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HERMANN IRVING SCHLESINGER

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A Biographical Memoir by

GRANT URRY

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BY GRANT URRY

IT IS A CONSIDERABLE pleasure for me to have been asked to comment on the life and career of my mentor. There are among his more than fifty former graduate students more distinguished scientists, but it is unlikely that any among them remembers their tenure in the laboratories of Dr. Schlesinger with more gratitude or greater fondness than I. At the outset it should be said that my memory has been greatly aided by information furnished through the kind efforts of Ms. Joan Shiu of the Department of Chemistry at the University of Chicago and Dr. Elizabeth J. Sherman at the National Academy of Sciences.

One of the outstanding chemists of this century, Professor Schlesinger's greatest contribution to his chosen profession was the discovery of the borohydrides and the aluminohydrides. He devised simple, high-yield syntheses of these classes of compounds, making them available as uniquely valuable reducing agents, particularly for various functional groups in organic compounds. Without the ready availability of lithium borohydride and lithium aluminohydride the current state of medicinal chemistry and molecular biology would undoubtedly be severely retarded.

He would have viewed the students he trained as his greatest contribution. It is true that among this select group are

several distinguished scientists who interpreted and extended his discoveries to make them commonplace utilities. A genuine humility made Professor Schlesinger a much-loved colleague; it also possibly accounts for the great disparity between his stature and the modesty of the reputation he was accorded during his lifetime.

Among the honors he enjoyed during his career, he prized most highly his election to the National Academy of Sciences in the spring of 1948. The other honors he felt worthy of mention were: honorary degrees from the University of Chicago and Bradley University; the honor scroll of the American Institute of Chemists; the George Fisher Baker lectureship at Cornell University; the Julius Stieglitz memorial lecture and the J. Willard Gibbs Medal of the Chicago section of the American Chemical Society; the Edgar Fahs Smith memorial lecture of the Pennsylvania section; and the William Albert Noyes memorial lecture at the University of Illinois. He was a member of the Bavarian Academy of Sciences and was awarded the Alfred Stock Memorial Prize of the German Chemical Society. The U.S. Navy presented him with its highest honor, the Distinguished Public Service Award. In 1959 he was awarded the Priestley Medal, the most prestigious award of the American Chemical Society. Near the end of his career he was much amused by being characterized in the popular press as "the Father of Rocket Fuel."

Hermann Irving Schlesinger was born October 11, 1882, in Milwaukee, Wisconsin. When his family moved to Chicago he was six years of age and enrolled in Schultz's School, a private grammar school established by the German-American community. The master's traditional German discipline had been tempered somewhat by the democratic principles of his adopted country. His school maintained typically Germanic attention to detail and rigor in all other respects.

Upon graduation from Schultz's School in 1896, the young Schlesinger entered Lake View High School of the Chicago Public School System. There he came under the tutelage of a science teacher, Mr. Linebarger, remarkable in his or any time. This high school teacher accomplished publishable research in his laboratory outside the usual heavy duties of teaching chemistry and physics. Under this benign influence Dr. Schlesinger's career choice was early and firm. When he entered the University of Chicago in 1900 it was to be educated as a chemist.

The faculty in chemistry included John Ulric Nef, Alexander Smith, and Julius Stieglitz. Albert Michelson and Robert Millikan were members of the department of physics. All of these, in addition to being eminent scientists, were exceptional teachers and their influence upon Dr. Schlesinger was evident throughout his long career. The ambience in this young university was easy going and informal. Relations between the faculty and the students were sociable and congenially close.

His undergraduate studies were completed with a laudatory record of achievement. In the overwhelming majority of his courses he earned the highest possible grade. His poorest performance earned average grades in two courses and in the recitation portion of a third.

After this stellar undergraduate performance he chose to complete a thesis under the direction of Julius Stieglitz. After an expected excellent graduate career, lasting just over two years, he was awarded the doctorate in 1905.

In September of that same year he journeyed to Berlin, there to work in the laboratory of Professor Nernst. The year he spent in Berlin was a greatly broadening intellectual experience for him. Again, he was in the midst of great movers of science. Landolt, though old and feeble, still came to the laboratory to conduct experiments that

extended his classic proof of the law of conservation of mass applied to chemical reactions. Nernst had just announced the third law of thermodynamics; Planck had recently developed the quantum theory; Emil Fischer was engaged in his study of proteins and amino acids; and Van't Hoff was busily studying the origins of the great Stassfurt salt deposits. With the exception of Nernst, who gave no lecture course that year, Schlesinger attended lecture courses by all of these great men.

In spite of close personal relationships with Nernst and Van't Hoff that developed during the year at Berlin, fifty years after the event Dr. Schlesinger would compare unfavorably the rigid and remote formality of faculty-student relations at Berlin with his earlier experience at Chicago.

In August of 1906 he left Berlin to hike through the Bernese Oberland prior to joining Professor Thiele at Strassburg in September. Apparently this time with Thiele was not as rewarding as his time in the German capital, in spite of his work on the diazotization of dichlorostilbene. His later memories of this period were more of his journey through the mountains than his work in Thiele's laboratory. Of his experiences with Thiele the only one he told me nearly fifty years later was humorous but meaningful.

One of Thiele's students had gained a reputation of sorts among his peers as being capable of obtaining fantastically accurate analytical data and unbelievably complete material balances in all his experimental work. Thiele, while passing this student's work area one day, tipped the ash from an ever present cigar into one of the samples on the bench. The next day, after this paragon had reported the results for this particular sample, Thiele exclaimed, "My God! The cigar ash weighed nothing!"

The message of this story was not lost on Professor Schlesinger. Throughout his scientific lifetime he was care-

ful to have any results reported to him confirmed by one means or another before he would prepare them for publication. This was managed in such a graceful manner that no implication ever existed similar to that of the Thiele story.

In February of 1907 he returned to the United States and joined Professor Abel, one of the world's leading physiological chemists, at Johns Hopkins University. He had just begun to make progress in isolating the toxic principle from *Amanitas phalloides* when Professor Nef invited him to return as an associate in chemistry at the University of Chicago. His principal duty was to be the teaching of general chemistry. To make his research interests compatible with this task, he began research in a field that was novel for him. His breadth of training in all the other fields of chemistry prepared him for a lifetime career during which he made many outstanding contributions to his newly chosen field of inorganic chemistry.

In 1910, the year he was appointed instructor in chemistry, he married Edna Simpson, a member of the gifted family that included the distinguished paleontologist, George Gaylord Simpson. Throughout their long life together Mrs. Schlesinger's intelligence and gentle good humor eased many of her husband's burdens. My own acquaintance with her came as a consequence of the dinner parties she gave for the Schlesinger research group. We all called Professor Schlesinger "the Boss" outside his hearing. I was never to reach the exalted state of Anton Burg and Herb Brown in addressing him as "Hermann." Mrs. Schlesinger sensed that our perceptions of "the Boss" were sometimes unrealistically greater than life. Her stories of her experiences with "Hermann" would invariably display him in a loving, more human light.

It is possible for me now to recall only one of these. When their sons were quite young she had asked "the Boss"

to take the children to the lakeshore at the 55th Street promontory for some time in the sun. Typically, he took the most recent journal to read while he overlooked their play. She was startled some hours later to see him returning, journal under arm, without the boys. His absorption in the chemistry he was reading had driven all thoughts of domestic responsibilities out of his mind! Dr. Schlesinger seemed always to enjoy the humor of these family legends as much as anyone. His contentment in life with his wife, "Teddy," was apparent to anyone seeing them together.

It was his lifetime habit to combine family and professional lives harmoniously. He lived for much of his life in an apartment building owned co-operatively with Professors Thorfin W. Hogness and Warren C. Johnson. He spent his summers with his family in a summer cottage compound, at Elk Lake near Traverse City, Michigan, shared with these and other university colleagues. Throughout his entire life he continued an active and important role in the family company, begun by his father. It was a fine woodworking factory specializing in architectural panelling and furnishings.

This melding of the personal with the professional had important scientific consequences in one case known to me. Both of his sons enjoyed very successful careers in advertising. Richard's career continued in Chicago, but Allen moved with his young family to Minneapolis. Dr. and Mrs. Schlesinger made many visits to the grandchildren there during which he would "touch base" at the University of Minnesota. During one of these academic visits he encountered a young assistant professor in chemistry who shared his interest in the structures of the boron hydrides. Over many years he was to carry carefully purified samples of all of the hydrides and related compounds prepared in his laboratory at Chicago for Professor Lipscomb's study by x-ray crystallography.

He was promoted to assistant professor in 1911, to associate professor in 1917, and to professor in 1922. Professor Nef had died in 1915 and Professor Stieglitz became chairman in that year. Upon becoming a full professor Schlesinger was appointed by Stieglitz as secretary of the Department of Chemistry. In 1933 Professor Stieglitz retired as chairman, but no new chairman was appointed and all of the administrative duties of the department were borne by the secretary. In 1946 Professor Warren Johnson was appointed chairman, relieving "the Boss" of the many duties he had discharged with such graceful equanimity throughout the period of great trials and triumphs prior to and during World War II.

My own university training was entirely at Chicago. I had no experience of other chemistry departments, but even I was perceptive enough to notice the unusual administrative structure in that chemistry department. The reluctance of the university administration, while Robert Maynard Hutchins was president, to appoint a chairman in chemistry must have had an interesting rationale. While I knew him, Professor Schlesinger never gave me any clue that would explain this unique situation.

Others have commented upon this matter so I feel free to add my speculation as to a possible reason for the patchwork administration in the chemistry department during these twelve years. Robert Maynard Hutchins arrived at the University of Chicago as president at the age of twenty-nine. He was full of himself and the glory of the logic in law. It is possible that he saw the huge costs of laboratory instruction, as have university presidents before and since, as an avoidable burden. He suggested and made as if to implement his plan to teach sciences without the necessity of costly laboratory instruction. Professor Schlesinger undertook the thankless task of educating this young adminis-

trator in the nature of science. In faculty meetings as well as in the published literature "the Boss" invested this battle. It is clear from history that Hutchins did not win this point. It is also clear that this disagreement did not prevent Professor Schlesinger from participating whole-heartedly in many science education projects dear to Hutchins's goals. His ground-breaking films for ERPI were among the best early audio-visual aids. These, along with Schlesinger's *General Chemistry* (1925), were used in the physical sciences survey courses at Chicago. I can only suggest that Hutchins was bruised in this exchange and was human enough to let his resentment express itself in this foolish way. Let me hasten to add that this is my own speculation and does not deserve the weight of fact.

A peripatetic academic career during the past thirty years has resulted in a perspective that allows me to assess Dr. Schlesinger's impact upon the Department of Chemistry at the University of Chicago with more confidence than I once could. As student and faculty member he lived in the department for most of the first sixty-five years of its existence. When I left Chicago to embark upon the first of several academic positions, I assumed that all departmental faculties were mutually supportive and that among such faculties the human conflicts and destructive competition I had encountered in the many non-academic jobs I held while financing my education would be ameliorated. Sadly, the Department of Chemistry at Chicago during Dr. Schlesinger's long career was unique in my experience. Since his death the department apparently has become more like all other departments. It is an inescapable conclusion, for me, that this temporal coincidence is a true measure of the idealizing influence that "the Boss" enjoyed during his time in this department.

The Schlesinger research group was active during the

days of World War II with research that has had far-reaching consequences. As was common the work was always directed towards a war-related goal. One of the earliest efforts was prompted by the need to find volatile compounds of uranium suitable for use in the diffusion separation of the uranium isotopes. Uranium borohydride is, after uranium hexafluoride, the most volatile compound of uranium known. Uranium hexafluoride presented serious handling difficulties, which required the directors of this effort to seek alternatives. When the synthesis problems had been sufficiently well defined by the Schlesinger group, it was decided to see what kinds of other difficulties might arise in the use of a boron compound for uranium separation. Fermi's purported comment when he observed the slow neutron cross section for boron—"My God! It's as big as the side of a barn!"—not only gave a name to the unit of cross section, it put finish to thoughts of using uranium borohydride for the diffusion process.

Meanwhile, some officer in the U.S. Army Signal Corps had been having problems with farmers. It was necessary for field units of the corps to generate hydrogen gas in the field in order to loft their radio antennas to altitudes where they were useful. They had been using a mixture of ferrosilicon and sodium hydroxide for this purpose. This reaction produced a satisfactorily large volume of gas per unit weight but suffered from two serious problems. After the hydrogen had been generated the generator was filled with a rock-like mass of silicates that a GI had to chip out before the generator could be re-used. These product silicates had a salty taste much loved by farmer's cows. Unfortunately, the acid in the cow's stomachs converted the silicates to silica. The cows with their stomachs full of silica would slowly starve to death, presumably without ever being hungry! Someone in the signal corps had noted in the

reports from the Schlesinger group the high yield of hydrogen in the hydrolysis of lithium borohydride. The Schlesinger group then began research under the auspices of the Signal Corps.

In the early days of the signal corp research program it was pointed out that this use of lithium borohydride would rapidly use up the world's known supply of lithium. Accordingly, the efforts of the Schlesinger group were directed towards the synthesis of its congeneric salt, sodium borohydride. This turned out to be a formidable task and it was in this effort that H. C. Brown made his greatest contributions to "the Boss's" research. Most of us will remember the monumental series of eleven papers co-authored by Brown and Schlesinger, along with many collaborators, that appeared after the war when they were free to publish.

What was never published was the great consternation at Chicago when the first carefully prepared sample of the new borohydride was "hydrolysed." It was to be an occasion of some importance, and the "brass" from the Signal Corps were invited. Everyone watched as the sample, which had been prepared and handled only in vacuum lines because of the remarkable reactivity of such hydrides, was dropped into water. This beautiful white crystalline solid merely dissolved! No hydrogen was evolved! The remainder of the Signal Corps project was devoted to a search for a catalyst for the hydrolysis of sodium borohydride. This search gave Brown the idea that later resulted in his hydrogenation chemistry, where a sodium borohydride solution in water is added to an olefin in which is suspended a platinum-charcoal catalyst.

Later experiences of the research group gave all of us the feeling that for defense purposes the boron hydrides would always be a bridesmaid but never a bride! There exists an overwhelming literature that documents the util-

ity of these interesting compounds in all aspects of chemistry. We have all, I am sure, been able to overcome any disappointment the failure of borohydrides in the defense effort may have caused us.

Near the end of his life Professor Schlesinger, to my knowledge, was twice nominated for the Nobel Prize in Chemistry to honor his landmark work with boron hydrides. My opinion of his generous spirit makes me confident that, were he alive, he would have been made as happy by the awards to Lipscomb and Brown as if he himself had been accorded the honor.

Professor Schlesinger did not suffer from the pride of ambition but he was human enough on one occasion to display pleased vanity. During my graduate student days, my wife was reading Saul Bellow's most recent novel, *The Adventures of Augie Marsh.* She came across a passage where the protagonist was instructing a novice in the fine points of shoplifting books from the University of Chicago Bookstore. In his instructions the expert pointed out that you should only steal good books that were easily sold such as Fuller's *Botany* and Schlesinger's *General Chemistry.* "The Boss" was delighted with this when I pointed it out to him the next day in laboratory.

There were also times in his research when fortune did not smile upon him. Professor Nan V. Thornton, who enjoyed a long and distinguished academic career at Randolph-Macon College in Lynchburg, Virginia, was a graduate student in his laboratory during the late 1920s. Her thesis research involved the study of arc-induced reactions of various chloro-fluoromethanes. Using the vacuum techniques that Dr. Schlesinger did so much to develop, she had characterized and measured the yields for all of the volatile products arising from this complex reaction. She was left with a pea-sized lump of a waxy white solid. It contained

enough material to prevent her from obtaining a material balance that was satisfactory to her. All of her efforts to dissolve this solid in order to confirm the composition, which she could estimate from the material balance data, were in vain. It did not dissolve in fuming nitric or sulfuric acids even near their boiling points. At the end of her tether she went to "the Boss" with her problem. He pointed out that she had completed enough work for a thesis and should devote that thesis only to the volatile products. This was a kind decision since it accelerated Professor Thornton's doctorate considerably. If Dr. Schlesinger had been less kind they might have been credited with the discovery of Teflon some fifteen years earlier than the DuPont work!

The most important lesson he taught me was two years after my leaving Chicago. It was at the national spring meeting of the American Chemical Society in 1958, and with the exception of the meeting at which he was awarded the Priestley Medal, was the last meeting he was to attend before his death in October of 1960.

With Anton Burg he had accomplished an elegant experiment in the early 1930s. In that experiment they treated a solution of diborane in liquid ammonia with metallic sodium and obtained only one-half mole of molecular hydrogen per mole of diborane. It was perfectly reasonable for them to conclude that this proved an ammonium salt of the novel "diboranoamide" anion as the best formulation of the "diammoniate of diborane." Later, in the same laboratory, Schlesinger and Burg, working with R. T. Sanderson, another student of Schlesinger's, discovered aluminum borohydride. The next twenty years were spent synthesizing and characterizing many examples of this new and useful class of double hydrides. During the same twenty years Anton Burg had become a widely respected professor of inorganic chemistry at the University of Southern California.

Early in the 1950s, the then-young R. W. Parry, an assistant professor of inorganic chemistry at the University of Michigan, began a series of experiments giving results that suggested that the earlier formulation for the "diammoniate of diborane" could be improved. His experiments suggested a novel cation, the "diammonoboronium" ion with borohydride as the counter ion. For the next seven years the literature and the national meetings served as a forum where every piece of Parry's experimental evidence was contested by Burg.

Anton was justifiably proud of his formidable experimental talents and it was inconceivable to him that Parry could be correct. Burg had become so involved in this controversy that a large portion of his energies, intellectual and experimental, were devoted to supporting his position. It also occupied much of Parry's attention.

Professor Schlesinger ended this wasteful polemic in a fashion that demonstrated his stature. At San Francisco, in September of 1958, Anton was on the podium criticizing Parry's just-presented paper of further experimental proof. In the midst of Anton's detailed (not to say nit-picking) queries, Schlesinger stood and proceeded, with some physical difficulty, to make his way from his seat in the front row of the lecture hall to the podium. Parry and Burg, with the entire audience, watched silently until this distinguished, fragile old man reached the microphone.

His comment, brief, to the point, and made in a surprisingly youthful voice, was: "Anton, it really is not so bad for the two of us to have been wrong nearly thirty years ago. Let us join in congratulating Professor Parry for an excellent piece of work." Saying no more, he returned to his seat and the battle was over.

At a time in this country when reputations often were based on the number of a chemist's publications, where contemporary titans such as Henry Gilman and I. M. Kolthoff

were in serious competition to be first to pass the magic number of one thousand publications, Professor Schlesinger's lifetime list is exceedingly short. The bibliography terminating this memoir includes all of Professor Schlesinger's publications. If, in his opinion, work was not sufficiently important or the results not susceptible to logical understanding, no manuscript was submitted for publication.

Usually, his decision not to publish an account of a given research effort was based upon the state of current knowledge. In the late 1930s one of his students (if memory serves it was Leo Horvitz) studied gas phase reactions of boron trimethyl with carbon monoxide. The results were difficult to represent by balanced equations. There was no suspicion that the experimental results were poorly observed or reported. It obviously was a complex system of reactions that resisted rationalization. After hydroboration was discovered and elegantly exploited by H. C. Brown, it became logical to study the boron trimethyl-carbon monoxide reaction in a solvent. Brown's boralkylation reaction with its powerful synthesis applications resulted from further study of the same reaction in an improved climate of knowledge.

Such examples of imperfect knowledge resided in "the Boss's" memory as poorly-digested meals. He often would describe his intellectual discomfort to later students, arousing serious interest on some occasions and extending knowledge as published work.

His insistence upon a lucid, if terse, style of exposition was impressed upon all of his students, usually during seemingly interminable sessions devoted to the rewriting of reports and theses. Each sentence was held up, not for the pleasure of criticism, but for recasting in a form requiring fewer, or more accurate, words. Many of his students' doctoral theses were so shortened that binding for deposit in the university library necessitated padding with blank fillers.

At the time of my own thesis preparation this process was a source of some growing pains. The end result was a manuscript consisting of twenty-three pages. It was a humbling experience, not intended by "the Boss," that four years of grinding effort produced so little of real substance. The memory of this experience has since evolved into a source of personal pleasure, partially as a result of the fact that the thesis was submitted, as written, and published essentially without change. This set a standard that I have found impossible to maintain throughout my own career.

Professor Schlesinger's standards of publication were made clear to me at the time I was serving as his research associate. At Chicago this position was informally known as a "Ph.D. pusher." He continued my education during this period to prepare me for my chosen career. He gradually increased my role in the day-to-day aspects of graduate education and research management.

An application for a postdoctoral research opportunity arrived from a student being trained at a prestigious university. It was an imposing application throughout, but the most impressive feature was a listing of some twenty publications that this young man had to his credit. After listening to my enthusiastic assessment of the candidate, Professor Schlesinger's terse comment, "It is unlikely that a scientist can produce twenty *good* publications while completing a thesis," made no mystery of his own judgment.

His judgment of the quality of a scientist's work always had more serious importance to Professor Schlesinger than the volume of production. Two of his junior colleagues at Chicago, during the period of his greatest influence, were promoted through the ranks of associate professor and professor without benefit of any formal publications. His wisdom in both of these instances was manifest by the illustrious careers enjoyed by both of these men. Such action is

unheard of in the present academic world where university administrators apparently can count but cannot read.

Professor Schlesinger spent his summers with his family at Elk Lake throughout the period of my doctoral and postdoctoral education. Through every such summer neatly written discussions of the work at the lab, raising cogent questions and suggesting solutions to current problems, arrived periodically, postmarked Rapid City, Michigan. It is still possible to evoke powerful images of "the Boss" by rereading these.

During the last summer I spent in his laboratory his missives became more chatty and I was startled to learn that he was planting 25,000 northern pine seedlings on his summer property. A less likely activity for him could not have occurred to me at that time. Now, it is easy to conjure a salubrious image of the mature forest that has resulted from this mundane activity.

It also gives me great personal pleasure to contemplate the mature forest of ideas and practical applications that have grown from the seedlings he planted in the loam of his career. His great joy in producing something of lasting value was intrinsic to both of these widely disparate activities.

It is difficult to overstate the impact of his discoveries upon the course of modern chemistry. A large majority of published work involving organic synthesis, whether it be natural product synthesis, medicinal chemistry, bio-organic chemistry, or molecular biology, cites the use of reagents that have come from his discoveries. Borohydrides, aluminohydrides, and derivatives of these are so commonplace as reagents that querying the present users would reveal few who can name their discoverer. As previously mentioned, at least two Nobel prizes were awarded for subsequent work closely related to Professor Schlesinger's. It

would be even more informative to count the Nobel awards where the honored work was enabled by the use of these remarkable reagents.

Professor Schlesinger's beloved wife, Teddy, died of a heart attack in 1957. One evening, in their apartment at 58th and Blackstone Avenue, she felt the onset of the attack. Unable to find her medicine, "the Boss" hurried to Sarnat's Drugstore a block away at Blackstone and 57th Street. She had died before he returned. Recounting this deeply personal experience to his many colleagues must have helped him come to terms with his loss.

The few years after Mrs. Schesinger's death were spent with little diminution in "the Boss's" activities. The day before he died, he worked at the laboratory, conducted a meeting of the cooperative apartment members, and indulged a lifetime interest by watching professional football on television. His death the next day, October 3rd, from pneumonia, was possibly a consequence of the absence of his wife's loving care.

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