CHAPTER TWO

The Middle Years

IF THE PERIOD 1892-1917 may be described as the Stillman era, then the years 1917-1940 undoubtedly must be called the Swain era. Robert Eckles Swain was trained as a biochemist in a period when biochemistry was relatively undeveloped. He paid a great deal of attention to the effects of atmospheric pollutants, notably sulfur dioxide, on the growth of various forms of plant life. At one time much of the basement was given over to “polluted greenhouse’’ work. As a pioneer in this field of research, Swain was soon in demand as a consultant for large-scale investigations of pollution. For example, in 1909-11 the U.S. Department of Justice carried out investigations of smelter smoke damage, for which Swain served as an agent of the government. Again, in 1910-12, he served as expert chemist with the Reference Board of Consulting Experts for the U.S. Department of Agriculture in similar investigations of the adverse effects of atmospheric pollutants. In 1920-21 a large suit was brought to court in connection with the Salt Lake Valley smelter smoke pollution, during which Swain served as a Commissioner to the U.S. Federal Court. During 1937-38 he served as scientific adviser to the International Tribunal established jointly by the Canadian and U.S. governments in order to settle disputes arising from pollution caused by the Trail Smelter Company in British Columbia. Many other bodies, local, statewide, and national, sought his advice on questions of atmospheric pollution, in 1957 he served as a member of the Hearing Board of the Bay Area Pollution Control District.

Swain was by temperament well suited to work on committees and boards. In an age when the Stanford faculty played a very active part in the municipal life of Palo Alto, he served as a member of the Palo Alto City Council from 1912 to 1921 and was Mayor of Palo Alto from 1914 to 1916. In University governance Swain likewise had a very active role. For a long period during the 1920s and 1930s it was a rare event for him not to be a member of the

Advisory Board, the powerful University body which advises the President of the University in matters of academic appointments, reappointments, and promotions. Swain also served as a member of the Board of Directors of the Hoover Library (now the Hoover Institution), and he had a large hand in establishing the Stanford Research Institute, during the early days when that organization was formally affiliated with the University.

Punctilious as to detail, and invariably well briefed on pending business, Swain inevitably had to be active in local, national, and international scientific societies. He served the American Chemical Society in many capacities. He was a delegate to the Eighth Congress on Industrial Chemistry at Strassburg in 1928; he was a member of the Committee on the Teaching of Chemistry in 1934-35; he was a member of the Committee on the Professional Status of Chemists in 1934-35; and he was a member of the Committee on the Professional Training of Chemists from 1936 to 1941, serving as chairman of that body from 1939 to 1941. He was a director of the American Chemical Society from 1937 to 1946, and he served as chairman of the Committee on Biochemical Nomenclature of the National Research Council from 1936 to 1941. Swain was appointed member of the U.S. Delegation to the meetings of the International Union of pure and Applied Chemistry in Cambridge (1923), The Hague (1928), Madrid (1934), Lucerne (1936), and Rome (1938).

All these activities involved travel in an age before airplanes were common, and gave rise to the irreverent quip that Swain was Stanford's "Pullman Professor of Chemistry". Yet despite these outside activities he was diligent in discharging his Departmental responsibilities: he regularly taught two or three courses per year in biochemistry and industrial chemistry. Given his rather formidable character, his long (twenty-three years') tenure as Executive Head, and his detailed knowledge of the pathways of power in the University, Swain inevitably left his firm imprint on the Department of Chemistry. The years during which he presided over the Department were financially strained, both as regards University funds and external resources. Even so, the appointments to the faculty during the period 1917 to 1940 were remarkable in that all the persons appointed by Swain were active in research and were, in addition, gifted and dedicated teachers. The list of men recruited to the faculty in Swain's regime is long and honorable: Parks, Leighton, Luck, Noller, Loring, Ogg, Berstrom, Koenig, van Rysselberge.

Swain has been described as "running the Department with a firm guiding hand".[1] By present-day standards, when significantly, we have Departmental Chairmen rather than Executive Heads, Swain may well be judged to have been an autocrat; though it is reasonable to suppose that in the matter of faculty appointments he was influenced by Franklin's wide knowledge of chemists and chemistry. The style of administration which involved the Executive Head in making decisions down to almost trivial detail has disappeared, and it is unlikely to return to academic departments in major universities. Yet it must be conceded that, as judged by the persons appointed to faculty posts and by the scholarly output of the Department, the Swain era was a very important period in which sound growth was added to Stillman's pioneering foundation. At the same time, it must be acknowledged that Swain operated the Department *so* economically—making it a matter of principle always to return part of the yearly budget unspent—that his successor, P. A. Leighton, encountered great difficulties in making ends meet. Much vigorous argument was needed to persuade the University that a larger budget was an absolute necessity in the post-Swain years.

Virtually all of the faculty-building of those years took the form of highly selective appointment at the lower ranks, followed by slow progress up the promotion ladder. Depression times made it difficult for young faculty to acquire research funds but also made it essential to promotion that they should be active in research as well as diligent in teaching. A major exception to this pattern of development occurred in the case of James William McBain.

James William McBain was born in Chatham, New Brunswick, in 1882. His undergraduate work at the University of Toronto was followed by doctoral studies under the direction of H. Quinke, a distinguished colloid chemist at the University of Heidelberg. After completing his doctoral work, McBain began a twenty-year career at the University of Bristol, England, originally as Lecturer and then as the first holder of the chair of chemistry endowed by Lord Leverhulme, the founder of the Lever Brothers industrial empire. McBain had broad interests in physical chemistry, but his main contributions lay in two fields, the adsorption of gases on solid surfaces and the elucidation of the properties of detergent solutions.

Although the uptake of gases by porous solids is a phenomenon which Dewar used as a means of improving the vacuum in closed systems, and although the uptake of dyes and other materials from

[1] **Memorial resolution presented to the Academic Council, June 6, 1961.**

solutions by fabrics has been a part of everyday experience from the earliest days of mankind, knowledge of these phenomena was largely limited to empirical information of a crude nature during the first years of this century. McBain and his students at Bristol made extensive studies of the adsorption of gases on charcoal and other solids of large surface area, obtaining relationships between the weight of gas adsorbed and the equilibrium gas pressure, thereby providing the basis for "adsorption isotherms". Such knowledge was of direct, practical importance in the manufacture of gas mask canisters in World War I, and also provided a framework on which an understanding of heterogeneous catalysis was later built. This side of McBain's researches resulted in a monograph "Sorption", published in 1931.

Much of McBain's work at Stanford represented a continuation of his work at Bristol on the unusual physical properties of soaps and soap solutions. In particular he devoted much effort to working out the complicated phase diagrams of a number of metal alkanoates and to characterizing their aqueous solutions. The peculiar properties of those solutions, which are markedly nonideal and are capable of dissolving large amounts of normally water-insoluble materials, were interpreted by McBain as arising from the formation in aqueous solution of clusters of ions, called micelles. McBain and his students at Stanford accumulated a large body of data on these systems, often at the cost of great labor. The metal alkanoates are tiresome materials to study in aqueous solution because, being the salts of weak acids, they are very sensitive to changes in pH. Only towards the end of McBain's career did less troublesome colloidal electrolytes become available, e.g. the alkyl sulfates and sulfonates. Later workers had a much easier task in studying alkyl sulfates. Measurements of the solubilities of organic liquids in soap solutions were of great practical value during World War II in connection with the large-scale production of emulsion polymers.

Although adsorption and solubilization were the areas in which he did his best and most extensive research, there was scarcely any part of colloid chemistry to which McBain failed to make some contribution. He classified a number of obscurities in the preparation and stabilization of sols; and by the use of an ingenious but well-nigh unworkable apparatus which, at great speed, sliced off a surface layer from a large expanse of aqueous solution, he made direct measurements of the amounts of surface-active materials adsorbed at the air-water interface. McBain was well acquainted with The Svedberg and much intrigued by Svedberg's massive oil-driven ultracentrifuge. Believing that such an instrument should be susceptible to modification and simplification, McBain invested considerable effort in developing the opaque, air-driven-top ultracentrifuge, the concept of which was simplicity itself—a rapidly rotating metal top driven by a turbine attached to a high-pressure air blast—but one which required extreme nicety in machine shop work. By dispensing with the elaborate auxiliary optical equipment of Svedberg's analytical ultracentrifuge, the McBain spinning top enormously reduced the space required for sedimentation measurements, but the instrument never became as popular as it deserved to be.

McBain trained many graduate students. For the times, McBain was unusual in that he succeeded—by dint of much effort—in obtaining sizable gifts from U.S. industry to support his researches, and he was much ahead of his time in gathering around him a group of postdoctoral scholars who, although informally, added much to the education of his graduate students. He maintained a very extensive card index of literature topics, and, even on a casual request, was able to provide his students with fifteen or twenty up-to-date references on almost any aspect of colloid chemistry. His research output was large: he wrote two books and more than 450 papers. Yet his scientific reputation, though in no way diminished by such productivity, rested mainly on his imaginative, simplified way of looking at problems. He rarely planned unnecessarily complicated experiments, and a great deal of his research was carried out using standard stockroom equipment. The bold-frontedness of his attack on scientific problems, and his refusal to be intimidated by quite formidable problems, were recognized by his election as Fellow of the Royal Society in 1925, by the award of that Society's Humphry Davy Medal in 1939, and by the award of honorary degrees from a number of institutions.

Retirement was fundamentally unthinkable to a man with McBain's temperament. Although he looked forward to the freedom for travel when he became emeritus, the notion of discontinuing his research was far from his mind. He planned to move some of his work to the fledgling Stanford Research Institute in Menlo Park, but before that move had proceeded very far he was invited by Prime Minister Nehru to build, and to serve as the first Director of, The National Chemical Laboratory of India. Overcoming great obstacles in the construction of the laboratory in Poona, he quickly assembled a competent staff which, with the initial assistance of a few young chemists who accompanied him,[2] soon settled down to a

2 **Notably S.S. Marsden, now Professor of Petroleum Engineering at Stanford.**

program of research on chemical problems of national economic importance to the nation. In three years McBain provided a base for the economic development of India, on a scale which rarely falls to the lot of any individual. When he left India, his successor, G. I. Finch, inherited a thriving scientific laboratory.

That the appointment of McBain to the faculty at the rank of professor was a marked aberration from the normal pattern is clearly shown by the fact that between 1903 and 1960 McBain was the only person appointed at tenured rank in the Department. (From the 1960s onward this pattern has changed radically: now the more common entry to the established faculty of the Department appears to be at the tenured level rather than by progression through the ranks.)

George S. Parks entered the department as instructor in 1920, after obtaining his Ph.D. at the University of California, Berkeley, and after one year's teaching service at the California Institute of Technology. As a graduate student he developed an interest in experimental thermodynamics, calorimetry in particular, which he followed unswervingly for the whole of his professional career. A quiet, unspectacular individual, he carried out most of his researches in the basement of a small building located where one of the lawns in front of the Stauffer Physical Chemisty Laboratory now spreads. That extraordinary building, which began its life as an Assay Laboratory, fitted with furnaces and cupels for relatively large-scale reduction of ores to metals, eventually became the home of Chemical Engineering, prior to the construction of the third Stauffer Laboratory. In the basement of that assay building there was a small laboratory which must surely rate as one of the most substantial, most isothermal rooms ever constructed on campus. When the time came to demolish the basement it was found to have concrete walls which were eight feet thick in some places. It was an admirable place in which to use sensitive suspension galvanometers, and, from the standpoint of the calorimetrist, the room had the magnificent feature that, with no artificial aids, the temperature remained constant to within less than +-1°C from one year's end to another. I used that laboratory in succession to Parks and, whatever its dungeon-like qualities, it was never bettered as a thermodynamic laboratory.

Parks' calorimetry was all developed in the days before recording instruments and stable dc amplifiers were available. Temperature changes were registered either by means of platinum resistance thermometry or multijunction copper- constantan thermocouples, and, in order to measure changes to a precision of $\pm 0.001°C$, it was necessary to use the most sensitive suspension galvanometers available, which in turn called for stable, vibration-free rooms. The "annex basement" formed the locus for Parks' work for nearly four decades.

Parks' thermodynamic measurements fell into two classes: measurements of heats of combustion, giving ΔU for the combustion process (from which ΔH is readily derivable by using the gas laws), and measurements of heat capacities, from which, with the third law of thermodynamics, entropy calculations may be extracted. Finally, from the relation $\Delta G = \Delta H - T\Delta S$ the free energy change, and the standard free energy change, of a reaction may be evaluated.

To chemists who only use thermodynamic data and have no hand in obtaining such data in the laboratory, experimental thermodynamics often appears to be dull. The fact is that although the measurements involve much repetition, the satisfaction which attends the precise measurement of a quantity which has a high probability of enduring for all time is enormous. Parks felt that satisfaction deeply, and he raised precise calorimetry, using relatively simple apparatus, to an art form. In his lifetime, and with the assistance of a small number of gifted graduate students, he obtained a significant fraction of the currently available thermodynamic data for hydrocarbons and oxygenated hydrocarbons. He took his pleasures simply, and, as befits someone who dedicates his life to the accumulation of dependable measurements, he was cautious and conservative. Yet it is often the truly conservative who yields the most progressive products. It was so in the case of Parks in at least three distinct areas. His researches made possible sound (i.e. thermodynamically sound) estimates of hydrocarbon isomerization, and this provided the stimulus for much research in catalytic conversion. Internally in the department, his term as Executive Head may have tended to give the impression of the modest caretaker, but it should be noted that it was on Parks' initiative-which was persistent rather than forceful—that chemical engineering at Stanford took its present form. Finally, when, in the late 1950s, it became clear that the University had made up its mind on the desirability of recruiting a new Executive Head from outside Stanford, he appreciated the logic of the situation and enthusiastically supported that policy, even though it implied a radical break with the habits and traditions that he had held for a lifetime. As a teacher of undergraduate classes, Parks was a conscientious and thorough, rather than a stimulating or sparkling, figure.

Parks' predecessor as Executive Head was Philip A. Leighton, who joined the faculty in 1928, after receiving his Ph.D. from Harvard University. His training and the bulk of his early researches were in photochemical reaction kinetics. An excellent experimentalist himself, he trained a succession of first-rate graduate students who pursued notable academic careers.[3] He delighted in well executed experiments, whether at the research level or in large freshman lecture classes; and he enlivened the chemistry 1, 2, 3 lectures for many years by appropriate and occasionally (literally) flamboyant demonstrations. Warm-hearted, good-humored, sociable, and utterly lacking in. pomposity, Leighton inspired devotion and affection in his colleagues. In return he dealt with them honestly, openly, and with unsurpassed loyalty.

Tom Kuhn, in discussing science,[4] likens problems to puzzles, on grounds that it is assumed, to an extent that makes the declaration of that assumption unnecessary, that every scientific puzzle has an answer. Granted that assumption, it is easy to argue that scientific skill may, and perhaps should, be judged on two scores: first, in obtaininga solution to a puzzle, and second, in obtaining an ingenious and elegant solution to a puzzle. Leighton loved puzzles, chemical or otherwise, and he loved ingenuity, whether it appeared in a joke, a reaction mechanism, or in mechanical gadgets. In the days when "hi-fi" was still relatively crude he took great pleasure in building amplifiers and complex speaker systems; and in the laboratory he took equal pleasure in ingenious apparatus. One such piece of equipment was the focused reflecting grating spectrograph built by Paul Cross, which for many years was mounted on heavy concrete piers in the basement under Room 20. Similarly in investigations of a number of weak absorption bands in gases it was necessary to have a very long path length for the optical beam which led to the grating spectrograph: Dave Volman and Leighton constructed a 150 foot Pyrex pipe system which ran down most of the length of the basement.

Although Leighton wrote many papers dealing with photochemical kinetics, two of his publications stand out particularly. His first monograph, written in cooperation with W. Albert Noyes and published as an A.C.S. monograph, "The Photochemistry of Gases (1941)", is now a classic and something of a collector's item: his second monograph, "The Photochemistry of Air Pollution (1961)", appeared at a most propitious time, contained a vast reservoir of

[3] F. Blacet, P. Cross, D. Volman, and N. Smith, to name but a few.
[4] T. Kuhn, The Structure of Scientific Revolutions.

information of great relevance to a major problem of large cities, and has been in good demand ever since it appeared.

During the latter part of World War I1 Leighton, the most unmilitary of men at heart, served as a colonel in the U.S. Army at Dugway Proving Grounds, Utah, and was involved in a large research effort there. Returning to Stanford after the War, he devoted much of his efforts to two projects. First, he was very active, along with Dr. Swain, in establishing the Stanford Research Institute, whose earliest contracts were almost all for chemical research, and which was housed temporarily on the Outer Quad until the Institute moved to quarters in Menlo Park. Second, with a group of collaborators[5] from the Dugway work, housed in wooden buildings on what is now the site of the Stauffer Physical Chemistry Laboratory, he began a long series of researches in micrometeorology. This group developed a wide variety of refined instruments for the study of microclimates, wind movement, and the movement of small airborne particles.

Although much of the work was classified, the members of Leighton's research group mixed freely with the members of the Department, who were regularly the recipients of useful advice and equipment. Grinnell, in particular, a gifted electronics designer, lent his time and skills to many graduate students and faculty members in the days when stable amplifiers, both ac and dc, as well as recording instruments, were becoming increasingly important in chemical research equipment. Grinnell also taught courses in the applications of electronics to research. Perkins, like Grinnell a chemist by training, had become a well-informed astronomer, and he contributed to the University's teaching by giving courses in astronomy in the Physical Sciences Program. In 1959 this research group was obliged to relocate its laboratories because plans for the Stauffer Organic Laboratory called for some of the space occupied by the wooden buildings. Rather than relocate at another site on campus, the group chose to organize as the ronix Corporation and they moved into a new building in the Stanford Industrial Park. Leighton became Emeritus Professor in 1962, retiring to Little Creek Ranch in Solvang, California, which he had owned for some years. In these days his visits to the campus are all too rare.

As has been hinted above, Leighton, an excellent teacher himself, was deeply concerned with the quality of instruction in the Department. It fell to his lot to make a rather large number of junior appointments in the Department in the mid-forties, and many current members of the faculty will recall that Leighton both encouraged and rewarded good teaching. For almost his entire career in the Department he taught the year-long introductory chemistry course. His lecture notes were up-to-date, his delivery, though the exact opposite of oratory, never left the student in any doubt as to which items in the lectures were of central importance, and his frequent use of the large-scale experimental demonstration both added much fun to his classes and drove home important principles.

The late 1940s and early 1950s represent, as has perhaps been said too often, a watershed in the history of the physical sciences in universities. Science was well regarded in World War I, though scientists were not; but by the time of World War I1 the close relationship between science, technology, warfare, and global politics had been well established and understood. The NDRC and OSRD had placed money in universities during World War I1 in contracts for particular researches. In the postwar years money began to *flow*, if not to flood, into universities, not only from the Office of Naval Research and other government agencies, but also from industrial concerns who were also beginning to learn that fundamental research is a commodity to be supported, not merely one to be exploited. Stanford's Chemistry Department was not particularly aggressive in seeking out funds for research in the mid- and late 1940s, but money was beginning to arrive. In addition, under the prodding from Leighton, the University began to supply modest but useful funds for faculty research. A sum of $400 per faculty member per annum appears miniscule by present inflated standards, but in those years $400 represented two sensitive galvanometers, a good quality analytical balance, a colorimeter, or a polarimeter. The same sum of money purchased a considerable amount of glassware, solvents, and specialty chemicals.

The Department, which had responded to the needs for a scientifically informed soldiery by teaching large numbers of accelerated courses in chemistry, found that it barely had time to take breath before the rush of ex-GI graduate students began. The Main Building was crowded, as more and more M.S. and Ph.D. students came to study under the GI Bill. Young instructors and full professors alike were in the position of having more graduate students than they could properly accommodate. Yet in spite of the crowded, unsatisfactory conditions; in spite of the fact that research funds were limited; and in spite of the fact that many questions, which are now answered in mere minutes by approp-

[5] **S. W. Grinnell, W. A. Perkins, and F. X. Webster.**

riate N.M.R., infrared, or positive-ion mass spectrometry, had to be solved by lengthy, tedious procedures, the quality of the graduate students and their achievements in those years was remarkably high.[6] Camaraderie was strong: all members of the Department, students and faculty alike, pulled together with a will; and, though the Department has never had a more conscientious, hard-working group of graduate students than those of the late 1940s and early 1950s, it was a cheerful, fun-loving place.

A very broad spectrum of chemical interests was followed vigorously during that period. To give only the briefest of outlines, the following list suggests the range of activities. Bonner, carbohydrates; Eastman, terpenes; Griffin, carcinogens; Hutchinson, micellar solutions; Koenig, component theory; Johnston, shock-tube reactions; Loring, tobacco mosaic virus and ribonucleic acids; Luck, plasma proteins; Mosher, heterocyclics; Noller, natural products; Ogg, gas phase kinetics; Skoog, polarographic analysis.

It was during this period that Hubert S. Loring carried out some of his most distinguished work. Loring took his Ph.D. degree with Vincent DuVigneaud at the University of Illinois and George Washington University, working on sulfur-containing amino acids. Later, at the Rockefeller Institute, he worked with Wendell Stanley on the crystalization of tobacco mosaic virus. At Stanford Loring began a long series of researches on poliomyelitis virus, and, in collaboration with Carlton Schwerdt (now of the Stanford School of Medicine), isolated the Lansing Strain of that virus. From that work, in collaboration with Sidney Raffel (Stanford Medical School), there emerged a prototype polio vaccine effective against cotton rats—a development which set the stage for polio vaccines effective in humans, through the work of Sabin and Salk. Loring continued his work on tobacco mosaic virus, and launched another series of researches on the effects of trace elements on the properties and structures of ribonucleic acids.

As an individual, Loring was shy and retiring almost to the point of being antisocial. As a scientist he was dedicated, methodical, and scrupulous. In the minds of some, his lack of boisterous pushiness resulted in his not receiving appropriate public credit for researches which were genuinely pioneering; but there is no reason to think that he himselt felt either regret or disappointment at the lack of public recognition. For him, his researches were all that mattered, and they .constituted his own rewards.

[6] We might note that Jack Tessieri (Texaco), Ed LaCombe (Union Carbide), M. Baruch (Chevron Research), and John Bills (Kerr McGee) were all graduate students during this period.

If Hubert Loring was quiet, retiring, and persistent in research, Richard Andrew Ogg, Jr. was the total antithesis. Flamboyant, brilliant but erratic, too easily bored with ideas to see them through to a satisfactory conclusion, and gregarious to the nth degree, Ogg became a chemistry legend even in his youth.

It is impossible to write about Ogg with any objectivity, and to do even scantjustice to this extraordinary individual would take many times the space available here. Born in Colorado, he entered Stanford as a freshman in 1925. Graduating with great distinction in 1929, he worked with Francis Bergstrom for the M.S. degree and then took his Ph.D. degree under Philip Leighton, studying the photochemistry of liquid ammonia systems. He spent a year with Kistiakowsky at Harvard in 1932 and a year with Michael Polanyi at Manchester in 1933, where he studied free-radical reactions using the "sodium flame" method which Polanyi had devised. Except for those two years, Ogg's entire career was centered on Stanford—and, it should probably be said, on Ogg. He possessed one of the most genuinely catholic minds that I have encountered in a long career. To paraphrase the old Latin tag, of Ogg it could truly be said "*nihil chemici mihi alienum est*" (nothing chemical is alien to me). He had a mind of such versatility that he could discuss with equal ease and unbounded fluency the laboratory conditions for an organic synthesis, the structure of a complex inorganic ion, the mechanism of a photochemical reaction, and wave mechanics. His mind was receptive beyond measure. His memory and recall were prodigious. His intuitive grasp of the essentials of even lengthy theoretical papers was, in my experience, unique. Had he been purely and simply a chemist (and one could not with adequate representation of his talents label him as a particular kind of chemist), Ogg might well have been merely admirably, yet tiresomely, well-informed. But the original Latin tag also applied to him with full force: "*nihil humani nihi alienum est*"(nothing human is alien to me). He was a voracious reader, especially of verse, which he could command up from the storehouse of his memory with great ease; he was well read in history; he was well informed and appreciative as regards music of all tastes and periods; and his circle of acquaintances was enormous. His make-up had in it much of the theatrical and dramatic, but withal a good sense of the ludicrous—a sense which was called upon at least once when, in my early days at Stanford, we appeared as two raggle-taggle pirates in the Christmas production of Gilbert and Sullivan's "Pirates of Penzance" at the Palo Alto Community Center. If these varied talents did not suffice, Ogg could call on considerable athletic ability. He was an accomplished horseman (riding to the laboratory during wartime gasoline rationing), a keen tennis player, and an almost recklessly able skier. In short he was, and quite self-consciously strove to be, as closely as is possible in this age, the polymath or Renaissance man. Anecdotes of Ogg abound, but to recount one would offer the irresistible temptation to tell a hundred, and that seductive temptation must be put on one side.

Yet if one looks at the chemical literature, one is immediately struck by the fact that the lasting record of Ogg's achievement is incommensurate with his inordinate talents. To be sure, some things stand out. His work in .elucidating the role of NO_3 in the decomposition of N_2O_5 (that reaction which resolutely refused to turn into a second-order process at low pressures), clarified much of the mystery of a classic reaction from which modern gas-phase kinetics can in some degree be said to spring. His penetrating intuitive sense of the value to chemical investigations of nuclear magnetic resonance, and of the importance of spin-spin coupling, was a sure sign of his highly developed flair for the significant. On the other side, however, much of his misplaced enthusiasm and work, related to what he believed to be superconductive behavior in metal-ammonia systems, has simply not stood up to more careful study. However, when that has been said, it is necessary to say immediately that quantitative records of papers published, papers cited, and papers surviving, are a misleading and, as in Ogg's case, quite inadequate representation of a person's contribution to scholarship. They are the mute, impersonal record; Ogg's unrecorded contributions may be found (but not seen) in the publications of many of his colleagues and friends. For Ogg generated ideas with such ease, with so little provocation, and in such large numbers, that he was able to be prodigally generous with them. Graduate students, his own and others', and seminar speakers all felt the impact of his fertile mind-sometimes involuntarily.

It was, perhaps, inevitable, that such a highly charged collection of talents should consume itself quickly—Ogg once told me that he found no savor in routine research work, and that if he ever made any great contribution to chemistry it would result from what he described as "chemical wildcatting". With his death the Department became a less turbulent place, but it undoubtedly became less stimulating and less stimulated.

A man of totally different stamp was Frederick O. Koenig. Born in New York, the son of a German immigrant who built and established an outstanding private preparatory school, Koenig earned his A.B. degree at Harvard. From there he went to the

University of Munich, where he studied for the doctorate under the eminent electrochemist Kasimir Fajans. As a result of his doctoral work he was invited to contribute to the Handbuch der Physik what has become a classic paper on the electrocapillary curve. Following a year at Berkeley as the holder of a National Research Council Fellowship, Koenig joined the Stanford faculty in 1931. From that time until his retirement he was one of the mainstays of the freshman teaching program. At the graduate level he taught courses in Gibbsian thermodynamics which, for elegance of exposition and rigor of intellectual tone, have rarely been equaled and never surpassed.

It is doubtful that a sloppy phrase or a muddled idea was ever expressed by Koenig. His was a logical, ordered mind which rejected the commonplace and tawdry. He worried -- oh, how he worried — about the fundamentals of thermodynamics. I recall that for two quarters a few of his graduate students and I joined him in a seminar dealing with the thermodynamics of surface layers. From that seminar emerged the first really clear statement of the absolute necessity to import into thermodynamics, by means of an auxiliary equation, a physical or conceptual view of the position of the boundary surface separating two phases. Quite typically, that had been a subject which many surface chemists had glossed over, in spite of its basic importance; but for Koenig, until the matter was thrashed out and clarified, it was a near scandalous lacuna in a subject to which soundness of logic is essential. In another quarter, I recall, a similarly small group of us dissected and compared the various statements of the Second Law; and I recall that, with elegance and irresistible logic, Koenig drew us to the conclusion that Planck's formulation is the statement of greatest generality. It was characteristic of the man that when Koenig perceived that in order to carry out this particular analysis it was necessary to learn Boolian algebra he promptly taught himself. Few people, I suspect, were really aware of the days, even weeks, which were often devoted to serious study which formed the basis of short, disarmingly simple statements which formed part of his freshman lectures. He undertook no task loosely and he made no statement glibly.

In both the style and, to some extent, the content of his scholarship, Koenig had many points of resemblance to Stillman, Stanford's first chemist. It is natural, nay essential, that if one wishes to teach a subject soundly and fully, then one is obliged to become well informed regarding the accurate history of the subject, and, indeed, regarding history in general. In Koenig this logical requirement was strongly reinforced by a genuine, scholarly love of history; and it was but natural that, like Stillman, he should have become captivated by the history of science. Early in his Stanford career and again in his retirement years, Koenig gave classes in the history of science which were models of their kind. Unhappily, Stanford has never seen fit to venture much of its assets in this essential scholarly field, and Koenig felt the disappointment keenly.

The recorded body of Koenig's work is not extensive. Perhaps he read and deliberated too much to leave behind a large corpus of written work—if, in written work, we exclude the beautifully prepared lectures that he wrote anew each year. The greater is the pity, for he had in his lifetime educated himself to a level of scholarship which it seems almost criminal to lose at one stroke in the death of a great scholar. Orally, he cleared away much trash and incorrect knowledge from the minds of his colleagues, yet writing never appeared to come easily to him in spite of great ability with language. No doubt his reluctance—his distaste, even—for statements which could not be supported by the fullest scholarship, prevented him from indulging in facile writing. From the heritage of scrupulously accurate scholarship that he acquired from his father (and no doubt, also, further developed in his years in Munich) he could not escape: nor would he have wished to. Nevertheless it is a matter of deep regret that he never completed a projected text on Gibbsian thermodynamics. But, to use a thermodynamic metaphor of which Keonig might have approved, a person's scholarly contributions constitute an extensive property, the product of an intensive factor (quality) and a volume factor (quantity). The fact that Koenig's scholarly contributions were large in spite of their small quantity says much about the quality of all that he wrote.