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THE WAY IT WAS

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Introduction

At one of Bill Libby's bacchanalian birthday parties in Chicago in the early 1950s, Bill made a solemn pronouncement, which he often did out of the blue sky: "Of all people who have ever lived, 15% of them are living now." The statement was received solemnly by the assembled graduate students and young postdoctorals and elicited very little comment. Sometimes these pronouncements of Bill's formed the theme of discussion for the rest of the party, with a great deal of conversation and argument as to whether the statement was reasonable or nonsense. This one did not, as I remember. It was only months or even years later after I happened to run into some discussion by an historian of census taking in antiquity that I remembered Bill's statement. I tried to see if I could fit some simple algebraic curve to the data given in the article and to a few other figures that I found or remembered. I succeeded; I do not remember the formula; but not to my surprise, the answer was close to Bill's. I concluded that a maximum of about 15% of all humans were still alive, and almost certainly as many as 7%.

Obviously, whatever date one selects as a starting date for such a calculation, one selects a generation that has ancestors for millions of years back to the first lungfish that climbed out of the primordial ocean. One has to start with some arbitrary date. I felt that one might start with ten thousand years ago, which presumably predated any of the large city states in China and also in the Near East and Egypt. If one asks what fraction of *literate* humans who have ever lived are now alive, the number must be greatly increased.

The same kind of difficulty arises when one tries to set numbers for the fraction of scientists living at any year. What does one have to do to be called a scientist? I decided that anyone who spent on science more than

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10% of his waking, thinking time for a period of more than a year would be called a scientist, at least for that year. I suppose most young scientists of the present era believe they spend more, very much more indeed, than 10% of their time thinking about science. But I wonder if they do not find that most of that time is spent filling out forms and applying for more research money. In any case, one has to be able to include a man whose paid occupation was director of the British mint, and also one employed by the Swiss patent office.

If one asks what fraction of published science has appeared since I began proudly calling myself "scientist" after my 1927 PhD, the answer is almost all of it! A walk through the stacks of any research library shows much more than ten-fold linear feet of shelf space devoted to "archive" science journals per year now than then. An increase of more than one order of magntidue in any comparison of world science in 54 years from 1927 to 1981 is a completely quantitive change of the characteristics of the milieu.

Childhood

My father was one of 18 siblings, 16 of whom grew to maturity, born to his parents in the small town of Schruns in the Montafon Valley of Vorahlberg, the most westerly province of Austria. As far as I know he was the only one whose education went to collegiate level. He attended the "Technische Hochschule" in Innsbruck in the Tirol. After graduation he went to Paris to the Sorbonne, where he received the degree of "Docent" of applied mathematics, after which he emigrated to the United States and was employed there as a civil engineer by the Union Bridge Company.

My mother, Catherine Proescia, an American-born New York City school teacher, was twenty years my father's junior in age. When I was less than five we moved to Montreal. My father became assistant chief engineer employed by the Canadian government to be responsible for the design of the new Quebec Bridge to replace the first-designed structure which had collapsed.

I learned and understood that if you made a bridge just like the one that stood up, with a given span between piers, and then doubled the span as well as the linear dimensions of every steel member in the design, the strength of the units would be four-fold greater but the weight eight-fold more. Any five-year-old who could count could understand that, if he were sufficiently interested to pay attention to a clearly illustrated lecture by a patient teacher.

My fascination with mechanical construction was greatly stimulated by a fabulous toy brought to me by a young German engineer from Karlsruh: this was a big "Mechano" set. My father was dissatisfied with the mathematical training of American civil engineers. The young German, Hans Grether, became a constant visitor to our household in Montreal until early August 1914, when he avoided the British blockade by shipping as a crewman on a Greek freighter bound for Hamburg, where he jumped ship and assumed his German military duties.

I think my father always considered himself a scientist, an avid reader of Darwin, Huxley, Herbert Spencer, as well as of Freud and Oswald Veblen. Civil engineering was a practical source of income. His family in Schruns were practicing Roman Catholics, but Dad disliked catholicism. He and my mother regularly attended the Unitarian church on Sundays on Sherbrooke Street and I its sunday school. The minister, Frederick Griffin, lived down the street from us. He was a frequent guest at our house, tolerant but disapproving of my father's professed agnosticism.

I have adopted the same agnostic belief, modified by two events. One, through a scolding by a peer when I was a graduate student in Berkeley that the claim of agnostic was a cowardly dodge: Was I not really a convinced atheist? But second, after two round trips of the world, I have attained a theistic realization that all gods worshipped by man have existed or still exist and have been very important in human history. I have seen many! I have seen Shiva's most important organ three meters in circumference and five meters high worshipped by Nandi the bull from without the temple door. I have seen the enormous benign bronze Buddha at Kamakura and the sleeping Buddha in Bangkok. I have seen the great lions of the gods of the Phoenicians shattered by their fall from the lofty pillars of the greatest temple of antiquity dedicated originally to the mighty Baal, but later defeated by Rome who renamed him Jupiter. Beside the fallen lions of Baalbeck is the more modest, but more durable, still-roofed temple to a god at least surreptitiously worshipped throughout the world since the origin of man, whom the Romans called Bacchus.

I suppose that north of cancer 23°N and west of 70°E, for almost two millennia, worship on the Eurasian continent has been pretty much dominated by the various sects that originate from the Judaic tradition—Judaism, Christian, and Moslem—and for the past half millennium, through conquest this is also more or less true of both Americas.

The gods created by man require a priesthood to codify their worship and see to it that temples are erected to demonstrate their power: the might of the doctrine and of the priests of the church. Many are the bloody wars that have been fought about trivia in the theological phraseology of doctrine. It seems as though the teaching of the gentle Jesus has been used as the cause and excuse for many of the cruelest wars of history.

My Introduction to Chemistry

I had a fortunate experience in Hollywood High School, California, in having an excellent chemistry teacher, Mr. Gray, who interested me in the methods of chemistry and with whom I took a second course with a very few (four or five) other students on quantitative analysis. At that time the worldwide price of sugar had recently more than doubled. I spent two campaigns after high school graduation working in two sugar mills, one in Huntington Beach, California, and one in Hooper, Utah. But the price of sugar plummeted at the end of the second campaign and it was clear that chemists were not going to be employed at the bench level. One could guess how to run a sugar mill without paying slaves to titrate the out-put every half hour.

Cal Tech

One of the young high-school educated chemists, Lee Prentice, and I applied to Cal Tech in the midyear class of 1921, the last year that class was given. The name California Institute of Technology had just been adopted by Troop Institute that January with Mullikan's ascension to the presidency of the institution. The midyear class started in January, 1921, and went until the Friday preceding the normal sophomore year beginning on Monday. Without a vacation, we joined the regular sophomore class of the students who had entered the Troop Institute in fall of 1920.

Cal Tech at that time was, as it always has been since, an excellent school. A. A. Noyes was head of the Chemistry Department. He took a great interest in the students although he did not teach any courses. I was amazed to have him greet me by name once in the hall when I was not quite sure who he was. Physical chemistry at that time was pretty much limited to kinetics and thermodynamics.

Richard Tolman came in 1922 as professor of both physical chemistry and mathematical physics. In the fourth year that I was at Cal Tech, Paul Ehrenfest from Leiden, Netherlands, came and spent a semester. Linus Pauling and Paul Emmett came as graduate students in their first year from Oregon Aggie in Corvallis, as I was in my third year as an undergraduate. In my senior year, one of our courses taught by Tolman was based on Summerfeld's *Atomic Structure and Spectral Lines*. The English edition had just come out. The course was listed as graduate, but a few undergraduates took it. Since the undergraduate chemists had not had a thorough course in mechanics, we were pretty much floored. I failed the final examination completely and received a grade of F, which threatened my graduation. Somehow without getting the grade changed they permitted me to graduate.

I had a job as a slavey assistant in Roscoe Dickinson's laboratory, where Linus Pauling was starting his dissertation work on x-ray crystal structure. My first and most serious occupation was to put up a chicken wire grid to protect Linus. Roscoe Dickinson was quite short. The laboratory was in the cellar of Gates Hall and the ceiling was fairly low. The line from the transformer to the x-ray tube went under the ceiling most of the length of the laboratory. Chicken wire had to go under the line to keep Linus's hair from standing on end and shorting the line.

Between Linus's first and second graduate year, Linus purchased Roscoe Dickinson's old Ford—I think it was a 1911 model, in any case it was somewhat antique—and drove up to Oregon where he had done his undergraduate work at Oregon Agricultural. When he came back, he brought his bride, Ava Helen. I was in the lab when Linus walked in upon his return. Roscoe greeted him with obvious relief and the immediate question: How did the Ford behave? "Fine." "It stood up to Corvallis and back without breaking down!?" "Yes." "No troubles at all?" "Well,...we did tip over once on the way back." "My God, weren't either of you hurt?" "Only a few black and blue bruises, but when we got the Ford upright again she started fine."

I had had a very pleasant relationship of my own as a child in Montreal with Mary Bishop. Mary had since gone to Pratt Institute and graduated in fine arts there. Following graduation she had an assignment to interior decorate a new elegant hotel that was being built at the mouth of the Saginaw River where it flows into the Gulf of St. Lawrence, and

Roscoe used Noyes & Sherrill's *Chemical Principles*, which I think was a remarkable textbook. The book was new; the very beat-up edition that I still have was copyrighted in May 1922, but has references to preliminary editions of parts by Arthur A. Noyes alone, copyrighted in 1917 and 1920. The most used phrase in the text that I have is "an important principle is illustrated by the following problem." The student learns nothing unless he does the problems: there are many, and they are mostly hard. I took the course checking, as I now find in my copy, all but a very, very few of the problems. The next year I corrected the problem homework for the following junior class. Since most of the problems were sophisticated, there were several correct approaches as well as an infinity of wrong methods. I learned the material treated in that text very well.

In my senior year I undertook an experimental research problem under the direction of David F. Smith, a National Research Fellow (1). The work was published in the *Journal of the American Chemical Society* in

1924—my first publication! My most lasting lesson from that research was to distrust labels. The title of the paper was "The Free Energy of Aqueous Sulfuric Acid," which we determined by establishing equilibrium at 80°C for the reaction $H_2SO_4+6HI \rightleftharpoons 3I_2+4H_2O+S_{rh}$, approaching equilibrium from both sides in 20 or more days. Our first attempts used "chemically pure HI" from the stockroom: a clear colorless liquid that reacted instantaneously with even dilute sulfuric acid, due to some phosphorous-containing compound producing H_2S but no I_2, probably phosphonium iodide at about 0.3 normal concentration, which seemed a mite high concentration of impurity in a bottle labeled "C. P." We made our own HI after that.

One of the very long-standing mysteries of science was the behavior of relatively dilute solutions of salts in water. The laws of perfect solutions were known, and relatively low molecular weight, nonpolar molecules were known to obey the laws well at the two ends of the composition mol-fraction diagram. Molecules of similar size and shape deviated but little from the perfect solution laws, even in the middle of the diagram. A. A. Noyes had recognized the quite different behavior of the highly ionized salts in water solution and even before the turn of the century had published some interesting papers calling attention to peculiarities in ionic solution behavior. G. N. Lewis in 1911 and 1912 introduced the concept of "ionic strength," μ, the sum over all ions (i) in a solution of one half the molar concentration times the charge z_i squared, $\Sigma_i \frac{1}{2} c_i z_i^2$. Thus, for a one-one salt like NaCl, the ionic strength is equal to the molar concentration but four-fold greater for the same molar concentration of magnesium sulfate. Lewis then observed that the activity coefficient of any solution of all salts with given valence type z_+, z_- depended on the ionic strength alone at reasonably low concentrations, and its deviation from unity at very low concentration was proportional to $\sqrt{\mu}$.

There was no theoretical explanation until in 1923 the Debye-Hückel treatment of the thermodynamics of ionic solutions was published in the Physikalische Zeitschrift. A. A. Noyes immediately recognized its importance and gave a special colloquium lecture on it. As far as I remember it was the only scientific lecture I ever heard from Noyes, which is a shame because he was so clear and precise that I left with the illusion that I understood it perfectly.

Interlude

After graduating from Cal Tech, my mother, father, and I took a trip to Hawaii. At that time the Moana Hotel was the only hotel in Waikiki except for some bungalow cottages, located where the Royal Hawaiian is now. We stayed at the seaside cottages but visited some of the other islands. It was the first time I had seen true tropical luxuriance. When we returned I stayed in Berkeley, and my mother and father returned to Pasadena. It was the last I saw of my father before his death, which was just before Christmas of that year.

My mother was terribly shocked at my father's death, and more so because she had been so anxious to go to Europe with him and to meet his brothers and sisters in Schruns, most of whom were still there. I agreed to go with her and we arranged to leave by train and boat at the end of the spring term.

I had had a very pleasant relationship of my own as a child in Montreal with Mary Bishop. Mary had since gone to Pratt Institute and graduated in fine arts there. Following graduation she had an assignment to interior decorate a new elegant hotel that was being built at the mouth of the Saginaw River where it flows into the Gulf of St. Lawrence, and she wished to spend her remuneration for that work on a trip to Europe. My mother suggested that she come with us. This was a very happy thing for me. Mary's knowledge of European art was of course exactly what I needed. Mary used the method on museums that I have since learned to adopt, namely to rush through the complete museum, glancing at everything, and then to go back and repeat, stopping only at the things found interesting. I fell in with Mary's tastes very quickly. It was a real revelation to me how much more one saw in a museum with a really good guide than going through by one's self. The present arrangement in many museums of renting a talking machine that tells you what you are looking at and where to go next of course did not exist then. The guides that took groups through then were something of a nuisance.

The three of us "did" Europe together, enjoying the usual tourist traps, but the high point was the visit to Schruns. There we stayed with Uncle Wilhelm and his wife Victoria. Uncle Wilhelm had the commodious second floor over the one bakery in town. I hadn't ever seen a really modern electric kitchen until Tanta Victoria's kitchen in Schruns. Of course another Mayer, a brother of my father's, owned the Electricitätes Werke that supplied the electrical railroad from Bludenz to Schruns as well as the city lighting. Another uncle owned the one inn in town. Schruns at that time was not a well-known tourist resort. Since then it has hosted the International Winter Olympics.

Wilhelm Mayer (Myer is the German pronunciation of the name) had purchased the flour mill in St. Poltern, which is a city much closer to Vienna. Since it was summer when we were there with Mary, the family moved to their summer cottage, away from the big city of Schruns, into the higher mountain valley of Gargellen. We traveled by horse and buggy four hours, from Schruns to Gargellen, a difference of about two thousand meters in altitude. We spent a few days in Gargellen where the cows were for their summer pasture and the cheese was made. We went home about Christmas time so that I would be in Berkeley to begin the spring semester, which began in January.

The last time I visited Schruns with my present wife, Peg, only a few years ago. There were practically no Mayers; the uncles and aunts of course were all dead.

Berkeley and Gilbert Lewis

My textbooks as an undergraduate often contained an introduction quoting Roger Bacon's thirteenth century admonition that no theory was valid if it contradicted observation, that is, experimental fact. Another caution known as Occam's Razor is an added lemma that was then seldom discussed, but states that no unnecessary verbiage should be added to the bare bones of the theory necessary to account for all pertinent facts. These two principles have dominated all good science for several centuries. In my student days there were rare but occasional polemics in the literature concerning the validity of a particular theory versus an apparently contradictory experiment. It was a hardy theoretician who dared enter such a polemic unless he knew the theory had been misapplied or that the experiment had an obvious error. James Franck often remarked that nothing looked more like an important new discovery than a poorly conducted experiment. The other explanation, that the experimentalist misused the theory, also occurs. In the early 1930s I had an evening's discussion with a Johns Hopkins faculty colleague who insisted that themodynamics did not apply to organic chemical reactions! He had a Harvard chemical PhD too!

In the five decades since my student days there are still disagreements between scientists, a negligible fraction of which concern pure science, but more the wisdom of social action in a technical matter. The few serious disagreements in science itself practically never involve identifiable conflict between pure theory and pure experiment. Experimental procedures have become so involved that when conflicts in interpretation occur, it is usually unclear which party is invoking theory and which simple demonstrable experimental fact. Both parties usually use a mixture of both.

The seven-century-old heretical philosophies of Roger Bacon and William of Occam are now so deeply ingrained in the thinking of all scientists, and even, although far less thoroughly, in the consciousness of all scholars, that their principles are seldom now discussed. The methods of science are now, as they always have been, the methods used by the individual practitioners of the disciplines. Since all human individuals are unique, and no two are identical, there is a wide diversity of approach. Nevertheless, there are some characteristics of the ratiocination of scientists, particularly in the physical sciences, that differ from those most commonly met in scholars of other fields.

One of these is both the cause and the effect of the diversity of fields of research. Science seldom, perhaps one should say never, discusses a vague question and never answers one. The vague question is broken down piecemeal into small well-defined parts, and answers to each is painfully sought. The essential feature is that the individual, smaller question is clearly formulated and defined.

One of my early experiences as a graduate student taught me this from a great master. After graduating with a B.S. in chemistry from Cal Tech, I applied to the University of California for a teaching fellowship, which was awarded to me. The next four years I spent in Berkeley. Gilbert Newton Lewis, known as the "Chief" or just as "G. N.," was head of the School of Chemistry. He soon became and remains to this day one of my greatest idols. He was a great and tolerant person, a very important scientist, and a fabulous teacher. The department was a happy one with a very strong leaning towards physical chemistry, which in those days was almost exclusively the application of thermodynamics. Even the organic chemistry faculty knew and used thermodynamics, which was unusual at that time. There were no regularly scheduled graduate courses. Most graduate students from other colleges were advised to take the senior thermo-course. We were advised to take graduate courses in mathematics and physics, and some took courses in biology.

I can imagine no milieu more beneficial to the development of a graduate student than that department at that time. The atmosphere was that of unravelling the intricacies of nature in one of its important aspects. Pure knowledge of an assortment of unconnected facts was seldom emphasized, but a deep understanding of principles and originality in interpretation were most admired. I was never aware of jealousy or friction between faculty members and in four years I grew to know most of them very well. All of them seemed to admire and love G. N. That the atmosphere was good for students has been evidenced by the relatively large fraction of them elected to the National Academy of Sciences some ten to fifteen or more years after their doctorates.

One of the nearest to a required graduate course at that time in the Berkeley Chemistry Department was the Monday evening seminar which all graduate students were expected to attend, and which I think most of us did gladly. Since travel from the East Coast, or even from the Midwest, was then a five-day train ride, we had few visiting scientists, and the notables who did come generally stayed a semester or more and gave a full series of lectures on a special topic. There were seldom more than one on hand. The Monday seminar was rarely attended by any but chemistry faculty, one or two post-docs, and graduate students. During my stay the routine did not vary. A faculty member or a graduate student presented a summary of a paper in the literature that some faculty member had suggested would be interesting. The faculty sat at a long table, students along the wall. The assigned time was twenty minutes. I do not remember slides ever being used, and certainly there was no vue-graph, but blackboard and eraser were in constant use.

After the presentation there was discussion, often for longer than the time taken by the speaker and sometimes involving heated argumentation. When the discussion began to lag G. N. would look around the room, select one of the attendees, student or faculty, and address him: "Mr. John Doe, tell us about your research," or some other equivalent request. If addressed to a beginning student in his first year, the nuance of the request was to describe the question to be answered, the importance of the problem, and the projected experimental approach. Completed research was delivered elsewhere, in special announced lectures by faculty, or in the public PhD examination of students.

I think that the emphasis, which we soon began to recognize, on problems of general importance, rather than on adding only one more example to many similar worked out and well understood cases, was probably the prime characteristic that we, as students, took away from our experience at Berkeley as our most important legacy. Of course, then as now, the research problem was almost always suggested by a faculty member, but the student was expected to be able to critically and dispassionately evaluate its significance and importance to scientific understanding. The answer, occasionally given by students now, "It is part of Professor X's project," by itself would have been regarded as unsatisfactory without an understanding of its place in the project and the project's place in science.

The answer to G. N.'s request seldom exceeded a quarter hour, and the faculty discussion followed another quarter hour with many helpful suggestions of procedure or experimental technique. I think that most of us enjoyed being called on. As I remember, usually two or three students were called upon each week, so that each of us reported several times on the progress of our incomplete doctoral dissertations. The seminar was open-ended in time and often lasted two hours, but the diversity of subject matter kept it from becoming tiresome.

One experience in that seminar I shall always remember. I was elected to give the twenty minute report on a paper, I think in the *Zeitschrift für*

Physik, on a subject I have now forgotten but which involved high vacuum technique as did my doctoral research. Towards the end of my carefully prepared and, I hoped, very clear presentation, I became quite aware of incipient troubles. Unusually dense clouds of aromatic smoke were being emitted from G. N. and his ever present "Fighting Bob" nickel cigar to which he had become addicted in the Philippines. Worse still, the cigar was being shortened from the chewed end rapidly and G. N.'s scowl looked ferocious. I finished and awaited the explosion. Smoke and scowl continued for some time. Finally the cigar came out and after that a question, long and involved and obviously concerned with the scientific conclusion of the paper. I found the question incomprehensible and after some hesitation said, "I'm sorry Professor Lewis, but I don't understand the question." More smoke, more scowl, and finally, "Damn it Mayer, of course you don't. If I understood it I'd probably know the answer." Laughter in the room.

I have often thought that that episode illustrates a rather frequent stage in the development of science. The most important part of any real advance is to formulate clearly the correct question.

It was typical of G. N.'s eclectic interest in all of science that he became interested in relativity, which is as far from physical chemistry as one can imagine in any physical theory. I remember a fascinating lecture on special relativity that he gave, at which I saw for the first time the now oft repeated time-space diagram.

I don't remember exactly when I actually began work on what became my dissertation. It was a fairly difficult experimental stunt, and I think that actually had we started it after we really understood quantum mechanics, we would have thought it not worth doing. The dissertation was finally published under the title, "The Disproof of the Radiation Theory of Unimolecular Reactions" (2). If chemical reagents require activation energy to react, this activation energy must either come from radiation or from collisions between molecules. If it is due to collisions between molecules, the rate should be proportional to at least the square of the pressure and the reaction would not be unimolecular, but bimolecular. This had been observed, of course, and Jean Perrin postulated that all unimolecular reactions were due to the absorption of radiation. I set up a very high temperature radiation field such that, if activation were by radiation, a beam of molecules passing through the field would react. The reaction we studied was the racemization of pinene which, from its known rate measured at room temperature and extrapolated to our field temperature, should show racemization on passing through a few centimeters if it were indeed activated by radiation. The radiation bath was simply a quartz cylinder about three centimeters long, through which the pinene coming through two holes in platinum foil was caught in liquid air at the other end and later transferred to a capillary tube and the optical rotation measured. The effect was nil. Of course this is no mystery now and I think it was recognized at that time or very soon afterwards that at sufficiently low pressure the racemization might indeed go with the square of the pressure. However, the negative result of the measurement was accepted for publication and I was granted a doctorate degree.

I do not know how it is now, but at that time teaching fellows in the chemistry department had the privilege of belonging to the faculty club. I ate there at lunchtime rather regularly, usually sitting at a table with other chemists, although quite often when the chemistry table was full I would sit with faculty members of other departments. The chemists, along with one mathematician, frequently played cards for a half hour after lunch.

I became pretty good friends with Wendell Latimer. Latimer at that time had lost his first wife and was, I guess, in a really unhappy period of his life. However, he met his second wife, Latha, and they were happily married before I got my degree. Before that time we often went to San Francisco. Several of the graduate students, often with Wendell, had dinner at a favorite second floor Italian restaurant, "Mimi's." We were always well fortified with drinks, particularly with good Italian red wine. Mimi used to regale us with stories of the opera singers he knew. He had been an impresario in Vienna and in Rome. We always took Mimi's tales with a grain of salt, but one day the New York Metropolitan Opera was playing in San Francisco. The two stars showed up after the opera at Mimi's while we were there having dinner. The two stars, if my memory is correct, were Martinelli and Galicurci. Galicurci embraced Mimi effusively as her old teacher. We were greatly impressed and we really believed all of Mimi's stories after that. The two opera singers had a postman's holiday and we enjoyed it thoroughly. They sang loud and lustily for several hours after a tiring opera.

Hildebrand, of course, had the freshman chemistry course at that time. I was employed as a teaching fellow; my memory is that every entering student was a teaching fellow. I was impressed by the system. The younger faculty members were in charge of laboratory sections. I think all laboratory sections were about 25 students. At that time, experienced teaching fellows were assigned with the beginning teaching fellows for their first year in a laboratory section which was officially in charge of a faculty member. The faculty member would not necessarily stay throughout the whole afternoon after the first few weeks, if he knew he could trust the teaching fellow.

There was a careful way of correcting for differences in the grading of papers so that the students were not penalized by getting a very strict teaching fellow doing the grading. At the end of the semester there was of course a big final examination, and the grading was done by the teaching fellows all at once in a big room, usually one teaching fellow taking one question. In following years, I did not have a faculty member in the laboratory section but had it all to myself.

As I mentioned above, there were no graduate courses in chemistry listed in the catalog and none given, except that most of the beginning graduate students were advised to take the undergraduate thermodynamics course, which was based on Lewis & Randall. I was excused from that since it was assumed that Cal Tech would have given me a thorough basis in thermodynamics. My final examination was during the 1927 summer session and was open to the public. At that time there was a certain amount of pressure on the high school teachers to know the subject matter that they were teaching rather than only to have had courses in education. There were quite a number of them attending my examination. I had a pleasant introduction to my committee by having lunch with them at the faculty club and playing cards afterwards until it was time to go across the street to Gilman Hall for the examination itself.

Gilbert Lewis at that time was known to have usually one tricky question and he certainly had one for me: Take two independent tungsten filaments, in high vacuum, one of them at a temperature approximately 2000 degrees or below, and the other one really white hot, and both filaments are on separate lines by which the current and voltage across can be controlled. A very small pressure of chlorine is allowed into the apparatus, something like 10^{-4} or 10^{-5} mm pressure. The observation is that the higher temperature filament's resistance decreases: it gets thicker, whereas that at a lower temperature gets thinner. After a few promptings to consider possible chemical reactions, I saw the light: The answer was that at the lower temperature the chlorine attacks tungsten, forming a volatile tungsten chloride, but at the very high temperature this compound dissociates, depositing tungsten on the hotter filament itself. I was very proud that I got the answer correctly.

After the examination, Lewis asked me if I would stay as a postdoc, that he would like to discuss a few problems with me. I do not remember what I was paid but I did get a little more than the teaching fellowship and I felt very flattered by the invitation. I actually stayed until the autumn of 1929 when I went on a National Research fellowship to Göttingen to work with James Franck.

With Gilbert Lewis I had a very stimulating two-year experience. I had no knowledge of statistical mechanics and Lewis had never worked in the field either. He had become interested in the discovery that had just been made of the difference between quantum mechanical statistical mechanics and the classical, and the Bose-Einstein versus Fermi-Dirac systems.

During the day I tried to learn statistical mechanics using Tolman's two books, the first of which I found clear and interesting, but the second seemed to me to be too talkative. I also tried Fowler & Guggenheim, which I did not really like, probably mostly because I was unacquainted with the mathematical methods used.

Gilbert and I spent the evenings together, usually at about eight o'clock, sometimes until about midnight. That was really an experience. It was most interesting to see how Gilbert Lewis thought. He was not infinitely brilliant, but he would go over and over a problem until he really understood. Eventually he decided that we ought to publish and the result was three papers that appeared in the *Proceedings* of the National Academy (3).

I still like the method that we evolved for deriving thermodynamics from statistical mechanics, that is, from the mechanical laws for the motion of molecules. Of course the black body radiation problem had been solved but hardly explained. I remember one remark of Gilbert Lewis at that time. He said that the first papers on black body radiation by Planck were clear and concise. The later papers got fuzzier. Actually, we were quite conscious of the fact that the quantum mechanical interpretation was so strange that it was scarcely believable.

The Michaelson-Morley experiment uses half-silvered mirrors which split a beam of photons into two paths which then are reflected back by two mirrors at right angles to each other, such that there are two beams at right angles going through the half-silvered mirror at 45 degrees, which come back and then half of both beams are coalesced again and interfere. There is a beam of light, half of it moving in one direction, half of it moving at 90 degrees in another direction; both beams are completely reflected approximately the same distance and one-half of each of the beams goes to a receiver after passing through the half-silvered mirrors. If one of the two totally reflecting mirrors is blacked out, the pattern is a simple one: one quarter of the beams arrive at the receiving station. If the other mirror is blacked out, the pattern looks almost identical to the naked eye, but the peaks in intensity are actually shifted. Of course when both of the totally reflecting mirrors operate, the two beams interfere with each other and give the interference pattern. There is no mystery with intense classical beams of electromagnetic waves. The mystery, of course, is that if you picture beams consisting of single photons which always have the energy $h\nu$, how do the photons know what the other photon does at a large distance, that is, whether or not it is reflected back? This, of course, is the essence of the difficulty that Niels Bohr and Einstein argued over years later and is responsible for Einstein's statement, "Rafiniert ist der lieber Gott aber Boshaft ist er nicht." (God is sophisticated but not mean.)

In 1929 I was the proud possessor of a National Research Fellowship paid by the Rockefeller Institute for postdoctoral work in Europe, and I had arranged to be able to study in Göttingen and work with James Franck. Franck had been in Berkeley. I gave him a letter of introduction from Hans Grether. After World War I, Hans Grether had gone to Peru and worked on the railroad that was to run over the Andes into the Amazon Valley. His brother had a ranch in Somis and also one in Salinas, California. The brother, Karl, had come to California from Germany while we were still in Montreal before World War I, and Hans was still in Montreal. Karl stayed with us there in Montreal for a few days. We made contact with Karl in California and I visited the ranch several times. Hans came up once from Peru to visit his brother, and when I told him that I was hoping to go to Germany to work with Franck he gave me a letter of introduction. He had worked with Franck during the war, actually, I think, on poison gas.

Göttingen at that time was known as the source of quantum mechanical knowledge. Heisenberg, working with Max Born, simultaneously developed, at the time that Schrödinger developed wave mechanics, an equally logical system of quantum mechanics. As you may remember, for quite a while there was some difficulty concerning this, as Schrödinger's wave mechanical approach seemed to do exactly the same things as the matrix approach of Heisenberg and Born. It was Schrödinger who finally reconciled the two systems as being really different mathematical formulations of the same theory. Max Born at the time was in Berkeley, and I heard later from Maria that he was rather annoyed that he was scooped by Schrödinger simply because he had too much to do in Berkeley.

Thorfin Hogness, who was a member of the Berkeley faculty, had been in Göttingen for a year previously. He told me that most Americans in Göttingen stayed at a pension on Nicholausberger Weg, the Kreuznacker House, but if I could possibly get a room in a private dwelling, it might be more pleasant than a pension. He mentioned that his pediatrician, Professor Göppert, who was a professor of pediatrics, had died and he understood that Frau Göppert might have a room.

When I went to see James Franck I asked him where I should stay and he gave me almost the identical advice—namely, that there was a pension on Nicholausberger Weg, the Kreuznacker House, and it was very satisfactory and most of the Americans stayed there. I'd probably be happier if I could possibly get a room somewhere alone, and he suggested that Frau Professor Göppert had rented a room to an American (it turned out that the American was Robert Mulliken) a year ago and I might try there.

The maid who answered the door said the Frau Professor Göppert was ill and wouldn't see anybody. Actually she had a nasty cold. However, the maid told me that she would ask the daughter to come down and speak to me. Well, the daughter came, smiled benignly at my frantic German, and then answered in beautiful Cambridge English, that her mother was sick, that it was just a cold, but she did not want to see anybody, that I should come back in a day or two, which I did. I was staying at a hotel close to the railroad station. I was much impressed with the daughter and particularly by her perfect English, which I later found she had acquired in one semester at Cambridge on a student fellowship from Germany, in Rutherford's laboratory. She had lived in Girton College while she was in Cambridge, which was the only girl's student house at that time.

Well, I was feeling relatively wealthy and I purchased on Opel. The Opel was not a General Motors car at that time; the firm was later purchased by General Motors. The Opel was a wonderful car. It was put together with picture-hanging wire and sealing wax. I think the existence of the Opel changed my future life. It was a beautiful machine and I had the only automobile of any of the students or of any of the young faculty.

Maria was the belle of Göttingen, as I soon found out. She and the two daughters of Marianna and Herr Professor Landau, along with Titi Stein, seemed to make up the acceptable female contingent of every student party.

On Wednesday afternoons the students of Göttingen frequented Maria Springs—nothing to do with my Maria. Maria Springs was a natural amphitheater in the woods about 10–12 miles north of Göttingen on the main road and on the railroad. The German band played there and there was dancing on the floored area at the bottom of the amphitheater. The Corps, that is the fraternities, all came in decorated coach-and-fours and made quite a show of their arrival. The girls of Göttingen were welcomed in the early afternoon. The last train from Maria Springs to Göttingen left at 6 o'clock in the evening, which in summer time, of course, was bright daylight still. Proper girls had to go home by the 6 o'clock train. Even with my car I could not get Maria to stay longer than the time the train left.

The German school system, like the American, is a state affair, that is, it is different in the different provinces, but essentially similar, just as the American states have very similar schooling arrangements. Until the students are 10 years old, schooling is uniform, simply a neighborhood school; after that there is a separation. The Volkschule does not prepare students to go into a university, and in general students who start in at the Volkschule never can get into a university. Now I say "in general" because I know at least one exception, and that was Hans Jensen, who was the son of a poor gardener and could not afford to go to an Oberrealschule or Gymnasium, the two schools that do prepare for the university. His instructor at the Volkschule recognized his ability and managed with great difficulty to obtain permission for him to take the abitur, which he passed perfectly. The Oberrealschule, the purely classical school preparation for the university, had both Latin and Greek as required subjects. The Gymnasium permitted one to take an abitur also but was more technical. I think both required calculus. The abitur was very much feared. It was generally regarded as disastrous to fail it and one could not get into a university without passing it. The American department idea with several professors in the university was nonexistent. On the contrary, each full professor had his own institute and apparently operated completely independently of any other full professor in the same field. For instance Göttingen had three physics institutes. The Erstes, the first institute, was under Herr Professor Pohl and was primarily solid state work. The Zweites was James Franck's institute to which I was assigned. The Drittes institute was that of Max Born and was purely theoretical. The only common feature was a weekly seminar that all members of all three institutes attended. The seminar had other attendees, for instance both Hilbert and Courant quite frequently came; less often the Professor of aerodynamics, Prandtl, came.

I know very little about the chemistry departments. The physical chemist was Professor Gustav Tamman, who was regarded as a fierce man to have on an examination. One student, who had Tamman on his PhD examination, came out trembling. Tamman had asked him: "Na Kerl was ist lambda?" "Lambda ist Wellenlänge." "Falsch! Lambda ist specifische Conductivität!" Many physics students had Tamman on their examination, but I don't know of any student who was ever flunked because of Tamman's questions, although a good many students were not passed on their first examination.

Only a few weeks after I got to Göttingen there was a meeting of the German Physical Society in one of the Hartz mountain towns. There I met quite a few German physicists whose names I had heard, but had not met before. Among them was Polanyi, the Hungarian physicist philosopher. His son is now professor at the University of Toronto and was a long time in Ottawa.

In Göttingen, Professor Franck came around on a certain afternoon each week with his entourage of assistants and visited everybody working in the laboratory. The group would always include Herta Sponer, who was Franck's assistant, and later after Franck's wife died he married Herta. Otto Oldenberg was the oldest of the people who came on these tours. He was Auserordentlich professor, which corresponds pretty much to an associate professor in America and implies tenure. Otto Oldenberg later emigrated and was in Harvard.

One of the important holidays in northern Germany is Pfingsten, which I think is the same as Whitsuntide in England. Dick Badger, who was in Göttingen from Cal Tech, and I took our first Pfingsten vacation together walking from Lyon to Marseille down the Rhone Valley. It was a long walk, rather too long, walking is slow; however, we did see the French countryside and enjoyed the Rhone wines enormously on the way. One afternoon a car stopped and the two Canadian occupants asked us if we wanted a ride. We did. Later in the afternoon they stopped at an inn and decided they were going to stay there for the night. Dick Badger and I also found it a good idea. The two Canadians asked for a room with bath. Dick Badger and I were satisfied with a room without a bath. At supper the Canadians suggested that we ought to come up and look at their room, that it was worthy of a visit. We did after supper and found the room gorgeously furnished—an enormous room with a platform in the middle of it and a big bathtub on the platform! Dick Badger and I parted in Marseille and both of us went back to Göttingen independently.

In Göttingen I found several letters from Johns Hopkins University offering me a position as associate. The first one had evidently come just about as I left on the Pfingsten holiday. I responded affirmatively and felt very happy that I had a position assured when I got back to the United States. In the meantime I was getting more and more interested in trying to induce Maria to come back as my wife to the United States. This was not completely trivial; the German immigration quota was filled for several years in advance. However, I found out from the consulate that my wife could get in on a special visa. Well, that worked out. I remember that Maria's favorite aunt, who was not very much older than Maria, the wife of the youngest brother of her father, said to her: "You are fortunate in going to America. My sons will be caught up in the next war." They were. One of them survived but was badly wounded.

Return to USA

Maria and I married, and Maria finished her exam shortly before we left. We traveled on the Nord Deutscher Loyd ship, Europa, on its maiden voyage. Arriving in New York on April Fools day, we were met by my cousin and his wife. We stayed with them a few days and then went on to

Baltimore where we found accommodations in a pension. However, the summer vacation broke out pretty soon and we found that there was going to be a special summer session for graduate work at Ann Arbor, Michigan. Enrico Fermi and Paul Ehrenfest were to be the two lecturers. We both knew Ehrenfest quite well. He had been a regular visitor in Göttingen and on his invitation we had once driven to Leiden and stayed with him and his wife. He locked Maria in his study and scolded her that she was not working on her dissertation. He let her out only after she produced *n* pages, and I forget the number *n*. In the meantime I was assigned the guest room; the guest room was on the third floor of the house or maybe it was the fourth. It was a whitewashed room lined with bookcases which contained paperback detective stories. The detective stories were in all languages. There were many in English, some in Russian, and of course various ones in Dutch and German. And there were signatures on the whitewashed walls of guests who had stayed there, signatures with dates attached. I signed under that of the last guest, Albert Einstein!

That particular Ann Arbor summer session was enormously successful. Both Enrico Fermi and Paul Ehrenfest were extremely good lecturers. Each sat in the front row when the other was lecturing and corrected the other's English, much to the amusement of the audience. But both were extremely clear. The audience included Robert Atkinson, an English astronomer and physicist, Lars Onsager, Serge Korff, Donald Andrews, Charles Squire, and of course Sam Goudsmit and George Uhlenbeck, both professors at Ann Arbor at the time.

We became particularly good friends of the Fermi's. Laura Fermi was always a delight and Enrico was always interesting and informative. He was a lot of fun too.

After the session was over I undertook to show America to Maria. We drove in our very conservative secondhand Buick, which we labeled "Connie" for conservative, and tried to see as much of the West as we could. We included the Black Hills, the Tetons, the Yellowstone and Glacier Parks, and then went further west to Seattle, stopping at Mt. Rainier, which I climbed. In Seattle, we visited Henry Frank who was at a summer resort on one of the islands in Puget Sound at that time. Actually we dug clams, much to the horror of the natives, who were not brought up in New England. On driving up to the village at the bottom of Mt. Rainier, as far as we could go in a car, we passed Nisqually Glacier, which was then down almost to the road. We chilled the clams on the glacier and enjoyed them.

We drove south from Seattle, stopped at Crater Lake, and then took the Redwood Highway from there to the San Francisco Bay area and

Berkeley. At Berkeley we visited old friends of mine, including Robert Oppenheimer and his wife Kitty. In the last years that I was at Berkeley, Oppenheimer had come back from Germany and gave a lecture on quantum mechanics. I don't think I understood anything of it but I was enormously impressed and felt that I was getting quite a bit from hearing his stories. I was amused recently at reading an article in the Cal Tech magazine by Carl Anderson who had listened to Oppenheimer's lectures on quantum mechanics in the same year at Cal Tech. According to Anderson's story, the class kept getting smaller and smaller until he was the only one left. Oppenheimer then came to him and said, "Please don't leave, I can't go on with nobody in the class. Let me have at least one student to the end of the quarter." At Berkeley there were several of us who went through the whole semester, or whatever the length of time that Oppenheimer was scheduled to lecture, and we enjoyed it, but I think we were pretty well snowed. Oppenheimer was not a good lecturer at that time. His great facility for making things clear came only later. I think actually that this is a common failing of very brilliant young fresh PhD's in not being able to talk down to students that are not as able as they are. Or even if the students are actually very good, they still tend to talk over their heads.

From Berkeley we went down the coast to Pasadena where we visited the Paulings. We camped in the Pauling yard. They had a house very close to Cal Tech, and we imposed ourselves on them by setting up our tent in their yard. It was a delightful visit for us. I was very sad to see in the newspaper recently that Ava Helen died. She was a delightful person.

The most exciting thing that happened on the trip back from Pasadena to Baltimore was simply a series of tire failures which depleted the ready cash that we had. At that time it was almost impossible to cash checks, although I think we had some money in the bank in Baltimore. If I remember correctly, we simply drove through the bridge at Harper's Ferry without paying any toll. When we got to Baltimore late at night, we had no key to our house, having left it with Frank and Kitty Rice, who lived very close to us on the second floor of an apartment building. We went there and after a while managed to awaken the Rices, went up to their apartment, where we were given a few drinks and then, with the key to our house, proceeded to our home. Frank and Kitty Rice were our most intimate friends at that time. Frank was professor at Johns Hopkins and Kitty was a student in the Johns Hopkins Hospital Medical School, where she later got her degree, Doctor of Medicine with a specialty in psychiatry. Frank left Johns Hopkins for Catholic University in Washington in 1938 shortly before I was fired and went to Columbia. The administration at Johns Hopkins was making it uncomfortable for people to stay. I believe there was a real financial difficulty.

In summer of 1931 we went to Göttingen. Maria was employed by Max Born to help write an article on crystal dynamics in the *Handbuch der Physik*. I went with her, or course, but in addition, my first real student at Hopkins, Lindsay Helmholz, came with us.

It was impractical to start any experimental work in the summer, at least trying to do the experimental work Lindsay and I were interested in, so I worked on crystal theory, that is, lattice energy theory. I consulted with Max Born, and Lindsay helped me. We published two papers, one of them under the names of Max Born and Joseph E. Mayer (4) and the second one Joseph E. Mayer and Lindsay Helmholz (5). This was really very much a copy of the methods used by Born and Haber years earlier, but went into much more detail, attempting to get really good semiimpirical values for the lattice energies of the alkali halide series using exponential repulsive potential. The results were extremely good, really, and I think that for many years they were the best theoretical calculations of the energies for any chemical reactions. Of course, the lattice energy, which is the energy difference between the vapor ions and the normal crystal, is very large, much larger than any directly observable chemical transformation of the ions. Actually there is very little difference in energy between the crystalline salts and the salts in water solution where the ions are dissociated. However, the differences in the chemically observable solubilities were actually fairly well given by our theoretical values at that time. Certainly rather better than almost any quantum mechanical calculation of the energy of the chemical reaction then current. Lattice energies are of the order of 100 kcal per mole or more and the results showed fairly good values to the order of 1 kcal. Later on we evolved a different method of getting at lattice energies. The only unknown at that time was always the electron affinity of the halogen, that is the reaction chlorine neutral atom as a perfect gas to chlorine minus, also a perfect gas. The new method was simply to observe the ratio of electrons to ions coming off a hot filament in an atmosphere of very low pressure of chlorine or any other halide. The electrons could be deflected by a relatively small magnetic field parallel to the length of the filament; they curled themselves up and did not go to a positively charged plate cylinder of a centimeter diameter. This was a far simpler and more satisfactory method than the awkward treatment of the salt crystals. It worked quite well on the halides but did not give good results when used to measure the electron affinity of oxygen.

In the meantime, a Wiley representative got Maria and I interested in trying to produce a book on statistical mechanics and it actually materialized in 1940, the first edition of Mayer & Mayer's *Statistical Mechanics* (6). The book was quite successful as books on statistical mechanics go—enough so that Wiley even tried to get us to produce a second edition very much later. It was in doing that that I got interested in attempting to improve the method of deriving equations for the virial coefficients of normal molecules. Philip Ackermann was working on an experimental problem of seeing whether we could use low energy electron beams for molecular structure studies. Ackermann had trouble getting a satisfactory position and he stayed and helped me with the statistical mechanics calculations. The first two papers on the theory of condensing systems (which was an unfortunate choice of titles) were done with Ackermann's help (7, 8).

In general, I had a most interesting and excellent group of students while I was at Hopkins. It was an unfortunate time, much worse than at present, to get positions. Science was not a popular subject and well supported as it still is now, in spite of our complaints.

Maria and I tried to build an electron microscope. It was a joke and we took it as a joke; we never even tried to publish it. The apparatus was essentially made of wood with window screening to produce the electric fields. I give this only as an example of the sort of tomfoolery that we often were forced to use. Of course, essentially we did not have the courage to think that an electron microscope would have a real use in the future. There are other scientists who have occasionally thought ahead of their time in trying to develop experimental methods, but given them up, wisely probably, because everything has a time to be successful. The only thing I object to is that in many cases these people think they have been cheated. We knew what we were doing was foolish, but it was fun.

The sad thing of that time was how extremely difficult it was to get good positions for our students when they graduated. Particularly the women. I was very fortunate in having several excellent women students at Hopkins: there were Sally Harrison, Sally Streeter, Irmgaard Holdner Wintner, who was the wife of a mathematician, a professor at Hopkins, Aurel Wintner. I also had Willard Bleick and Louis Roberts as well as J. J. Mitchel. Louis Roberts came with me to Columbia and actually took his PhD degree at Columbia instead of Hopkins. Willard Bleick was one of the more impressive students.

In 1938 I was informed that I would be discontinued at Hopkins after a year and a half. It was very fortunate for me, although I was happy at Hopkins and I would have stayed there had I not been fired. I wrote to various friends, including Harold Urey at Columbia and James Franck, who had left Hopkins to go to the University of Chicago. At both places I was given an offer to come as associate professor, presumably with tenure or at least tenure after one year; I felt very happy to go to Isaiah Bowman, the president of Hopkins, and resign long before I was obliged to leave. I finally decided after visiting both Columbia and the University of Chicago to go to Columbia. I think it was probably a very good choice at the time; in any case I have followed Harold Urey ever since.

Maria and I went often in summer to Germany. In 1937 she received a telegram that her mother had had a stroke. We managed to get her on the Europa the same evening, which was quite a feat and managed only because we knew the German consul in Baltimore who was also the representative of Nord Deutscher Loyd. Maria was received at disembarkation in Germany with the news that her mother had died. We managed to get a load of furniture out of her house but never succeeded in being paid for the sale of the house.

Of course, the trips to Germany were always sad after Hitler's arrival as Führer.

I learned many things at the time. Americans are too polite and not nearly as direct as the Germans or, particularly, as the Dutch. Several times Germans told me they had a position in the United States and showed me the letters they had received. Those letters did not offer a position at all. They were usually in the tone, "Of course we would love to have you here, all my colleagues would like it, they would all recommend to the administration that we create a position for you, but the financial situation is so bad that it is almost impossible that anything will happen..." and so on.

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