in more detail their exact character, the nature of rock formations on their walls, and the nature of processes which are in operation along the channels. Another field will have to do with the nature of sediments accumulating on the continental shelves and on the continental slopes with the hope of deriving information from a study of these sediments for comparison with the ancient sediments on the lands. A third field will have to do with the geophysical prospecting such as Ewing has already carried out with the purpose of finding the nature of the sedimentary covering on the continental shelves.

"As regards the younger geologists, it seems to me that the field of submarine geology is one in which they should expect to find a wealth of valuable material. The field is so full of problems that large numbers of students could work in it and have the opportunity to make valuable contributions. While some of them could work in association with established oceanographic laboratories, others would find problems from the study of sediments collected from the ocean bottom during expeditions, and still others could work on physiographic problems from the great accumulation of soundings which can be subjected to all sorts of analyses."

Geo. W. Bain[2], after referring to the work of Shepard and others on submarine canyons, suggests the appropriateness of certain complementary studies. He says:

"The canyon cutting period is assigned to post-Miocene and pre-Pleistocene time; it represents a very limited and similar interval on both sides of North America. Drowned valleys of about the same period have been mapped off the European coast. These facts seem to indicate a fairly general distribution throughout the northern hemisphere.

"The late William Morris Davis in his brilliant and thorough work, particularly on coral islands, pointed out the necessity that development of a feature in one region required a complementary one to appear in another district. Thus, if all the northern hemisphere was elevated 7000 feet more or less at the end of the Miocene period, the southern hemisphere should be inundated a proportionate amount and sea beaches should be cut at the level of greatest flooding, or the ocean floors should be elevated the above amount to cause the water to spread out over the land. The water from the continental shelf in the northern hemisphere must have gone somewhere and volume in ice caps and in the ground cannot account for even a small part of it.

"The elevated beaches of the Pacific Coast might be a record of the necessary late Tertiary depressions to balance the North Atlantic elevations of that period if this same region did not have the drowned canyons mapped by Shepard. It seems that an essential complementary part of this physiographic study of submarine canyons is an examination of the late Tertiary elevated beaches of the world."

---

1) University of Illinois.
2) Amherst College.

ECONOMIC GEOLOGY

Metalliferous Deposits

In the field of ore deposits the following general comment by Donald McLaughlin[1] may well serve as a preface to other more detailed suggestions.

"In connection with research on ore deposits, I am more and more impressed with the need for observation and measurements of pertinent data in far greater detail than it is done in the usual field investigation. Geologists on the staffs of operating companies are almost the only ones who have opportunity to undertake work of this sort, and in some cases where they have appreciated the need, they are accumulating quantitative data of fundamental value. The restrictions imposed by commercial conditions, of course, limit publication in many cases, but not infrequently the chief cause is lack of time or energy on the part of the geologist concerned, under the stress of active work."

In the great class of ore deposits that is related to vulcanism the most fundamental scientific problem is doubtless the placing of the mineralization in its proper place in the total sequence of the igneous history of the district, tracing the mineralization if possible to parent magmas of determined age and composition. In the orthomagmatic and contact metamorphic deposits and the ores of pegmatitic affinities these relations are of course usually clear. In certain classes of hydrothermal deposits, particularly those deposited relatively close to the magmatic source, the relationships are also clearly discernible as in the case of the tin veins. For a great array of deposits formed at a greater distance from the parent magma the tracing of genetic relationships is usually attended with many uncertainties but must be accomplished before the processes of ore deposition can be viewed as they should be as merely one part of the general igneous history of a region.

---

1) Harvard University.

In the following paragraph W. L. Whitehead[2] stresses this need and one of the methods by which it may be accomplished.

"Among the fields of research in the problems of the economic geology of mineral deposits that of precise correlation of metalliferous deposits with the igneous bodies to which the genesis of the deposits is referred is to me of great interest. Time relations are often obscure or are fixed only within a long period. Accurate correlation is now possible in some instances by means of determination quantitatively of radioactivity decomposition products and has been accomplished successfully in certain cases by Dr. Urry. Extension of this type of research will confirm or disprove in many mining districts qualitative time correlations of metal deposits with intrusive bodies and, I believe, offers in future research a most fruitful field which is excellently adapted to cooperative effort."

With increasing opportunities to study the deeper portions of mine workings it has often become possible to recognize certain main channelways that have been followed by the mineralizing solutions on their journey upward to the more productive levels of ore deposition. G. F. Loughlin and his associates of the U. S. Geological Survey have been particularly successful in locating such channelways in their studies of Cripple Creek, Colorado. The recognition of these trunk channels has in some cases been an important aid in further development. Donald McLaughlin emphasizes this matter in the following words.

"The study of channelways by which metal-bearing solutions migrate through rocks and along which metallic ores are formed impresses me as one of the most fruitful fields for research from the viewpoint of a mining geologist. A vast amount of work still needs to be done on permeability of different types of rocks, not only under ordinary laboratory conditions, but at high pressures and possibly temperatures."

Similar suggestions are made by W. L. Whitehead as follows:

"Recently I have been engaged in studies of the mineralization below orebodies in which the economic constituents are of late origin in the mineral sequence. These roots or channels of mineralization are of economically barren nature and contain minute quantities only of the diagnostic minerals and elements. It is of decided importance in the determination of the structural relation of the ore deposit that such

---

2) Consulting Geologist, Cambridge, Mass.

channels be known, and investigation of the non-orebearing zones under orebodies has given results in several mines that persuade me to the belief that the method might well be used more widely and the general features of channel mineralization be definitely determined."

While the mineralogy of an ore is of prime importance as indicating the minerals that were stable under particular conditions of ore formation, the other side of the shield is revealed by the wall-rock alterations which indicate other minerals as unstable in the presence of the mineralizing solutions. The need for more exact work in this field is indicated by Donald McLaughlin in the following paragraph.

"Quantitative studies of rock alteration associated with ore deposits, involving adequate sampling, chemical analyses and petrographic investigations offer excellent opportunity for combined field and laboratory attack that might lead to much clearer understanding of ore forming processes - and when translated into mappable field terms might afford helpful guides to ore. Our investigations of such problems to date - especially in the field aspects - have been too qualitative."

With these recommendations the editor of these notes is in entire accord. In spite of some brilliant work, the study of wall-rock alterations is still in a pioneer stage. The number of variables involved presents grave difficulties in the classification of wall-rock alterations but the direction of progress is clearly indicated through the multiplication of exact chemical and mineralogical data and the abandonment of a nomenclature that at best is only partially and qualitatively descriptive.

The following suggestion from W. L. Whitehead is pertinent at this point.

"A field of great interest is that of research on the mechanical and chemical characteristics of wall-rocks and the effect of such properties on the structure and contents of veins and replacement ore bodies. Quantitative data are most essential in this field and are largely lacking."

Wall-rock alteration is not to be thought of as an economically inconsequential process accomplished by the mineralizing solutions after

their main work of ore deposition has been completed. Replacement ore bodies are rock alterations and even in fissure veins the solutions that leave the fissures at lower levels may in part return at higher levels, modified in composition to exert a direct and potent influence on ore precipitation. It is pertinent to note the recent inauguration by G. H. Anderson and H. J. Fraser at the California Institute of Technology, under a modest grant from the National Research Council, of experiments to test the degree to which granites, marbles and other rocks are permeable to aqueous solutions at various temperatures and pressures and to determine the pores and channelways utilized by these solutions.

Related to such researches as have just been considered is the recommendation of W. A. Tarr[3] for a fuller consideration of what becomes of ore-depositing solutions after they have accomplished their mineralization. He says:

"Studies in mineralization usually end with accounting for the deposit; of the minerals removed, of the possible changes induced elsewhere and what becomes of the solutions, little is said. Removal of overlying materials may handicap such studies, but does not justify neglecting them."

The following statement by Geo. W. Bain[4] touches on allied problems.

"The most impressive feature of mineral deposits is the characteristic enrichment of the mineral zone in silicon or alkalies or both. These elements appear most often in quartz and sericite, but also in a host of other minerals and they probably were a part of the solution which brought in the metallic ores. Questions which arise include:

1. Did their removal diminish the solubility of the ore minerals in the solution?

2. Were they precipitated in space produced by solution, and did the dissolved material precipitate the metallic minerals?

3. Would the metallic minerals have been precipitated even though no chemical change occurred in the solution? Was precipitation due to physical changes?

---

3) University of Missouri.
4) Amherst College.

These questions can be answered satisfactorily only when more complete data on equilibria at higher temperatures and pressures have been determined and for a wider range of substances than are now available in current chemical literature. Although I remain in thorough sympathy with a study by mining geologists of the structural control of the movements of mineralizing solutions, I favor this other study by mineralogists and geochemists."

Among other suggestions received that relate to ores of igneous affinities should be mentioned those of Alfred L. Anderson[5] for the critical study of thermal conditions during each stage of metallization in those deposits that have been subject to "reopening" or repeated mineralization and for a more detailed study and valuation of mineral thermal indicators, particularly under conditions simulating those under which ore deposition is believed to take place.

In the field of ore deposits related to the processes of gradation rather than of vulcanism, the suggestions received from two well-known students of the sedimentary iron ores are particularly significant. Says Ernest F. Burchard:[6]

"In the iron ore field more should be done in the interesting study of iron depositing bacteria following the work of Harder, although perhaps much may have been done that has not come to my attention. In my own observations of iron ore deposits in the Cretaceous and Tertiary formations of the Southern States I have been impressed with the apparently important rôle that greensand has played as a source of the iron. I believe that much study might be devoted to the nature, origin, and alteration of greensands and the climatic conditions under which such alterations take place. I should like to see more knowledge obtained as to the many ways in which limonite is deposited, and more of the details bared as to the deposition of oolitic hematite."

Of somewhat similar import are the recommendations of A. O. Hayes[7] with respect to studies not only on the sedimentary iron ores but on glauconite deposits.

"Further studies should be made of such ferruginous sediments as the Reagan sandstone of Oklahoma of Upper Cambrian age; the Neda iron ores of the Iron Range near Mayville, Wisconsin of Upper Ordovician age; the

---

5) University of Idaho.
6) U. S. Geological Survey.
7) Rutgers University.

Wabana iron ores of Newfoundland of Arenig age; the hematites of Cape Breton of Arenig age; the Ordovician and Silurian iron ores of Arisaig, Nova Scotia, which occur together in the same locality, and the Torbrooke iron ores near Middleton, Nova Scotia of Devonian age. All of these deposits offer special inducements for investigation. A continuous diamond drill core, representing about 1000' thickness of strata has been completed in the Wabana deposits in Newfoundland, and has not yet been carefully studied."

The origin of glauconite is important in its bearing on certain of the sedimentary iron ores. Hayes says:

"A continuation of the study of the origin of glauconite should be carried on coincidently with stratigraphic investigations. The glauconite deposits of the Upper Cretaceous and Tertiary sediments of New Jersey offer an opportunity which has not been sufficiently improved. The Upper Cretaceous-Tertiary boundary has not been definitely defined."

The editor of these notes has long been impressed with the rich rewards likely to flow from the study of the sedimentary iron ores of the Paleozoic with the aid of the more refined methods developed in recent years by the sedimentologists. In particular the detailed study of continuous samples across the ore-beds and into the bordering sediments. Also more complete studies of the paleogeography of the ore-beds and their associated sediments with a view to determining the degree of isolation of the seas in which the ores were deposited from access to the open ocean. Further studies of the fossil content of the ores with particular reference to dwarfing and other evidence of unusual environmental conditions are also needed. While the conditions of deposition of the pre-Cambrian sedimentary iron ores were in many respects unique, there can be little doubt that a fuller knowledge of the Paleozoic iron ores will materially aid in their interpretation.

A. O. Hayes cites the Torbrooke region of Nova Scotia as affording an especially fine opportunity to study the contact metamorphic effects of granitic intrusions on sedimentary iron ores. He says:

"The oolitic iron ores of this region, of Oriskany age, are composed of green iron silicates, probably chamosite and thuringite, hematite and other minerals in a fine grained sandy matrix with much quartz. Iron carbonates are also plentiful in some beds and the strata, both ferruginous and non-ferruginous, are replete with fossil spirifers and other Oriskany fossils. Granitic magma intruded these ferruginous sediments and the contact of the batholith with the sediments is excellently exposed in South Mountain, south of Torbrooke.

"The ferruginous minerals exhibit progressive metamorphism from magnetite at the contact to less and less magnetization away from the contact. The influence can be traced for half a mile or more on the surface. The vertical depths of the contact are not known.

"Since magnetization is found in the Reagan ferruginous deposits in Oklahoma, in the Neda deposits in Wisconsin, and these appear to be due to dynamic and static metamorphism and not to contact metamorphism, the Torbrooke region offers an opportunity to compare magnetization as an effect of batholithic invasion to the effect of slight folding under heavy load which is found in Oklahoma and Wisconsin.

With respect to the ores of aluminum E. C. Harder[8] is particularly competent to speak with authority and the following paragraph is quoted from his letter:

"While aluminum ores seem relatively simple in their structure and occurrence, they are, nevertheless, not without interest as a research problem. They are particularly interesting from the point of view of rock weathering, and a study of bauxite and laterite formation involves a wide field, including rock decomposition, soil formation and the chemistry of soil processes. It has even been suggested, with some justification, that bacteria might be instrumental in bauxitization and lateritization. An investigation into the solution and deposition of aluminum in relation to the formation of bauxite and laterite, therefore, appears to me to present a most interesting research problem involving, as it would, a range of geological, chemical and biological studies."

Petroleum

A tremendous amount of successful research has been done on the very practical problems of the conditions governing the migration of oil and its storage in the natural reservoirs from which it is recovered commercially. The problem of the origin of oil in the source beds is more largely of scientific interest and has received less attention. It is,

---

8) Aluminum Company of America, Montreal.

however, the most fundamental of all petroleum problems. It has been approached of recent years by Parker H. Trask through extensive studies of modern marine sediments. John L. Rich[9] emphasizes in the following words the importance of continuing and broadening studies of this sort and of supplementing them by laboratory experimentation.

"We have had a great deal of work done by Parker H. Trask on the organic content of sediments and sedimentary rocks. It seems to me that Trask has established the fact that oil is not present as oil in the rocks when they are deposited. He has also established the fact that most sedimentary rocks contain some organic matter which might be a source for oil, and others contain a great deal. What we need now is research on the processes by which organic matter buried in the sediments becomes converted into oil. Hawley and others at the University of Wisconsin have tried the effects of pressure and shearing with negative results. We need someone to try the effects of the combination of pressure and elevated temperatures ranging from a few degrees above surface temperatures, corresponding to depths of two or three thousand feet, to temperatures corresponding to the greatest depths that are probably reached by the sediments in our larger geosynclines. Pressures and temperatures should correspond to those encountered under natural conditions. By that I mean that at a certain depth the normal temperature gradient would give a certain temperature, and the pressure would correspond to the weight of a rock column of that depth. These temperatures and pressures should be used together in the experiments."

The natural sequel to studies such as Trask's on modern sediments is obviously the detailed study of sediments associated with petroleum in the younger geologic formations. Doubtless there are many regions where such studies can and should be prosecuted and one region that seems peculiarly promising is suggested by A. O. Hayes in the following paragraph.

"Following the investigations made by Parker H. Trask on modern sea-bottom deposits, it seems to me that an investigation of certain interbedded shales and chert deposits of Ecuador with which petroleum is associated, offers one of the best opportunities to study the relation of petroleum to such deposits. The stratigraphy is well revealed in westward-dipping rocks in the Chognon Hills, west of Guayaquill, and the formations are exposed again in folded and fractured condition at the west end of the Santa Elena Peninsula. An exhaustive study of this Tertiary accumulation of siliceous and argillaceous sediments should be fruitful in furthering our knowledge of the origin of petroleum and yield information as well on the conditions of chert deposition."

---

9) University of Cincinnati.

In discussions of genesis we should think and speak plurally of the origin of petroleums for the great variations observed in crude petroleums further complicate the problem of origin.

Turning from the problems of genesis to those more directly related to production, the recognition of regions potentially favorable or unfavorable for the commercial occurrence of petroleum is of fundamental importance. The mild diastrophism suited to the formation of structures available for the trapping of oil and gas may progress into a more severe type that will drive the fluids from the rocks. The importance of developing criteria for recognizing the degree of mild metamorphism fatal to oil occurrence was long ago recognized by David White when he proposed the use of the carbon ratios in coals as such an index. Though not without challengers, White's hypothesis seems to have proved its usefulness in coal-bearing regions. But much prospective oil territory is not coal-bearing and the development of <u>other</u> criteria of mild metamorphism is desirable for such regions. In this connection the suggestions offered by W. B. Wilson[10] are of interest.

"The carbon ratios of coals have long been and still are valuable guides to the degree of metamorphism that might be detrimental to oil prospects. I believe some geologists are disposed to belittle the evidence of carbon ratios in recent years, but they are still good, as far as I am concerned, and I know of no important oil fields where the fixed carbon is above 60. However, the usefulness of carbon ratios is limited to coal bearing areas. We need greatly a similar criterion for recognizing stages of metamorphism which can be obtained from shales. This would be a study that might very well require the assistance of chemists and physicists. My idea would be to select a single coal seam that shows considerable gradation in its carbon ratios. Detailed examination of samples of associated shales might very well yield most interesting and significant results. At the present time there is considerable discussion among geologists of the Mid-Continent as to oil possibilities of the Ouachita Mt. area of Oklahoma south of the Choctaw fault. Some consider that the western part of this area would not show high carbon ratios if coals were present, but there is little or no scientific basis for such conclusions."

---

10) Gypsy Division, Gulf Oil Corporation.

Somewhat similar implications are carried by the following suggestion of John L. Rich.

"Another research would be to take an organic shale, such as the Chattanooga shale, and determine the proportions of volatile organic consitituents and fixed carbon under a variety of conditions ranging from those of regions where the sediments could never have been deeply buried or have been subjected to dynamic metamorphism, through to those where the sediments have been deeply buried without dynamic metamorphism, and also to conditions where the sediments have been subjected to dynamic metamorphism with and without probable deep burial. Dr. David White once expressed the idea that a shale like the Chattanooga in undisturbed areas contains a considerable amount of organic matter which will be changed to oil on heating, but that the same shale in the mountainous regions, which is as black as before and appears the same to the eye, has lost its volatile matter. The inference which I would draw from such a fact is that the volatile matter has been converted into oil and gas in the process, and that a fixed carbon residue must remain. It seems entirely within the scope of research possibilities to check this matter in the way I have outlined."

Geo. C. Branner[11] also urges the importance of developing, if possible, criteria for determining the relative metamorphism of shales.

Whether oil and gas pools were in the main formed early or late in the diastrophic history of a region is a problem emphasized by M. G. Cheney[12] in the following paragraph.

"Unless I am seriously mistaken most oil accumulations are in structures which were formed early geologically speaking and most of the late structures are unproductive. This leads to the rejection of the theory of formation of oil and gas through the influence of heat and pressure, which theory had such eminent advocates as the late Dr. David White. A general report comparing productivity of early versus late folds throughout the world would be of much value. Explanation of the few exceptions in nature where oil occurs in structures which did not have early origin would be of interest. Also what percent of structures having late origins are productive of oil and what percent are productive of gas? There seems to be a probability of late readjustment of gas but not of oil. Of course the time of folding is revealed by the variation of thickness of sediments overlying the producing reservoir."

The mechanism of the transfer of oil, gas and water from source beds to reservoir beds is still an open problem to the solution of which the laboratory method of approach may contribute. Cheney suggests the

---

11) State Geologist of Arkansas.
12) Petroleum Geologist, Coleman, Texas.

need of experimentation in "the expulsion of fluids from compacting muds interbedded with porous layers simulating as nearly as possible the progressive changes due to increasing overburden which take place in geosynclinal areas. Differential pressures resulting from unequal thicknesses of overburden should be allowed for in these experiments, such differential being as much as 200 pounds per square inch per lateral mile. Such experiments should do much to prove or disprove the compaction theory as applied to the migration of oil or gas. I have discussed the importance of this subject as I see it on page 558, Vol. 13, Bulletin of the American Association of Petroleum Geologists. One of the main questions is whether most of the expelled fluid will migrate vertically or laterally under normal conditions of sedimentation. Results would help clear up the important question of distance of migration of oil and gas. This in turn has some bearing upon source beds and origin of oil and gas. Other important considerations are also involved such as time of migration, time of conversion of material to oil and gas and the process which causes conversion."

The quality and concentration of the waters associated with petroleum in oil pools has long been recognized as of significance in any attempt to determine the source of the oil. Particular interest attaches to those oil-field waters that closely resemble sea-water in the kinds and proportions of the salts present and in some cases are also closely similar to sea-water in concentration. In some of them that are deficient in sulphates, living sulphate-reducing bacteria have been found. Because they so closely resemble sea-water such waters are often referred to as connate, although there are obviously several alternative possibilities as to their origin. F. H. Lahee[13] suggests the need for further and more detailed studies of oil field waters in their bearing on the occurrence and origin of petroleum. If the term "connate" is restricted to those waters, regardless of composition or concentration, that became enclosed in the interstices of the sediment at the time and place of its deposition and have not since migrated far or undergone serious changes in composition

---

13) Sun Oil Company, Dallas, Texas.

then it is doubtful whether any of the oil field waters that the geologist deals with are strictly connate. To what extent are they originally connate waters that have migrated and undergone chemical and biochemical modifications? To what extent have they been introduced into the sediments long after their deposition? Some of them are almost identical with modern sea-water in spite of their occurrence in rocks that were deposited in ancient seas.

Lahee further suggests the need of studies in the relation of the chemical composition of underground waters to position on geologic structures. Some beginnings have already been made along this line. In short, the whole matter of oil-field waters requires further and more critical study.

In concluding the section on Petroleum mention should be made of the recent compilation by F. B. Plummer[14] of a list of research work in progress in the United States pertaining to some phase of petroleum geology. This mimeographed list was compiled for the Research Committee of the American Association of Petroleum Geologists under date of March 10, 1936.

Coal

In the field of Coal Geology no one is better qualified to summarize research opportunities than Gilbert H. Cady[15] and his letter on this subject is quoted below almost in toto.

"It is apparent to anyone who examines the present standing of coal geology in America that opportunities for pursuit of technical training in this branch of our science are very limited. The geologists now engaged in coal research are almost without exception doing such work in research laboratories of bureaus not closely associated with educational institutions. The laboratories at Pennsylvania State College are a notable exception. Education for technical work in coal geology outside of such laboratories is

---

1) The University of Texas.
2) Senior Geologist and Head of the Coal Division, Illinois Geological Survey.

essentially impossible. These laboratories are therefore forced to educate their own personnel, a system which is costly to the laboratories and unsatisfactory to the personnel because carried forward very largely under the actuation of expediency. A well supported program of academic research in one or more of our larger educational institutions would assist in the education of coal technicians in the geological field and stimulate an appreciation of the value of such research both academically and in the practical field. There is little doubt that scientists must lead the way by coal research toward a widening of the field of coal utilization. The failure of those in authority in the educational field to recognize the value of coal research to a society so greatly dependent upon coal for its existence and perpetuation is an educational anomaly difficult of understanding.

"Because of the failure to consider coal scientifically most of our knowledge in regard to coal has been acquired by empirical tests designed to meet particular needs related to the use and marketing of coal and have failed to provide fundamental facts upon which safe generalizations can be based. It is therefore impossible to interpret correctly much of the empirical data.

"There are a number of lines of invostigation in the nature of geochemical border-line studies.

1. Chemical petrology. Chemical studies in coal constitution are equally important with physical studies. An accurate understanding of coalification is probably possible only as the chemist and geologist work in cooperation. Similarly coal metamorphism is both a chemical and physical problem; the chemical studies should recognize the geological factors involved in the problem. This is essentially an unexplored field of investigation and research at the present time would be largely exploratory and academic in character.

2. Fuel studies. Chemical study of coal as a fuel rather than as a rock material is desirable particularly if such studies are fundamental rather than empirical in character. There is need for an understanding of coal as a fuel on some other basis than that provided by the proximate or ultimate analysis.

3. Coking studies. The coking phenomena as related to the physical constitution of coal has been explored to some extent by Dr. R. Thiessen. More comprehensive investigations along this line are desirable, physical and chemical phenomena being correlated.

4. Hydrogenation. The problem of hydrogenation and gasification of coal is one undoubtedly intimately related to coal constitution. The correlation of the factors involved is desirable.

"It is probably unnecessary to point out that items listed under 2, 3, and 4 above lie within or near the field of applied research, although there are certain fundamental facts involved only upon the basis of which applied research can proceed."

PROBLEMS OF THE PRE-CAMBRIAN

The following letter from Francis J. Pettijohn[1] is a unified and forceful - almost impassioned - plea for more active interest in the geology of the pre-Cambrian. The editor has not wished to weaken it by distributing its material under several captions.

"Why study the pre-Cambrian? The pre-Cambrian contains the oldest life record, it reveals best the process of ultrametamorphism, it contains most of the important mineral deposits (more than half the iron, 70-80 per cent of the gold, much copper, and nearly all the nickel) and on the basis of evidence in hand it is known to contain more cycles of sedimentation and diastrophism than all the later geologic records combined. In any event we may say, with Sederholm, that the pre-Cambrian 'far from being an inconsiderable appendix to the succession of fossiliferous rocks must contain the greater part of the geological record' and is therefore worthy of intensive study.

"Reason for lack of progress in pre-Cambrian research today. Unfortunately work today on the problems of the pre-Cambrian is lagging. There are still great gaps in our knowledge of the pre-Cambrian history. Much of what we now know will have to be rewritten according to new principles. The classification of the early pre-Cambrian remains to be worked out.

"To the writer it seems anomalous that such a complex area as the pre-Cambrian shield, lying at our back door as it were, is in the main neglected while geologists in neighboring regions dwell at length on minute problems. (A week's study in the pre-Cambrian calls for the exercise of more analytical judgment than ordinarily does a field season in the flat Paleozoics.) As Prof. Lawson has pointed out, 'this is due to the fact that for paleontologists and so-called historical geologists, geological history begins with the Cambrian. All that precedes the Cambrian is lumped together as the pre-Cambrian, a paleontologically uninteresting aggregate of rocks . . . . It is difficult for them to grasp the notion that the Cambrian is a relatively late period in geological history. And their interest in the subject is fundamentally biological rather than geological. This difficulty of comprehension is perpetuated through successive generations of geologists, since those afflicted with it are themselves the teachers of geology and steadfastly inculcate the doctrine that all geology is divided into three parts: pre-Cambrian, Cambrian to end of Tertiary, and Pleistocene, and that the first and third are but unimportant prologue and epilogue to the second, which thus becomes the whole drama.'

"It is quite true that a considerable number of geologists are engaged in field studies in the pre-Cambrian shield. These workers are

---

1) University of Chicago.

either consulting geologists engaged in economic work or members of the several governmental surveys under whose jurisdiction the pre-Cambrian shield falls. But as also pointed out by Prof. Lawson:

"'In all geological survey work attention is necessarily directed primarily to areal mapping; but geological maps and the sections constructed from them do not always reveal geological history. The elucidation of time relationships requires an intensive and critical study of spacial relations in the field, which can be given only by enthusiastic and capable geologists. A field in which such relationships have been deciphered at available exposures yields much more to geological science than a mere areal map, accompanied by perfunctory sections. I do not deprecate maps; but I am of the opinion that the wholesale production of maps, in which the energies and interest of the field geologist are absorbed in covering paper with color, rather than in studying the relationships of the rocks, contributes comparatively little to geology. The chief, perhaps the only, excuse for this excessive devotion to covering areas of paper with color is the demand of the prospector and miner. There can be no objection to meeting this economic demand; but that is not the whole function of the geologist. There is enough enlightened sentiment in North American communities to support, in addition to that, a large amount of purely scientific work, having for its purpose the elucidation of geological history. Moreover, the more familiar we become with the detailed sequences of events in pre-Cambrian history the more readily shall we be able to solve its economic problems.'

"Probable future trends. Sederholm said (Chicago, 1933) it is probable 'that in the field of tectonic geology the greatest progress will be made in the future. The methods of studying the tectonics and of drawing sections of pre-Cambrian rocks have long been crude.' Such studies must necessarily be detailed and proceed at a slow pace. Only by such patient and slow building of the details will a secure foundation be laid for the larger generalizations. Studies of this type are indeed rare today.

"Also as Sederholm has said, 'Geologists of pre-Cambrian inclinations need the aid of geophysics just as much as geologists prospecting for ores need the assistance of the electrical engineer. But there ought to be collaboration. Especially is it necessary that the material investigated by a geochemist or geophysicist is collected by geologists possessing a thorough knowledge of the region in question.'

"'Above all, however, it is necessary to facilitate the work of those younger geologists who, in the near future, will continue the work of the older generation as it gradually leaves the stage.'

"A program for today. As a specific program to meet the above criticisms of Lawson and Sederholm I suggest:

"(1) Special encouragement to properly trained geologists to make detailed petrographic and structural studies in the pre-Cambrian, preferably in well-chosen 'key areas.' Dr. Collins has for many years had a

number of such areas in mind - thesis areas. The writer can list some half dozen or more in northwestern Ontario.

"(2) Preparation of bibliographies and digestions of earlier literature for the various pre-Cambrian subprovinces (as has been done by Leith, Lund and Leith in Professional Paper 184 for the American portion of the Lake Superior region).

"(3) Formation of a committee on the pre-Cambrian to focus attention of workers on its petrographic, stratigraphic and structural problems and to handle editorially bibliographies (No. 2).

"(4) Cooperative geochemical and geophysical work on pre-Cambrian problems such as:

(a) iron ores and iron formations.
(b) varved phyllites and slates of the early pre-Cambrian.
(c) graphitic carbon content of pre-Cambrian sediments.
(d) problem of palingenesis and batholithic invasion.
(e) study of P and S content of pre-Cambrian rocks.
(f) petrographic study of the greywackes, their classification and significance.
(g) regional petrographic and chemical study of the 'late pre-Cambrian' dikes (one over 80 miles long in the Rouyn, Quebec, area).
(h) study of genesis and structural control of gold ores.

"(5) Field work (petrographic and structural) on:

(a) Coutchiching problem and the related
(b) Keewatin problem (one or two periods of widespread volcanism?) (check geological evidence by helium method of age determination).
(c) the problem of the granites. How many ages of? How distinguished from one another? (to be studied by means of minor accessory minerals in part).
(d) the Grenville problem.
(e) the Steeprock problem (relative age and position of the Steeprock and Seine series).
(f) the problem of the stratigraphic position and age of the Sudbury series.

"(6) Another possible way in which pre-Cambrian research would be greatly furthered is to somewhat institutionalize these studies much as Princeton has succeeded in doing in the Red Lodge area of Montana and Wyoming. This will serve to direct the attention of structural geologists, petrologists and economic geologists to the major problems in their science and to the pre-Cambrian. Chicago, Wisconsin, Minnesota and perhaps Northwestern might cooperate to advantage with each other and their Canadian colleagues. Such a connection between educational institutions and the research would, as Sederholm put it, 'facilitate the work of those younger geologists who will continue the work of the older generation as it gradually leaves the stage.' The cooperation of local chambers of commerce, the institutions concerned and others ought not to make establishment of a base camp either difficult or costly."

GEOMORPHOLOGY

While relatively few geologists have forwarded suggestions in this field, some of those received are of much interest.

Apropos the very live current interest in soil conservation Geo. B. Cressey[1] writes:

"One geological field which merits added attention is geomorphology, within which correlated research is needed on problems of slope wash and the type of farm land erosion now being studied by the Soil Conservation Service. Geologists surely have a contribution here. I believe there might be a committee on erosion which would examine both the micro- as well as the megascopic aspects. In addition to slope wash they might consider the German concepts of Penck and others on slope inclination."

Bearing also on problems of soil conservation is the recommendation of Richard J. Russell[2] that "Geomorphologic studies should give greater weight to gentle slopes. The reference frame has too long involved idealistic concepts. Surface history is more significant than stream history. We need a breed of geomorphologists who really understand soils."

The following recommendations by Stephen S. Visher[3] are of broad implication.

"It is highly desirable that more work be done in discovering and adequately presenting regional contrasts with respect to the 'fundamental' factors of climate, natural vegetation, soil, topography, and chief resources. Fenneman's work on physiographic regions is good so far as it goes, and much preliminary work has been done on climatic, ecological, soil and resources regions, but it seems to me that so much remains to be done that persons seriously undertaking to improve our knowledge of regional contrasts in these respects should be encouraged. The existing regional climatic maps are too largely based on arithmetical averages, ignoring the departures, and hence fail to give a correct picture of the regional contrasts. Likewise the ecological maps are too generalized and the soil maps either too generalized or else so detailed as not to be very serviceable. The natural resources maps practically all deal with only single items, and for them only poorly.

"A problem distinctly between the recognized sciences is a study of the influence of climatic and ecological contrasts upon physiography; for example, upon erosion. It seems to me that studies along this line by competent men might yield results of notable significance to physiography

---

1) Syracuse University.
2) Louisiana State University.
3) Indiana University.

and geography. Not only might the various physiographies be better understood but light might be thrown upon the geologic past."

The need of more quantitative knowledge of the rate of weathering as an aid in the interpretation of physiographic forms is emphasized by Frank J. Wright.[4]

"In attacking physiographic problems, we are continually coming up against the proposition of the relative rate of weathering and erosion of different minerals and rocks. A single formation, or a group of rocks mapped as a unit, may exhibit marked variations in resistance to weathering agencies.

"I should like to suggest a thorough study of this problem in a region underlain by diverse types of rocks where the climate is reasonably uniform. A portion of the Tennessee River basin might be found suitable for such a study. Streams such as the French Broad, Little Tennessee, and Hiwassee drain areas of different types of crystalline rocks, while the main stem of the Tennessee between Knoxville, Tennessee, and Chattanooga, Tennessee, flows through an area of Paleozoic sediments of relatively low resistance. The study would involve physiographic and petrographic work."

The following comment by a paleontologist, Bruce L. Clark,[5] on research in physiography on the Pacific Coast holds a challenge which may be taken up by the physiographer.

"Very little research is being done on the physiography of the west coast. In my opinion and in that of other men who are competent to judge, a large proportion of the physiography which has been written in the past will have to be done over. Physiography is so intimately connected with structural geology that it is my opinion that it must be the structural geologist who works out the problem rather than one who is trained purely in physiography."

R. J. Russell contributes the following additional suggestions in this field of study.

"(a) Shore forms deserve further study in the field. The method of cataloging forms deductively and using the field only for examples, or more often substituting a map for the field, is barren. I am pretty well convinced that the deductive method most commonly used happened to get the wrong take-off in assuming that the fundamental classes are shore-lines of submergence or emergence. Possibly the starting point should be those of advance and retreat. The two are not the same.

---

4) Denison University.
5) University of California.

"(b) Is there a fundamental reason for the association of flat topography with steppe climate? Are the 'Pampas depressions' of Argentina duplicated in the high plains of the United States? Are there two types of depressions in the Staked Plains, one associated with Pleistocene climates, the other with wind erosion? I think so. One type truncates soil profiles, the other is conformable with them. The problem should be settled south of the Canadian River of northern Texas."

Aspects of geomorphology that integrate with astronomy are touched upon by A. K. Lobeck.[6]

"Astronomical aspects needing attention are a) the effect of Ferrel's law on streams, and b) the influence of the moon in producing strains and fractures in the earth's crust. This is a borderland topic and might yield some sane results in the hands of a student thoroughly trained in astronomy and geology."

F. F. Grout[7] stresses in the following paragraph the importance of more searching studies of peneplanation in Pleistocene and recent time as a key to the interpretation of evidences of more ancient peneplanation. He says:

"The great need of geology is for a study of paleophysiography. The evidence of old peneplains in the geologic column is fairly acceptable, but more details are needed. And the whole idea may be largely upset by the evidences being presented for common rather frequent oscillations of the crust or of sea level. The best prospect for conclusive data is in the recent geologic past. If the oscillations of crust or sea in Pleistocene or Recent time cannot be agreed upon it is almost hopeless to attempt a study of older crust movements. If they can be agreed upon we'll have a basis for dynamic geology with bearing on a wide variety of problems - on the inside of the earth, on glaciation, on orogeny, on batholithic invasion, etc., all of which are more or less in suspense for lack of certainty as to up and down movements of the crust."

---

6) Columbia University.
7) University of Minnesota.

GLACIATION AND THE PLEISTOCENE

The desirability of correlating phenomena of stream erosion well south of the limits of the Pleistocene ice caps with the various stages of glacial history is stressed by E. H. Sellards[1] as follows:

"I have been particularly attracted in recent years to problems associated with the development of stream terrace deposits of the southwestern part of the United States. During late Cenozoic times these streams have been in the main cutting downwards with the result that a fine record has been left in the form of successive terraces and terrace remnants. These streams in the main do not reach the glaciated part of the United States. It seems to me, however, that it will be entirely practicable to tie the terrace development of these streams into other streams which do reach the glaciated area and thus tie the terrace history of the streams of the Southwest into the glacial history of the North and Northeast. This problem, of course, necessitates many workers. We are accomplishing something in the Texas region, but the funds under which we work do not permit of carrying the work into other states. A group of workers could very well, I believe, correlate their work in such way as to carry this problem to a successful conclusion."

Suggestions offered by A. K. Lobeck[2] are of somewhat similar purport. He recommends researches in "the effect of the Glacial Period in non-glaciated regions, such as the greater humidity and its results in the Great Basin; river terraces along such rivers as the Ohio and Mississippi. Every student knows the ramifications of this topic but it remains for some concerted study and general correlation to be made. Changes of sea-level, marine benches, etc., all are related."

Stephen Taber[3] points out the excellent opportunities for studies in Pleistocene history in a critical region. He says:

"The non-glaciated parts of Alaska and the Yukon Territory afford a unique opportunity for study of Quaternary History at the present time because of the excellent exposures in the large placer mines where thick deposits of Pleistocene gravel, silt, and peat are being thawed and removed. This investigation requires the cooperation of geologists, physiographers, botanists, zoologists and anthropologists. Migration from Asia to North America probably passed through this region, and the frozen

---

1) University of Texas.
2) Columbia University.
3) University of South Carolina.

Quaternary deposits contain numerous well-preserved fossils of animals and plants. They also contain evidence of climatic changes occurring during the Quaternary."

Robt. W. Sayles[4] stresses the desirability of further studies of the botanical features of peat deposits as an aid to the understanding of glacial and post-glacial climates.

"It is more and more evident that there must be a closer relation between Glacial Geology and Botany. I refer to the importance of the study of peat and pollen in the study of glacial and post-glacial climates. They have done so much more of this in Europe than has been done in America. If there is any way in which these two could be linked up together I believe the advance in our knowledge would be greatly accelerated. I am trying to do this at Harvard."

Relating more specifically to the ice caps themselves is the problem suggested by Geo. H. Chadwick.[5]

"One of the most desirable matters to clear up, at the present time, is the question of the 'peripheral bulge' surrounding the continental ice-sheets, an idea slow of acceptance in America. Only a very discriminating, discerning, and intellectually honest as well as alert student should tackle this problem. There are no end of isolated puzzling phenomena connected with the pre-glacial and post-glacial deposits in our Hudson and Champlain valleys, inexplicable under the old rigid uplift idea that seem to me to have a direct bearing on the presence and behavior of this bulge. I shall be glad to cooperate by personal conference (not by correspondence) with any proper student who essays this problem."

---

4) Harvard University.
5) National Park Service.

METEOROLOGY AND CLIMATOLOGY

Climatology comes in for incidental consideration in connection with glaciation and with a number of the suggestions received from geographers. The only communication dealing entirely with these fields is that of Charles F. Brooks of the Blue Hill Meteorological Observatory of Harvard University. This letter is of such scope and interest that it is quoted in entirety below.

"I am pleased to submit a list of what I consider worthwhile in lines of investigation in meteorology and climatology.

I. IN METEOROLOGY

1. Local factors in weather forecasting. This would be a study of the weather records at the locality where one resides, and should show the normal sequences and the frequencies of different phenomena in the different seasons. For example, in Boston the sea breeze is a very common phenomenon in May and June, and will occur under almost any weather conditions when the sun is shining and the wind is not more than ten miles an hour in an off-shore direction. Recently Boston had a maximum temperature of 70 on a day when it was 85 in Milton. Anyone forecasting for Boston must take note of this local peculiarity.

2. Atmospheric cross-sections and weather maps for different levels aloft. These cross-sections can now be made on the basis of airplane weather ascents, observations at mountain stations and radio-meteorograph sounding balloon ascents. When data become available in greater quantities it will be possible to make many investigations of the structure of general storms, and probably to apply the results in improving weather forecasting.

3. How observations at mountain stations may best be used for aerological purposes and consequent forecasting. Investigations of this sort have been made recently with respect to Mount Washington, but need to be carried farther. The mountain increases the wind velocity and decreases the temperature of the passing air. It also modifies the rainfall. These local influences, however, can be allowed for.

4. Spectral disposition of sunlight. Without having to go into details available by means of a spectro-heliograph it is possible by the use of colored screens to obtain the proportions of solar radiation. These proportions vary with the dust and water vapor contained in the air, and can be used, therefore, to determine both of these elements.

5. A synoptic study of the distribution of ozone. The amount of ozone in the atmosphere, as shown by ultra-violet measurements on the earth's surface, varies considerably in different parts of high and low pressure areas. If the instruments can be obtained the data from them would be of interest. So little is known now about why the ozone varies in different parts of low and high pressure areas that it is difficult to say just what a study of this sort would reveal which would be of more than scientific interest.

6. Effects of weather on radio transmission at different frequencies. It appears from various studies that the condition of the lower atmosphere has much to do with the transmission of radio signals, especially those of ultra-high frequency. Such a study would be of interest from two points of view. One simply from the radio standpoint, the other from the possibilities of weather forecasting from variations in radio transmission.

7. Detailed investigations of extreme occurrences, for example, floods and droughts. By finding out why variations in our weather take place we may thereby learn how to anticipate them.

8. Variations of ocean temperature and seasonal weather. Since the oceans occupy more than 7/10 of the surface of the globe, there are variations in temperature effecting this portion of the earth's atmosphere. It is supposed, therefore, that departures of ocean surface temperatures from the normal are involved in the production of season abnormalities.

9. Elements in long range weather forecasting. The deviations of seasonal weather are apparently the combined result of many factors, each of which needs to be considered before reasonably successful forecasts can be made at all times. There are ocean temperatures; there is the state of ice in the Arctic; there is the condition of general temperature in the Antarctic; and there are variations in solar radiation, which may be the initiating impulse for all the other charges.

II. CLIMATOLOGY

10. Evaluation of climate in terms of human comfort and health. Enough is now known of climatic effects on health and comfort to make possible the numerical evaluation of climates from these standpoints.

11. Local differences in climate. Our maps of climate show the differences over wide areas. The size of local differences in terms of these general differences is much larger than is appreciated. Therefore, climatic maps, showing the limits within which local climates differ from the general climate should be constructed.

III. HYDROLOGY AND CLIMATOLOGY

12. Water content of snow on the ground. The amount of water locked up in a snow cover is available merely in the form of run-off during the spring or early summer months. A warm spell may unlock this water suddenly as in New England last March.

13. Proportion of the year's precipitation, which falls in the form of snow. The water which comes down in rain is immediately disposed of. That which falls as snow may or may not disappear from the surface at once. An important measure of the precipitation element in climate is, therefore, the proportion of the annual precipitation which comes to the earth in solid form.

"I should be glad to discuss any of these points in detail with any investigator who may be interested.

PALEOBOTANY

In the following communication Ralph W. Chaney[1] has outlined some of the research opportunities in the field of paleobotany, with special reference to the Tertiary. His communication should be of interest not alone to paleobotanists, but to all workers in the paleontologic field as it emphasizes a point of view which is designed to combine stratigraphic evidence, both organic and inorganic, in an effort to refine present methods of determining geologic age.

"1. Within recent years investigations of Cenozoic plant life have become so intimately concerned with distributional problems that any areal gaps in our knowledge prevent accurate interpretation or general understanding of the data now at hand. Up to the present time the studies of Professor E. W. Berry in the Atlantic Coast and Gulf Coast provinces, the studies of Dr. R. W. Brown on certain older Tertiary floras of the Rocky Mountain province, and the studies carried on by me and my students in the Pacific Coast and Great Basin provinces, have given reasonably adequate information regarding the development of Cenozoic vegetation in the United States. The extremely important problem relating to the Cretaceous floras of the Rocky Mountain area has recently been assumed by Dr. Erling Dorf, under the auspices of the Carnegie Institution of Washington and Princeton University. The decision of the University of Colorado to add to its staff an instructor in paleobotany will do much to further the study of later Tertiary floras in the Rocky Mountain area. There remain, however, the following critical gaps:

(a) The Tertiary of Alaska. - Although a professional paper is shortly to be published on this subject by the United States Geological Survey, much is still to be learned of the stratigraphy and floristics of the Kenai and younger floras. A start is to be made by me on this extensive problem next month.

(b) The Cretaceous and Tertiary of Canada. - For more than thirty years the Cretaceous and Tertiary floras of western Canada have received no attention. All of the earlier work was of a preliminary sort. No adequate conclusions regarding forest migrations from northern centers into the United States and Central America can be drawn without a detailed survey of this critical intervening area. I have previously suggested to Dr. Collins that he add a Tertiary paleobotanist to his staff, but up to the present time financial considerations have made this impossible.

---

1) University of California.

(c) The Pleistocene. - With the exception of California and of limited areas east of the Rockies, little information is now available regarding the vegetation and floral sequence of the Pleistocene. While deposits bearing leaves and fruits are relatively rare in the Pleistocene of the eastern United States, wood and pollen will yield data of great stratigraphic and floristic value. At a time when consideration of climatic change and erosion is foremost among the problems of agriculturalists and economists, it is unnecessary to emphasize the need of further knowledge regarding the vegetation of the immediate past.

"2. This suggestion involves the application of a point of view to paleontologic investigations. This point of view is not new, but it is so regularly disregarded by geologists and paleontologists, as to result in great confusion in age determinations. While determination of the age of rock layers is not the ultimate objective in studies of earth history, it necessarily makes up a large part of the work carried on by our group. Many students have of necessity a point of view based upon the use of only one line of evidence. Rather than discuss the undesirability of such a limited horizon, I may merely mention that a vast amount of controversy has arisen because of differences in opinion based on divergent lines of evidence.

"Any study involving the time element must depend upon recognizable trends. These are primarily based upon stratigraphic sequence; there has been developed and fitted to the stratigraphic column an additional sequence based on phylogeny. Since in many cases, especially in the study of Tertiary paleobotany, neither of these modes of approach are applicable, it has been necessary to add a third trend based upon changes of distribution in time. Relative age of rock layers is in most cases the major consideration, but a geologist is also expected to match the stratigraphic units of one area with those of another, and long-range correlations become a practical necessity. It is with these that the distributional trend is most concerned, particularly where differences in latitude or in position with relation to climatic barriers are involved.

"A case in point is the occurrence in the Eocene of Alaska of a temperate flora made up of Sequoia and broad-leafed deciduous trees. Due to a progressive trend toward cooling and drying, which resulted from the gradual emergence of western North America during the Cenozoic, this flora migrated southward into Oregon and adjacent states by Miocene time. Subsequently it has been restricted in part to the coast of California, to favorable habitats in the Cordillera south into Mexico, and in greatly modified form to the eastern United States and northeastern Asia. Similar changes in distribution have caused the southward migration of the subtropical flora which occupied the latitude of Oregon during the Eocene, and which now is found at low altitudes in Mexico and Central America. This Eocene flora, as represented near the Pacific Coast, is much more subtropical in aspect than that of the Rocky Mountain interior province. The barrier imposed by the Rockies resulted in a climate more continental and vegetation more temperate than that farther west adjacent to the ocean. To determine the age of any one of these fossil floras, it is necessary to

know the latitude of the occurrence, and the longitude in relation to then-existing topographic and climatic barriers. A flora finds its place in the time sequence as Eocene or Miocene in part on the basis of its constituents; but in addition its distribution is of critical significance in dating.

"The application of this idea of a distributional trend may be more sound in the case of plants than animals, since the latter are considered to migrate more rapidly. However, the cause of migration appears not to be a natural urge to wander, but rather the result of compelling forces in the environment. And since all animals depend either directly or indirectly upon plants for food, their movements in the past, especially upon the land, may be supposed to have been largely controlled by changes in vegetation as they are today.

"Disagreements are not rare among vertebrate paleontologists as to the relative age of certain genera of mammals from continent to continent. Invertebrate paleontologists express varying opinions as to the position in the stratigraphic column of marine faunas involved in a wide advance of the sea over a continental interior. It seems possible that inconsistencies and discrepancies in their results may be due to their failure regularly to recognize the factor of distribution in time.

"It is here suggested that stratigraphic paleontologists be urged to examine the sequence of fossils with the consideration of their space relations always in mind. The assumption of contemporaneity of two similar floras or faunas may involve discrepancies which will confuse their time relationship, if they lived in widely different habitats. The assumption of cosmopolitan distribution of floras and faunas can seldom do more than group too great a span of time under one heading, and obscure the sequence of physical events on which the stratigraphic column is based."

W. C. Toepelman[2] suggests the following lines of research within the Rocky Mountain field.

"(1) Studies of the Florissant floras and their ecological significance.

(2) The Creede volcanic ash floras and their relationships to the Florissant.

(3) A complete review of the "Laramie" and related floras of the Denver basin. This would require rather extensive field work to check stratigraphic position of collections and to study the stratigraphic sections."

---

2) University of Colorado.

PALEONTOLOGY

Edited by

Carl O. Dunbar

General Viewpoints

The most significant observations in the field of paleontology relate not to particular problems but to the general plan of attack. In its growth, paleontology, like other sciences, has passed through a series of stages, each characterized by a dominant motif and a general pattern of research. During the last stage, paleontology has been dominated by stratigraphic interests and to a large extent devoted to stratigraphic correlation. As J. B. Knight observes:

"The attack has usually been on faunas; the paleontologist, if he specialized at all, has been usually a specialist in the entire fauna of some time unit. The specialization has been stratigraphic or horizontal.

"I have no quarrel with the objective of stratigraphic correlation through paleontology. It is not the only value in paleontology, by no means, but it is the principal practical application of paleontology.

"It is my contention that paleontology in all of its aspects, including stratigraphic correlation, will be better served if the inevitable specialization be vertical rather than horizontal, that is, if the emphasis in research be placed on a detailed knowledge of one or a few zoological groups throughout their entire geologic history rather than on all or nearly all groups in a restricted division of geological time.

"Horizontal specializations require a detailed and deep knowledge of all, or a large part of, the animal kingdom--something wholly beyond the capacity of most of us in these days, a task far more difficult than the detailed knowledge of one or a few groups over all geologic time.

"It is on precise paleontology that we must rely for the more precise stratigraphic correlations that are required today. Precise paleontology cannot be done by men who are primarily stratigraphers or by men who are compelled to cover a field so wide, zoologically speaking, that they cannot hope to acquire an intimate knowledge of any of the zoological

groups with which they deal. It is the work of a lifetime to acquire the necessary detailed knowledge in any one zoological group of importance. And it is just this intensive knowledge that must be acquired if paleontology is to advance and become of greater usefulness."

The same thought is expressed by Paul Bartsch in these words:

"In paleontology the greatest need today is the monographing of not faunas, but families. The men engaged in faunal work rarely, yes, almost never, have had the experience of passing in review and studying species in their relation to each other. Each worker, as a rule, devotes his study to a circumscribed local problem, and his viewpoint becomes sadly colored by the local atmosphere. So true is this that as a rule, viewed from a systematic standpoint, the deductions thus produced are not meshable with similar endeavors, the personal equations in each instance presenting an undetermined denominator that future comparative studies alone will reveal.

"The studies in paleontology that I would urge, therefore, are the monographing of the various groups of organisms from their earliest inception to the living faunas. In this way the same individual would evaluate the members of the groups in question and for that group we would have only one personal error (?) involved."

The desirability of specialization along vertical rather than horizontal lines is also echoed by G. G. Simpson, F. B. Loomis, Rudolf Ruedemann, C. E. Weaver, and Marvin Weller. Weller contributes this comment:

"The field of paleontology seems inexhaustible. The great need now is for studies conducted on a zoological basis rather than a faunistic one. Facts regarding the evolution of the different groups of animals and their interrelationships and more detailed correlations require this type of work. Paleontology at first was little more than species-making. Then came faunal studies. This step was an advance but it resulted in many mistakes which are still largely uncorrected. It is impossible to recognize a guide fossil unless it can be distinguished from its near relatives, and such distinctions cannot be made by the study of a fauna here and there. The third and final stage of paleontology into which we are just beginning to pass involves the very careful study and comparison of large numbers of related forms, such as a family, from as many horizons and localities as possible. I think that all students should have impressed upon them the great importance of such studies."

It is also evident that we have attained a stage in the development of our science in which there is pressing need for comprehensive syntheses that require coöperative work. Such particularly deserve the encouragement and support of research institutions. Ruedemann writes:

"It is my impression that the first work in any newly-opened field is best done by individual effort, but that when a mass of data and divergent opinions has been produced, team work will produce the best results."

Writing of vertebrate paleontology, G. G. Simpson says:

"The greatest need in this general field seems to me now to be for synthesis and revision. Certainly a vast amount yet remains to be discovered and analyzed, but work of that sort is now actively going forward and seems to require little special stimulation or organization. What are needed and less likely to be forthcoming without special thought and cöoperation are general reviews and careful systematic revisions and coördination of the mass of scattered data.

"As a concrete example of what I have in mind and one which would perhaps be the best point of attack, it would now be practicable to undertake a revision and synthesis of the Paleocene and Eocene life of North America. Ideally, this would be under the guidance of one man or a small committee for coördination and direction, and the various parts of the revision would be assigned to the specialist most competent to handle each phase of the problem. The field covered should eventually be all of the life of those epochs, plants, invertebrates and vertebrates, and they should be studied not only as isolated groups but also in their relationships to each other. The aim would be to winnow out and record all that is now correctly known, and finally to show the life of that time as a whole, combining into a balanced and well-lit picture the haphazard and isolated observations that have been made."

Comparable schemes for comprehensive synthetic studies in other aspects of paleontology have been suggested. For example, E. C. Case, Charles E. Decker and Rudolf Ruedemann have each recommended the selection of a circumscribed area of which the paleontology and stratigraphy should be studied systematically from every possible point of view in order to show how complete a picture of its geological history it is possible to attain. This obviously would require the coöperation of many specialists.

Ecology

The influence of the environment on the distribution and evolution of life throughout geologic time is being realized with increasing force by many paleontologists. Two types of problems in this field are suggested,

first the study of the ecology of modern forms with especial relation to the bottom sediments in which they will be preserved as fossils and, second, a study of the relation of fossil faunas and floras to the stratigraphy and paleogeography with which they are associated. Since the interpretation of the environments of previous ages must be largely in terms of what may be learned from the ecology of modern faunas and floras, there is pressing need of observations on the modern shallow sea floor. W. P. Woodring recommends:

"A detailed investigation extending over a period of several years of shallow-water and moderately deep-water marine bottoms, selected to represent different environments, to determine the kinds of sediments deposited under different conditions and the animal communities associated with them; the effects of minor catastrophes, such as severe storms, heavy floods, and sudden changes in oceanographic equilibria; and the conditions determining the burial or non-burial of the animal communities."

Paul Bartsch urges the same need and offers two practical illustrations.

"In studying the faunas of the West Indies I was impressed by the fact that it was easily possible for me to recognize most of the littoral forms that occurred on the various blocks of that chain. Block-faulting resulting in deep channels across which larval forms were apparently unable to move, plus inbreeding, seemed the usual explanation of why the local environment should have produced certain distinctive features on each of these blocks, but the thing that puzzled me at the time that I made these observations was the fact that the forms dwelling on the Gulf or Caribbean side of these blocks were again distinguishable from those on the Atlantic side. That seemed strange to me at the time because it would stand to reason that these creatures should move along the shore line and furnish a belt around these island masses. It was not until the Atlantis made its hydrographic survey of part of the region in question that the factors responsible for this differentiation became apparent, for the Atlantis' researches showed a marked difference in the hydrogen ion concentration in the Gulf and Caribbean waters compared with those of the Atlantic side, which is Atlantic water.

"With such information at hand it is easy to interpret biological phenomena, and one of the things that we need is more extensive investigation of the physical and chemical conditions of the existing seas, particularly those bordering the land areas where paleontological records are being made daily, for this information will help us to interpret the facts that we find recorded in our geological history.

"At the Chicago meeting of the Paleontological Society I called attention to the paleontologist's need of experience in ecological studies of recent faunas. I pointed out that we had 22 distinct faunal associations represented in the lower tip of Florida between the Everglades and the Pourtales plateau. I wish here to call attention to the fact that we find here Halimeda and Porites reefs or flats in Biscayne Bay separated by leads almost narrow enough to jump across, with their associated elements, forming huge patches with features so distinctive that it scarcely seems possible that there should be so closely approximated or synchronous. From a paleontological standpoint I am sure had we these associations fossil, without knowing something of the existing faunal associations, we would be inclined to assign a different time element to these associations. What I have said of these two holds good for the other 20 faunal associations in this region. We therefore need badly an ecological survey of the various parts of our coasts with the view of faunal associations in mind, for that in turn will furnish elements for comparison and interpretation of things that existed in the days gone by."

Norman L. Thomas points out that the changes in faunas with increasing distance from the strand lines and with increasing depth offshore has a practical importance in commercial work, since the faunas of outcrops may be quite different from those contemporaneous horizons underground which were deposited under different environmental conditions, thus complicating the problem of correlation. W. H. Twenhofel also considers the study of modern marine ecology of the first importance for paleontology and stratigraphy. He states:

"The biologists are fully cognizant of the influence of the environment upon the organisms, but stratigraphers have learned little from them and the concept of the environment which geologists entertain are commonly almost entirely assumed. Our knowledge of the ancient sediments from the point of view of environment must be learned from the study of modern sediments, and little work has been done in this field. Studies of ecology are fundamental to sound stratigraphy and sedimentation."

C. E. Weaver and W. P. Woodring have both emphasized the need of a somewhat related study in modern zoology, namely the duration of the mero-planktonic stage of the young benthonic organisms. Weaver recommends:

"Biologic investigation of the embryos of shallow-water types of marine invertebrate life with experiments on the ability of such embryos to withstand the physical conditions which would be encountered while crossing broad expanses of water in the paths of ocean currents, such as from

Africa to South America within the tropics, or from eastern Asia to the western coast of North America through the colder waters of the northern Pacific. Information of scientific value concerning the migration of invertebrate faunas during Jurassic and Cretaceous from Europe into the Andean trough of South America must to a large extent be dependent upon such purely biological investigations."

It may be remarked that a direct paleontological attack may be made on the problem of the influence of bottom ecology through a comprehensive study of the faunas of a single limited formation, provided it is studied over an extensive area and its faunas carefully collected and analyzed so as to show variation from place to place and their relations to changes of lithology and sedimentary structures.

Bibliographic Aid

The rapid growth of paleontological literature and the ever-increasing number of genera and species described make us more and more dependent upon bibliographic aid and comprehensive systematic treatises which are largely beyond the scope of individual effort and require the support, both administrative and financial, of research institutions. A number of the contributors to this report have indicated the great need of additional bibliographic aid of various sorts. Kindle makes the following comment:

"One of the most urgent needs in paleontology is research leading to a stabilization of nomenclature of fossils and the integration of the descriptions and figures of fossils published in hundreds of different publications. Independent and unco-ordinated work in the fields of systematic paleontology has resulted in great numbers of synonyms, and the association of specific names with genera in which as the result of erection of new genera or for other reasons they no longer belong. The synonymy problem of the fossils of some of the geologic systems has in the past been dealt with by bibliographic indices such as Bassler's Bibliographic Index of American Ordovician and Silurian Fossils.

"In 1931 the Paleontological Society sponsored a new plan of dealing with such problems. It undertook for the Devonian invertebrate fossils to deal through a committee with the synonymy and the problem of

making the literature more accessible and usable. As chairman of this committee I have enlisted the collaboration of about 20 paleontologists. Each collaborator will have in hand two associated pieces of work. One of these, a bibliographic index, will give synonymy, geologic range and geologic distribution. Along with this bibliographic index will go a set of cards published independently with figures and descriptions of all North American units. The cards are issued in unbound units which permit the subscriber to shift species the genera of which may arise in the future through the splitting up of old genera, and to add new species as they appear in their biological order if he wishes to. This project is cited here as an example of co-ordinated co-operative research under committee direction which is expected to complete a project so large that no single worker could have hoped to finish it in a lifetime."

Charles Schuchert also urges support for the completion of Kindle's card catalog and adds:

"The local stratigrapher and paleontologist, far more than those situated within the reach of library facilities, needs bibliographic catalogues like the Weller index of 1898 and the Schuchert index of 1897. Such are already in hand as a labor of love but need to be completed before they can be printed."

E. H. Sellards urges the need of special bibliographic aid for micro-paleontology in the following words:

"A large number of workers are now engaged in the study of microfossils, particularly in connection with well drilling, the source of the fossil study being well cuttings. The studies that are in progress in this field are extensive and are being carried on under conditions that require immediate application of the results obtained. Under these conditions there arise special difficulties in adjusting the naming of species. It would help enormously if some organization with sufficient means could correlate the results of the many workers and aid in establishing and unifying the nomenclature applied to the micro-fossils."

Arthur K. Miller recommends a new edition of the Zittel-Eastman Textbook of Paleontology or the production of some equivalent comprehensive handbook for students. It may be noted that the projected Handbuch der Zoologie, of which Dr. O. H. Schindewolf in Berlin is editor-in-chief, will likely fill this need. Miller's comments are as follows:

"Perhaps no one book has done as much to further the development of invertebrate paleontology in this country as has the Zittel-Eastman Textbook of Paleontology. However, the last edition of that book is now about 23 years old, and with the rapid progress that has been made in

recent years, the book is much out of date. It seems to me that it is high time that we consider bringing out a new edition of that book which, like existing editions, I believe should be the work of a group of specialists rather than one or two men."

Louis B. Kellum recommends the preparation and publication of a supplement each ten years to the Catalogue and Bibliography of North American Mesozoic Invertebrates by C. D. Boyle (U. S. G. S. Bulletin 102) and of the Bibliography and Index of North American Mesozoic Invertebrates by S. L. Whitney (Bulletin American Paleontology, Volume 12, No 48).

Systematic Paleontology

It is evident that systematic paleontology still provides a rich field for individual research. Almost every group of animals has been recommended by one or more contributors as a field of needed investigation. It is brought out repeatedly that there is particular need of specialists with wide experience in particular biologic groups, and that there are several neglected groups for which there is at present no outstanding American student. Such, for example, are the radiolaria, the sponges, the cystoids, the blastoids, Paleozoic corals and some of the arthropods. Croneis points out that some of the lesser known micropaleontological groups, such as holothurians, scolecodonts, conodonts, trochiliscids, diatoms, pedicellaria, sponge spicules, and otoliths, may prove to have important stratigraphic value and are promising fields of study. In other important groups, such as graptolites and bryozoa, there is need of more workers to supplement the effort of the few present specialists.

Carey Croneis points out that there is still important research

to be done in the study of ontogeny of several of the groups of invertebrates, and particularly in the phylogenetic origin of some of the groups like the dibranchiate cephalopods. He also points out that bio-statistical studies which have been so successfully employed by a few European workers have received little attention in America. The phenomenon of the rapid decline or extinction following shortly after the climax of various groups of invertebrate fossils is held up by Croneis as a philosophical problem deserving more attention.

B. F. Howell particularly recommends the Cambrian faunas and especially the trilobites as fields needing more workers. A number of contributors, notably Bain, Croneis and Howell, think the search for evidences of life in the Pre-Cambrian rocks is a much neglected field. Turning to more comprehensive faunal studies, requiring co-operation, we may note this one by Chester Stock:

"Vertebrate faunal succession of the Quaternary. This involves the determination of the correct sequence of vertebrate faunas for the Pleistocene and the correlation of individual stages with the physical episodes that transpired during the several glacial and interglacial epochs. It also involves the dating of the first appearance of man in North America and a recognition of the environmental conditions under which he makes his appearance. Another project in this connection is the exploration of caves and cave-deposits, particularly in the arid Southwest and Mexico. The later Quaternary thus presents borderline problems of interest to the geologist, the paleontologist and archeologist.

"Another problem is the extension of our intensive study of the mammalian faunal succession in North America to that area of the continent lying west of the Cordillera. The great basin and the area to the north and south still offer a fertile field for the elucidation of Tertiary and Pleistocene history and for the uncovering of life zones and a record as yet unknown."

STRATIGRAPHY

Facies

The importance of changes of the facies and the stratigraphic problems that result therefrom are now appreciated by most American stratigraphers, but there is still much to be done in working out the actual relations between different facies of many of our stratigraphic divisions. The relation of the faunas to changes of lithologic facies is a part of this problem. Kindle writes:

"Another type of research which is of paramount importance to stratigraphic paleontology is concerned with establishing on incontestable evidence the change in composition which some fossil faunas undergo at short distances when followed in certain directions and of the uniformity which they display for great distances in other directions. The relationship which such changes have to character of sediments, depth of water, marine currents and other possible factors needs to be worked out."

Chadwick adds that:

"The Upper Devonian of New York and Pennsylvania is still an open field calling for greatly extended, honest field-work by the most modern stratigraphic methods."

C. E. Weaver recommends:

"Stratigraphic investigations of individual formations over wide areas with numerous detailed stratigraphic sections in order that the lithologic changes may be determined from base to top as well as laterally. With such information interpretations may be made of the history of the embayment from the time of the beginning to its maximum development and final withdrawal of the sea."

Correlation

The importance of stratigraphic correlation can hardly be exaggerated. It finds immediate, practical significance in the search for mineral wealth and oil and gas, and is involved in most attempts at synthesis or generalization as to the history of the earth or of any region, yet the stratigraphic literature is so vast that no single individual

could read it all and so full of conflicting opinions that even the best-trained specialist finds it difficult to winnow out the established facts and understand the most up-to-date correlations over any large area.

Norman L. Thomas recommends:

"Compilation and revision of the immense amount of correlation information which is in the literature, so that it is more accessible to workers. Real research will be needed as well as library studies if this is to be done properly."

The need of a comprehensive summary or handbook of the stratigraphy of North America is voiced by Ira Cram:

"In the last 20 years great strides have been made in unravelling the stratigraphy of North America. Since the publication of Professional Paper 71 by Bailey Willis in 1912 there has been no comprehensive summary of North American stratigraphy. The great fund of knowledge now existing could be assembled into very worthwhile publication. The geologist working on a project of this kind should not only assemble his published data but should assemble the available unpuslibhed data, such as information worked out by Dr. Ulrich. Men working on such a project could not argue that they were wasting their time inasmuch as the project would lead them into all branches of geology."

Along the same line is this suggestion of Carey Croneis:

"Research in stratigraphy should not be entirely divorced from stratigraphic paleontology in spite of what seems to me to be a growing tendency in this line. The already planned work of the National Research Council's Committee on Stratigraphy is, it seems to me, in the right direction here. Probably the greatest advance that will come from the work of this Committee, however, will be the making available of the now scattered information of stratigraphic import. When the Committee's very utilitarian job is completed, there will then be the basis for some really significant generalizations and advances in the field of stratigraphic paleontology.

One of the greatest needs in stratigraphy is a stratigraphic lexicon or dictionary of formational names which will record all names that have been used and give a reference to original publications and to subsequent publications that have altered the limits or significance of the term. Certainly many American stratigraphers would enthusiastically endorse this comment of Ira Cram:

"Many geologists see the need for a dictionary or glossary of geologic terms. Anyone who has written a paper and has attempted to use precise nomenclature has come in contact with the numerous definitions and interpretations of geologic terms. One can consult the various texts written by the so-called authorities and find as many different definitions for the term in question. Most geologists are not interested in research of this kind, but undoubtedly there are enough competent geologists available for the undertaking of the task."

It is understood that the stratigraphic lexicon now approaching completion by Miss Grace Wilmarth for the Committee on Stratigraphic Names of the U. S. Geological Survey will meet this precise need, and it is much to be hoped that this work will be published promptly. Probably no other single geological publication ever put out by the U. S. Geological Survey would have such extensive use.

Both Chester Stock and Norman L. Thomas suggest the need of more careful intercontinental correlations and see here a group of problems requiring coöperative work.

A number of stratigraphers have expressed an interest in the study of means of correlation. Norman L. Thomas makes this suggestion:

"The problem of correlation by bentonites. Bentonites have received considerable attention but little of a practical nature has been accomplished. There is a strong possibility that they may be of considerable importance, and their distribution is known to be very wide."

Chester Stock recommends:

"Statistical and morphological studies of the fossil marine mammals of America with special reference to the evolution of these forms and to their bearing on transoceanic correlation."

Another type of problem in correlation is suggested by Stock in the following paragraph:

"Problems of correlation which arise in any attempt to determine the time relationships between continental records and land vertebrate faunas on the one hand and the marine records of the Pacific and Gulf littoral regions on the other. The remarkable lower Tertiary land mammal assemblages discovered in the Brea deposits of southern California and their bearing on the interpretation of the Tertiary marine sequence furnish a case in point."

A more extensive interest is expressed in the study of rhythmic or cyclic deposition as a basis for correlation. Charles Schuchert observes:

"Geologic phenomena are cyclic in their nature, and as this periodicity is thought to be at the basis of our systems of rocks and periods of times, it is advisable to examine into all the periods to see how many are separated by universal breaks, and which of these periods have transition formations into the next period. It may be advisable to place this matter into the hands of a special chairman and to select the workers from each of the periods."

For more detailed correlation the study of cyclical deposition in the Pennsylvanian system in various areas is recommended by Harold Wanless and by J. Marvin Weller. Wanless writes:

"One of the types of research in which I have been greatly interested for several years is the exact correlation of the various lithologic units of the Pennsylvanian system within the various coal basins of eastern United States and between the several coal basins. Work by myself and Dr. Weller has nearly covered the Illinois coal fields. This problem needs further investigation in many of the areas in which we have carried on reconnaissance studies and also in other areas. At the outset it appeared that the results of such precise tracings of thin Pennsylvanian units might be of purely academic value, but it is becoming increasingly evident that this work is useful in working out the structure and oil possibilities of various parts of the Pennsylvanian basin."

Areas offering special promise are the southern Appalachian coal fields and the Pennsylvanian outcrops in the Rocky Mountains region.

"There are many different facies of cyclic sedimentation depending upon factors of physiography, climate, rate of subsidence and so forth. Moore has recently called attention to the more complex rhythms in Kansas sedimentation which he calls a megacycle. There are problems relating to the extent of the deposition formed during a similar cycle, the persistence or impersistence of individual members of cycles, the relations between the several factors, the exact geological duration of active cyclic sedimentation, and the causes climatic or diastrophic or both."

"Another somewhat related problem which it seems to me deserves investigation by one or more persons well trained in paleontology, ecology and sedimentary petrology is the study of the relations between the faunas of the late Paleozoic marine sediments and their physical characters. A start in studies of this sort has been made by Moore and Elias, who have concluded that faunal variations in Kansas Pennsylvanian limestones reflect increasing and decreasing depth of water up to a maximum of about 150 feet."

Areal Studies

It is evident that many parts of North America offer inviting fields for stratigraphic studies of an areal nature. Several correspondents, notably

C. E. Decker, Rudolf Ruedemann and E. C. Case, recommend the selection of some area which should be worked out systematically and in great detail from every possible point of view, both stratigraphic and paleontologic, as a sort of type or pattern showing the possibilities of stratigraphic work. Others have pointed out particular areas that seriously need study. B. F. Howell, for example, calls attention to the fact that the Cambrian of the western United States is in serious need of more investigation, and that, for the most part, the Ordovician of the Great Basin, and the Silurian, Devonian and Carboniferous formations of the Rocky Mountain region are as yet very imperfectly known and require much local investigation. Barnum Brown expressed a particular interest in areal studies of the Triassic of the western United States, and Marvin Weller suggests the Mississippian system from Tennessee southward. C. E. Resser adds:

"Intensive study of numerous areas containing Cambrian beds is highly important. Walcott spent fifty years in these studies but since he was the pioneer his efforts had to be directed chiefly toward the accumulation of fossils and stratigraphic data. The second generation of Cambrian workers has taken this material and systematized it, securing some additional information meanwhile. In this second generation there have been only a few workers interested in the Cambrian, insufficient to make much progress in such a wide field. Now that Cambrian information is systematized it is possible for even relatively untrained workers to secure valuable data in restricted areas."

Miscellaneous

Several lines of investigation were suggested by a single correspondent. G. H. Chadwick, for example, proposed a study of the sedimentary structures known as "storm rollers" which occur abundantly in the Upper Devonian formation. E. C. Case advocates a study of diagenetic changes in sedimentary formations, and Harold R. Wanless is interested in the construction of isopath maps as an aid in the interpretation of the stratigraphy and regional history of different areas.

SUGGESTIONS CONCERNING DESIRABLE LINES OF RESEARCH IN THE FIELD OF GEOGRAPHY

Edited by

Robert S. Platt

I. Land Classification

This has to do with work of a sort described as "site analysis" in connection with problems of land use in regions where general environmental conditions are known but where specific conditions from place to place vary through a series of complex types to be distinguished and mapped. Attention is called to this work by geographers of the Tennessee Valley Authority (G. D. Hudson) and of the Michigan State Economic Survey (K. C. McMurry), who have been faced with field problems requiring coordination and standardization. "It appears to me (D. H. Davis) that the most valuable contribution the earth sciences can make at the present time through cooperative undertakings lies in the field of complete inventories of the physical resources of definite regions which will serve as the basis for planning for long time use."

Special phases of this topic have to do with studies for reclamation of flood plain land in the Mississippi Valley (W. W. Atwood), and extension of land classification studies "over the entire South" with "publication of the findings in a form that is available to the people who need to know how better to use their land resources" (A. E. Parkins).

The Division has a committee on "Land Classification (K. C. McMurry, Chairman) which already is at work on some of these problems.

II. Land Forms and Soils

The first suggestion under this heading is a project for classification and mapping of regional areas on the basis of actual surface configuration, aiming at "a new system of classification for land forms other than that based largely on geognostic criteria and more suitable than the present classification to the requirements of the regional geographer" (V. C. Finch); "a re-study of the configuration of the landscape,....of such features as local relief and the slope of the land" (G. H. Smith).

Other suggestions under the same general heading have to do with technological investigations: "the relationship of surfaces and soils under the view that both are phases of the same series of processes" calling attention to "the neglected topic of weathering and the translocation of materials by gravity movement other than stream transport," "soil-slope profiles," "slope analysis," "rate of mass movement," "effect of natural vegetation on denudation," and "the dynamics of man-induced erosion" (C. O. Sauer), "for much more precise understanding of what happens on slopes subject to wash and to the action of frost and heat" (C. C. Colby). "Correlated research is needed on problems of slope wash and the type of farm land erosion now being studied by the Soil Conservation Service" (G. A. Cressey). Research work is suggested "in soil processes and soil types," in "the relationship between types of soils and types of vegetation,....between types of agriculture, and between systems of land tenure and land utilization on the one hand, and the qualities of soil and the character of natural vegetation on the other hand." Suggestions in physiography are made for "study of the work of winds

as geologic and physiographic agents....of the desert cycle of erosion.... of glacial action," of "glacio-volcanic" action, and for physiographic regional studies, particularly "a comprehensive treatment of the Rocky Mountain region" (W. W. Atwood).

The suggestion for classification of land forms seems to call for the cooperation of regional geographers and physiographers. The suggestions for other investigations seem to call in some cases for physiographers and in other cases for technical workers other than geographers.

III. Climates

One group of suggestions has to do with "local climates," "to know the weather and climate of specific areas. Until now interest in weather and climate has concerned itself with attempts to predict weather and to explain the causes of climate. All that is desirable, but it does not produce and will not produce the thing which is needed in land utilization work. We must know the weather and climate of particular elevations and particular slopes. We speak of thermal belts, but we know scarcely anything about those belts" (C. C. Colby). Another writer suggests "microclimatology," "phenologic surveys," and "climatic risk studies" (C. O. Sauer).

Other suggestions in climatology include: "the development of dynamic climatology, from air mass studies," "precipitation efficiency," and "effective temperatures and sunlight in plant growth" (C. O. Sauer); and "a study of the droughts in areas in the South having the Tennessee type of rainfall, with the idea of determining in so far as possible the effects of these droughts on agriculture" (A. E. Parkins).

The suggestions for local climates would seem to call for co-operation between field geographers and meteorologists, and the other suggestions would seem to call for specialists in meteorology and climatology.

IV. Regions

An impressive number of suggestions emphasizes regional geography. "We have come to believe that regional studies are basic.... It is of primary importance that we understand the environmental factors in each of the geographic regions of the world" (W. W. Atwood).

There are many problems to be solved in the regional geography of North America. Numerous studies are going forward in various places and with various modes of attack, unrelated to each other and yet with common objectives, making it desirable "to unify the techniques so that the results would be comparable." Implementation is suggested in the form of a committee which "would act as a sort of clearing house," which might "launch a series of regional studies" or "suggest the areas for study" leading possibly "to a series of regional monographs" (P. E. James).

Suggestions no less impressive deal with regional studies in other parts of the world. For each of the great continental areas there are several specialists working on projects of their own selection, some in field and some in library research, all admitting their inability to cover the whole great area individually and yet not organized to cover it collectively. Hopes and ideas of coordinated programs and cooperation have been expressed on several occasions in recent years and now these are reiterated with suggestions for "continental or regional committees," to "bring together the more active scholars who could point out serious

gaps in our knowledge, endorse desirable investigations, and through occasional mimeographed reports indicate research projects under way, obscure literature and the activities of foreign scholars" (G. B. Cressey).

Further suggestions look not only toward cooperation among specialists in each regional field separately, but also coordination of efforts in all such fields. Particular attention is urged to "areas of international concern" (D. Whittlesey). "Special studies should be made of the danger zones of the world. The marginal areas, often included in the buffer states, should be thoroughly studied from the geographic point of view" (W. W. Atwood).

These various regional suggestions seem to involve the major interests of many geographers and to lend themselves to appropriate and effective action.

V. World Mapping of Classified Items of the Landscape

Several suggestions have to do with classifications on a broad regional basis for understanding of certain world distributions (in contrast with the more detailed classifications for land use under I).

One such group of suggestions is for an authoritative quantitative classification of agricultural land use to be applied to the regions of the world. An example of the sort of procedure required has been worked out in a paper on "A Classification of the Agricultural Regions of Europe and North America on a Uniform Statistical Basis" (R. Hartshorne and S. N. Dicken, Annals A.A.G., XXV, 99-120).

A distantly related project is for "the reconstruction of the natural vegetation or at least the vegetation preceding civilized man....

the reconstruction of the native plant associations" (C. O. Sauer) - presumably the reconstruction on regional maps of the distribution of types.

Another group of suggestions is for a classification of types of settlement or habitation and mapping of the regions of the world on this basis to distinguish areas of agglomeration, of dissemination and of no population (R. B. Hall), significant distinctions not made on existing generalized maps of population density. The Division has a committee touching the subject of population, on "Cooperation with the Bureau of the Census" (J. K. Wright, Chairman), but this would not include such suggestions as that above in its present program of activity.

There seems to be no doubt of the desirability of coordination among geographers working on any of the projects mentioned in this section, and the furtherance of such coordination seems to wait only on the expressed desire of a considerable number of men actively interested and ready to participate.

VI. Mineral Resources

This subject is mentioned in two letters. "The geography of the mineral resources of the world should be worked up because of its very great value in industry and commerce, and in international affairs" (W. W. Atwood). "Modern industrial civilization is based on minerals. Research along this line should prove most profitable. Where are they and what can be done to relieve the strain on the most critical" (W. H. Haas).

Additional comment on this subject can be left to economic geology rather than to geography. Mention of it here serves chiefly as

evidence of a common interest.

VII. Urban Geography

This also is mentioned in two letters. "One of the fields which should be thoroughly investigated....is that of Urban Geography.... It seems to me that there are two aspects involved in association with Urban Geography, namely, concrete problems applied to specific urban centers on the one hand and the philosophy and mode of procedure on the other hand.... Some time might be gained and unnecessary research avoided....if a conscious effort were made....to delineate the field" (E. Van Cleef). "A humble beginning has been made in this country in the geographic study of cities. The field is opening and offers excellent opportunities. The results of such study should be of fundamental importance in city planning, or in the regional planning of a metropolitan district" (W. W. Atwood).

This matter is undoubtedly of much greater interest to many geographers than its mention by only two geographers would suggest.

VIII. Other Suggestions

Each of these has been mentioned in only one letter. The fact that they have not received widespread expression certainly does not mean that they will not prove important within the next decade nor even that they are not important now. Some of them need only be stated to be recognized as significant lines for investigation.

(1) Basis of industry. "The possibility of making pulp and paper from Southern slash pine" (A. E. Parkins).

(2) Transportation. "Our railways and our highways when once they are laid down become a definite and fixed part of the occupancy

pattern in which the great activities and the great flows of society occur. The airways likewise are definitely related to our earth features. All of these transportational facilities and patterns are developed with the idea of service to society. They do not, however, serve society as well and as thoroughly as they might. Many things contribute to this waste, but a part of that waste grows out of our lack of a sharp understanding of these facilities and patterns as a part of the physical environment in which human activities take place.... The rapid recent development of geography as a science of measurement leads me to believe that if we apply these new techniques to the study of our transport facilities and patterns we shall penetrate into new borders of facts and probably into new physical principles" (C. C. Colby).

(3) Geography and Medicine. "The geography of disease offers one of the most fascinating opportunities for an investigator who would like to do original work of great human significance.... There is probably no field in which the human significance of geographic studies offers greater possibilities" (W. W. Atwood).

(4) Fossil Geography. "Geographic studies should be carried out in association with archeological field work. An understanding of the soils, climate, natural vegetation, the resources available beneath the soils, and the history of the climate interpreted possibly in tree rings, may throw important light upon the interpretation of archeological records. The ancient home of the Mayan culture, of the Inca and pre-Inca people, of the Aztecs, of the Mixtecs, and of the cliff dwellers of our southwest open up a few of the attractive possibilities in the western hemisphere". (W. W. Atwood).

That the latter suggestions have not elicited more widespread comment in the present inquiry may be due to the fact that some of them have less to do with the physical side of geography close to geology, and more to do with the human side. In this connection some suggestions have been made regarding the relations of the two sciences in the division. "To my mind (C. O. Sauer) the position of the geographers with regard to the research councils of the country has been in error. I think personally that it has been a mistake for the geographers to be content with a minor position in the National Research Council and to have neglected the opportunity of associating themselves with the Social Science Research Council. I find this remark necessary by way of preface because I think that geography looks in both directions and needs contacts in both directions. I find the position foolish that geography is something apart from either natural or social science and also the one that it pretends to be entirely either the one or the other. Certainly I think it has been unfortunate that geographers have attempted to bring their social science problems into the National Research Council. My desire is to see the Association ask as well for admission to the Social Science Research Council and place itself in the dual position in which the anthropologists are.... I should therefore like to see the geographers present in your body only their natural science problems in the main. I should define most of the problems that concern principally the geographers of America at the present time as being outside of the field of the National Research Council."

But evidently there are at least two sides to the problem. Projects making no sharp distinction between the physical and social sides

of geography are recommended to the National Research Council with the justification that they "are distinctively geographical projects" (V. C. Finch), whereas other projects suggested because representing only the physical side are characterized as requiring "to be solved by technicalogical investigations which I do not recognize (V. C. Finch) as distinctively geographical." "I do not believe (C. C. Colby) that it will be wise to separate research projects in geography into the two categories.... After all, geography deals with man and the earth, and it becomes important only when it deals with the unit entities in which man finds his expression on the earth. For the National Research Council to be concerned only with problems dealing with the earth would mean endorsement of the minutiae rather than the whole of a science." Probably the "foolish" position "that geography is something apart from either natural or social science" or "that it pretends to be entirely the one or the other" (C. O. Sauer) does not occur to anyone as a real argument.

Whether or not a change in divisional relations is desirable ultimately there seems to be no reason for immediate action. Present relations may be considered innocent until proved guilty, particularly while their innocence is strongly maintained. Apparently neither geologists nor geographers now actively concerned in the work of the Division are dissatisfied at the present situation, and apparently both groups are functioning effectively with mutual respect and with scope for developing their respective interests as rapidly as they desire. Even the inclusion of interests in the field of human geography does not seem to have been objectionable in causing interference with other interests or failure in themselves. A going concern operating successfully may well be continued

even though it does not conform to an ideal logical arrangement. Ideal alternatives are worth keeping in view as a basis for rearrangement when geography outgrows the present arrangement and begins to feel hampered by association with geology in one division of one research council. Probably geography should "be content with a minor position in the National Research Council" until constructive discontent grows to such a point that the leaders are ready to take positive action and build up geographical relations into a different and more perfect structure.