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Brief Portrait of the Scientist as a Young Man: Researches on Dungey's "Open" Magnetosphere From the 1960s to the 1980s

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Abstract The author's early research career and scientific output is outlined, spanning the interval from the late 1960s to the late 1980s when he transitioned from Jim Dungey's PhD student at Imperial College, London, to full professor. The work, principally concerned with elucidating theoretical aspects of Dungey's "open" model magnetosphere, is set within the context of burgeoning observations from both ground and space, resulting in greatly improved knowledge and growing international acceptance. Scientific encounters with some of the major players in the field during that time are recounted, now recollected in tranquility.

Key Points:
- The author's early career is outlined from the 1960s to the 1980s, researching mainly on topics connected with Dungey's open magnetosphere model
- Theoretical work is set within the context of burgeoning observations, resulting in growing evidence and international acceptance
- Scientific encounters with some of the major players in the field at the time are recollected, many no longer with us

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1. Getting There

I have been asked a number of times what first inspired me to a career in space plasma physics. The answer to that is very short and clear, namely, "Sputnik 1," the first "artificial moon" launched into orbit by the Soviet Union on 4 October 1957 when I was ten years of age. It is almost impossible at this distance in time to imagine the worldwide sensation this caused, filling many pages of newsprint over many days, only to be followed by the unfortunate dog Laika in Sputnik II the following month, and Sputnik III, scientifically instrumented to support studies during the ongoing International Geophysical Year (IGY), the following May. Quickly realizing that the popular press could rarely get their facts straight, I would cycle down to the public library each week to read the aerospace magazines, avidly following the latest triumphs and disasters of the US Explorer and Vanguard programs. This set me firmly in the direction of "STEM" subjects at secondary school in Coventry, England, and eventual applications to undertake Aeronautical Engineering at university at age eighteen. I was fortunate to be accepted onto the prestigious course at Imperial College, located over the road from the Albert Hall in central London, only for it to become very apparent after a couple of weeks that where my interests really lay were in fundamental topics of physics rather than applied areas of engineering. Again very fortunately indeed, the system at Imperial was sufficiently flexible (and my "Advanced Level" grades sufficiently good) to agree a transfer to the Department of Physics (subsequently the Blackett Laboratory), located a bit further west along Prince Consort Road, SW7. I, together with a group of other latecomers, was assigned Dr. John Clegg as academic tutor, who introduced himself, lighting one cigarette from the butt of the last, with the statement "Yes, Jodrell Bank started with me and Lovell tramping round a field in gum boots." Led by Professor Bernard Lovell at Manchester, the Jodrell Bank dish, inaugurated in 1957 and then the world's largest fully steerable radio telescope, had been involved in a number of early high-profile space-related activities, including tracking the orbiting booster that had launched Sputnik 1. Clearly, I had found my true scientific home. Many years later, by the time I had made full professor at Imperial, it was a thrill and honor to shake the hand that had last touched Sputnik 1 on top of the R-7 rocket at Baikonur Cosmodrome, that of Konstantin Gringauz who had been in charge of the satellite's bleeping radio transmitter.

During undergraduate years 1965–1968 my inclinations drifted toward theoretical aspects, and in the third and final year I decided on the "Theory Option" courses. This was not because I was much of a hotshot mathematician, though I could shovel a few equations around satisfactorily and generally puzzle out physical problems, but mainly because I was then excused the (in my view) unilluminating and tedious work offered in the undergraduate laboratory. Although much of the theory option course content was reflective of the Department's central interest in "particle physics" (Abdus Salam, Paul Matthews, and Tom Kibble then being leaders in the so-called "Theory Group"), one of the items on offer was a course on "Hydromagnetics" given by Professor James Wynne Dungey. As might be expected, this course covered aspects of frozen-in flow, low frequency waves, and plasma equilibria, including cryptic mention of the stability of systems containing magnetic neutral points with “interesting applications in space physics.” He also mentioned the spiral structure of the interplanetary magnetic field as an application of frozen-in flow, then a “hot” recent discovery, confirmatory of Parker's prediction, in IMP 1 spacecraft data. Although Jim Dungey was never the world's greatest lecturer, as far as I recall this was the only course even to briefly mention space physics topics during the three-year program, such that his group was of potential interest for subsequent doctoral studies. Looking again at my lecture notes from Jim's course, on page 2 I particularly note the solemn maxim “We do not question Maxwell's equations.” This advice I have certainly taken to heart over the years and have passed on to many generations of undergraduates lecturing on electromagnetism both at Imperial and at Leicester.

During our final year the Department organized a series of minitalks by academic staff looking to recruit PhD students for the coming October. Jim Dungey gave one of these presentations but somewhat characteristically at the end of his pitch announced that we need not waste our time applying, since he had already offered his funded PhD studentship to another of our number. I do not precisely recall my response but likely applied instead for a studentship in the allied “Cosmic Rays” group led by Professor Harry Elliot. At that time his group was (among other things) developing experiments to be flown on the first series of scientific spacecraft developed by the European Space Research Organisation (ESRO) founded in 1964, including Peter Hedgecock's magnetometer flown on HEOS-1 and HEOS-2. Again fortunately for me, said prospective PhD student subsequently declined Jim's offer in favor of a studentship in the new field of computational physics being developed in the Plasma Physics group at Imperial (led by Malcolm Haines), and so it was that I instead was invited to join Jim's group at Imperial as his PhD student in the fall of 1968.

2. Jim Dungey's Group in the 1960s—PhD Studies

Jim Dungey's own early career—spanning PhD studies at Magdalene College Cambridge during 1947–1950 as a student of cosmologist Fred Hoyle (Dungey, 1950), through publication of his ill-received thesis paper on “magnetic reconnection” (Dungey, 1953), to the eventual appearance in January 1961 of his seminal two-page paper on the “open” model of the Earth's magnetosphere, also from his thesis (Dungey, 1961)—has been well rehearsed in the historical literature and need only be briefly recalled here (e.g., Cowley, 2015; Southwood, 2015). By the time I became his PhD student in 1968, Jim had been at Imperial for five years, having moved from the Atomic Weapons Research Establishment in Aldermaston, with his “open” magnetosphere being regarded as a theoretical curiosity by some and with scientific hostility by others. However, two years prior, in 1966, one of Jim's “long-distance” PhD students at Pennsylvania State University, Don Fairfield, had obtained the first strong but indirect evidence supporting his model, finding at Jim's suggestion a clear connection between the disturbance field recorded in IGY high latitude ground-based magnetometer data and the sense of the north-south component of the magnetic field measured in the “transition region” (magnetosheath) upstream of the Earth by the Explorer 12 spacecraft (Fairfield & Cahill, 1966). To quote from the published version of his professorial inaugural lecture in May 1966, Jim considered this evidence for the “open” model to be “overwhelming,” and, I think, he did not seriously doubt the basic veracity of his idea from that point onward. Indeed, in the same lecture he says that his “major interest” is in the physics of wave-particle interactions, a statement largely borne out by his publication list up to his retirement in 1984. It is also evident in the topics pursued by his other near-contemporaneous PhD students, who included David Southwood studying Kelvin-Helmholtz waves at the magnetopause and ULF field line resonances, together with both David Nunn and Maha Ashour-Abdalla working on the theory of the side-band instability and triggered emissions in electrostatic and whistler-mode wave packets, respectively. Earlier students Anath Chandra Das and Mike Houghton, then completing their studies, had also worked on whistler waves.

I, however, was put to the study of the physics of the current sheets where “ideal” conditions break down and reconnection may occur, that are of central importance to the “open” model. These comprise both the dayside magnetopause, first clearly observed in 1961 in Explorer 12 data (Cahill & Amazeen, 1963), and the current sheet in the geomagnetic tail, first observed in 1961 in the postdusk sector by Explorer 10 (Heppner et al., 1963) and then in the dawn and midnight sectors in 1963–1964 by IMP 1 (Ness, 1965). Our focus at that time was principally on the tail system as the more symmetrical and simpler of the two.

The objectives of the overall theoretical program at Imperial were set out by Dungey and Speiser (1969), involving two steps. The first was the construction of current sheet equilibria in which the fields are self-consistently generated by the motions of the collision-free plasma particles, satisfying both Liouville's theorem and Maxwell's equations. Ted Speiser, the second of Jim's "long-range" PhD students at Penn State, had already made theoretical and computational studies of the special motions of charged particles in model current sheets (Speiser, 1965a, 1965b, 1967), following earlier qualitative discussions by Dungey (1953). The second was to study the stability of the particle distributions so formed within the current sheets, to determine the extent to which the particles might be scattered, and the equilibria disrupted, by wave "noise."

The first problem I was set to examine was the stability to plasma oscillations of an electron distribution consisting of two equally dense thermally distributed beams, meant (I suppose) as a first approximation to the form of the accelerated electrons in a current sheet. The solution for the case in which one beam is much weaker than the other (the "bump on tail" problem) was well known since the work of Landau (1946)—plasma oscillations grow for waves whose phase speed match the electron speed in the "bump" where the distribution function increases with the velocity. But what happens in the case of two equal beams? After first immersing myself for several weeks at Jim's behest in the tome on plasma waves by Stix (1962), not the world's lightest read, I tackled the problem and succeeded in showing, over many pages of algebra, that the only growing waves are those occurring with zero phase velocity, at the minimum in the distribution function between the two beams. Jim was clearly not impressed by my lengthy analysis, however, and asked Ira Bernstein (of Bernstein waves fame), who happened to be visiting the group at the time (and lecturing on lunar wakes as I recall), whether he had any suggestion. He took a brief look at the velocity space integral in the dispersion equation and suggested to fold it about the center of the distribution. I duly followed his suggestion, and lo, the result popped out in a very few lines. This was my first paper, published in the Journal of Plasma Physics in 1970 (Cowley, 1970). Unfortunately, being young and ignorant of appropriate scientific etiquette, I did not mention Bernstein's contribution in an acknowledgment. I am glad to have the opportunity to correct that omission, if a little belatedly, here.

One advantage of being in Jim's group in central London was the numbers of influential and senior scientists who were happy to visit and to lecture and discuss, including Paul Coleman and Chuck Sonett (first interplanetary field measurements on Pioneer 5 in 1960 cited in Dungey's classic paper), Atsuhiro Nishida, Alfredo Baños, Norman Ness, and Ian Axford. Axford in particular was an early supporter of the "open" model, a man with a commanding though softly spoken presence, and a distinctive personal manner in discussion, first concentrated and thoughtful, then making an emphatic point. David Beard was another frequent visitor, originator of computational models of the shape of the dayside magnetopause. He it was who kindly donated the "Beard Chair of Physics" to the group, a rather battered blue leather armchair located in the "D.B. Beard Room" next to Jim Dungey's office on the tenth floor of the Physics building, into which group members might relax after lengthy bouts on the noisy computer card punch machines.

Tradition in the group was that the problems to be tackled by the PhD students would be sketched out by Jim longhand on a single sheet of paper, consisting of a few equations and a few "explanatory" notes. The joke among the students was that one was finally ready to write and submit one's thesis when one had finally comprehended the hieroglyphs on that sheet! After the plasma waves work, I duly received a piece of paper—involving a unidirectional wave equation applied to a time-dependent current sheet as I recall—which I duly took away to study. I had already been immersing myself in Ted Speiser's papers, thinking about issues of current sheet self-consistency, and coming to the conclusion that what had been set down had its limitations, rather boldly told Jim so. After some discussion the upshot was that my agreed focus shifted toward detailed study of current sheet equilibria, which could form the basis for subsequent development. In particular Jim pointed me to a brief paper recently published in JGR by Alfvén (1968), which considered a neutral sheet system contained between parallel conducting boundaries, intended by him as a model of the dayside magnetopause, but applied by us to the geomagnetic tail. The key feature of the model was that the current-carrying particles in the current sheet are continuously supplied from the region of "reversed" magnetic fields by $\boldsymbol{E} \times \boldsymbol{B}$ drift from either side, following which they oscillate about the current layer in "Speiser" orbits and are accelerated along it by the electric field, ions in one direction and electrons in the other, self-consistently generating the sheet current. In effect, Poynting flux flows into the current sheet from both sides where the field is "annihilated," the field energy being fed where $\boldsymbol{j}.\ \boldsymbol{E} > 0$ directly into accelerated particles without benefit of "noise" or increase of entropy. The equilibrium condition (Ampère's law) then yields the value of the total electric potential across the system in terms of the field strength and number density in the "lobes." My main thesis problem thus became an examination of the equilibrium structure of Alfvén's current sheet model (which I did remember to cite), showing that the electric field must be nonuniform, with the inflow to the current sheet being concentrated near the "end" of the layer where the ions exit the current sheet (Cowley, 1973a). Exceedingly scarily, at Jim's behest I was invited to give a 30 min presentation of my results at an international discussion meeting on the "Polar Ionosphere and the Magnetosphere" held at the Royal Society in London, 15–16 December 1970, my first public presentation. Paul Coleman, Tom Holzer, Roger Gendrin, Fred Scarf, and Ian Axford were other presenters. At the end of the presentation I recall being asked by Vincenzo Ferraro (then in the Maths department at Queen Mary College in east London) from the audience where I thought the electric field in the model came from. I replied briefly that system equilibrium required such a field to be present, a response that appeared to satisfy Jim, who was a master of brief semicryptic responses. Perhaps I had subconsciously recalled Jim's further assertion in his Hydromagnetics course, that "it is unhelpful to ask such questions as 'where does the ***E*** field come from?'." The following year it was a wonderful encouragement to receive a letter from Alfvén, marked Stockholm, 20 August 1971 —the year after he won his Nobel Prize—"Dear Dr Cowley [it should have been "Mr" at that point], I write to congratulate you to your very important paper on the Magnetic Neutral Sheet System, which we are studying here with much interest. Please give my best greetings to professor Dungey. Yours sincerely, Hannes Alfvén." The graciousness of his taking the trouble to send this message seems no less now than it did at the time.

Curiously, a number of years later I chanced to see a copy of Alfvén's list of publications—for some reason his 1968 neutral sheet paper was omitted. Around this time, however, Alfvén was busy berating the international community for too blindly applying the results of his earlier studies, in particular the "frozen-in" condition. Although evidently a centrally important theoretical concept, there are, of course, limitations. On one occasion he spent a few days visiting our group, and presented a seminar in which he referred to the frozen-in condition which had earlier been introduced by "some fool" (meaning himself, of course), at which point Jim broke in to ask whether he would therefore give back his Nobel Prize. Alfvén took no offense at this provocative joke, however, instead having a good laugh in response. On the same visit, I was deputed on one occasion to see Alfvén safely back to his hotel, around the corner of Prince Consort Road in Queen's Gate. In the course of conversation during the walk he told me that the biggest surprise of his career had been the discovery of the magnetopause (in 1961 as mentioned above). Being ignorant at the time of the deep backstory of his history with Chapman (see Southwood, 2015), I responded that this surely had already been predicted by Chapman and Ferraro back in the 1930s. "Oh no," was his reply, "that was something completely different." He did not elaborate on the reason for this rather unexpected opinion, which was, I presume, related to Chapman and Ferraro's neglect of the interplanetary magnetic field (IMF) upstream of their cavity. Alfvén himself had proposed a "closed-field" magnetosphere model with a superposed northward-directed IMF, opposite to the "open" field geometry with a southward-directed IMF that had been discussed by Dungey, in which the solar wind plasma simply streamed continuously antisunward across the topological field boundary and through his magnetosphere (Alfvén, 1950).

I conclude this section by commenting that although Jim Dungey was a rather singular character in many respects, not famous for keeping his opinions to himself or putting people at their ease, he was nevertheless unfailingly supportive of his junior colleagues when it mattered. Though I have met very many very clever people over the years, Jim came the nearest to genius, with a physical intuition, often based on very marginal information in the early days, that uncannily almost never failed him. As illustration, I might mention another important project undertaken at Jim's suggestion at this time, namely, "Knight's theory" of field-aligned auroral electron acceleration by parallel electric fields (Knight, 1973). Stephen Knight was an MSc student who spent a year in the group under Jim's supervision before moving on, having written one of the most highly cited papers in the field (~600 citations to date on WoS). I see that Stephen was good enough to acknowledge "helpful discussions" with me at the end of his paper (as well as Jim for suggesting the topic), though my most vivid memory of him is his accidentally setting fire to his frizzy hair while "lighting up" during one my introductory talks for new group members. As indicated at the start of this memoir, tobacco was still an accepted thing in those days, Jim and Dave Southwood sporting pipes, me with Old Holborn "roll ups," and Maha with her favorite Marlboroughs—the fug slowly descended from ceiling to floor during the weekly group literature review meetings. Many years of abstinence later, it was satisfying to revisit Stephen's theory and to generalize it to the relativistic regime when it became apparent that such extreme auroral conditions occur some times during "dawn storms" at Jupiter (Cowley, 2006).

3. Postdoc Studies—Boulder, Balls Park, and Beyond

After submission of my thesis on the neutral sheet model in February 1972 and following a successful viva (conducted by Jim himself as internal examiner as was then the way, together with stellar physicist Leon Mestel as external examiner—who had also been a PhD student of Fred Hoyle at Cambridge near-contemporaneously with Jim), I applied for and was awarded a one-year Visiting Fellowship at the Cooperative Institute for Research in Environmental Sciences in Boulder, Colorado, for academic year 1972–1973. My wife and I spent a wonderful year in Boulder, exploring the Rockies, meeting new people with allied scientific interests such as Larry Lyons, Art Richmond, and Ray Greenwald (and their families), as well as people who I had met previously in London, such as Ted Speiser and Tom Holzer. Scientifically, I was mainly making initial explorations of new theoretical problems to tackle, which pretty much set me up with topics for the next several years. One significant effort I completed while there, however, was a conceptual study of exactly how the reconnectivity of field lines takes place during reconnection in a 3-D magnetosphere, modeled using the dipole plus uniform IMF paradigm introduced originally by Hoyle (1949). I recall discussing this work with Tom Holzer and George Reid in Boulder before it was published in Radio Science in a collection of papers presented at the Chapman Memorial Symposium on Magnetospheric Motions held in Boulder in June 1973 (Cowley, 1973b). Chapman, who I never met, had died in 1970. Also memorable was a series of lively discussion meetings on radar studies of the polar ionosphere led by Ben Balsley. This was a new research area that later figured prominently, centered at the time on measurements in Alaska (Chatanika) by a transportable incoherent scatter radar system originally built to remote-sense atmospheric weapons tests in the South Pacific (that were subsequently banned by international treaty in 1963), which was later relocated even further north in Greenland (Sondrestrom). Also memorable were seminar invitations by Ian Axford to La Jolla, California, and by David Beard to Kansas, as well as my first Fall AGU in San Fransisco. While it was very tempting to try a visa extension to stay in the US, after much discussion my wife and I decided to stick to our planned return to the UK, to see where that might lead.

Thus, returning to Imperial in the fall of 1973, I next succeeded in securing an ESRO Fellowship for 1974–1975 to be held in Roger Gendrin's group at the Centre National d'Études des Télécommunications in Issy-les-Moulineaux (near Paris), where Maha Ashour-Abdalla was then also working (before later moving to a lengthy career at UCLA). However, this was a time of great social and political unrest in France not conducive to productive research, and so after a few months my wife and I beat a retreat back to the UK, picking up again at Imperial in 1975 after a short and exhausting stint as a temporary lecturer at Balls Park Teacher Training College in Hertfordshire. During this period Maha and I published a number of studies examining the transport of charged particles in the closed quasi-dipolar field region (e.g., Ashour-Abdalla & Cowley, 1974; Cowley & Ashour-Abdalla, 1975). I also messed around with simple fluid models of reconnection, an examination of which suggested that reconnection could occur at a current sheet under a much broader range of conditions than hitherto supposed (Cowley, 1976).

My big career break at Imperial came with the award of a five-year Advanced Fellowship by the UK Science and Engineering Research Council in 1977, the first year these highly competitive fellowships (now called Ernest Rutherford Fellowships) were offered. On the strength of this award Head of Department Dan Bradley offered me a tenured lectureship to commence at the termination of the fellowship in 1982, thus finally securing my position. It had been a long and rather uncertain haul on short-term contracts, but by 1988 (and almost the end of this memoir) I had risen through the ranks to make full Professor, succeeding David Southwood (who led the group following Jim Dungey's retirement in 1984) as head of the Space and Atmospheric Physics group in 1990. I then remained at Imperial until moving to head the Radio and Space Plasma Physics group at Leicester in 1996, succeeding Professor Tudor Jones.

4. Being There

During my years as Advanced Fellow and junior academic at Imperial Jim Dungey pursued his interest, among a number of other topics, in the effect of instability and wave noise on the accelerated particles in a current sheet, working with a sequence of PhD students and postdocs until his retirement and disappearance from the field, though I did pitch in to help from time to time (e.g., Robertson et al., 1981; Smith et al., 1984). I spent most time, however, considering how the field properties and current sheet dynamics in an "open" magnetosphere would structure the internal magnetospheric plasma distributions, publishing a typically lengthy paper on the topic in 1980 showing how the major populations would naturally arise (Cowley, 1980). My favorite "party-trick" during seminars on the topic was to show separately how each population formed using a set of hand-drawn "viewgraph" sheets on an overhead projector, presentational state of the art at the time (earlier it had been photographically produced "slides"). Red dot regions showed the hot plasma sheet and ring current on closed field lines, green dots the solar wind, boundary layers, and mantle populations on interplanetary and open lines, and blue dots the ionospheric polar wind and plasmasphere. I then superposed all these sheets on the projector to show how they combine in a collision-free "open" magnetosphere to produce the complex overall plasma system observed. These considerations also provided expectations for the nature of the structures present on "disconnected" field lines at large distances down-tail, essentially unobserved at that time.

I also gave thought to the magnetospheric asymmetries that should occur under more general reconnection geometries than just a southward directed IMF, specifically the effect of an east-west (B_y) field component such as is generally present in the solar wind, whose field tension effects are directed east-west oppositely in the Northern and Southern Hemispheres. I showed that this should result in a coherent set of dawn-dusk asymmetries in the magnetospheric flow and plasma unique to the "open" model, which were then beginning to be detected, together with a large-scale penetration of a weakened IMF inside the magnetospheric cavity (Cowley, 1981). Observations of the latter field showed that the magnetosphere is indeed truly magnetically "open" (Cowley & Hughes, 1983; Fairfield, 1979). I believe these results had significant impact in the international space physics community on the growing acceptance of Jim Dungey's fundamental ideas, but there were still quite a number of very vocal detractors at the time, whose objections sometimes seemed irrational, ignoring of the evidence, and curiously more emotional than scientific. I never did understand where that came from.

A particular area of rational contention, however, concerned the properties of the boundary layer plasma at the Earth's dayside magnetopause. This should show accelerated, heated flows when reconnection is present but had not been detected in the high-latitude data from ERSO's HEOS-2 spacecraft, launched (with Hedgecock's magnetometer) in 1972. The next generation plasma instrumentation on the NASA/ESA International Sun-Earth Explorer (ISEE)-1 and ISEE-2 spacecraft, however, launched in 1977 as part of the International Magnetospheric Study (IMS), did often detect accelerated flows at the magnetopause that were in approximate agreement with quasi-steady reconnection (Paschmann et al., 1979; Sonnerup et al., 1981), this being a major breakthrough at the time. In addition, signatures of few-minute pulses of reconnection were observed as "flux transfer events" or FTEs (Russell & Elphic, 1979). In retrospect it became clear that HEOS-2 had earlier observed FTEs at the dayside magnetopause, but they had not been clearly understood at the time (Rijnbeek & Cowley, 1984). In 1982 I published a lengthy review of the transformational experimental progress that had taken place during the IMS, as part of which I discussed in some detail the singular properties in the ion distribution functions that should result from interaction with the magnetopause current sheet, having a telltale cutoff at the de Hoffman-Teller speed at which the reconnected field lines contract away from the reconnection site (Cowley, 1982). It would be 10 years before these predictions were confirmed, in ion data acquired in the Active Magnetospheric Particle Tracer Explorer (AMPTE) program (of which more below), first in E/Q data from the UKS spacecraft (Smith & Rodgers, 1991), and then in mass and charge-state resolved data from the CCE (Fuselier et al., 1991). In retrospect it seems clear that my strong productivity during these years (biological as well as space physical) had been released by the new-found security that allowed for extended considerations, and just how cramping previous scientific life had been a year or so at a time.

The ISEE program also had a third component, ISEE-3, launched in 1978, whose initial purpose was to monitor interplanetary conditions upstream of the Earth from a halo orbit about the Lagrange point. In 1982, however, the spacecraft was detached from this orbit to undertake a series of looping passes out to ~200 Earth radii antisunward of the Earth where it made the first detailed measurements of the more distant geomagnetic tail. The spacecraft instrumentation included an ion detector designed to study solar energetic particles, the Energetic Particle Anisotropy Spectrometer (EPAS), whose PI was Bob Hynds of the Cosmic

Rays group at Imperial, and due to my related theoretical work published in 1980, I was invited to participate in the analysis of these data. It did not take long to recognize the tailward-streaming accelerated particle regimes anticipated for the downstream region (Cowley et al., 1984), nor, working at Imperial with Ian Richardson, the embedded tailward-ejected plasmoids associated with substorm intervals that had been advocated by Ed Hones (Richardson & Cowley, 1985). At the end of 1986 ISEE-3 was redirected by lunar swing-by toward the first spacecraft encounter with a comet, 21/P Giacobini-Zinner in September 1986, then being renamed the International Cometary Explorer. Being invited by Bob to participate in the flyby jamboree at JPL, I believe I was the first to recognize the signatures of energetic water group pickup ions in the solar wind surrounding the comet in the EPAS data and recall showing first results to Ed Stone who made a tour round the gathered teams poring over their quick-look data. However, comets are another story (Cowley et al., 1991). Overall, the excitement and success of these ISEE studies encouraged me toward a future research approach involving interplay between theory and related data analysis.

Around this time I also became involved in the international EISCAT association, a ground-based project to measure ionospheric properties at polar latitudes using incoherent scatter radars, with quarter shares at that time held by the France, Germany, UK, and a Scandinavian consortium consisting of Norway, Sweden, and Finland. The UK scientific contribution was led by Henry Rishbeth at the Rutherford-Appleton Laboratory (RAL) and Phil Williams at UCW Aberystwyth, who enthusiastically solicited ideas and involvements from other UK institutions. (Aberystwyth was also home of Professor Sir Granville Beynon, who was a driving force in getting EISCAT started in the mid-1970s, and had previously played a major role in the setting up of the IGY and related World Data Center system.) Unlike the single transmitter-receiver operated at Chatanika/Sondrestrom, the new feature of EISCAT lay the tristatic design of UHF system, consisting of a transmitter-receiver at Tromsø in Norway, with further receivers in Sweden and Finland, each consisting of fully steerable 32 m dishes, which could therefore uniquely make vector measurements of the flow within a common ionospheric volume. The "common programs" foreseen for this system consisted of multipoint scans with cycle times of at least a few tens of minutes. My interest, however, was in examining the flow with much higher time resolution, with a view to determining its response to changes in the IMF at the dayside magnetopause. Such a response was already implicit, of course, in the results of Fairfield and Cahill (1966) and the many subsequent studies of high-latitude ground-based magnetometer data in relation to the IMF. To this end I proposed that the Tromsø beam should be pointed at the lowest practicable elevation, and beam-swung either side of magnetic north with an overall cycle time of 5 min, to provide latitudinal profiles of the flow over the range in which the beam passed through the F region ionosphere, near the cusp on the dayside, between magnetic latitudes of ~71° and ~75°. Although this proposal somewhat subverted the perceived "tristatic" main selling point of the new UHF system, I nevertheless received the necessary support from the EISCAT staff at RAL, led by David Willis and aided by Mike Lockwood, to set up and run this "Polar" flow program, which rapidly became one of the largest users of UK "special program" time.

A great opportunity to obtain definitive measurements arose through the simultaneous UK involvement in the US-German AMPTE program, mentioned above, which was being developed in the early 1980s during the time that the EISCAT UHF system was becoming operational. A principal goal of AMPTE was to inject traceable ions into the solar wind flow upstream of the Earth using the German Ion Release Module (IRM) spacecraft and to try subsequently to detect them inside the magnetosphere by the US Charge Composition Explorer (CCE) spacecraft. The UK was invited to instrument the thrust cone that joined the two main spacecraft in the launch stack, with particle detectors provided by Duncan Bryant's group at RAL who led the project scientifically, while the group at Imperial led by David Southwood provided the magnetometer, a refurbished ISEE-1/-2 flight spare from UCLA. For purposes of the main ion tracer experiments (though in the event no ions were ever so traced) the orbits of the IRM/UKS spacecraft pair were chosen to hover at apoapsis in the solar wind just upstream from the bow shock. For my purposes, however, this was also the ideal location at which to monitor the incoming field and flow on those orbits when EISCAT was simultaneously traversing the dayside sector on spaceship Earth, measuring the ionospheric flow with the "Polar" experiment. It is hard to appreciate at this remove of time just how operationally difficult it was to get those two systems to work simultaneously together in preinternet, pre-email days. Nevertheless, we did do so on a significant number of occasions during 1984/1985, before the demise of the UKS and then the IRM, these being some of the first fully coordinated ground-space observations to be undertaken. The results showed that the dayside flow is excited over the whole (limited) radar field of view within a minute or two of the expected arrival of southward directed fields at the magnetopause, and similarly began a ~10–15 min decay within a minute or two of a northward turning (Etemadi et al., 1988; Rishbeth et al., 1985; Willis et al., 1986). The flow indeed responded very promptly to changes in direction of the IMF, as Jim Dungey had foreseen more than twenty years previously.

It took a while to assimilate these observations and to consider their further significance. Eventually, however, building in particular on the earlier discussion by Siscoe and Huang (1985), they led to the publication of the "expanding-contracting polar cap" (ECPC or "easy-peasy") picture as an appropriate way of thinking in general terms about flow excitation and decay in the "open" terrestrial magnetosphere (Cowley & Lockwood, 1992; Milan, 2015). This then seems an appropriate point to draw this memoir on the early days to a close. I only note for the record that while Jim Dungey's open model epiphany famously occurred in 1960 on stirring a cup of coffee in a sidewalk café in Montparnasse while thinking about flow in the polar ionosphere (Dungey, 1994), my related but far easy-peasier epiphany finally occurred thirty years later while walking my dog Sally in a park in north Watford, thinking on the same topic. Good ideas can clearly come when one gets out from one's desk and into the open air.

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