Editor's introduction

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The name Joseph Paxson Iddings (1857–1920) first came to my attention during undergraduate and graduate student days in the early 1960s. In a petrology course, I was introduced to the CIPW norm calculation and learned the names of its inventors: Whitman **C**ross, Joseph **I**ddings, Louis **P**irsson, and Henry **W**ashington. While examining thin sections with a petrographic microscope, I occasionally encountered iddingsite, a reddish-brown alteration product that formed partial coronas around olivine phenocrysts. Years later, my growing interest in the history of igneous petrology led to acquaintance with several of Iddings' other contributions. My research culminated in the publication of a book on the history of igneous petrology, *Mind Over Magma* (Young, 2003), and convinced me of the leading role Iddings played in the development of igneous petrology in the years prior to the debut of Norman L. Bowen on the petrological stage around 1910. While engaged in that research, I learned that the Field Records Library of the U.S. Geological Survey branch in Denver contained a voluminous archive of Iddings' correspondence, much of which pertained to the creation of the CIPW quantitative igneous rock classification; several drafts of the classification at different stages of its evolution; and autobiographical accounts written by Iddings, one of which was a 109-page handwritten draft of his experiences in 1879–1880 studying in Heidelberg with Harry Rosenbusch, and the other of which was a typescript of *Recollections of a Petrologist,* a 533-page document that includes Iddings' account of how the classification scheme came to pass. Upon my request, Carol Edwards of the Field Records Library graciously sent me photocopies of the entire typescript of *Recollections*; the handwritten draft of Iddings' Heidelberg years; thick stacks of letters exchanged by Iddings, Pirsson, Cross, George Huntington Williams, and Washington; and multiple drafts of the CIPW classification and its nomenclature. Acquisition of this documentary bonanza prompted me to publish the detailed story of the evolution of the CIPW classification (see Young, 2008, 2009, 2010, 2011, 2012).

Although focused initially on the history of the genesis of the classification, I was fascinated by the typescript of *Recollections*. It became clear that Iddings had every intention of publishing his scientific memoir upon its completion. Unfortunately he died on September 8, 1920, before completing the project, which ends with his description of the early stages of his second excursion to Europe and the Orient in 1914. After Iddings' death, his closest friend, petrologist Charles Whitman Cross (1854–1949), took it upon himself at some point to complete Iddings' story. On the basis of his knowledge of the final years of Iddings' life and correspondence to which he had access, Cross reconstructed a narrative of Iddings' second Asian trip up to his return to the United States in 1916, a narrative that he intended to append to a published version of Iddings' manuscript. Even in Cross's hands, however, the final four years of Iddings' life were omitted from his addendum. Cross died in 1949, so he had nearly three decades to guide the manuscript through the publication process, but, for reasons unknown, *Recollections of a Petrologist* remained unpublished. After becoming thoroughly familiar with *Recollections,* I lamented that Iddings' fascinating story had remained unknown to the geological community for nearly a century. I decided to produce an edition of this significant document for the history of igneous petrology written by and about one of America's great geologists.

An important question arises, however: *Why is a scientific autobiography by Iddings significant?* The answer, I believe, can be boiled down to three primary reasons: (1) *Recollections* introduces the reader, through the eyes of a contemporary observer and participant, to the critical period of the final decades of the nineteenth century and the first decades of the twentieth century during which descriptive microscopical petrography evolved into the theoretical science of igneous petrology. (2) The author of *Recollections* was one of the major participants in, and drivers of, that evolution. And (3) the narrative of *Recollections* is marked by high literary quality, readability, clarity, honesty, humor, and

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intellectual integrity. Each of these three reasons calls for further elucidation.

THE EMERGENCE OF IGNEOUS PETROLOGY AS A SCIENCE

The Early Nineteenth Century

To begin a closer look at the three factors supporting the significance of *Recollections,* I offer a brief summary of the growth of nineteenth-century knowledge about igneous rocks. This will shed light on the radical change that came about in the late nineteenth century when theoretical igneous petrology emerged as a scientific discipline and took its place as one of the fundamental fields of geological investigation.

While the rough contours of earth history and the broad framework of the geologic timescale were coming into focus in the early nineteenth century, thanks to the mapping of stratigraphic sequences and the determination of the stratigraphic positions of various fossils, progress in understanding crystalline rocks lagged behind. The study of crystalline rocks was by no means ignored, but the field relations of coarse-grained rocks were not always easy to decipher. Moreover, advances in understanding were hampered by the fact that investigations of the mineral composition of rocks were largely restricted to hand specimens, supplemented by a few difficult, incomplete, and costly chemical analyses and, in rare instances, microscopical examination of crushed mineral grains. Accurate identification of the constituent minerals of crystalline rocks was not always feasible, even with a hand lens. To be sure, the volcanic origin of basalt and a few other fine-grained rock types, such as trachyte, had been recognized after a long struggle with the Neptunists, and the intrusive, possibly magmatic, origin of sills and dikes began to make more sense as a result of field study by James Hutton and some early experiments by James Hall, but the origin of large masses of crystalline rocks remained enigmatic. Were they the product of crystallization from subterraneous lava or were they the result of the chemical action of fluids permeating once deep-seated rocks?

With the benefit of hindsight, we can see that the pioneering efforts in the first half of the nineteenth century by David Brewster, William Nicol, Henry Witham, William Williamson, and others to formulate optical principles, devise polarizing lenses, and invent methods for producing thin transparent slices of rock unleashed enormous potential for the microscopical study of igneous rock thin sections. Geologists, however, were slow to seize the opportunity because of the difficulty of constructing microscopes and grinding thin sections, but also because they generally failed to grasp that the investigation of tiny mineral grains in thin sections of rock could yield information of much value for solving important geological problems.

Publications based on microscopic examination of thin rock slices by John Phillips and especially Henry Sorby in the 1850s sparked some interest, and momentum began to build in England when David Forbes passionately challenged geologists during the 1850s and 1860s to take advantage of microscope work. By the 1870s, use of the polarizing microscope in petrographic work had spread to Germany, France, Russia, Norway, and elsewhere in Europe. Although thin section work was hampered by the difficulty of distinguishing pyroxenes from amphiboles, and of distinguishing among the different kinds of feldspars, those problems were eventually overcome as Gustav Tschermak and others worked out reliable methods for identification.

The Zirkel-Rosenbusch Era

The study of igneous rocks took a quantum leap in the 1860s and 1870s in Germany thanks to the prodigious labors of Ferdinand Zirkel, who had been introduced to microscopical petrography by Sorby, and Rosenbusch. Both petrographers published numerous papers thoroughly describing the mineralogy and texture of crystalline rocks as viewed in thin section. Rosenbusch, in particular, generated a plethora of textural terms and coined a host of new igneous rock names. Each of these German scholars published massive volumes on microscopical petrography, primarily of igneous rocks, that went through several editions, thus establishing their reputations as the acknowledged world-class authorities on microscopical petrography. As a consequence of their growing fame as masters of an exciting new field of science, they attracted students from far and wide eager to acquire skills in microscopical petrography at the University of Leipzig or the University of Heidelberg.

Although Zirkel and Rosenbusch showed some interest in igneous petrogenesis, they concentrated primarily on descriptive microscopical petrography, at times giving a minimum of attention to the field contexts of the rock specimens they described. Nonetheless, the work of the two giants made it clear that the petrographic microscope was contributing significant advances in the study of igneous rocks. Not only could the hand specimen identification of many major rock minerals be confirmed or corrected, but minute grains of minor minerals unidentifiable or even undetectable in hand specimens were now being recognized in thin section. It also came within the realm of possibility of making better estimates of mineral abundances in rocks. The texture of crystalline igneous rocks could also be characterized much more thoroughly than in hand specimen. These ongoing improvements in mineral identification, methods for estimating abundance more accurately, and characterization of rock texture stimulated new interest in classification of igneous rocks. Unfortunately, the advances were a mixed blessing because microscopical igneous petrography became overburdened with a surfeit of new rock type names and textural terms, acceptance of igneous rock classifications was far from unanimous, and classifications remained qualitative.

Scientific Igneous Petrology Is Born

In the late nineteenth century, the emerging field of physical chemistry began to yield insights into the nature of solutions that bore the promise of being applicable to the behavior of magmas and crystallization of minerals. In addition, chemical analyses of igneous rocks became more commonplace and also included some of the minor elements. Improvements in the accuracy and comprehensiveness of chemical analyses led to better understanding of the chemical composition of rock-forming minerals, igneous rocks, and, indirectly, magmas. The time was ripe for a new generation of geologists to apply the growing body of chemical data, theoretical considerations drawn from physical chemistry, and studies of crystalline rocks whose field contexts were understood, to questions of the origin, crystallization, and diversification of magmas.

Owing to an influx of youthful minds into the field of microscopical petrography during the last three decades of the nineteenth century, new theoretical concepts and methods of analysis flourished. New varieties of igneous rocks were discovered and described, but petrographers also applied microscopical results to field studies of regional geology and ore deposits. The general order in which minerals crystallized from cooling magmas became clearer, as did the sequences in which various rock types were erupted or intruded. The concept of petrographic province was suggested by Judd (1886a) to apply to regionally associated igneous rock suites sharing similar mineral content and textural characteristics. Coupled with growth in knowledge of igneous rock types, numerous attempts to devise an acceptable igneous rock classification, including those of Ferdinand Fouqué and Auguste Michel-Lévy, Ernst Kalkowsky, Frank Rutley, Frederick Hatch, Justus Roth, Alfred Harker, William Hobbs, and William Stanley Jevons, were published. Among others, Hatch, Arnold von Lasaulx, Alfred Osann, Franz Loewinson-Lessing, and especially Cross, Iddings, Pirsson, and Washington, proposed quantitative classifications. Both geologic age and geologic occurrence gradually died out as a factor in classification. At the same time, more attention was given to chemical composition as a basis for igneous rock classification.

Charles Whitman Cross (1854–1949) and field party. Cross at far left. Courtesy of the U.S. Geological Survey; photo by Hunter.

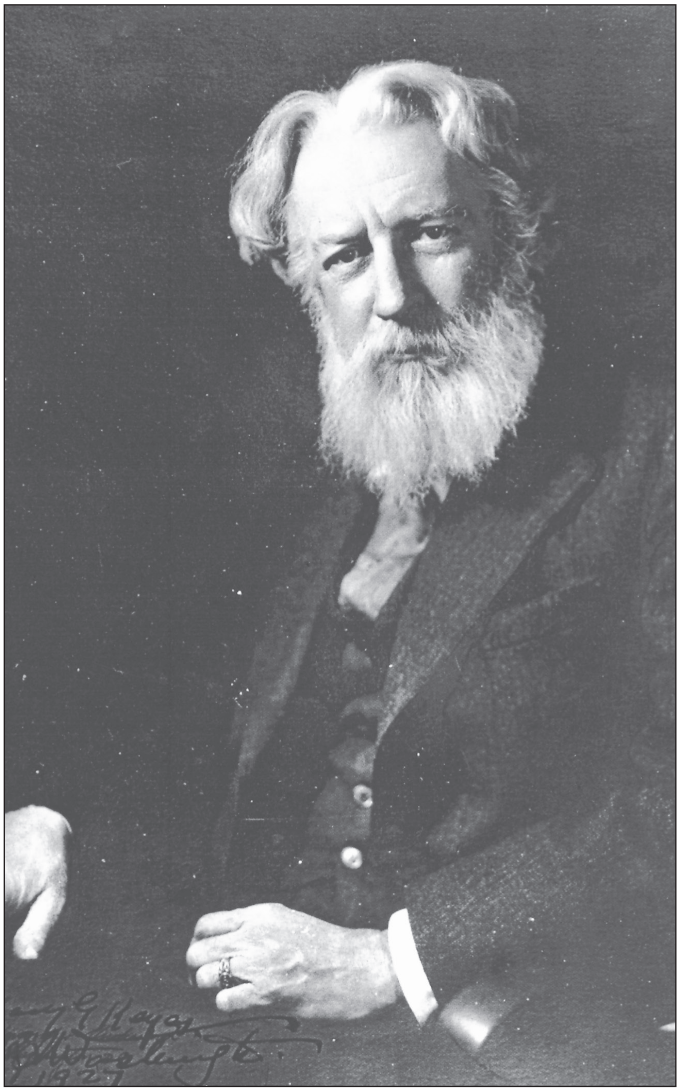
Petrographers became more inclined to speculate about sources of variation among magmatic rocks. Instead of appealing to magma generation in deep-seated source layers of differing compositions and subsequent mixing of magmas, petrographers gave more support to the hypothesis of magmatic differentiation, but their opinions about the mechanism(s) differed widely. Some authors appealed to the Soret effect, in which compositional differences were thought to be generated by thermal gradients within a magma body; electrical currents; and separation of crystals from magmas. Eutectic crystallization and fractional crystallization were also invoked to explain diversity.

For a long time, geologists had employed familiar terms for various igneous rock bodies: *dikes, sills, stocks,* and, for larger bodies, *masses*. However, as large igneous (or "eruptive") rock bodies were mapped in increasing detail, especially in the western United States, where exposure is exceptionally good, new forms of intrusive bodies were recognized and defined, including G.K. Gilbert's *laccolite,* Israel Russell's *plug,* and Iddings' *bysmalith*. After Gilbert (1877) first described the laccolithic form in the Henry Mountains of Utah, geologists discovered laccoliths in Colorado and in the mountain groups east of the Rocky Mountains in Montana and western South Dakota, some of which, such as the Shonkin Sag laccolith in Montana, appeared to have undergone differentiation. Eduard Suess also applied the term *batholite* to very large masses of granitoid rocks formed at depth. The discovery of laccoliths, dike swarms, radial dikes, and plugs prompted attempts to describe the mechanics of magma emplacement in diverse geological contexts.

A proliferation of chemical analyses in the latter nineteenth century induced Henry Washington to compile all the published analyses that he could locate and to assess the quality of such analyses, a painstaking task for which the results were first published in tabular form in 1903. The plethora of chemical data led to the invention of numerous diagrams for displaying selected data for spatially related igneous suites in the hopes of discovering meaningful chemical relationships and patterns within rock series, and for comparing the chemical compositions of distinct groups. Chief among these diagrams was the variation diagram, pioneered by Iddings and Harker, a plot of the abundance of the various oxide constituents (ordinate) in an analyzed igneous rock versus silica content (abscissa) in terms of molecular proportions (Iddings) or weight percents (Harker).

Researchers, such as John Joly, Ralph Cusack, and Cornelio Doelter, obtained experimental data on the melting points of rock-forming minerals. At the same time, improvements were made in accurate measurement of high temperatures and calibration of temperature scales. This complex of various scientific developments contributed to the professionalization of igneous petrology.

As the twentieth century got under way, the formulation of a robust research program oriented to igneous petrology at the newly founded Geophysical Laboratory set the stage for major advances in understanding igneous rocks and processes.



Henry Stephens Washington (1867–1934). Courtesy of the Geophysical Laboratory Archives, Carnegie Institution of Washington.

George Huntington Williams (1856–1894). Courtesy of the Ferdinand Hamburger Archives, Sheridan Libraries, Johns Hopkins University.

THE STATURE OF IDDINGS AS AN IGNEOUS PETROLOGIST

The late nineteenth century was an era of tremendous ferment in which descriptive microscopical petrography matured into theoretical igneous petrology. Now the question to be addressed is whether Joseph Iddings was a petrologist of sufficient gravitas that either petrologists or historians of geology should care about his personal recollections into that era. Below I seek to demonstrate that the answer is an unequivocal *yes*.

As noted above, microscopical petrography became a fashionable, exciting, cutting-edge aspect of the geological sciences in the 1870s and 1880s, and students from around the world flocked to Germany to sit at the feet of Rosenbusch in Heidelberg or Zirkel in Leipzig. Among the multitude of eager young North Americans to study in Germany were Joseph Diller, George Hawes, George Williams, Whitman Cross, Henry Washington, Louis Pirsson, Joseph Iddings, George Merrill, A.C. Lane, and Frank Dawson Adams of Canada. Several of them obtained doctoral degrees and returned to North America to occupy teaching positions in leading universities or work with government geological surveys.

Foremost among this rising generation of petrographers, at least in the judgment of none other than Harry Rosenbusch, was Joseph Paxson Iddings. Writing to Iddings upon learning of the premature death of George Hawes in 1882, Rosenbusch lamented that he "had put great hopes on him for the promotion of petrography and mineralogy in America," but now others would have to realize those hopes "and among those you also stand in front of the line, my dear Mr. Iddings."[1] A couple of years later, Rosenbusch wrote that it always gave him great joy in reading about the work of his former students. He continued, "I may always demand more from you."[2] His own contemporaries expected great things from Iddings. He was tremendously respected and admired by his petrological peers in the United States and around the world. We can justly claim that Iddings ranked with Waldemar Brögger and Alfred Harker among petrologists of his generation from around the world, based on the accolades that he constantly received attached to. We seemed to agree as to the probable manner of growth of phenocrysts and as to mineral variations in rocks. He admitted the fact of the presence of quartz in basalt, but could not explain its production. He did not treat the matter in the fashion he did when one of his American students asked him what he thought of quartz basalts, and reported he replied: "I will say as you do in America —To Hell with your quartz basalts!" Our intercourse was of the most amicable and polite nature.

Another matter considered was that of quartz-porphyry and rhyolite, porphyrite, and andesite. The distinction between the two sets of terms in Germany was only one of age. And such a distinction can no longer be recognized and will disappear in the third edition of his *Physiographie,* but such things must happen gradually; a book must be developed. It is a mistake for us in America to use the term *porphyrite,* as I proposed to do, for very fine-grained andesite-porphyries. I should give such rocks a new name if I want to name them specially. He makes no middle class between coarse-grained rocks and volcanic lavas except for his Ganggesteine. He said there were two things American petrographers should do: (1) To describe rocks carefully and exactly and illustrate them fully. (2) To use names so they may be understood in Germany. Wadsworth claims to have said propylites were altered andesites, but "Dear God! What does he mean?"

In discussing the character of rocks and their facies, Rosenbusch thought the definition of anything should apply to the major part of it. Rocks should be treated as geological bodies, not as hand specimens, so he could not call different parts of the same rock body by different names. We seemed to agree on the matter of primary intergrowths of hornblende and pyroxene in some diorites, but he had not seen them in glassy andesites. He regretted his lessening powers of work; he said he could no longer study till two and three o'clock in the morning and be on hand at his lectures at eight.

Monday morning we were at it again in the laboratory, with further discussion of Ganggesteine, so-called when found only in dikes. They may someday be found as extrusive rocks at which point they will cease to be Ganggesteine. He explained further his chemical views and thought he would draw the line between basalt and andesite on a basis of the Ca molecule. He discussed the philosophy of science and said he aimed to make petrography not merely descriptive but a philosophical science. Other topics of conversation were the relations between the chemical composition of rocks and their mineral characteristics and combinations, also the relation between geological occurrence and the physical conditions attending solidification, which were considered interdependent.

As he grows older, Rosenbusch said, he felt less personal interest in the solution of petrographical problems and more in the establishment of the facts themselves. He expected petrography, some years from that time, to be quite different from what it was then. But it should be gradually developed, not revolutionized. He did not expect to change his system materially during his lifetime, but he was sure it would be changed in time.

After dinner, when I called at his house to bid him farewell, he discoursed enthusiastically on metamorphism and the pleasure it gave him. The study of igneous rocks seemed tame in comparison. If I should take up the subject myself, he advised me to go to Norway, where he had been with Brögger and Williams in 1888. And so ended my second and last course of instruction under Rosenbusch, a most agreeable and profitable experience. Nothing was said about translating his textbooks, and no direct reference was made, as I remember, to my letter on the classification of igneous rocks. We avoided our differences as such and enjoyed a prolonged discussion of many debatable subjects, including his own theories, which I attacked two years later in the paper on "The Origin of Igneous Rocks" (Iddings, 1892e). I found Rosenbusch held views more advanced than some he was advocating in his textbooks, but he had a curious notion that he must develop his system slowly, in frequent editions of his books, or else his countrymen would not follow him! This was perhaps a just tribute to the intelligence of German petrographers, possibly a mistaken attitude on his part, but certainly an incomprehensible one considering the changes he did introduce in successive editions.

A VISIT TO MOUNT VESUVIUS

Passing through a storm which started soon after the train entered Switzerland, and nearly cheated me of a glimpse of its glorious scenery, I reached Naples under a clear sky and plunged into the confusion and noise, dirt, and squalor of that picturesque Mecca of volcanologists "where every prospect pleases and only man is vile."[16] I can think of no place among many I have visited in widely scattered regions where human nature is so obtrusively disagreeable, where the traveler is the object of so much beggarly solicitation and quarrelsome dispute. It is a bad place in which to be introduced to the Italian people, whom in other parts of the country, seen through Italian eyes, I learned to regard with the friendliest feeling. As for Vesuvius, my first complete view of it was gotten toward evening from the quay near the old castle, when the clouds that had partly concealed it disappeared, leaving the upper third of the mountain covered with a thin coating of snow, lit by the setting sun, beautifully white against dark clouds, and strongly contrasted with the somber lower slopes—a delightful surprise to one not expecting a snow-capped Vesuvius.

Dr. Johnston-Lavis,[17] an English physician and student of Vesuvius, whose descriptions of volcanic phenomena and Vesuvian lavas had made us correspondents, became my friendly adviser and guide to Mount Somma and the vicinity of Naples. I wanted to see explosion breccia from an active volcano for comparison with those of Yellowstone Park. So he and his little son, Mark, nine years old, piloted me to the northwest base of Mount Somma at Pollena, from which place we climbed a steep gulch to the top of the west end of the Somma rim. On the way we saw fine exposures of breccia, which were quite like those of Eocene age in Yellowstone, except for the abundance of leucite and other mineralogical differences. They were not so coherent as the ancient breccias, and there were more intercalated lava flows.

The pitch of the flows and layers of breccia was much steeper than in the great western breccias, which were associated with much larger volcanoes than Vesuvius. The cliff of the Somma ridge, facing the Vesuvian cone, furnishes a fine section of breccia and flows traversed by dikes at various angles, which radiate from the center of the volcano. From the top of the ridge, there was a beautiful view of the modern cone of the active crater, from which amber and white steam was projected against the blue sky. The sound of periodic explosions within the distant crater was like that from large geysers in action. Below, in the Valle del Cavello was the well-defined lava stream of 1872. Crossing this on the way down, we studied bombs, which, as Johnston-Lavis showed, were formed of blocks of rock enclosed in lava. At a rest house we tasted "lachryma-Christi"[18] and found ourselves more fatigued than we thought, and the walking over loose lava somewhat precarious when we resumed our journey. There were fields of horrid aa and masses of twisted ropy lava. Johnston-Lavis pointed out lava flows of various dates until we reached Cappuccini at the southwest base of the mountain.

A visit to the summit of Vesuvius to see the inside of the crater was postponed until my return from Lipari and, on account of Easter holidays and bad weather, was unsuccessful. The trip being made on a threatening day, I found myself after a stiff climb in soft ashes staring into a mist-filled pit from which arose acid odors and periodic thumpings and the showering of scoria. The solfatara near Pozzuoli were very insignificant to one familiar with the hot springs of Yellowstone Park. The crater of Astroni with its trachyte domes, small wood, and wild boars was most interesting. From the hill of the Camaldoli there was a fine view of the small cones scattered over the Phlegrean fields and of the islands as far as Ponza. In Naples I caught a glimpse of the ancient university, and, glancing into one large auditorium, saw the younger Scacchi[19] lecturing to a small group of students who were lounging about him with their hats on, smoking with no outward sign of discipline or decorum.

THE LIPARI ISLANDS

A young physician, Dr. Sambon,[20] of British and Italian parentage, accompanied me to Lipari, where he had been a short time before with a party of English geologists. He was an agreeable companion, whose cheerful spirits brought out the friendly side of the Italian nature. At five o'clock one beautiful evening, after being nearly torn to pieces by contending boatmen, we boarded a small steamer for Messina and enjoyed the Bay of Naples on a smooth sea having a fine view of Vesuvius. At half past three the next morning, we were awake to see Stromboli[21] close at hand. On its summit were periodic flashes of light reflected on a cloud above its crater. After one eruption, lumps of red lava rolled down the mountain almost to the sea. By half past six we were out to get a look at Sicily and catch sight of the snow-clad cone of Etna before it clouded over.

Messina presented a fine appearance from the water with its front of large white buildings. As the day was a high church festival, the people made an equally good impression; the ruins in back of the town were bright with abundant flowers and green trees. By nine o'clock in the evening, we were on the sea again and, at five the next morning, arose to find ourselves approaching the Island of Lipari with Stromboli, Panarea, and Vulcano in sight, and the sky bright at sunrise. We were met at the landing by Bartolomeo, a small, bare-footed native who was to act as guide and porter during our visit. After a breakfast of goat's milk and eggs, we set out for the Island of Vulcano on a calm sea. At the end of an hour's rowing, we reached a bay along the side of the small cones of Vulcanello, consisting of light-colored fragments and dust resting upon a base of scoriaceous basalt.

Recent eruptions, which ceased ten days before our arrival, had covered the level ground between Vulcanello and the larger volcano with dust and lapilli, easily obliterating Mr. Narlean's vineyard and partly destroying his house by large bombs of pumiceous light-gray lava, so-called bread-crust bombs. Narlean told of one that made a hole twelve meters in diameter in the loose tuff and went far out of sight.

The slopes of the cone of Vulcano[22] consist of fine, angular fragments of gray glass. At the center of the crater were fumaroles and from it, ten days before, violent explosions had projected great clouds of dust and vapor. The rocks within the crater were coated yellow and orange, the center of the fumaroles being white. Near the base of the mountain is the obsidian flow of 1772, almost covered by the dust of recent eruptions. Reaching our inn, tired and hungry, I watched our guide, Bartolomeo, who dropped in to finish up the dishes, eating macaroni in correct Italian style.

The following morning, mounted on two very small donkeys, we climbed the hills in back of the town and of Mount St. Angelo, the narrow road leading up drainage channels between high exposures of bedded tuff or stone walls topped with cactus, prickly pears, or thin brush. Sloping banks were covered with a kind of gorse full of brilliant yellow flowers, as fragrant as jasmine. There were wild sweet peas and lupine, beside daisies, fragrant violets, and purple heather. On the rocks ran little lizards, green-spotted and brown. The morning clouds disappeared, and the atmosphere became clear and beautiful as the cool fresh air warmed under the brilliant sun.

Passing around the west side of Mount St. Angelo, we ascended the white pumice slope of Mount Bianco and crossed the ancient crater. The northwestern slope of the mountain, deeply gulched, was covered with heather, the southern slope with vineyards; down a northeast spur to a valley near the crater of Campo Bianco, the ground of white pumice carried fragments of black obsidian. The valley was full of vineyards with scattered houses of white stucco, one-storied and square, surrounded by cactus. Pumice was being excavated high up on the mountain by men, thinly clad in white cotton, who carried their loads in baskets on their heads to the seashore and stored them in arched caverns near the beach.

Passing down a bare gulch, we met a group of boys with empty baskets coming up, one finely formed young fellow, so handsome, with bronzed skin, dark eyes and hair, and beautiful

Chapter 12

Reception of the classification and new challenges

RECEPTION OF THE C.I.P.W. IGNEOUS ROCK CLASSIFICATION[1]

It was to be expected that so radical a departure from current petrographical usage, and so novel a nomenclature as that proposed for the new system of rock classification, which was essentially a classification of magmas, would be very differently received by different petrographers according to their own views of igneous rocks and their individual temperaments. Indeed, we were not surprised that by some it was not received at all. "For," as Moses exclaimed, "their rock is not as our Rock, even our enemies themselves being judges."[2] They did not even acknowledge the receipt of the volume sent to them; at least, in a few notable instances no acknowledgment ever reached us.

Canada

Anticipating the shock, which might be occasioned in the minds of some petrographers, by the first sight of the full-fledged scheme, we wrote letters to a number of them stating the general plan of the undertaking and the method of its development. To Frank Adams, who had been taken into our confidence at an earlier period and, but for his inability to take part in our frequent conferences, would have been included in our committee, we submitted a draft of the complete proposition. His comment was:[3]

> The whole scheme is excellently set forth and the proposed classification seems to me to be good. Of course, as we all grumble at the introduction of new terms in the present classification, the setting forth of an entirely new series of names will of course meet with opposition. To do away with the entire nomenclature of a science and substitute a new one is a sweeping reform. But, as you show, half measures are useless, and if the plan is to be followed a completely new nomenclature is required. To entirely discard old terms will create much less confusion than to attempt to give them new meanings…. I fancy at first your nomenclature will be used together with the older one, and will gradually supplant the latter.
>
> Let me congratulate you on the completion of so long and so thorough a piece of work, which I think will be of real value in simplifying the science of petrography.

After the appearance of the book, Adams (1903a) reviewed it at length for *Science* and has been a consistent user of the system in his petrographical publications (e.g., Adams and Barlow, 1910).

Germany

The most appreciative, and to us most satisfactory, acknowledgment of the system came from Professor Zirkel in a letter to his former pupil, Cross. In part he wrote:[4]

> I have studied with the very greatest interest the "Book-of-Four," in which I must acknowledge a true scientific accomplishment of the new century in the petrographic-geologic field…. Furthermore, I will simply confess equally that the principles, which lead to the development of the new classification, as to theory have found my full approval: the predominant emphasis on the chemical composition and the weight proportion of the standard minerals. The alferric minerals, however, properly do not come into play. I must admire at one time or another the colossal mental work, which is embedded in this working out of the leading ideas. The scientifically rational consequence, with which it will lead the way, merits the very greatest acceptance, and I believe that the chemical classification principles, which they will present, really get at the core of the matter better than all other attempts. It is only to be regretted that, because of that, the "Natural Relationships" of the rocks must, of course, step somewhat into the background, but both regularly do not even enter into consideration.
>
> As a result, it is certainly now clear that a new nomenclature must be created, which brings to expression everything that is grounded in the new characterization. In that case, the old names are meaningless, and it only becomes bewildering if one wants to lay another sense under them for further usage. There is no denying that the principles of the new word formation, which to some extent reminds one of the famous, so ingeniously invented Volapük, are entirely practical, logical, and indeed the only acceptable ones for those who take the reformatory view. How much trouble they went to [those] who dreamt it all up!

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Zirkel raised the question as to whether the proposed nomenclature would adopt itself readily to international use, as it seemed somewhat too English. People were so conservative that there was no telling whether it would be generally adopted. He also pointed out the difficulty of applying it to geological maps, a limitation appreciated by the authors themselves in their suggestions for the use of old names for field terms.

What Rosenbusch thought of the new system we never learned; we only know that he adopted two of its mnemonic terms in different senses from those in which we had used them! Brögger was very ill during a long period after the appearance of the new system, which accounted in part for our not hearing from him, but it was not to be expected that it would find favor in his sight on account of the radical difference in our fundamental ideas on rock classification.

France

In reply to my preliminary notice to Michel-Lévy, I received the following:[5]

> I received your interesting communication during my summer campaign, and I beg your pardon again for not having found the time to get back to it. In a general way I strongly approve of your idea of separating the proportion of the white and dark components to establish the initial subdivisions, and your way of estimating and ordering the alumina (which is what I have also recommended).
>
> I think that a classification based on the chemical nature of magma will be a useful thing and is truly necessary; but I believe that it will have to be superposed on a much larger classification on a qualitative and quantitative mineralogical basis, utilizing the old names as much as possible in specifying them with precision, in short, serving as that which Rosenbusch, you, and I more or less use in our understanding. The chemical analyses are long, delicate, completely subject to caution, above all for the separation of alkalis.
>
> The geologist and the professor ought to be able to name the rocks before lengthy researches, and more often than not, a thin section and some hours of work are sufficient, when one is forced to determine with precision the plagioclase by the optical methods.
>
> In any case, allow me to wait for a more detailed exposition of your new classification and the examples that you ought to hand out, before sending you more explicit support.

I have no record of his opinion of the work when published. The comments of the lovable Barrois, however, were sufficiently explicit and flattering to satisfy the cravings of the most fastidious appetite. In a letter to me he wrote:[6]

> I thank you for the parcel that you have really wanted to give me of your classification of the igneous rocks […] and which I have read in the *Journal of Geology*.
>
> There is no doubt that this ought to be a monumental work, and that it marks a stage in the history of geology. I congratulate you and I ask that you will transmit my congratulations to your colleagues for having attached your names to a work of this importance.
>
> You certainly will be unanimously admired in Europe, as in America, but there you will be … —do you catch my drift? We are very old and awfully attached to our habits, to our traditional expressions for us to line up voluntarily to some revolutionary processes, which do not frighten the young Americans, which only come at the final advance.
>
> Receive my felicitations for the merit of your work, the tribute of my admiration for your audacity, and the assurances of my amicable memories of you and your collators.

Lacroix acknowledged the receipt of the book on his return from Martinique, where he had been studying the effects of the eruption of Mont Pelée (Lacroix, 1904), before he had had time to study it carefully. His appreciation of the new system has been shown in his constant use of it in the petrographical descriptions of rocks from Martinique, Madagascar, and elsewhere; and in his suggestions of names for newly described magmatic divisions.

Duparc[7] of the University of Geneva recommended its translation into French by one of his students, who completed the work successfully but was unable to find a publisher.

Finland and Russia

From Victor Hackman of Helsingfors, Finland, we received an appreciative note of thanks, closing with the expression:[8] "I would like the classifications system proposed by you to catch on generally!"

Loewinson-Lessing wrote:[9]

> Everyone must approve the tendency to base the classification on the chemical composition and the relative quantities of the different rock-making minerals. But I must confess that several points in the classification are as arbitrary as in other classifications. And I am very sorry that I cannot approve the nomenclature; it is too artificial for finding a large propagation and application.

He hoped some of us would attend the Vienna congress of geologists[10] when we might discuss the matter. Unfortunately none of us went to Vienna that year.

Great Britain

The judgments of our English colleagues in petrography were anticipated with great interest, and our forecast of their several opinions was not wide of the mark. Their letters are highly characteristic. Judd wrote:[11]

> I have received the important volume in which you and your fellow-workers have so clearly set forth the principles and methods of the new petrological nomenclature. [Judd's principal interest was in the words.] Of the necessity for reform we are all agreed and time alone can show how rapidly a consistent system can be made to replace a cumbrous and unsystematic mass of names.
>
> I am very grateful for the glossary you have drawn up for it will be of much service during the period—be it long or short—of transition. I fear that I shall be somewhat in the position of a watcher from an arm-chair, for after seven and twenty years of lecturing, I begin to feel that the time approaches when, in the interest of students, as well as myself, a fresher mind should deal with the ever new problems that arise.

Teall wrote:[12]

> I am delighted at the prospect of seeing you. Mind you must make our house your home if we are in London. Now as to your scheme of clas-

really looks now as though our fundamental problems are likely to be attacked in a very earnest manner.

This was the beginning of the Geophysical Laboratory of the Carnegie Institution of Washington, which was to play so important a role in the advancement of petrology.

MINING LITIGATION

Iddings' Role in the War of the Copper Kings

Allusion has been made to a visit to Butte, Montana, in the winter of 1900 at the invitation of Clarence King. Like most of King's moves it was sudden, and surprised me at work on university duties and the reforming of petrographical system. A telegram summoned me to a meeting at the Auditorium Annex in Chicago, where King unfolded the proposition that I was needed in connection with mining litigation between powerful adversaries, involving vast sums of money.[24] A definite object was set for my determination microscopically, which I was sure was indeterminable from the nature of the case. Upon my suggesting that I might fail to prove the point King had in mind, he said I was needed anyhow for other reasons and that I should have been employed on a previous suit in which an expert for the opposing side had testified falsely concerning microscopical conditions, and there had been no one to disprove his testimony. So it was agreed that I should contract for six weeks' work at a fixed honorarium, which was sufficient to distract me from my other interests and also compensate for the loss of time from university duties.

In less than a week, I was traveling westward through North Dakota into Montana for the twelfth time, but this one in the end of winter which added to the interest of the trip. The Bozeman valley lay under a thin covering of snow, while floating snow filled the air and veiled the surrounding mountains; most of my recollections of the place were with the heat of a July sun. Butte itself was a dreary place, scattered over the slope of a barren hill of gray rocks and sterile earth, soaked with acid fumes from smelters, and wet with melting snows that remained but a short time on open southern exposures. At times the unpaved roads leading uphill ran with liquid mud that had to be shoveled from rails along the more lively thoroughfares that crossed them. In dry spells heavy trucks turned up clouds of dust along the principal streets. Dreary at the best of times, Butte was most repulsive about the end of February, and a five-week sojourn might have been a depressing prospect to one snatched from university life. But it wasn't, either in prospect or in daily experience, for it was lived with Clarence King, with whom I enjoyed two meals daily in a snug little booth in a reasonably good restaurant nearby.

With King as a companion, a desert journey would become a pleasant memory. A wanderer by instinct, a Bohemian by temperament, and an endless source of wit, humor and merriment, King was the embodiment of good spirits and buoyant cheerfulness, which quickened one's mental activity and widened the horizon of one's imagination, for King was also a delightful romancer.

There was nothing tiresome about waiting for breakfast waffles or a dinner's steak. The mixing of a salad dressing called up lively memories of chance meals in out-of-the-way places—the wonderful dinner prepared in a lonely shack in southern California in the 1860s by a man who had once been chef to Maximilian in Mexico. Travels in Hawaii in early days, when the native maidens were more naïve and charming than in later times; Samoan idylls; the fascination of the West Indies and his life in Cuba with Henry Adams before the Spanish War; his admiration of dusky women and their cordial manners; and the picturesqueness of his Spanish travels—all recalled with the happiest memories and most amusing incidents. He was fond of alluding to the plans he had sketched for a romantic novel, which, he said, did not meet with the approval of his literary friends, though he himself was captivated by it.

In the matter of the mining case, he was frank and confidential and placed me in the midst of the argument as a legal proposition which showed the difference between a strictly scientific and a legal point of view—on one hand, the simple facts in themselves as ascertained truths, on the other, the use of them in an argument in which the psychological effects on the minds of judge or jury with reference to previous cases must be studied. It was for the lawyers to decide which facts to put forward and which to withhold. In the construction of an elaborate model of the ore veins in dispute, it was important to limit it to those parts that were clearly defensible and not extend it into highly debatable grounds.

My own work consisted in studying thin sections of ore and country rock from various localities, a report of which was turned over to the company engaging me. As the results were not material to the question under litigation, I was relieved of any anxiety as to cross-questioning in court. My presence and knowledge of the ground held in check any attempt of our proponents to introduce microscopical testimony of a questionable character.

Nathaniel Shaler's Testimony at Trial

As the time for the trial approached, experts and legal counsel arrived upon the scene, and, as those on our side of the case dined together at the hotel, the social phase of the experience widened and became still more entertaining, as King had more material to work upon. Professor Shaler[25] of Harvard and Rossiter W. Raymond[26] took active parts in the conversations, and I passed very agreeable evenings with them, listening to Shaler recalling his early experiences during the Civil War and as State Geologist of Kentucky. They had lively debates on the authenticity of Shakespeare's works and swapped anecdotes regardless of mines and mining.

Shaler was one of those genial spirits on whom the mantle of scientific obligation rested lightly. He did not seem to possess a strictly scientific conscience, did not appear to be troubled by reasonable doubt upon any subject on which he had made up his mind, and the criteria of his judgment did not seem to be exacting. However questionable his scientific opinions might be held by some of his geological colleagues, they were not doubted by Shaler himself. For this reason, his testimony as an expert witness before a judge in a court of law was more effective and, to the legal counsel behind him, most satisfactory. As the gray-haired professor of geology at Harvard University, Shaler spoke ex cathedra, without hesitancy and without doubt. He had made no detailed examination of the underground workings and had kept no notes, but had acquired an understanding of the case to be presented from his colleagues, and enunciated it with a definiteness that left nothing to be desired. On cross-examination he displayed a frankness that disarmed criticism. Upon being asked by the counsel for the opposition whether he had been through all the ground in dispute, Shaler replied, he had. The engineer on our side informed me afterwards that there were miles of the mine he had never been in. The opponent's lawyer then asked him what he found at a particular spot. Shaler responded that he kept no notes of his observations, but, if he, the lawyer, wished to know what was at that particular place he should ask Mr. King, for he kept notes of everything and had studied every foot of the ground, which he had, with his customary thoroughness. In discussing the model of the veins with Shaler, the opposing counsel said: "Professor Shaler, if this yellow vein were not here, and this red vein were not here, wouldn't you say that all this ground between them was one and the same thing?" "I should like to take time to study it over, " said Shaler. "Now, Professor Shaler," retorted the counsel, "weren't you brought out here for the expressed purpose of studying the ground, and making up your mind about it?" "Yes," said Shaler, "but I wasn't brought here to say what might be, if something else wasn't."

Afterwards Shaler remarked: "Never answer a hypothetical question, you don't know what may be behind it."

In his direct testimony he stated that upon one of his walks about the country in pursuit of healthful exercise, he was standing on a mountain ridge some miles distant from the town of Butte and happened to notice a fault crack cutting the rocky crest of the ridge. He sighted along it and found its trend passed directly through the Anaconda Mine, where the chief question at issue was the presence of such a fault. The psychological effect of this observation on the judge may have been good, but its effect upon his geological colleagues was different. Anyhow, our side won its case.

Returning to Chicago, my mind was filled with conflicting interests. To the obligations and pleasure of university teaching and the fascination of my petrographical research into a quantitative method of rock classification, there had been added the lure of possible mining studies in cases of litigation, under the direction of Clarence King. I had been notified that I should be needed in forthcoming suits and had been sent to inspect certain properties, "acting on the part of" two mining companies named in the note of request for admission to the mines. I was not in love with the calling, but the recompense was great, and it was greatness thrust upon me. I began to feel more at ease financially.

Then the unexpected happened—King, alive to all the possibilities of western enterprise, full of vigor and of restless energy, fell a victim to "the pestilence that walketh at noonday,"[27] and unable to withstand its deadly spell, sought the dry desert, and in solitude, like some wounded monarch of the wilds, would have died alone.

Friends, who by chance had learned his worth, hearing of his plight, hurried from their distant home to be with him at the end. Abram Baldwin, his wife, and daughter thus repaid King for the transient friendship they had shortly won.

BRÖGGER IN AMERICA

In the spring of 1900, Brögger delivered the George Huntington Williams Memorial Lectures at the Johns Hopkins University, following Sir Archibald Geikie, who had given the first course in 1897, and soon after repeated the petrographical portion of them at the University of Chicago. The correspondence connected with these lectures revealed Brögger's mental attitude toward his own work in a manner that reflected the unfortunate depression that characterized his temperament and colored his philosophy.

A strain of sadness runs through the musings in his occasional letters. It is like the plaintive harmonies in some of Grieg's music. The long, dark Norwegian winters, accompanied by protracted periods of bodily illness, furnished opportunities for brooding regrets and undermined the resolution of his stronger days and of his masterly will. Before his visit to this country he wrote:[28]

> A fairly long time, more than half a year, has already passed into the sea of eternity since we said goodbye in the dining hall of the Slawjansky Bazaar (in Moscow); days, weeks, and months regularly followed one another in strict order, never to return. Already spring is drawing near; the snow is gone; the black earth begins again to allow germination in thousandfold vitality; the children are jumping around in the garden in front of my window; winter is at an end.
>
> As life again renews its eternal cycle, when I have become too gloomy to rejoice rightly about that as I should, my experience, in any case, tells me that it must be that way. And I again feel a little hope and joy if I think about the next summer; in winter I divide my thoughts between memory of the past summer and hope for the coming one, but now that spring is here, I again look ahead more…. The more I sink into depression about life, the more I isolate myself with the years and feel alone. Now that scarcely allows for help any more.
>
> Since we saw each other in Moscow, I have fortunately been rather healthy physically; spiritually a little worse off, as I feel myself always more isolated and always more sunken into depression which, in my position, with a dear wife and seven healthy children, is really entirely unnatural. However, the relationships in the local university are frightfully petty and annoy me so deeply that, if only I were younger, I would have joyfully attempted to find suitable employment in America. Now I am probably already too old for that, younger employees themselves are available everywhere in America, and I must probably stick like an oyster to life here where I was born…. If my sons are any good, I hope they will find their way to America, to the great land of freedom and the future.
>
> So finally I also must heartily thank you myself for the beautiful mementos that you and the other English-speaking friends have sent to me—[a silver pitcher which we sent him, upon Louis Pirsson's suggestion]. It was, however, dear friend, an all too magnificent gift! My wife and I have already frequently taken pleasure in pouring fine

Chapter 14

Journey to the Orient: Japan

THE DREAM

Japan! The dream of my life since the time my father had imported a collection of curios and things Japanese, because of their artistic beauty and strangeness, and had given them to me, a boy in his teens, an incipient collector of everything interesting. The grace and beauty of Japanese decorations and utensils; the charm of the scenery as it appeared in pictures; and the attractiveness of the people and the friendliness of those I had met, developed a longing to visit the country and live among them as several of my older cousins had. The strangeness of the hermit people had been learned from the illustrations in the report of Commander Perry's[1] visit, and a familiarity with their [culture] had been gotten from a college mate in the Sheffield Scientific School, Mitsukuri Kakichi,[2] afterwards professor of biology in the university at Tokyo; he was a boy of fine culture who at college could write better English than I. There could be no feeling of aloofness toward a people having the qualities of Mitsukuri, and it was a source of sad regret when I reached Tokyo that my college mate was too ill for me to visit and soon passed away.

Dr. Kochibe, formerly director of the Imperial Geological Survey, and I had met in Chicago and afterwards traveled together in Russia; and Professor Kotō[3] of the University of Tokyo had also visited me in Chicago. Japan, as a region of volcanoes, of lordly Fuji-san, naturally excited the interest of a petrologist, especially as little description of its rocks had appeared in any western language, although studied by modern petrographic methods by Japanese geologists.

For me the Philippines also had a fascination—a volcanic country in the tropics, a part of the world I had always hankered after, with visions of luxuriant vegetation and brilliant butterflies. In Manila I had a friend in Warren D. Smith,[4] at one time a student in the geological department of the University of Chicago and then head of the mining section of the Bureau of Science. With him to direct me, it seemed possible to see something of the islands and to learn more of their petrography.

Funding the Trip

Having retired from university work,[5] I was free to wander regardless of terms and seasons, and was only limited in the length of my travels by the extent of my resources, a problem with several unknown variables, chiefly the cost of living and the available funds. The promotion of scientific research was much talked about at that time; special endowments had been established for the purpose, and I had friends closely associated with boards of directors and committees concerned with the disbursement of the same. But I found on presenting my case that appropriations were not available for traveling expenses, as experience had shown that such expeditions too often were more in the nature of junketing trips than serious scientific investigations. Certainly it would not be easy to disassociate pleasure from the strictly serious work of such an undertaking, neither was it expected that the whole expense should be defrayed by funds so allotted. The matter was not urged upon the gentlemen concerned, but eventually their assistance was secured for working up the results obtained independently.

The Smithsonian Institution, in the person of its secretary, Walcott, took a more liberal view of the proposition and granted money for the collection of Cambrian fossils in Manchuria and for a collection of volcanic rocks wherever I might seek them. My friend Charles M. Pratt contributed a sum for rock collections for two colleges in which he was interested. These sums and my own resources were sufficient to keep me going for an indefinite period, which was all the more enjoyable because of its indefiniteness. There seemed to open before me an endless vista of possibilities. And in the spring of 1909, I set out with official letters from the Smithsonian, the U.S. Geological Survey, and the State

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Department, and with such letters from Charles Pratt as to relieve me of any anxiety of financial mishap in case of an accident.

EN ROUTE TO THE PACIFIC

The Grand Canyon

Traveling west, I stopped to see the Grand Canyon of the Colorado River and enjoy the marvelous scenery in the neighborhood of the comfortable inn, El Tovar. The first impression, with the sun directly behind me and a slight haze in the air, was disappointing. It did not seem as large as I had expected it. But it looked very familiar in form and color, thanks to Holmes' pictures. Toward evening, when shadows brought out the relief of buttress and promontory, it took on larger proportions, and views from different points increased the impression of grandeur. The effect on me was awe-inspiring and somewhat repellant. It seemed terrible in the danger of its cliffs and the heat and aridity of its valleys.

Yosemite

The Yosemite Valley is in striking contrast in every way, a most interesting and accessible place, one which every geologist should see before he dies. There was a great deal too much dust along the stage road from El Portal with a torrent of water all the way beside one. This was the dry season, but "exceptional" weather set in, and the dust was laid and my hoped-for butterflies nearly cut off, there being only three hours of my visit when I could find any. Two cloudy days were devoted to a megascopic study of the rock walls of the valley with a field glass, looking for any visible differences in composition vertically. Having called attention in my book to the uniformity in composition of the rock of El Capitan from top to bottom, in arguing against the considerable settling of heavier minerals upon crystallization, I was naturally curious to see whether I had made a safe hit! Of course, I had, but it was risky to put it into the book without having any definite information on the subject. Still experience elsewhere went for something. A study with a good field glass, which recognized aplite veins and dark-colored schlieren, indicated uniformity in character of the mass of granodiorite from top to bottom. Specimens of broken rock from high up the face of the cliff, found in the material at its base, show great uniformity of composition and grain with a certain amount of variability as might be expected. From Glacier Point I had my first view of the Sierras.

Palo Alto, California

At Palo Alto, Branner[6] showed me the effects of the recent earthquake[7] on the [Stanford] University buildings. At this time of year, the appearance of the university is extremely picturesque, like a great cloister in an oasis surrounded by a desert of yellow fields and rolling hills. The ruined chapel, exposing gorgeous new mosaics, was a most pathetic sight, obvious wealth reduced to broken fragments of walls and roofs. But most of the university buildings were in good condition. As this was vacation time, the place was deserted. Contemplating the possibility of settling amid such environment as I had the opportunity of doing in 1892, my feelings were much the same as then. It seemed to me like going into a monastery for seclusion and isolation, and I am decidedly gregarious by instinct. "I prefer to be in the current, but can utilize a quiet nook," as I wrote Whitman Cross at the time, "'A rolling stone gathers no moss,' but it gets its corners knocked off and in time is polished. Besides moss is more becoming old age and promotes disintegration." The short visit with Branner's family was the last taste of homelike hospitality I was to enjoy for some time to come and was fully appreciated. It was here that I caught a glimpse of Harker's new book.

Hawaiian Islands

Having been delayed a month in starting because of the serious illness of a sister, I postponed indefinitely a visit to Kilauea and other parts of Hawaii in order to have time enough for Japan and the hunting of fossils in Manchuria before cold weather should set in. It seemed strange to pass through the midst of the islands I had planned so long to study, and only spend a few hours at Honolulu as a tourist might, but my ambitions were set beyond, and others had already learned much about the rocks. Cross had made a longer visit than I could have made at the time and had collected much interesting material, so I merely studied the coast of Oahu in passing, and enjoyed for a short time my first sight of the tropics and the wonderfully beautiful fish.

At Ease across the Pacific

A long voyage in a clean steamer, in fair weather over smooth seas, is a delightful rest and, with good company, a most agreeable existence. To sleep soundly, eat well, and breathe fresh air in comfort, with no sense of obligation to be actively doing something useful for oneself or others, is the condition of happiness to be found on a long voyage after prolonged exertions in accomplishing the task of life. A legitimate license to loaf, leisure for day dreams, was about to be realized.

While wandering about, watching Chinese steerage passengers on the deck below playing dominoes, I fell in with a fellow passenger ready to enlighten me on the subject of Chinese games in general. He was a smooth-faced, bright-eyed, small man of my own age, and it turned out upon further conversation, that we had lunched together at the Cosmos Club in Washington some weeks before, in company with Bailey Willis,[8] and had discussed the possibility of visiting the Orient. Stewart Culin,[9] ethnologist at the municipal museum in Brooklyn, New York, was well versed in Chinese lore and an admirer of Chinese art, and was on his way to purchase for the museum things both Japanese and Chinese. Many an hour was spent exchanging opinions on many topics, and Culin and I got on so famously

NOTES

1. Matthew Calbraith Perry (1794–1858), a veteran of the War of 1812 and the Mexican-American War, was a commodore in the U.S. Navy at the time of his voyages to the Far East. Besides being instrumental in modernizing the navy and promoting the education of future naval officers, Perry played a major role in establishing a trade treaty between Japan and United States with the 1854 Convention of Kanagawa. For the reports of the expedition see Hawks (1856) and Tomes (1857).

2. Mitsukuri Kakichi (1857–1909) was professor of biology at the Imperial University of Tokyo from 1882 to 1909. In 1901, he became dean of the College of Science of Tokyo University. Mitsukuri was an expert on the embryology of turtles. He died just a few days after Iddings arrived in Japan. For further biographical information see Jordan (1909).

3. Kotō Bunjiro (1856–1935), the so-called father of Japanese geology, made important contributions to metamorphic petrology, volcanology, seismology, and geotectonics. After advanced studies at the Universities of Munich and Leipzig under Credner and Zirkel, Kotō joined the faculty of the Imperial University of Tokyo and served until 1921. For further biographical information see Jaggar (1936).

4. Warren D. Smith (1880–1950) worked on the geology and mineral resources of the Philippines between 1905 and 1914 with the Philippines Division of Mines. He later became a professor at the University of Oregon (1915–1947). For further biographical information see Anonymous (1950).

5. What is striking about Iddings' recollections of the period from 1906 to 1909 is that he said almost nothing about his sudden resignation from the University of Chicago in the spring of 1908. To this day, the reasons are not fully clear. Some insight has been offered by an account given in 1968 by Arthur C. Trowbridge (1885–1971) before the Geology Club of the State University of Iowa, the legal name of the University of Iowa, where Trowbridge taught on the faculty between 1911 and 1952. In 1934, Trowbridge, a stratigrapher and sedimentologist, became head of the geology department as well as director of the Iowa Geological Survey and state geologist. As an undergraduate and graduate student at the University of Chicago, Trowbridge had been enrolled in Iddings' petrology class. Of Iddings he recalled, "he was certainly a gentleman and he was certainly very scholarly, but he was no teacher. He just didn't like to teach and he made no bones about it." The only reason he taught was that he had to make a living. Trowbridge recalled that Iddings, Rollin D. Salisbury (also on the Chicago geology faculty), and two other single men lived in a four-story house, with each one occupying one floor. He recounts that "One day we went to class with Iddings, and Iddings didn't appear. We waited and we waited, ten minutes and still no Iddings, so we appointed a committee to go over and ask Salisbury where Iddings was; they always had breakfast together as they lived in the same building, and Salisbury always came down earlier than Iddings. Salisbury replied, 'I don't know; I had breakfast with him, he seemed to be all right then, and said he'd be over for class.' Well what we finally found out was that between breakfast and the time that he was to come over for his class[,] he had received a cablegram saying that he had inherited a fortune in England."

It appears that the inheritance of a fortune in England was an ill-founded rumor. As noted at the end of Chapter 13, Iddings suggested that the proceeds from his consultation on a legal case were likely the source of funds that put him over the top financially so that he could quit teaching. It is also clear that he received supporting funds from other sources, including friends and the Smithsonian Institution, as Iddings describes in this chapter. The timing of the sudden disappearance, however, remains somewhat murky. It is also strange that he would desert his students and colleagues without a word of explanation before the end of the semester.

6. John C. Branner had come to Stanford University in 1891 from the Geological Survey of Arkansas.

7. The great San Francisco earthquake of April 1906 was responsible for the damage.

8. Bailey Willis (1857–1949) worked with the U.S. Geological Survey on the structure and stratigraphy of the southern Appalachians from 1884 to 1910, in which year he undertook an expedition to China. From 1910 to 1914, he worked in Patagonia for the government of Argentina. From 1915 to 1921, he was professor of geology at Stanford University. Willis wrote several books about his geological explorations in the African rift valleys (Willis, 1930), Patagonia (Willis, 1947), and China (Willis, 1949). For further biographical information see Blackwelder (1961).

9. Stewart Culin (1858–1929) was an American ethnographer interested in games and decorative art. In 1892, Culin became director of the Museum of Archaeology and Paleontology at the University of Pennsylvania. In 1899, he also became curator of ethnology at the Free Museum of Science and Arts in Philadelphia. He left Penn in 1903 to take the position of curator of ethnology at the Institute of Arts and Sciences of the Brooklyn Museum. For further biographical information see Lawrence (1989).

10. Tarumai (Tarumae), located on Hokkaido, is a vent on the Shikotsu caldera, which is 1320 m (4331 ft.) high. There have been thirty eruptions from Shikotsu between 1667 and 1982, all of them from the Tarumai vent. Iddings would visit Tarumai a few days later.

11. Ernest DeWitt Burton (1856–1925) was appointed in 1892 as professor of New Testament literature and interpretation at the University of Chicago. He served from 1923 until his death two years later as the third president of the university.

12. Inouye Kinosuke (1873–?) worked with the Imperial Geological Survey of Japan and ultimately became its fourth director in 1907. He was also the first president of Ryojun Technical College in Manchuria.

13. Okamura Yozo (dates unknown) was working with the Geological Survey of Japan in 1910 and later undertook hydrogeological studies.

14. Kôzu Shukusuké (1880–1955) was a prominent Japanese petrologist with special interests in volcanology, alkalic rocks, and experimental petrology. Kôzu, who had been a student of Kotō, joined the Imperial Geological Survey of Japan as a geologist in 1907. In 1911, he became lecturer at the University of Tokyo, and the following year lecturer at Tohoku Imperial University. From 1913 to 1916, he spent time in the United States, including a year at the Geophysical Laboratory, and then he went to Europe for further study. Upon his return to Japan, Kôzu was appointed professor of mineralogy and petrology at Tohoku University, where he served until retirement in 1942. From 1921 on, he was head of the Institute of Mineralogy, Petrology, and Economic Geology at Tohoku. For further biographical detail see Yagi (1956).

15. In the quantitative classification (Cross, Iddings, Pirsson, and Washington, 1902), the term *bandose* was applied to a rock whose chemical composition placed it in Class II (dosalane) with salic/femic $< 7/1 > 5/3$; Order 4 (austrare) with quartz/feldspar $< 3/5 > 1/7$; Rang 4 (docalcic bandase) with $(K_2O + Na_2O)/CaO < 3/5 > 1/7$; and Sub-rang 3 (presodic bandose) with $K_2O/Na_2O < 3/5$. In Iddings (1909b), presodic bandose was assigned to Sub-rangs 4–5.

16. Jimbo Kotora (1867–1924), a mineralogist, worked for the government of Hokkaido studying the geology of Hokkaido, beginning in 1887. In 1894, he became a professor at the Imperial University of Tokyo.