Curiosity as a Career

Eugene Newman Parker

Indulging my curiosity has been a gratifying theme in my life since my earliest memories of childhood. The world is mysterious and fascinating, filled with marvelous things that are understandable in terms of simple physical concepts. And I should add, it is gratifying to know that there are still many mysteries waiting for explanation.

When I was about five years old, I lived in a suburb of Buffalo, New York about two blocks from a busy railroad yard. The coming and going of the locomotives and train cars fascinated me, and I wanted to know how a steam locomotive worked. Then, how did a switch work, shunting the locomotive and its cars from one track to another? My father, who was an aeronautical engineer, provided explanations that I could understand, in terms of pushing and pulling, and I was thrilled.

Then, how does an internal combustion engine work? I entertained the absurd idea of making one out of wood, but was restrained by my clumsy carpentry.

How does an airplane fly? What is sound? I had played with the primitive “tin can” telephone, where a string is connected to the centers of the bottoms of two tin cans and then pulled tight. I could feel the vibrations in the string as someone spoke into one can, transmitting the vibrations along the string to reproduce the sound at the other can.

The wonders of the world were endless. Why do not the molecules of water wear out as they are perpetually rubbed against each other by the waves on a lake, or get broken as a raindrop impacts a rock?

My father worked at Consolidated Aircraft Company. My sister and brother were a year and a half and three years younger than I, respectively. My mother looked after us children, with my father participating in the evening. We sometimes went for walks along the deep gorges below Niagara Falls. The erosion of the swift river has cut a 100 meter down through layers of limestone, and it was a revelation to learn that the limestone was laid down in a shallow sea millions of years ago, from sediments eroded off the land, long before there were people in this world. I wondered what it might have been like in those far off times.

When I was six I was given a 50 power microscope and shown the fascinating active microscopic world in a drop of water from a nearby pond. The family had inherited an excellent microscope from my deceased grandfather. That was the primary instrument for investigating the world of micro-organisms, which I could see to a lesser degree with my own little instrument. Crystals of sugar and salt were beautiful to behold, and I marveled at the different shapes, sizes, and colors of grains of sand. My father possessed a pocket compass, with which I was fascinated. How did it know which way was north? And why did it become confused when near an automobile. I played with magnets, of course, and realized their awareness of each other. What a strange world!

My parents guided us children into simple household responsibilities, e.g., helping with cleaning, making beds, preparing meals, so that we felt ourselves to be a working part of the family unit from relatively early in life. They encouraged us to draw and paint, play board games and card games, and to be interested in nature and the natural geological monuments of that part of the country. River valleys and moraines were reminders of the ice age, now gone these 10,000 years. We learned to read, but we greatly profited from the more advanced reading that they did for us, with stories of adventure, fairy tales and journeys to other parts of the world.

When I was seven, we lived near Detroit, and my father worked for the Chrysler Engineering Division. It was about that time that he read a popular book on geology and biological evolution to me, with the title *Earth for Sam*. It started with the Silurian period and carried the narrative through to the Pleistocene.

I must confess that I was not an enthusiastic student at school. Interesting things turned up occasionally, but spelling lessons were difficult because of the ambiguities in the quasi-phonetic spelling of English words. My memory for isolated details was not good, nor has it improved as I have grown older. But I persisted, and with the help of a dictionary, I can do fairly well nowadays. I struggled with multiplication tables and eventually got that under control. Division can be done systematically using multiplication and subtraction, so that part was easy. Fractions were something new and interesting, except that after learning how to handle them in the Spring, we repeated the whole operation in the Autumn.

History in elementary school hinted vaguely at fascinating happenings, but the presentation was superficial, concentrating on dates and names rather than describing the nature and causes of the action. It has not occurred to some of the authors of introductory history texts that they have an interesting story to tell, with profound implications for our world today.

In high school I was introduced to algebra, Euclidean geometry, and trigonometry, which I liked very much because of their simple principles, unlike the baffling details of biology and social studies encountered in other classes. In my senior year I had the course in physics. It was, fortunately, an old-fashioned course that dealt directly and solely with the many phenomena and principles of mechanics, sound, electricity, magnetism, light, and thermodynamics. It made no extensive effort to digress into the social implications. The textbook used woodcuts instead of photographs and was written by a real physicist, at the University of Michigan. The gruff old man who taught the course was less than inspiring. But the fascination of the subject matter made up for what he lacked in knowledge and pedagogical skill. It was apparent that physics is the basis for all physical phenomena, and it was not long before I realized that I wanted to become a practicing physicist in the adult life that lay ahead of me. My grandfather had been a physicist, and I had an uncle who was a physicist at Bell Laboratories, so I knew I could earn a living that way.

I took a competitive examination and won a tuition scholarship to Michigan State University, in East Lansing. So in the Autumn of 1944, I went off to MSU to study physics and mathematics. I felt I was the luckiest young man in the world, studying the most interesting subject in the world. My first year I trained with the wrestling team, but by the second year my studies had become so time-consuming that I had to give it up. Amateur wrestling is a great sport. I gained 12 kilos that first year.

I should point out that I had another project running besides physics. During my years in high school I worked at odd jobs and eventually accumulated $120. The goal was to purchase 16 hectares of forest land from the State of Michigan. Substantial areas of land had reverted to the State because, once the timber was cut, the land had no value, so the owner stopped paying taxes. The State had no use for such land, and my $120 bought 40 acres (one quarter mile square) or 16 hectares in Cheboygan County in northern Michigan.

My brother and I rode our bicycles for three days (500 km) from Detroit and began building a log cabin during the summer of 1944. We finished the cabin the next year. In retrospect, we did many things the hard way, but we persisted and learned as we went along. About five years ago we put another new roof on the now nearly sixty-year-old cabin, along with other repairs, so it should be with us for many years to come. It is nice to be able to spend a few days in the forest, away from everything, with nothing but the swish of the wind in the trees and the croak of a distant raven. My children and grandchildren have learned to enjoy the cabin as they were growing up. My son and my brother get there a few times a year, more than I usually manage.

At MSU my studies progressed and I graduated in March 1948. I had outstanding physics teachers, and I particularly remember Professor Thomas Osgood and Professor Y. Kikuchi. They taught excellent courses and encouraged me in many ways. I applied to the California Institute of Technology (Caltech) in Pasadena for admission to graduate school. Some years later I learned that Caltech had never had a student from MSU before, and it was Professor Osgood's letter of recommendation that convinced Caltech to give me a chance. So I was admitted without financial support, and it was fortunate that I could work in the Physics Laboratory at Chrysler Engineering before going off to Caltech at the end of September. I had six months pay in my pocket when I caught the bus from Detroit to Los Angeles, a 72 hour ride.

The Physics Laboratory at Chrysler was headed by Dr. C. R. Lewis, who had known me from childhood because he and my father started work at Chrysler at the same time, and the two families were acquainted. I was the extra hand in the laboratory and learned a great deal assisting in various projects. I remember our investigation of the Hilsch tube as a possible source of air conditioning. It is a remarkable device. Compressed air is injected through a nozzle aimed tangentially around the tube, next to a diaphragm with a hole in the middle. Cold air comes through the hole and out that end of the tube, while hot air comes out the other end. We achieved temperatures of −15℃, but unfortunately the overall efficiency is so low that there was no useful application. To my regret I still do not understand how the thing works. Why the cold air in the middle?

We developed a "poor man's" radar, using a super-regenerative oscillator, which is extremely sensitive to the arrival of a returning echo. It was intended for small boat owners, and it worked just fine, thanks to the electronic skills of James Lunen, who was in charge of the project. However, the Federal Communications Commission refused to allocate a frequency band, and the project died.

At one point we were ordered from somewhere high up in the management to develop a clock for automobiles using a thermal cycle rather than a mechanical balance wheel. It was a conspicuously stupid idea, because a thermal cycle, involving active heating and passive cooling, is very sensitive to the ambient temperature. But orders are orders, so we built several models. They worked fine, and if you timed the cycling, you could readily tell the temperature in the room. You may be amused to know that the temperature occasionally reaches 65℃ in the interior of an automobile with the windows closed while sitting in the noontime sunlight on an asphalt parking lot. The thermal clock ran only about half as fast in that circumstance.

Fall Quarter started at Caltech and I was enrolled in Professor W. R. Smythe’s course in electricity and magnetism and in Professor W. A. Fowler’s course in nuclear physics, along with a course in optics and another in mathematics. Professor Smythe’s course was an exercise in problem-solving. I read the text and solved the assigned problems. I had never had a course that moved so fast and had so many new concepts to master. I spent my waking hours struggling with the assigned problems, occasionally devoting a little time to nuclear physics and optics as the situation demanded. Three weeks into Smythe’s course I began to wonder if I could learn enough to get a passing grade. Then, somehow, by six weeks the problems did not seem to be so hard anymore. They still demanded my full attention, but I began to feel I was on top of the subject. At the end of the quarter (ten weeks) I aced the final examinations in both Smythe’s and Fowler’s courses.

It was fortunate that things came out well because I could see that my money from Chrysler Corporation would get me through Winter Quarter, but no more. Tuition took a big bite out of my finances, and, like most other humans, I was in the habit of eating and sleeping, in addition to studying. So I stopped by Professor Fowler’s office one day and asked him if he knew of any jobs available, assisting in the laboratory, or whatever. He seemed surprised that I did not have an assistantship like the other students. He picked up the telephone and called Dean Watson, who was in charge of the sophomore physics course, in addition to being Dean of Students. It turned out that there was a position as a teaching assistant—someone had dropped out. So in ten minutes I had a teaching assistantship and my financial problem was solved.

Over the years I learned that Professor Fowler’s boost for me was characteristic of the man. In later years I sat on various national committees with him, and he invariably played a strong positive role, with important suggestions and ideas, encouragement and a constructive approach to the task at hand. We were all happy to see him awarded the Nobel Prize in 1983, jointly with Professor S. Chandrasekhar, for his work in the low energy nuclear physics relevant to thermonuclear burning in stellar cores. You may recall that Professor Chandrasekhar was awarded the Nobel Prize for his early work on the mass limit for degenerate white dwarfs.

This seems as good a place as any to comment on some advice I received in preparation for attending Caltech. As I was leaving MSU, Professor Osgood remarked that when I got to Caltech I would meet the occasional person who was so smart that it could be intimidating to know them. A person who listens to a lecture on a complex subject, effortlessly recalls the fine points the following day, and never seems to study or practice. Professor Osgood said not to be discouraged by such people. Just keep working away with enthusiasm and with confidence in the future.

I was glad for his remarks because I did meet a couple of fellow students who were superbright in the manner he had described. His remarks prepared me to see them in perspective and recognize that the future belongs to a plodder like me as much as it does to them. One of my superbright classmates subsequently did very well as a physicist. How could he fail? The other had no deep interest in physics and drifted off into something else.

The important thing to remember in the practice of science is to be sure that you thoroughly understand the basic principles and that you know how each application to specific phenomena follows from those principles. This is not an empty platitude. Some important branches of space physics and geophysics have suffered badly from a careless, seemingly plausible, leap into the dynamics of magnetic fields and ionized gases, thinking that the electric currents associated with the magnetic fields behave as they would in a fixed circuit in the laboratory. It is all seems so plausible. But it is contrary to the basic principles of Newton, Maxwell, and Lorentz, if you take the trouble to work it out. For instance, pursuing the electric circuit analog has led to the idea that the magnetic field represents a huge inductance in the path of the current flow. Then blocking the electric current must produce enormous electric potential differences, which would accelerate electrons and ions to enormous velocities, thereby accounting for the production of high energy particles in flares on the Sun and in the magnetotail of Earth. Unfortunately there is no such effect because the ionized gas or plasma always moves in the frame of reference in which there is no significant electric field. At the same time the topology of the current changes, detouring around the blocked region. So there is no great potential difference and generally no significant particle acceleration.

So do not concern yourself with being, or not being, superbright. Take your physics, or whatever subject you wish to pursue, one step at a time, and be sure that you understand each step. You do not need to know everything, but you must be absolutely sure that you understand the principles that you actually use.

Getting back to my experiences at Caltech, my thesis advisor was Professor Leverett Davis, with whom I formed a lifelong friendship. He was another positive individual, who gave encouragement and assistance to his associates, including his students. Incidentally he constructed and taught a superb one-year graduate course in classical mechanics.

To relate an incident from my student days, I received a note from the Office of the Registrar about two months before I was expecting to receive the PhD. The note said, in effect, you think you are going to receive the PhD in June 1951, but you are not, because you have not earned enough credits since declaring to us your candidacy for the PhD. I was thunderstruck and went to seek Professor Davis's advice. He told me that Professor Smythe was the man who knew how to survive the mysterious ways of the Registrar. Professor Smythe looked at the note from the Registrar and leaned back in his chair and remarked that there were three ways to get around the difficulty. I felt much relieved. He outlined the three and recommended that the most effective procedure would be to petition the Registrar for retroactive registration of credit for research. I had not bothered to ask for credit for my research activities, because, by that time, I had plenty of academic credits to qualify for the PhD. I had been unaware of the obscure rule that a certain number of credits were required after declaring myself an actual candidate for the degree. Anyway, thanks to Professor Davis and Professor Smythe, I graduated in June 1951, as anticipated. I left Caltech with the warm feeling that I had been among friends. Not the Registrar, but the faculty.

This is perhaps the place to relate my awakening with my first encounter with scientific publication. My PhD thesis consisted of two parts. One was a dynamical treatment of interstellar gas clouds, then newly studied and thought to be self-gravitating individual objects. Then, assuming that the individual cloud represents a Hamiltonian system-in retrospect, a dubious assumption－I worked out the statistical mechanics of the clouds and showed that they all end up either expanded and dispersed to infinity or collapsed into some very compact form, e.g., a star. That portion of my research was accepted without question by the editor and referee for publication in the

Astrophysical Journal.

The other part of the thesis pointed out that the long thin curved dust striations observed in the Pleiades can be understood only if there is a magnetic field of a microgauss or more in that region. The point was that the individual dust grains slip a long way when hit by an interstellar wind, so any initial striations would be blurred into a fuzzy cloud as the different-sized grains were picked up in the wind. On the other hand, the grains are charged photoelectrically, which would tie them to the local interstellar magnetic field, thereby preserving the initial cross-field inhomogeneities and providing the thin striations seen in the telescope.

It should be understood that at the time I was working on the thesis research, Professor Davis and others were showing how the observed polarization of starlight indicates that the interstellar dust grains are aligned, presumably by a galactic magnetic field. Enrico Fermi had already proposed an interstellar magnetic field in order to understand cosmic rays. So I had magnetic fields on my mind when I saw the dust striations in the Pleiades. My conclusion complemented the work by Fermi and by Davis, Greenstein, Spitzer, and Tukey, and others, with an independent line of reasoning that led to the same conclusion. So I was surprised, upon submitting the work to the *Astrophysical Journal*, to have it rejected out of hand by the editor on the grounds that magnetic fields have nothing to do with astrophysics. The paper had not been sent to a referee for review. Just rejected. So I published the work many years later—in 1958—in a review article for *Reviews of Modern Physics*. This was my first encounter with the “rational” character of some in the scientific community.

I mention it because it illustrates an endemic problem in scientific publication, which is as fierce today as it was fifty years ago. The principle victims are the young and relatively unknown scientists, particularly those proposing a new idea. The editors of the scientific journals can, in principle, alleviate the problem to a large degree, if they choose to take a more rational position than their colleagues. However, they would do so at some risk to their own research support. The eminent referee who declares a paper to be nonsense can be furious if the paper is published anyway. The eminent referee may subsequently be a reviewer of a grant proposal from the editor. Or worse.

It is usually easy to spot the irrational negative referee’s report, because it lacks specific backing of the negative comments. There is no commitment to any facts, so the author receives nothing with which he can argue. I recall with amusement the referee’s report on my first paper on the dynamical instability of the magnetic field in the gaseous disk of the Galaxy, submitted to the *Astrophysical Journal* in 1966. The report began with the words, “Well, I had always thought that Parker was competent, but…,” and there followed a lot of negative words that said nothing specific. The report said simply that if Parker knew as much about the Galaxy as the referee did, then Parker would not have submitted such nonsense for publication.

Chandrasekhar was editor of the *Astrophysical Journal* at that time. His office was just a little way down the hall from my office, and in discussing the report with him, I made the point that the only papers I had submitted to the *Astrophysical Journal* that drew strongly negative referee reports were the papers that contained something really new. I reminded him of the vigorous condemnation of my 1958 paper first pointing out the existence of the solar wind as a consequence of coronal expansion. He had sent the paper to two “eminent” referees, and both had condemned the ideas. He had published the paper anyway when I pointed out that the referees, for all their eminence, had no specific criticism to make.

The difficulty with scientific publication is serious and deserves attention, although I do not have space for further elaboration here. All I can say to young scientists is not to be surprised nor intimidated. Politely but firmly insist that the negative referee back up the criticism with hard facts. And if no hard facts are forthcoming, then suggest that publication is the only honorable outcome. Editors should have the courage to publish a paper if there is no factual criticism, and a brief reading of the report from a negative referee usually shows immediately whether there is sufficient substance to enable a scientific exchange with the author.

In 1951, jobs for physicists were not plentiful, and I ended up as an instructor in the Department of Mathematics, University of Utah in Salt Lake City, Utah. The department hired five new PhDs as instructors, with the verbal understanding that in two years we could expect to be promoted to assistant professor. As it turned out, however, toward the end of the second year the dean leaned on the chairman of the department to hold a firm line on the departmental budget. The chairman achieved that goal by dumping four of us instructors, thereby avoiding having to give raises. In retrospect we instructors were extremely naïve, failing to raise the issue of promotion earlier in the second year. So one day in May a senior member of the department walked into the office that we all shared and, looking rather angry, announced that the department had no intention of keeping us on in the autumn. Then he turned around and walked out. It was evident that he had just learned of the situation and was particularly annoyed by the fact that we were not told of it. I walked down to the chairman's office and asked him if it were true that we were fired at the end of the spring quarter. He looked a little surprised that I knew about it, but admitted that such was the case.

In fact, being fired from the department was one of the best things that has happened to me. I had become acquainted with Professor Walter M. Elsasser in the Department of Physics, and we had had several interesting conversations on various scientific points. So I walked by his office and asked him to let me know if he heard of any positions becoming available, explaining that I had just discovered that I was out of a job at the end of June. The next day, Elsasser came by my office and asked me if I would like to be a 2/3 time research associate with him and a 1/3 time assistant professor in the Department of Physics. Physics needed someone to teach the elementary course in astronomy, and I guess I was as much an expert as any. Needless to say, I accepted on the spot and my scientific career began to move forward.

To make a long story short, Walter Elsasser was one of the great minds of his generation of physicists. He was the first to propose an electron diffraction experiment to test the reality of the de Broglie wave hypothesis. Unfortunately, as a Jew at Heidelberg, he was obliged to leave without doing anything about it. He was the first to point out that the enormous thermal neutron absorption cross section of the beryllium nucleus was a measure of the de Broglie wavelength of the thermal neutron and had nothing to do with the actual size of the nucleus. He was the first to point out nuclear shell structure. He was the person who pointed out that the only tenable hypothesis for the origin of the magnetic field of Earth is by induction in the convection in the liquid metal core. Later, in the 1960s, he became the first to apply the concepts of information theory to biology, which has all been reinvented in the past couple of decades without realizing that Elsasser did the basic work about 45 years ago.

When I went to work for Elsasser in 1953, he was still grappling with the problem of producing the geomagnetic field in the convecting core. So my education in magnetohydrodynamics got underway, using an extensive article by Stig Lundquist in *Arkiv f. Physik* and a paper by Elsasser that was in the process of publication. I could not have had better textbooks. I should explain that up to that point Elsasser had shown that the non-uniform rotation of the core produces a strong east-west (azimuthal) magnetic field in the core through its interaction with general dipole (north-south) field that we see up here at the surface of Earth [Fig.1]. The problem was to understand how the north-south dipole field was generated. Elsasser had shown that without ongoing regeneration, the north-south field that we see would decay away in a characteristic time of 30,000 years. Professor T. G. Cowling had shown 19 years before that it is impossible for fluid motions to sustain a field with rotational symmetry about any axis. And the north-south and the east-west fields were symmetric about the axis of Earth. So I thought about the problem for a while, and drew many sketches of how the magnetic field lines would be deformed by diverse fluid motions. I finally realized that local circulation in meridional planes, representing the north-south field, is produced by rising convective cells in the core which also rotate about a vertical axis－cyclonic convection [Fig.2]. The rising fluid exhibits rotation because Earth is rotating. It was then a straightforward task to write down the dynamo equation describing this effect, so that with the equation describing the production of the east-west field by the non-uniform rotation, I had the complete set of dynamo equations. Their solution exhibited steady magnetic fields sustained by the combination of non-uniform rotation and cyclonic convection. So we had the explanation for the regeneration of the magnetic field of Earth. Then I was interested to see that the solutions varied periodically in time when applied to a relatively thin layer of convection, as is the case for the Sun. Thus the combination of non-uniform rotation and cyclonic convection in the Sun also accounted for the creation of the 11-year magnetic cycle of the Sun. I was greatly encouraged when my two papers on the subject were accepted for publication. Ten years later, others began to be interested in the subject, and have explored the solutions of the dynamo equations at considerable length. It appears that the combination of non-uniform rotation and cyclonic convection is the principal source of magnetic field in some planets, most stars, and probably the Galaxy.

During this time it became clear that there was not much prospect for advancement at the University of Utah, and Elsasser advised me to begin looking elsewhere. He was thinking about leaving too. So when I was offered a position as a research associate with John Simpson at the University of Chicago, I accepted and departed Salt Lake City for Chicago at the end of June 1955. I shall be forever grateful to Walter Elsasser. He introduced me to magnetohydrodynamics and proposed an important problem for solution. My success rode on his fundamental formulation of that problem.

In my last year in Salt Lake City, Niesje Meuter and I were married, providing another big boost to my life. I built a trailer to carry our worldly goods and we made the long drive to Chicago. We rented a small apartment across the Midway from the university. Our daughter Joyce was born in our first year in Chicago. Our son Eric was born three years later, by which time I was an assistant professor in the Department of Physics and in the Institute for Nuclear Studies, now the Enrico Fermi Institute. Life was looking up. The University of Chicago is a great place to pursue research. The teaching responsibilities are modest and my colleagues were congenial and interested in each others’ work.

I learned later, from a remark by Simpson, that Professor S. Chandrasekhar had recommended me for the research associate position. I had become acquainted with Professor Chandrasekhar through a mutual interest in the theory of turbulence during my time in Utah. Turbulence was a hot topic in those years, inspired by the work of Kolmogoroff and Heisenberg.

John Simpson was vigorously pursuing world-wide studies of the cosmic rays, and I was brought on board to look into the theoretical implications of the observations. I was fascinated by the observations and eager to make some sense out of them. It should be understood that cosmic rays consist mostly of protons, traveling near the speed of light. They come from somewhere out there in the Galaxy (presumably from supernovae etc.), and those that collide with Earth have passed through interplanetary space on the way. The fact that the cosmic ray intensity is reduced during times of solar activity (sunspots and flares) is somehow a consequence of conditions in space. It must be appreciated that there was no means in those days for sending an instrument into space. So Simpson was studying the time variations of the cosmic rays as a means for probing the unknown conditions in space. He used the magnetic field of Earth as a spectrometer to separate the variations of the low energy and the high energy cosmic rays. The north-south magnetic field of Earth allows only the higher energy cosmic rays to arrive at the surface of Earth at the equator, whereas all cosmic rays are free to come in at the poles. His goal was to restrict the theoretical possibilities as to how the variations were created. As one might expect, the variations of cosmic rays at high latitudes were substantially larger than at the equator. This simple concept is complicated by the fact that the cosmic ray protons are stopped at the top of the atmosphere—lucky for us—so that it required Simpson's invention of the ingenious cosmic ray neutron monitor to have a sensitive cosmic ray detector here on the surface of Earth.

The traditional view of space was a hard vacuum. Nothing there except for the lone cosmic ray particles (they are about 10 meters apart in space) and the occasional burst of solar corpuscular radiation and solar cosmic rays spit out by a flare on the Sun. It was inferred that the solar corpuscular radiation consisted of equal numbers of protons and electrons traveling at perhaps 1000 km/sec to arrive at Earth in 1–2 days after the flare. But space was "known" otherwise to be empty. So the observed cosmic ray variations were attributed to powerful electric fields in space, or to fluctuations in the magnetic field of Earth that somehow excluded more cosmic ray particles when the Sun was active and the geomagnetic field was agitated.

Simpson's extensive measurements swept this away, showing that the energy dependence of the variations can be understood only in terms of variable magnetic fields in space.

The conditions in interplanetary space were vividly illustrated by the great flare of 23 February 1956, which emitted an intense burst of solar cosmic rays, i.e., protons traveling at nearly the speed of light. Their prompt arrival at Earth showed the interplanetary magnetic field to be connected more or less directly from the Sun to Earth, while the subsequent slow decline of the solar cosmic rays indicated transverse fields of some form beyond the orbit of Mars [Fig.3]. I was fascinated. It was evident that there was moving plasma in interplanetary space to manipulate the magnetic fields, because the magnetic fields would otherwise not have this remarkable form.

Then one day in late 1956 Professor Ludwig Biermann, from the Max Planck Institut für Astrophysic in München, visited John Simpson, and I had a chance to talk with Biermann about his point that the gaseous tails of comets, observed always to point away from the Sun, do so because the tails are swept away by streams of solar corpuscular radiation. Although no one else seemed to take it seriously, it struck me as remarkable that comet tails point away from the Sun no matter what the general level of activity of the Sun and no matter whether the comet passed by at low or high heliographic latitude. Evidently the solar corpuscular radiation originated in no special feature of the Sun and filled all of interplanetary space all the time.

A little later, while visiting the High Altitude Observatory in Boulder, Colorado, I found myself in a very interesting discussion with Professor Sydney Chapman. He was working out the consequences of the fact that the million degree gas that makes up the corona of the Sun is such an excellent conductor of heat, and cools so little by radiation, that the million degree temperature extends far out into space [Fig.4]. Indeed, he showed that the corona extends well beyond the orbit of Earth. So space was not empty, but filled with the extended atmosphere of the Sun.

That was an important inference, with profound consequences. It occurred to me sometime later that so extended a corona would block the universal solar corpuscular radiation inferred by Biermann from the comet tails. The blocking followed from the fact that, if one plasma moves rapidly through another, the electrostatic forces between the ions and electrons of the two streams kick up plasma waves and the two streams lock together. So the ideas of Biermann and Chapman seemed mutually exclusive. Either there was universal solar corpuscular radiation or there was an extended corona, but not both. And yet I could not see how either could be avoided, given the evidence at hand. It was something to think about. When you have a contradiction, it is likely that you may learn something interesting when you follow up on it.

Now, if the solar corpuscular radiation is emitted in all directions from the Sun at all times, it must have some simple origin, something that does not depend of flares or sunspots. I began to wonder if it might arise from the continuing expansion of the million degree corona. This was not considered previously because a million or two degrees is not hot enough for the individual ions to escape from the powerful gravitational field of the Sun. But Chapman's work showed that the temperature was still a significant fraction of a million degrees out here at the orbit of Earth. Hence it was obvious that the coronal gas out here easily escapes from the Sun. Indeed, the gas cannot help but escape, expanding out through the solar system. Some earlier work that I had done, exploring the large-scale dynamics of a tenuous plasma, showed that the conventional hydrodynamic equation properly describes the expansion of the very tenuous coronal gas. Writing down the equation for the radial outflow of the gas, it was evident that there was a mathematical solution of the equation that started very slowly in the dense gas near the Sun and gradually accelerated outward in the sustained high temperature, eventually reaching supersonic speeds of 300-1000 km/sec far out from the Sun. That provided the universal solar corpuscular radiation that streams away into the vacuum of the surrounding interstellar space. There were other expanding mathematical solutions that never reached high speeds, but they required an inward pressure from interstellar space to prevent them from expanding supersonically. Lacking any such inward force, the only possibility was the supersonic expansion, which I eventually decided to call the *solar wind* instead of solar corpuscular radiation. The term *solar wind* properly conveys the concept of a hydrodynamic flow of a gas rather than a lot of free particles shot out from the Sun, as implied by the term *solar corpuscular radiation*.

Once it is understood that the solar corona expands away into space, it is a simple exercise to show that the solar wind stretches magnetic fields outward from the Sun into a spiral [Fig.5], extending out past the planets into interstellar space for as far as the wind can push back the interstellar gas and interstellar magnetic field. The best estimate today is that the supersonic solar wind passes through a standing shock wave and slows to subsonic speeds somewhere in the vicinity of 100 A.U., that is to say, at a distance 100 times farther from the Sun that the planet Earth [Fig.6,7]. A friend from my student days, Alexander Dessler, later came up with the name *heliosphere* for the region swept out by the solar wind. It is the term in use today. It was immediately evident that this magnetic configuration fulfilled the requirements set down by the solar cosmic ray flare of 23 February 1956 with the field pulled out radially from the Sun and strongly wound around beyond the orbit of Mars.

It was clear that the outward sweep of the transverse magnetic fields carried in the solar wind inhibits the penetration of the cosmic rays into the inner solar system, and, therefore, accounts for the observed reduction of the cosmic ray intensity here at Earth during the years of strong solar activity. The outbursts on the Sun produce shock waves and other disturbances in the magnetic field in the wind, so that it is more difficult for the cosmic rays to move in along the kinky magnetic field to reach the inner planets. So the whole picture fell together in a natural way.

As I mentioned earlier, my first paper in 1958, setting out the solar wind and its origin as the expansion of the million degree corona, was condemned by two "eminent" referees, and published by Chandrasekhar because the referees had no specific criticisms. I found that general disbelief was the reaction of most workers in the field. The first detections of the solar wind were made by K. I. Gringauz in the Soviet Union, with instruments on the Luna 2, Luna 3 and the Venus 3 spacecraft. These detections showed a flux of ions from the Sun with speeds in excess of about 100 km/sec. One inferred that the ions were accompanied by an equal flux of electrons, of course. Subsequently Bruno Rossi's group at the Massachusetts Institute of Technology sent out a more sophisticated plasma detector on the Explorer 10 spacecraft and measured average ion speeds of about 300 km/sec, but the ion flow had the strange habit of switching on and off over periods of the order of an hour. So there were enough ambiguities in the measurements that they were not accepted by the skeptics as definitive.

It remained for Marcia Neugebauer and Conway Snyder at NASA's Jet Propulsion Laboratory in Pasadena, California to establish the wind with six months of uninterrupted data from their instrument carried to Venus on the Mariner II spacecraft. The solar wind became an established fact. The big surprise was that the density of the wind at the orbit of Earth was only about 5 protons/cm^3, so that the net flux of ions was approximately $2x10^8$/cm^2 sec. Both Gringauz and Rossi had measured about that same total flux, so there could be little doubt as to its validity. The surprise lay in the fact that previous indirect estimates based on comet tail acceleration, the zodiacal light etc. had favored 500-1000/cm^3. Unfortunately the hydrodynamic theory of coronal expansion cannot predict the density of the wind because of the extreme sensitivity of the density to the run of temperature out through the corona, which is hardly known to better than a factor of two even today. So 5 or 500/cm^3 made little difference to the theory of the origin of the wind, but it made a big difference to the estimated radius of the heliosphere and to the degree to which the impact of the solar wind compressed the geomagnetic field on the sunward side of Earth. It became clear that the Explorer 10 spacecraft had barely reached the outer of the geomagnetic field, and the intermittent character of the wind represented the boundary moving back and forth across the space craft. Explorer 10 was in the wind only when the boundary had moved inward. When the boundary moved outward over Explorer 10, the spacecraft was shielded from the wind by the geomagnetic field.

Subsequently I speculated on the theoretical nature of the stellar winds of other stars. I explored the unstable dynamics of the magnetic field of the Galaxy, which is rooted in the gaseous disk of the Galaxy. I worked out some models of the galactic dynamo that presumably creates the magnetic field. I was fascinated by the way in which the field is inflated (at some 50 km/sec) by the continuing production of cosmic rays, presumably by supernovae and related phenomena, in the gaseous disk. Then it occurred to me that the existence of the magnetic field of the Galaxy rules out the existence of any significant number of magnetic monopoles, i.e., free magnetic charges, in the universe, because free magnetic monopoles would quickly dissipate the galactic magnetic field. Michael Turner, Thomas Bogdan, and I looked into the question pretty thoroughly and could find no way to the monopoles from destroying the magnetic field of the Galaxy. So the monopoles must be extremely rare, if there are any at all. Even magnetic monopole plasma oscillations do not avoid the catastrophe. In fact, I found that a universe with magnetic monopoles, in which the present galactic magnetic field is to be understood as the field associated with the monopole oscillations, would have the curious property that the magnetic fields would not move with the ionized gas, in contrast with the observations. Instead, the galactic magnetic fields would move in a frame of reference exactly half way between the moving plasma and the background magnetic monopole plasma. It would be a strange world indeed.

In 1972 I first recognized the tendency for magnetic fields with untidy field line topologies to develop internal surfaces of tangential discontinuity when the fields are allowed to relax to equilibrium. It took a long time for me to develop a comprehensive understanding and perspective on that subject, which I finally published in 1994 as a research monograph, *Spontaneous Current Sheets in Magnetic Fields*. I have enjoyed that pursuit very much because it has taken me into the elementary physics of magnetic fields. It is my guess that the dissipation of magnetic energy at the surface of the discontinuities, or current sheets, is a major player in creating the X-ray coronas of many stars, including the Sun. So the world around us is a never ending source of amazement.

I think this is enough detail on my adventures in curiosity to give some idea of my efforts. In looking back over the years, it is clear how much I owe to the friendly help from so many others. Broad encouragement from my parents, encouragement from my teachers, encouragement from senior colleagues while getting started, a job that allowed me to pursue my scientific curiosity, others who shared incomplete ideas, and a generally congenial scientific atmosphere in which to work. The Enrico Fermi Institute and the Department of Physics, and more recently the Department of Astronomy and Astrophysics, of the University of Chicago have been my intellectual home for the forty years between my arrival in Chicago in 1955 and my retirement in 1995. My colleagues have been congenial companions along the way, even though my work has been outside the mainstreams of physics and of astronomy.

I entered research at a time when there were many astonishing new discoveries, e.g., the recognition that the magnetic field of Earth can originate only in the convective motions of the liquid metal core, the million degree temperature of the solar corona, the enormous outward extension of the million degree temperature, the universal solar corpuscular radiation, etc. Then it should be emphasized that the rate of appearance of astonishing discoveries has increased since that time. There is, for instance, the remarkable structure of the magnetic carpet on the Sun, the remarkable internal convection and rotation of the Sun, the peculiar meridional laminations of the magnetic sunspots, the structure of the outer heliosphere, the neutrinos from the core of the Sun, the nonzero rest mass of neutrinos, the existence of dark matter in the universe, the recent inference of dark energy, and the inhomogeneities in the early universe that left their mark on the background thermal radiation, to mention some of the discoveries that spring immediately to mind. On a more sinister note, the Sun has revealed the mysterious property of shutting down its outward magnetic activity for half a century or more, as exemplified by the Maunder Minimum in the second half for the seventeenth century, and it has become clear that terrestrial climate tracks the general level of solar activity to an amazing degree. The present concern with global warming here at Earth, driven in part by the accumulation of anthropogenic greenhouse gases in the atmosphere, brings an urgency to understanding the diverse drivers of terrestrial climate if we are to react rationally and effectively to the impending environmental challenge.

It is curious that one of the editors of *Scientific American* a few years ago wrote a book proclaiming the end of physics. You may have seen it. The thesis was that in another twenty years, when string theory, or whatever, finally accomplishes the theory of fundamental particles, physics will be finished, with nothing more to do. The book was eagerly read by the intellectual public and was a best seller. The royalties to the author must have been of the order of a million dollars or more. The author is clearly one very smart cookie. Yet the basic idea of the book is naïve in the extreme, for at least two obvious reasons. One is the great number of scientific mysteries that have emerged in recent decades. I have just mentioned a few of those mysteries, and it should be appreciated that biology is beginning to pose challenging and interesting problems in physics as well. No limits can be foreseen.

The second reason is the assertion that, when the basic mathematical theory of fundamental particles is constructed, there will be nothing left to do in that field. Now if we look back three hundred years to Newton's formulation of mechanics and gravitation, we see that that it was only the beginning, not the end, of that subject. The Newtonian mechanics of planets, stars, and galaxies is an active field of investigation today, three hundred years later. Maxwell wrote down the complete electromagnetic equations around 1865. That marked the beginning, not the end, of electromagnetic theory, with experiment and application, rushing ahead today, nearly 140 years later. Einstein wrote out the relativistic equations of gravitation in 1916, and the field seems to be still gaining momentum nearly 90 years later. Schrödinger, Heisenberg, Dirac, et. al. wrote down the basic equations of quantum mechanics in the period 1926－1929. That was the beginning, not the end, of quantum mechanics and atomic physics. So if theoreticians can formulate fundamental particle theory by 2020, we can look forward to the beginning, not the end, of a new era in particle physics.

So if you find yourself curious about the world around you, physics offers exciting prospects for the foreseeable future. But there are other fascinating subjects as well. The most important thing that a young person can do is find out what he or she does best. What interests you is the best indicator of what you might do best. So explore your interests. A serious interest deserves a serious effort if you are to know whether it is right for you. For instance, in my teens I thought it would be exciting to be a writer of short stories. So I tried my hand at it, writing many short, and not so short, stories over a period of a year or so. It was soon evident that I was not very good at it. So I looked elsewhere to mathematics and physics. I soon discovered that I have no particular ability with the abstract concepts of pure mathematics. It is with the physical concepts of geometry and of push and pull, on which physics is based, that I do best, so I followed that lead into theoretical physics.

I hope that you receive as much encouragement along the way as I have. On the other hand, you will surely run into discouragement at times, perhaps from parents, or teachers, or your peers. So you have to decide for yourself ahead of time whether you are really serious in your pursuits. If you are not determined, maybe you should not pursue hard science. However, if you feel strongly about your subject, then do not falter because someone criticizes your decision. Similarly, if someone gets ahead of you with inspired research, do not be discouraged. In the long run progress serves only to boost you and everyone else working in the field. Then do not be surprised by destructive referees when you come to publish your scientific achievements. If a referee is critical, think about it carefully. The referee may have a point, and, if you really made an error, you want to be among the first to know. And remember the advice of Chandrasekhar, from his days as editor of The Astrophysical Journal, that even the worst referee's report contains valuable information, because it shows you the reaction of a typical reader to your written words. Perhaps you need to modify, re-emphasize, or clarify some point in your writing.

I should remark that science, whether biological or physical, can be pursued in many ways. To begin, you must become knowledgeable in your chosen subject. That should be a pleasure if you are genuinely interested in it. Then you have to decide whether extending that knowledge through experiments or through theory is your strong suit. Or are you better at expounding existing knowledge, something at which many researchers are not so proficient? Perhaps you shine brightest as a teacher at an elementary level or at an advanced level. Do not let peer pressure make any of these decisions for you. There is a lot more glory in good teaching than in mediocre research, in spite of what some may try to tell you.

Finally, in closing, it is obvious that the foregoing exposition has dealt with the scientific side of my life. I would like to emphasize that that is only half of my life. The other half is the personal side. When I was a student at MSU, I developed the habit of working twelve hours a day, seven days a week on my studies, with little exception. My father pointed out to me that too much of anything is not good. He suggested that I should learn to be the master, not the slave, of my chosen profession. I gradually took this to heart, and I comment now that no one should neglect developing the personal side of life. It is your personal relationship to others, concentrating on immediate family, that carries you along through the years and decades ahead. That means applying a substantial fraction of your time and effort day by day to those relationships. Your profession may be exhilarating, but it is not sacred, and the exhilaration will not go on forever. You will find that diverting time and energy to personal relationships helps you to free your mind for a fresh look at your work. So you need to develop a deliberate balanced life style if you are to enjoy a satisfying life. In particular, pass on your concept of curiosity and interest to your children that they may one day be successful in choosing their own life work.

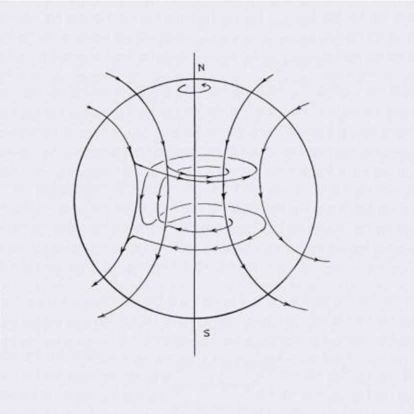

図 1　対流する液体鉄の核内部の南北(双極子)磁場の磁力線を示す図。核内部の回転が速くなると、磁力線が核の周りに伸び、北半球では東方向の磁場、南半球では西方向の磁場を形成する。内核は図から省略してある。

Fig.1　A sketch of the field lines of the north-south (dipole) magnetic field in the convecting liquid iron core of Earth. The more rapid rotation of the inner regions of the core stretches the field lines around the core to form the east pointing field in the northern hemisphere and the west pointing field in the southern hemisphere. The inner solid core is omitted from the sketch.

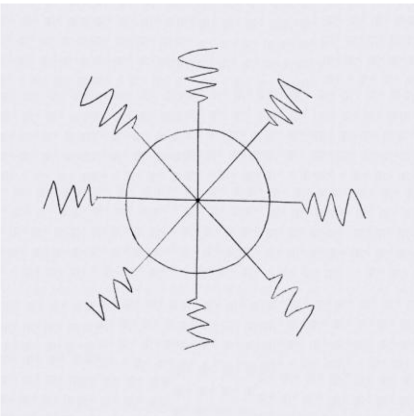

図 3　1956年2月23日のフレアから放出された太陽宇宙線の動きによって示される内部太陽系の磁場を示す図。円は地球の軌道を表し、あまり遠くない未確定距離では、磁場は軸方向に直交している。

Fig.3　A sketch of the magnetic field in the inner solar system suggested by the behavior of the solar cosmic rays from the flare or 23 February 1956. The circle represents the orbit of Earth, with the field becoming transverse to the radical direction in some unspecified way not far beyond.

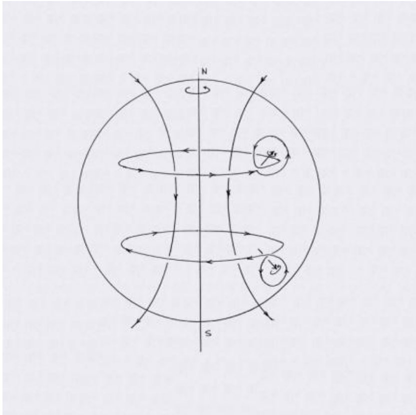

図 2　個々の回転する上昇対流セルにより東西磁場上に生じたループの上昇と回転を示す図。個々のループの回転方向は、南北(双極子)磁場の回転方向と同じ。

Fig.2　A sketch of the raised and rotated loops formed in the east-west field by individual rising cyclonic convective cells. Note that the sense of the circulation of each loop is the same as the circulation of the north-south (dipole) field.

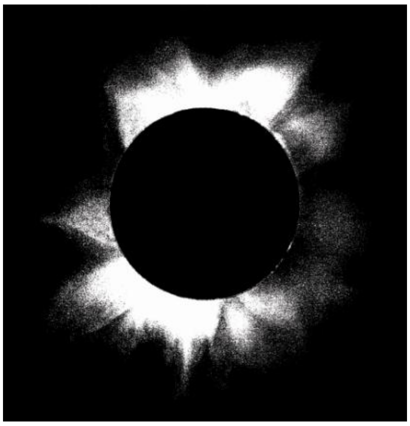

図 4　1980年2月16日の皆既日食に太陽のコロナから放出された可視光線の写真。高高度観測所とメンフィス大学チーム(High Altitude Observatory and Southwestern at Memphis College)が撮影。

Fig.4　A picture in visible light of the corona of the Sun during the total solar eclipse of 16 February 1980, taken by the High Altitude Observatory and Southwestern at Memphis College team.

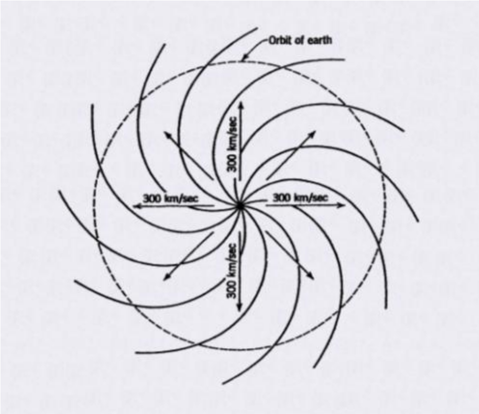

図 5　地磁気静穏日の理想化された均一太陽風（300km/秒）による太陽磁場の拡大から生じた静穏日の惑星間空間磁場の磁力線。

Fig.5　The lines of force of the quiet-day interplanetary magnetic field resulting from extension of the general solar field by an idealized uniform 300 km/sec quiet-day solar wind directions.

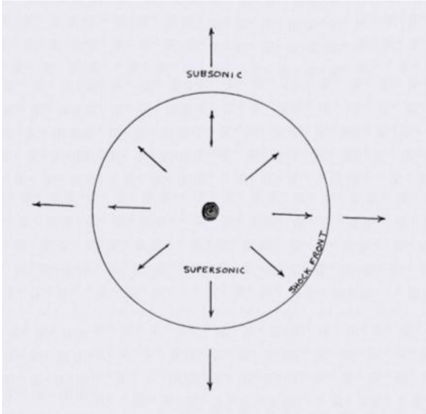

図 6　定常衝撃波まで伸びる超音速の太陽風を示す太陽圏の図（R=100AUの付近に位置すると推測される）。太陽風内のらせん状の磁場は、太陽に近い場所にのみ示した。

Fig.6　A sketch of the heliosphere showing the supersonic solar wind extending out of the standing shock, estimated to lie somewhere in the vicinity of R=100 AU. The spiral magnetic field in the solar wind is indicated only in the region close to the Sun.

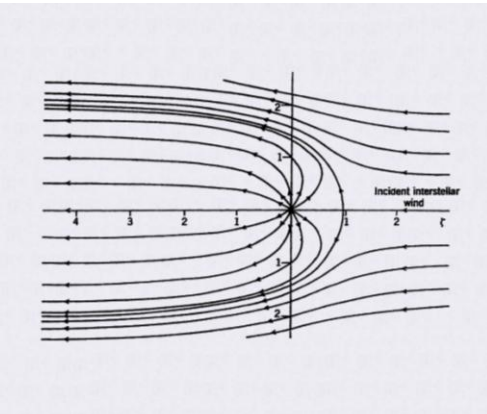

図 7　有意な磁場を伴わない亜音速星間風の存在下における衝撃相転移r= Rを超える亜音速、近非圧縮性の星間風の流体力学的流動。距離は、よどみ距離*L*を単位として測定する。

Fig.7　The streamlines of the subsonic, nearly incompressible, hydrodynamic flow of a stellar wind beyond the shock transition $r=R$ in the presence of a subsonic interstellar wind carrying no significant magnetic field. Distance is measured in units of the stagnation distance L.