DAVID TURNBULL
Autobiography

Youth

I was born (on February 18, 1915) and reared on a family farm in northwest Illinois. Our community, Elmira, was settled in the 1840's, primarily by immigrants from the border and western highland regions of Scotland. Later there were migrants from the Appalachian hills who were attracted by jobs in the nearby soft coal mines and by the relatively cheap land in the region. Among them was a woman, widowed by a Kentucky feud, who was determined to remove her several sons from the cycle of violence.

Three of my grandparents were, as children, among the Scottish immigrants and the fourth, my maternal grandmother, was an orphan, thought to be of Pennsylvania Dutch descent, who had been adopted into the family of one of my father's great uncles. The Turnbulls were an old border family who, according to my English friends, were notable for trans-border raids on English cattle herds. My father's mother was from a highlands family, the McLennans, whose native tongue was Gaelic. My mother's father, George Murray, presumably descended from the Atholl Murrays, became a prosperous farmer in the community. My grandmother Murray was the only one of my grandparents alive when I was born. As a little boy I much enjoyed the trips in a buggy or sleigh, with my mother, to visit her at the Murray homestead. My only sibling, a brother, Theodore, was four years younger than I. He became a medical doctor.

My father operated our farm in partnership with his brother, Edwin, and sister, Mary Ellen, neither of whom ever married. They lived in the house of their parents with a niece, Mary Irene, fifteen years older than I, whose mother died when she was an infant. An exuberant young woman and skilled equestrienne, Mary Irene was as a sister to my brother and me. My parents lived in a nearby house, adjacent to my aunt's, built just after their marriage. Three of my father's brothers

had farms in the neighborhood, and with their families often visited us on Sunday afternoons. Some of my recollections go back to the time of World War I. I remember the concern of the family for my favorite cousin, Willie, a brother of Mary Irene, who was in the Argonne offensive and my joy when he returned unharmed.

Our farm's extent was 400 acres, only half of which was cultivated. The remainder was quite hilly and wooded, mainly by oak and walnut trees. It was crossed by a spring-fed stream that provided water for our livestock. Our principal crops were corn, oats, barley, soybeans, clover, and alfalfa. We had a steady animal population of about 100 cattle, 80 sheep, 300 pigs, 100 chickens and 30 horses. We farmed entirely with horses until, in my fifteenth year, we obtained a tractor. The quality of our soil was well below the norm for the region, but through hard work and the skill and shrewdness of my father and uncle, we lived quite comfortably until the onset of the great depression. We led quite a cloistered life, rarely venturing more than a few miles from home. We had no radio until I was age sixteen and I never entered a movie theater until my second year in college.

My father had a high school education, supplemented by a short accounting course. As a young man he was quite an outstanding athlete, starring in track and football. He was dynamic and pragmatic and exceptionally skilled as a mechanic as well as in the other agricultural skills. He was somewhat impulsive and would fearlessly herd, quite unprotected, the most powerful and potentially dangerous of the farm animals. He advised me that if I showed it no fear, an animal wouldn't bother me, but I was never able to face a huge shorthorn bull or a neighbor's German shepherd dog as unconcernedly as he did. For many years he served as township supervisor, as well as chairman of the county board of supervisors. Also he was, for a considerable period, president of the county Farm Bureau Federation. He was a strict but devoted parent with a touch of Gaelic sentimentality. I was never in a state as forlorn as that of the prodigal son, but had I been I am certain that my father's response would have been like that of the father in the parable.

After a two-year course at Oberlin College my mother was, until her marriage at age 35, a country school teacher. She was a quiet and gentle woman who ardently supported the feminist causes of her day. She did paintings and played the piano quite well. About the time of my brother's birth, at age 42, she developed an arthritic condition which increased in severity until her death thirteen years later. Despite her illness, she was always cheerful and managed to perform, with some help from my brother and me, the varied and onerous duties of a farm woman at that time. In temperament and intellect I was most like her. In analytical ability I somewhat resembled my father but, to his considerable disappointment, I had none of his athletic skill.

My family belonged to a strict Presbyterian sect of the type described by Barbara Tuchman in her "Bible and Sword". The sect placed great emphasis on the Bible and stressed the teachings of the Old Testament as much, or even more, than those of the New. Only Psalms were sung at the main religious services and, in view of the uncompromising monotheism of the Old Testament, the Trinitarian doctrine, while formally accepted, was treated rather gingerly and with a bit of embarrassment. My father and aunt were active members of the church and I was required to attend Sunday School and listen to the sermons. At home the Bible was read aloud, one chapter each morning, serially from Genesis through Revelations. Though formally a member, my mother did not attend the church and was, I believe, quite skeptical of its dogmas. In guiding my brother and me she stressed right conduct, with no reference to "right" belief, and often cited the ethical precepts expressed in the Sermon on the Mount, the Lukan parables, and the Old Testament wisdom literature. As a little boy I was fascinated by the tales of such Biblical heroes as Gideon and David. While still quite young I thought much about philosophical and religious questions. Admiring my father I wanted to accept his beliefs, but I became skeptical of them quite early. While repelled by the Christian salvation theology, I came in my early teens to admire the exalted ethical ideals of Jesus and for a time tried, I'm afraid with rather little success, to live up to them. The problem of Jesus as a historical figure is one that has always intrigued me.

I began elementary school at age 5 1/2. The school, located in Elmira village (pop. ~100), 1 1/2 miles from our farm, had two classrooms, but when I attended, there was but a single teacher and only one classroom was used. I walked to and from school until I was in grade six; after that I reached it on ponyback. About thirty students were distributed over the eight grades of the school, and only two, besides me, were in my class. Following the usual practice in country schools, lessons were recited to the teacher by each grade in front of her desk, while the students in the other grades were expected to continue their study.

Apparently I had a mild case of dyslexia, for I was virtually unable to read until my third year in school. Because of this disability I couldn't read the arithmetic problems, so my learning in all subjects was impeded. When my difficulty became apparent, my mother proceeded to instruct me each evening at home, working patiently with me for more than a year. My progress was discouragingly slow until one evening, when I was faced with what seemed to be a most formidable lesson, bristling with many strange new words. My mother braced herself for the worst, but much to her and my amazement, I quickly read the lesson through without prompting or error, and then went on to read several lessons in advance. Ever after I read well and voraciously. I quickly caught up with my class in all subjects and also became a flawless speller. This remarkable change in my reading ability occurred, I think, when I grasped the use of context as a guide.

Our community was not a part of any secondary school district, but my parents managed to have me admitted to Kewanee High School which, at that time, was considered to be the most outstanding one, academically, in the area. Kewanee was a manufacturing (steam boilers, tools, and textiles) town with a population of 17,000 located about eight miles from our farm. It could be reached only by way of unimproved dirt roads and no transportation was provided. I drove at first a model "T" and then a model "A" Ford car to the school or, when the roads were impassable, walked. One or two neighbor children, who also attended the school, rode with me. Upon beginning high school my age, 12 1/2 years, was below the state specified minimum, 15 years, for drivers. However, in the rural areas this minimum was not enforced and largely ignored; also there

was, at the time, no driver licensing. Fortunately, I drove to and from school the entire four years without mishap.

The transition from a country to a strange city school, with 600 students, was quite difficult for me. The few farm students were looked down upon and sometimes ridiculed for their country ways by many of the city students. Academically the school entirely came up to my parents' expectations. It was staffed by a group of outstanding and dedicated teachers, mostly women. Undoubtedly students at the time were fortunate that most highly qualified women chose the teaching profession since, as Derek Bok and others have noted, the dictates of the temporal mores virtually excluded them from most of the other learned professions.

I was especially inspired by the Misses Minnie Trask and Wildred Ewan, who posed demanding intellectual challenges and encouraged imaginative but rigorous modes of thought. Minnie Trask, a frail and elderly spinster, taught mathematics with a rather "corny" style, but she had a profound grasp of her subject and assigned difficult problems. On occasion she would pose a geometry problem and predict that no one in the class would be able to solve it. Such challenges were, for me, irresistible and, after strenuous effort, I always solved the problem. To my considerable embarrassment, Miss Trask would display my solution and tell the class that I had done a great thing. To that point, which was in my third high school year, my scholastic performance had been considerably above average, but far from outstanding. After this I excelled in mathematics, physics, and the social sciences. The latter were taught by Wildred Ewan, a young woman who encouraged her students to "think the unthinkable" and to examine all viewpoints, objectively and critically, however unconventional they might appear. It seemed that I learned most from having to solve difficult problems and rather little from classroom exposition, however excellent it was. Because of this, the emphasis of my own teaching has been on posing meaningful and challenging problems. In this I have tried to follow the dictum attributed to Galileo, "You can't teach a person anything, you can only help them to find it within themself."

Often I spent the high school lunch hour at the well-stocked city library and there discovered periodicals such as *Harper's*, the *Atlantic*, the *New Republic*, and the *Nation*, all of which I enjoyed reading, as well as some of the more popular magazines, such as the *Saturday Evening Post*. I well remember the rosy predictions on the economic future in most of these magazines, as well as in the newspapers, shortly before the onset of the great depression. I completed high school in 1932 at the depression's nadir and the death of my mother occurred at the same time. After her death, our aunt Mary Ellen provided, with great devotion, what maternal care my brother and I needed. My father remarried four years following my mother's death.

My parents began assigning me regular farm chores, which I eagerly undertook, when I was age six and by age twelve I was doing, when not in school, the work of a grown man. During the busiest season, from mid-May to mid-September, this meant rising at 4:30 A.M. and working till 7:00 P.M. six days a week, with rather brief intervals for meals. However, I much enjoyed the woods and fields and most of the varied farming activities, especially working with horses and herding cattle. Indeed, I would have preferred to spend my life operating our farm, but an asthmatic condition, which became apparent when I was age two, greatly impaired my ability to perform certain essential tasks. By the time I completed high school, it was clear that I would have to find some career other than farming. In the post-Calvinist milieu of my home, it was taken for granted that any career should be directed toward human betterment, as it was understood. However, I had only vague notions of what the possibilities were. I definitely did not want to become a minister or medical doctor, but thought that I might prepare to teach mathematics and history. To this end, I entered Monmouth (Illinois) College, a small (~400 students) liberal arts institution governed by our religious sect.

College and Graduate Study

The Presidential campaign of 1932 was in progress at the beginning of my first college year. I joined about 25% of the other students in voting for the socialist candidate, Norman Thomas, in a straw poll conducted by the college newspaper. Later I was highly impressed by the policies and leadership of Franklin Roosevelt which, in my judgment, saved the American family farm for at least two more generations. Since that time I have always supported the nominees of the Democratic party for the Presidency. In this, I broke with the political persuasion of my father's family that, since the Civil War, had always been Republican or Progressive.

I found that to qualify as a teacher I had to complete five or six courses in a subject called "Education", so I took two of these during my first college year. Never before nor later did I encounter courses that seemed so trivial and devoid of intellectual content. I resolved, rather than take any more such courses, to give up my plans to become a teacher. Later I learned that at most other colleges the program in "Education" was much the same as at Monmouth.

At the end of my first college year economic conditions were still very grim and it appeared that we would surely lose our farm if the depression continued. I recall, at the beginning of a "bumper" oat harvest, calculating that the 3 cents/bushel return for our crop could hardly pay the taxes on the land on which it was grown. I was giving much agonized thought to values and aims when I came to feel that we are happy to strive for a "good" society, but would be quite bored to live in a society, if ever attained, with no major problems. Suddenly it seemed that life would be pointless if my ideals ever were realized. This outlook, which I later found expressed vividly by Dostoevsky in his "Notes from the Underground", deeply depressed me for a considerable period. However, I gradually realized that my knowledge and perspective were much too limited for any conclusive assessment of values and ends. At that point I decided that I would find greatest satisfaction in endeavoring to advance knowledge and understanding through the practice of science. I sensed that my intuitive intellect was, perhaps, better suited to the social than to the

physical sciences. However, I decided to take up the physical sciences because I thought them more effective for enhancing knowledge. From that time, when I was eighteen, to the present, I have, though often frustrated and discouraged, directed my efforts to the limits of my capabilities, to the advancement of physical science.

My high school experience inclined me toward physics, but Monmouth's physics offering was very limited. At the beginning of my first college year an adviser had suggested that I keep my options open by taking a chemistry course. This I did and found the lectures of the chemistry professor, W.S. Haldeman, highly interesting and inspiring. They showed me that chemistry was much more challenging, intellectually, than I had appreciated during my high school course. Therefore, I decided, at the beginning of my second college year, to concentrate in chemistry and to take all of the mathematics courses offered. Analytical and physical chemistry were taught by another outstanding professor, G.W. Thiessen. Actually, I learned more from him, because of his challenging problem assignments, than from Haldeman and it was his teaching which attracted me to physical chemistry. I excelled in chemistry and Haldeman urged me to go on to Ph.D. studies. He recommended me to several graduate schools and I received and accepted a scholarship offer from the University of Illinois which would cover my tuition and most living expenses.

At the time, Worth H. Rodebush and Thomas E. Phipps were the physical chemistry professors at Illinois, and I was fortunate in having close association with both. My first year at Illinois was tense and very difficult. The entering graduate students in chemistry were informed that the department's research facilities could accommodate only half of them, so the half with the lowest grades and qualifying test scores at the end of the year would have to leave. As a scholarship holder, I was required to serve quarter-time as a teaching assistant. In the first term I was assigned to grade problem sets in a physical chemistry course which I at the same time audited, since it was considerably more advanced than my Monmouth course. During the second term I supervised a section of the physical chemistry laboratory and developed a new experiment for it. I found that my Monmouth training was substantially less advanced than that of first-year students

from Illinois and other research universities. However, well before the end of the year I had caught up with these students and probably had passed most of them. I was able to do this, I believe, because at Monmouth I had been taught the simple basic concepts well. In striving to provide their undergraduates with the most advanced training possible, the research universities have tended, perhaps, to slight the simpler concepts central to the discipline. Often such neglect becomes painfully evident during oral qualifying examinations. Perhaps the small liberal arts colleges can still compete effectively in the training of science undergraduates by concentrating on these simpler concepts. Also, because of their small classes and the greater commitment of their faculty to teaching, these colleges can outdo the large universities in individualized laboratory instruction. Such instruction was, indeed, an invaluable part of my Monmouth education.

Having excelled in courses, including several in organic chemistry and physics, and qualifying tests during my first year, I was awarded a fellowship in each of the following two years. These came with no teaching obligations and provided, more than adequately, for my living expenses. Out of the $1,400 total of scholarship and fellowship money received in my three years at Illinois, I saved $300.

Rodebush was, at first, my adviser. With Wendell Latimer he is credited with having introduced the hydrogen bond concept into chemistry. He was considered, rightly I think, a genius of sorts, but one who did not exert himself unduly. He was curt and sometimes snappish, but behind his rough facade he was fair and quite caring of his students. His lectures were disjointed and cryptic and highly unpopular with most of the students. Nevertheless, they usually dealt with challenging issues and often exhibited profound physical insight. It was as a result of these lectures that I became fascinated by the problem of nucleation. Rodebush pointed out that to persist and grow, a nucleus had to reach a critical size such that it would contain hundreds of molecules. How this size could be reached mystified me. Perhaps I would not have been so mystified had I grasped the full implications of the principle of microscopic reversibility. Anyway, I resolved to

study nucleation if the opportunity arose, and I listed as a problem in my notebook "the kinetics of crystal nucleation in liquid Hg".

For my earliest seminar at Illinois, Rodebush asked me to review the paper, then just published in the *Journal of Chemical Physics* by Henry Eyring and coworkers, associating crystal melting with an abrupt appearance of "communal entropy". Upon studying the paper, it seemed to me that the argument hinged on the assumption, which I thought incorrect, that self-diffusion could not occur in the crystalline state. In reviewing the paper, I emphasized this criticism, though fearful that I might have misunderstood the development. Actually, to my considerable relief, Rodebush approved and liked my critique.

At Rodebush's suggestion I took on what must be almost the ultimate challenge in metastable structure synthesis, that of producing pure ammonium metal. Rodebush thought that such a synthesis might be achieved by electrolysis of ammonium salts dissolved in a non-hydrogen bonded liquid of very low dielectric constant, like dioxane. He was encouraged by the reports of Kraus and coworkers from Brown University that in similar solvents certain salts did exhibit appreciable solubility and electrical conductivity. Phipps undertook to instruct me in the techniques of glass blowing and high vacuum necessary to proceed with the investigation. Our efforts to synthesize pure ammonium came to naught, but I gained valuable experience from them.

Guided by Phipps, I then began an investigation of the periodic, with field, deviations of the thermionic emission currents from the Schottky theory. Phipps had discovered these deviations while checking his techniques preparatory to a planned study of the contact potential of I_2 on W. He and earlier students, Seifert and Clemens, then measured the deviations at intermediate fields. Phipps thought the behavior quite bizarre and hardly believable. My task was to repeat the measurements and extend them to higher fields. During my study I benefited greatly from the tutelage of Ralph Seifert as well as that of Phipps. The attainment of ultra-high vacua was crucial to experimental success, and the emission behavior during my final measurements indicated vacua

better than 10^{-13} torr. My results fully confirmed Seifert's and showed further that the deviations persisted with increasing period and amplitude to still higher fields.

With these results Phipps decided that publication could be risked so two papers were prepared, the first by Seifert and Phipps and the second by myself and Phipps, and submitted to *Physical Review*. Both were accepted, and then published in the autumn of 1939. Later, at Irving Langmuir's suggestion, Wayne Nottingham of MIT repeated our experiments and obtained the same results. The deviations were interpreted as a quantum interference effect by H. Mott-Smith and by Eugene Guth and coworkers.

Learning and working with Phipps was an invaluable experience. I have not known a more selfless and dedicated scientist than he was, nor any who imposed higher standards of care, accuracy and repeatability in experimentation on himself and his students.

Having completed my thesis, I received the Ph.D. degree in June, 1939, and then sought an academic position. Few such positions were open at the time, but following Rodebush's strong recommendation, I was offered and, as he advised, accepted an instructorship in physical chemistry at Case (then known as Case School of Applied Science). The practice of visitation, when applying for a university position, was not then common, and I neither visited Case nor was interviewed by anyone from there before accepting their offer.

Case School of Applied Science

When I began teaching there, Case was just initiating doctoral programs in chemistry and other fields, but its major emphasis was still on undergraduate instruction. I found that I was required to carry a very heavy teaching load, amounting to 24 hours of laboratory and classroom instruction per week. My duties included teaching: all sections of the physical chemistry laboratory, a required course in physical chemistry for the metallurgists and another for the physicists, and enough sections of elementary chemistry to complete the load. There were about 20 to 25 students per

section of the physical chemistry laboratory which meant that 10 to 12 experiments had to be in operation simultaneously. At Monmouth we set up our own experiments and had 6 to 10 hours to complete each one. In contrast, the Case schedule allowed a student only 3 hours per experiment. This meant that every experiment had to be "canned" and I had to do the "canning" and maintenance with very little assistance.

My first afternoon of instruction in the Case physical chemistry laboratory was my worst teaching experience. I found that most of the students, though they had taken laboratory courses in analytical chemistry, were quite inept with little sense of what precautions were essential when using delicate equipment. All the afternoon I rushed from one crisis to another as ammeters smoked and glassware broke. Somehow the class and I survived. In time I learned where the troublespots were and eliminated some by redesign of experiments.

Most of the Case students were quite intelligent and teaching a section of Freshman Chemistry could be very stimulating. The students were lively and I was kept "on my toes" by their, usually, penetrating questions. However, as they advanced to upper classes, most became rather docile and less vibrant intellectually. In many of the engineering courses they were required to write detailed and lengthy reports and to present solutions to the same problems, with trivial variations, many times over. This routine left little time for reflection and exercise of the imagination and so, I believe, stultified intellectual development. Such a mode of education was not unique to Case but was, I think, prevalent in most engineering schools at the time.

My first term at Case was very strenuous, and soon after it began I developed, at certain periods of the day, severe stomach pains. I had experienced similar pains from age thirteen, but they never were serious enough to impede my work or study. However, at Case they became so nauseating that I could hardly teach, so I had to seek diagnosis and treatment. Fortunately, I was directed to a brilliant diagnostician, Harley Willims, who immediately attributed my symptoms to a duodenal ulcer. When his diagnosis was confirmed, I went on a strict diet with medication and the

symptoms quickly disappeared. They recur periodically, but are relieved when I exclude certain items from my diet for a short period. Our elder son, Lowell, developed similar symptoms, with the same diagnosis, also at age thirteen.

Beside carrying a heavy instructional load I managed to do some research at Case. This research included: investigations, in collaboration with Samuel Maron, of the validity of certain scaling relations for the thermodynamic properties of gases at high pressures; some experimental studies, which I still think highly of, in the physical-organic field; studies of the kinetics of oxidation of metals and of the corrosion of certain alloys in non-aqueous media; and studies, in collaboration with Carl Prutton, of the mechanisms of lubrication under high loads. Also I guided the Ph.D. research of two students, Delton Frey and Paul Hartman. The results at Case of my collaborators and me, are described in eighteen papers.

In late 1942 I was offered a position on the Manhattan Project, at the University of Chicago's "Metallurgical" Laboratory. In my middle teens I had been, as was my mother, an absolute pacifist, but my pacifism eroded as it became clear that the German dictator really meant to inflict the horrors promised in his writing and oratory. Thus, I had little moral objection to the project research, which was clearly motivated by a near hysterical fear that Germany would soon perfect the "bomb". However, I was reluctant to sever my academic connection, and so decided to remain at Case where, throughout the war, I was heavily engaged in training the several classes of naval cadets graduated each year and in research on synthetic rubber and on the improvement of additives for heavy-duty lubricants.

Various experiences at Case aroused my interest in solid state science, or "materials" science as we now label it. Since I was teaching the metallurgists physical chemistry, I decided to inform myself about metallurgy, and found it quite fascinating. My investigation of metal oxidation kinetics led me to the papers of Carl Wagner, which highly impressed me. The arcane always appealed to me and, from my chemist's perspective, it seemed remarkable that transport rates in

crystals could, as Wagner's theory indicated, be increased many orders of magnitude by the presence of minute concentrations of point defects. I wondered about the role of extended imperfections in such transport, and resolved to investigate it when I could. My admiration of Wagner was such that I hoped to study with him when the war was over. Actually, he joined the MIT faculty after the war and I was fortunate to have had some professional association with him. I felt greatly honored when, in 1952, he tapped me to succeed him as chairman of the Gordon Conference on the "Physics and Chemistry of Metals"; this conference later fissioned into the conferences: "Condensed Phase Physics" and "Physical Metallurgy".

My interests in solid state science deepened further during my final years at Case. In this period I was especially instructed by the books of Hume-Rothery and Seitz on metal physics. I resolved to conduct my future research in this field, but my Case situation did not seem favorable for this, so I looked for opportunities elsewhere.

When I decided at age eighteen to go into science, I visualized for myself a monkish type of life and so thought that I would never marry. However, as time went on, I came to feel more keenly a void from the absence of family life. In my last year at Case I married Carol Cornell, who has been my loving and steadfast companion ever since. Two sons, Lowell and Murray, were born to us and we adopted a daughter, Joyce, when she was one year old.

One of my metallurgy students was Jack Lubahn who, after his undergraduate study, remained at Case investigating, with George Sachs, the flow and fracture of metals. Early in 1946 he informed me that he was joining a new metallurgy group being formed at the General Electric Research Laboratory, in Schenectady, N.Y., by a Captain J.H. Hollomon. He said that Hollomon expected to hire a physical chemist and asked if I would be interested in the position. Shortly afterward I met with Hollomon at Lubahn's home. Hollomon, then a trim young man aged 26, was a most unusual person with quite an overpowering personality. He was brash, intense, completely self-assured and overflowing with enthusiasm about prospects for the new group. He described the

fascinating, but poorly understood, responses of metals to mechanical and thermal treatments and his plans to form an interdisciplinary team, with representation from metallurgy, applied mechanics, chemistry and physics, to attack the problems posed by this behavior. He was certain that these researches would lead to greatly improved ability to design and synthesize new materials that would find important technological uses and expressed the view that equipment performance was becoming more materials than design limited. He assured me that I would be provided with the freedom and resources for the studies of nucleation and diffusion, which I hoped to make, and also aroused my interest in the martensite transformation and solid state recrystallization. The position was offered, at a salary about 15% less than I was receiving at Case, and I accepted it. My career at Case had been somewhat disappointing, but I learned much from it that would prove useful in my later research.

General Electric Research Laboratory

I joined G.E. in late 1946, becoming the fourth member of Hollomon's group. John Fisher, who was trained as an applied mathematician, joined the group not long after I did. The ambience of G.E.'s research laboratory, then guided by Guy Suits, was pervaded by an aura of excitement and excellence and of collegiality that was quickly extended to newcomers. I was fortunate to have joined it when most of its distinguished elder scientists were still active and to have had interactions with some of them, including Irving Langmuir, Albert Hull and Willis Whitney. At this time Suits spent most of his time at the laboratory and periodically reviewed the research of individual staff members at luncheons, also attended by some of the senior scientists. At these meetings I found Suits' comprehension of the wide variety of topics discussed remarkably acute and his suggestions for my own research to be generally quite helpful. Unfortunately, Suits' direct interactions with his research staff dwindled sharply as the laboratory grew and as the demands on his time by the G.E. executive offices in New York increased.

Most of my associations at G.E. were within Hollomon's group. However, I had many valuable interactions, besides those with the more senior scientists, with members of other groups, especially with Bernard Vonnegut, Vincent Schaefer, Fraser Price and with Leroy Asker and several others in his Semiconductor Studies section.

Hollomon was like no other manager. He was rarely neutral about anything and had very strong likes and dislikes of people and ideas. These were expressed openly and vehemently and often changed dramatically from time to time. Those closely associated with him usually were welcomed to his inner sanctum or consigned to his outer doghouse. Most of us made, I think, several circuits between the sanctum and the doghouse. Hollomon would advocate an idea or model vociferously and stubbornly but, if confronted with contrary evidence of a convincing nature, would quickly and completely reverse his position without the slightest show of embarrassment and then uphold the contrary view with as much vigor as he did his former one. He adhered to no set rules. One time he arranged, all for the same date, that he would go to Cleveland to talk about my research, Fisher would go to Syracuse to talk about Hollomon's research, and I would go to Pittsfield to talk about Fisher's research. At the time none of us thought these arrangements odd.

Hollomon's leadership was, indeed, dynamic and inspiring. He established a climate highly conducive to interdisciplinary cooperation and to daring modes of thought and experimentation. In this ambience we were inclined to choose the most outrageous over the more conventional explanations for newly discovered phenomena. By adhering to this practice we would, on occasion, look rather foolish, but we were also led to ideas and insights which proved to be very important.

Hollomon's group grew rapidly and by the early 1950's its achievements were widely recognized. These achievements and those of several other groups--especially the Shockley group at Bell Labs, Chicago's Institute for the Study of Metals under Cyril Smith's leadership, and Nevill Mott's group at the University of Bristol--demonstrated the power of the interdisciplinary approach

to materials research and led to the recognition and labeling of a "Materials Science" "superdiscipline".

In these and other laboratories there were, at the time, highly effective interactions between physicists, metallurgists, chemists, and applied mechanicians. Metallurgists were remarkedly receptive to the ideas of physical scientists because they felt, perhaps, that they had gone as far as it seemed possible to go with the empirical approach to alloy design and development. In turn, leading physicists were challenged by metallurgical problems and willingly listened to and learned from metallurgists. Interdisciplinary exchanges occurred at various meetings, sponsored mainly by the metallurgical and physical societies. A highly influential forum for such exchanges was provided by the yearly weekend seminars sponsored by that most applications-oriented of metallurgical organizations, The American Society for Metals. These seminars were attended by several hundred metallurgists from industry and the universities. Among the participants were such physicists as Clarence Zener, Conyers Herring, Percy Bridgman, and Harvey Brooks, and metallurgists such as Cyril Smith, Larry Darken and Paul Beck, as well as a number of us from G.E.

Our research attracted visits by many distinguished scientists from other laboratories and arrangements were made, a rare practice in industrial labs at that time, for some to spend extended periods with us. Among these was F.C. Frank, whose inspiration and collaborative efforts, in the summers he spent with us, resulted in several major contributions. One distinguished foreign scientist, after spending two weeks with us, remarked to Hollomon that never before had he seen "such first class research done by such second class people". I prefer to think that most of the people were first class in one respect or another and needed only a climate such as pervaded the Hollomon group to realize their potential. This, of course, may have been what the visitor really meant. It is, I believe, fair to say that when Hollomon, later in his career, had charge of large organizations which he himself had not formed, that his leadership was not as effective as in our G.E. group. He was brilliant as an innovator but less successful as a renovator.

Thus, I began at G.E. with the freedom and resources and in a climate which seemed close to ideal for doing the research I most wanted to do. Also during my early years at G.E. I developed a more mature perspective on the nature and practice of science from reading some of the philosophical writings of Percy Bridgman and Philip Frank. They did not lead me to accept logical positivism, but did impress on me the provisional and utilitarian nature of scientific theory. This impression was, I believe very helpful to my thought.

Broadly, my General Electric research centered on the experimental characterization of, and theories for, phase changes and atomic transport in condensed systems. The materials investigated, with many collaborators, included glasses, ceramics, semiconductors, organic compounds, and polymers, as well as metals. I began by examining the theory of nucleation and with experiments on the short-circuiting effects of extended imperfections on metal diffusion and on the undercooling of liquids. My examination of nucleation theory led to consideration of transient nucleation and a numerical evaluation of the transient for a very simple set of conditions. John Fisher and I then collaborated in a number of studies in which we adapted the theory of nucleation kinetics to condensed systems and developed a theory, with Hollomon's collaboration, for nucleation of the martensite transformation. In these studies Fisher exhibited his remarkable ability for circumventing the complexities of problems and so exposing their essence.

The investigations of short-circuit effects in diffusion were made in collaboration with Robert Hoffman. We found that grain boundaries, indeed, serve as highly effective short circuits for self-diffusion in silver and that edge dislocations, under conditions where they should be little dissociated, are equally effective. In extracting the short-circuit component we used a simple analysis made by Fisher, which related the concentration profile of the diffusing species to the crystal and short-circuit diffusivities.

My research on the undercooling of liquids was, at first, exploratory and somewhat desultory; then it evolved into a major effort. When I arrived at G.E. the experiments of Schaefer and

Vonnegut, in Langmuir's group, on ice nucleation were underway. Also, Vonnegut was investigating the solidification behavior of dispersions of oxide coated tin droplets. Keeping informed on their experiments, I began investigating the undercooling of liquid gallium, which was convenient for study since its thermodynamic crystallization point, T_m, was only a little above room temperature. I found that 1 cm^3 specimens sealed in evacuated tubes: readily undercooled several tens of degrees, $\Delta T°$, before nucleation onset; growth following nucleation was very rapid, recalescence raised the temperature at continuing growth almost to T_m; $\Delta T°$ varied considerably from sample to sample but, for a given sample, generally remained the same through several melting ↔ crystallization cycles; with increasing superheating of the liquid prior to cooling, $\Delta T°$ increased to a point and then remained constant; at large undercooling, nucleation could be initiated by a slight tap on the wall of the tube containing the specimen. These effects were all very interesting but, upon examining the literature, I found that, beginning with the observations of Fahrenheit on the freezing of water in the early eighteenth century, they had all been documented, for one liquid or another, much earlier. So it seemed hardly surprising that gallium behaved as it did. Then, I believe in the autumn of 1948, I was told by Hollomon that John Chipman had asked him to recommend a speaker on "Solidification" for an M.I.T. Metallurgy Department colloquium. Hollomon promptly recommended me as "a world expert" on solidification, and I found that he had already arranged for me to speak at the colloquium on a date only two weeks later. I was quite disconcerted and set about thinking what I could say, without seeming foolish, to an audience most of whom, I presumed, would be better informed than I about my topic. At this point, the import of the droplet studies of Vonnegut on tin and of Schaefer on water suddenly became clear to me. I then concluded that any liquid should exhibit large undercooling at nucleation onset if dispersed into isolated droplets with a number density much larger than that of heterophase impurity nucleants in the original mass. To test this idea I formed a dispersion of mercury droplets by shaking mercury in a test tube containing a solution of sodium oleate in alcohol. The droplets were quite large, their

diameters were probably in the 0.1mm range, but they undercooled 30 to 40° before there was measurable crystallization, as indicated dilatometrically.

I then went confidently before the M.I.T. audience and explained the droplet and thermal history effects and described my results on mercury. The presentation seemed to be well received and almost as an afterthought I expressed the view that even those liquids which crystallize to close-packed structures might undercool deeply if the heterophase nucleants could be isolated, e.g. by the droplet technique. I suggested that the common experience that these liquids begin to crystallize at almost negligible undercooling might reflect only that close-packed "dirt" was far more ubiquitous than dirt with more exotic structures. My "afterthought" was greeted with polite skepticism by Bert Warren and others in the audience. This skepticism was based on the then prevalent view that the atomic short-range order in monatomic liquids was only trivially different from that in close-packed crystals. Thus it was thought that with slight undercooling the liquid should collapse almost continuously into the crystal or perhaps crystallize by a grain coarsening mechanism. Such a mode of crystallization also seemed consistent with the common experience that crystals melt at only slight superheating. At the time many theorists identified the temperature at which the crystal would become unstable, due, e.g., to a vanishing shear modulus or increase of vacancy concentration to some critical level, with that at thermodynamic melting. The implication seemed to be that the melting of close-packed crystals was, thermodynamically, almost a second order transition. Actually, as Clarence Zener had pointed out, the shear moduli of most close-packed crystals are quite high at T_m and falling, with temperature, only slowly on a course which would extrapolate to zero only at temperatures exceeding 1.5 T_m. Also Mendenhall and Ingersoll had reported in 1908 that droplets of certain metals which formed f.c.c. crystals deeply undercooled before crystallizing. However, their observations were not followed up and their significance went unappreciated. They came to my attention only after I had completed most of my undercooling studies.

Soon after the M.I.T. colloquium John Fisher asked Vonnegut and me to discuss our solidification results at a meeting of the Science of Metals Club. This club had been founded by

Fisher and met one evening each month in an Italian-style Schenectady restaurant. It drew scientists interested in metals from all laboratories in the Schenectady area. In my talk I again expressed my prediction that liquids which form close-packed crystals would deeply undercool if freed of heterophase nucleants. The response was again quite skeptical and, this time, not so polite. David Harker dissented vehemently, and rashly promised to eat his hat if molten copper could be undercooled more than a few degrees. This was a challenge I could hardly ignore.

To make the undercooling studies I enlisted the collaboration of Robert Cech who designed and had constructed a microscope hot stage in which the metal particles could be heated and cooled while they were observed microscopically. Crystallization was manifested by an abrupt surface roughening and, for the higher melting metals, by a sudden brightening or "blick" which attended recalescence. We demonstrated conclusively that molten droplets of a number of high purity metals, including copper, silver, gold, nickel, palladium, cobalt, and iron, could be deeply undercooled to temperatures at least 18% below their melting points before crystal nucleation occurred. Also droplets could be held at 15% undercooling for indefinite periods without crystallizing. David Harker graciously accepted the validity of our findings and appeared at the next Metals Club meeting with a headpiece fashioned from Swiss cheese.

Microscopically the high crystal nucleation resistance revealed by our experiments showed that crystallization hardly can result from mere collapse of the liquid structure, rather some essential reconstruction of the liquid short-range atomic order (SRO) must occur. F.C. Frank proposed that an icosahedral type of SRO, which by itself is non-crystallographic, might predominate in the liquid. Its transformation to close-packed crystalline SRO would require reconstruction. In support of this proposal Frank showed that, with an interatomic potential of the Lennard-Jones type, the energy of a 13 atom cluster is lower in an icosahedral than in either close-packed crystalline configuration.

In terms of classical nucleation theory our results indicated a large crystal-melt interfacial tension. At that time I thought, as I expect David Harker did, that this tension would be primarily energetic in origin. Probably Harker understood, as I then didn't, that it should be possible to form a crystal-melt interface in a monatomic liquid at negligible energy cost, provided there is no density deficit. However, the considerations of Frank, as well as those arising later from the dense random packing models for amorphous structure, indicated that the density deficit could be eliminated only by marked localization of positions on the liquid side of the interface. Such

localization, effectively a layering in liquid structure, would be attended by substantial negentropy. Thus while the energy or negentropy can vanish separately they cannot do so simultaneously. Later, at Harvard, Frans Spaepen and Robert Meyer showed that the upper limiting entropy decrease associated with this interface localization would be more than adequate to account for the magnitude of the crystal-melt tension indicated by the nucleation experiments.

In retrospect, I feel greatly indebted to David Harker for the stimulus which impelled me to investigate the undercooling of liquids such as Cu and Ni. Also I benefited from his occasional sharp criticisms of my ideas about certain other problems. He was an incisive thinker who was quick to detect a flawed argument.

After the microscopic studies I investigated, by dilatometric and calorimetric methods, the kinetics of nucleation in various Hg droplet dispersions. Those dispersions constituted by droplets, 3-5 microns in diameter, coated with Hg laurate or Hg benzoate films exhibited very large onset undercoolings, about 0.35 T_m, when cooled at 1°/minute. The kinetic studies revealed that isothermal nucleation in these dispersions was stochastic in both volume and time and so described by a single valued frequency/volume. These were, I believe, the first experiments in which such single valued frequencies of solidification were found and measured. The large undercooling at nucleation onset, $\Delta T°$, exhibited by Hg suggested that the other metals might show even larger $\Delta T°$ than we found initially. However, I didn't find time to investigate this possibility.

The results of the undercooling experiments naturally suggested that liquid metals might form glasses if quenched rapidly enough. At first I discounted this possibility since extrapolation of the melt viscosities into the undercooled regime, on an Arrhenius course set at T_m, indicated that they would approach solidity level values only at temperatures approaching 0°K. This was still my view in the autumn of 1957 when I began a seven-month leave from G.E. spent mainly at the Cavendish Laboratory of Cambridge University, where I shared an office with Morrel Cohen, then on leave from the University of Chicago. In this period I visited several European laboratories, including that of J.A. Prins at Delft. During my visit Prins pointed out--as I could have known had I read certain of the papers cited in Kauzman's 1948 review--that some liquids composed of small compact molecules and bound only by van der Waals forces, exhibit viscosities which, though small at T_m, deviate positively from an Arrhenius course as temperature falls and rise sharply, but continuously, to the solidity level at temperatures far above those indicated by the Arrhenius extrapolation from T_m. These observations suggested that the liquid structure becomes locked by repulsive forces at some point during its collapse with cooling.

Upon returning from Cambridge I discussed what I had learned from Prins with Morrel Cohen. In these and later discussions we developed the ideas about the glass transition and conditions for glass formation which were described in several papers published over the following few years. A major feature of our development was the association of configurational locking of the liquid during its compaction, by cooling or pressurizing, with a vanishing "free volume". We supposed that the elementary step in liquid flow would require a local density attenuation of amplitude greater than some critical value. Glass formation was associated with a transition in the atomic movements from a predominantly translatory to oscillatory character with increasing compaction. These considerations and the evidence that crystallization was reconstructive led us to predict that all fluids, including metals and other monatomic ones, would form glasses, when sufficiently compacted, by undercooling or pressure, if crystallization did not intervene.

Upon returning to G.E. I set out to experimentally test our prediction that all liquids should form glasses if crystallization could be bypassed. I had noted the extremely deep eutectic in the Au-Si system and decided that the eutectic alloy melt might be the best candidate for glass formation since the undercooling required to reach the glass temperature from its low liquidus should be minimal. I was preparing to test for glass formation by this alloy, using Cech's drop tube method, when the report of Duwez and his students, that this alloy did form an amorphous solid during rapid melt quenching, appeared in *Nature*. Morrel Cohen and I then pointed out in a paper, also published by *Nature*, that our theory indicated that deep eutectic compositions should be especially favorable for glass formation. Professor Duwez later noted that he was guided by our deep eutectic criterion in his further search, following the Au-Si discovery, for alloy glass formers. In retrospect, it is clear that the cooling rates realizable in the Cech drop tube would not have been adequate to quench the Au-Si alloy to a glass. Its glass temperature is exceptionally low so that its glass formation requires imposition of the very high thermal gradients achieved in the rapid quenching techniques pioneered by Duwez and his students. However, the drop tube method would have been sufficient to quench droplets of the Pd rich Pd-Si eutectic alloy to a glass, as was demonstrated by the later experiments of my student, Alvin Drehman, at Harvard. This alloy proved to have a glass temperature relatively much higher than that of the Au-Si eutectic.

Following our studies of the undercooling of liquid metals my colleagues and I applied the dispersion technique to characterize the nucleation of various other transformations. These included investigation, in collaboration with Robert Cormia, of the crystallization of dispersed droplets of various n-alkane liquids, with chain lengths ranging from 16 to 32 carbon atoms. We found that the undercooling, $\Delta T°$, at measurable nucleation onset was surprisingly small, only of order 0.04 T_m, compared with the metal values of 0.2 to 0.35 T_m. Our kinetic studies showed conclusively that isothermal nucleation in each of the high purity liquids, n-hepta- and n-octa decane, was stochastic in both volume and time. Further, the extremely high measured value of the temperature coefficient of nucleation frequency/volume, a factor of seven to eight thousand per

degree, was in excellent agreement with the predictions of classical nucleation theory. The abnormally low undercooling at nucleation onset may reflect considerable local alignment of the chain segments in the melt so that reconstruction of the SRO necessary for crystallization is substantially less than for other liquids. Later Fraser Price, Robert Cormia and I measured the frequency of crystal nucleation in polyethylene by the droplet method. We found that $\Delta T°$ was about $0.15 \, T_m$ which is 5 times larger than that typically exhibited by bulk specimens. The high nucleation resistance of the polymer relative to that of the C_{16}-C_{32} n-alkanes was attributed to resistance to chain folding, which we supposed necessary for nucleation.

Having found that crystallization in liquids usually is initiated by heterophase nucleants, present inadvertently, I became actively interested in the problem of heterogeneous nucleation in solids. It was well known that nucleation of incoherent phases in solids often is initiated at extended imperfections but that particles of *coherent* phases may form copiously within the crystal. What was not known were the departures from equilibrium required for the onset of measurable nucleation of new phases within the perfect crystal. J.H. van der Merwe and Harvey Brooks, independently, had adapted the Read-Shockley theory for grain boundaries to the calculation of the interfacial tension, $\sigma_{\alpha\beta}$, of incoherent interphase boundaries. Their calculations indicated that with increasing crystallographic misfit, δ, $\sigma_{\alpha\beta}$ should at first increase rapidly and then become approximately level at δ of only a few percent. Putting these values of $\sigma_{\alpha\beta}$ into the expression for the classical nucleation rate Brooks showed that the homogeneous nucleation frequency of those incoherent phases having the larger misfits should never reach measurable levels.

The austenite-martensite interface in iron alloys is thought to be incoherent. To assess the role of heterogeneities in martensite nucleation Cech and I investigated the transformation behavior of small, 50-100 μm diameter, single crystal spheres of Fe-Ni alloy formed during the fall of the molten alloy droplets in a tubular furnace. We found that the martensite start temperature in some of the spheres was at least 175° below that in the bulk alloy. From this behavior we concluded that

martensite formation in these alloys must be initiated at extended imperfections rather than by homogeneous nucleation, in accord with Brooks' calculation.

The interfaces between face-centered cubic lead and tetragonal tin also are expected to be incoherent. Thus it would seem that the diffusive motion of Sn in Pb(Sn) alloy solutions should be virtually frozen before an undercooling sufficient to nucleate Sn homogeneously could be reached. Borelius and his students had made careful resistometric studies of the precipitation of Sn from these alloys. In interpreting their results they supposed that the nucleation was homogeneous, starting from homophase concentration fluctuations, but ignored any contribution of interfacial tension to the nucleation barrier. Also they supposed that precipitate particle growth occurred by volume diffusion. Their suppositions seemed quite unreasonable to me. My calculations indicated that the growth rate of their Sn particles must have been several orders of magnitude greater than the maximum permitted by the accepted volume diffusion rates. Harry Treaftis and I then undertook experiments on these alloys in which we thoroughly characterized the isothermal time dependence of, and the effects of, thermal cycling on the precipitation rates.

Our experiments indicated that in any isothermal experiment all the operative nuclei were present from time zero; there was no measurable homogeneous nucleation. Also the activation energy for precipitate growth proved to be only 0.4 of that for volume diffusion of Sn in Pb. To account for these results I supposed that precipitation actually was effected by sweeping short circuits which deposited rods or plates behind them. This model was fully confirmed by our later microscopic observations which showed that precipitation occurred by the deposition of colonies of more or less parallel tin lamellae from moving grain boundaries, which served as short circuits for the transport of Sn to the leading edges of the lamellae. To test the model further Warren DeSorbo and I followed the precipitation calorimetrically as it occurred in a dispersion of small Pb(Sn) alloy spheres, with diameters less than 40 microns. After thorough homogenization most of the alloys were single crystals. We found that the total extent of precipitation was only 0.0025 to 0.10 of that exhibited, under the same conditions, by bulk specimens, thus, confirming that extended

imperfections are essential for the nucleation of precipitate. I learned later that our observed Pb(Sn) precipitate morphology was similar to that sometimes found in other alloys and that its evolution, known as "discontinuous" precipitation, had been explained phenomenologically by Cyril Smith.

Our results thus confirmed Brooks' prediction that the resistance to homogeneous nucleation of incoherent phases should be extremely high.   They also demonstrated the power of the dispersion technique in reducing or eliminating transformation from heterogeneous nuclei. Left open was the question of the resistance to homogeneous nucleation of coherent phases.

The definitive theory for the initiation of coherent precipitation was developed by John Cahn beginning with his treatment, in collaboration with John Hilliard, when both were members of our G.E. group, of spinodal decomposition in strain free ($\delta = 0$) systems. A central parameter of the theory is the gradient energy, which determines the interfacial tension between the coherent and parent phases and imposes a minimum on the wavelength of concentration fluctuations which can be amplified in the spinodal regime. Following the appearance of the Cahn theory the kinetics and morphology of precipitation beginning in the spinodal range came to be extensively investigated. However, there were no measurements of the frequency of coherent nucleation in the temperature ranges above the spinodal points. Shortly after I moved to Harvard, I.S. Servi, then at the Kennecott Copper Research Laboratory, invited me to consult with him on his resistometric study of the precipitation of cobalt from its dilute solution in copper; in this system δ is quite small and the initial precipitate is coherent. Servi's measurements were highly accurate and we decided that they could be adapted to determine the nucleation frequency of coherent cobalt.   The essential experimental requirements were: that the specimens be brought directly from the homogenization to the precipitation temperature without intermediate quenching and reheating; and, of course, that precipitation temperatures be chosen which were well above the spinodal limits. We found, for initial cobalt concentrations ranging from 1 to 2.7 at.%, that undercoolings of 60 to 90°, corresponding to supersaturation ratios about 2.0, were required for the onset of copious nucleation. We deduced the isothermal nucleation frequencies and found that their temperature and

concentration dependences were in reasonable agreement with the predictions of classical nucleation theory. Subsequent determinations by other investigators have been in fair accord with our results.

The results on liquid undercooling also raised the question: why do crystals generally melt at slight superheating? In classical nucleation theory the nucleation resistance is due, primarily, to the work of forming the crystal-melt interface. This same resistance should oppose the formation of such interfaces within the perfect crystal.

The explanation proposed by Frenkel and others for the slight superheating actually observed was that, at capillary equilibrium, the external surface of a crystal, at its melting point, would be coated with a thin molten film. As heat is supplied this film migrates inward, but if the interfacial rearrangement frequency k_i is very high the interfacial superheating will be hardly detectable when thermal gradients of the usual magnitude are imposed. Under these conditions little can be learned of the ultimate superheatability of the crystal interior. However, there are materials, usually covalently bonded, in which k_i is so small that the interface temperature is virtually identical with the ambient when only small thermal gradients are imposed. It was known that some of these materials, e.g. albite, would withstand large superheating without internal melting. The kinetics and morphology of melting of two materials of this type, quartz and P_2O_5 crystals, were investigated by J.D. Mackenzie, Norman Ainslie, Robert Cormia, and myself. We measured the dependence of the rate of melt-in from the external surface on superheating and found no evidence of interior melting to the highest temperatures imposed, which for quartz corresponded to a superheating exceeding 350°.

Early in the 1950's Suits approved the initiation of a high pressure-high temperature project which was to have as its main object the synthesis of diamond. Hollomon had a prominent part in the deliberations leading to this decision. The history of the high pressure project, culminating in the synthesis of diamond by the team of Bundy, Strong, Hall, Bovenkerk, and Wentorf, has been thoroughly aired from opposing viewpoints, elsewhere. In my judgment credit for the synthesis

belongs to all the individuals on the team and to the G.E. Laboratory management for taking the decision to proceed with the project and for providing the considerable resources which were crucial for its success.

There was a long history of efforts to synthesize diamond in a metastable state at one atmosphere pressure. Since my research had shown that up to moderate departures, at least, from equilibrium crystal nucleation in liquids and in many solid state transformations is effected entirely at heterogeneities I became interested in the possibility that diamond might grow in the metastable regime providing the heterogeneities which presumably nucleated graphite could be separated from them. I was further attracted to this possibility by Bridgman's remarks to Zener at the authors' breakfast preceding an A.S.M. weekend seminar, that he took seriously evidence that diamonds found in certain meteorites formed under metastable conditions. He mentioned a failed attempt at M.I.T., motivated by Karl Compton, to grow diamond crystals by exposing them to hydrocarbon vapor at an elevated temperature.

Sometime later I initiated, with the collaboration of Gerald Sears, William Rocco, and Richard Oriani and with Hollomon's enthusiastic support, a project to grow diamonds metastably. Essentially, our strategy was to avoid graphite nucleation by using diamond seed crystals so small and perfect that they, hopefully, would not include graphite nucleating heterogeneities. Our calculations indicated that, under these conditions, the crystals should grow as diamond when exposed, at sufficiently elevated temperature, to gaseous or condensed solutions supersaturated with carbon. However, our attempts were unsuccessful. Sears and Rocco noted that, under some conditions, the surface morphology of crystals exposed to carbon vapor changed in a way suggestive that there had been some growth, but the changes were small and the crystals actually had lost a little weight in the process. When the high pressure synthesis was achieved we discontinued our efforts. In retrospect, I think that we gave too little attention to the state of the carbon in the nutrients and to possible growth inhibition by tightly bound contaminants which may have been present on the surfaces of our seeds. In recent years other groups apparently have

succeeded in growing diamond in the metastable low pressure regime, but I am not familiar with the detailed procedures used.

I joined G.E. at a time when the Laboratory and, indeed, the company as a whole were rather loosely organized. The lines of authority often were indistinct and the leadership was quite informal. The president of the company, Charles E. Wilson, was a self-made man, with little formal education, who had advanced, from a lowly position, through the ranks. He was a huge man with a striking presence and spoke with a booming voice in an evangelical and rather "corny" style. He held extremely conservative economic and political views. However, he was a humane person, passionately devoted to his company and very proud of its technological achievements. He would, I think, rather have G.E. renowned for "lighting the world" than for 15% after tax profits. He was proud of the Research Laboratory and was reported to have said that the Company, having benefited so much from science, had a special obligation to support it.

While he was president, there were summer meetings of groups of selected professional and managerial personnel at the company-owned "Association" island in Lake Ontario. The groups met serially, each for three days, and were constituted by a mix--engineers, scientists, managers, salesmen, etc.--of about 300 people drawn from all parts of the company. Each day there were meetings at which the group was regaled with rosy accounts of performances in selected enterprises and company components. Liberal amounts of time were allotted to recreation, organized by assigning each attendee to one or another of four teams--"red", "green", "orange" or "blue"--which then competed in various sporting events. Near the end of the meeting a winning team was announced and duly recognized. The competition seemed to free pent-up juvenile instincts in most attendees. Sumptuous meals were provided but hardly could be enjoyed because at every mealtime the dining hall was turned into a virtual bedlam by shouted slogans and almost continuous boisterous cheering and table pounding by members of one or another of the teams. At the end of the first day there was held what was meant to be a solemn ceremony, under a huge and aged elm tree, in which the first-time attendees were inducted into the G.E. professional elite. The climax of

the meeting came on the afternoon of the third day with the arrival of Mr. Wilson. His arrival, by seaplane, was announced by the firing of blanks from a cannon and, as he was launched ashore, the entire group lined up in the dock area while the band played martial airs. Upon landing he shook hands with everyone present and when we reassembled in the meeting hall he delivered an impassioned oration which profoundly affected most of his audience.

For me the affair was rather distasteful but, in some ways, very funny. Certainly it was highly vulnerable to satire and ridicule. Indeed, Kurt Vonnegut, who was present at one of the sessions I attended, apparently found it a rich source of material for his first novel. At that time Vonnegut was the Research Laboratory's publicist. He wrote two news releases on the research of my colleagues and myself. I thought that he, as well as all other science writers I've known, overdramatized their news stories. However, he understood much better than the great majority of them what he was writing about. In retrospect, I suppose that the Island meetings did serve to knit the diverse and scattered G.E. professional community together and boost the *esprit de corps* of most of its members.

In the early 1950's Mr. Wilson was succeeded, as company chief executive, by Ralph Cordiner, who had been trained as an accountant. Cordiner's approach to management was, in sharp contrast with Mr. Wilson's, cold and analytical and he seemed utterly obsessed with the "bottom line". Apparently he thought the loose organization of the company somewhat scandalous and set about imposing a rigid managerial hierarchy with the responsibilities of all managers, from the lowest to the highest levels, precisely defined. Actually, before he assumed full responsibility there was already a trend, motivated by the rapid company growth, toward better defined organization.

The Research Laboratory also grew rapidly in the immediate postwar period and it was reorganized in 1950. William Ruder, a gentle low-key person, was head of the Metallurgy Department at the time of this reorganization. However, the reorganization of Metallurgy was designed primarily by Hollomon, who became department head at Ruder's retirement. The

department was divided into four sections. Hollomon asked John Fisher to manage the Physical Metallurgy Section and me to manage the so-called Chemical Metallurgy Section. This was a type of responsibility that I never asked for nor expected to have, but I accepted it, as it was understood that I would continue to participate directly in research. The broad objective of the department was to develop new materials and processes, hopefully technologically important, through improving the understanding of: structure and its evolution and control, properties, and materials-property relations. The Chemical Metallurgy Section was to concentrate mainly on the characterization and understanding of structure, from the atomic level up, and its evolution. Its steady population was 20-25 staff scientists and about 10 technicians. The scientists, even those who were new Ph.D.'s, were free to choose, after due consideration of the advice of others, their problems and associations. Thus my responsibility was not to direct the research of 20 scientists, rather it was to: stimulate interactions between them, represent them before higher management, allocate resources, and bring important new research opportunities to their attention. Such opportunities were approached through voluntary decisions of staff members and by hiring new personnel who were able and interested in the new area.

At one time or another, 35 scientists and engineers were members of the Section during the period (1950-1958) that I headed it. About two-thirds of them began their post-university careers in the Section. Most achieved high professional distinction.   Among the major lines of investigation initiated and carried forward by the Section were: the development of the theory of spinodal decomposition by J.W. Cahn and J.E. Hilliard; studies of the formation and mechanical properties of metal whiskers by S.S. Brenner and G.W. Sears; J.S. Kasper's determinations of the complex crystal structures of certain intermetallic phases and his and F.C. Frank's prescription of the rules for the structures now known as Frank-Kasper phases; other structural studies by W.L. Roth, B.W. Roberts, B.F. Decker, L. Guttman, M.B. Webb, E.H. Jacobsen, and C.W. Tucker; studies of physi- and chemi-sorption on clean and structurally well characterized surfaces by G. Ehrlich, T.W. Hickmott and E.G. Brock; D.A. Vermilyea's research on anodic oxidation and

electrocrystallization; research on the kinetics and morphology of crystal growth by G.W. Sears, R.L. Fullman, J.W. Cahn, W.B. Hillig, J.B. Newkirk, and J.L. Walker; characterization and interpretation of thermal properties by W. DeSorbo and R.A. Oriani; atomic transport by R.E. Hoffman, E.W. Hart, and R.H. Doremus; kinetics and morphology of solid state transformations by J.W. Cahn, R.E. Cech, J.E. May, A.H. Geisler, H. Rosenbaum, and J.B. Newkirk. I am proud of the achievements of these scientists as members of the Section and in their later careers. The scientific opportunities were, on the whole, well chosen and the overall program was, I think, quite well balanced. In retrospect, I think that one serious lack was a strong effort on inorganic materials synthesis.

As section head I sometimes was called upon to help resolve various personal and interpersonal problems of staff members. These experiences, I'm sure, led me to a much better understanding of the range and variety of human personality. A yearly ritual, in which section heads were heavily involved, was the rating of staff members for the purpose of salary action. To begin the process each section manager ranked their staff members from first to last on the basis of their perceived actual and potential achievements. A young scientist who might have accomplished little might be ranked ahead of a productive older scientist if he was thought to show great promise. The next step in the procedure was interleaving the section rankings within a department. Then the department rankings were interleaved, with much argument, to reach a laboratory-wide ranking from one through the number, of order 350, of the laboratory staff. There was even considerable controversy over the precise order in the middle of the ranking. After the final order was agreed to salary changes were approved such that the salary of each individual would be brought into near conformity with the salary-age curve corresponding to the decile which included their rank. The dispersion of these curves, especially at the middle deciles, was far from sufficient to justify the effort spent in fine tuning the order. After the salary changes were decided the section head met with each section member to inform them of the salary action and assess their performance. In these meetings there was the temptation, resisted by the more experienced and perceptive ones, for

managers to assume an air of technical omnipotence and moral superiority. The evaluations were, of course, quite subjective and it is my experience that moral excellence has rarely been a prime criterion in the selection of executives, department heads, managers, etc. Perhaps the most surprising reaction I encountered in an evaluation meeting was that of a young staff member who, when informed that his salary was to be substantially raised, berated me severely and sincerely because he thought the increase was much more than his performance deserved. He told me that, as a company stockholder, he feared that such "irresponsible" salary action would jeopardize his investment. I was pleased to learn that there existed someone with such depth of conviction.

The G.E. salary review process seemed overly elaborate and complex, but it apparently was typical of those of many other industrial laboratories at the time and even of some now. It had the merit that every staff member was at least noticed and his efforts reviewed at least once each year.

While the size of the sections remained about constant the administrative responsibilities of section heads were gradually increased to the extent that they began to interfere seriously with my direct research activity. I preferred to concentrate on science rather than administration and relinquished my managerial position after my return from Cambridge in 1958.

In 1954 I accepted Frederick Seitz's invitation to join him in editing a set of volumes on *Solid State Physics*. Seitz's treatise on "The Modern Theory of Solids", published in 1940, had a profound influence on the definition and development of the discipline of solid state physics. Due to the tremendous growth of the field following World War II he felt a pressing need to revise and expand the treatise, but the task seemed too great for one author and the essential subject matter too voluminous for containment in a single volume. The plan developed was to publish volumes serially each containing several comprehensive, in depth treatments of basic topics by experts in the fields. The coverage was to be very wide, ranging over the entire fields now labeled "Condensed Matter Physics" and "Materials Science". I was to represent the materials science end of the spectrum. Dr. Kurt Jacoby, a scholarly man who was then vice president of Academic Press,

enthusiastically supported the project and arranged to have the books published by his company. The volumes, now about 40 in number, became the *Solid State Physics* series. Henry Ehrenreich joined us as a co-editor in 1967. Seitz had a remarkable capacity to inform himself of and to digest significant developments in all areas covered in the series. I know of no one more scrupulous than he was in answering his correspondence, apparently all of it, promptly and in detail. He seemed able to read and write anytime and anywhere, quite oblivious to surrounding distractions.

During most of my G.E. career I was also an adjunct professor of metallurgy at Rensselaer Polytechnic Institute (R.P.I.). This activity was a part of a cooperative program between G.E. and R.P.I. whereby young G.E. staff members could do research at G.E., supervised by an adjunct professor, which could constitute an R.P.I. Ph.D. thesis. The G.E. students had to fulfill the R.P.I. course and examination requirements for the degree. In this program I guided the Ph.D. research of David Vermilyea, Sidney Brenner, Herman Rosenbaum, and Norman Ainslie. They all worked on problems which were of Ph.D. caliber and fitted well with the G.E. research program. As an adjunct professor I also gave, on occasion, a graduate course in kinetics which met after hours at the laboratory and was taken by R.P.I., as well as by G.E., based students.

By the late 1950's it appeared that G.E.'s management had become somewhat disenchanted with their liberal policy, especially as it had been exercised in the Hollomon group, of permitting scientists more or less free rein in their choice of problems. Mr. Charles Wilson's sentiments notwithstanding, any industrial organization expects substantial visible returns from support of its own basic research. It is a truism that, for its part, a research laboratory must keep a close check on the pulse of the company's technology to insure that the important needs of that technology are clearly appreciated by the laboratory scientists. Also, there is greater likelihood that research results will be utilized when a continuum of activity, from basic research through manufacturing operations, is maintained. Such a continuum assures overlapping interests and effective communication all along the line. Insuring that it exists is the joint responsibility of laboratory and company management. For its part, company management must promote a climate in the manufacturing

laboratories which will attract and hold outstanding engineers and applied scientists. When I joined G.E. such a climate seemed to exist in most of these laboratories as shown by the ability of their members to understand and critically evaluate the Research Laboratory's activities. It is my judgment, with which many may disagree, that later G.E. management policies led to major shifts of emphases in the applied laboratories toward short-term goals and "fire fighting". As a result the central laboratory had to take much greater responsibility for "technology transfer" and it redistributed its efforts accordingly. Another reason that industries often become disenchanted with their basic research stems from the view, which seems prevalent throughout industry, as it was at G.E., that the time between a scientific advance and its technological application, if any, should take no more than 5-6 years. Actually, there are numerous examples in the materials field, G.E.'s Lucalox development is one, where this time has taken more than 15 years. In my opinion, the difficulties that industries have had in benefiting from their research are due in part to their preoccupation with short-term goals and in part to the often too cavalier attitudes of their laboratories toward the company's technological needs.

Anyway, because of the seeming shift in the laboratory's emphasis toward more programmatic and applied research and since I was approaching mid-career I began in 1960 to consider more seriously the academic positions which were offered to me from time to time. Other reasons for my changing attitude were: my leave at the Cavendish Laboratory reminded me how pleasant and stimulating a university environment can be; the retirement policy of G.E., as well as in other companies, was quite rigid, requiring retirement at age 65 or even before--as one of my colleagues put it, "one can't grow old gracefully in this setting"; perhaps the activity at G.E. which I found most pleasant was in helping to train new staff members for research. About 15 of these--including the 4 whose Ph.D. research I guided in the R.P.I.-G.E. program--began their professional careers in collaborative research with me. Naturally I was attracted by the prospect of devoting full time to this activity; finally, my experimental designs were always quite simple and fell far short of fully utilizing the magnificent facilities for sophisticated experimentation provided by G.E.

I soon received quite a number of highly attractive academic offers. After visiting several universities and spending the 1962 spring semester there I was most attracted to the offer of a professorship by Harvard University. The reasons that Harvard most appealed to me were: it was the only university I visited where there were no organizational barriers between solid state physics, metallurgy, and applied mechanics; Harvey Brooks, Bruce Chalmers, Nico Bloembergen and Bernard Budiansky were all in a single division. This arrangement, I thought, should be especially conducive to the interdisciplinary associations to which I was accustomed at G.E.; the Harvard graduate students, while relatively few in number, were of the highest quality; Brooks, who was the Division head, made it clear that I would not be expected to form a large research operation, it was hoped, in accord with my own objectives, that I would become an educator rather than an "empire builder". On the negative side I sensed what one of my most distinguished academic friends warned me of: that Harvard's sociology, with its heavy emphasis on the more formal and theoretical side of science, and learning in general, would not be conducive to understanding of and sympathy for the discipline of materials science. However, I noted that Bruce Chalmers was successful and happy at Harvard and I was confident that Harvey Brooks fully understood and appreciated the importance of materials science in both the educational and scientific frameworks. My wife and our children, who were in their early teens, were all happy in Schenectady and were no more than lukewarm towards the prospect of moving to the Boston area. Because of their feelings and my long and happy association with the Research Laboratory I wavered for some time over the Harvard offer. Finally I did accept it after two of my best friends, John Fisher and Fraser Price, on whom I could always rely for frank advice, convinced me that it was in the best interests of my family and myself to do so.

In retrospect, my G.E. experience was a very exciting one of professional growth and maturation and it provided invaluable perspective and preparation for my educational and research efforts at Harvard.

Harvard   University

In the autumn of 1962 I began my Harvard career as Gordon McKay Professor of Applied Physics at a salary about 40% below my last one at G.E. However, over the years at G.E. I had been generously compensated and saved enough so that my family lived comfortably on the lesser income.

Every year at Harvard, excepting one when I was on sabbatical at Stanford, I taught a graduate course on "Kinetics of Condensed Phase Processes". It was presented in four parts: (1) basic reaction rate theory, (2) atom movements, (3) theory of capillarity, and (4) kinetics of phase changes. During the 23 years it was taught, more than 200 persons from Harvard and M.I.T. either took it for credit or faithfully audited it. They were graduate students, postdoctoral fellows, and young faculty members drawn from the fields of physics, metallurgy, chemistry, geology, ceramics and applied mechanics. Various of my students urged me to write a book based on my lectures, but I never found the time and energy to do so.

As part of the course I gave several laboratory demonstrations. In one, showing the superheatability of water, the procedure, adapted from Wakeshima and Takata [*Journal of the Physical Society of Japan*, Vol. 13, p. 1398 (1958)], was to inject water droplets (a few tenths mm in diameter) into the lower end of a column of liquid along which a thermal gradient was imposed, such that the temperature ranged from 30° to 300°C between the foot and top of the column. The essential properties of the column liquid were: virtual immiscibility with water; density greater than that of water; and normal boiling point in excess of 300°C. As the water droplets rose in the column they became superheated upon passing the 100° level. When cavitation, from the first vapor nucleus, occurred, the droplet exploded in quite a conspicuous manner. The superheating was inferred by noting the level at which the explosion occurred. In some demonstrations droplets reached the 225°C level before exploding. One year Robert Apfel, a student specializing in acoustics and fluid mechanics, was fascinated by the demonstration and decided to develop it into a

Ph.D. thesis study, which F.V. ("Ted") Hunt, the acoustics professor, and I agreed to co-direct. Apfel ingeniously modified the procedure so that water droplets could be "trapped" acoustically, and then observed, at various levels of the column. In this way he characterized the temperature and pressure dependence of the cavitation and demonstrated that water droplets could be heated to 279°C, corresponding to 179° superheating, before cavitation onset.

The other formal courses I taught at Harvard were taken, mainly, by undergraduates and were in the areas of thermal and introductory solid state physics. I developed thermodynamics from its experiential basis and strongly emphasized problems whose solutions demanded deep physical insight. My problems and mode of instruction in thermodynamics were not, I'm afraid, overly popular with the upperclass students; most were applied mathematicians who would have been more comfortable with the more formal approach to the subject. For a number of years I taught one section of a course in applied physics in which students learned physics by carrying through major laboratory projects and solving problems. Each student was required to complete four projects during the year, distributed over the areas: mechanics, electricity and magnetism, optics, and thermal physics. I was responsible for the thermal physics section in which students carried out projects such as: measurement of the Joule-Thomson coefficient; determination of the equation of state of rubber bands; the tensile strength of liquids; construction and characterization of a fuel cell; study of monolayer behavior using a Langmuir trough. Students, 8-14 in each section, mostly set up their own experiments and were called upon to acquire an in-depth understanding of the physics underlying their projects. I was pleasantly surprised to learn how well first year students grasped, e.g., the thermodynamics of the Joule-Thomson effect or of rubber elasticity. The course was quite popular, attracting very good students who welcomed a science course in which the experiments were not "canned". However, it was highly consumptive of faculty time and for that reason was finally discontinued.

Upon arriving at Harvard I started research on the formation and properties of amorphous solids of all types and on molecular transport and structural changes in polymeric and other organic

systems. Later, my students and I also did research on: "fast" diffusion in metals and its ramifications; crystal growth and phase separation in covalent systems; morphology and kinetics of precipitation from solid solutions; and crystal nucleation in liquids and glasses. During my Harvard career I have guided the doctoral research of 40 excellent students. Also 10 postdoctoral fellows, who had not been my Ph.D. students, have been associated with my research. Their disciplinary backgrounds, students and postdoctorals, were quite varied: about 50% physics or applied physics; 25% materials science; 10% chemistry; and the rest electrical or mechanical engineering.

Much of our research was directed at understanding the formation and nature of metallic glasses. That the melt $\leftrightarrow$ glass transition may occur in a metal was first demonstrated by Ho-Sou Chen and myself. We first looked for the thermal manifestation of the transition by the alloy Au_4Si, which Duwez and students had melt-quenched to an amorphous solid. However, upon reheating, the amorphous alloy crystallized without exhibiting the transition. We then thought that we might retard the crystallization, which was to the metastable γ phase, by replacing a substantial part of the silicon with germanium. Indeed, the ternary alloy so formed did exhibit the transition, both thermally and rheologically, and it could be cycled several times between its undercooled melt and glass states without crystallization which, however, did occur at a temperature, T_{kc}, only a few degrees above the glass temperature, T_g. That the rheological transition occurred in the range of the thermal transition was shown by partial spheroidization of alloy films at $T > T_g$ and by measured shear viscosities, 10^{10} to 10^{13} poise, of the fully relaxed alloy, well within the fluid range. The changing isothermal viscosity as the alloy approached its relaxed state also was measured. Later Chen and I found that the binary alloy $Pd_{82}Si_{18}$, as well as certain ternary alloys, formed by some replacement of Pd or Si in the binary, also exhibited the glass transition, both thermally and viscometrically. In these alloys the displacements of T_{kc} above T_g were generally greater than in the Au-Si-Ge alloy. The displacement, T_{kc}-T_g, is, of course, one index of the resistance of the alloy glass to crystallization, the larger it is the less should be the cooling rate needed for quenching the

alloy melt to a glass. Chen and I found that T_{kc}-T_g could be increased considerably, by *selected*, but not indiscriminate, substitution of other elements for part of the Pd or Si. Later, at Allied Chemical, Chen in collaboration with Donald Polk, also one of my former students, designed selective alloying procedures whereby the glass formability of transition metal based alloys was increased sufficiently for them to be processed cheaply into large masses of glassy metal strip.

Our later research at Harvard indicated that, in accord with the theory for crystallization kinetics, T_{kc}-T_g increases with the glass temperature scaled by the liquidus temperature, T_l, i.e., T_{rg} = T_g/T_l. Impurity additions may increase T_{rg} either by lowering T_l or raising T_g. It appears that the liquid lowering effect is, as Cohen and I proposed, the predominant one, but the impurity effect on T_g is, in some alloys, quite appreciable. Recently H.W. Kui, A.L. Greer, A.J. Drehman and I found that, when heterophase nucleants were removed, the alloy $Ni_{40}Pd_{40}P_{20}$, which has one of the highest known T_{rg} (~0.67) for metals, could be melt-quenched to a glass in virtual bulk form (thickness ~1 cm) at rates of only 1 deg/sec. Then Kui and I showed, again taking care to eliminate nucleants, that the alloy could be reheated from its glass to its liquidus temperature at rates as low as 2.5 deg/sec without crystallizing.

Chen and I also obtained the first evidence, by transmission electron microscopy, that upon reheating certain metallic glasses, they were ternary Pd-based alloys, phase separate before crystallizing. Later, my students C-P.P. Chou and M.A. Marcus characterized the occurrence and course of the phase separation in certain of these alloys more quantitatively by small angle x-ray scattering studies. Thus the thermal, viscometric, relaxation, and phase separation behaviors of metallic glasses were all shown to be remarkably parallel to those of the common nonmetallic glasses.

Another major line of our Harvard research was the characterization and modelling of the structure of metallic glass. My students, Brian Bagley, Charles H. Bennett, Slade Cargill, Donald Polk, and Frans Spaepen, all made important contributions to this research. In the late 1950s J.D.

Bernal showed that the structure (DRP) formed by the densest random packing of hard spheres, constrained by curved surfaces only, was amorphous and apparently stable internally. Detailed examination of the local configurations showed that most were tetrahedral and only a very small fraction could be described as half octahedral. Bernal and others thought that the structure modelled that of monatomic liquids, but in 1964 Morrel Cohen and I noted that the structure apparently was "locked" like that of a glass, and proposed it as prototypical of a monatomic glass structure. Bennett formed a DRP structure by a computer simulated condensation of hard spheres in which the spheres were placed at surface binding sites, selected in a various ways, with no subsequent reconstruction allowed. The Bennett structures were somewhat less dense than Bernal's, but tetrahedral configurations were predominant while half-octahedral ones were scarce. Bagley and Cargill carefully characterized the diffraction of x-rays by electrodeposited Ni-rich amorphous Ni-P alloys. Cargill then showed that the radial distribution function (RDF) of Ni atoms deduced from these results and the RDF's of other glassy metals then available, were in good agreement with the DRP structures of Bennett and with that constructed by Finney following Bernal's prescription. Later Polk suggested that the metalloid atoms were sited in the larger holes of a DRP structure formed by the metal atoms. This second "Polk" model is now thought to be oversimplified, but it implied a highly developed short range order such that each metalloid atom is virtually surrounded by metal atoms. Such compositional ordering in amorphous Au-Si alloys had been indicated by Chen's thermal measurements and its existence in other metal-metalloid alloys was shown by measured partial distribution functions.

Bagley, one of my earlier students, and I thought that properly selected amorphous materials might exhibit ferromagnetism. We looked first for a liquid ferromagnet. It appeared that certain molten cobalt-based alloys might exhibit ferromagnetism if sufficiently undercooled. Bagley prepared some of these alloys, but found no convincing evidence, using the rather crude probes at our disposal, that any of the molten phases were ferromagnetic. Bagley then turned his attention to the properties of amorphous films formed by condensation techniques and he and I were among the

first to demonstrate the occurrence of ferromagnetism in amorphous solids; it was exhibited by a Co-P alloy formed by flash evaporation and condensation. However, apart from investigations by J. DeLau, who spent a leave from the Philips-Eindhoven Laboratory with us, and Dawning Pan, we had little subsequent effort on ferromagnetism. Other electronic property studies were made by my students, C.H. Lin and M.A. Marcus, who characterized, using sensitive differential techniques, the effects of structural relaxation on the electrical resistivity of some Pd-based alloy glasses.

My students and I became interested in the low temperature mechanical properties of alloy glasses after being informed by Pol Duwez that some Pd-based glasses exhibited remarkable room temperature ductility. This subject also interested Michael Ashby, then on our faculty. To account for the highly localized mode of the low temperature plastic deformation, Polk and I proposed a model in which compositional disordering produces flow softening. Later, Spaepen and I suggested a complementary explanation for flow softening based on the free volume model. Ashby and his students measured the elastic moduli of amorphous Co-P alloys and confirmed that the shear modulus was low compared to that expected for crystalline materials. Denis Weaire, then a postdoctoral fellow at Harvard, in collaboration with Ashby and his students, accounted for the relatively low modulus, using the DRP model for glass structure. Ashby and his students also found that the creep behavior of amorphous Co-P alloy, at temperatures well below T_g, exhibited substantial relaxation effects. Later, Alan Taub and Spaepen characterized the creep of Pd-based alloy glasses, in the temperature regime just below T_g, precisely and thoroughly. They found that, depending on the extent of structural relaxation, the isothermal shear viscosity varied by at least four orders of magnitude and they were the first to measure the temperature dependence of the viscosity of the glass in an isoconfigurational state.

The relation between atomic diffusivity and viscosity of glasses is a problem that always intrigued me, but it was very difficult to determine experimentally because of the extremely low values of glass state diffusivities. One of my students, Martin Rosenblum, decided to try to measure these diffusivities by monitoring the decay in amplitude of concentration differences in

compositionally modulated films--a technique used by Hilliard and his students at Northwestern to measure very small interdiffusivities in crystalline materials. Preparing compositionally modulated films, a sputter deposition technique was used, was experimentally very difficult, but Rosenblum persevered and eventually produced good compositionally modulated films with wavelengths ~25Å. With Spaepen and myself, he measured metal atom interdiffusivities, D, as low as 10^{-22}cm^{2}sec^{-1} corresponding, as one wit put it, to an average atomic jump rate of "one per semester". We found that the isothermal D was sharply decreased by structural relaxation, but were able to determine its temperature dependence, which matched that of the viscosity, for the glass in an isoconfigurational state. Later A.L. Greer, who joined us as an S.R.C. postdoctoral fellow and later became a faculty member, and his student, R.C. Cammarata, improved the deposition techniques and then determined D for Pd- and Fe-based glasses over a wavelength range from 2 to 70Å. Again, they found that the isothermal D depended strongly on the relaxation state.

Before beginning his research on metallic glass, Ho-Sou Chen had prepared specimens of amorphous germanium (a-Ge) by electro- and vapor-deposition and was investigating their structure and thermal properties. He and I made the first determination of the heat of crystallization of a-Ge; our result has now been confirmed by a number of other investigators. Much later Chen and Bagley, when at Bell Labs, and Spaepen and I independently used this data to show that there must be a metastable equilibrium between the amorphous and liquid metal phases of Ge, as well as of Si, at a point about 200-250° below the equilibrium melting point of the crystal. Our prediction has been confirmed, at least qualitatively, by recent studies of the melting and solidification behavior of amorphous overlays on the crystal following short high-energy laser pulses.

Following a custom started by Bruce Chalmers, our group met for coffee at 10:30 each morning and 3:30 each afternoon. At these meetings my students and I often discussed structural models for a-Ge. One day John DeNeufville, I believe, brought in some interconnectible tetrahedral units. He, Polk, Meyer, and some of the others explored what types of four-connected structures could be formed from these units and it was quickly realized that a fully-connected "random"

network was one such structure. About that time Simon Moss and his student, Joseph Graczyk had, at MIT, carefully determined, by scanning electron diffraction, the RDF of amorphous silicon films vapor deposited in high vacuum. Moss described these results to us and Polk compared them with the RDF of a random network model he had hand-built from the tetrahedral units. The match of the model to the experiment, including the average distortions of the tetrahedral angles from their ideal value, was excellent and so originated the first "Polk" model. About this time interest in amorphous semiconductors, partly stirred by S.R. Ovshinsky's claims that they had exceptional electronic properties, became widespread and William Paul and his students at Harvard began experimental studies of their structure and other properties. Also, Henry Ehrenreich became interested in the theory of amorphous semiconductors and other disordered systems and, in this period, our three groups--Paul's, Ehrenreich's, and mine--often held joint seminars and other meetings to discuss amorphous solids. Earlier I had useful interactions with Arthur Bienenstock, then on our faculty, on the structure of nonmetallic glasses. He and my student, Brian Bagley, made an important analysis of the upper limiting intensity of small angle x-ray scattering which could arise from microcrystalline solids.

The Ph.D. studies of my students on polymeric and other organic systems included: the liquid-glass transition of a van der Waals bonded material, o-terphenyl, by Richard Greet, my first Harvard research student; the kinetics and morphology of crystallization of linear polymers by Joanne Burns; point defects and diffusion in molecular crystals by Ray Baughman; viscosity and kinetics and morphology of crystallization of amorphous selenium by postdoctoral fellows Kee S. Kim and Richard B. Stephens, and student Geraldine Gross; and effects of magnetic and electric fields on the structure of liquid crystals by Robert B. Meyer. I had interested Meyer in the problem of the kinetic resistance to crystallization of liquid crystal phases. We thought that this resistance might be significantly lower than that of isotropic liquids. Actually, Meyer found that, in its nematic state, p-azoxyanisole sustained a rather large undercooling, about 70-80°, before the onset of measurable crystal nucleation. Following my suggestion, he read F.C. Frank's paper, in the

Faraday Society Discussions, on the structure of liquid crystals. Immediately, he comprehended the paper fully, and, inspired by it, began his investigations, carried through with minimal guidance, of the magnetic and electric field effects on liquid crystal structure. Perhaps 12-15 of my other research students transformed, as Meyer did, their original research project into quite a different one, usually better or anyway more suited to the student's interests and abilities. Fortunately, the funding agencies, usually the Office of Naval Research or the National Science Foundation, were quite tolerant of these changes.

Meyer's results aroused the interests of other Harvard faculty in liquid crystals and, I believe it is fair to say that, largely due to his stimulus, there developed an interdisciplinary program of liquid crystal research in which faculty from chemistry, physics, and the applied sciences participated. Meyer had a central role in this program, first as a postdoctoral fellow and later as a faculty member. Other faculty who were centrally involved included Peter Pershan from Applied Physics and David Dolphin from Chemistry. The program was considered a model of the type most worthy of support by the NSF-sponsored Materials Research Laboratories.

While I was at G.E., F.C. Frank and I developed our "dissociative" mechanism for diffusion with which we interpreted the transport behavior of copper in germanium. In this mechanism, the impurity is primarily in a substitutional state, but after dissociating from its reciprocal vacancy, it migrates interstitially until trapped by another vacancy. This mechanism is thought to be responsible for the "fast" diffusion of many noble and transition metal impurities in four-coordinated semiconductors. It was long known that gold diffused with abnormal rapidity in lead and the studies of Ascoli and Kidson showed that the abnormality could not be due to short circuiting by grain boundaries or dislocations. Many thought that the transport occurred by the "tumbling" of tightly bound gold-vacancy (Au-v) pairs, formed by association of a vacancy with a *substitutional* Au atom, but I thought that, rather, it might be effected by the dissociative mechanism. I interested my student, Thomas R. Anthony, in the problem, and he pointed out that the tumbling motion should produce a large and readily measurable enhancement of the lead self-diffusivity,

D_{Pb}. The critical experiment was to measure how D_{Pb} was affected by Au. While preparing for the experiment, we discovered that Seith and Keil had already, in the late 1930s, carried it out with a null result and had concluded, therefore, that Au must migrate in Pb mainly by an interstitial mechanism. Then, Anthony, Brian F. Dyson, a postdoctoral fellow, and I, confirmed Seith and Keil's results on the effect of added Au on D_{Pb} and in addition established that: Cu and Ag are fast diffusers in Pb; and that the increase in D_{Pb} with added Cu was orders of magnitude less than demanded by a Cu-v tumbling mechanism.

Later another of my students, J.W. Miller, showed that D_{Pb} actually is increased measurably by added Au, but by an amount very much less than it would be if the tumbling mechanism predominated. Thus, their transport behavior indicated that the noble metals must, contrary to Häggs' empirical rules, dissolve, in part at least, interstitially in Pb. We proposed that such interstitial solution behavior might be expected provided: the host ion cores were separated widely enough that they would not be overlapped by interstitially sited solute; and the valence of the solute atom was relatively small. These criteria suggested that fast diffusion in metals by interstitial mechanisms should be more widespread than had been thought. Our predictions were confirmed by the experiments of Anthony, Dyson, and myself showing that tin, indium and thallium were hosts for fast diffusion and by experiments in several other laboratories which indicated the occurrence of fast diffusion in many other systems fulfilling the specified conditions. Other Harvard studies of interstitial solution behavior and its ramifications were made by my students: Wesley Akutagawa, Ann Campbell, Barney M. Cohen, J.W. Miller, William K. Warburton, and Ali-Reza Yavari, and by my postdoctoral fellows: Gregory R. Gruzalski and Curtis W. Owen. Professor Stephan Berko of Brandeis University collaborated in some of our studies and Akutagawa's thesis was co-guided by Professor James W. Mayer of Caltech.

Other main areas of my Harvard research and those collaborating in my investigation of them were: (1) kinetics and morphology of precipitation from solid solution, with my students King-Ning Tu and Alexandre N. Rossolimo; (2) kinetics and morphology of phase separation and

viscosity in covalently bound systems with students T.P. Seward, John P. DeNeufville,   Charles H. Drummond and postdoctoral fellow Donald R. Uhlmann; (3) the pressure enhancement of crystal growth and atom transport rates in covalently bonded materials with Professor J.F. Hays of the Harvard Geology Department, Donald R. Uhlmann, and students Vincent J. Fratello, Michael J. Aziz and Eric Nygren. Dr. John M. Poate and associates of Bell Labs joined Aziz, Nygren and myself in our studies of the pressure dependence of crystal growth and atom transport rates in silicon. We found that the kinetic coefficients of certain of the processes in four-coordinated systems were increased 1 to 2 orders of magnitude by application of 30-40 kilobars of pressure, indicating that they are attended by large *negative* activation volumes; (4) nucleation and growth of crystals in metal liquids and glasses with Frans Spaepen and Lindsay Greer and students, Genevieve Devaud, Hin-Wing Kui, and Carl V. Thompson; (5) the heats of crystallization and crystallization rates of ion-amorphized Ge and Si with Poate and his associates of Bell Laboratories, Spaepen and student Edward P. Donovan; (6) morphology and properties of superconducting alloys formed by rapid solidification processing with Professors Joze Bevk and Michael Tinkham, postdoctoral fellow Fawwaz Habbal, and students Chen-Hsi Lin and Gregory Clemente.

Interacting with my Harvard students and postdoctoral fellows has been one of the most rewarding and pleasant activities of my career. In general their intellectual power and imagination has exceeded even my initial expectations. However, at the beginning of their research many students were quite inexperienced in the laboratory and their initial progress sometimes was painfully slow. The beginner tends to idealize the research process and seeks to achieve step by step perfection using the most sophisticated techniques that can be devised. This approach often leads to impasse and frustration. The research strategy which I commend to my students, having found that it worked best for me, is an opportunistic one in conjunction with what, in common parlance, is known as the "quick and dirty" approach; i.e. to first do a variety of easy experiments with the crudest workable procedures. Such experiments may not lead to publishable results, but

often they indicate quickly in which direction further effort should be focused. If promising results are obtained they can then be tested and quantified by more refined techniques.

In this day of complex and expensive methods, many wonder if a small experimental research group, constituted mainly by graduate students and with modest financial resources, can any longer have a significant impact on science. However, while the development of advanced experimental techniques is indispensable to scientific progress, the developers often are specialists who are more "technique" than "problem" oriented. It is still crucial to recognize the significant problem, whose solution often demands a concert of techniques. To perform the "critical" experiment, it may often be necessary for the university scientist to seek out and attract the collaboration of those, wherever they may be found, with the needed experimental facilities. I emphasize to my students that while we can hardly achieve distinction with our hardware, we may do so with our wits.

As my students gained experience, their pace quickened to the point that they controlled their research and often took productive initiatives that I had not anticipated. My role as thesis adviser has been to acquaint students with significant problem areas and to help them acquire the intellectual discipline and the appreciation of the standards of achievement essential for the successful practice of science. When very good students have their research well underway, the best policy for a research adviser is to "stay out of their way". At Harvard I missed the wide spectrum of mature and expert advice to which I was accustomed at G.E., but my thought has been continually renewed and invigorated by my associations with the many brilliant and intellectually uninhibited Harvard students I've known.

Perhaps the most unpleasant period in my Harvard experience was in the era of student militancy, roughly between the years 1966 and 1972. This militancy, which developed about the same time in many universities throughout the world, was in the U.S. at first directed against the American involvement in Vietnam. Later it was broadened to attacks on the academic, industrial, and governmental "establishments". By 1968, the overwhelming majority of the Harvard faculty

and administrators probably considered the U.S. Vietnam policy to be seriously mistaken. Opposition to the policy was generally expressed by faculty as individuals in the broad political arena. Excepting on issues concerning academic freedom, the University or faculty as a whole rarely took positions on public policy matters.

Because of the openness and freedom of campus discourse and the important role of the faculty in the determination of University policies, it was generally thought that Harvard might be immune to disruption by student radicals. However, this expectation was dashed by the occupation of Harvard's University Hall by about 200 students in the spring of 1969. The immediate issue was whether or not to terminate the military officer's training program (ROTC) on campus, but in view of the ever more threatening tone of the radical rhetoric, it seemed that some issue precipitating a major clash of the militants with the University was bound to arise. The not too gentle clearing of the Hall by outside police called in by the University generated much sympathy for the radicals and their causes among moderate students and faculty. For about the next three years there was much unrest in the University, marked by shouting down of campus speakers and occasionally of professors in their classrooms, student strikes, and blockades. This type of student activism continued into 1972 when, at Harvard and elsewhere, it seemed to abate quite suddenly. After that time most students became preoccupied with academic performance and career preparation and were rather indifferent to activist causes. The reasons for the abatement still are not clear, though some associate it with the ending of the military draft.

As did other faculty, I favored some of the positions and social reforms advocated by the activists, but was completely opposed to their strong-arm attempts to impose them. Reforms and other changes in public policy in the U.S. can only be effected, I believe, through the long and often frustrating political process of convincing the general public that they should be made. However, the student activists expended most of their effort, not in the public arena, but in trying to force various policy changes on the universities. With their faculties keenly sensitive to the logical and moral ambiguities of most positions, universities were the easy targets. The militants were like

puritans in their self-righteousness but, partly debilitated by affluence, were, unlike puritans, quite deficient in self-discipline.

The militancy had little direct effect on my activities, but the ambience of the University in that period was sullen and depressing.   In 1968 I was elected to the U.S. National Academy of Sciences. It was a recognition that I had no hint of and never expected. Before my election, I imagined that the Academy had a central and exalted role in forming national science policy. However, I was disappointed to learn that membership is more honorary than operative; the main duty of most members being to elect new members. Also, the election processes seemed rather less comprehensive and objective than I had thought. Election requires that some Academy member take the initiative in seeking out detailed information on the candidate's career and making a case for their election, which will be intelligible and convincing to the other members, drawn from many diverse fields, who will vote on it.

At Harvard, "materials science" occupies the middle part of a condensed phase science spectrum which extends from applied mechanics through condensed matter physics. For the graduate student, it is one of the options within this spectrum. Often in committee and panel meetings, I have listened to hours of argument, frequently heated, about the definition of "materials science". Perhaps I am naive, but the definition has always seemed quite clear and simple to me. Broadly, I believe that it might be stated--as in my von Hippel lecture and prefatory chapter in Vol. 13 of Annual Review of Materials Science--as "the characterization, understanding, and control of the structure of matter at the ultramolecular (some prefer "supermolecular") level and the relating of this structure to properties (mechanical, magnetic, electrical, etc.)". However, in professional and educational practice, materials science is viewed, rather more narrowly, as the characterization, origin, and property effects of structure as delineated by the extended imperfections (interphase and intercrystalline boundaries, dislocations, etc.). This area is the special *forte* of those who label themselves "materials scientists" on the university campuses as well as in Washington, D.C., and it constitutes what is most unique in the curriculum offerings of materials science. It is, as my

colleague James Rice put it, the "core component" of materials science. It should hardly need stressing that this component has been and will continue to be an indispensable element of science and technology, and any university which expects to have significant representation among leaders of the field must provide effective instruction in it.

At Harvard we offer just three graduate level half-courses in this core area; they are: "Structure of Solids", "Kinetic Processes in Condensed Systems", and "Deformation of Solids". Through the default of other disciplines it has also been necessary for us to provide, in one way or another, instruction on the thermodynamics of capillarity and of phase equilibria. During my time at Harvard the research and instructional program in materials science was conducted by just two to four faculty, and more often two than four. At first, Chalmers and I were the only "permanent" faculty. Later, Ashby accepted a tenured position, but with the understanding that, in view of his strong family and other ties to England and Cambridge, he would be strongly attracted to an appropriate chair, if offered, at Cambridge University. He accepted the offer of such a chair by the Cambridge Engineering Department in 1974, after a few years on our faculty. Following Chalmers' retirement, I was for the next five or six years the only tenured materials science professor at Harvard; then Frans Spaepen was offered and accepted tenure. While I was the single permanent faculty member, a substantial part of the instructional burden in materials was borne by Joze Bevk, Frans Spaepen, and A. Lindsay Greer, who were, at different periods, on our junior faculty.

Many wonder how the Harvard materials effort, having only one or two permanent faculty and no autonomy, can be viable. In most other leading universities, the materials science faculties are quite large and autonomous. For example, at present, the MIT materials faculty numbers more than 40, and that at Cornell about 15. To survive and flourish at Harvard, "core" materials science must maintain close interactions with the much larger efforts in condensed matter physics or applied mechanics or both. Such interactions are facilitated at Harvard by the absence of organizational or geographic barriers between these fields. During my Harvard career I have had substantial and meaningful interactions with perhaps 8-10 faculty from fields, mostly condensed matter physics,

other than my own. I was instructed and much stimulated by my associations with these faculty and I believe it fair to say that they, in turn, benefited much from their interactions with me and other materials science faculty. Also, in the Harvard milieu the materials science professors have excellent opportunities to attract graduate students originally enrolled in other disciplines to their research programs. Attesting to this, only three of my first twenty research students were initially my advisees.

On the negative side the small size and lack of autonomy of the Harvard materials faculty places it in an especially vulnerable position when decisions on appointments and other matters affecting its health and survival are made. All permanent positions on the Harvard faculty of arts and sciences are, in effect, chairs, each of which is supported by an endowment sufficient to cover, at least, the academic year salary of the professor. This policy severely restricts the number of permanent appointments and there is no commitment of even a single chair to the core area of materials science. When any chair is to be filled there is considerable competition for it among a faculty with widely distributed, but often quite parochial, interests most of whom have little understanding or knowledge of core materials science and its history. It is in this situation that the Harvard sociology poses, as my distinguished friend warned that it might, a serious problem for materials science.

The Harvard procedure for making a permanent faculty appointment is quite complex and drawn out. The object is to identify, with extensive advice from outside experts, a world leader (hopefully "the" world leader) in the field and then attract him/her to Harvard. There are perhaps five or six stages in this process culminating in a meeting of an *ad hoc* committee formed by the university president, after receiving, with the faculty dean's endorsement, a department's recommendation. Typically, the committee includes the president, the dean of the faculty, several faculty from departments other than the one requesting the appointment, and three outside experts. Meeting for about a day, it reviews (or at least has available) *all* documents pertaining to the appointment, interviews some faculty who favor, and possibly some who oppose, the department's

proposal, and then recommends a course of action to the president, which he may accept or reject. Later the university corporation approves *pro forma* the president's recommendation, if any.

No Harvard junior faculty position is "tenure track". Even though a junior professor (J.P.) has an excellent performance record, he/she will not be tenured unless perceived at, and outside of, Harvard as a world leader in a field in which a chair is to be filled. Due to this stringent policy, only about 1 in 10 J.P.'s is offered tenure. In justification of the policy, it is argued that the J.P., after his/her Harvard experience, almost certainly will receive an attractive tenure offer from another major university. However, for a young experimental scientist, denial of tenure means uprooting and disrupting a research program in the middle of what is likely to be one of the most creative phases of their career. Also, the policy makes it difficult to recruit outstanding young faculty trained at universities other than Harvard. The appointment policies of most major universities are *de jure* similar to Harvard's, but *de facto* they are much more relaxed, so that a J.P. is likely to be tenured if his/her performance is perceived by the university to be merely outstanding. In my judgment, the Harvard appointments policy is overly pretentious and rather less objective than it is intended to be. Clearly, much depends on whose advice is sought and which advice is listened to by the university administrators. However, the policy assures that the permanent appointments generally will be of high quality.

Some Thoughts on Directions in Materials Science

Some thoughts on the historical development and future course of materials science were expressed in my von Hippel Award lecture ["Laser and Electron Beam Processing of Materials" (eds. C.W. White and P.S. Peercy), pp. 1-5, Academic Press, N.Y. (1980)], my Campbell Memorial lecture [Metallurgical Transactions **12A**, pp. 695-708 (1981)], and my prefatory chapter in *Annual Reviews of Materials Science* [**13**, pp. 1-7 (1983)]. Here I will only recapitulate and update them.

Of course, materials technology, empirically based, is thousands of years old, but the development of materials science, as we know it, did not begin until about the 18th century. Nevertheless, it is interesting that the general atomic and molecular concepts underlying materials science were advanced in the ancient world. The Ionian philosophers, Leucippus and Democritus, seem to have been led to an atomic theory of matter through realization that the identity and quantity of a material persist through a succession of physical transformations (e.g. evaporation ↔ condensation, melting ↔ freezing). Later, Epicurus suggested that just as a vast vocabulary can be constructed from only a few symbols so the tremendous variety of material forms might result from differing combinations of only a few elementary units.

In the modern era, solid state science began, after discovery of the basic principles of chemistry, with an initial emphasis on the characterization and understanding of ideally structured crystals and the coexistence of phases. Following notable advances in the understanding of interatomic cohesion in the early part of this century, it became clear that the rates of atomic and charge transport in many solids were orders of magnitude greater than predicted by ideal crystal models. To resolve these disparities point defect models for transport were devised by Frenkel, Schottky, Wagner and others. Later, the low resistance, relative to that predicted for perfect crystals, to plastic flow was accounted for by the dislocation models of Orowan, Taylor, and others. Still later, F.C. Frank showed that the easily measurable growth of polyhedrally shaped crystals at small departures from equilibrium could be effected by screw dislocations emergent in the crystal surfaces. Much of the materials science effort in the 1940's and 1950's was centered, as it was at G.E. in this period, on the development, refinement, and testing of these defective crystal models of solid behavior. These efforts culminated in vindicating, to a remarkable degree, the model predictions on the nature, density, and actions of the imperfections. Especially impressive were the microscopic studies which showed that dislocations exist and act in real crystals in the ways that had been forecast. In contrast with point defects, extended imperfections increase entropy so minutely that they should be practically absent from finite crystals in thermodynamic equilibrium to

quite high temperature. Therefore, it was presumed that they must be introduced into actual crystals by various growth accidents and surface damage. These presumptions were confirmed experimentally by several investigators and, perhaps more importantly, it was shown that crystals (e.g. metal "whiskers") could be grown virtually dislocation-free and that such crystals did exhibit the mechanical strength and growth resistance nearing the levels predicted by the ideal crystal models.

From my chemist's perspective, the amazing feature of these developments was that the properties of the crystals were altered so dramatically by the presence of such minute fractions (e.g. 1 in 10^6) of displaced atoms. These small deviations were aptly called "imperfections in nearly perfect crystals".

While studies of nearly perfect crystals continued, by 1960 there was a growing research emphasis on the formation and properties of materials in frozen metastable states, including amorphous and other highly disordered solids. This new and still continuing emphasis was stimulated in part by the challenge to theory posed by disordered systems and in part by advances in experimental techniques adaptable to metastable structure synthesis. Among the principal techniques used in these syntheses are: ultra-rapid melt quenching, various condensation procedures (vapor-, sputter-, electro-), ion implantation, ion beam mixing, and laser pulsing. They have in common the feature that they can effect extraordinarily high rates of energy flow into and out of thin material sections. Exploitation of these techniques has, during the past three decades, led to the synthesis of a wide variety of metastable structures. Among these are: amorphous solid forms of semiconductors and metallic alloys, which have already been alluded to; "quasicrystals", new and unexpected structures which are, strictly, aperiodic, but which exhibit sharp diffraction patterns with non-crystallographic (e.g. icosahedral) point group symmetries; solid solutions with much higher solute concentrations than known heretofore, indeed, it now seems possible, in principle, by ion implantation or deposition processes, to form homogeneous solid solutions of

virtually arbitrary composition; interphase dispersions, prepared by deposition or processing of *in situ* composites, with interfacial area densities approaching the ultimate limit.

There are other methods of structure synthesis which I expect may become more widely exploited in the future. One of these, developed by W.L. Johnson, R.B. Schwarz, and others, is the formation of metastable structures by solid state interdiffusion of selected materials. There is much to learn about the thermodynamics and kinetics of these processes and their potential for producing new materials is probably far from full realization. A somewhat neglected approach in metastable synthesis is that of eliminating heterophase impurities, e.g. by the isolation or fluxing techniques alluded to earlier, which may nucleate the more stable state during deenergization before a metastable one forms. I believe that wider use of this approach would prove quite productive.

Another important area that seems to deserve increased attention is that of interface modification, by, e.g., irradiation or chemical reaction, in order to increase interfacial strength. Such processing can be crucial for forming composites with acceptable mechanical strength.

As I have noted elsewhere, the historical emphasis in materials research might be viewed as having progressed toward the more disordered (and non-equilibrium) states of matter. Indeed, it appears that we can now fabricate solids in which the ultimate degree of disorder is approached, but the understanding of the structure and properties of such materials is still in a highly primitive state. In my judgment, the opportunities for the discovery and study of novel metastable structures are still so great that they will heavily engage material scientists for some time to come. The present situation in the synthesis of such structures seems somewhat akin to that in organic chemistry following establishment of the principles of chemical synthesis; the possibilities for new and interesting combinations of structural elements seem almost limitless.

The application of the new understanding of materials to ceramics has not progressed as rapidly as it has to semiconductors and metals. The slower pace of ceramic science has been due partly to the greater difficulty of processing (e.g. by melting and shaping) ceramics which makes

experimentation on them much harder. I expect that we will see growing emphasis on this branch of materials science and wider application of the newer techniques to the synthesis of ceramics in novel metastable forms.

Also, I believe that we will see greatly increased effort in the materials science of condensed organic systems, especially polymers. Progress in this field has been hampered, I believe, by a lack of communication between organic chemists and materials scientists. However, there is, I think, great potential for achieving new understanding and for discovery of still more new polymeric materials with exceptional electrical, mechanical, and thermal properties.

Some General Observations

As may be clear from what I have written, I am confident that the enlightenment and vastly improved human capabilities resulting from scientific progress is, on the whole, good for society. Of course, it is a truism that some of the novel capabilities deriving from physical science can be and have been put to very bad uses, such as: the creation of terribly destructive weapons; the development of processes which can lead to serious, and possibly catastrophic, environmental damage; and the over-indulgence of society with consumer goods. Concerning consumer over-indulgence, I find myself in general agreement with the viewpoints expressed by J.K. Galbraith in his "The Affluent Society".

Certainly it is a responsibility of scientists to do their utmost to inform society of the destructive potential of new weapons and of the great damage that certain practices, e.g. profligate consumption of fossil fuels, may inflict on the environment. The public response to such information will be determined by the outcome of political processes. My preference has been to participate in these processes as an individual citizen rather than as a scientist since, in my experience, scientific ability does not necessarily assure civic sagacity. Thus, I believe that the

political choices of a well informed and educated public are likely to be as wise as those of scientists as a group.

Looking back, I am fortunate for having been able, during most of my career, to work toward the objective I set for myself at age 18--that of trying to advance understanding through the practice of physical science. I have tried to do this by my own research and by training students to do research. This activity has been for me intellectually challenging and highly fulfilling.

I am impressed ever more deeply by the natural order, which seems remarkable and awesome. It can be described by science, but its existence and origin are still a great mystery which seems beyond our capabilities to resolve. For me it is not necessary to accept some metaphysical solution of this mystery; it is enough that the order exists and is esthetically pleasing. Thus, I have sought a maximum commitment to life and humanity with a minimum commitment to dogma. In my view, commitment to humanity arises naturally from a wide ranging and deeply sensitive human consciousness which leads one human to feel sharply within one's self the aspirations or pains of another. I believe it is such consciousness which has led people to imagine or feel that there exists a just and humane basis for ultimate reality.