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Scientific Reminiscences

Reminiscing my sixty year pursuit of the physics of the Sun and the Galaxy

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Abstract Reminiscing begins with childhood and passes on to student days through graduate school and the first real contact with research. Then early academic positions and stumbling efforts to pursue my ideas. The first significant progress came as a research associate with Prof. W. M. Elsasser at the University of Utah, beginning with an introduction to magnetohydrodynamics and the generation of the geomagnetic field through induction in the liquid metal core of Earth. A move to the University of Chicago to work with Prof. J. A. Simpson, on the implications of cosmic ray variations and interplanetary magnetic fields, led to the theory of coronal expansion and the solar wind and then to exploring the dynamical effects of cosmic rays on the galactic magnetic field. Spontaneous current sheets and intrinsic rapid reconnection in interlaced magnetic field line topologies were the next big project, leading up to retirement. Finally, it is a pleasure to recall my many associates, whose fresh thinking helped stimulate the daily research activities.

Key words: dynamo — magnetohydrodynamics — plasma

1 EARLY DAYS

For as far back in my life as I can remember, I have been fascinated by the physical world around me. Before I was old enough to go to school, we lived near a railroad yard, and the steam locomotive attracted my attention. The mechanical operation of the engine was on display, and, when explained to me, the mechanical principles were fascinating, and fun to watch in action. The four cycle internal combustion engine was the next revelation, although obscured by the internal location of the working parts. Then there was the strange world to be seen in the microscope. The "micro-motor boats" — rotifers — were my childish favorites among the zoo to be found in pond water.

When I attended school we were exposed to the numerical combinations and patterns that make up arithmetic. On the other hand, spelling was never fun, and my mediocre memory made it a doubly laborious task, not entirely completed to the present day. Story hour in the evening at home entertained with children's activities and thrilled with an old book *Earth for Sam*, providing an introduction to geology and the successive ages through which the world has passed on its long journey through time.

Mathematics gradually became more interesting as it emerged from the labor of multiplication tables into algebra, geometry, and calculus. Then my fourth year in high school (1943), seventy years ago, introduced the general subject of physics, providing the vision that led to my eventual career. Physics can, in principle, explain everything, and it is up to the reader to figure out how. What could be more fun than that?

A tuition scholarship lured me to Michigan State University, where I found a happy and productive relation with my physics professors. They were competent, friendly, and always patient enough to answer my dumb questions. It was satisfying to me to discover how physics is made up of so many interwoven threads. Each thread is a story in itself, and yet the threads fit flawlessly together in the interweaving, as if some exceedingly clever mathematician had deliberately designed the world ahead of time.

Caltech admitted me to graduate school, but without financial aid, because they had no calibration of students from Michigan State. Fortunately this turned out alright because Michigan State had a strange but ironclad rule that undergraduates shall not take graduate courses for credit. So, having finished the undergraduate physics courses and having enough credits to graduate, I dropped out of school at the end of Winter Quarter, 1948 and took a job as a technician in the physics laboratory at Chrysler Engineering in Detroit, thereby earning six months pay before climbing on a Greyhound bus in Detroit in September and heading for Los Angeles. Seventy two hours later I climbed off the bus in Los Angeles and caught the tram out to Pasadena and Caltech, and commenced my graduate studies.

The new crop of incoming graduate students each year was enrolled in Prof. Smythe's yearlong course in electricity and magnetism, using Smythe's textbook on that subject. That course was the filter through which incoming physics graduate students had to pass. The course consisted of assigned reading in the text as preparation for solving the assigned problems. It was overwhelming at first. I put in forty or fifty hours each week and then worked on my other courses in what time was left. At first I could barely keep up with the problem solving. I had visions of flunking out. However, after several weeks of effort the problems became easier, and by the midterm exam in the sixth week it was going smoothly and I aced the exam. There was daylight ahead and I was passing successfully through the physics filter.

By the end of Fall Quarter I had a good measure of my expenses, and a linear extrapolation showed that I could get through the Winter Quarter and pay my tuition for Spring Quarter with the last of my funds. So I needed a job. I had done well in Prof. W. A. Fowler's course in nuclear physics, and he ran a large laboratory, so I asked him if he needed another pair of hands. He was surprised that I had no remunerative position, and he phoned Dean Watson, who was in charge of the sophomore physics course, to say that I was available to fill the vacant teaching assistantship. I survived on a teaching assistantship for the remainder of my three years at Caltech. That is how it was with "Willie" Fowler. Things always went better when he was around.

My first year at Caltech I worked with Prof. H. P. Robertson, who suggested that I pursue the dynamics of the interstellar medium, interpreted at the time as made up of numerous self-gravitating clouds because the interstellar medium shows itself as separate discrete absorption lines in the spectra of distant stars. Robertson remarked to me once that, "When the math gets too hard, it is time to stop and think about the physics." That advice has come to my aid many times in later years (Fig. 1).

At the end of the year Robertson left for Washington, and Prof. Leverett Davis became my sponsor. I continued to pursue the dynamics of interstellar clouds, using the time dependent virial equation (Parker 1954) to describe the internal dynamics of spherically symmetric clouds. The idealized cloud can be described by a Hamiltonian, so I applied statistical mechanics to their distribution in radius, assuming the clouds preserve their identity during intercloud collisions and showing that in statistical equilibrium each cloud is either shrunk to a point or dispersed to infinity (Parker 1953a). Subsequently, Spitzer & Savedoff (1950) and Spitzer (1954) showed that the individual "clouds" represent the stable cool (10^2 K) state of thermal equilibrium in starlight interspersed with the hot (10^4 K) phase, so it became clear that the clouds are not self-gravitating and do not represent a Hamiltonian system. So much for my well intentioned labors.

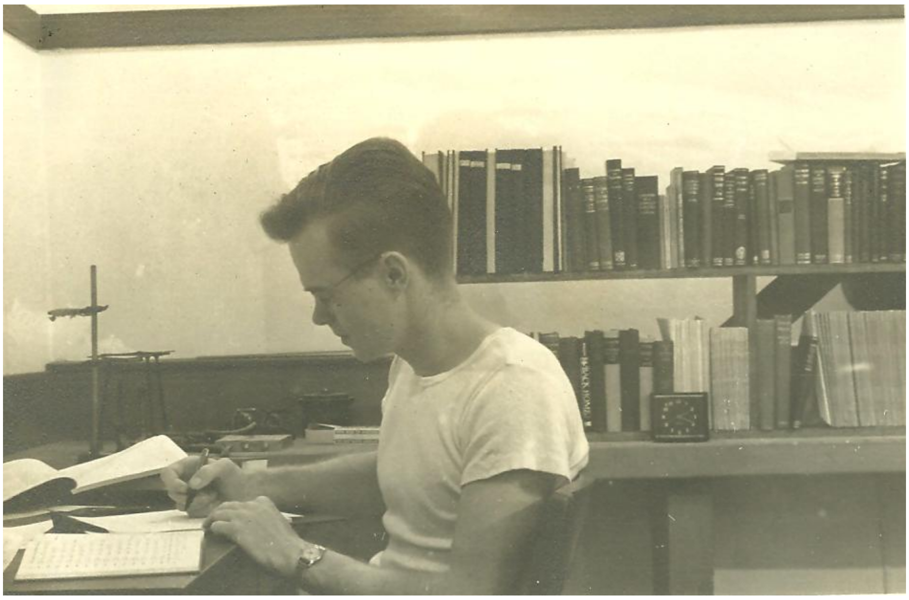

Fig. 1 The author as a student at Caltech, 1950.

One day I saw a photograph, taken with the 48 inch Schmidt at Palomar, showing the fine dust striations (10^{16} cm $\times 10^{18}$ cm) stretching across the Pleiades. I had worked out the very loose collisional coupling between dust grains, and the hydrogen atoms of the background gas, so I was puzzled as to how an interstellar wind could stretch out the dust over a light year and keep the dust confined to such narrow threads. One might have expected dispersal of the dust into a cloud. However, I recalled that the individual grains are presumed to be electrically charged by the photoelectric effect, emitting an electron or two and tying each grain to the galactic magnetic field of several microgauss. The calculated cyclotron radius of the individual grains turned out to be rather less than the observed 10^{16} cm thickness of the dust striations, thereby explaining the thin striations and confirming the existence of a local magnetic field of a few microgauss. I sent the paper to *The Astrophysical Journal*, and it was returned with the ultra-conservative editor's statement that, "Anyone who knows anything about astrophysics knows that magnetic fields have nothing to do with it." That was my first lesson in the sociology of science. More lessons were to follow in succeeding years.

I should mention that in 1951 the only known astrophysical magnetic fields were the geomagnetic field, first studied quantitatively by Gilbert (1600), and the kilogauss magnetic fields of sunspots, first detected and measured by Hale (1908a,b,c,d), and the microgauss magnetic field of the Galaxy, inferred by Fermi (1949, 1954) from the existence of cosmic rays. Hale (1913, 1935) thought he detected a field of 25 – 50 G in the polar regions of the Sun, but that turned out to be the noise in his observational data, as the polar field is more like 5 – 10 G.

The definitive paper, setting theory on the road to the origin of astrophysical fields, was written by Elsasser (1946a), in which he pointed out that there are no known atomic thermal effects, e.g. the thermoelectric effect, sufficient to produce the geomagnetic field. That left induction effects arising in the convection in the inner molten iron and nickel core as the only possible explanation for the origin. The secular drift of the magnetic inhomogeneities observed at the surface of Earth shows that the core is subject to convection and non-uniform rotation, the latter driven by the Coriolis force of the former. Elsasser showed that the principal induction effect is the generation of a strong azimuthal (toroidal) field as the non-uniform rotation shears the dipole (poloidal) magnetic field in the core. That left the problem of producing the poloidal field from the toroidal field, mindful of Cowling's proof (Cowling 1933) that a magnetic field with rotational symmetry cannot be regenerated by fluid motion. Elsasser (1946a,b, 1947, 1950, 1955, 1956); Bullard & Gellman (1954) and many others attacked the problem by expanding both the toroidal and poloidal fields in terms of spherical Bessel functions and tesseral harmonics. The difficulty was the lack of convergence of the expansions. Only years later did Gubbins (1973) in a heroic and sophisticated effort, with more than a hundred terms in each expansion, using a computer to assist the algebra, succeed in obtaining convergence. These efforts were all unknown to me at the time, but that would soon change.

2 GETTING STARTED

When I left Caltech with my PhD in 1951, jobs were scarce, and I accepted a position as instructor in the Department of Mathematics at the University of Utah with the verbal understanding that in two years I could expect promotion to assistant professor. There were five of us new and naive PhDs hired as instructors in 1951. In 1953 they fired four of us to keep the budget down. It was the best thing that could have happened to me. I had gotten acquainted with Prof. Walter Elsasser in the Department of Physics, and I asked him if he knew of any opportunities for employment elsewhere. The next day he came by and asked if I would be interested in a position as a one-third time assistant professor in the Physics Department and a two-thirds time research associate with him. I accepted and my career began to move forward.

When I went to work for Elsasser, he gave me a copy of the excellent review of magnetohydrodynamics (MHD) by Lundquist (1952) and a preliminary copy of some of his own writing (Elsasser 1954). I could not have had better teachers. I was greatly impressed by the basic simplicity of MHD, with the field graphically described by the field lines, which move exactly with the highly conducting fluid while the magnetic stresses push on the fluid. Thus MHD is a mechanical problem in which the Maxwell stress is pitted against the Reynolds stress. So the dynamics is formulated in terms of $\boldsymbol{B}$ and $\boldsymbol{v}$, and the Faraday induction equation reduces to the familiar form in which the time derivative of $\boldsymbol{B}$ is equal to $\nabla \times (\boldsymbol{v} \times \boldsymbol{B})$. The momentum equation for MHD is just the familiar hydrodynamic equation with the addition of the magnetic force $(\nabla \times \boldsymbol{B}) \times \boldsymbol{B}/4\pi$.

Recalling Robertson's advice, I ignored the formal expansions then in use and, instead, began drawing pictures of the deformation of the magnetic field by various patterns of fluid motion. I turned my attention to the convective cells in the liquid core, represented by cyclonic updrafts, rotating because of the Coriolis force of the rapid spin of Earth. Each updraft produces an upward bulge in the toroidal field component and rotates the bulge around into the meridional plane, thereby providing circulation of magnetic field around the meridional plane, i.e. adding to the poloidal field. The poloidal field is best described by a toroidal vector potential, so one ends up with two dynamo equations in the simple case of a 2D dynamo, either rectangular or with rotational symmetry. One equation is for the toroidal or azimuthal vector potential produced by the cyclonic convective cells from the azimuthal or toroidal magnetic field, and the other is for the azimuthal field produced by the non-uniform rotational shearing of the meridional or poloidal field (Parker 1955b). For a given nonuniform rotation and cyclonic convective cells, the equations are linear and can be treated by analytic methods. The solutions of the equations can be thought of as waves, providing such phenomena as the periodic field of the Sun, or, if the propagation of the waves is blocked by the boundary, there are stationary solutions, such as the geomagnetic field (Parker 1955b, 1957a).

This simple combination of nonuniform rotation and cyclonic convective cells, now called the $\alpha\omega$-dynamo, appears to be the principal source of magnetic field regeneration in most astrophysical objects. Subsequent theoretical research has turned up a fascinating array of additional dynamo effects, with the general conclusion that almost any fluid motion lacking basic symmetries has at least some small dynamo effect (cf. Steenbeck et al. 1966; Steenbeck & Krause 1966; Braginskii 1964a,b,c,d; Rädler 1968, 1969a,b; Roberts 1972; Parker 1979, 1982a,b; Krause & Raedler 1980; Childress & Gilbert 1995 and refs. therein).

I had the pleasure of working with Elsasser for two years. Elsasser was a world class physicist from whom I learned a lot. However, there was little opportunity for advancement at Utah at that time, and Elsasser advised me to look elsewhere. Not long after that, in early 1955, I received a phone call from Prof. J. A. Simpson at the University of Chicago. He was studying the time variations of the galactic cosmic rays as a means for probing conditions in interplanetary space. He was interested in a research associate to pursue the theoretical implications of the observed cosmic ray variations. The variations were clearly associated with solar activity, but notions of space conditions in those days ranged from powerful electric fields in a hard vacuum to a "vacuum" filled with the solar corpuscular radiation (SCR) that is presumed to be responsible for the anti-solar pointing of comet tails. I accepted the offer, and my wife, Niesje, and I moved to Chicago in June of 1955, where we have lived ever since, retiring from the University of Chicago in 1995 and 1998, respectively.

I should remark that when I was a member of the Math Department at Utah, I spent some time working on the theory of hydrodynamic turbulence (Parker 1953b), and I had some correspondence with Prof. S. Chandrasekhar at the University of Chicago on that subject. My contributions to turbulence theory were negligible, but it led to Chandrasekhar mentioning my name to Simpson as a possible theoretician for the job with Simpson. I owe much to the support of Simpson and Chandrasekhar over my forty years at Chicago, and I feel greatly honored to have been appointed to a chair bearing Chandrasekhar's name.

3 PRINCIPLES

Over the course of my career I have worked on a number of problems, but rather than reminiscing over a laundry list, it seems more interesting to describe some of the general principles. Broadly speaking, the subject is *astrophysical plasma dynamics*, differing from *laboratory plasma dynamics* in the large scale of the astrophysical phenomena to be treated. Thus, apart from the microstructure of shock fronts and the intense current sheets involved in rapid reconnection, the physics lies in hydrodynamics and MHD. Each new discovery made by solar and astrophysical observations provides a new scientific challenge to hydrodynamics and MHD, and sometimes points the way to new physical effects within the strictures of Newton, Maxwell, and Lorentz. There are two facets to the research. We hope to build up an understanding of the diverse observed phenomena, and, equally important, we discover new unforeseen properties of the fundamental physical equations – things to be taught in physics classes. We cannot help but note a natural trend in contemporary computer modeling to be preoccupied with technical details of the modeling, while sometimes forgetting the fundamental importance of the underlying physical principles.

Before getting into specific cases, reminiscing can take us into the darker corners of the theory of plasmas, recognizing that in some quarters the subject of astrophysical plasmas is still bedeviled by fundamental misconceptions that contradict the equations of Newton and Maxwell, even though the correct theoretical concepts are simple and available. The misconceptions have a superficial plausibility, are fondly held, and are not mentioned in the standard physics textbooks nor have they inflicted significant injury to main stream astrophysics. Magnetospheric physics has been the principal victim. But, curiously, the misconceptions are not widely recognized for what they are, and are often accepted in "scientific" discussions and presentations on astrophysical topics. The reader has surely encountered them in the past and can look forward to exposure to misconceptions in the future. Understanding the errors in the popular misconceptions provides insight into the proper treatment of large-scale plasmas from Newton and Maxwell (Parker 2007).

To begin, then, it is widely believed that the large-scale bulk motion within a body of collisionless gas or plasma is not described by the Newtonian equations of hydrodynamics. In the absence of interparticle collisions the pressure may be anisotropic, of course, represented by a tensor p_{ij} rather than a single scalar p. But whether interparticle collisions happen or not, the bulk flow conserves particles, momentum, and energy, and when those three conservation conditions are written down, they provide the equations of hydrodynamics, with the familiar gradient of p_{ij}, compressibility, etc. Most textbooks derive these hydrodynamic equations by computing the zero, first, and second velocity moments of the collisionless Boltzmann equation, but the simple idea of flux conservation of particles, momentum, and energy can be used directly (Parker 2007). It is important to understand that the pressure p_{ij} represents the momentum flux in the ith direction transported by the thermal motions in the jth direction and has nothing to do with collisions.

The same misunderstanding applies to MHD, with additional concerns about the electric field $\boldsymbol{E}$ and current $\boldsymbol{j}$. It is forgotten that the equations of Newton, Maxwell, and Lorentz are self-consistent. A complete solution in terms of $\boldsymbol{B}$ and $\boldsymbol{v}$ automatically implies the proper treatment of $\boldsymbol{E}$ and $\boldsymbol{j}$.

To establish the MHD equation, consider a collisionless plasma in a magnetic field varying over a scale that is large compared to the cyclotron radius R of the ions and electrons. Define the electric drift velocity $\boldsymbol{u}_D = c\boldsymbol{E} \times \boldsymbol{B}/B^2$, from which it follows that $\boldsymbol{E} = -\boldsymbol{u}_D \times \boldsymbol{B}/c$. The electric field $\boldsymbol{E}' = \boldsymbol{E} + \boldsymbol{u}_D \times \boldsymbol{B}/c$ in the reference frame moving with velocity $\boldsymbol{u}_D$ is zero. Thus the ions and electrons move in circles in that reference frame. The Poynting vector $\boldsymbol{P} = c\boldsymbol{E} \times \boldsymbol{B}/4\pi$ shows that the electromagnetic energy flux is $\boldsymbol{u}_D B^2/4\pi$, where $B^2/4\pi$ represents the magnetic enthalpy density. Thus the magnetic field moves exactly with the velocity $\boldsymbol{u}_D$, and the induction equation is the familiar $\partial\boldsymbol{B}/\partial t = \nabla \times (\boldsymbol{u}_D \times \boldsymbol{B})$. The motion of the individual ions and electrons is readily computed using the guiding center approximation. Then sum over those computed particle motions to obtain the electric current $\boldsymbol{j}$, and substitute the result into Ampere's law, from which we find that $\boldsymbol{u}_D$ satisfies the familiar Newtonian hydrodynamic momentum equation (Parker 1957b) with an extra term representing the centrifugal force of the thermal motions along curved field lines, particularly evident when the particle pressure p_s parallel to the magnetic field exceeds the particle pressure p_n perpendicular to the field. Next, sum over the motion of the particles to find the bulk plasma mass velocity $\boldsymbol{v}$. The result is that $\boldsymbol{v} - \boldsymbol{u}_D \neq 0$, representing the Hall effect. Note then that the Hall effect is small to order $O(R/\lambda)$ compared to $\boldsymbol{u}_D$. That is to say, the magnetic field drifts only very slowly $O(R/\lambda)$ relative to the plasma. So in the large scale astrophysical limit we have the familiar hydrodynamics and MHD, and we can include the Hall effect where it might be significant.

Strong thermal anisotropies, with $p_s - p_n \approx B^2/8\pi$, can have interesting effects, but strong anisotropies are unstable and do not exist for long, assuming that somehow strong anisotropy might be created in a transient phenomenon, e.g. a shock front. As a practical matter, it is usually adequate to assume thermal isotropy as a first approximation. In summary, if we use Newton's equations to compute the individual particle motions and describe the bulk motion by the hydrodynamic momentum equation, then Ampere's law is automatically satisfied. We cannot get away from hydrodynamics and MHD in the large-scale collisionless plasma, arising from the fact that the bulk motion $\boldsymbol{v}$ of the plasma is close to $\boldsymbol{u}_D$ in which moving reference frame there is no electric field. Now it is sometimes suggested that $\boldsymbol{E}$ is the force driving the plasma at the electric drift velocity $\boldsymbol{u}_D$. But with $\boldsymbol{E} = -\boldsymbol{u}_D \times \boldsymbol{B}/c$ it follows that the electric stress $E^2/8\pi$ is small to second order in $|\boldsymbol{u}_D/c|$ compared to the magnetic stress $B^2/8\pi$, and unable to drive anything of significance. It is the fluid motion $\boldsymbol{u}_D$ or $\boldsymbol{v}$ that determines $\boldsymbol{E}$ and deforms $\boldsymbol{B}$, and not vice versa.

Then there is the notion that $\boldsymbol{j}$ causes $\boldsymbol{B}$ and is, therefore, the more fundamental field variable. But Maxwell's equations cannot be written in useful form in terms of $\boldsymbol{j}$ rather than $\boldsymbol{B}$. One has to use the Biot-Savart integral to eliminate $\boldsymbol{B}$, thereby converting the partial differential equations into intractable global integro-differential equations. So, lacking a tractable field equation, it is declared that $\boldsymbol{E}$, induced by $\boldsymbol{v}$, drives $\boldsymbol{j}$, which in turn produces $\boldsymbol{B}$. But it is the electric field $\boldsymbol{E}' = \boldsymbol{E} + \boldsymbol{u}_D \times \boldsymbol{B}/c$ in the moving frame of the plasma that drives the current $\boldsymbol{j}$, so in the absence of resistivity, $\boldsymbol{E}'$ is zero and there is no significant driving of $\boldsymbol{j}$. The magnetic field varies because it is mechanically deformed as it is carried along with the bulk fluid motion $\boldsymbol{u}_D$. The energy of the magnetic field changes because of the work done on it by $\boldsymbol{v}$.

The same objection applies to the popular notion of the electric circuit analog (Alfvén & Carlqvist 1967) for an MHD system. The electric circuit resides in the laboratory frame and experiences the laboratory electric field $\boldsymbol{E}$, while the current carried in the plasma experiences the vanishing electric field $\boldsymbol{E}'$ in the reference frame of the plasma. What is more, the connectivity, or topology, of the electric circuit analog is fixed while Ampere's law may provide varying connectivity of the current as the field is carried in the swirling plasma or blocked by a local region of high resistivity (Parker 2007). The electric circuit analog is correct only in trivial circumstances.

4 MHD APPLICATIONS

Armed with hydrodynamics and MHD one quickly encounters a number of elementary effects, with magnetic buoyancy among the simplest. The magnetic fields that continually come up through the visible surface of the Sun are propelled at least in part by their own buoyancy. A flux bundle with field strength B and gas pressure $p_{\rm in}$ is confined by the external ambient gas pressure $p_{\rm ex}$ equal to $p_{\rm in} + B^2/8\pi$. That is to say, the internal gas pressure is less than the external gas pressure and hence is less dense than the external gas, so the flux bundle represents a bubble (Parker 1955a).

In another direction, imagine that the bipolar magnetic fields of two active regions on the Sun are pressed together where one field is pointing up and the other field down. The magnetic pressure squeezes plasma out from between the two fields and they move progressively closer together. The field gradient and associated current density between the two increase without bound with the passage of time, until sooner or later the current becomes so intensely concentrated that even the small electrical resistivity of the coronal gas causes severe dissipation. That is to say, the two opposite fields rapidly eat away at each other, reconnecting the field lines across the current sheet. That is the phenomenon generally referred to as rapid reconnection, first recognized by Sweet (1958) and Parker (1957c). The opposing fields need not be oppositely oriented, so long as they are not precisely parallel. The current sheet may become so thin that plasma kinetics comes into the dynamics of the current sheet (Drake et al. 1994), and the simple MHD picture spelled out here (Parker 1957c) is only to remind the reader of the basic large-scale magnetic dynamics that provides the increasing field gradient. There is an active plasma physics community (cf Petschek 1964; Drake et al. 1994, 2005; Biskamp et al. 1994; Biskamp et al. 1997; and refs. therein) that has succeeded in developing the very complex theoretical dynamical geometry and structure of the current sheet, and its relation to the surrounding fields, showing finally that the reconnection often proceeds very much more rapidly than predicted by the purely resistive dissipation of MHD. So rapid, in fact, that rapid reconnection evidently plays a central role in the activity of the geomagnetic tail and in the flaring and explosive expulsion of coronal mass ejections from the Sun as well the magnetic dissipation that maintains the solar X-ray corona (Parker 1988).

To see how rapid reconnection generally arises in solar magnetic fields, consider the equilibrium of the magnetic fields forming re-entrant loops extending out from the surface of the Sun. The random convective shuffling of the photospheric footpoints provides an interlacing field line topology within each loop. For purposes of discussion the background plasma is assumed to have infinite electrical conductivity and uniform pressure. Equilibrium throughout the interlacing fields is described by the familiar force-free field equation, $\nabla \times \boldsymbol{B} = \alpha \boldsymbol{B}$. This simple field equation is like no other with which we are familiar, possessing two families of complex characteristics while the field lines represent a family of real characteristics. Thus most solutions to the equation have surfaces of discontinuity – current sheets – extending along the field lines where rapid reconnection occurs. Such fields play a fundamental role in coronal heating (Parker 1988, 1994, 2012a,b; Rappazzo & Parker 2013).

The essential point is that the field line topology controls the nature of the equilibrium solutions, which may be seen from the fact that the divergence of the equilibrium equation is $\boldsymbol{B} \cdot \nabla \alpha = 0$. This requires the torsion coefficient α to be constant along each field line no matter how the field lines interlace. An arbitrary field line interlacing requires a torsion that varies along the field lines if the field is to be continuous everywhere. So if the torsion is constrained to being constant as the field lines writhe and twist their way along, there will be surfaces of discontinuity where the torsion and the writhing of neighboring flux bundles do not match. To quantify this concept, greatly stretch the field along the mean direction of the interlaced configuration. This operation preserves the field line topology, of course, and stretches out the interlacing pattern so that the transverse field components become very small compared to the mean field along the loop. In this limit the equilibrium field equation reduces to the 2D vorticity equation (van Ballegooijen 1985). The vorticity solutions have very special topologies – a set of measure zero compared to the infinity of all possible interlacing topologies (Parker 1994). The magnetic field is anchored at both ends, as with the magnetic loops extending out from the Sun, so we know they have an equilibrium, but their arbitrary topology does not permit them to be a member of the vorticity solutions. Hence they are to be found among the much larger class of equilibrium solutions containing surfaces of discontinuity.

It follows that the relaxation of an interlaced field to equilibrium is a matter of the magnetic stresses pushing the plasma and magnetic field, thereby steepening the field gradients without limit, and asymptotically approaching the final discontinuous state of equilibrium. Given the presence of a small but nonvanishing resistive dissipation, the relaxation cannot proceed to the singular state, of course, but is halted where the resistive dissipation is in quasi-steady balance with the steepening of the field gradients. That is to say, there is rapid reconnection at the site of each incipient discontinuity. Thus rapid reconnection is a universal phenomenon in magnetic fields with convective interlacing of their field lines. We have, then, the interesting situation in which the magnetic stress tensor automatically creates sites of rapid reconnection for almost all field line topologies that are found in nature. One would never have guessed that the innocent looking stress tensor $M_{ij} = -\delta_{ij}B^2/8\pi + B_iB_j/4\pi$ possesses this singular property.

5 SOLAR WIND

As a final example, consider the trans-sonic solutions of the hydrodynamic equation for the radial expansion of the solar corona. The story began more than a century ago with the solar corpuscular radiation (SCR) from the Sun, strongly emitted at the time of a big flare and with a Sun-Earth transit time of a day or two, implying velocities of the order of 10^3 km s^{-1}. The SCR is electrically neutral and, hence, made up of equal numbers of protons and electrons, i.e. today we would recognize it as a plasma. The kinetic energy of a proton at 10^3 km s^{-1} is 5 keV, and how they are accelerated at the Sun was not immediately obvious. Now the SCR comes out through the corona, and the outstanding characteristic of the corona is its million degree temperature and strong gravitational binding to the Sun. The million degree temperature provides an enormous thermal conductivity, with electron thermal velocities of the order of 7000 km s^{-1}. Chapman (1957) showed that the temperature in such a corona falls off with radial distance only as $1/r^{2/7}$, extending far out into space. The barometric law extends the corona beyond the orbit of Earth, and the coronal temperature at the orbit of Earth is about 10^5 K. Chapman remarked that Earth orbits inside the corona of the Sun.

At about that time Prof. L. Biermann visited Prof. Simpson's laboratory, and I had a chance to talk with Biermann about the point that comet tails point away from the Sun because of the impact of the SCR. I was very impressed by the fact that comet tails never fail to point in the anti-solar direction (Biermann 1951), indicating that SCR never ceases anywhere around the Sun, regardless of the level of activity of the Sun. So the origin of SCR must be a simple ongoing effect. However, there was a serious problem. The electrically neutral SCR plasma cannot pass freely through Chapman's static coronal plasma. Rapid passage excites the two-stream instability, quickly reducing the relative velocity of the two plasmas to small values. Yet it is a fact that the corona near the Sun is more or less in static equilibrium whereas far from the Sun the SCR is streaming freely at 10^3 km s^{-1}. The only possibility is that somehow Chapman's static corona near the Sun becomes Biermann's high speed SCR far from the Sun. That suggested hydrodynamics.

Whether there are interparticle collisions or not, the simple hydrodynamic equation for the radial velocity $\boldsymbol{v}$ is easily written down to demonstrate the basic principles. To explore the situation assume thermal isotropy with a pressure $p = 2NkT$ for ionized hydrogen at temperature T, number density N, hydrogen mass M, and solar mass $M_\odot$. The momentum equation for a stationary radial flow is

$$NMv\frac{dv}{dr} = -\frac{d}{dr}(2NkT) - \frac{GM_\odot}{r^2}NM.$$

Combined with the equation $Nvr^2 = \text{constant}$ for conservation of particles, and introducing the characteristic thermal velocity $u = (2kT/M)^{1/2}$, the result is

$$\frac{dv}{dr}\left(v - \frac{u^2}{v}\right) = \frac{2u^2}{r} - \frac{GM_\odot}{r^2}$$

for uniform T. It is evident that if dv/dr is bounded at the sonic point, where $v = u$, then the sonic point lies at $r = a = GM_\odot/2u^2$. Integration provides the trans-sonic solution

$$\frac{v^2}{u^2} - \ln\frac{v^2}{u^2} = 4\ln\frac{r}{a} - \frac{2GM_\odot}{au^2}\left(1 - \frac{a}{r}\right) + 1$$

with the constant of integration chosen so that $v = u$ at $r = a$. Inside the sonic point, where $r < a$, conservation of particles causes the stationary flow to accelerate outward because of the very rapid outward barometric decline of N in the strong gravitational field, yielding

$$\frac{v^2}{u^2} \cong \left(\frac{a}{r}\right)^4 \exp\left[\frac{2GM_\odot}{u^2a}\left(1 - \frac{a}{r}\right) - 1\right].$$

Beyond the sonic point, where $r > a$, the gravitational binding becomes weak, and the gas expands into a vacuum. In the simple isothermal model used here the expansion velocity increases with increasing r, slowly but without limit, with

$$\frac{v^2}{u^2} > 4\ln\frac{r}{a} + \frac{2GM_\odot}{u^2a}\left(\frac{a}{r} - 1\right) + 1.$$

Thus the extended coronal temperature T causes the strongly bound corona to flow outward from the Sun to form the supersonic solar wind. The wind velocity at the orbit of Earth would be about 400 km s^{-1} for $T = 10^6$ K and close to 700 km s^{-1} for $T = 2 \times 10^6$ K, demonstrating the general principle of the hydrodynamic stellar wind phenomenon (Parker 1958a). It was then obvious that the solar wind was filled with magnetic flux drawn out from the Sun into an Archimedean spiral.

I was pleased with the outcome of the theory, of course, and I wrote it up for publication in *The Astrophysical Journal*, of which, fortunately, Prof. Chandrasekhar was editor at that time. The referee's report came back in a few months with the suggestion that the author should spend some time in the library to familiarize himself with the SCR before attempting to write a scientific paper on the subject. There was no specific criticism of the mathematics or of the interpretation of the observations. So Chandra sent the paper to a second "eminent" referee, with essentially the same result. I emphasized to Chandra that these two referees, for all their hostility, could find no scientific error. Then one day Chandra came to my office and said, "Now see here, Parker, do you really want to publish this paper? I have sent it to two eminent referees, and they both say the paper is wrong." I replied that the referees had no scientific criticism. He thought for a moment and then said, "Alright, I will publish it." Some years later he told me that he had been skeptical about the paper, but without objective criticism, he felt obliged to publish it. To my regret I failed to save the two referee reports to be framed and displayed on the wall of my office. This was another lesson in the sociology of science, demonstrating the importance of editors who do not fear to contradict eminent referees. The careers of many young scientists have been seriously injured by negative referees.

Hardly anyone believed the trans-sonic expansion of the solar corona. So I had the field to myself for about four years, elaborating the analytic theory of the expanding corona, producing two hydrodynamic models of the heliosphere depending on the existence or absence of an interstellar wind. I went on to constructing simple spherically symmetric blast wave solutions before the concept of the coronal mass ejection was defined by observation, and then speculating on stellar winds in general, all summarized in Parker (1963, 1965a,b) and refs. therein. As is well known, the first direct detection and measurement of the solar wind, by Shklovskii et al. (1960); Gringauz et al. (1961) and Bridge et al. (1961), and Bridge et al. (1961) were largely ignored in the general skepticism. The solar wind was then unambiguously logged by the JPL instrument carried on the Mariner II spacecraft on its six month voyage to Venus (Neugebauer & Snyder 1962, 1966a,b; Snyder & Neugebauer 1964; Snyder et al. 1963) and the spiral interplanetary magnetic field was first reliably logged by the IMP Spacecraft (Ness et al. 1964). Later authors investigated the theoretical behavior of shock waves, sector structures, high speed streams, and the very complex dynamics of the heliosphere.(cf. Parker 1965a; Hundhausen 1972; Phillips et al. 1995; Burlaga 1995; Parker 1969, 2002). A whole new arena of complex interactions has now been recognized where the solar wind meets the local interstellar wind (cf. Zank 1999; Zank et al. 2013), with the Voyager and IBEX space missions leading the way.

An important theoretical calculation was subsequently carried out by Lemaire & Scherer (1971) when they treated coronal expansion as the free evaporation of particles from a coronal exosphere with a temperature T, providing a supersonic wind of about 300 km s^{-1} for 10^6 K, typical of a quiet day wind. This demonstrated the error in the earlier evaporative calculation by Chamberlain (1960) providing about 50 km s^{-1} – the popular solar breeze concept before the supersonic wind was confirmed by observation. The calculation reminds us once again that the large-scale bulk motions of collision-free and collision-dominated plasmas obey the same general hydrodynamic equations. Each approach provides its own insights into the dynamics of the solar wind phenomenon.

6 OTHER INTERESTS

My interests subsequently turned to the gaseous disk of the Galaxy and the vigorous dynamical effects of the galactic cosmic rays (Parker 1965b, 1966) on which I. Lerche and I wrote several papers together (Lerche & Parker 1966, 1967, 1968) including the creation of the galactic halo. J. R. Jokipii and I delved into cosmic ray acceleration and cosmic ray transport in stochastic magnetic fields (Jokipii & Parker 1969a,b, 1970, 1976; Parker & Jokipii 2000), a central subject that Jokipii has subsequently developed extensively. Some years later Jokipii and I were briefly involved in evaluating a laboratory scheme for using magnetic fields to shield astronauts from cosmic rays. M. Turner, T. Bogdan, and I worked up a detailed account of how the existence of the galactic magnetic field establishes the absence of magnetic monopoles (Turner et al. 1982, 1984; Parker 1970, 1987). A. J. Dessler and I pointed out that the geomagnetic storm is basically an MHD phenomenon, with the decline of the horizontal component of the field caused by inflation of the magnetosphere with energetic particles, and the relaxation of those energetic particles largely through charge exchange with the ambient neutral atmosphere (Dessler & Parker 1959, 1968). Our pursuit of common interests dates back to student days. In the limited space available here I can only list other collaborators and associates over the years: D. A. Tidman (Parker & Tidman 1958; Parker 1958b), T. E. Abdelatif (Abdelatif & Thomas 1987; Abdelatif 1987), C. M. Ko (Ko & Parker 1989), A. Thayagaraja (Parker & Thyagaraja 1999), K. MacGregor, and Bernard Roberts who is well known for his contributions to wave propagation, among other things, and recently retired from the faculty at St. Andrews University. I learned something new from each of these many associations.

In closing, note that reminiscing provides a window only into the past, whereas PhD students provide a path into the future. There were fourteen who worked directly under my supervision. That is to say, I was available for discussion whenever they felt so moved, but they were responsible for their own research. Their PhD thesis was theirs. They have proceeded into diverse and successful ca-

Fig. 2 Sixtieth birthday celebration, University of Chicago, 1987. (*left to right*) Eugene Levy, Tom Bogdan, the author, Boon Chye Low and Kanaris Tsiganos.

reers, and it is with pleasure that I recall our associations. In chronological order they were William A. Whitaker 1963; Jack P. Friedman 1965; Edward Walbridge 1967; George Valley 1971; Eugene H. Levy 1971; Boon-Chye Low 1972; Lloyd Walker 1973; Guang Yu 1974; Kanaris Tsinganos 1981; Thomas J. Bogdan 1984; Arnab Rai Choudhuri 1985; William Collins 1988; Steve Arend, 1992; Nicholas Boruta 1995. I am pleased to have maintained acquaintance with four of them over the succeeding years. B. C. Low is a senior scientist at the High Altitude Observatory, and we have pursued common problems in solar physics and MHD (cf. Janse et al. 2010). Kanaris Tsinganos is a professor in the Department of Physics, University of Athens and a leading scientist in the Greek Space Agency (Parker & Tsinganos 1979). Thomas Bogdan has turned to science administration with positions at the National Science Foundation, the National Oceanic and Atmospheric Administration, and now President of the University Corporation for Atmospheric Research. Arnab Rai Choudhuri is a Professor of Physics at the Indian Institute of Science in Bangalore with ongoing interest in the solar dynamo, producing monographs on MHD and solar physics. Figure 2 shows my sixtieth birthday celebration at the University of Chicago, 1987.

It should be noted that Eugene Levy, after some work on dynamos, went into scientific administration and later became Provost at Rice University. Then it should be noted that William Collins went into cloud physics when he left Chicago and so distinguished himself that he was lead author on the fourth assessment report on climate change by the IPCC, for which the IPCC was recipient of the 2007 Nobel Peace Prize. Collins is also the lead author on the fifth panel report.

The collective reminiscences of this group of scientists will one day provide a fascinating overview of their intellectual contributions and experiences.

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