SOME POINTS RELATING TO THE GEOLOGICAL EXPLORATION OF THE FORTIETH PARALLEL.

BY M. E. WADSWORTH, PH.D.

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INTRODUCTORY.

Since the preceding paper[1] is exclusively devoted to a criticism of an abstract[2] of mine published some over two years ago, it is necessary that some reply be made, on account of the importance of the questions to American geology, and to lithological science.

So far as Mr. Merrill's paper goes it concerns the subject in two points of importance only:

1. That the present writer had been mistaken in his use of the term "original micro-felsitic base," since the *original* use of the term was different: hence the present writer was unqualified for his work, and had made his statements ignorantly.

[1] "Concerning the Lithological Collection of the Exploration of the Fortieth Parallel," By Mr. N. F. Merrill.

[2] Bull. Mus. Comp. Zoöl. 1879, V, 275-287. "On the Classification of Rocks."

2. The present writer's statements of what the rocks are in the collection under discussion do not agree with Mr. Merrill's definitions of diorite, diabase, hornblende porphyry, gabbro, basalt, andesite, etc.; and therefore the same conclusion is to follow as in the preceding.

To the first it is simply to be said that the word *original* was used in direct opposition to the terms *alteration and devitrification* in the same sentence, and had no reference to the "original use" of the term micro-felsitic. The word *original* meant in the sentence, that which was believed to be the direct product of the cooling magma, in contradistinction to that which was the product of subsequent changes in the rock. The truth of my previous statement is virtually admitted by Mr. Merrill; who, however, fell into the error of not regarding it as the point at issue.

Concerning the second point, it is to be said that the usual classifications had been rejected by me and the principles of another briefly sketched. Further, it was pointed out where, in this classification, certain of the specimens described in Vol. VI, of the Fortieth Parallel reports, would be placed by me.[1]

Now it was not intended that these specimens should be made to conform to any of the artificial classifications that were rejected, but which Mr. Merrill has adopted from his teacher — Zirkel.

In both of these counts, which cover all of importance in Mr. Merrill's paper, he has totally failed, on account of erroneous views regarding my statements and opinions. These errors of Mr. Merrill were unnecessary, since he could have easily ascertained my meaning upon any points not clear to him, either in person or through mutual friends.

Furthermore, it would seem that sufficient attention had been called in my previous abstract to the three distinct classes of materials found in rocks, to prevent the first mistake. Surely in the second case it was unnecessary, since it is in violence of the canons of criticism, to demand that an author's statements shall agree with that which he has especially rejected. This, Mr. Merrill evidently knew, since he has adroitly endeavored to turn my own principles against me when he thought he could do so.

[1] It may be remarked that the originator of a classification may possibly know where specimens are to be placed according to the principles of that classification as well as any one else.

It should be understood that the writer rejects the common lithological method that Mr. Merrill seems to be using and expects the writer's work to conform to. This method can best be characterized, in a homely way, by supposing that there were placed in the hands of a zoölogist a great number of specimens of one species of some carnivorous animal, in every condition, from a fresh state to that of an advanced stage of decomposition; also of those of the same species that had lived during distinct periods of time, as well as of those that had lived for different lengths of time. With these, too, let there be given to the zoölogist a number of packages of the bones of this animal, part of the bones having been worn and part unworn.

Now imagine this zoölogist naming as new species, every specimen more decomposed than a preceding one; as new species, those which showed different products of decomposition; as new species, those that gave any variation, through that decomposition, upon chemical analysis; as for instance, one and forty-seven one hundredths, or even forty-six one hundredths of one per cent, for which the reader is referred to Mr. Merrill's paper. Continuing, let it be supposed that our zoölogist makes new species, or at least varieties, out of all specimens in which he finds any teeth or bones of other animals which have been swallowed, changing the species or variety as often as the inclosed fragments differ; creating new species out of all that have lived for different lengths of time; new species out of those whose bones are fractured crosswise, as distinct from those whose bones are broken lengthwise; new species out of the distinct packages of fragments; new species according as these fragments are worn or angular. Also, above and beyond all, fixing an arbitrary date, and demanding that all the specimens of this animal, that had existed prior to that time, should be held as distinct species and in general of different origin from those that were of a later period. Suppose too, that in addition, our zoölogist should advocate that a part or the whole of the specimens submitted to him were made out of the remains of their defunct ancestors by a species of fermentation. Also that this creative chemical action was brought about by the deposition of the more recent remains upon the older, and that then the older forms successively came from beneath, and lay down on top, thus

producing a perpetual cycle. Let the reader suppose all this and he will gain some idea of the principles and methods commonly employed in lithology as well as in a greater or less degree in chemistry as applied to rocks.

This is no mere fancy sketch, but so far as can be done, by taking an illustration from a distinct science, shows some of the principles of lithology as taught *to-day* and some of the methods upon which rocks, *even now*, are classified.

These principles and methods were utterly rejected by me in 1878, which rejection was distinctly set forth in the abstract which Mr. Merrill criticised. Yet he demands that my work should conform to these very principles and methods!

So far as the real weight of Mr. Merrill's paper goes, the subject might be dropped here, but owing to the implied charges and the importance of the subject, it is necessary to carry the discussion further in a manner somewhat personal to the present writer. The important question is this: shall the theories and classifications put forth by Messrs. King and Zirkel with so much confidence and sustained by the influence of the United States Geological Survey, as well as apparently by that of almost every scientist in the country, be longer accepted or not? If correct they are to be accepted; but if I am right, their acceptance places an incubus upon American lithology and geology that only long years can remove.[1] The importance is infinitely beyond that of any man or set of men. It is a question of some of the fundamental truths of science.

THE LITHOLOGICAL COLLECTION.

When Prof. J. D. Whitney placed in my charge the lithological collections made by himself and by others in his employ, he stated to me that the rocks from California were in general the same as those collected by the Fortieth Parallel Survey, and described by Professor Zirkel. Professor Whitney expressed no dissent from the work of that survey, but on the contrary stated his belief that the arrangement and study would be simple and easy on account of Professor Zirkel's work.

[1] It is to be remembered that Richthofen and King's classification is not accepted in Europe and was not adopted by Zirkel until his visit to New York.

The collections were unpacked by me and the thin sections made under my supervision, while Professor Zirkel's "Microscopical Petrography" with Richthofen's Memoir was accepted in good faith as the basis for the work. It was found, however, that not only was it impossible for me to arrange any systematic series of specimens by following Professor Zirkel's work; but that also there appeared in the modern volcanic rocks no diagnostic characters established at one point in the series by Zirkel, that he had not abandoned elsewhere. After trying in vain to make out of Zirkel's work any system that should be applicable to the collection under my charge, I visited New York for the purpose of reconciling the apparent discrepancies of Volume VI, by a study of the actual specimens described.

Mr. Merrill has endeavored to show his devotion to the interests of science, as well as his disinterestedness in this case, by stating that his work was done at his own expense. Doubtless this remark was made in reference to one of mine [1] which applied only to my work upon the collection at Cambridge. The work in New York was done not only at my own expense, but also at the loss of a much needed vacation.

On studying the collection of the Fortieth Parallel Exploration, not only was the same lack of system observed in the arrangement of the rocks, and more strongly than before; but there were also many mistakes found even in the determination of common minerals, such as quartz, biotite, olivine, etc. Rocks widely differing in character had been classed together, while others closely allied were placed in distinct groups. Now it is true that many of the rocks placed in various groups appear to belong there; and therefore, as the collection is arranged, the work might appear on superficial examination, to have been well done. It seems that Mr. Merrill was able to make a partial examination only, but even then, taught as he had been by Zirkel himself, he was unable to agree in a number of cases with Zirkel's determinations.

On the other hand, there was not a slide or specimen in the collection, at the time of my visit, that was not examined by me. Especial attention was given to the classification and relations of

[1] Bull. Mus. Comp. Zool., 1879, v, 275.

the rocks, as arranged by Zirkel; and the specimens of each species were compared with one another, and with those of the other groups. Comparisons were also made with the undescribed rocks and sections.

Now Mr. Merrill's remarks regarding the want of publication of the "original paper" were uncalled for, since any competent, independent lithologist could have found out, as well as myself, the mistakes in the determination of minerals, and the unsystematic arrangement of the collection. Does Mr. Merrill mean, as his paper seems to imply, that he was unable to distinguish fragmental from non-fragmental rocks?

It may be said that no secret was made of the results of my examination, but they were discussed from time to time as the work went on, with the officers of the American Museum and with Mr. King. Also, in accordance with Mr. King's request, the writer then offered to point out actual mistakes in the determination of the minerals, and to indicate the positions to which he would assign the rocks in question. It is to be remembered that this was *prior* to the publication of Mr. King's Systematic Geology (Vol. I), a work largely dependent upon Zirkel's determinations and therefore based on the assumed correctness of many of the very points in question. But Mr. King then excused himself upon the plea of want of time, which he has never found since! Want of time to examine the foundations of a work upon which it may be said ten years of his life had been spent! This too, when the incorrectness of some of the statements as a matter of fact, and not of theory, could have been shown him in fifteen minutes!

The writer has been ready and willing, at all times, to go to New York and, in the presence of competent lithologists, endeavor to establish the correctness of his statements as to matters of fact; and that, as has been said before, was something, as it appeared to him, which any competent lithologist could have done for himself if he would.

The writer having thus discovered various errors in *matters of fact*, felt that he had a right to think for himself in *matters of theory*.

In order to understand how the work on the lithological collection of the Fortieth Parallel Survey was done, it is necessary to

retrace our steps. Mr. King adopted as the basis of his work on the volcanic rocks, Richthofen's "Natural System," which, however, in detail and interpretation the former appears to have carried out to suit himself. The rocks were classified and arranged according to the ideas of himself and his assistants in the field, and, later, Professor Zirkel was invited by him to study the collection microscopically.

All of Professor Zirkel's previous work had been done from a different method of classification; and it seems to be generally understood that, at the time he visited this country, he did not believe in the classification adopted by Mr. King. Be that as it may, he returned from his visit to Mr. King a professed believer in it. Of this visit Professor Zirkel wrote to Mr. King: "You then enabled me to become acquainted with the geological distribution, relative age, and reciprocal connections of the rocks; and if I have been able to study their mineralogical and chemical constitution from a geological point of view, and to present more than a sterile and dry petrographical description, the merit is originally yours. * * * You know that when we examined the collection macroscopically I entirely agreed with the determination and nomenclature you and your able colleagues had already arrived at in the field. There were only some doubtful occurrences, whose true nature could not at that time be decidedly cleared up. Now, after having carefully studied more than twenty-five hundred thin-sections under the microscope, I have only to testify again that your original designations should almost never be altered or corrected."[1]

After the preliminary examination, a selected collection, I understand, was taken to Europe, and the sections made there which were then studied microscopically. The results of the microscopic examination, together with much material derived from Mr. King and his assistants, were published in the "Microscopical Petrography,"(Vol. VI). According to that work the number of specimens reported upon out of a collection numbering 2823 was 670; this last number however ought to be increased somewhat on account of some mistakes made in the numbering in the volume. The whole number of thin sections deposited at the time of my

[1] Letter to the Geologist-in-charge, Vol. VI, p. xv.

examination, in the American Museum, with the hand specimens was 914; with an additional collection, said not to have been studied by Zirkel, of 434 slides of rocks purporting to be sedimentary or fragmental, that were also examined by me. It may be noticed that Mr. Merrill states that he was informed that the slides were studied separately from the hand specimens; while I was given to understand that the hand specimens corresponding to the described sections were taken to Europe. I was further repeatedly informed by Mr. King, on inquiring for missing slides and hand specimens, that they probably had not been returned by Professor Zirkel, who had retained some for further study.

That Mr. Merrill was misinformed is to be seen from Zirkel's own statement, which freely translated from the French reads thus: "I went myself to America in order to see the collection and to chose the specimens (échantillons) to be sent to Europe." (l. c. p. 18).

It would seem that the arrangement, classification, and in fact everything except the simple microscopic descriptions came primarily from Mr. King and his assistants; Professor Zirkel's manuscript even being changed in some cases by them before publication.

It does not seem to be right, in the light of the methods employed, for Mr. King to claim that the correctness of his work is proved by Zirkel's observations (I, pp. 109, 551), and for Professor Zirkel to claim that the correctness of his work is likewise proved by Mr. King's observations (VI, p. 132).

Had the entire, not a selected, collection been placed in Professor Zirkel's hands, without his having the slighest clue to their field relations, or to Mr. King's conclusions, and had he, independently, separated the rocks into divisions which corresponded with those of Mr. King, then the latter's views would have been greatly

[1] Les Roches Cristallines de la Coupe du Fortieth Parallèle au Nord Ouest des États-Unis. Par le Dr. F. Zirkel. Annales de la Société Belge de Microscopie, 1878, IV, 17–109.

See also Ueber die krystallinschen Gesteine längs des 40 Breitegrades in Nordwest-Amerika, F. Zirkel. Berichte über die Verhandlungen der königlich sächsischen Gesellschaft der Wissenschaften zu Leipzig, 1877, II, 156–243.

strengthened. As it is, the writer fails, except in some cases, to see wherein Professor Zirkel's work is anything more than a simple description of the microscopic characters of a series of rocks classified by Mr. King.

Occasion arose in 1879 for the publication of the results of my study of the collection under my charge, and in connection with it, it also became necessary to give briefly the reasons why the results of the Fortieth Parallel work could not be accepted by me, A minor portion only of the abstract was devoted to the work of Messrs. Zirkel and King. It was then *hoped, not promised*, that within a year the work upon the Sierra Nevada rocks would be ready for publication. But my time, since the publication of the abstract, has been largely absorbed in other duties, while the material to be studied has greatly increased and the plan of the work been entirely changed. Mr. Merrill was informed by me as to the reason of the delay at the time of his visit to the Museum of Comparative Zoölogy, and he seems to have overlooked the fact that the publication of the results of the Fortieth Parallel Exploration was delayed seven years beyond the time it was originally promised.

At no time has the contemplated publication been abandoned; but heretofore it would not have been proper to publish the material contained in the present paper, independently of my completed work.

In what is said in this paper no personal reflections are intended to be cast upon those whose work is criticised; but it is intended to criticise the methods, the accuracy of the statements made, and the manner in which it has been sought to substantiate the work.

If now the report of Professor Zirkel, together with those of Mr. King and his assistants, be examined, one of the first things observed is "the fine silvery-white, scaly mica-slate" which "bears such a striking resemblance to the well-known beautiful paragonite slate from Monte Campione, near Faido, at the St. Gotthard, Switzerland, that it is difficult to distinguish one from the other, — the more so since it contains excellent large crystals of pale-blue disthene (cyanite)," (VI, p. 28). Of this Messrs. Emmons and

King state that the rock contains cyanite (II, p. 270), and was richly charged with "minute crystals of cyanite" (I, p. 43). King overlooks the fact that if his statement was correct, the cyanite would be seen in the microscopic section, in which Zirkel had expressly stated that none could be seen.

This rock was set apart as an abnormal and peculiar occurrence in the Fortieth Parallel district, on account of Professor Zirkel's statement as to its mineralogical composition. Accounts of it have been published in Germany and Belgium and, together with the Haüyne, to be spoken of later, have crept into text books and various publications both in this country and in Europe. In the face of all this Mr. Merrill, while admitting that the rock contains no cyanite (disthene), thinks it a matter of no importance, and insinuates that the next fragment of the rock might contain this mineral.

He overlooks the fact that the writer had stated that the rock was a mica schist "similar to many mica schists in New England, and except that its color is grayish-white, has no resemblance to the paragonite schist from St. Gotthard;"[1] that is, the rock is not a *paragonite schist* but an *ordinary mica schist.* Furthermore, the rock and the section both give evidence that they were the ones described by Messrs. Zirkel, King, and Emmons. Also neither this specimen (Col. No. 2647) nor any other in the collection, when examined by me, gave the slightest evidence of bearing cyanite like that from St. Gothard. Neither was any evidence obtained that other specimens of this rock could be procured that did contain cyanite, or that were composed of paragonite. Hence, I claim that the rocks of the Red Creek district can no longer be kept in the abnormal position in which they were placed, through Zirkel's mistake, as an occurrence of hydrous micaceous rocks on the Fortieth Parallel — the only occurrence.

Again, this schist has an important bearing upon the question of the care and accuracy of Zirkel's work; and taken in connection with other similar errors of fact, it enables one to decide regarding the reliableness of other portions of the work concerning which, from their theoretical bearings, there would be greater chance for difference of opinion.

[1] Bull. Mus. Comp. Zoöl. 1879, v, 284, 285.

Granite.

Looking next at the granites it is to be seen that in 1870 (Vol. III, pp. 1-9), all the granite in the district (at least all between the Sierra Nevada and the eastern side of the Wahsatch range) covered by the Fortieth Parallel Survey was regarded as eruptive, and of late Jurassic age. It was promised that reasons for this view would be fully given in Volume I. It was also stated that the last of the publications would be issued in 1871. From this, as well as from other data, it is to be understood that the field work had ended prior to the publication of Volume III (1870).

In 1876 Volume VI, by Professor Zirkel, was issued. The arrangement of the granites was found to be changed in this, but no reason for the change, nor any mention of the former view was given. In this the granites were held to be thus classed: (Vol. VI, pp. 39, 40, 58, 59.)

I. Metamorphic granites.
II. Older eruptive granites of ante-Jurassic age.
III. Younger eruptive granites of Jurassic age.

It was again stated: "The full details of the reasons of this assignment will be found in the chapter upon granites in Vol. I of this series." (VI, p. 39.) It was further remarked that "Clarence King has long since shown that the eruptive Jurassic granites, and only these, are characterized by the presence of macroscopical titanite." Where? Professor Zirkel also shows by the following remark that his classification of the granites was derived from the work on this collection alone, and not from his previous studies: "It should be particularly stated that the described contrasts are valid only for the examined rocks of the Fortieth Parallel, and that it is not allowable to generalize from them for other countries" (VI, p. 59). Since there is no *a priori* method whereby eruptive granites can be distinguished from supposed metamorphic ones microscopically, and Professor Zirkel had not studied them in the field, the classification must have come originally from Mr. King and his assistants.

Turning now to Vol. I, published in 1878, it is to be seen that Mr. King divides the eruptive granites alone into four types, remarking, "This classification, based upon field observations,

is interestingly carried out by Zirkel, whose microscopic examinations in every way confirm the field arrangement." (I, 107-109.) He also states: "There is absolutely no evidence whatever in favor of the belief of granitic extrusions later than the Archæan age" (I, p. 111). This is the result of the promise to show why all of the eruptive granites were regarded in 1870 as Jurassic and in 1876 as part pre-Jurassic and the remainder as Jurassic. No mention is made of the previous views as such, nor are any reasons for the changes given, that the writer can find. No evidence is given by Mr. King to prove the correctness of his assignment of the granite to the Azoic (Archæan) derived from the study of the rocks in his district, beyond lithological evidence, — evidence which he condemns (I, p. 111). Mr. King assumed that granite in solid points has been thrust up through the Paleozoic and more recent rocks, without giving any facts to sustain the view. He states that in many cases the granite formed islands in the Paleozoic sea while deposits of immense thickness were formed around them, but generally fails, until he comes to the more recent rocks, to show that the supposed surrounding rocks contain the debris of the island ones.

In fact, his statements of his geological dynamics and the geological age of the granites are left as unproved assertions. Had any evidence been observed it is difficult to suppose it would have been passed over in silence.

The rock from Cherokee Butte Mr. King calls a gneiss and states that "Zirkel calls attention to the condition of the quartz which is made up of small worn and rounded fragments" (I, p. 33). This rock Zirkel called a granite and, instead of stating what King says he did, he remarked: "The course of these lines gives the quartzes something the appearance of fragments or even of worn fragments" (VI, p. 55).

The Wahsatch granite in 1876 presented "eminently characteristic types of eruptive granites" of Jurassic age (VI, pp. 50-52); but in 1878 it was referred to the Azoic. Statements of a series of geological phenomena of the most remarkable kind were advanced without proof of their correctness, in order to sustain the latter view of their age (I, pp. 44-51, 122–125, 174, 184). These tatements were in some measure criticised by the Director of the

Geological Survey of Scotland, who also remarked regarding one of the geological sections: "The section, I submit, involves a series of physical impossibilities, or at least of such glaring improbabilities as to demand full and incontrovertible proof in its support. For, in the first place, it requires us to believe that the cliff against which the Paleozoic sediments were deposited, must have been at least *twelve miles high!* * * * In the next place it necessitates the admission that this stupendous precipice was subsequently *turned over on its back* carrying with it the adhering later rocks." (Amer. Journ. Sci. 1880, (3) XIX, 363-367.) The granite of the Wachoe Mountains was held to be Jurassic in 1876 (VI, p. 49), but Azoic in 1878 (I, pp. 59-60).

The granite at Agate Pass was regarded by Zirkel as an excellent example of the titanite granites (Jurassic ?) (VI, p. 48), but King is inclined to object to this determination and prefers "to consider it rather as a diorite than as a granite;" that is a granitoid modification of the surrounding "diorite" (I, p. 72). When the writer studied these rocks in the summer of 1878 he saw no reason to dissent from Professor Zirkel's determination of the above granite as granite, but he did see reasons for objecting to the assignment of the Agate Pass "diorite" to the "diorites". He then held that this "typical diorite" was a granite holding abundant quartz and biotite, as well as hornblende, plagioclase, and orthoclase (VI, p. 91, No. 184, Col. No. 1858).

He would explain the resemblance of the above granite and "diorite" in the reverse way from Mr. King, that is: the diorite is a modification of the granite. In like manner the "diorite from Mill Creek Cañon, which Zirkel says "is very peculiar," and which Mr. Emmons thinks may be best classed as a diorite, although it presents many features of a fine grained granite, Mr. King holds is an intermediate link between granite and diorite (VI, p. 91, No. 183, Col. No. 1823; II, p. 571; I, p. 74).

The writer regards this as a fine grained granite, the same as the fine grained modifications of granite which can be seen at Rockport, Mass. These modified fine grained portions of the Rockport granite are continuous with the main granite (coarse grained) mass.

The Toyabe range granite was described by Emmons in Vol. III (p. 323) as Jurassic, and evidence that appears to be sound given in support of the view, but in Vol. I (pp. 75-77) it is referred by King to the Azoic, without giving any evidence therefor or mentioning the former view.

In 1877 the Ravenswood Peak granite was regarded as post-Azoic, and from the evidence given it was probably eruptive since the Carboniferous (II, pp. 637-639). However, in 1878 King states "there is little doubt of its Archæan age, but its reference to that period is only on general lithologieal grounds" (I, pp. 78, 108).

A granite dike in the Havallah range is regarded by King as an exceptional occurrence in his district, but the writer thinks that the difficulty can easily be explained (I, p. 81; II, p. 675; VI, p. 46, No. 77, Col. No. 1488). Previously to the publication of Mr. King's report, the writer satisfied himself that the section (1488) described by Zirkel never came from the hand specimen (1488), a not uncommon mistake, apparently, in Zirkel's work on this collection.

The Pah-Ute granite, Professor Zirkel stated in the manuscript report, was doubtless eruptive, but in the published report (Vol. VI, p. 44) he was made to say that it "doubtless belongs to the metamorphic group."

The younger eruptive granite of the Pah-tson was regarded by Zirkel as Jurassic (VI, p. 43), by Emmons and Hague as probably Jurassic (II, p. 778), but by King as Azoic (Archæan), (I, p. 92). This change of views between 1877 and 1878 as usual is not noticed in Vol. I, and no proof is advanced in behalf of the latter view. This is the case with the Pah-supp granite, and with but few exceptions, so with all the granite westward to the Sierra Nevada: Jurassic of Zirkel, Emmons, and Hague, but Archæan of King who tells us Zirkel's "microscopic examinations every way confirm the field arrangement."

Syenite.

Mr. Merrill does not take up the question of the syenites; although, since there were but two specimens, he had the informa-

tion he professes to have desired as to the exact specimens in question. The first (No. 152, Col. No. 1826, VI, p. 81), is very greatly decomposed, so much so that the writer does not regard it as a suitable specimen for classificatory work. He holds that the quartz is an alteration product, and that the present structure of the hornblende is also the result of alteration, and not original as Zirkel holds it to be. The same may be said of the quartz in the second syenite (No. 153, Col. No. 1853, VI, p. 82), while the rock itself is much altered. The writer regards these as old and altered andesites.

Diorite.

The diorites of the Fortieth Parallel described by Zirkel are regarded by the writer as a varied body of rocks, having but few characters in common, except the hornblende. In part of these he holds that the hornblende is an alteration product, and in others an original one. Nos. 162 (235), 172 (1497), 173 (1500), 185 (2221), 188 (1637) are regarded by the writer as old and altered andesitic rocks. No. 162 contains much alteration quartz, and in No. 185 may be seen the remains of well outlined augite crystals, which appear to have been unnoticed by Zirkel.

The diorite of Kawsoh Mountains (VI, p. 86, Col. No. 688), the writer regards as an old basaltic rock — a diabase, macroscopically and microscopically. It contains some augite which was unnoticed by Zirkel. Nos. 164 (903) and 167 (1386) are regarded as metamorphosed fragmental rocks and therefore not properly classed as diorites even by Zirkel; 164 is probably an andesitic ash.

The other specimens of "diorite" the writer is inclined to consider as belonging to the granitic and felsitic rocks. Some cases are indeed doubtful, as Nos. 163 (836), 174 (1513), and 187 (2723), which are so much altered that their diagnosis is difficult, as Zirkel evidently found. In fact it may be stated, that it is believed that many of the mistakes arose from the collection of surface and altered specimens, which were unfit for the classificatory work required. This was a matter of more importance, since Zirkel appears to have proceeded, in the larger portion of this work, upon the principle that these rocks were created in the

state in which he found them. Of No. 168 (1389) Zirkel stated: "The rock has no mica" (VI, p. 87), but the writer found mica in it. So, too, quartz was found in No. 187 (2723) although Zirkel says it contains no quartz. The hornblende in this rock is regarded by me as an alteration product.

In the cases of the mica-diorites, Nos. 169 (1378), and 170 (1380), it would seem as if some mistake had been made in the hand specimens, since they show but little trace of mica, and resemble some metamorphic schists.

Hornblende Porphyry.

The specimens of hornblende porphyry, Nos. 189 (1435), 190 (1418), are regarded by me as but little altered andesites which would never have been separated from the andesites if it had not been for theoretical views regarding their age. For Mr. Merrill's benefit the writer would state that he does not believe in any "ante-Tertiary precursor" of andesite, basalt, trachyte or rhyolite. What he means when he speaks of a rock being an *altered* andesite, basalt, etc., is that the rock originally was the *same* as andesites, basalts, etc. of the present day are; and that the difference is in general owing to the alteration they have undergone since their consolidation. These altered rocks are sometimes old (Mesozoic, or older), sometimes not. As a rule, however, an old rock, especially if basic, is an altered one. The writer, agreeing with Professor Dana, denies that there is any valid lithological distinction between Tertiary and pre-Tertiary rocks, beyond this: other things being equal, the greater the age the greater the alteration. The writer holds that during the earlier geological ages basalts, andesites, rhyolites, etc., existed, which in their unaltered state were identical with those of the present day; and that it is these rocks which, in their altered state, are now classed as diorites, melaphyrs, diabases, porphyries etc., etc.

Diabase.

Nos. 191 (750), 192 (754), 193 (761), 194 (764), 195 (773), I hold are altered andesites, containing much secondary quartz. Nos. 196 (520), 197 (532), 198 (533), and 199 (562), are taken by

me to be unaltered basalts, of which No. 197 is almost identical with No. 551 (565), (VI. p. 237), while No. 199 has the same "globulitic half glassy base" as the basalt No. 551. All these numbers (196, 197, 198, and 199) form with the basalts of Zirkel, Nos. 551 (565) and 556 (689), a common series of rocks closely alike, and it seems to me they should never have been separated.

No. 200 (1285) I regard as a diabase, identical with that described by me, as forming the edge of a large mass of diabase at the "Powder House" Somerville, Mass., the quartz in both cases being a product of alteration (Proc. Bost. Soc. Nat. Hist. 1877, XIX, 231).

No. 201 (1387) is a diabase almost identical with the rock forming the large dike at the "Pumping Station" Brighton, Mass. (l. c., p. 231).

No. 202 (1680) can be regarded as a coarsely crystalized melaphyr or a fine grained diabase, preferably the former; while Col. No. 1704 (VI, p. 102) is a diabase.

The terms melaphyr and diabase are used by me to indicate altered, and therefore generally old, basalts.[1] It may be said, once for all, that my criticisms against Zirkel's species diabase, melaphyr, etc., were made, because he uses the terms as indicating species distinct from the rocks whose alteration forms I believe them to be; therefore, as was proper, it was pointed out to what species they were supposed to belong to as varieties.

Had Mr. Merrill used more care he would have seen that his criticism relating to diabase and olivine diabase, basalt, and augite-andesite does not apply to work in which such mineralogical definitions of rocks are especially rejected; but that it does apply to Zirkel's work, since in his report he has given, without distinction between them, under diabase, basalt, and augite-andesite, rocks both "olivine-bearing" and "olivine-free."

My statements regarding gabbro and diorite will be maintained in proper time and place; but instead of confining myself to Mr. Merrill's bald definitions, I shall try to show to what rocks these names have actually been applied.

[1] The use of these names is not advocated by me, but the present state of lithological science demands them, therefore I comply, but endeavor to show their relations to the unaltered rocks.

Melaphyr.

No. 209 (407) is held to be an altered andesite, containing much quartz as a product of decomposition. The larger crystals and the blackish grains described by Zirkel, which he regards as augite or unknown, I hold are the remains of the hornblende so common in the andesites (Bull. Mus. Comp. Zoöl. 1879, V, 280, 281). Nos. 203 (313), 204 (314), 205 (321), 206 (326), 207 (332), 208 (340), are regarded by me as melaphyrs, that is, altered basalts, and they contain much alteration quartz. No. 208 (340), I hold to be a melaphyr as defined by me.

Propylite.

Since this rock has attracted much attention from its relation to silver mining and from its forming the base of the classification proposed by Richthofen and adopted, with modifications, by King, it demands a somewhat extended notice.

It is first necessary to give some lengthy extracts from the writings of Messrs. King, Hague, Emmons, and Zirkel, in order that its "habitus" may be understood. The italics in the extracts are mine.

Mr. King states (I, p. 550, 551): "The science of petrography offers no more interesting example of the delicate shades on which lines may be successfully drawn than the case of this rock. Richthofen's subtle observation and great practice as a field geologist enabled him to detect the essential characteristics of the *habitus* of this rock, while at the same time he clearly saw its relations to the other hornblende-plagioclase species. The subsequent microscopic analysis of the rock by Zirkel has firmly established its independence as a species. The English petrographers especially have been inclined to deny its existence; but the *shade of habitus* upon which Richthofen founded his first assertion of the species is *so evident in the field* of the Fortieth Parallel Exploration that there has *never been the slightest doubt* on the part of Messrs. Emmons and Hague and myself as to the identity of propylite. When the large collection of specimens brought in by us came to be studied microscopically by Zirkel, it was found that we *had never wrongly assigned a specimen to*

propylite. In certain instances the microscope revealed the presence of minute grains of quartz, and the rock thus characterized came to be classed as quartz-propylite; but there *was never any doubt* as to the *generic nature* of the rock. There *was not a solitary instance* in which the rock by *us called propylite* proved to be either diorite, andesite, or plagioclase hornblende-trachyte. I am *careful* to mention this *fact*, not as a *guarantee of the correctness of our determinations, for that has been placed beyond question by the microscopical analyses of Zirkel*, but because later in this chapter I shall have occasion to discuss what constitutes a species of volcanic rock, and the factor which *habitus* must necessarily play in classification."

Now Professor Zirkel says (VI, p. 132): "Perhaps it may not be superfluous to *insist* that *all the rocks described* in the foregoing pages *as propylites* and *andesites* were first referred to one or the other group by *geological observations in the field*, and that the petrographical diagnosis and the classification of them *have not been influenced by any artificial point of view or preconceived opinion.* The examinations have proved that in *every rock the geological and petrographical differences perfectly accord.*" The assertions in the above quotations do not seem to me to be sustained by the facts, as I think will appear.

Regarding King's statements about himself or his assistants never wrongly assigning a rock to propylite in the field, and that the microscopic work always agreed with the field work, it is only necessary to quote their own statements. It will of course be admitted that they have not taken pains to afford contradictory evidence.

Mr. King states in the same chapter, the italics as before being mine (I, p. 567):

"The *field habit* of this dacite is *decidedly more propylitic than andesitic.* . . . in the field and in hand specimens *we were often unable to distinguish between it and quartz-propylite.* But in the case of this outburst it might *readily be mistaken* for the *neighboring quartz-propylite.*"

Zirkel describes this rock as "a dacite, which envelopes so many strange fragments of another variety of dacite as to form

a real breccia" (VI, pp. 140, 141; Nos. 280 [1866], 281 [1884]). This dacite the writer regards as identical with specimens obtained by Mr. W. A. Goodyear from a quarry near the Manhattan Mercury Mine, Johntown, Yolo Co., California, now in the Lithological Collection of the Museum of Comparative Zoölogy. These specimens are composed of fragments of pumice (the "strange angular particles of a dull, milky looking substance" of Zirkel), andesite, quartz, feldspar, etc., cemented by a fine mud and water-deposited silica. The rock is composed simply of detritus that has been somewhat acted upon by water, etc., and belongs with the clastic rocks.

It may be remarked that these rocks are about as unlike the propylite of Richthofen as any rock could well be. It is perhaps permitted to me to speak somewhat authoritatively upon this point since there is under my charge a collection of propylites, determined as such by Richthofen, from the typical localities of Silver Mountain, Washoe, etc.

But to continue, King states of a rock from another locality (I, pp. 569, 570):

"Of all the dacites, in external habitus this *most closely resembles the propylite type*, and *it is by mistake colored upon our geological map as quartz-propylite*, close examination having been made too late for a change." The analysis, too, is given in the table of analyses No. VIII (analysis No. 132) amongst the quartz-propylites, and not in Table IX where Mr. King states it is to be found.

Speaking of this dacite of Professor Zirkel, Mr. Hague remarks (II, pp. 844, 845): "It is regarded as belonging to the *quartz-propylites*, and has been represented as such on the geological maps, although *microscopical analysis* indicates that it is more closely allied to dacites, the quartz bearing variety of the andesites. The rock has the *characteristic greenish-gray groundmass of the typical Washoe propylite*, with the same arrangement and structure of the plagioclase and hornblende and *the same general field aspect*." Yet this is a rock that Zirkel describes as "one of the *most typical dacites*" (VI, p. 139, No. 276, Col. No. 814). It is regarded by me as an old rock probably long antedating the Tertiary, and would by most lithologists be called a quartz or a granite porphyry.

Again Mr. King remarks (I, p. 629): "Certain of the *plagioclase trachytes*, *andesites*, and *propylites bear a striking resemblance* in the relation of their secreted minerals to the groundmass by which the resulting porphyries are *puzzlingly similar*." Of his observations he again says: "But as since the period of these observations, quartz-propylite and dacite have been separated, a doubt is thrown over the reading of that locality" (I, pp. 683, 684).

Mr. Emmons says of a "rough gray sanidin-trachyte": "Both in habit and mineralogical constitution, this rock shows close affinity to propylite, and would be so classed, but that the orthoclase predominates over the triclinic feldspar" (II, p. 580). Zirkel states that the same "rock exhibits a considerable measure of similarity to propylite, and it would be so classed if orthoclase did not unquestionably predominate" (VI, p. 153). King also says of the same: "The groundmass resembles that of propylite. * * * But for the predominance of sanidin over plagioclase, the rock, from the peculiar disposition of the hornblende, would be closely related to the propylite" (I, pp. 598, 599).

The studies of the writer upon the propylites of the Fortieth Parallel Survey, as well as upon the collections made about Silver Mountain, in the Washoe District, and elsewhere, convinced him that the true propylite is an altered andesite standing in similar relations to it that melaphyr and diabase do to the basalts. No definite line can be drawn between the propylites and andesites as the alteration products are the same, the difference being simply in the degree of alteration.

The hornblendic dust; the color of the rock, of its groundmass, and of the hornblende; the microscopical epidote; the absence of a glass-bearing groundmass; the rarity of augite, the structure of the hornblende, etc., are in my opinion the result of alteration. Professor Zirkel states the reverse regarding part of these characters, yet he described rocks as andesites that cannot be distinguished by me either macroscopically or microscopically from others belonging to the same district that he described as propylites. In the well marked specimens there is no difficulty in classing in the field, or by a mere examination of a hand specimen with the unaided eye, a rock as a decomposed or altered andesite

(propylite) or as an unaltered andesite; but this does not entitle propylite to be regarded as a distinct species any more than the altered form of any rock or a decomposing specimen of any animal should be. It is at best a variety merely, and not a strongly marked one at that.

Mr. King points to the fact that the propylites are generally decomposed (I, p. 557), and it is to be remarked that every one described by Zirkel was more or less decomposed. It is further to be noticed, as bearing upon this, that glassy propylites are unknown (I, p. 722), and "a glass-bearing propylitic groundmass has never been found" (VI, p. 139). If it is necessary to give names to the altered states of rocks and regard them as varieties merely, the writer accepts the use of the term propylite; but he is opposed to its erection into a species holding equal rank with andesite, basalt, and rhyolite.

Professor Zirkel states that "the brown hornblende of the andesites never produces secondary epidote" (VI, p. 112), and then points to the alteration of an andesitic hornblende to epidote later (VI, pp. 130, 133). He states however that no other occurrence has been observed. The writer claims that such alterations are comparatively common in the more altered of the so-called andesites, not only in Sierra Nevada rocks, but also in those of the Fortieth Parallel collection, described and undescribed by Zirkel.

Of the quartz-bearing propylites, No. 226 (1641) I hold is an old felsitic rock; also that Nos. 228 (1863), 229 (1868) are the same as some of the rocks Zirkel has described as granite-porphyry; that No. 227 (1453) is an old, decomposed, fragmental rock, while Nos. 230 (1869) and 231 (173) are altered andesites containing alteration quartz. They are the only ones, even if species are to be founded on alteration characters, that are entitled to be called quartz-propylites. This then disposes of all of the quartz-propylites except No. 232 (317). The writer holds that the section Col. No. 317 never came from the rock with the label No. 317, but that it was made from some old, altered rock. (See also Vol. I, pp. 554, 566; Vol. II, pp. 841, 842). My conclusions regarding these quartz-propylites are borne out by the fluid inclusions in the quartz found in them, the chemical analyses, and by the fact that it does not appear that their age is known.

Had Mr. Merrill rejected the analyses under No. 140 (Table IX) which are from a rock regarded in the reports as an abnormal form, he might have had six analyses of propylites and six of andesites, and perhaps have seen their chemical relations better.

Andesite.

The separation of the andesites into hornblende and augite-andesites is opposed, for the reason that the two minerals, hornblende and augite, are of diverse origin in these rocks, as set forth by the writer in the previous publication.[1] Professor Zirkel's hornblende-andesites usually contain augite and the augite-andesites, hornblende. The division between them is no more a geological one than it is a lithological one, Zirkel's statement that the augite-andesite is younger in age than the rhyolites, being, according to King himself, an erroneous one. While Zirkel places the augite-andesites as geologically closely connected with the basalts, King holds that they are separated from them by both the trachytic and rhyolitic eruptions (I, p. 576, VI, p. 219).

While Zirkel states that the augite-andesites "will always upon merely a macroscopical examination be classed as basalts," the writer holds that such confounding of the two rocks on macroscopic study ought to be comparatively rare, and then as a rule, only in the case of specimens closely related to the basalts. Herein is one of the striking differences between our methods of work and classification.

Professor Zirkel states that in the andesite, Col. No. 17 (VI, p. 122), no quartz could be found, but quartz in comparatively large amounts was seen by me in the section. This quartz is regarded, from its microscopic characters, as an alteration product.

No. 243 (272) I take to be a volcanic tufa (andesitic) and to belong with Zirkel's clastic rocks.

Dacite.

Some of the dacites have been spoken of under the head of propylite and need not be referred to here. Of the others No. 271 (90) I regard as a rhyolitic tufa containing andesitic frag-

[1] Bull. Mus. Comp. Zoöl. 1879, v, 280, 281.

ments, and therefore belonging with Zirkel's clastic rocks. Nos. 277 (897), 278 (1783) I hold are fragmental rocks, of which the latter at least is old. They both appear to be formed from rhyolitic (felsitic) debris. The remainder are considered by me, with but one exception, to be rhyolitic (including felsitic) rocks of various ages. No. 279 (1852) is the exception. This I hold is an old andesite containing quartz as an alteration product, and would therefore be the only dacite, properly so-called, according to King and Zirkel's definition. Since the rock is an altered andesite, and the quartz a secondary product, there seems to be no use for the term dacite here to indicate a quartz-bearing andesite, unless secondary minerals are to be raised to the same rank in nomenclature that original ones hold; and in this case the rock is the same as the only real quartz-propylite, *i. e.* an altered andesite. We thus have two names to indicate the same thing.

Trachytes.

Nos. 289 (293), 323 (2589), 324 (2611), 325 (2601), 327 (2616), 329 (2617), 330 (2620), 332 (2621), I hold are both macroscopically and microscopically basalts, and except one or two which are allied to the andesites, are well marked basalts. These in part belong to the same series as No. 612 (2619) a resemblance, which Professor Zirkel also noticed (VI, pp. 161, 251; II, pp. 169–173). The number of the rock 612 (VI, p. 251) is erroneously given as 328 on page 161 of Zirkel's report. It should be noticed here that while Zirkel states that this rock contains no olivine, this mineral can be seen in the section in considerable quantity (VI, p. 251). The quartz, except some alteration quartz, observed by Zirkel in these rocks, is held by me to be of prior origin to the consolidation of the basalt, that is, it is foreign to the rock. However, had these rocks been assigned to their proper species, Mr. King might out of them have made his quartziferous division of the basalts, and not plunged the rhyolites into the basaltic lake as he has done. It is further thought that the analyses of two of the above rocks (the only two analyzed) given in Table X of King's report (Analyses 143 and 144) bear out my views as to their relation.

In describing the above rocks, Zirkel stated of No. 289 (293): "There is no olivine which expresses the absolute separation of the rock from basalts" (VI, p. 148). Yet No. 325 (2601) and 332 (2621) are described as trachytes containing olivine. He has indeed explained the presence of the olivine in 325 as a crystallization from the rock to neutralize the quartz contemporaneously produced. When it is realized that the quartz is not a crystallization from the rock, but foreign, like a stick in dough, and that 332 contains olivine but no quartz, some idea of the value of his theory can be gained. Furthermore, a number of his basalts are said to contain no olivine. How then does the absence of olivine absolutely separate a rock from the basalts even in his own work?

The slide No. 326 (2614) is not believed by me to have come from the rock with the label Col. No. 2614. Apparently the slide and the hand specimen are the ones described by Zirkel, King, and Emmons. Had the discrepancy between the slide and hand specimen been observed, Mr. King would probably have been saved his difficulty in classifying it (I, pp. 547–549; II, p. 170; VI, pp. 160, 161).

Professor Zirkel described in Nos. 300 (1664), 301 and 613 (2623), some Prussian blue grains which he thought in all probability belonged to Haüyne (VI, pp. 151, 152, 251; I, p. 596; II, pp. 568, 598). Grains of the same kind were found quite commonly in the sections of rocks from the Cordilleras, in the Lithological Collection of the Museum of Comparative Zoölogy, also in many European sections in that collection. They seem never to be in the groundmass, but always above or below the section in the Canada balsam; frequently they were observed in the balsam at a distance from the section. I can but regard them as impurities that have got into the balsam at some time previously, or else upon the section during its preparation. The most probable explanation for the California occurrences is this: the chips to be ground were placed in small boxes, many of which were made of paper colored by ultramarine. The paint readily rubbed off upon the fingers and specimens, and microscopic examination of prepared slides containing this powder in balsam showed that its characters are the same as those of the supposed Haüyne

grains. Part of the collection from the Fortieth Parallel was in similar boxes, which would account for the presence of the grains in the slides examined by Zirkel. Whether the occurrence of these grains in the European sections can be accounted for in this way or not can not be told; but one thing is certain, they are foreign to the rock slice.

Since these Haüyne grains occurred, according to Zirkel, in only three specimens, and all these occurrences were rejected by me, Mr. Merrill had "here the elsewhere vainly coveted knowledge of the precise specimens in question." But not a word on this!

Rhyolite.

Nos. 464 (1799) and 303 (1805), (VI, pp. 152, 194) were collected by Mr. James D. Hague in the *same locality*, on the *same day*, and *numbered consecutively* in the field as 2359 and 2360. As will be seen above they were *not numbered consecutively* in the Cabinet. Now so far as the writer could find, the hand specimens and sections are identical, that is as identical as two specimens of the same rock can be. The only observed difference being that the section 303 (1805) is a little thinner than the other. Yet while Zirkel describes 464 (1799) as a rhyolite the other is said to be a *beautiful trachyte*!

Belonging in the same series with these two rocks are Zirkel's granite-porphyries 120 (1961) and 133 (2054), his trachyte 308 (1895) and his rhyolites 355 (777), 469 (1918), and 487 (2205), (VI, pp. 62, 64, 154, 172, 195, and 200). The last diffiers simply in the presence of the augite.

No. 340 (270) I hold is a much weathered, surface specimen, belonging to the andesites. No. 369 (889) is said by Zirkel to contain "very small fragments of strange rhyolitic particles" (VI, p. 174). These fragments the writer takes as andesitic fragments, and claims that fragments of andesite can be seen in 351 (558), 352 (557), 362 (852), 370 (892), 371 (893), although Zirkel says they are wanting in the last two, and appears not to have seen them in the others.

Other rhyolites containing andesitic fragments are Nos. 416,

(1446), 423 (1471), 473 (2063), 491 (572), 492 (809), etc. Zirkel calls the andesitic fragments in No. 492 (809) "felsitic rhyolite." No. 473 (2063) is said to contain *no biotite*, but it *does hold biotite* that can be seen not only with the microscope but also with the naked eye (VI, p. 196). The "pieces of dark-gray rhyolite," *b* in 479 (2130), are apparently fragments of a melaphyr, but it is somewhat doubtful if the section came from Col. No. 2130.

Nos. 364 (870) and 367 (878) are regarded by me as andesites; 365 (872) is too much decomposed to be sure about it, but I consider it to be a decomposed basaltic rock, as is also apparently Col. No. 868, although no section of it could be found. Col. No. 868 and Nos. 365 (872) and 366 (875) are said on the labels to overlie the basalt. Col. No. 875 is a rhyolite, hence here is an exception to Richthofen's law although King says he found none. The section No. 380 (948) is probably not from hand specimen 948. Rhyolite No. 384 (980) is the same as the chalcedony No. 656 (980), (VI, pp. 177, 271).

No section of No. 386 (913) could be found by me, but the hand specimen is evidently a basalt. No. 423[1] (1518) is evidently an andesite, as Emmons suggests, containing a fair amount of plagioclase, but none of the quartz it was said to hold by him could be found by me (I, p. 188, II, p. 642). No. 467 (1876) is held to be a fragmental rock.

The divisions into which Zirkel has separated the rhyolites are not regarded by me as natural ones, since no line can be drawn between them except an arbitrary one. Mr. King is mistaken in his statement that the "nevadite" has only slight traces of "vitreous binding material" (I, p. 722). It is furthermore true that many of the porphyritic and felsitic rhyolites contain less glass than the "nevadites."

The quartz found in the rhyolitic groundmass is regarded by me as a devitrification product, and therefore not a suitable mineral to found lithological species upon (II, p. 463).

On page 117 of Vol. VI, Professor Zirkel in speaking of the fluid inclusions in the quartz of No. 226 (1641) stated: (The italics are mine.) "As respects this point, indeed, the quartz of this

[1] This in Vol. VI is a misprint for 433, since No. 423 occurs on page 186.

Tertiary propylite, and, without an exception, that of all which follow, *behaves* exactly like that of the ante-Tertiary dioritic porphyries, and *differently from that of all other Tertiary quartziferous rocks, dacites and rhyolites, which only contain glass inclusions.*" On page 197 [No. 475 (2059)], he again says: "This *rhyolitic* quartz individual is the *only one* of the thousands and thousands that have been examined with the microscope which has been found to bear *fluid inclusions.*" On page 201 [No. 488 (2625)] it is said "that this rock, the only one of this division whose doubtless primary quartz *bears fluid inclusions*, can be *pronounced a rhyolite* by its other petrographical characteristics. It properly *belongs to the trachytes.*" The inconsistency of the above statements can be easily seen. The presence of fluid cavities in the quartz-propylites is accounted for, even on Zirkel's statements of where fluid cavities ought to occur, by my statements of what these propylites are as given in the preceding pages. Regarding the quartz in No. 475 (2059) Zirkel stated: "This curious quartz was joined (to) a quite dull and entirely decomposed feldspar, like those in granites. . . . The observer is permitted to conclude that this singular quartz and the adjoining altered feldspar are also foreign inclusions." This might very naturally be the case since the rock is a brecciated one, and the part in question is an inclosed fragment of a granite.

Basalt.

No. 529 (23) I consider as a diabase both as regards its macroscopic and microscopic characters.

No. 531 (204) is considered by me to be an old, altered and much decomposed andesite (propylite). This, both macroscopically and microscopicall , is identical with some of Zirkel's propylites, and contains some quartz. Zirkel mistook for "macroscopical olivines" some greenish (viriditic) spots arising from the alteration of the groundmass in this rock! Its chemical analysis also indicates that it belongs to the andesites.

No. 534 (263), I claim is a melaphyr, as are Nos. 537 (325) and 538 (337). The two last contain considerable alteration quartz. Nos. 604 (1726) and 605 (1727) are said to be geologically older

than the rhyolites, and are classed by King and Emmons as andesites. King in objecting to Zirkel's determination of these specimens as basalts states: "At the time Professor Zirkel's examinations were made, the field-notes were not written out, and he was not informed as to the condition in the field" (I, p. 573, II, p. 592). King's statement, then, shows that the determination of the rock species was made by him, in general, and implies that Zirkel could not properly determine the species a rock belonged to unless he was informed as to its field characters. This further implies that there is no science in lithology, and that Zirkel's work was merely to describe the specimens that King had already classified. It is exceedingly unfortunate for the view of the independent conformation of the field and microscopic work that they should conform when Zirkel was informed as to King's views, and disagree when he was not so informed.

No. 608 (2006) is microscopically a rhyolite.

Nephelite Basalts.

No. 616 (646) I believe to be an andesite. The presence of nephelite in it can hardly be considered as proved by Professor Zirkel's method. Furthermore, claiming to have proved the presence of nephelite in this rock by its gelatinization when olivine was absent, he immediately assumes that he has proved, by the same method, the presence of nephelite in other rocks which contain olivine. While nephelite may be present in the andesites and feldspar basalts described on pages 255 to 258 of Zirkel's report, it is claimed that it needs a more accurate and thorough method of research to establish its presence here, than that employed by Professor Zirkel.

Clastic Rocks.

No. 629 (2618), is described as an old clastic rock, but some mistake must have occurred here since 2618 is a volcanic rock and the section corresponds with it.

Of the rhyolitic clastic rocks, Nos. 630 (574), 631 (703), 635 (851) and some others contain fragments of andesite. Of the basaltic clastic rocks the section No. 667 (856) was probably not made from specimen Col. No. 856.

CONCLUSION.

In questions relating to this Fortieth Parallel collection regard has to be paid to the conditions under which it has been preserved and the difficulties under which one examining it must labor. The specimens as collected in the field were numbered, but in the cabinet different numbers were again assigned them, and these latter in general are the numbers on the microscopic sections. Again, when Zirkel's report was published, the numbers given therein were entirely distinct and intended simply to enumerate (with some omissions) the rocks in the order in which they were described. The specimens, *unnumbered, lay loose* in drawers, placed on the labels which bear the first two sets of numbers. The specimens, not even being in paper trays, roll off their labels and about the drawers when they are opened. In this condition, without any numbers upon the specimens, they have been twice moved, I am informed, to different buildings and into different drawers, as well as part having been twice sent across the Atlantic. Also they were handled at will by all visitors who were admitted to the room.

Such was the condition of things at the time of my examination. In all cases of criticism the writer gave Professor Zirkel the benefit of the doubt that must arise in a collection treated in that manner, and required that the specimens and slides should correspond with the descriptions given in Volumes VI and II sufficiently to identify the rocks as the ones described. The slides, of course, bore the same numbers as the numbers in Zirkel's manuscript, and therefore comparatively only a few cases of doubt could arise regarding them.

Again, the names of the localities have in many cases been given differently in the published reports, and upon the labels. No set of coördinate numbers of reference have been employed, whereby the history of the rocks can be traced through the different reports, while the numbers of the pages referred to have rarely been given. The tracing of the descriptions of the specimens through the different reports is a work that involves much time and labor, which not unfrequently is fruitless.

In the case of the chemical analyses, part have been made from rocks not described by Zirkel; and the same difficulty, as mentioned above, occurs even in greater force in tracing the analyses to the descriptions given in Volumes I, II, and III, and when they exist, in Vol. VI. In some cases the writer has been unable to find any account of the rock analyzed, beyond the simple analysis. In other cases the specimens analyzed came from outside of the district; while in some cases it was found that the analyses given in Vol. I differ in some of their percentages from the analyses in Volume II, although they purport to be the same analyses. No explanation of this has been found in the reports, and although the differences are not great they must, until explained, throw some doubt on the accuracy of the work, especially since the changes tend to bring them more in accordance with the theory.

In fact, it would seem difficult to throw more obstacles in the way of getting at the facts which underlie the theories than has been done in the reports and with the collection of the Fortieth Parallel Exploration. Each volume by itself appears complete; to connect them and to trace the facts and theories from one volume through the others and to the specimens themselves is the difficulty.

In some cases either the slides or specimens or both were missing. Attention was called by me by writing upon the labels, to certain errors in slides and to the misplacement of the hand specimens. One of the curious mistakes observed may be pointed out here, as it will illustrate the difficulties one labors under in studying the collection. There are two slides numbered 2772 (No. 55, Vol. VI, p. 35). One of these belongs to Col. No. 2777, and is the slide described by Zirkel (No. 55). The second section 2772 was taken from specimen Col. No. 2773, and is described by Zirkel as No. 111, Col. No. 2775 (VI, p. 56). Now specimen Col. No. 2772 belongs in reality with slide 2773. Such was the state of these slides and specimens in 1878, and is only one out of many such cases, generally less complicated, that the writer had to trace out. In no case, when they could be accounted for in the above manner, have any of the errors been placed to Professor Zirkel's account. The statements as to the cyanite, that a

muscovite mica schist is a paragonite schist, that a greenish altered groundmass is olivine, that olivine, quartz, and biotite are absent when they are present, etc., etc. are things the responsibility of which he can not escape.

It would seem that evidence enough has been brought forward from volumes I, II, III, and VI to show that Professor Zirkel accepted Mr. King's statements, and outside of the simple and quite often inaccurate descriptions of the specimens and sections, registered the rocks in general as they were given him.

It would seem that the present writer, in the light of all that is given before, as well as much not given, had the right to think for himself and to reject the dicta of Messrs. King and Zirkel when the facts were not consonant. It is not to be taken that I endorse all the determinations of Professor Zirkel to which I have not here objected, since in many others agreement does not exist. But since my examination was made in 1878 for a different purpose than that of this paper, it is better to defer other statements until at least a desirable re-examination can be made of the collection discussed. It is possible that in some cases the writer's views would be modified on a re-examination, as regards the theoretical belief under what species a specimen belongs.

Although only a small portion of the original paper is given in the present article, it is hoped that enough has been presented to answer the questions arising out of that portion of my preceding paper devoted to the Fortieth Parallel Exploration, and to show that Mr. Merrill misstates my views in almost every particular.