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THE NOMENCLATURE AND CLASSIFICATION OF SEDIMENTARY ROCKS

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ABSTRACT. According to the purposes they serve, classifications of sedimentary rocks may be classified into descriptive and genetic, and these further into purely descriptive and analytical descriptive classifications and into purely genetic and operational genetic classifications. No one classification can serve all purposes; at the least a descriptive and a genetic classification are needed. Moreover, field workers need primarily a purely descriptive classification and laboratory workers an analytical descriptive classification. A field classification might well be built around the recommendations of the Committee on Sedimentation of the National Research Council. For a laboratory classification, the confused state of igneous rock terminology warns against the pigeon-hole type of classification. It is suggested that a continuous variable type of classification, expressed entirely by measured parameters instead of rock names, would provide precise and objective designation of rocks in terms of their properties. The paper makes no attempt to present a detailed classification but is intended to stir up discussion of the principles of classification from which, later, general agreement may emerge.

INTRODUCTION

THE subject of classification is always a live one in geology, and the classification of sedimentary rocks is being especially debated at present, as witness the symposium recently published in the Journal of Geology (Pettijohn, 1948; Shrock, 1948; Krynine, 1948). All seem agreed that the existing classification is unsatisfactory, but there agreement ceases. The wide present interest insures, however, that all attempts to create new classifications will be subjected to many-sided analysis, and we may confidently expect that from this analysis a more satisfactory classification will ultimately emerge. The present paper is intended as a contribution to that analysis of classifications; it hopes to focus attention on certain points that may deserve consideration as we work at constructing a satisfactory scheme. The paper has benefitted greatly by the helpful criticisms of Adolph Knopf, Eleanora B. Knopf, Carl

O. Dunbar, and Richard F. Flint, but I must be held accountable for all that is crude, unclear, or simply erroneous in it.

The problem of classification inevitably entails the problem of terminology or nomenclature; indeed they are but facets of a single problem. Most scientific names imply a classification, whether actually carried through or no, and many classifications consist simply in the systematic arrangement of preexisting names. I shall assume therefore that the purposes served by nomenclatures are in general the same as those served by classification, and I shall begin by discussing the several purposes of sedimentary rock classifications.

KINDS OF CLASSIFICATIONS

The first purpose to be served by a nomenclature or classification of sedimentary rocks is to provide terms that will convey from one geological mind to another an adequate conception of the physical character of a given rock. To fulfill this purpose, the terms should be descriptive, objective, and precise; genetic terms, such as till, should be avoided because they depend on hypotheses of origin and are not necessary to convey the description. A classification that serves this purpose may be called a *descriptive* classification.

A descriptive classification should provide categories for rocks of every kind, thus facilitating for teacher and student a survey of all the possible varieties that may be encountered. But in addition the classification can be of great value to the research investigator if, by grouping the like and separating the unlike, it calls attention to genetically significant correlations between likenesses and unlikenesses. The properties that might be chosen for analyzing the relationships of phenomena as complex as sedimentary rocks are of course legion, but what distinguishes a good classification of this sort from a poor one is that the properties chosen are those that best bring out such correlations and thus lead toward genetic understanding of the phenomena. In such a classification a neat balance must be struck between the descriptive and the genetic; genetic terms as such must be avoided because they beg the question and prejudge interpretation of the observed correlations, but those descriptive terms that have in fact the highest correlation with genesis will make possible the most fruitful analysis. A descriptive classification that serves this additional purpose may be called an *analytical* descriptive classification.

A quite different purpose that may be served by a classification of sedimentary rocks is to provide terms that will convey from one mind to another an adequate conception of what is known, inferred, or believed to be the origin of a given rock. To fulfill this purpose, the terms should be genetic and unequivocal; terms that indicate the origin directly, such as till, are preferable to terms that do not, such as boulder clay. A classification that serves this purpose may be called a *genetic* classification.

A genetic classification should provide categories for rocks of every origin, thus facilitating for teacher and student a survey of all the possible hypotheses that may need to be considered. But in addition, the classification can be of great value to the research investigator if it calls attention to genetically significant criteria that may be applied in specific instances. The criteria that might serve as a basis for classifying such complex phenomena by origin are legion, but what distinguishes a good classification of this sort from a poor one is that the criteria chosen are such that the investigator can observe them unequivocally in the data available to him. To borrow a word from the jargon of the methodologists, such criteria are "operational." Here again a nice balance must be struck between the genetic and the descriptive; those genetic traits that have the highest correlation with descriptive data will provide the most useful "operational" criteria. A genetic classification that serves this additional purpose may be called an *operational* genetic classification.

Clearly the analytical and the operational classifications approach each other, and when every fact is known and every interpretation secure they will coincide to form the ideal classification toward which we are striving. But until that happy day (when all of us in sedimentary geology will be looking for other jobs), no one classification can serve all the purposes enumerated, and a classification satisfactory for one purpose, such as description, will necessarily be less than satisfactory for others, such as expressing origin. To repeat the example already mentioned, the term till gives a clear conception of the genesis of the material it refers to, but it does not provide an adequate description of that material, which might be a strongly cohesive boulder-bearing clay, a compact but weakly cohesive silty clay with scattered chalk fragments, or a loose silty and sandy rubble. Moreover, if it is not known whether the material in question was deposited by ice or by mudflow, the genetic term cannot properly be applied and if applied will be misleading, but an objective description is still valid. On the other hand, the genetic classification conveys readily whatever can be inferred about the origin of the material and is especially helpful in suggesting additional relationships that may be looked for in checking the original inference; few if any purely descriptive classifications can accomplish this end.

For sedimentary rocks then at least two types of classification are needed, the descriptive and the genetic; neither will suffice alone. The use of descriptive terms alone would indeed make sedimentary geology a dull and mechanical subject, leading to little understanding of phenomena and hardly deserving the name of science. On the other hand, the use of genetic terms alone is not dull but dangerous; for once a term is attached to a given rock it tends to conceal ignorance and discourage re-examination, yet one of the cardinal principles of the scientific method is that no hypothesis, theory, or law shall ever be so firmly established and enshrined that it is secure from restudy, reappraisal, and perhaps rejection, whenever new facts are discovered that appear to conflict with it. If, however, the descriptive classification used is in addition analytical, then the dull process of cataloguing rock types is given point and significance and helps us to understand those types; if the genetic classification used is operational, the dangerous tendency to theorize beyond the data can be kept in check by frequent reference to objective facts. In the present imperfect state of knowledge, both classifications are needed, and neither should usurp the place of the other.

I must therefore protest vigorously against such statements as "There can be no such [descriptive] classification worthy of consideration" (Pettijohn, 1948, p. 113). Yet to quote this one statement out of context is quite unfair, for Pettijohn in the passage cited is pleading in fact for a classification in which "the characters used for the classification are meaningful or significant." To me, however, such a classification is not properly a genetic classification, as he seems to imply, but rather one of the type to which the term analytical was applied above. Pettijohn notes that the igneous petrologists do not classify their rocks by color, but he fails to note that they do classify them by such purely descriptive characters as grain size, mineral content, and feldspar composition, and in general have stopped classifying igneous rocks according to whether they are Paleozoic or Cenozoic and are tending away from the older subdivision into deepseated, hypabyssal, and volcanic. Feldspar composition seems a better basis than color because it appears to be of more genetic significance, but nevertheless the classification is strictly descriptive. That a purely genetic classification has value, I would be the last to deny, but I would suggest that the problem of genetic classification should be kept quite separate from that of descriptive classification, and furthermore that the first order of business for sedimentary geologists at the moment is to reach agreement on a satisfactory descriptive and preferably an analytical classification of our rocks.

EXISTING CLASSIFICATIONS

A touchstone for evaluating classifications of sedimentary rocks is provided by the rocks commonly grouped as limestone, for these rocks display an overall descriptive homogeneity, as shown by the use of a single field term for them, but are genetically varied. It is of some interest, therefore, to examine the disposition of these rocks in some of the principal classifications that have been proposed to replace the conventional subdivision of the sedimentary rocks.

The most uncompromisingly genetic classification ever proposed is that set forth by Grabau in his Principles of Stratigraphy (Grabau, 1913), and his treatment of limestone illustrates this very well. As Professor H. Ries once pointed out to me, the word limestone does not even appear in the index of the book, the rocks in question are scattered through four or five major subdivisions of the classification, and they bear a variety of names such as calcilutyte, hydrogranulyte, biopulveryte, etc. As a genetic classification, this is thoroughly logical, but a student wishing to learn about the rocks usually called limestone or to run down the origin of a particular one will find it of little use. For the most part operational criteria are lacking, and the classification has been largely ignored.

In recent textbooks and articles, other classifications have been proposed as genetic by Hatch, Rastall, and Black (1938, pp. 36-39, 152), by Twenhofel (1939, pp. 260-261), and by Shrock (1948, pp. 118-119). All of these writers state that the rocks called limestone are diverse genetically, yet all of them actually group them into a single category. Surely these classifications are not genetic at all but descriptive classifications masquerading as genetic or possessing genetic subcategories, thus mixing genetic and descriptive in unsystematic fashion, and they do not differ in any essentials from the conventional classification found in most elementary textbooks of geology.

A quite different approach is that of Krynine (1948, esp. pp. 147-149), who in contrast to the men just mentioned proposes a frankly descriptive field classification. Krynine makes it evident however that he is not content with a *purely* descriptive classification but is constructing one that in the terminology suggested in this paper is analytical. This approach he applies in the first instance to the subdivision of sandstone, and one does not have to believe that there is 100 percent or even 80 percent correlation between, for instance, arkose and extreme uplift to see the great value of the method. Krynine then applies the same subdivisions to the other main groups of sedimentary rocks. Limestone he divides on two bases; first on the presence or absence of 5 percent or more of recognizably fragmental non-calcareous material and of 10 percent or more of recognizably fragmental calcareous material, second on the mineral composition of the non-calcareous fragmental fraction, using the same subdivisions as for sandstone.

This second basis seems rather less significant for limestone than for sandstone, for it depends on a minor constituent of the rock and, as pointed out to me by John A. Elson, Krynine admits it will often require studying an insoluble residue, hardly a field procedure. The first basis on the other hand might be expanded (though perhaps not in a field classification) by recognizing subdivisions based on the dominance or relative proportions in the calcareous fraction of the following four components: non-fragmental recognizably organic material, recognizably organic fragments, inorganic fragments, apparently inorganic and non-fragmental material (which might be either the result of direct chemical precipitation or of thorough recrystallization, a distinction for which we have as yet no good operational criterion). But details are of minor importance compared with the principle to which Krynine has adhered, that of selecting as the basis of classification objective and descriptive characters, but those that have the highest correlation with genesis. Krynine's classification thus possesses a vitality that Grabau's lacked, and many of his proposals will almost certainly find their way into the classification toward which we are all working.

SUGGESTIONS FOR FUTURE CLASSIFICATIONS

Krynine's paper underlines another problem that faces the maker of classifications, the conflict between the needs of the field and the laboratory worker. The need of the field man is primarily for a purely descriptive classification, by which he can convey clearly, precisely, and objectively what he sees. Hence a field classification is more likely to use obvious characters than less obvious but genetically more significant ones. That the tendency to record obvious characters only and to ignore all the others has been overdone cannot be gainsaid, and it diminishes greatly the value of many field reports, but the field man commonly has many things to observe in a limited time beside the details of sedimentary rocks, and if the classification is too complicated or difficult he simply will not use it at all. In contrast, the laboratory worker is ordinarily primarily interested in genetic understanding, and the best *descriptive* classification for him (I leave to one side genetic classification) is an analytical one. Moreover, he is presumably equipped to determine any determinable character, and his only concern is not to waste time determining characters that have no significance. For him, therefore, it does not matter how subtle the characters used in classification are but only whether they have a high correlation with genesis; in other words, the more analytical the classification the better.

For a field classification, I would like to call attention to the classification set forth in a series of reports to the Committee on Sedimentation of the National Research Council (Wentworth and Williams, 1932; Wentworth, 1935; Allen, 1936; Twenhofel, 1937; Tarr, 1938). Here is a classification beaten out after considerable thought and consultation among sedimentary geologists and providing for the most part simple but quantitative definitions of descriptive rock terms (a number of convenient genetic terms are also defined). The classification has no overall guiding principle except convenience and a proper respect for the coiners of terms, but it may be questioned whether phenomena as complex as sedimentary rocks will yield to any single-track classification. Probably no one will agree with all of the classification; for instance I object to the extension of the term mudstone to include not only all consolidated but all unconsolidated fragmental sediments with particles less than 1/16 mm., as I prefer the commoner restriction to a consolidated rock in that range which does *not* show fissility, but as pointed out to me by R. R. Shrock, this is not the original definition of the term, and perhaps it has been used in so many senses that it should be abandoned. Admittedly also the classification fails the limestone test miserably, for the Committee never made a proposal for the nomenclature of the carbonate rocks. But this gap could readily be filled along the lines of the existing classification; as a basis for discussion I venture to suggest a primary distinction between limestone (made of calcite) and dolomite[1]; a secondary distinction between carbonate rock made principally of organic material and that made principally of inorganic or indeterminable material; further distinction in the organic carbonate rocks according to the dominant organism, if recognizable, and according to whether the organic remains are roughly in place and whole (as in a coral reef) or mechanically transported and fragmental, if that can be determined; and further distinction in the inorganic carbonate rocks according to the presence or absence of recognizable fragmental grains or mechanical structure (such as cross lamination) and according to grain-size. In any case, the Committee recommendations could be made the basis for future discussions of a descriptive

[1] Dolostone (Shrock, 1948, p. 126) seems unnecessary, as the term dolomite was first proposed (in the form dolomie) by de Saussure fils (1792) for the rock; Dolomieu (1791) and both de Saussures (1792; 1796, pp. 17, 109, 110) write "pierre" repeatedly. As the mineralogists are the ones who have duplicated the term, they might choose some variant, such as dolomine.

field classification of sedimentary rocks, and we could attempt to sharpen up the Committee classification and fill in its gaps rather than try to start from scratch and convert the field geologist to radically new usages.

For a laboratory classification, I have no complete classification to offer, only a few negative remarks to make. Such a classification could demand relatively complete information on all the important measurable properties of sediments, and hence could use any of these properties for its subdivisions. The prevailing theory of classification seems to be to set up named compartments, quantitatively defined of course, in a grid system of 2, 3, or more dimensions, each dimension being a different parameter. If I might be facetious, I would christen a classification of this type a pigeonhole classification. Pettijohn seems to advocate (1948, p. 114) the ultimate in this type, in which each important rock type would receive its own separate name, commonly geographical; for example, bradfordite for sandstone like the one described by Krynine (1940) from well samples of an oil sand in the Bradford district, and spergenite for rock like the familiar Indiana building stone. Pettijohn points to the analogous terminology of igneous rocks, but surely the condition of igneous rock nomenclature should be a warning to the sedimentary geologists rather than a guide. The number of individual rock names for igneous rocks that had been coined before 1935 was 900 or more (Tröger, 1935, pp. 3-5), and few of these give in themselves a hint as to the kind of rock denominated. Among the igneous geologists themselves there is a growing dissatisfaction with the whole haphazard and unwieldy scheme and the burden it places on the memory (for a recent searching criticism, see Shand, 1944). In view of the unhappy experience of our colleagues, we sedimentary geologists would do well to forego the heady wine of indiscriminate name-giving (and credit-claiming) in order to avoid the hangover of a confused and illogical terminology.

As a counter to the pigeonhole type, I would like to suggest what might be called the continuous variable type of classification. An example in another field would be the modern description of the plagioclase feldspars. During the earlier period of study of the feldspars, a number of different mineral species were recognized within the plagioclase series and each was given a name. With more study it became clear that the series is continuous from albite to anorthite, and the usage of these names was standardized to imply fixed quantitative limits, but at the same time an alternative system of designation was adopted based on percentage composition in terms of "end members." Thus a given feldspar may be described by the term oligoclase or by the symbol $Ab_{89}An_{11}$ (note that Ab and An do not stand for the *minerals* albite and anorthite but for chemical abstractions). Of these, the latter is not only shorter, but a great deal more precise. Moreover, the use of species names unduly emphasizes artificial boundaries. Why, for instance, should $Ab_{89}An_{11}$ and $Ab_{71}An_{29}$ be grouped together as oligoclase but $An_{89}An_{11}$ and $Ab_{91}An_{9}$, which are clearly more closely related, be separated as though quite different? In the case of the feldspars no one is likely to be misled, but according to several igneous rock classifications, the same distinction may cause two rocks to be separated as soda granite and granodiorite, though the entire difference between them is 2 percent in the composition of the plagioclase feldspar.

It will be objected that boundaries, even though necessarily arbitrary, are essential. But why? The rocks we are dealing with show a continuous range in most of the variables we can measure, and a classification that faces and makes use of that fact should be more useful than one that overlooks and distorts it. To be sure, we may well find breaks in such ranges that will give a basis for "natural" boundaries between classes of rocks. But in most quantitative schemes of classification, the boundaries are set up first, mainly with a view to a symmetry which the rock distribution itself does not possess, and the rocks are then expected to fit the neat pigeonholes. This is no criticism of quantitative classification as such, but only of the pigeonhole type; for example, the pigeonholes, the grads and rangs, of the C.I.P.W. system are dead and forgotten, but the continuous variable system on which they were based, the norm, is still widely used in igneous geology.

Defenders of the pigeonholes will point, however, to the convenience of group terms such as basalt, diorite, granite, for conveying the general character of the rock, which is after all the first purpose of classification as mentioned at the beginning of this paper. But that purpose is especially the province of the field classification; the purpose of the laboratory classification is to convey not general but precise information, such as can be obtained by laboratory techniques. And again the constant confusion between field and laboratory terms for igneous rocks should give sedimentary geologists pause. It is often very difficult to tell whether, in a given paper, the term granite is being used in the precise laboratory sense of a rock consisting largely of potassian feldspar (according to some classifications it might be albite) with more than 10 percent quartz, or in the broad field sense of any light-colored coarse-grained quartzose rock. Thus we read again and again of the great granite batholiths of the Cordillera, most of which consist primarily of quartz monzonite and granodiorite. We can avoid this confusion in sedimentary geology if we will restrict the older terms such as sandstone, shale, limestone to the field classification and construct the laboratory classification on a new basis. For that basis I would like to suggest the measurement of basic parameters and their expression in formulae like those for the feldspars. Instead of first getting up our own categories and confidently pigeonholing the rocks in this one or that one, let us first study and measure the rocks and plot the measurements on triangular diagrams and other graphs, and then look diffidently to see whether there are natural groupings of those rocks that might deserve special terms.

There have been certain attempts of this sort already. I think first of Alling's paper on "microlithologies" (1945), which frankly proposes a laboratory classification of rocks without reference to the usual field terms. The classification is based on a triangular diagram, the end members of which represent the argillaceous, calcareous, and siliceous fractions of the rock, computed according to certain quantitative rules. Thus a rock called in the field a limy shale might be designated by the formula $A_{70}C_{25}S_5$. Alling has marred his own case by permitting such quantitatively meaningless expressions as "5 to 10 percent of the clastic carbonate" (p. 745) to slip in (cf. also the entirely qualitative and therefore quite unreproducible description of types of fissility, p. 753), but no one who has struggled with facies faunas can look at diagrams A and B of his figure 6 (p. 751) without realizing the power of the method.

A second proposal for the use of continuous variables in sedimentary classification is that of Krumbein and his associates (Krumbein, 1948; Dapples, Krumbein, and Sloss, 1948). Krumbein also uses a triangular diagram and supplements it by ratios between the end members; these ratios permit the plotting of significant isopleth (isolith) lines on maps, as he has demonstrated. To be sure, points in his triangle refer not to individual sedimentary rocks but to summations of the rocks in stratigraphic sections of the order of magnitude of a system. The principle, however, could certainly be applied to the classification of rocks, provided the end members were quantitatively and operationally defined so that results could be reproduced from laboratory to laboratory.

Krumbein has also shown (1948, pp. 1916 ff., fig. 5) how readily the principle can be extended to all parts of a classification by subdividing each end member of the original triangle on additional triangular diagrams, and by adding additional triangles for other subdivisions, including genetic ones.

Similarly Krynine (1948, fig. 4) has illustrated the subdivision of sandstone on the basis of a triangle whose end members are mineralogically defined; thus a given sandstone might be designated as $Q_5A_{25}G_{70}$[2] (in Krynine's terms a high rank graywacke). (That his diagram shows arbitrary boundaries and that its end members are not quantitatively defined is irrelevant, for in the paper in question he was not proposing the triangle as a basis for a detailed laboratory classification but using it to illustrate the principles of his field classification.)

These examples indicate the form which a laboratory classification of the continuous variable type might take. The fundamental designation might well follow Alling's lead and be expressed in proportions of argillaceous, calcareous, siliceous, and other. Further distinctions could be based on subdivision of the dominant end members of any rock; thus the siliceous end member might be divided according to Krynine's scheme (as suggested by Krumbein, 1948, pp. 1917-1918, fig. 5), the argillaceous by the proportions of the main groups of clay minerals. Again subdivision might be made on grain size or on grain fabric, say the proportion of interlocking to non-interlocking (presumably fragmental) borders. Subdivision into three end members at each step has, of course, no special magic

[2] Standing for quartzite, arkose, and graywacke.

virtue; two or four components might be called for at various steps. (Four components can be handled on triangular diagrams by replacing the point that expresses three components by a small triangle whose size is proportional to the amount of the fourth component, which should be the least important in the given group of data, or by indicating beside each point its altitude in the four-component tetrahedron of which the given triangular diagram is the base.) The most important requirement would be that each end member be simply, quantitatively, and if possible operationally defined, so that workers in different laboratories might arrive at comparable figures. A set of three or four formulae constructed in this fashion would indicate the composition and perhaps other characters of the rock in less space and with far greater precision than the conventional terms, even if there were general agreement on quantitative limits to the pigeonholes those terms represented. For example, the formula $A_{30}C_{0}S_{70}$; $Q_{15}A_{80}G_{5}$; $K_{90}M_{0}I_{10}$[3], plus a cumulative or frequency curve of the size distribution (or a statement of the first three moments) would describe a highly feldspathic non-calcareous arkose with abundant kaolinitic cement, and at the same time would indicate precisely where in the range of arkose the given rock would fall.

CONCLUDING REMARKS

In conclusion, I venture to sum up the suggestions made in this paper. Classifications of sedimentary rocks serve a number of purposes, and no one can serve all; we need both descriptive and genetic classifications, but the need for the former is at present the greater. Among descriptive classifications we need a purely descriptive classification for field use and an analytical descriptive classification for laboratory use. The field classification might well be built around the recommendations of the National Research Council Committee on Sedimentation. For the laboratory classification, I suggest abandoning the traditional pigeonhole type of classification and attempting to replace it with formulae of the continuous variable type which, although abstract, express the data precisely and without subjective coloration.

[3] Standing for kaolin group, montmorillonite group, and "illite" group of clay minerals.

Finally, I realize that many of the proposals here made are shots in the dark and require careful study and much modification before they can hope to be considered for acceptance. If this contribution has any value, it is likely to be negative, by leading to a review of the tacit assumptions and unquestioned procedures that have underlain our attempts at classification, rather than positive, by presenting fully blown the classification to end all classifications. I believe that no one of us alone can presume to create such an ideal classification for no one of us sees all the needs, those of the field man and those of the laboratory worker, those of the teacher and student and those of the research investigator. But by working together, by friendly interchange of ideas and constructive criticism, we should be able to devise a classification or rather classifications which if not satisfactory to all in all respects will at least provide us with an adequate common language.

References

Allen, V. T., 1936. Terminology of medium-grained sediments (with notes by P. G. H. Boswell): Nat. Research Council, Rept. of Comm. on Sedimentation, 1935-1936, pp. 18-47.

Alling, H. L., 1945. Use of microlithologies as illustrated by some New York sedimentary rocks: Geol. Soc. America Bull., vol. 56, pp. 737-756.

Dapples, E. C., Krumbein, W. C., and Sloss, L. L., 1948. Tectonic control of lithologic associations: Am. Assoc. Petroleum Geologists Bull., vol. 32, pp. 1924-1947.

Dolomieu, Déodat de, 1791. Sur un genre de pierres calcaires trés-peu effervescentes avec les acides, et phosphorescentes par la collision: Jour. physique, tome 39, pp. 3-10.

Grabau, A. H., 1913. Principles of stratigraphy, A. G. Seiler & Co., New York.

Hatch, F. H., and Rastall, R. H., 3rd edition revised by Black, Maurice, 1938. The petrology of the sedimentary rocks, George Allen & Unwin Ltd., London.

Krumbein, W. C., 1948. Lithofacies maps and regional sedimentary-stratigraphic analysis: Am. Assoc. Petroleum Geologists Bull., vol. 32, pp. 1909-1923.

Krynine, P. D., 1940. Petrology and genesis of the Third Bradford Sand: Penna. State College Min. Industries Exper. Sta. Bull. 29.

———, 1948. The megascopic study and field classification of sedimentary rocks: Jour. Geology, vol. 56, pp. 130-165.

Pettijohn, F. J., 1948. A preface to the classification of the sedimentary rocks: Jour. Geology, vol. 56, pp. 112-117.

Saussure, H-B. de, 1796. Voyages dans les Alpes, tome 4, Geneva.

Saussure, N. T. de (le fils), 1792. Analyse de la dolomie: Jour. physique, tome 40, pp. 161-173.

Shand, S. J., 1944. The species concept in petrology: Am. Jour. Sci., vol. 242, pp. 45-52.

Shrock, R. R., 1948. A classification of sedimentary rocks: Jour. Geology, vol. 56, pp. 118-129.

Tarr, W. A., 1938. Terminology of the chemical siliceous sediments: Nat. Research Council, Rept. of Comm. on Sedimentation, 1937-1938, pp. 8-27.

Tröger, W. H., 1935. Spezielle Petrographie der Eruptivgesteine, Deutsche Mineralogische Gesellschaft, Berlin.

Twenhofel, W. H., 1937. Terminology of the fine-grained mechanical sediments: Nat. Research Council, Rept. of Comm. on Sedimentation, 1936-1937, pp. 81-104.

———, 1939. Principles of sedimentation, McGraw-Hill Book Co., New York.

Wentworth, C. K., 1935. The terminology of coarse sediments (with notes by P. G. H. Boswell): Nat. Research Council, Rept. of Comm. on Sedimentation, 1932-1934, pp. 225-246.

———, and Williams, Howel, 1932. The classification and terminology of the pyroclastic rocks: Nat. Research Council, Rept. of Comm. on Sedimentation, 1930-1932, pp. 19-53.