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1887—1956

A Biographical Memoir by

HANS P. EUGSTER

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> In the history of experimental research in petrology, in particular the study of heterogeneous equilibria in silicate melts, he was the great pioneer, in fact his position is unique, for no one has broken so much new ground, nor contributed in such rich measure to the solution of fundamental problems of petrogenesis.
>
> Tilley (1957, p. 18)

NORMAN LEVI BOWEN, the leading figure of the century in igneous petrology, died more than twenty years ago. This time lapse is both a disadvantage and a challenge. Personal details have faded, but we are perhaps better able to appreciate his enormous contributions and influence. I do not intend to assess Bowen's work in the light of today's knowledge. That would be unfair, because since 1956 we have lived through a revolution in the earth sciences. Many basic tenets had to be abandoned or severely modified, including some of Bowen's ideas on igneous rock evolution and parentage. Rather I propose to illuminate, by a careful reading of his principal published works, the sources and evolution of his ideas from *within* his time and his work. We will learn how he chose his problems and how his concepts developed; this will afford us an intimate glimpse of an unusual mind at work. At the conclusion we contemplate briefly what Bowen's work means to today's petrologist.

I did have the pleasure of being associated with Dr. Bowen at the Geophysical Laboratory, but only late in his career, after his retirement. Nevertheless, it was an unforgettable experience to watch him work with O. F. Tuttle on the granite system. I particularly remember a characteristic episode. One afternoon I gave Bowen an account of my unruly and seemingly contradictory data on the system muscovite-paragonite. The next morning I received a sheet of paper on which was drawn the only possible phase diagram, neatly solving my problems. Here was his legendary kindness and willingness to help.

BIOGRAPHICAL DATA

Norman Levi Bowen was born at Kingston, Ontario on June 21, 1887. His father, William Alfred Bowen, was a native of Chigwell, England, and his mother, Elizabeth McCormick, was from Kingston. Norman had one brother, Charles, born in 1879. Bowen was educated in local schools and attended Queen's University from 1903 to 1909. He completed a B.Sc. degree in mineralogy and geology, and subsequently studied at MIT from 1909 to 1912 where he obtained his Ph.D. In 1911 he married Mary Lamont of Charlottetown, Prince Edward Island, Canada, and they had a daughter, Catherine Lamont Bowen, born December 18, 1914 in Washington, D.C.

Bowen worked at the Geophysical Laboratory of the Carnegie Institution of Washington from 1910 to 1911, 1912 to 1918, 1920 to 1937, and 1947 to 1952, when he retired. During the period from 1919 to 1920 he taught at Queen's University and from 1937 to 1947 at the University of Chicago. Bowen became a naturalized U.S. citizen in 1933.

Honorary degrees were conferred upon him by Harvard, Queen's, and Yale Universities in 1936, 1941, and 1951. He was elected a member of the U.S. National Academy of Sciences in 1935, a foreign member of the Royal Society of London in 1949, and a member of Societies and Academies of London, India, Finland, Halle, Rome, and Belgium. He received the Bigsby Medal of the Geological Society of London (1931), the Penrose Medal of The Geological Society of America (1941), the Miller Medal of the Royal Society of Canada (1943), the Roebling Medal of the Mineralogical Society of America (1950), the Wollaston Medal of the Geological Society of London (1950), the Hayden Medal of the Academy of Natural Sciences of Philadelphia (1953), and the Bakhuis Roozeboom Medal of the Royal Netherlands Academy (1954).

During his last two years his health was failing and he died on September 11, 1956.

1907–1917: INITIAL SCIENTIFIC CONTRIBUTIONS

During his studies, Bowen put particular emphasis on chemistry, a fact of central significance in his later career. Equally important was his early and long association with geological field parties, to this day a typical and valuable aspect of Canadian geological education. The first summer, 1907, was spent at Larder Lake in Eastern Ontario, and the subsequent summers of 1908 and 1909 at Abitibi and Gowganda Lake, respectively, all parties being supported by the Ontario Bureau of Mines. During these studies, Bowen observed the intimate association of granophyre with diabase, and this observation may well have been the critical impetus for his lifelong involvement with igneous rock parentage and evolution. Originally, he accounted for the granophyre as a hydrothermal product between diabase and the surrounding slate, but he later changed his mind (1915c, p. 49).*

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*Years in parentheses without authors refer to Bowen's publications.

During his studies at Queen's, and because of his double involvement with chemistry and geology, Bowen became aware of the pioneering work of J. H. L. Vogt in Norway on silicate liquids (Vogt, 1903) and decided to make this his field of inquiry. Vogt did not encourage study in Norway and hence Bowen decided to enroll at MIT in the fall of 1909. After one year he joined the Geophysical Laboratory in Washington to carry out his thesis project. Meanwhile, in the summer of 1910, he had his own field party in the Thunder Bay area of Lake Superior, where he encountered large masses of plagioclase enclosed in diabase. This observation awakened a permanent interest in monomineralic rocks, such as anorthosites and ultramafics.

The choice of thesis topic, the system $NaAlSiO_4$-$CaAl_2Si_2O_8$ (nepheline, carnegieite-anorthite), was probably strongly influenced by Dr. A. L. Day, director of the Geophysical Laboratory. Day and Allen (1905), in a pioneering study, had investigated the plagioclase feldspars, and what was more natural than to add a feldspathoid to anorthite? Again that choice had a profound effect on Bowen's future work, because it produced a lifelong interest in alkaline rocks. The question of technique was easily answered: In addition to the well-established heating curves, very useful except in viscous silicate melts, Shepperd and Rankin (1909) had recently introduced the quench method, which is more tedious and time-consuming, but also less equivocal. Bowen (1912d) lost no time in applying both methods and produced his first phase-equilibrium paper, which became a model for his later contributions. The essential raw data are given, and from this an equilibrium diagram is constructed. That diagram is then fully discussed in terms of crystallization paths. Next detailed optical data are given for both glasses and mineral phases. Comparisons with earlier results follow, and the paper concludes with a section on applications to natural rocks. Steeped in Bowen's approach, modern petrologists find the discussions of the ternary liquidus diagrams easily accessible, but we can imagine how forbidding this material must have been for the majority of the contemporary petrologists.

A brief application of thermodynamics, referenced to the classic text of Ostwald on physical chemistry, is included in the nepheline-anorthite paper (1912d). The experimental work apparently was completed in one year, 1910–1911, as Bowen returned to MIT in the fall of 1911 and received his Ph.D. in the spring of 1912. He then joined the Geophysical Laboratory as a staff member and there ensued three years of exceptional activity, culminating in his first major statement on igneous rock evolution (1915c). Looking at his productivity, one can conjure up the image of a happy young fellow who had been handed the key—the quenching method—to open those doors behind which he would find the answers.

While still at MIT, he wrote another seminal paper (1912c), entitled "The Order of Crystallization in Igneous Rocks." It would eventually lead to the famous reaction principle paper (1922c). Though I cannot trace the source of the original ideas, it touches upon questions any thin section course raises: how to read the sequence of crystallization from textural information. Furthermore, the quench method allowed a direct determination of the crystallization sequence, though the rock types he was considering were still well beyond his experimental grasp. Curiously enough, Bowen reached a rather negative conclusion. He decided that because of the random sections through the three-dimensional fabric, it was impossible from thin section studies to deduce the order in which minerals appeared. Rather, textural studies would only give information on when they *ceased* to crystallize. In a brief but illuminating paragraph, he considered the effect of crystal settling on differentiation, another topic which was to assume central significance in later years.

The summers of 1911 and 1912 were again spent in the field, this time in British Columbia, working for the Geological Survey of Canada. These were the last full field summers, but by now Bowen had become a professional field geologist, an important asset in terms of his eventual impact. From now on, his major energies would be devoted to laboratory studies, but contact with the field was maintained continuously through field trips and sample collecting.

When Bowen assumed his new job at the Geophysical Laboratory in the fall of 1912, he had important choices to make: what system should he tackle first? Between 1912 and 1915, the sequence goes: albite-anorthite, MgO-SiO_2, diopside-forsterite-silica, albite-anorthite-diopside, and it reveals his uncanny knack for finding the jugular. Except for Fe- and H_2O-bearing systems, these four systems form the cornerstones of his petrogenetic deductions and contributions. The first choice was obvious. Bowen had successfully dealt with feldspars and feldspathoids in his thesis and had come to fully appreciate the importance of solid solutions. Then what more obvious solid solution was there than albite-anorthite? Day may have given his permission somewhat reluctantly, because the classic plagioclase study of Day and Allen (1905) had led to the founding of the Geophysical Laboratory. But, though able to demonstrate isomorphism, Day and Allen (1905) could not define the melting interval. That had to await the development of the quenching method (Shepperd and Rankin, 1909). In 1912, everything was ready for Bowen to step in, and he produced what is still one of his most famous studies: "The Melting Phenomena of the Plagioclase Feldspars" (1913a). That study, not improved upon to this day, has one unusual aspect not common to most other papers of Bowen's, in that it contains a detailed calculation of the liquidus and solidus curves based on thermodynamic theory. Using ideal solution theory, Bowen closely followed arguments presented by Van Laar (1906), but he demonstrated that he was in full command of the subject. The agreement between calculated curves and experiments is spectacular and makes it doubly curious that in his subsequent writings, with two notable exceptions (1922e and 1935a), Bowen never again used a quantitative thermodynamic argument. We can only surmize that he must have considered his ternary systems too complex for this approach. In any case, his purely geometric deductions gave him all he needed.

For the next choice of topics we must read between the lines:

> In the course of work on two supposed binary systems of which $MgSiO_3$ was one component, the writers have found that $MgSiO_3$ is unstable at its melting point and can itself be treated only as part of a binary system and not as a separate component. The supposed binary systems mentioned must therefore be treated as ternary systems and before proceeding to the study of our separate ternary systems we have worked out jointly the binary system MgO-SiO_2 which is common to both. [Bowen and Andersen, 1914, p. 487]

The two ternary systems alluded to are diopside-forsterite-silica (Bowen, 1914) and anorthite-forsterite-silica (Andersen, 1915) and the originally chosen "binary" systems must have been enstatite-diopside (or wollastonite) for Bowen and enstatite-anorthite for Andersen. Bowen's choice was logical, but not inevitable. Though he was clearly aware of the importance of pyroxenes to basic igneous rocks, he was probably looking for another simple binary system. Also, $MgSiO_3$-minerals had previously been studied by his colleagues (Allen et al., 1906). But now fate intervened. As he and Andersen found out quickly, $MgSiO_3$ melts incongruently to forsterite and a siliceous liquid at atmospheric pressure. They joined forces and produced the brief but highly significant paper on the system $MgO\text{-}SiO_2$ (Bowen and Andersen, 1914). Actually Bowen did not realize the full significance of the incongruent melting of enstatite until later that year, when he was working out the ternary system forsterite-silica-diopside (1914). They clearly recognized the crystallization and then resorption of olivine: "This resorption takes place as a necessary result of equilibrium . . . during the normal course of crystallization as a simple result of cooling. . . " (1914, p. 499).

They also pointed to the possible discrepancies between chemical and mineralogical classifications: "We have then what might be termed an olivine-bearing lava in simplified form, though the actual total composition of the mixture shows no olivine but an excess of free silica" (1914, p. 500).

Having clarified the behavior of $MgSiO_3$, Bowen could proceed to $MgSiO_3\text{-}CaMgSi_2O_6$, but now in the context of the ternary system diopside-forsterite-silica (1914). That paper closely follows the earlier models, but it also contains an explicit discussion of binary and ternary solid solution in ternary systems that shows Bowen had absorbed Schreinemakers' many papers. These principles are applied in the detailed discussion of crystallization paths in the system diopside-forsterite-silica, which are quite complex, because the L (Fo, Px) boundary crosses the En-Di join. In other words, enstatite (En) melts incongruently, whereas diopside (Di) melts congruently. He points out that under certain conditions, forsterite (Fo) may first crystallize, then be resorbed and finally again crystallize. Two central ideas are clearly expressed now. The difference between equilibrium crystallization ("first type") and fractional crystallization ("second type") is stressed, and Bowen shows that zoned crystals indicate fractionation. Secondly, differentiation by gravitational settling of early crystals is discussed in detail, and Bowen feels "that the settling out from a basic magma of the more calcic plagioclases and the pyroxenes rich in magnesia and iron is the dominant control in the differentiation of the ordinary lime-alkali series of igneous rocks" (1914, p. 260). The die was cast, and Bowen was to elaborate, extend, and defend this idea for the next forty-two years.

The final system of this group Bowen turned to next (1915b) is the system diopside-albite-anorthite, a system he considered to represent simplified basaltic or dioritic magmas. It shows solid solution in a limiting binary system and a ternary cotectic curve which crosses the entire ternary system. A new argument enters, based on the abundance of mafic constituents: "as the plagioclase becomes more alkalic the percentage of diopside (colored constituent) decreases rapidly" (1915b, p. 183). We have the explicit statement, "the more acid types are not original magmas . . . the more acid magmas are regarded as derived from basic material, being, as it were, successive mother liquids from the crystallization of the basic magmas" (1915b, p. 184).

The basic experimental work is now completed and the main arguments have been assembled for an assault on the accepted ideas for the origin of igneous rocks. Missing only is the direct demonstration that crystal settling can actually occur. Using artificial melts, Bowen (1915a) next showed that forsterite and pyroxene settle, while tridymite floats, with the effects visible in a few hours. He calculates viscosities from Stoke's law and shows that the olivine layer of the Palisades sill probably formed by crystal settling in the gravitational field.

Next follows Bowen's first major statement, delivered in an eighty-nine-page paper entitled "The Later Stages of the Evolution of the Igneous Rocks" (1915c). This contribution, an important stepping stone towards Bowen's book on igneous rock evolution (1928a), cemented his rapidly growing international reputation. It contains few new ideas, but it marshals much evidence, experimental and field, to support the contention that basaltic magma is the parent material for all igneous rocks and that gravitational crystal differentiation is the principal evolutionary mechanism. Assimilation and liquid immiscibility are dismissed. The normal "line of descent" is basalt→diorite→granodiorite→biotite granite→syenite→nepheline syenite. In other words, the alkaline rocks are the final differentiation products. At this stage, volatile constituents also become important. On page seventy-five (1915c) Bowen gives a table of crystallization products which is tantalizingly close to the famous reaction series of 1922. Also touched upon is the problem of forming monomineralic rocks, such as peridotites, pyroxenites, and anorthosites. Many of the ideas expressed in this paper had been espoused by others, such as his teacher R. A. Daly, but never before had so much experimental and field evidence been assembled to support them. Also, Bowen went beyond most petrologists by making basaltic magma the parent for nearly all igneous rocks and differentiation the sole mechanism.

At the age of twenty-eight, Bowen had clearly carved his niche. His unusual effectiveness was due to his mastery of both classical petrology and physical chemistry. He had steeped himself in the phase theory of the Dutch school (Bakhuis-Rooseboom and Schreinemakers) so essential for interpreting his diagrams. He had adopted a new technique, the quench method, and had determined several of the most important diagrams. He wrote clearly and convincingly, and he had no hesitation to repeat his results and conclusions in print (the albite-anorthite and albite-anorthite-diopside diagrams each appear in at least nine of his publications). No wonder that his influence increased rapidly. By now, most of the ideas of his subsequent career had been touched upon.

In a paper entitled: "The Problem of the Anorthosites"

(1917b), Bowen points out that melting points of individual minerals are too high for such rocks to be emplaced as liquids, and he suggests a crystal mush instead, formed by crystal accumulation, which accounts for the cataclastic texture and the absence of dikes, as well as the intimate association with gabbros and syenites.

1917–1928: GLASSES, DIFFUSION, ASSIMILATION, LIQUID IMMISCIBILITY, ALKALINE AND ULTRABASIC ROCKS

The First World War brought an interruption. Day had accepted the challenge of producing optical glass (see also Abelson, 1975). Together with other staff members, Bowen moved for a time to Rochester to the Bausch and Lomb factory. This work had an obvious effect on his scientific interests, and he subsequently published a number of papers directly related to problems of glasses, the best-known being the paper entitled "The Significance of Glass-Making Processes to the Petrologist" (1918a). His interests in diffusion in silicate melts (1921) and liquid immiscibility (1926) were also fostered during this period.

From 1918 to 1920 Bowen was professor of mineralogy at Queen's University, Kingston, Ontario. Meanwhile, his differentiation theory of crystal settling was attacked as inadequate by some of the foremost petrologists, such as Daly, his former teacher, as well as Grout and Harker. The objections were manifold, but revolved principally around Bowen's rejection of assimilation, diffusion, liquid immiscibility, and convection currents as significant processes. Bowen (1919e) answered his critics in detail. He does not yield much ground but he does use the occasion to sharpen his ideas. About the only substantive change is a shift in emphasis: filter pressing of liquids now receives equal billing with crystal settling and zonation.

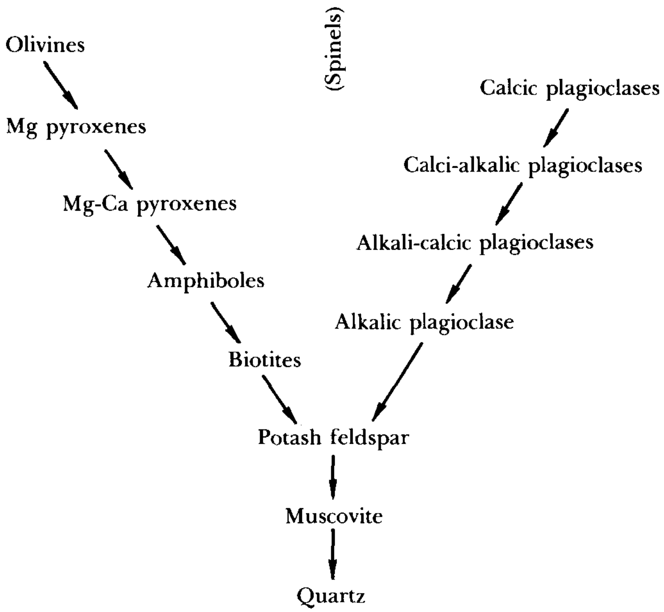
At Queen's, Bowen had no opportunity for experimental work and it is not surprising that he returned to the Geophysical Laboratory in the fall of 1920 to begin an extraordinarily fruitful period of association that lasted until 1937.

The first experimental problem to be tackled stems directly from his involvement with silicate glasses (1921a). Bowen was anxious to substantiate his contention that diffusion in magmas was a slow, small-scale process and he succeeded probably beyond his own expectations. Using diopside and plagioclase melts, he measured diffusivities of 3×10^{-6} to 2×10^{-7} cm^2/sec at 1500°C and he concluded "that the movement of large quantities of material through long distances by diffusion in a magma cannot be credited when the relatively rapid rate at which the magma must cool is considered" (1921a, p. 316).

He also showed that the Soret effect, diffusion in a temperature gradient, is insignificant in geologic settings, and that diffusion is important only for the formation of reaction rims. The diffusion paper is characteristic of Bowen's approach. He begins with a petrologic problem, devises a method for obtaining the data needed, gets the data, and then immerses himself in the theory necessary to interpret the results. In this case he must have read most of the literature on diffusion and heat flow and acquainted himself with Gaussian distributions and error functions. Very few petrologists of his generation had that much courage, and this explains, in part, Bowen's effectiveness. But we should not overlook the advantage he had in being surrounded by physicists and physical chemists.

There now follows a seminal paper for petrologic theory and one which was immediately accepted by many of its practitioners: "The Reaction Principle in Petrogenesis" (1922c). The chief conclusion, in the form of a simple flow sheet of mineral reactions, found its way into every text on igneous petrology:

REACTION SERIES IN SUBALKALINE ROCKS



It is the contribution of Bowen's best known to geologists in general, and yet when we analyze the paper in detail, we find no radically new ideas. But there is a subtle shift, even in style. We have here the mature Bowen: elegant, persuasive, and in complete control of his material. The central problem remains unchanged: How to explain the bewildering variability of igneous rock compositions. For this purpose, J. H. L. Vogt had used eutectic systems and Bowen once more shows the inadequacies of such models. Using his favored systems, Ab-An, Ab-An-Di and Fo-Q, he defines two reaction series between minerals and liquids, a continuous reaction series represented by solid solutions and a reaction pair or discontinuous reaction series as exemplified by incongruent melting. Then, going back to his earlier ideas on the order of crystallization in igneous rocks (1912c and 1915c), but curiously enough, using a table from Harker (1909) as his take-off point, he devises the reaction series in sub-alkaline rocks. It is greatly simplified from the 1915c version and for that reason alone more easily acceptable. In the words of Schairer (1957): "One of the great ideas developed by Bowen is the significance of the reaction principle in petrogenesis. Pentti Eskola of Finland called it the most important contribution to petrology of the present century" (p. 118).

Order of crystallization and the theory of magmatic differentiation have now been linked successfully and inextricably, and Bowen had surpassed his mentors, Vogt in Norway, Harker in Britain, and Daly in the USA.

Building on earlier ideas (1915c), Bowen next turned to assimilation (1922e) in an extensive paper on the behavior of inclusions in igneous magmas. Extracting heats of mixing from liquidus curves, he points out that such heats between silicate liquids are small compared to heats of solution and that the common assumption that acid and basic materials mix with evolution of heat is not warranted. He then evaluates the effect of adding plagioclase to liquids in the system Ab-An-Di. Using the just developed concept of the reaction series (1922c), he concludes that "a liquid saturated with a certain member of a reaction series is effectively supersaturated with all preceding members of that series" (p. 568), and hence, "saturated granitic magma cannot dissolve inclusions of more basic rocks" (p. 539).

It does react with them, however. A discussion of the effect of adding limestone, with particular reference to the origin of alkaline rocks, of quartzites and of shales, with reference to ultrabasic rocks, concludes with the characteristic statement: "It is doubtful whether the presence of foreign matter is ever essential to the production of any particular type of differentiate."

Assimilation had been the "deus-ex-machina" of his teacher Daly for explaining igneous rock variability, and Bowen leaves no doubt that he holds this position untenable. This paper (1922e), like many others, makes it clear why critics found it so difficult to refute Bowen's conclusions. Taking advantage of every scrap of experimental data, Bowen argues his case quantitatively for a highly simplified situation, then extrapolates to the natural case, and bolsters his extrapolation by quoting observations from the field. The weak link is the extrapolation, but this is difficult to attack without further data, and hence many rebuttals become little more than battles of faith. In the end, as we shall see, Bowen often did extrapolate beyond safe ground.

Looking over the papers between 1922 and 1928, we detect a distinct shift in working habits. Before 1922 only one paper was coauthored (1914a) and that more by default than by choice. After 1922, Bowen frequently joins forces, first with G. W. Morey, J. W. Greig, his former Queen's student, and R. W. G. Wyckoff, and finally with J. F. Schairer and O. F. Tuttle. New topics are tackled but, at least between 1922 and 1928, the work is more diffuse and less obviously planned. Nevertheless, for Bowen everything contributes to his central concern: the evolution of the igneous rocks.

To the rich harvest of 1922 belongs one more paper (1922d), in which Bowen can hardly suppress his glee. Morey had, using water pressure, synthesized pure K-feldspar for the first time, and he invited Bowen to help out with optical and petrologic interpretations. To their surprise they found that K-feldspar melted incongruently to leucite + liquid at 1170°C. This gave Bowen a chance to forge a genetic link between the subalkaline rocks of the "reaction series" and the alkaline rocks of his thesis (1912d) and his 1915c and 1917a papers:

> It is with considerable satisfaction, therefore, that we announce a laboratory demonstration of the fact that a mass consisting in one part of feldspar and quartz and in another of feldspar and feldspathoid can form from a single homogeneous liquid. The method of formation of these contrasting parts, which may be referred to as subalkaline and alkaline, respectively, is the method of fractional crystallization. . . ." [p. 20]

Discovering unsuspected incongruent melting among minerals seems to have been Bowen's special province. In the assimilation paper (1922e), Bowen had noted that "as a consequence of . . . the instability of sillimanite in contact with liquid rich in anorthite or magnesian silicates, alumina is set free as corundum" (p. 570), and he interpreted noritic material as the reaction product between basaltic melts and aluminous sediments. Thomas (1922) described examples, from the Island of Mull, where the liberation of corundum at the contact took place in the presence of ample silica ("Sillimanite" buchites containing corundum and glass, p. 240–41).

Clearly something was wrong with the accepted congruent melting of sillimanite. When J. W. Greig joined him, they decided to reinvestigate the system Al_2O_3-SiO_2 (1924a), so important to the ceramic industry. They concluded: "Corundum is frequently formed from shales and clays when these are attacked by natural magmas . . . even when there is plenty of silica present to form sillimanite. This . . . suggested that sillimanite itself must melt incongruently, breaking up into liquid and corundum" (p. 254).

Indeed, the stable compound at 1 atm was found to be not sillimanite ($Al_2O_3 \cdot SiO_2$), but $3Al_2O_3 \cdot 2SiO_2$, later named mullite (1924b). Mullite melts incongruently to corundum + liquid at 1810°C and is an ideal material for high-temperature ceramics. The technological applications of

Bowen's work now spanned the glass and ceramic industries and were soon to encompass the steel industry.

In the summer of 1923 Bowen sailed for Europe, first to visit the igneous rocks of the Island of Skye under the guidance of A. Harker, and then to Norway to see J. H. L. Vogt and to study the alkaline rocks of the Fen area recently described by Broegger (1920). Two papers resulted from the Fen trip (1924c, 1926e), both proposing carbonate replacement of silicates rather than carbonate magmas. The latter Bowen rejected because of the high temperatures necessary.

A brief note on assimilation in the norite sheet of Sudbury (1925c) contains a clever use of two-component mixing diagrams of the type later used in his book (1928a). Bowen points out ". . . the remarkable similarity of the less siliceous portion of the diagram with an ordinary variation diagram of a normal calci-alkalic series of igneous rocks" (p. 827). Consequently, he dismisses assimilation as a reasonable hypothesis for the Sudbury sheet.

One more major contribution follows before the close of the 1917–1928 period, one which stems from his interests in monomineralic rocks (1927b) and must have been nurtured by his trips to Skye in 1923 and 1926 with A. Harker who had mapped the area and examined the ultrabasic dikes with him. He restates his conviction that: "anorthosites and probably other monomineralic (ultra-basic) rocks were formed as a result of the accumulation of crystals from a complex melt and that there were probably no liquids of the composition of these extreme rocks" (p. 89). He adds, with a note of triumph: "but the assumption is usually made that the accumulated crystals have been redissolved or remelted in depth. Since his recent conversion to crystallization-differentiation Vogt has been a champion of this assumption" (p. 89).

Next, using a statistical approach based on the chemical analyses in H.S. Washington's (1917) tables, an approach he later employed so successfully for granites (1954), he demolishes the idea of remelting. He points out the difference in the plagioclase compositions between porphyritic and non-porphyritic basalts and concludes: "The most calcic composition of the total plagioclase of a . . . fine grained or aphanitic, basaltic rock appears to be about Ab_1 An_2. . . . There is thus no escape from the conclusion that the process of accumulation of crystals is not supplemented by significant remelting or re-solution of the accumulated crystals" (p. 100).

He proceeds to document a similar conclusion with respect to olivines and remarks that olivine phenocrysts must have been present before extrusion. He states that: "there are no liquids corresponding in composition with . . . ultra-basic rocks. They must originate through the local accumulation of crystals of either plagioclase or olivine which are not significantly re-melted or re-dissolved" (p. 108). Thus his ideas and conclusions first put forth in the anorthosite paper (1917b) are fully confirmed and extended to all monomineralic igneous rocks.

1928: THE EVOLUTION OF THE IGNEOUS ROCKS

In the spring of 1927, Bowen was invited to Princeton University to deliver a series of seminars at the suggestion of A. F. Buddington, who had been a former colleague at the Geophysical Laboratory. The lecture notes were later expanded and published in book form as Bowen's famous text (1928a). It was a revolutionary text both in its contents and its effect on the field. As Tilley (1957) reminds us: "In this vigorous presentation of the problems of the igneous rocks, Bowen provided a survey and synthesis which has exerted a profound influence on petrologic thought" (p. 13).

Based largely on Bowen's previous writings, the book is an excellent and convenient summary of his major contributions up to this period. I will briefly trace the sources of the material.

Opening with a concise statement in favor of a genetic link between igneous rocks of different compositions as exemplified by "natural series" and "petrographic provinces," Bowen emphasizes his belief in the "parental nature of basaltic magma" and the principle of fractional crystallization. He dismisses liquid immiscibility with arguments from his 1919e paper, shored up by recent experimental work by Greig (1927). The three mechanisms leading to fractional crystallization—crystal settling in the gravitational field, zonation, and filter pressing of the liquid, ideas enunciated in 1915a, 1915c, 1919e, and 1922—are elucidated. Next follows, as chapter IV, a brief course in phase theory with the aid of silicate melt diagrams principally determined by Bowen himself. Nothing reveals his enormous *experimental* contribution to this data quite as dramatically, and every diagram presented illuminates a different aspect of crystallization behavior. The reaction principle follows naturally and is essentially an abbreviated form of the (1922c) paper. In discussing the crystallization of basaltic magma, Bowen defends his position that "plagioclase and pyroxene crystallize out together at a very early period." He takes to task his own colleague, C. N. Fenner (1926), for holding a differing opinion and thereby precipitates a lifelong animosity.

Using a mixing diagram of the type presented in 1925c and material from the Mull Memoir (Bailey et al., 1924), he demonstrates that the non-porphyritic central magma cannot be formed from plateau magma by assimilation, but that settling of olivine and plagioclase does the trick. Quartz can also be freed by crystallization of biotite, he maintains in a section taken verbatim from the (1915c) report.

Chapter VII presents an approach Bowen has not employed previously: variation diagrams to delineate liquid lines of descent. He points out that when oxides are plotted against silica, smooth curves are formed for differentiating liquids. This holds for the Mull rocks as well as Fenner's own Katmai series. In a daring extrapolation beyond experiments, Bowen considers the effects of $KAlSi_3O_8$ and SiO_2 on crystallization paths involving pyroxene and plagioclase. Daly's averages from basalt to rhyolite define a generalized variation diagram into which ultrabasic rocks emphatically do not fit.

Having acquired an important new line of evidence, Bowen concludes: "In many igneous series all the rocks from basic to acid can be plotted on curves that require little smoothing and this fact, taken in conjunction with the evidence of the control of crystallization, points to the basic (basaltic) magma as the parental liquid" (p. 124). This approach, with less notable success, is extended to the glassy rocks.

In his paper, "The Later Stages of the Evolution of the Igneous Rocks" (1915c), Bowen has a brief section entitled "The Earlier Stages of Igneous Rock Evolution Not Revealed" (p. 73), in which he says that he assumes the existence of a parental basaltic magma, but does not know where it comes from. In his paper on ultrabasic rocks (1927b), and again in chapter VII, he states clearly why ultrabasic rocks cannot be the parents of basaltic magmas, but that such rocks are the result of crystal sorting. This conclusion is amplified in chapter IX, with material mostly from the 1927 (b) paper, except for a central section on peridotite dikes of Skye. A summary of his ideas on anorthosites stems from the 1917b contribution. The conclusion of part I (chapter X on assimilation) again is familiar to us from the 1922 (c) paper, which is reproduced in toto.

Part II opens with a section on K-feldspar liquids, and a chapter (XII) on the genesis of alkaline rocks. In 1915 (1915c, pp. 55–66), Bowen discussed the production of nepheline syenites from granites by fractional crystallization in the presence of high concentrations of volatiles. A desilication reaction was held responsible, such as the formation of biotite from K-feldspar. In 1919 (1919e, p. 426) he emphasized filter pressing of liquids rather than crystal settling, but in 1922 (1922d, pp. 16–20) a more convincing mechanism was presented in the form of the incongruent melting of K-feldspar to leucite and a siliceous liquid. This idea is exploited in chapter XII for trachytes and nepheline syenites, by constructing "skeleton diagrams" involving feldspars, feldspathoids and silica. These extrapolations are quite considerable, though by using natural assemblages Bowen presents a rather convincing case with respect to the location of field boundaries. Also of concern to him is the unknown effect of the presence of FeO.

A brief chapter on lamprophyres (XIII) is concerned directly with one special class of these rocks, the alnoites, which carry olivine, mica, and melilites. The material is taken largely from a paper entitled "Genetic Features of Alnoitic Rocks at Isle Cadieux, Quebec" (1922a). The presence of melilites is accounted for by the reaction of diopside with nepheline, and the olivine lamprophyres are the product of the interaction of an alkalic liquid with accumulated femic crystals (p. 269). The final chapters (XV–XVIII) are all brief and concerned with ancillary matters, such as fractional resorption, reverse zoning, volatiles, geophysics, and rock classification. Particularly revealing are Bowen's statements with respect to volatiles and geophysics.

In G. W. Morey and Paul Niggli, Bowen had two associates who advocated the importance of volatile constituents in the magma. On the other hand, as the leading experimentalist on the crystallization of dry silicate liquids, Bowen felt that "to many petrologists a volatile component is exactly like a Maxwell demon; it does just what one may wish it to do" (p. 282), and he concludes his discussion with a quote taken from Vogt (1922, p. 672): "I cannot endorse the statement that the volatile components have been the important factor in magmatic differentiation" (p. 302).

Bowen does, however, grant the effect of volatiles in lowering crystallization temperatures and viscosities, effects he was later to document himself forcefully in collaboration with O. F. Tuttle.

In the penultimate chapter, Bowen marshals those geophysical facts which bear on magma genesis and he concludes: "we can see little chance of escape from the necessity of deriving all magmas *ultimately* from matter much more basic than basalt and probably from peridotitic substance as represented in stony meteorites" (p. 311). Some of his conclusions with respect to partial melting of mantle material have a decidedly modern ring.

In writing his book (1928a), Bowen selected material of his fifty-five published papers that he felt was most significant, and he included entire sections word-for-word. Thus the book represents less of a beginning than of a summing up. Except for the chapter on liquid lines of descent (VII), new material and ideas are subordinate. Nevertheless, writing the book had a cathartic effect, and it presented Bowen with the opportunity for a new beginning, an opportunity which was reinforced by the appearance of a young collaborator, J. F. Schairer. It should not surprise us that Bowen clearly recognized his chance and took advantage of it brilliantly.

1928–1937: IRON-BEARING SYSTEMS AND "PETROGENY'S RESIDUA SYSTEM"

For Bowen, a new beginning does not mean a break with the past, and hence the new direction is linked to previous problems. Not surprisingly, therefore, Bowen and Schairer first tackled the system leucite-diopside and the melting of acmite (1929a and b). Both systems are concerned with alkaline rocks, one with leucitites and the other with the effect of iron. Acmite, $NaFeSi_2O_6$, a member of the pyroxene family, is common in alkaline rocks and its melting relations can be determined in air. Should we be surprised to learn that acmite melts incongruently? Bowen and Schairer (1929b) conclude that: "when there is an incongruent melting of a silicate compound it is always of such a nature as to throw excess silica into the liquid" (p. 373; see also 1928a, p. 298). This assertion was later to be qualified (1933e, p. 278). The acmite studies were extended to the ternary system Na_2SiO_3-Fe_2O_3-SiO_2 and the results were published in 1930c.

In 1929 Bowen attended the International Geological Congress in South Africa and on his return he visited alkaline volcanoes both in the western (Mufumbira volcanics N of Lake Kivu) and eastern (principally in the Lake Naivasha area) Rift Valley. An early account appeared in 1930 (1930a), with more interpretative results to follow (1937a, 1938b).

Next follows a series of contributions (1932, 1933a, 1933c) that lead up to the two monumental papers on the systems CaO-FeO-SiO_2 (1933e) and MgO-FeO-SiO_2 (1935a) with a combined 160 pages. These studies demanded innovations in experimental techniques as well as utmost mastery of the theory of ternary phase equilibria, but they yielded a rich harvest of petrologic insights and technological applications. They represent one culmination of the new direction begun in 1928, the other being the granite memoir with O. F. Tuttle (1958).

The first problem to be solved was the oxidation of iron during melting experiments. As pyroxenes and olivines contain iron in the ferrous form, the effects of oxidation had to be excluded. This was achieved by placing the charges in crucibles of metallic iron and passing a stream of nitrogen through the furnace. Any changes in bulk composition due to reaction with the crucible walls were evaluated by chemical analysis. Oxygen could not be excluded quantitatively, and hence some Fe_2O_3 was always present in the liquid. It was recalculated to FeO to be able to treat the equilibria in a ternary system.

In iron crucibles, fayalite was found to melt incongruently with separation of iron (1932). The join Ca_2SiO_4-Fe_2SiO_4 (1933c) also brought some unexpected results: "It is rather surprising that Ca-Fe olivines of the series we have prepared are not found in nature" (p. 292); and "The finding that $CaFeSiO_4$ forms a complete series of solid solutions with fayalite, which in turn almost certainly forms a complete series with forsterite, renders it rather surprising that the ordinary rock-forming Mg-Fe olivines do not contain more CaO" (p. 293).

The impetus for working on the system CaO-FeO-SiO_2 seems to have stemmed from an earlier, unsuccessful attempt to determine the relations on the join diopside-hedenbergite (1933e, p. 193), a fundamental join of the pyroxene quadrilateral. In working out the liquidus surface, Bowen and his co-workers confirmed the observation that iron lowers liquidus temperatures substantially. They found no ternary eutectic liquids, but they did encounter hedenbergite, $CaFeSi_2O_6$, but only below 965°C. It forms a series of solid solutions with $FeSiO_3$, ferrosilite, but the $FeSiO_3$ composition itself is represented by fayalite + tridymite.

Wollastonites presented another puzzle: "the remarkable feature of β-$CaSiO_3$ (wollastonite) is that it is capable of taking some 76 percent $FeSiO_3$ into solid solution yet this series of solid solutions is not known in nature . . ." (p. 269). They point out that ". . . metasilicate solid solutions . . . melt with separation of free silica and consequent throwing of excess of bases, especially iron, into the liquid, and the lowest melting liquid . . . is the iron-rich liquid . . ." (p. 278). "The possibility of alternative residual liquids, the one silica-rich, the other iron-rich, has been brought out in another system . . ." (p. 278).

The system MgO-FeO-SiO_2 (1935a) was to be considerably richer in petrologic implications. No less complex, it encompasses one of the most important substitution pairs in silicate minerals, $Mg^{++} \rightleftarrows Fe^{++}$, and three solid solution series: the wüstites, olivines, and pyroxenes, the latter two as fundamental to petrology as the plagioclases. No ternary eutectic appears, and the liquidus surfaces slope down towards the FeO corner. The olivines proved to be even more regular than the plagioclases, and the calculated liquidus and solidus curves agree well with the experiments as far as the latter could be taken. As we have made a point of Bowen's sparse use of thermodynamics, we should note his remark with respect to the use of ideal solution theory by Van Laar (1906): "The method of derivation of the equation is not altogether acceptable in these times" (p. 205).

The heat of fusion combined with a volume change estimated from the law of Gladstone and Dale gives an evaluation of the pressure effect on melting, which Bowen finds to be small for moderate pressures.

The pyroxenes present greater difficulties. Extensive solid solution was found, but again the composition $FeSiO_3$ (ferrosilite) is represented by fayalite + quartz. Perhaps the most important information contained in the join $MgSiO_3$-$FeSiO_3$ presented by Bowen and Schairer (p. 164) is the location of the orthopyroxene-clinopyroxene inversion. Synthetic orthopyroxenes could not be obtained in their pure form and the inversion was located with natural material, some of which had to be fluxed. For many years, this inversion curve has formed the backbone of pyroxene interpretation. Curiously enough, Bowen and Schairer themselves were not too clear about its meaning. At first they considered the orthopyroxenes primary and most pigeonites (or clinopyroxenes) as "formed metastably during rapid crystallization" (p. 203). But, in a footnote, they express less certainty: "Plainly it is necessary to investigate pyroxene compositions of more complex character before a satisfactory general explanation of pigeonites is forthcoming" (p. 203).

The high melting temperatures of olivines and pyroxenes confirmed Bowen's earlier contention that "rocks . . . made up entirely of pyroxene or of olivine . . . were not formed by simple crystallization of a melt of their own composition but rather by accumulation of crystals from a complex magma" (p. 204).

Emplacement is connected "with perhaps some special mode of intrusion of a crystalline mass. . . ." (p. 205).

Fractional crystallization based on the incongruent melting of clinoenstatite also presents a problem, because as little as 20 percent $FeSiO_3$ changes the pyroxene behavior to congruent melting (p. 211). On the other hand, the presence of anorthite stabilizes the incongruency.

Except for some brief subsequent papers (1935d, 1935e, 1936c) and the link-up of fayalite with feldspars (1937a, 1938a), Bowen's involvement with iron-bearing systems is terminated with the system MgO-FeO-SiO_2 (1935a).

The paper entitled "The Broader Story of Magmatic Differentiation, Briefly Told" (1933d) is an important foray into problems of ore deposits. Bowen had previously written a summary on geothermometry (1928b) for a volume on economic geology, but its scope was limited. He now specifically focuses on the "hyperfusible" or volatile components and concludes once again that they do not modify the fundamental course of fractional crystallization. If boiling occurs, however, acids accumulate in the gas phase and condensation may yield very corrosive solutions. He concludes:

> Under less deep-seated conditions the residual liquid of crystallizing salic magmas frequently boils and a fractional-distillation column is set

up. . . . This action results in the formation of an acid aqueous solution and it is such acid solutions that are probably the principal ore bringers. The solutions deposit their load as a result of reaction with the rocks. . . . They thus become neutral and finally alkaline. . . ." [p. 128]

We now turn to the other principal thrust of the Bowen-Schairer team: the system $NaAlSiO_4$-$KAlSiO_4$-SiO_2. The results are contained in the 1935c, 1937a, 1938a,c, 1947b, 1955, and 1956 papers. Bowen's address at the Tercentenary Conference of Arts and Sciences at Harvard University, September 1936, where he received an honorary degree (1937a), combines three interests: feldspar-feldspathoid equilibria, iron minerals (fayalite), and African Rift lavas. He shows that the additions of anorthite, diopside, and fayalite to the feldspathoid-silica systems all produce residual liquids enriched in alkali-alumina silicate. This conclusion is contrary to Fenner's (1926) assertion of iron enrichment in residual liquids and his own experience in the CaO-FeO-SiO_2 and MgO-FeO-SiO_2 systems (1933e, 1935a), which he rationalizes by stating that "absolute enrichment of iron silicate does not occur but rather only enrichment in iron silicate relative to magnesian silicate" (1938a, p. 408).

The residual liquids are closely approximated by the system $NaAlSiO_4$-$KAlSiO_4$-SiO_2, henceforth dubbed "Petrogeny's Residua System." Bowen points to the existence of a "belt of low-melting temperature, the lowest valley upon the fusion surface. . . ." (1937a, p. 12) that straddles the alkali feldspar join. He shows that his African lavas, as well as Daly's averages of alkaline rocks, granites and rhyolites, all fall in this trough and he concludes: "The process exerting the dominant control over the composition of liquids . . . has been fractional crystallization" (p. 18).

Mindful of assimilation, he modifies this to "The factor exerting the dominant control . . . has been crystal $\rightleftharpoons$ liquid equilibrium" (p. 19).

Bowen's involvement with the alkaline lavas from E. Africa surfaces once more (1938b), in an unsuccessful attempt to correlate magma chemistry with tectonics.

The final data relating to Petrogeny's Residua System were not published until 1955 and 1956, in papers by Schairer and Bowen on the systems K_2O-Al_2O_3-SiO_2 and Na_2O-Al_2O_3-SiO_2. Although Bowen must have been substantially involved in the data collection over the many years, the papers are principally Schairer's contribution and show little of Bowen's hand. Perhaps the most significant petrologic implication comes from the demonstration that the silica-feldspar-feldspathoid join in both systems is a binary section. In other words, this thermal divide cannot be crossed by liquids from the high-aluminous to the alkaline side and vice versa, in part, negating Bowen's own contention (1945): "There may develop also residual liquids very rich in alkali silicates, which . . . coupled with the inevitable concentration of water, will give rise to a waterglass-like residuum . . ." (p. 75).

1937–1956: LIMESTONES, FELDSPARS, AND GRANITES

In the fall of 1937 Bowen accepted an appointment as Charles L. Hutchinson Distinguished Service Professor of Petrology at the University of Chicago, a position he held until 1947. During this period a number of papers appeared which are concerned with matters already discussed (1938a,c, 1942, 1947b,c) and others which are summaries or addresses (1938d, 1939, 1940b, 1941b, 1943).

One particularly famous paper is entitled "Progressive Metamorphism of Siliceous Limestone and Dolomite" (1940a). It is Bowen's first paper on metamorphism and it was occasioned by teaching duties. Brilliantly argued, it gives us an inkling of what might have happened to metamorphic petrology had Bowen turned his mind to it earlier. Following the approach pioneered by V. M. Goldschmidt (1911), he identifies a series of decarbonation reactions to be expected during the progressive metamorphism of impure dolomitic limestones involving silicates such as tremolite, diopside, forsterite, wollastonite, and others. Assuming a dry system, the reactions are univariant and can be represented by curves in P-T space. These curves "... cut the general P-T diagram up into a grid which we may call a petrogenetic grid" (1940a, p. 274).

Calibration of the qualitative grid Bowen provided has kept and will continue to keep experimental petrologists busy.

At Chicago, Bowen continued his interests in alkaline rocks with the help of a number of students. Some of the work was published in the Daly volume (1945), where he stresses fractional crystallization as an alternative hypothesis to Daly's limestone assimilation: "Recent studies . . . in portions of the . . . system, Na_2O-CaO-Al_2O_3-SiO_2, show that . . . there can thus develop by fractional crystallization . . . a series of differentiates which may be referred to as simplified melilite nephelinites, tephrites and phonolites" (1945, p. 75).

A delightful presidential address before the Geologic Society of America (1947a) leads up to our next topic and also demonstrates the flexibility of Bowen's mind. It is entitled "Magmas" and it takes cognizance of the granite controversy which was heating up at the time. In it, replying to a jibe from H. H. Read, he coins the famous phrase: "We can, indeed, for rough purposes, separate petrologists into the pontiffs and the soaks" (p. 264). And he concludes:

> Yet it is not to be supposed that I do not have an open mind on the possibility of some granitic magma being the product of refusion of granite (Bowen, 1928, p. 319) or of selective refusion of geosynclinal sediments. To one who believes that both the laboratory and the field point to a granitic liquid as a late-crystallizing residuum of fractional crystallization . . . , the hypothesis that granitic liquid would likewise be an early product of selective fusion . . . cannot fail of sympathetic reception. [p. 275]

The next installment (1948a), entitled "The Granite Problem and the Method of Multiple Prejudices," was delivered again from the Geophysical Laboratory which he rejoined in 1947. It is part of a symposium on the origin of granites and it is fascinating principally because of the tone. Bowen, at sixty, had spent forty years perfecting his beloved theory that granites resulted from basaltic magmas by fractional crystallization. In the minority, and attacked vociferously, he becomes defensive for the first time.

Referring to the magmatist's position on the origin of granitic magma: "There is the view that it formed by fusion of geosynclinal sediments, or . . . by the refusion of . . . the granitic layer . . . or, finally, that it is the product of differentiation of basaltic magma as such. These are again arranged in the order of decreasing respectability today" (1948a, p. 87). And even more dramatically: "Nevertheless the disgraceful representative of the lowest group who now speaks to you . . ." (p. 87).

While said in jest, some of the criticism must have hit its mark and he might have begun to wonder whether he had bet on the wrong horse. It did not prevent him from demolishing H. H. Read, one of his chief antagonists: "Pushed to its logical consequences for a large mass, that corollary of wet granitization, the 'basic front,' is thus a basic affront to the intelligence of the geologic fraternity" (p. 88).

One final confession is contained in "The Making of a Magmatist" (1950c). Referring to Larsen's work on the San Juan region and the Southern California batholith, Bowen reiterates his belief in basaltic magma as the parental material for granites. For these examples, at least, partial melting is rejected emphatically.

The publication of 1949 introduces a new collaborator: O. F. Tuttle. While this team existed for but a few short years, it had an astonishing effect on petrology, still felt today. The substance of the contribution is contained in four papers (1949, 1950a,b, 1958).

Like liquid immiscibility and assimilation, volatile constituents had often been used by others to support unsubstantiated claims, but again, it was up to Bowen to provide the quantitative evidence. The effects of volatiles had occupied his mind for some time, and when the opportunity presented itself, Bowen was quick to recognize it. Through the efforts of G. W. Morey and R. W. Goranson, considerable experience had been accumulated at the Geophysical Laboratory in handling high water pressure, but the equipment was cumbersome. When Tuttle (1948) designed a simple hydrothermal quenching apparatus the way was open to evaluate the role of the volatiles and to attack one of the most pressing problems, equilibria in the system granite-water. First, Bowen and Tuttle (1949) chose a relatively simple system, the system MgO-SiO_2-H_2O. An extension of an early system of Bowen's (1914a), it promised information on the emplacement of peridotites and and serpentines. The locations of five univariant curves involving talc, brucite, serpentine, forsterite, enstatite, and periclase were determined at pressures up to 3 kb. No liquids were encountered below 900°C and: "Our results seem to exclude the possibility of the intrusion at comparatively low temperatures of a magma of serpentine composition which crystallizes, wholly or in part, directly to serpentine" (p. 453).

Next, as a stepping stone to the granite system, the alkali feldspars were studied in the presence of the water (1950a,b). Results were startling. Referring to K-feldspar and albite, we learn that, "At high temperatures they appear to form a complete series of solid solutions showing continuous variations of lattice spacings. . . . When crystallized at still lower temperatures, the complete solid-solution relation no longer obtains, and two feldspars form side by side" (1950a, p. 489).

We can only surmise how pleased Bowen must have been by these findings, as they provided a link to his earliest work on the solid solutions of the other feldspars, the plagioclases. With respect to the role of water Bowen and Tuttle note "that the principal function of water is a fluxing one, . . . but it affects equilibrium positions . . . only moderately. . . ." (p. 510) and hence ". . . crystal-liquid equilibrium is the dominant control in the development of these rocks" (p. 509).

An address given by Bowen (1954) upon receipt of the Hayden Medal of the Academy of Natural Sciences of Philadelphia on November 14, 1953 represents a final summing up of his views (1954), but it also contains some exciting new results on the granite-water system.

In the long-standing argument with his colleague C. N. Fenner with respect to silica vs. iron enrichment in residual liquids, Bowen remains firm: ". . . the final residual liquid from fractional crystallization will be an alkali-alumina-silica-rich liquid. . . . The contention that there will be absolute enrichment in iron oxides in final residual liquids is not supported by the experimental results . . ." (p. 6).

Exciting progress has also been achieved with respect to the question of the origin of granite. Referring to the results on the system albite-K-feldspar-quartz–water obtained with Tuttle, Bowen notes: "The existence of a lowest-crystallizing composition near the center of the triangle . . . is now readily established. This, then, is the composition toward which the liquid should migrate in the fractional crystallization. . . ." Analyses of natural granites fall near this temperature trough and, "We can conclude only that such clustering of composition of granites is strong confirmation of the hypothesis of their origin by crystallization of the liquid which is a residuum of fractional crystallization" (p. 10). But just to be safe: "There is, however, one other possibility. This minimum crystallizing liquid would also be the first liquid formed in the selective fusion (melting) of random rock material" (p. 11). And in conclusion: "In their extreme clustering about the composition of the liquid of *minimum* crystallizing temperature, the vast majority of granites loudly proclaim their origin through crystallization of that liquid" (p. 12).

These are just about the last printed words we have which clearly come from Bowen's hand.

The full story of the work on the granite system was published much later (1958), but it was written by Tuttle after Bowen's death. Bowen contributed substantially to that monumental contribution, but understandably, the emphasis is on Tuttle's views: "It is proposed that this zone of melting, where temperatures are high enough to melt granite completely and more basic compositions at least partially, may offer a mechanism for producing large batholithic masses of granite" (p. 2).

Would Bowen have been willing to go this far, abandoning his beloved fractional crystallization? I doubt it. Nevertheless, the work on the granite system today remains unchallenged, a pinnacle of experimental petrology, the quantitative foundation compatible with either fractional crystallization or partial melting.

CONCLUSIONS

In the preceding pages I have attempted to bring some order into Bowen's scientific evolution, an evolution which was uniquely unified, with a minimum of "accidental" developments. Impressed by the bewildering range and variability of the igneous rocks and by the possibility of forging a genetic link among them, he adopted the theory of differentiation by fractional crystallization, a theory which he never relinquished. Beginning with early field observations on the granophyre-diabase associations and the crystallization experiments on the plagioclase feldspars, he accumulated field and laboratory evidence tirelessly to shore up this theory and to relegate liquid immiscibility, assimilation, diffusion, volatiles, and iron enrichment to minor roles. He elaborated, embellished and extended and extrapolated as far as deriving granites and alkaline rocks from a basaltic parent. Ultrabasic rocks seemingly did not fit into the fractionation scheme and this is perhaps why he lavished so much attention upon them. In his mind, his final triumphant contribution to the granite system fully justified his tenacity.

His desire and need to impose order upon nature at whatever cost was shared by other prominent contemporaries in the earth sciences: V. M. Goldschmidt, Paul Niggli, Pentti Eskola, Reginald Daly, Alfred Harker. It found expression in the elaborate classification schemes, such as those of Rosenbusch. In that sense, Bowen's style of research seems to be clearly rooted in the nineteenth century. Is it fortuitous that Darwin, the epitome of the nineteenth century naturalist, was the first to invoke fractional crystallization?

But Bowen was a true revolutionary. He forced his largely descriptive contemporaries to accept a more quantitative style of reasoning, based largely on laboratory experiments and physical chemistry. To retain his credibility, he sharpened and expanded his appreciation of critical field relations, traveling extensively in North America, Europe, and Africa. In that sense, he was the first modern petrologist, combining field work with experiments and theory. His approach is still valid today, but some of his more extreme extrapolations are not acceptable any more, largely because of new geophysical and isotopic evidence. While differentiation is still an important mechanism in certain well-defined circumstances, few petrologists would use it to derive granites from basalt, except on a small scale. According to modern views, basalts originate in the mantle, while large-scale granitic magmas are derived from crustal material by partial melting. Intermediate compositions, such as andesites, are accounted for by partial melting of subducted oceanic crust with subsequent fractionation. The energy source for partial melting, whose existence Bowen questioned so persistently, is associated with mantle convection.

Where should we fault Bowen, if he should be faulted at all? Perhaps in extrapolating too far, in imposing too much order. A crucial stumbling block to his unified scheme was known early in his career: By far the most abundant igneous rocks are basalts and granites, one extrusive and the other intrusive, with intermediate members subordinate. Bowen never satisfactorily accounted for this observation. His teacher Daly held a different, no less popular, opinion: "Amid so many uncertainties one truth stands fast: the more closely the process of differentiation is considered in terms of its possible or probable units, the more unsafe is the reference of the diversity among igneous rocks to only one mode of origin or any one mechanism of separation" (Daly, 1933, p. 332). Lest we forget, Bowen's contribution to igneous petrology remains unmatched. His many equilibrium diagrams, from the plagioclases to the granites, are a monument to his insight, zeal and plain hard work. They continue to be used to check new ideas and deductions. Even more important, perhaps, is Bowen's example of how to approach the earth sciences. His insistence on a quantitative, chemically sound method in place of vague assertions is so well embedded today that we forget how revolutionary it was at the beginning of the century. Among his contemporary practitioners such as Vogt, Van't Hoff, Goldschmidt, Boeke, and Niggli, N. L. Bowen surely stands as a giant.

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