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ASTRONOMER BY ACCIDENT

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This is intended as a partial autobiography covering my astronomical work up to about 1957. Most research workers have only a hazy idea about what happened in their research field more than ten years before they entered it, and they may be interested in what happened in the remoter past.

My father was a highly intelligent person. His own father died young, and he had to start work at age 14 as an office boy at five shillings a week. He worked his way up, studying for examinations in the evening: After some years as a telegraph operator, he at age 30 won his way into the Post Office engineering staff, where he finally reached the grade of executive engineer. He may have been partly responsible for my interest in magnetism; he acquired a large horseshoe magnet from the breakup of a dynamo, and my three brothers and I enjoyed playing with it.

My mother (who was trained as a teacher) led a more sheltered life. Both my parents were loyal members of Baptist churches, and in this their four boys followed them, the eldest dying as a missionary in India in 1964. With this background, we four naturally adopted what is often called the Puritan work ethic.

The secondary school that we boys attended was called a grammar school. It was founded by a former Lord Mayor of London in 1527, but it had only a handful of pupils until taken over by Essex County under an Education Act of 1902. Its development was checked during the First World War. In 1917, when I sat for a scholarship (free place), it had fewer than 200 pupils (boys). Of the annual intake of 30–40, half were fee payers, half scholarship holders, paid for by the county. My oldest brother and I won scholarships, but my other two brothers were fee payers. After the written part of my scholarship exam, my brothers and I all fell ill with diphtheria, and I was lucky in that I was excused from the second part of the exam (interview and oral). My mother successfully nursed us all through the illness, but the strain, complicated by rationing, left her health permanently impaired.

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The Grammar School was strong on mathematics. This suited me, because I was fond of the subject, and at an early age I knew the powers of 2 up to 8192, with other similar bits of information; however, I was a slow and untidy writer, losing no opportunity of making blots and smudges. We had laboratories for physics and chemistry, quite good for schools at that date. I was fascinated by chemistry but too clumsy ever to have made a success of experimental work.

In 1920 the school had only a handful of sixth formers. The parents of scholarship boys had to sign a declaration that they would keep their boys at school until at least the age of 15, and in fact many boys left at 15. However, by 1922 numbers had grown enough for a new headmaster to think of dividing the sixth form into Arts and Science sets. On the advice of a civil servant who was a member of our church, I was placed on the Arts side; he thought the Arts course was the less narrow of the two. This meant that in my last two years at school I did no chemistry and only a little physics. However, I offended against the concept of a broad course; since mathematics was by far my best subject, I was permitted to count it as a double subject and so take French only as a nonexamination subject. During those last two years, the possibility of my taking a degree had to be considered. In those days the only recognized career for mathematicians was in teaching. I would need, in order to pay for a university course, substantial grants to supplement such a modest contribution as I might expect from my parents. In December 1923 I sat for a mathematics scholarship at Cambridge and was unsuccessful. I was told that I should have been awarded an Exhibition had I entered for one, but I had not done so, thinking the emoluments too small. Most of the scholarships to our older universities were awarded in December, and my parents had to face up to the sacrifices that they must make if I had to stay on at school for an extra year.

There was, however, a group of Oxford colleges holding its scholarship examinations the following March, and I resolved to work all out to win one of their scholarships and so relieve my parents of this extra burden. It worked; I did win an open scholarship in mathematics to Brasenose College. With this evidence before them, other bodies were ready to add extra grants; grants roughly equal in value to my scholarship were made by both Essex County and the UK Education Department, the latter grant being one made to prospective teachers. I have to confess that I regarded my success over the scholarship as in part an answer to intensive prayer before and during the examination, forgetting St. Peter's saying that "God is no respecter of persons."

The Oxford mathematics course on which I embarked in October 1924 was old-fashioned and run inadequately by tutors who had other responsibilities too. My own tutor (I. O. Griffith), though a mathematician, took a substantial share in the running of the Clarendon (Physics) Laboratory. He was satisfied to give me general guidance and otherwise to leave me to work through books (an arrangement that suited me). In 1926 he passed over to me a volume containing Schrödinger's fundamental paper. I had neither enough German nor enough Hamiltonian mechanics to understand the paper, but it intrigued me. However, the closest that my undergraduate study got to theoretical physics was through an optional course on electricity and magnetism, based largely on the book by Jeans.

After getting a First Class in my degree Examination, I was awarded a postgraduate scholarship for three years at Brasenose. Since I had been receiving a teacher training grant, I contacted the head of the Oxford teacher training department, explaining that of the three years, I intended to spend two on research and one on his teaching diploma; which should I take first? His advice was to take the diploma first, which I did (1927–28). The diploma did little for my subsequent career, but Mr. Griffith said that he thought that taking it was good for me in widening my outlook after a narrow mathematics course.

It was good for me also in another respect. When I came to discuss the question of research with Mr. Griffith, he told me that a new Applied Mathematics chair had just been instituted, and that the first incumbent, Prof. E. A. Milne, would take up duties in the next January. I could, perhaps, be his first research student at Oxford. For the term before Milne arrived, he (Griffith) could be my official supervisor, while I actually worked on a reading list supplied by Milne. So I wrote to Milne, offering myself to work under him, if possible on atomic theory, which I was trying to read up. He replied that he was ready to take me on, but his atomic theory was rusty; if I came to work with him, it must be on an astronomical subject. I of course accepted. Thus, the year spent on the teaching diploma was the reason why I was ready to start research when Milne arrived, and so for my entering the field of astronomy.

The books suggested by Milne for me to read before his move to Oxford included Planck's *Heat Radiation* and Jeans's *Dynamical Theory of Gases*, so that I imbibed at any rate a little quantum theory. On the astronomical side, there were, of course, Eddington's *Internal Constitution of the Stars*, and also Stratton's *Astronomical Physics* and Dingle's *Modern Astrophysics*. When Dingle's book first appeared, Milne gave it an extremely critical review; Dingle thought this unfair because a reader would have gathered only Milne's adverse opinions and not what the book was about. Dingle was surprised to be told (in 1951) that Milne, despite his criticisms, had included the book in my reading list. After reading Milne's review he had resolved that whatever his opinion of a book sent him for review, he must at least explain what the book was about.

I first met Milne in January 1929, after the first of a course of lectures he gave on stellar atmospheres. (This was the first course on astronomy that I attended; to my regret I never attended a course on general astronomy.) Milne then was recognized as one of the leading astronomical thinkers in Britain. He started peacetime research in 1919 at Cambridge with Sydney Chapman. They produced a joint paper on the Earth's atmosphere, after which Chapman left for Manchester and Milne turned his attention to stellar atmospheres, on which he became a recognized authority.[1] When I met him, he was beginning to work on stellar interiors.

He at first was not impressed by me as a research student. He had been invited to give the prestigious Bakerian Lecture of the Royal Society; he had intended to talk about his most recent work, but since this was incomplete and controversial, he switched his subject to stellar atmospheres. The switch left him short of time to make the necessary computations. He therefore asked me, at the start of the Easter vacation, to help him out with these. Unfortunately, he did not tell me exactly where to find statistical weights used in the calculations, and I produced the desired results, but only with "simplified" statistical weights. Milne used these results and did not publicly blame me.

Being temporarily without a simple problem to give me, Milne had appealed to Chapman for one. Chapman had written a paper explaining the radial limitation of the Sun's magnetic field—an effect which the observers had claimed to have identified, but which was later to be found to be spurious. He suggested that I might calculate the heights of origin of the various spectral lines showing Zeeman effects. The calculation was superficially simple, but it involved a number of unrelated factors, and I made virtually no progress with it. Milne, just before leaving for Ann Arbor in June 1929, told me to consider whether I ought not to think in terms of teaching as a career, not research.

He had been gone only about four days when light dawned. A study of velocity distributions showed that Chapman's explanation of the supposed field limitation was itself spurious. When I wrote to tell Milne, Rosseland (also at Ann Arbor just then) expressed approval—my result was also later welcomed by R. H. Fowler and others who had queried if Chapman's proposed mechanism was consistent with the laws of thermodynamics. Milne now retracted his suggestion that I should think in terms of teaching, not research, as a career. Chapman himself accepted my result in due

[1] See S. Chandrasekhar, *Q. J. R. Astron. Soc.* 21:93 (1980).

course; he showed his generous nature by offering me a post in his department when I left Oxford in 1930. A different sequel was that I developed an almost proprietorial interest in the Sun's magnetic field, and also an interest in the velocity distributions that were part of Chapman's stock-in-trade.

When Milne arrived at Oxford, he was already working on a theory of stellar structure to rival that of Eddington; he expected that I would work on a facet of his theory when he had developed it further. I did in fact work on a simple (point-source) model of a star in the session 1929–30. He gave us at Oxford a preview of his ideas in a talk to the Senior Mathematical Society (dons and research students). The chairman at this meeting (Hodgkinson of Jesus College), suspecting that Milne's ideas might prove controversial, recalled that some of those present had joined the Royal Astronomy Society (RAS) a few years earlier just to hear Jeans and Eddington slanging each other's work on stellar structure.

Milne's basic idea was that, since all that we know about a star is its surface properties, we should seek relations involving only those properties. He even suggested that properties of the deep interior, being unobservable, should be regarded as candidates for Occam's razor. His aim was to show that configurations of a star might exist other than those yielding Eddington's mass-luminosity law. Eddington of course disagreed, with some asperity. Jeans at first sided with Milne, but he later withdrew his support because he believed in liquid stars, and Milne in later developments thought of stars as being gaseous, though the gas might be degenerate. I myself kept my head down and computed a variety of configurations of my point-source model of a star. However, when in January 1931 a meeting of the RAS was devoted to a discussion of Milne's ideas, I was asked to speak in a supporting role. Eddington (correctly) said that my point-source model was not the best to represent actual stars. For the rest, I was of little use as a support to Milne; my computations had suggested to me that to get configurations differing much from Eddington's, something drastic must occur near the center.

Milne suggested that a way of getting models different from Eddington's might be through the gas becoming degenerate near a star's center. This appeared unlikely because of the reduced compressibility of a degenerate gas, and B. Strömgren, H. N. Russell, E. Hopf, and I each produced a disproof of it within a few months. (I think that mine came first.) Russell's disproof was mathematically the most interesting, because he used special nondimensional combinations of the physical variables (as Milne too had done). All four of us used approximate formulae for the pressure of a degenerate gas; it was left for Chandrasekhar later to give a more exact discussion, and to show definitely that (as we thought) degeneracy was unimportant until white-dwarf radii were being approached. Thus, one could regard Eddington as vindicated. Actually he was right only if attention was confined to stars reasonably uniform in composition, but this was not realized until much later. For the present, one might imagine that all that survived of Milne's work was the special nondimensional variables that he introduced. As Martin Schwarzschild showed much later, these were admirably suited to the fitting of external and internal solutions at an interface where gas properties alter discontinuously.

During the excitement of 1931 I had my only serious tiff with Milne. Feeling isolated after leaving Oxford, I wrote him long letters weekly until he told me to stop, as they were interfering with his own work. I then wrote less, but more critically, until at last he wrote a reply that shattered me, saying that I was being disloyal in a number of ways. I took his letter to Chapman, under whom I was now working; I acknowledged to him that some of Milne's criticisms of my conduct were well founded, but I was shocked to feel that there was danger of an actual breach. He gave me good advice, telling me to remember what I owed to Milne; he also contacted Milne as peacemaker. In consequence, when the RAS met a few days later, Milne and I were able to walk around the square at Burlington House and make things up. Our relations were less close than before, but they were easy and I could always rely on his support when needed. Perhaps because of the tiff, when we were fellow-examiners at Oxford in 1939, he made a special comment at the end of the examining on the harmony in which we had worked together.

In 1931 I began regularly to attend RAS meetings. These were held in a lecture room of only moderate size, furnished with bench seats with old-fashioned upholstery. There normally was plenty of room for all who wished to attend. The front two rows were by custom reserved for members of Council and other distinguished people. A fair proportion of the papers submitted to the Society were read at its meetings: It was not unknown for the secretaries to have to scout around to find people to fill out a meeting's program by describing work not yet complete. (The pressure of papers did not compel the adoption of a formal refereeing system until a few years later.) One was expected to wear more or less formal attire at meetings; after my first reading of a paper to the Society, a senior friend told me of his shock at seeing me wearing (my best) flannel trousers. It was probably as a result of the old-fashioned setup that about this time I had a recurrent dream in which one passed from a well-lit reading room with comfortable armchairs to a roof-place in order to study the stars among smoky chimneys.

My post under Chapman (1930–33) was, like all my subsequent posts, in a Mathematics Department. Chapman then had a manuscript containing the skeleton of a book on gas theory, which he wanted to see completed by a collaborator who had more spare time than he. He had already approached two possible collaborators, who each gave up when they realized how much work would be involved. Chapman lent me his manuscript: When I expressed my pleasure at its novelty and power, he after due thought invited me to be the collaborator. I of course accepted. He largely left me a free hand, apart from drawing to my attention a number of developments since his manuscript had been written. When I had largely completed my task, I had to wait a year for him to go through the new manuscript and suggest improvements. Our joint book came out in 1939. It had contacts with astronomy through sections on ionized gases (plasmas), but its main emphasis was on fundamentals, especially mathematical ones. This was mirrored in the "motto" he suggested for the book, based on a verse of St. Paul: "The weapons of our warfare are not carnal, but spiritual, and mighty to the pulling down of strongholds." I heartily approved of this, but Chapman dropped it, perhaps because of a changed attitude to religion.

When not working on gas theory, I did a lot of computing of stellar models in the 1930s. Thus it was natural that I received from Ludwig Biermann a series of papers describing similar work of his own. These papers, like my own, were at first concerned with stars in radiative equilibrium throughout. However, in 1932 Biermann used mixing-length ideas to show that if a region of a star is convectively unstable, the resulting convection is far more important for the transport of energy than radiation, so that the temperature gradient in that region is nearly adiabatic. I myself (1934) calculated the maximum rate at which energy generation could increase with increasing temperature in a star without the onset of convection near the center.

I needed this maximum rate in connection with the work I was then doing on the vibrational instability (overstability) of a star. In his *Internal Constitution of the Stars*, Eddington had found that a star would be overstable if there was more than a moderate
with temperature, and a research student (J. A. Edgar) had confirmed this result. Using methods developed by Jeans and Rosseland, I was able to show that the result was incorrect. No star in radiative equilibrium throughout would be overstable; a star in which the rate of energy generation increased fast enough with temperature for overstability to be expected would already be convectively unstable.

The question remained, what was the situation for stars with a convective core? For the stability work I needed a stellar model which deviated as far as possible from wholly radiative equilibrium, and so had its energy sources concentrated as far as possible toward its center. I was therefore led to consider a model with a convective core and a radiative envelope, with energy sources confined to the core: It was immaterial just where these sources were, since the temperature gradient in the core would in any case be adiabatic. This was the origin of what Chandrasekhar was later to call the Cowling model. The numerical integration required for the model was more complicated than any that I had done before, but not impossibly so.

Using methods similar to those developed for purely radiative models, I was able to show that the model was vibrationally stable unless the rate of generation of energy increased like a large power of the temperature T, say like T^{100}. However, this result depended on the damping effect of the outermost layers being large, and I was slightly apprehensive lest later work on these layers might show that I had overestimated their effect. Still, it seemed unlikely that such doubts were enough to affect the conclusion that stars were justified in continuing to exist—a truly satisfactory result!

The Cowling model was introduced as a subsidiary part of a stability investigation, but it had independent value. It led to a first letter from Biermann, who confessed that he had been disappointed that his (fundamental) work on convection had so far received scant attention. This was the start of a vigorous correspondence between us in the years 1936–39, each of us stimulating the other's work. I myself had noted that the Cowling model differed far less than one might have expected from Eddington's standard model in the predicted internal distributions of density and temperature. Biermann drew attention to more fundamental deductions concerning the mass-luminosity relation, etc. Similarly, when in 1936 Biermann discussed wholly convective stars, which appeared to possess the extra degree of freedom that Milne had sought, I pointed out that the freedom was limited by the existence of a thin surface layer, in which radiation was dominant. Another topic discussed was the possible anisotropy of turbulent viscosity and diffusion.

My work on stellar stability was mainly done in 1933–37, when I held a post at Swansea—a relatively junior post, but one with tenure. I also worked on the theory of sunspots at about this time. Milne in 1930, following H. N. Russell, had discussed the structure of spots on the assumption that their coolness was due to the adiabatic cooling of rising gas; he found that the cool layer below the visible region would be relatively thin, not more than about 100 km deep. I pointed out in 1935 that the assumption of adiabatic cooling was untenable; the region surrounding the spot was convectively unstable, so that rising gas should be warmer than its surroundings, not cooler. Moreover, upward motions in a cool spot would be opposed by gravity because of the greater weight of the cool material; some unknown force at the spot base would be required to maintain any upward motion.

My antidynamo theorem (1933) was given in the course of a discussion of sunspot magnetic fields. Sir Joseph Larmor, in a paper often quoted by

Chapman, had suggested that the magnetic fields of cosmical bodies might be maintained by a dynamo process in which an electric field is generated by motions across the field lines of the existing magnetic field. My theorem was that an axisymmetric field cannot be maintained by motions likewise axisymmetric. The work that led me to my theorem began as an attempt to compute the sort of field that might be maintained by a Larmor-type mechanism. For some reason I thought that it might be convenient to start by considering what happened at a neutral point (strictly circle) of the field. I found that the induction equations could not be satisfied at the neutral point; the equations require a nonzero electric field at that point, but the vanishing of the magnetic field there means that the induced electric field also vanishes. When the new science of magnetohydrodynamics (MHD) had developed far enough, a more physical interpretation of the theorem was possible. The field lines are nearly "frozen" in the material, and a Larmor-type flow can increase the field locally by pushing the field lines together, but it cannot create new field lines.

This interpretation was still in the future—though I was not far from the concept of frozen-in field lines in thinking that spot fields might be explicable in terms of a local compression and twisting of a dipolelike general field of the Sun. I have to confess that often too much emphasis was put on my theorem, which was taken to preclude any dynamo maintenance of cosmic magnetic fields. Actually the theorem did not forbid such maintenance; it only asserted that if there was dynamo maintenance, the fields and the motions associated with them could not be simple ones.

The theory of sunspots was one of the topics about which Biermann and I corresponded. I am not clear about all the details now, but I think that we independently came to the conclusion that magnetic pressures must play a vital part in ensuring that the cooler spot material is not overwhelmed by the hotter material outside. However, I am certain that he was the first to realize that the coolness of a spot as compared with the surrounding photosphere might be due to the magnetic field in the spot column interfering with the convection of heat to the surface there, whereas outside it there is no similar interference. I regarded this suggestion as important, and one for which he should be given due credit. Thus I made no attempt to publish it during the wartime, although I did mention it at an RAS meeting, ascribing it to Biermann. When we resumed contact after the war, I wrote to ask where he had published it, expecting that it would have appeared in a substantial paper in an important journal. He was able only to mention a short note in the *Vierteljahrschrift* in 1941. It was left to me (spurred on by Claes Walén) to popularize and extend the suggestion.

In 1938, just after the Munich "settlement," I wrote to tell Biermann that if the decision rested with us, I was sure that war between our two countries would be unthinkable. A year later, when war seemed imminent, I wrote a carefully worded letter concluding "With all good wishes, whatever may transpire during the next few days." After the war we did indeed resume our happy personal relations, but out astronomical interests now tended to diverge. Our first actual meeting was not until the Rome International Astronomical Union (IAU) meetings in 1952.

In addition to the collaboration by letter with Biermann, I (in 1938) produced a paper on the rotation of the apsidal line in close binary systems. Zdenek Kopal had used the rotation rate to estimate the degree of central condensation of the stars in a binary; he found the density to be much more uniform than Eddington's theory indicated. I queried the soundness of Kopal's mathematics but found that any purely mathematical error did not affect the correctness of Kopal's result. However, there was a more physical error; he had not taken into account the tidal distortion of the stars, which at any instant is close to the equilibrium value. When this is taken into account, theory and observation agree tolerably well; once again, the stars were permitted to exist as we see them.

During the war I branched out in new directions. According to what I later heard, I was regarded as one who should not be entrusted with state secrets because I had unreliable associates. Thus I was left undisturbed to teach Applied Mathematics at Manchester University, with a sizable amount of free time. One new topic that I discussed was the nonradial oscillations of stars; I set to work on this because I felt that if a star was convectively unstable in any region, a normal mode analysis should reveal the existence of growing nonradial oscillations. I found that there were two classes of such oscillations, the *p*-oscillations driven mainly by pressure fluctuations like those in a sound wave, and the *g*-oscillations driven mainly by gravity fluctuations like those in the terrestrial atmosphere; these were separated by a (fundamental) *f*-oscillation. Others have since shown that often there is no such tidy separation between *p*- and *g*-oscillations, and the *f*-oscillations do not always exist. I regarded this work as having primarily only mathematical interest, not suspecting that it would later lead to a useful method of probing the subsurface layers of the Sun. Much of the computation required in this work was done in the middle of the night, when as an Air Raid Warden I was manning a blockhouse during a raid.

Other work during the war years was suggested to me by Chapman. This included work on gas theory (connected, I have since come to believe, with the enrichment of atomic bomb material) and on the effect of a magnetic field on the conductivity of an ionized gas (of most interest in ionospheric physics). There was also work on the heating or cooling of the Earth's upper atmosphere by infrared radiation; this started well, but it petered out when one reached the real difficulties.

More important was my being led into a first contact with Hannes Alfvén. Chapman sent me copies of two of Alfvén's papers, which presented an altogether different picture of geomagnetic storms and the aurora from that given by Chapman and Ferraro ten years earlier. Chapman had already, at a private meeting prior to 1939, pointed out to Alfvén certain aspects of the latter's theory which he thought were inconsistent with the observations. He now invited me to exercise on these papers the critical faculty of which he had had personal experience. His idea was that I should produce a critical review of the papers, starting with a concise account of them for the many readers who would not have access to the originals. The review that finally appeared in *Terrestrial Magnetism* in 1943 was a third version; Chapman rejected the earlier two because, in my hurry to get on with my criticism, I had failed to give an adequate and fair account of what Alfvén had actually said.

Chapman and Ferraro had in 1930 given a theory of the impulsive first onset of geomagnetic storms, based on the recognition that a stream of charged particles emitted from the Sun could be treated as a perfectly conducting fluid. (I first met Ferraro in 1930, when we were both appointed to teaching posts by Chapman: Each of us then spent quite a time enthusiastically explaining his research to the other.) The Chapman-Ferraro theory, although it satisfactorily explained the onset of magnetic storms, could not be developed to give an explanation of the storm's later phases. Alfvén, on the other hand, stressed these later phases; his picture of a storm, and of the aurora associated with it, was that a stream of highly energetic solar particles passed without great hindrance through a solar magnetic field and the outer geomagnetic field, being finally precipitated along the geomagnetic field lines to the Earth's auroral zones.

I found it easy to criticize Alfvén's theory, notably his assumption of highly energetic particles and their passage through the interplanetary magnetic field without great hindrance, and (still more) the disregard of the cooperative effects which justified Chapman and Ferraro in regarding a stream of solar particles as behaving like a perfect conductor. At the same time I had to approve of Alfvén's description, in these and later papers, of the motion of a charged particle in a magnetic field in terms of a guiding center and adiabatic invariants. My review actually included a mathematical justification of some of Alfvén's ideas in this regard. It was the assumption that it was enough to consider only the motions of separate particles that I criticized.

I now regret that my first encounter with Alfvén was as a bespoken critic. Nevertheless, as the last survivor of Chapman's original group, I feel it incumbent on me to defend their ideas against the assaults which Alfvén still continues to make on them. Much of the difference of opinion was due to the difference of the models which they had at the back of their minds, Chapman and Ferraro thinking in terms of solar streams colliding with the Earth's magnetosphere, and Alfvén taking the precipitation of charged particles onto the ionosphere as all-important. Each was partially correct; I still regard Alfvén's original presentation as the more seriously defective.

I must now return to chronological order. In 1945 the gap between the ideas of Chapman and Alfvén did not appear to be as unbridgeable as they later were made out to be. In that year I considered the decay of the Sun's general magnetic field, assuming (as was customary then) that the field had dipolelike structure. I found that the time of decay due to ohmic effects alone was comparable with the lifetime of the Sun, suggesting that the present field might be a "fossil" relic of the field with which the Sun was born. I was reluctant to press this suggestion, since it made the Sun's field depend on a preexisting field in the galactic clouds that condensed to form the Sun, and this field in turn would need explaining. A few years later, the Babcocks showed that the Sun's general field is neither dipolelike nor steady. This led others to consider afresh the possibility that the fields of the Sun and stars might be maintained by a dynamo process similar to that of Larmor. I did not join them because of increasing responsibilities and lack of time to master the use of electronic computers.

In 1946 I returned to the problem of the magnetic field of sunspots, stimulated thereto by a presidential address by Chapman to the RAS. Correcting Chapman's approach, I found that the time of ohmic decay of a static sunspot field would be of order 300 years, as against the few days taken by actual spots to disappear. I concluded that the appearance and disappearance of spots were not to be explained in terms of their creation and decay in situ, but rather by the emergence and submergence of parts of a magnetic "rope" (bundle of field lines). The relatively permanent spot field was not produced by the transient dark spot; instead, the transient spot must result from the relatively permanent magnetic field, a conclusion which Alfvén had already surmised. Actually, when I started on this piece of work I expected to find that Alfvén was in error: I had to acknowledge that he was right.

This does not mean that I accepted Alfvén's ideas about sunspots in toto. Alfvén explained the sunspot cycle in terms of MHD waves traveling to and fro along the field lines of a dipolelike magnetic field; when a wave packet reaches the Sun's surface, a spot pair appears and at the same time the packet is reflected back along the field lines. Alfvén gave no account of the way in which the wave generates the sort of field that we see in a spot pair, or of the way in which a moving wave can generate a spot field which persists unchanged in form for days or weeks. Instead, he spent most of his time examining the propagation of the waves below the Sun's surface, claiming that spots in one hemisphere in one 11-yr cycle could be correlated with spots in the other hemisphere in the next cycle. When it was found that the Sun's general field was not dipolar, and in any case was too weak to permit propagation of the waves from one hemisphere to the other in the allotted 11 yr, he tried to show that the real field is much more regular and stronger than that which the observers claim to see. His theory of sunspots was one that I found especially easy to criticize (1946).

Let me change to a happier story. In 1949 Alfvén sent Stig Lundquist to work with me for a couple of months. Alfvén had recently suggested, by analogy with a twisted telephone wire, that a magnetic rope twisted too far would become unstable and form loops. I suggested to Lundquist that he might study this idea and suggested an energy argument for doing so. The result was to confirm Alfvén's suggestion: The method was used later in discussing the stability of thermonuclear plasmas. This is the closest that Alfvén and I have gotten to working together.

In 1951 I spent six months in the United States, half at Caltech, half at Princeton. It was the first time I had worked abroad, and I enjoyed meeting American astronomers, especially Horace Babcock, Greenstein, Spitzer, and Schwarzschild. While there I also met Swings and Ledoux, the latter of whom I had first met in Belgian Air Force uniform during the war. The sequel was that I have paid a number of visits to Liège, especially enjoying several of the annual colloquia there. I profited greatly from such visits; I had been too much of a home bird.

In 1951 I began to work on the magnetic fields of stars, then recently detected in a number of stars by Horace Babcock. The fields were often found to vary periodically. Two explanations of this variation were proposed. The first and simpler was that the star was rotating about an axis inclined to its magnetic axis, and so presenting a variable magnetic aspect to the observer. Babcock did not accept this, believing that it required larger rotational velocities than those inferred from spectral line broadening. The alternative, preferred by him, was that the variations were due to a magnetic oscillation, or to a magnetic cycle like that of the Sun. The magnetic cycle variant, demanding the wholesale destruction and re-creation of the surface field, appeared to be ruled out by the slowness of the field changes which a cycle could be expected to produce. The magnetic oscillation variant was studied by Schwarzschild, Gjellestad, and Ferraro, with rather indecisive results. I found that the oscillations expected to be found in nature were likely to be too fast, and in any case could not explain an overall reversal of the field, as observed in some stars. Thus the oblique rotator theory was left in possession of the field of combat, and Babcock has accepted it. It was ironic that at the RAS meeting where I did my hatchet job on the magnetic oscillator theory, I followed immediately after Ferraro, who gave an account of his as yet incomplete results. I was sorry to be thus exploding his work; he was modest and unassuming, and one could not help liking him.

In 1952 I attended the Rome IAU meetings and had the great pleasure of meeting for the first time (as well as Biermann) Severny and Massevich from the Soviet Union. In 1954 I was invited to attend the ceremonial reopening of the Pulkovo Observatory, destroyed by the Nazis during their attack on Leningrad. My invitation was by accident. The USSR Academy of Sciences invited four from the UK, the Astronomer Royal (Spencer-Jones), the Director of the Nautical Almanac (Donald Sadler), the President of the RAS (Herbert Dingle), and myself; my invitation apparently was as the Vice-President of the RAS whose name came first on the alphabetical list. Of the four of us, Spencer-Jones declined the invitation, having visited the USSR recently; Dingle also declined because he was uncertain about what to expect. I felt much the same as Dingle, but I decided to go after encouragement from Spencer-Jones and the Vice-Chancellor of Leeds University. I need not have worried; we were right royally entertained. Stalin had died a few months earlier, and the USSR Academy was bent on making it clear that there was a thaw in the Cold War. I took advantage of my status as guest to visit the Baptist Church in Moscow (with an official interpreter). I was encouraged to learn that there still were more than a half-million Baptists in the USSR, but less encouraged when told that Baptists in Russia had to be fundamentalists.

At the Pulkovo celebrations I was glad to renew contacts with Severny and Massevich, as well as meeting Ambartsumian and other Russian astronomers. Among the dozen or so there from the West, I was glad to find Jan Oort. He was perhaps the most distinguished person there, and in the official photographs other participants seemed to crowd toward him. It was typical of his modesty that in the photographs he seemed the only one unconscious of the camera.

The rebuilt (and much enlarged) Pulkovo Observatory taught me that when the USSR authorities decided that a project was a really good one, they put their whole weight (and a lot of money) behind it. This was a lesson that others had to learn from the successful launching of the first Sputnik a few years later. I also found that the Russians were ordinary people, very much like us and wanting to establish friendly relations, but that even their astronomers seemed to think that the West was only awaiting a favorable opportunity to attack them.

The program arranged for us visitors was exhausting. We were woken up before 6 A.M. by the clatter of heavy boots of workers going to work, and our day often finished about midnight after a theater visit. I sometimes wonder if this was partly responsible for my having to have a duodenal operation a few months later, but in any case my university and external work rose to a maximum about then. I had to produce a report for Commission 35 of the IAU (on stellar structure, a subject on which my own original work was now virtually at an end); I had also been invited to write an Interscience tract on MHD. These activities must bear part of the blame for my duodenal trouble.

I enjoyed writing the MHD tract. Even the duodenal operation helped, in that it gave me time to get my ideas in order. During my convalescence, when for some months I was forbidden to go to the university, I was able each day to read or reread a relevant paper by 11 A.M. and then stop work and go for a walk or indulge in other nonacademic activities for the rest of the day. Thus when I came actually to write the tract, I had the material at my fingertips and could write at my top speed, which gave freshness to the work. MHD was then in its infancy; I touched on a number of its branches, but the main emphasis was on applications to astronomy and geophysics. Also, although the last chapter introduced certain particle aspects, the tract was mostly concerned with continuous isotropic fluids. Because of the size of cosmical bodies, it was often permissible to regard the magnetic field lines as frozen into the material, or nearly so. However, this meant leaving out of consideration a number of phenomena of the Earth's magnetosphere and the Sun's corona—although I confess that I was delighted later to find that this "ideal" MHD, applied to the Van Allen belts, could give at least a qualitative explanation of certain of the later aspects of geomagnetic storms.

With the completion of the Interscience tract, my original work was nearly done. Parker's papers (1955) on magnetic buoyancy and on the solar dynamo came early enough to be mentioned in the tract; I have been unable to vie with Parker in his subsequent meteoric career. In 1957 I had to cope with something like a slipped disk, while at the same time taking over duties from a sick colleague. (He had similarly taken over from me when I was ill in 1955.) In 1960 I had a heart attack, fortunately mild, but serious enough to restrict my activities for a year or two. Also, although I had overall responsibility for the Leeds electronic computer (installed in 1958), I never had enough time to learn to use it, and so my interest in dynamo theory had to be only that of a spectator. In place of original papers, my publications after 1957 were reviews, explanatory articles, presidential addresses, and new editions of my tract and of the gas-theory book with Chapman. I had to labor far harder on the new editions than on the originals, and the signs of effort must have been clear to readers.

The rivalry between Alfvén and Chapman, with their different theories of geomagnetic storms, has unnecessarily lingered on. In view of my earlier criticisms of Alfvén's work it was ironic that in 1967 I, as president of the

RAS, had the duty of presenting the Gold Medal of the Society to Alfvén and explaining the importance of some of his ideas. As mentioned earlier, I still stand by some of my earlier criticisms, but I do not wish to indulge in polemic here, the more so since most workers in their field seem now to believe that the ideas of Chapman and Alfvén have been subsumed into a later synthesis. I would simply say that my congratulations to Alfvén for his Nobel prize do not imply acceptance of all his ideas.

I have never been able to build up a research school. After leaving Chapman's department in 1933, I had five years (at Swansea and Dundee) in Mathematics departments with only four members of staff, of whom I was usually the only Applied mathematician. It was a little better at Manchester (1938–45), where in G. L. Camm I had a colleague of similar interests; but the war meant that I had just two research students in the seven years; these attained MSc degrees, but not in astronomy. At Bangor, where I was Professor of Mathematics (1945–48), I was back in the small departments, though enjoying contacts with R. A. Newing.

At Leeds, where I moved in 1948, I found a Mathematics Department with ten staff members (four Applied) and a strong emphasis on Applied Science in its teaching. My predecessor, Selig Brodetsky, had worked on problems of aircraft flight, and at first it was taken for granted that any new research students on the Applied side should work on this subject under Dr. (later Prof.) H. L. Price, who had gained a wartime PhD working under Brodetsky. When (in the later 1950s) I first tried to branch out with such research students as I could get, the result was not always happy. Two people lost the theses which they had already started to prepare, one through having a bag stolen on a bus, the other through my carelessness.

However, there were bright patches. In 1951–54 I had as research fellow in Leeds Leon Mestel (now Prof. Leon Mestel, FRS), the nephew of Brodetsky. He had been a research student under Fred Hoyle, who recommended him to me. My chief care for him (apart from introducing him to MHD) was to try to get him to restrict his abounding energy to one thing at a time. Initially this made him diffident, but when he found his feet he branched out in no uncertain fashion.

Just before I retired from my Leeds post in 1970, I had as a PhD student Eric Priest (now Prof. E. R. Priest at St. Andrews). It was only after some hesitation that I took him on, since I felt that I was getting too far out of touch with recent developments. His cheerful enthusiasm more than compensated for my deficiencies. With me he worked on the problem of solar flares; after leaving Leeds he made contact with American solar physicists, especially those at Boulder. This helped him supplement his mathematical training with an appreciation of the observations. As with Mestel, I enjoyed having him at Leeds. I might also mention two other PhD students, Alan Hare (who went to Bangor) and K. R. Singh (from Bengal), but these worked on nonastronomical MHD.

Readers who have stayed with me this far may fairly ask what lessons can be learned from my experience. I have acquired the reputation of a Doubting Thomas because of my work in exploding dubious theories. I am slow both in writing and in absorbing new ideas; the extra time spent in mastering an argument has often led me to recognize where that argument may be defective. But I have not always followed the excellent rule of Fred Hoyle, who in a paper in this series states that when confronted by an attractive but possibly dubious argument, he tries to pick out what part of it is satisfactory. I commend that approach, but I myself have been too prone to see ideas as absolutely black or white, thereby justifying Milne's advice to me not to confuse scientific errors with moral defects. Of course, sometimes one has to weed out bad ideas to leave space for the good; but as Portia put it, "We do pray for mercy, And that same prayer doth teach us all to render The deeds of mercy."

As a theoretician one is always liable to forget that no proof or disproof of a statement, save a purely mathematical statement, can be absolute. For a proof to be applicable to the real world one must start with a model of the real world, and the soundness of the proof depends on the correctness of the model. This is, of course, generally understood by theoreticians, although often forgotten. I think that observers advancing a theory "based wholly on observations" are those most liable to a similar oversight.

A critical paper should not end with a purely negative conclusion. Indeed, as Albert Schweitzer (following Aristotle) stated, a critical discussion of earlier work often is the best prelude to developing a better system. But pure nihilism is unhelpful.

Again, one should cultivate a roving eye. Often one can find that, when attacking a particular problem with all one's force and making little progress, one suddenly realizes that one's work has thrown an altogether unexpected light on a different problem. This does *not* mean that one should just sit and wait for inspiration. As Poincaré said, the inspiration comes only when one is deeply involved in a struggle.

During my most active period of research, I had to work alone and not as a member of a team. More recently it has become expected that one should work as a member of a team, and funds far beyond one's prewar dreams have thereto been made available. We must be grateful for the extra funds, but the increased specialization and dependence on large computers, etc., has its dangers. The team members have to be encouraged to think for themselves and to look over the edges of their grooves.

Finally, not only did I as a student receive no systematic instruction in general astronomy, but as a lecturer most of my teaching has been on branches of Applied Mathematics not related to astronomy. I regard this as a serious handicap; when one lectures on a subject, one is continually forced to reconsider its foundations and development. While it is an open question how far astronomy should be taught as part of an undergraduate course, research workers can often be helped by having to teach their own specialties. Of course, there is always a danger that an excessive teaching load may block one's way toward getting research done; Milne advised me, at the start of my teaching career, to find time for research each day, even if for only a half hour. The ideal is that one's teaching should minister to one's research, and vice versa. Whereas it once was possible for a mathematician like Chapman to be offered a post in astronomy immediately after graduation and be left to pick up his astronomy as he went along, this is not to be expected nowadays. In any case, the Chapmans of this world can find their way to the top, whatever the system.