

Sydney Chapman: Sedleian Professor 1946 – 1953

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Abstract

Sydney Chapman FRS was the Sedleian professor between 1946 and 1953. He was also one of the outstanding geophysicists of the 20th century. This chapter reviews his career with particular reference to his work on solar terrestrial relations.

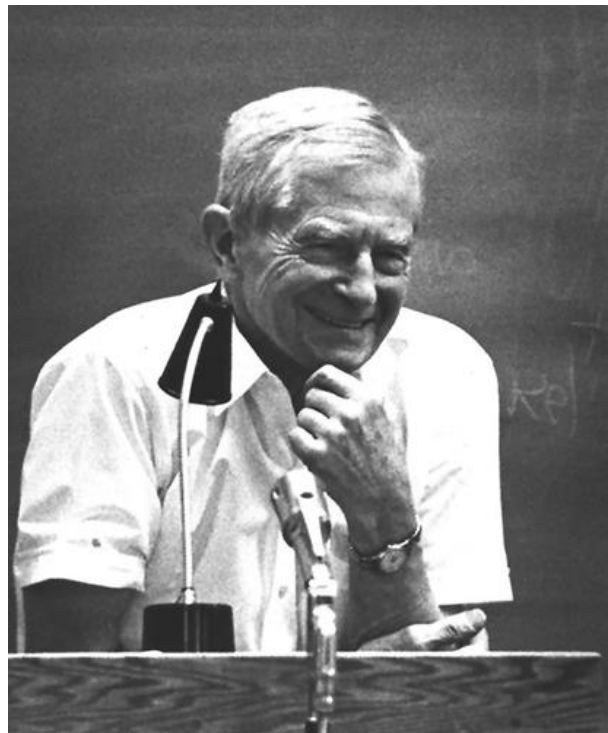


Figure 1. Sydney Chapman. On the left his formal photograph as President of the Royal Astronomical Society 1941 – 1943. On the right at the University of Alaska, dated June 1967. [Left courtesy of the RAS, right courtesy of University of Alaska.]

Introduction

Sydney Chapman was born on January 29 1888 in Eccles, Lancashire. His early years are summarised in his obituaries¹ and other aspects of his career can be found in the further reading. He was admitted

¹ See further reading

to the University of Manchester in 1904 to study engineering, graduating in 1907, but, finding mathematics more congenial, he was advised to go to Cambridge, gaining a scholarship from Trinity College in 1908, completing the mathematics degree in 1911 and being awarded a Doctor of Science in 1913². He held appointments at the Royal Greenwich Observatory, Trinity College Cambridge, Professor of Mathematics at Manchester (1919) and Head of Mathematics at Imperial College (1924). He was appointed to the Sedleian chair in 1946, retiring from Oxford in 1953: this appointment is covered in detail below. He subsequently held positions in the USA: at the University of Alaska in winter and at the High Altitude Observatory (HAO) in Boulder, Colorado in summer, where he died on June 16 1970. He was elected a Fellow of the Royal Society in 1919 and awarded many prizes and honorary degrees. He is remembered by the Royal Astronomical Society (RAS) and American Geophysical Union (AGU) through their Chapman medal and Chapman conferences respectively.

Chapman was regarded at the time of his death as one of the great figures of 20th century geophysics, an assessment that remains true³. His work on the kinetic theory of gases, the upper atmosphere, ionosphere, magnetosphere, and geomagnetic storms stand out. Another of his achievements was the initiation and implementation of International Geophysical Year (IGY) in 1957-58, which saw the birth of space exploration using satellites. All of these are discussed in this chapter. Finally, we will refer to the topic of magnetohydrodynamics (MHD) frequently. A short summary for the interested reader can be found here⁴.

1911 – 1946: Cambridge, Manchester and Imperial

While in Cambridge, Chapman was offered a post by the Astronomer Royal [(Sir) Frank Dyson⁵] as a chief assistant at Royal Greenwich Observatory (RGO). Chapman then returned to Cambridge with a five year Trinity fellowship, though also spending time at RGO. He received an exemption from conscription during the First World War due to his pacifist views. At RGO the Astronomer Royal had asked him to lead the construction of a new magnetic observatory and this was his introduction to a

² Cowling's obituary does not comment on how he became Dr Chapman but the first use of D.Sc was on a paper published in the autumn of 1913, the year that Chapman was the winner of the prestigious Cambridge mathematics Smith's essay prize. J. Barrow-Green in 'A Corrective to the Spirit of too Exclusively Pure Mathematics': Robert Smith (1689-1768) and his Prizes at Cambridge University, *Annales of Science*, 56 (1999), pp 271- 316 discusses the importance of this prize in Cambridge mathematical circles and one may assume that this had some impact on the award of his D.Sc. The PhD as known today did not exist in Cambridge in 1913.

³ His obituaries provides his complete publication list of over 400 papers covering a wide range of topics.

⁴ A plasma is an ionised gas where electromagnetic forces are dominant. Plasmas can have low ionisation (e.g. the solar photosphere and the ionosphere) or be fully ionised (e.g. the solar corona, solar wind and magnetosphere). A plasma can be described by the distribution functions of its components through the seven dimensional (3 space, 3 velocity space and time) Boltzmann equation which is intractable without assumptions. Taking the first three moments of the Boltzmann equation leads to the equations of magnetohydrodynamics (MHD) which reflect the conservation of mass, momentum and energy with closure through an equation of state: they are the equations of hydrodynamics extended to include the Lorentz force. The MHD equations are useful for describing large-scale properties but are analytically tractable only in highly symmetric situations, and in general their solution relies on computational modelling. Note that the formal framework of MHD was only established in the late 1930s and 1940s by, amongst others, T.G. Cowling and H. Alfvén.

⁵ Titles in parentheses such as (Sir) denote the award was made after the time being discussed.

lifetime interest in geomagnetism⁶. There was also work on stellar observations, a series of papers on the kinetic theory of gases and his initial model for geomagnetic storms as discussed below.

Chapman's methodology was well established by 1920. He was a pioneer in the use of large data sets, as demonstrated by his early work on the solar and lunar aspects of geomagnetic activity⁷. But he was, at heart, a mathematician, reducing complex problems to ones that could be solved with the tools available at the time. Sometimes this led him up the wrong path, as in the models for the currents associated with geomagnetic activity. In these older papers, at times he wrote with great clarity, such as the 1931 Bakerian lecture on Ozone, and his pioneering work on the magnetosphere, when he expressed the basic idea in a single page in *Nature*⁸. But other papers can be harder work to read, in part due to their length, and one of the merits of his two-volumes published with Bartels⁹ is that when topics were revisited, a greater clarity was evident.

One should not under-estimate the difficulties confronting Chapman and others. Computers as we know them were non-existent, making both calculations and plotting difficult tasks. Computational modelling in plasma physics and MHD did not arrive until the late 1960s, so everything involved some sort of approximation. In the late 1930s and 1940s the physics framework needed to understand geomagnetic storms did not exist. These were formidable handicaps and it is to Chapman's credit that he got quite a lot right. The data limitations were also severe, being entirely ground-based¹⁰.

S.I. Akasofu who was a close collaborator with Chapman after 1960 wrote:

"Although Chapman had many deep insights into physical processes, he tended to become an applied mathematician when he encountered mathematical uniqueness issues. Mathematical rigor was his life and it was part of the reason which caused some friction with Alfvén...."¹¹.

T. Gold also comments on Chapman's reactions to Alfvén's formalism for MHD:

"Many others, especially those with a puritanical mindset like Sydney Chapman rejected these (ideas) out of hand. I had many discussions with Chapman and he insisted that nothing short of a complete calculation could ever be used to solve a problem in MHD: as Chapman put it, "You might be misled terribly by such approximations""¹².

Indeed we are still trying to do complete MHD calculations on very large computers with approximations that perhaps do not always bear scrutiny. Perhaps Chapman had a point!

After five years in Manchester Chapman moved to London to become head of the Imperial College mathematics department. He was given the task of reviving the department from a low spell. Once this had been accomplished, people who had subsequent successful careers moved through the department

⁶ Chapman had more success with geomagnetic observations at RGO than another distinguished scientist. Tom Gold writes that in 1956 after constructing a solar cosmic ray observatory, he was told by the Astronomer Royal [(Sir) Richard Woolley] to "dismantle the building forthwith since such observations were not part of astronomy...." (see footnote 12 for citation). This led to Gold's move to the USA.

⁷ The Chapman and Bartels volumes have a good description of this work. See footnote 9.

⁸ S. Chapman and V.C.A. Ferraro, A new theory of magnetic storms, *Nature*, 126, (1930) 139-140.

⁹ S. Chapman and J. Bartels, *Geomagnetism*, Vol 1&2, (1940), OUP.

¹⁰ Space-based measurements arrived in 1958.

¹¹ See Akasofu (2003), page 274.

¹² T. Gold, Early times in the understanding of the Earth's magnetosphere, in Gillmor and Spreiter (1997), 77-82: page 78. The final sentence uses Chapman's words.

including George Temple (1901 – 1992) who was Chapman’s Sedleian successor at Oxford in 1953, (Sir) Bill McCrea (1904 – 1999) a distinguished astronomer and (Lord) Bill Penney (1909 – 1991), leader of the UK atomic bomb project and subsequently rector of Imperial College. Chapman had also married in 1922 and established his family in Wimbledon, Chapman cycling to work on occasions: his bicycling exploits were legendary¹³.

In these years Chapman began his involvement with UK and International scientific organisations such as the International Union of Geodesy and Geophysics (IUGG). He led the UK delegation to the IUGG general assembly in Washington between 4 and 11 September 1939 and was able to maintain contact with his friend Julius Bartels until the end of the assembly (Chapman, 1965)¹⁴. Chapman was also involved with the RAS. He was the president from 1941 – 1943, the first president in the 20th century of whom it could be said unequivocally to represent geophysics. As was customary until 2014, he was nominated by his predecessor (H.C. Plummer) and in 1943 Chapman nominated his old friend E.A. Milne as his successor. The war years were not the time to attempt major changes in the RAS (however much it may have needed them) but D.H. Sadler in Tayler’s history of the RAS¹⁵ draws attention to efforts in these years to formalise various procedures of Council and the meetings of the fellows.

Theory of non-uniform gases

Before looking at Chapman’s time at Oxford, we present in detail the major achievements of the first part of his career. The first concerns the kinetic theory of non-uniform gases, triggered by discussions with Sir Joseph Larmor, and which led to several lengthy papers¹⁶. Non-uniform here means that there are spatial gradient of, for example, the temperature and velocity. The basic theory of gases was developed in the 2nd half of the 19th century, leading to the Boltzmann equation and the Maxwell-Boltzmann distribution function. The moments of the Boltzmann equation lead to the hydrodynamic (or MHD) equations. These hydrodynamic equations also involve terms that arise from an integration over the collision terms in the Boltzmann equation, leading to expressions for viscosity, thermal conduction, electrical resistivity and thermal diffusion when more than one gas species is present, generally referred to as transport coefficients. The difficulty in solving the problem lies to a degree in what model is used for the molecules in the gas, and can lead to different scalings of the coefficients with temperature.

Chapman’s work involved the study of a strongly collisional gas, as defined by the Knudsen number (K_n) being small¹⁷. He used solutions of the Boltzmann equation to obtain estimates for the transport coefficients, although (Sir) James Jeans who was the first referee of Chapman’s 1912 paper felt that these results lacked general validity. This weakness was dealt with in a later paper to the eventual satisfaction of Jeans who again acted as the referee. In parallel, David Enskog in Sweden undertook a

¹³ Akasofu (2020)

¹⁴ S. Chapman, Obituary notices, Julius Bartels, *QJRAS* 6 (1965), 235-245. Bartels was at the Carnegie Institute of Terrestrial Magnetism at the time and in 1940 made his way home to Germany the long way: via Japan and the Trans-Siberian Railway.

¹⁵ R.J. Tayler, *History of the Royal Astronomical Society Volume 2, 1920-1980* (1987), Blackwell.

¹⁶ For example S. Chapman, The Kinetic Theory of a Gas Constituted of Spherically Symmetrical Molecules, *Phil. Trans. Roy. Soc. A.* 211, (1912), 433-483. S. Chapman, On the Law of Distribution of Molecular Velocities, and on the Theory of Viscosity and Thermal Conduction, in a Non-Uniform Simple Monatomic Gas, *Phil Trans. Roy. Soc. A.* 216, (1916), 279-348

¹⁷ $K_n = \lambda_{mfp}/L$ where λ_{mfp} is a typical mean free path and L is the characteristic scale of the temperature, density or velocity gradient.

different calculation that reached the same results: the expansion procedures are known as the Chapman-Enskog method¹⁸.

In the late 1920s, Chapman began to pull these ideas together with the view of writing a book and at this stage, T.G. Cowling appears¹⁹. He was under the supervision of E.A. Milne at Oxford in 1928, but Milne approached Chapman for a problem that Cowling could work on. Chapman had earlier claimed there was a radial limitation to the solar magnetic field, however on working through Chapman's paper, Cowling realised that his result was incorrect²⁰, which impressed both Milne and Chapman and led to Cowling moving to Imperial College. Cowling's involvement with Chapman's proposed book began as follows²¹:

"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 time than he. 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 my attention to a number of developments that had occurred since his manuscript had been written. When I had largely completed my task in 1935, I had to wait over a year for him to go through the new manuscript and suggest improvements. Our joint book came out in 1939"

This is the famous Chapman and Cowling book "The mathematical theory of non-uniform gases"²², one of Chapman's great achievements. It is his best-cited piece of work: the updated 1970 edition currently averages around 40 citations a year on the Astrophysics Data System, a testament to its longevity. The book uses Enskog's approach to calculate the transport coefficients rather than Chapman's. Chapman and Cowling in their Introduction freely acknowledge the greater clarity of Enskog's methods, a generous gesture from someone of Chapman's standing, in effect saving Enskog's work²³. Cowling also benefitted from this collaboration, subsequently writing important papers concerning electrical conductivity in the presence of a magnetic field.

Ozone and Chapman layers

¹⁸ Enskog's work was published as a Swedish dissertation in 1917 and subsequently in German journals. He was a school teacher for 13 years, before becoming a Professor in Stockholm in 1930 (S. Chapman, Prof. David Enskog, *Nature*, 161, (1948) 193-194).

¹⁹ T.G. (Tom) Cowling (1906 – 1990) collaborated extensively with Chapman in the 1930s on their classic textbook (Chapman and Cowling, 1939). He studied for a PhD with E.A. Milne in Oxford and subsequently was taken on by Chapman at Imperial. He held subsequent posts at University College Swansea, University College Dundee (subsequently Queens College, Dundee, and part of the University of St Andrews until 1967), Manchester and Bangor before finally settling as professor of applied mathematics in Leeds. He was one of the founders of MHD and made major contributions to ionospheric conductivity, sunspots, stellar structure and planetary dynamos. He is the author's "academic grandfather". His career is summarised in Cowling (1985) and L. Mestel, T.G. Cowling, *Biographical Memoirs of Fellows of the Royal Society*, 37 (1991).

²⁰ T.G. Cowling, On the radial limitation of the Sun's magnetic field, *MNRAS*, 90 (1929), 140-154. Chapman continued to have a "blind spot" about the existence of a large-scale magnetic field filling the heliosphere well into the 1950s.

²¹ Quoted from Cowling (1985), page 7.

²² S. Chapman and T.G. Cowling, *The mathematical theory of non-uniform gases*, CUP (1970)

²³ The book is not for the timid. One approach for those interested in just the results and the comparison of theory and experiment would be to look at Chapters 1 – 6 and then 12 – 14 in the 1970 edition. Although plasma transport is discussed in the final chapter, S.I. Braginskii, Transport processes in a plasma, *Reviews of Plasma Physics*, 1 (1965), 205 is more satisfactory.

Chapman's work on the formation of ozone is well described in his 1931 Royal Society Bakerian lecture²⁴. Making use of recent data concerning the presence of ozone and higher ionised layers (Chapman layers), he proposed a series of reactions that led to the formation of ozone, confined to a layer a few tens of kilometers above the Earth's surface (Figure 2).

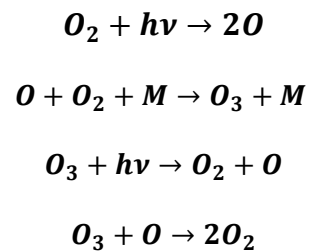
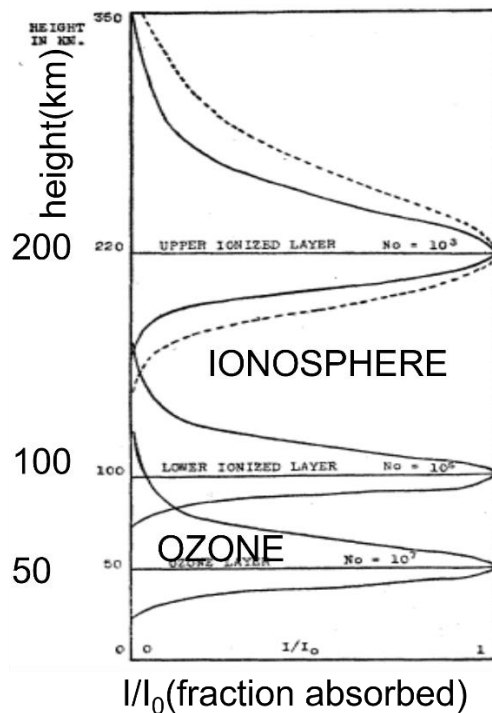


Figure 2 Left: the fraction of incident UV radiation absorbed as a function of height. The three different ionised (Chapman) layers are shown and correspond to different wavelength bands. The lowest is where ozone is formed. Right: the reactions involved. $h\nu$ is a UV photon and M a catalyst. (Adapted from S. Chapman, Proc. Roy. Soc, 152, 353, 1931)

Considering solar UV radiation with wavelengths around and longer than the then-observable lower limit of 290 nm Chapman argued that the incident radiation led to a series of reactions involving the creation and destruction of ozone level (right hand column of Figure 4) that led to an equilibrium estimate of its level. Here M is a catalyst such as a molecule. The first reaction uses photons with wavelengths < 240 nm to create oxygen radicals from an oxygen molecule, the second leads to the formation of ozone, the third uses longer wavelength photons (320 nm) to destroy ozone while the fourth also removes ozone and creates molecular oxygen.

The modelling has been updated in the past decades, as well as the ozone layer being lower in the atmosphere than Chapman proposed, yet the basic ideas remain. The upper two layers correspond very roughly to the present E and F layers of the ionosphere and Chapman noted that they would need to be formed by different radiation from the ozone layer. The relevant wavelengths are in the Extreme Ultraviolet (EUV) and Chapman outlined some possible processes due to EUV radiation in his paper, though today's picture is more complex.

Chapman and geomagnetic storms

²⁴ S. Chapman, Some Phenomena of the Upper Atmosphere, *Proc. Roy Soc A*, 132 (1931), 353-374

Geomagnetic activity in the form of the aurora has been known for millennia while in the 19th century the ability to measure perturbations to the Earth's magnetic field led to the idea that currents would flow in the very upper atmosphere (the ionosphere). Modern studies began on September 1 1859 when, at 11:15 GMT, Richard Carrington and Richard Hodgson observed a large solar flare. A few minutes later, magnetometers at Kew observatory recorded a modest change²⁵. 18 hours after the flare, a large disturbance in the Earth's magnetic field was recorded. Now known as "The Carrington Event", this geomagnetic storm was the first recorded event that demonstrated the link between solar and geomagnetic activity²⁶. The initial magnetic field change on Sept 1 was due to the abrupt ionisation of the very upper atmosphere, leading to localised electric currents. The arrival of the interplanetary disturbance on September 2 produced first a magnetic field enhancement and subsequently a deep depression of order 400 nT²⁷. The average velocity between Sun and Earth of whatever caused the main storm phase was in excess of 2000 km/s. The direction of the currents associated with the field disturbances can be deduced from Ampere's law. The initial enhancement on September 2 is due to a current flowing in the West/East direction and the depression due a current in the opposite direction. Seen from above the poles, these currents are counter-clockwise and clockwise respectively, and are indicated in a modern sketch (lowest panel of Figure 9) as the magnetopause and ring currents

A modern example of a major geomagnetic storm is shown in Figure 3, the cause being the "Bastille day" flare of 14 July 2000. The left upper panel shows the Sun's corona (the hot, 1 MK outer atmosphere) as seen in the extreme ultraviolet with the flare at disc centre. The right upper panel shows an artificial eclipse created by a coronagraph on the SOHO spacecraft. The field of view extends to 30 R_S (2.1×10^7 km, where R_S is the solar radius). The lower panel shows a spatial average of the change in the surface magnetic field, as defined by the storm (D_{st}) magnetic index²⁸. The eruption began with a major flare at 10:24, which in turn expelled part of the corona into interplanetary space. This is termed a Coronal Mass Ejection (CME) and is the faint halo in the centre of the middle image: the "snowstorm" is due to mildly relativistic particles accelerated in the eruption hitting the instrument detectors. The CME is the agency by which the solar disturbance is communicated to the Earth, as would have been the case in the Carrington event.

²⁵ The Kew magnetic field measurements were reconstructed in 1938 by J. Bartels, Solar eruptions and their ionospheric effects – classical observation and its new interpretation, *Terr. Mag.* 42 (1937), 235 - 239 to which the reader is referred.

²⁶ See H.S. Hudson, Carrington Events, *ARA&A*, 59 (1971), 445-477.

²⁷ The undisturbed magnetic field magnitude at Kew is of order 47500 nT or 0.475 Gauss, so these perturbations are of order 1%.

²⁸ The storm index, D_{st} (units nanotesla) is commonly used to measure the strength of magnetic storms. It is derived from multiple near-Equatorial measuring stations.

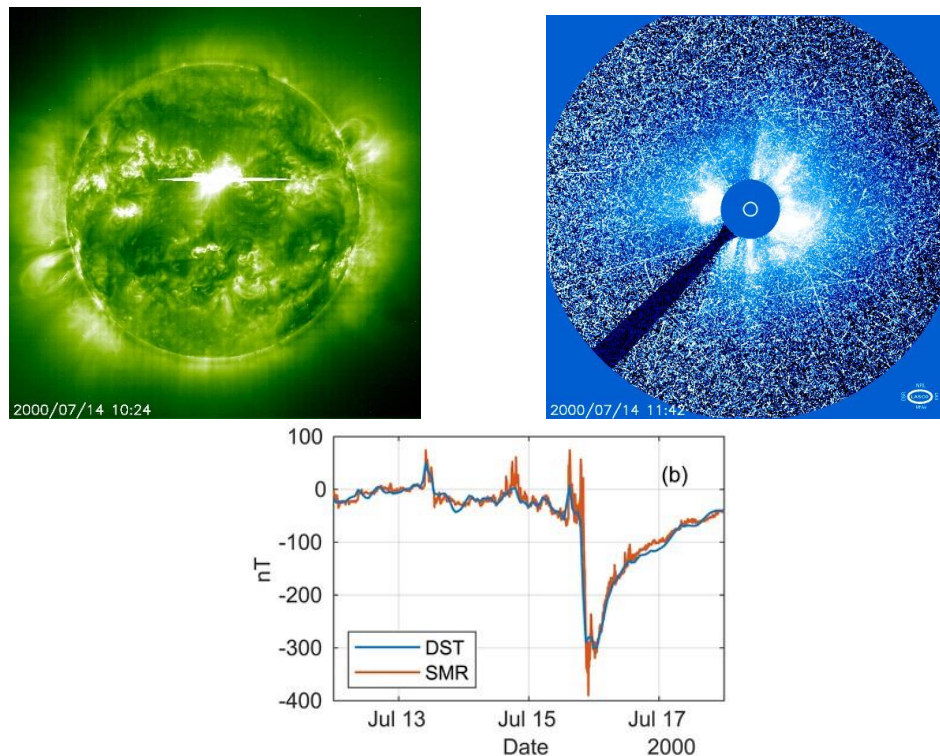


Figure 3. The Bastille day flare and associated geomagnetic storm. Upper left and right show the Sun in EUV with the flare in the centre and a coronagraph image showing the CME (faint halo). The lower panel shows the disturbance to the Earth’s magnetic field. [The upper two panels were obtained by instruments on the Solar and Heliospheric Observatory, a joint project of the European Space Agency and NASA and are reproduced courtesy of the Extreme Ultraviolet Imaging Telescope and the Large Angle and Spectrometric Coronagraph Experiment instrument teams respectively. The lower panel shows the magnetic D_{st} index and an alternative index proposed by Bergin and collaborators²⁹.]

In the early 20th century, the Norwegian scientist Kristian Birkeland (1867 – 1917) made major contributions to geomagnetism with work published in the Proceedings of Norwegian Polar Expeditions. He proposed that the aurora, and associated surface magnetic field changes were due to electric currents in the upper atmosphere. To address the origin of such currents, he conducted an experiment where a beam of electrons was fired at a terrella³⁰, and it was found that they concentrated in regions around the poles. He then extended his experimental result to suggest that a stream of solely electrons emanating from the Sun caused the aurora³¹. Such a stream cannot exist due to electrostatic repulsion, but Birkeland revised his model to have a neutral stream (a roughly equal number of electrons and protons). He suggested that a large-scale circuit would be set up, with electrons streaming into the auroral region along the terrestrial magnetic field, and closing through a cross-field current in the ionosphere (the Birkeland current, see upper left panel of Figure 4). Thus, by 1910 a picture of geomagnetic activity being associated with solar activity and having some sort of large-scale current flow in near-Earth space had been proposed. Birkeland’s early death led some to see his work as being “forgotten”: perhaps it is better to say that it did not receive the credit it merited.

²⁹ A. Bergin et al, AE, DST, and Their SuperMAG Counterparts: The Effect of Improved Spatial Resolution in Geomagnetic Indices, *JGR*, 125, (2020), <https://doi.org/10.1029/2020JA027828>

³⁰ A terrella is a small, magnetised model of the Earth. It originated with William Gilbert in 1600 and was used extensively by Birkeland.

³¹ For historical background, see Southwood (2017)

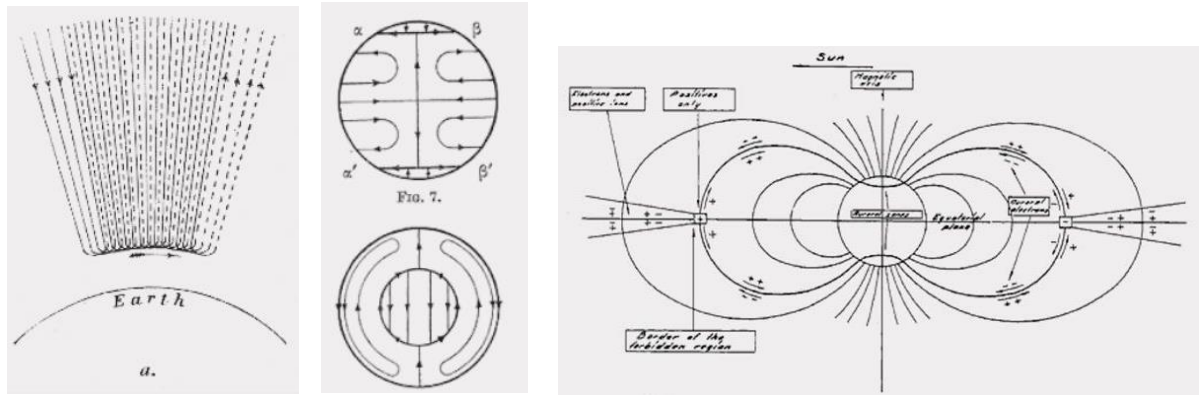


Figure 4. Three possibilities of storm-associated electric currents referred to in the text (adapted from Figure 3 of de Moortel et al, 2020). The upper left plot shows Birkeland’s scenario. Currents flow Earthward (solid lines) and outward (dashed lines) along the Earth’s magnetic field. The currents close across the magnetic field in the ionosphere (lower arrow). The central plot shows an example of Chapman’s current scenario. The upper and lower panels are the current system as viewed from the Sun and the poles respectively (Chapman, 1927). The right panel shows Alfvén’s model including field-aligned currents indicated on the right.

We mentioned earlier Chapman’s initial interest in geomagnetic activity with studies of solar and lunar diurnal variations attributed to atmospheric tidal-induced ionospheric currents. Involving the study of large data sets, this interested him for most of his life. However in 1918 he proposed a theory of larger (geo)magnetic storms linked to major solar activity, which he argued were separate from the tidal induced variations³². Like Birkeland, an important aspect was the idea of a stream (or beam or cloud) of solar particles being responsible for the storm. Also like Birkeland, Chapman proposed that the solar stream must involve charged particles of the same sign, arguing that otherwise recombination of positive and negative charges would lead to a neutral gas. Lindemann³³ noted that it is the impossible for a stream of single charges to survive in the interplanetary medium due to electrostatic repulsion between the particles. Instead, Lindemann proposed a stream with a roughly equal number of positive and negative charges (a plasma). This is consistent with a plasma being quasi-neutral with an almost (but not exactly) equal number of positive and negative charges. He also refuted Chapman’s argument concerning the formation of a neutral gas. Lindemann suggested that the beam/cloud is accelerated by radiation pressure, attaining velocities of several hundred km/s.

Through the 1920s Chapman continued to develop a picture of geomagnetic activity based on large data sets and characterised by various indices and in particular calculated the ionospheric currents using a simple shell model (see Figure 4 for an example). Unlike Birkeland, there is no connection to solar particles and the current patterns show smooth large-scale properties. The nature of storms as individual, distinctive events was neglected. By 1929 he was ready to return to the theoretical aspects of geomagnetic storms. This was in collaboration with a student at Imperial College, Vincent Ferraro³⁴.

³² S. Chapman, An Outline of a Theory of Magnetic Storms, *Proc. Roy. Soc. A*, 95 (1918), 61-83

³³ F.A. Lindemann, A note on the theory of magnetic storms, *Phil Mag.* 38 (1919) 669-684. Frederick Lindemann (1886 – 1957) was created Baron (subsequently Viscount) Cherwell in 1941 and 1956 respectively. To avoid confusion, I refer to him as Lindemann.

³⁴ Vincent Ferraro (1907 – 1974) was the main collaborator with Chapman on his theory of magnetic storms. He began working at Imperial College in 1927 and eventually contributed enormously to the theory: an excellent source for Ferraro’s contribution is T.G. Cowling, Vincent Ferraro as a pioneer of hydromagnetics, *QJRAS*, 16, (1975), 136-144. Moving onto what was then called hydromagnetics (now MHD), he obtained what is now called Ferraro’s law of isorotation in 1935. He moved to Kings College

The initial announcement of the work appeared in *Nature*³⁵ followed by a series of papers which were in effect one long paper split into five parts with a length of 76 pages³⁶. Chapman was the lead author on all the (sub-)papers, though Ferraro was listed as the author of Section 9 in the 2nd 1932 paper. The initial discussion benefits from the Figure that appears in their *Nature* paper, reproduced here.

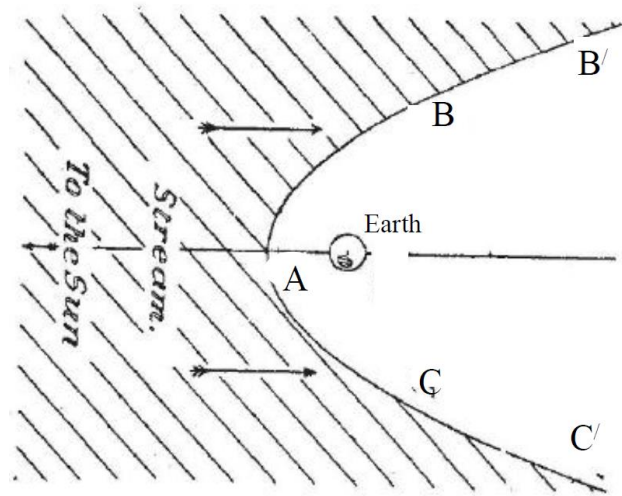


Figure 5 The Chapman-Ferraro model (*Nature*, 1930, partly redrawn). The Sun is at the left of the Figure, the Earth in the middle, and its magnetic field carves out a cavity in the solar stream.

Having taken on board Lindemann's criticism, a transient quasi-neutral unmagnetised fully ionised stream approaches the Earth, travelling with a velocity of 1000 km/s through interplanetary space and with a plasma density at the Earth of 60 particles cm^{-3} . [These values are a bit larger than contemporary measurements for storm-causing solar events, but not excessively so.] Since the electrical conductivity is effectively infinite, the solar stream cannot penetrate the dipolar terrestrial magnetic field, and compresses it on the dayside. Thus a boundary (B'BACC'), which is a layer of electric current, is set up between the solar plasma and terrestrial field a few R_E (where $R_E = 6390$ km is the radius of the Earth) on the dayside and a cavity is carved out by the terrestrial field in the solar stream, the cavity now being called the magnetosphere. This initial compression of the terrestrial field accounts for the observed positive excursion of the surface field at the start of the storm. The estimate of an upstream distance of 5 R_E for the boundary is a bit smaller than today's estimates, but again the difference is not excessive³⁷. This part of Chapman and Ferraro's model is of the highest importance and is the foundation of the concept of a magnetospheric cavity as well as opening an entirely new aspect of solar-terrestrial relations.

The first four longer papers focus on the dayside boundary layer and today add little to what was said in the *Nature* letter. In places they are a good example of Chapman's mathematical approach in reducing a 3D problem with an (analytically) undeterminable boundary shape to a 2D slab with an infinitely long

London, then to University College of the Southwest (now University of Exeter) and finally to Queen Mary College in 1952, and died in early 1974 after some years of illness.

³⁵ S. Chapman and V.C.A. Ferraro, A new theory of magnetic storms, *Nature* 126 (1930), 129-130.

³⁶ S. Chapman and V.C.A. Ferraro, A new theory of magnetic storms, *Terr. Mag.* 36, 77-97 (1931a), 36, 171-186 (1931b), 37, 147-156 (1932a), 37, 421-429 (1932b), 38, 79-96 (1933).

³⁷ The thickness of the boundary is determined by the need for quasi-neutrality with modern models and simulations suggesting a thickness of a few 10s of km. See for example P.J. Cargill and T.E. Eastman, The Structure of Tangential Discontinuities: 1. Results of Hybrid Simulations, *JGR*, 96, (1991), 13763-13779.

and wide boundary between the solar stream and terrestrial field. It is only in the fifth paper that the key question of the main phase of a storm arises. It is argued that an (ion) current flows between the surfaces BB' and CC' forming a “current ring”, though significant questions were left unanswered, especially how this relates to the 2D currents calculated by Chapman elsewhere. This part of the model is best forgotten.

Chapman returned to much of this work in his two-volume “*Geomagnetism*” co-authored with Julius Bartels³⁸. The origin of these volumes lay in Chapman’s winning submission for the 1928 Adams prize essay sponsored by the Cambridge department of mathematics and St John’s College. Thereafter, Chapman and Bartels agreed to collaborate on these volumes which took almost a decade to see publication. Over 80 years later, the books read well and in the view of the author possess a clarity not seen in the original papers by Chapman. They also address the work of Birkeland³⁹. The preface is dated 1940, and is unusual in that the nations of the two authors had been at war with each other for several months.

³⁸ Julius Bartels (1899 – 1964) studied at Gottingen then taught meteorology at the Forestry High School in Eberswalde. From 1930 he was associated with the Carnegie Institute of Terrestrial Magnetism in Washington DC and in 1936 became director of Potsdam Geophysical Institute. He met Chapman in the 1920s and collaborated on the 1940 two-volume “*Geomagnetism*”. In 1945 he and his wife fled to Gottingen where after some inquiries about his relationship with the Nazis, he was appointed director of the Geophysical Institute and subsequently the Max Planck Institute for Aeronomy in Lindau. He made many important contributions to geomagnetic measurements. [Adapted from S. Chapman, Julius Bartels, *QJRAS*, 6, (1965), 235-245.]

³⁹ The two volumes are one book. The index, references and many tables are all in volume 2. They were reprinted in 1951 and 1962 and can be found second hand.



Figure 6 3 collaborators and a critic. Clockwise from top left: Cowling, Ferraro, Bartels and Alfvén. (Credits: Mestel (1991), Journal of Plasma Physics, European Geosciences Union, Pease and Lundquist, 1998)

Alfvén as a critic

The Swedish scientist and Nobel laureate Hannes Alfvén received his PhD in 1934 and worked on a variety of topics throughout his career⁴⁰. He developed a theory of geomagnetic storms in 1939, submitting the paper to *Terrestrial Magnetism and Atmospheric Electricity* (a journal of the AGU, subsequently incorporated into *Journal of Geophysical Research* and hereafter referred to as *Terrestrial Magnetism*). The paper was rejected⁴¹ and Alfvén would have suspected that the referee was one of

⁴⁰ Hannes Alfvén (1908 – 1995) was the Nobel Physics Laureate in 1970 for his fundamental work on plasma physics. He began his career in Uppsala, subsequently moving to Stockholm and the Royal Institute of Technology in 1940 (see R.S. Pease and S. Lundquist, Hannes Alfvén, *Biographical Memoirs of Fellows of the Royal Society*, (1998) and C.G. Falthammar, The Scientific Legacy of Hannes Alfvén, *EOS*, 93 (2012), 201). Along with Cowling, he was a pioneer of MHD, with his work on the “frozen flux theorem” and the Alfvén wave (see de Moortel et al, 2020). He subsequently spent two decades at University of California San Diego and proposed “new paradigms” for plasma physics (Alfvén, *Cosmic Plasma*, (1981), Reidel; S. Brush, Alfvén’s programme in solar system physics, *IEEE Trans Plasma science*, 20 (1992), 577).

⁴¹ See D. P. Stern, Forum, *EOS*, 45 (2003), 488 and de Moortel et al (2020).

Chapman's circle. The work subsequently appeared in the *Proc. Royal Swedish Academy*, a journal now defunct. The AGU published a synopsis of the important aspects in 1970⁴².

Alfvén's papers, like those of Chapman and Ferraro, introduces important concepts. These include particles both "trapped" in the terrestrial magnetic field as well as drifting around the Earth as a ring current, and the presence of a 3D current system connecting the particle "belts" and the ionosphere/aurora along the lines of Birkeland (Figure 4). All these aspects have subsequently been proven to be present although the way both sets of authors argued how the "ring current" formed does not stand up to inspection. The required tools and data did not exist in 1940. However, Alfvén's model did not include the magnetospheric cavity of Chapman and Ferraro and required solar particles with rather a high energy.

Chapman did not respond to Alfvén's paper, but a comment came from Cowling who wrote a short paper⁴³ at Chapman's instigation:

"Chapman sent me copies of two of Alfvén's papers..... 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. His idea was that I should produce a critical review of the papers..... The review that finally appeared in *Terrestrial Magnetism* in 1943 (*sic*) 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."⁴⁴.

The paper presents a brief but fair summary of Alfvén's work but is severe on the field-aligned currents. It is not difficult to imagine Alfvén's reaction. His papers submitted to *Terrestrial Magnetism* has been rejected and yet here in the same journal was Chapman's colleague criticising his model! The delay in the appearance of Alfvén's ideas in a widely-read journal is to be regretted.

Although Alfvén attempted to discuss their respective models with Chapman after the war, the latter remained reticent, not enjoying controversial discussions⁴⁵. Alfvén was then silent on storms until 1951 when he published a scathing note in *Nature*⁴⁶, responding to a short article by David Martyn on the Chapman-Ferraro model⁴⁷. In the first paragraph Alfvén states

"It is not my intention to review here the objections to this theory (Chapman – Ferraro) - objections which I believe to be fatal - nor is it worth while to discuss the curious superstructure which Dr. Martyn tries to erect on this weak ground."

Martyn responded robustly while Chapman was more polite. Alfvén followed this up with papers in the 1950s and it was difficult to see any peaceful resolution⁴⁸. This 1957 paper also criticises Chapman (correctly) for neglecting the interplanetary magnetic field: "It is pointed out that the fundamental

⁴² A.J. Dessler and J. Wilcox, A theory of magnetic storms and of the aurorae, *EOS*, 51, (1970), 180.

⁴³ T.G. Cowling, On Alfvén's theory of magnetic storms and of the aurora, *Terrestrial magnetism*, 40 (1942), 209-214.

⁴⁴ Quotation on page 11 of Cowling's 1985 memoir. Why Cowling agreed to write this article is unclear. Chapman was very productive during the early war years and could have done this himself. I assume Cowling was unaware of the history of Alfvén's submission to, and rejection by, *Terr. Mag.*

⁴⁵ An entertaining episode is described in Southwood (2015).

⁴⁶ H. Alfvén, The Theory of Magnetic Storms and Auroras, *Nature*, 167 (1951) 984.

⁴⁷ D. F. Martyn, The theory of magnetic storms and aurora, *Nature*, 167 (1951a), 92-94.

⁴⁸ For example H. Alfvén, On the theory of magnetic storms and aurorae, *Tellus*, 10 (1957), 104. This paper contains a generous acknowledgment of helpful discussions with Cowling. But any channel of communication with Chapman seems to have been abandoned.

assumption about a non-magnetized beam is in conflict with cosmic ray evidence” and here Alfvén uses the language of magnetohydrodynamics (MHD), something Chapman never seemed to feel comfortable with. Incidentally Alfvén’s 1951 objections related to the need to verify any theory with experiment, such as those conducted by Birkeland, who Alfvén regarded highly. Chapman on the other hand held the opposite viewpoint about Birkeland. There are accusations that Birkeland’s work was ignored but in fact Chapman and collaborators give an account of it⁴⁹. At that time they formed the opinion that the 2D currents in Chapman’s work fitted the available magnetic field data better than Birkeland’s 3D scenario, although whether Chapman should have retained this viewpoint for a further three decades is another matter. And so arguments raged between “the Nordic school” and “the Chapman school”. Lest the reader think that this is just another academic disagreement, this was an argument between two gigantic scientific figures of the 20th century, with others of comparable status such as Cowling being involved. Personality differences did not make any resolution easier.

Eventually in 1966 space based observations suggested that the currents proposed by Birkeland did exist⁵⁰, and, with the encouragement of A.J. Dessler, quite suddenly the paradigm changed. Southwood⁵¹ documents an incident at a symposium commemorating Birkeland’s birth centenary in 1967 at which Chapman was a speaker and comments on how he, to the astonishment of the audience, belittled Birkeland’s contributions. In fact, Chapman gave a fair assessment of Birkeland, but rather ruined things by stating at the end of his written article that “Birkeland made little direct contributions to our observational knowledge of the aurora”, though crediting “indirect influence”! If nothing else, this was bad manners. It does though seem that towards the end of his life Chapman began to realise he had missed some aspects of Birkeland’s work, especially the three-dimensional currents⁵². Incidentally, it strikes the author as remarkable that for two decades after 1940, Chapman published no significant developments of his original model.

Sadly these disputes continued for long after Chapman’s death in 1970. In 1985 Cowling wrote⁵³:

“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 (*emphasis added*).”

This was 43 years after Cowling’s paper was published at a time when new observations had rendered much of the debate pointless. Cowling was regarded as a kindly though austere person, but the reason for this comment is clear. On pages 1 and 2 of his monograph “*Cosmic Plasma*”⁵⁴, Alfvén launches into another criticism of the Chapman – Ferraro theory, with other comments on the theory later in the book. The book also criticises the approach of many other scientists, though not by name. After his Nobel prize, Alfvén advocated various concepts that were not at the time part of mainstream plasma physics, some being of interest, and also indulged in bouts of iconoclasm. Cowling reviewed the book in stark terms, though others praised Alfvén’s approach⁵⁵.

⁴⁹ See E.H. Vestine and S. Chapman, The electric current system of geomagnetic disturbances, *Terr. Mag*, 43 (1938), 351-382 and the volumes due to Chapman and Bartels.

⁵⁰ A.J. Zmuda et al, Transverse Magnetic Disturbances at 1100 Kilometers in the Auroral Region, *JGR* 71, (1966) 5033-5045

⁵¹ Southwood (2015, 2017)

⁵² Akasofu (2003)

⁵³ Cowling (1985), page 11.

⁵⁴ H. Alfvén, *Cosmic Plasma*, Reidel, (1981).

⁵⁵ T.G. Cowling, A review of “*Cosmic Plasma*”, *GAFD*, 21 (1982), 324. The author also read this book in the early 1980s and was unimpressed. Re-reading it in 2022, his reaction is even more negative.

In the past decades, some historians have presented a balanced account of the disagreements⁵⁶. Others such as Fukushima do their best to be impartial, but let slip their real view⁵⁷. Unfortunately Borowitz⁵⁸, for reasons best known to himself, attributes the ill-feeling between the Chapman and Nordic schools to Chapman's xenophobia, an absurd accusation given Chapman extensive travel and range of collaborators.

Chapman and Oxford

As the Second World War approached, Chapman's pacifist views had moderated due to his exposure to what was happening in Europe, and especially to young German scientists. His duties during the 1939 – 45 interval are not clear: incendiary bomb work and operational research are mentioned by Cowling. He returned to Imperial in 1945 but nearing age 60 did not feel up to building up the department again. Hyman Levy (1889 – 1975) took over, having been acting head of department during the war.

Meanwhile, on June 5, 1940, A.E.H. Love, the incumbent Sedleian professor since 1899 and an authority on wave propagation and elasticity, died, aged 77⁵⁹,⁶⁰. The war delayed replacing him. There was an initial discussion in October 1944, but a subsequent delay was at the insistence of the university's governing Hebdomadal Council due to the need to review "the future of physics in the university" (which sounds like a bureaucratic tactic to do nothing). H.M. Margoloiuth (Secretary of the Faculties) in January 1945 presented cogent reasons why the chair should be filled and the appointment board was concerned that other prestigious chairs would be advertising. The Sedleian chair was advertised in March 1945 with an annual salary of £1200 and a retirement age of 65, the latter a new feature.

Input was sought from those responsible for election of the chair. In a letter dated May 30, 1945 to (Sir) Douglas Veale (1891 – 1973) at the University registry, Sir Geoffrey (G.I.) Taylor apologised for his absence from the election meeting, being in the US on Manhattan Project related duties, and resigned from the board of electors. On May 31, E.A. Milne contributed a lengthy handwritten letter. He would be in Leningrad (St Petersburg) on the date of the election meeting and offered no specific suggestions. Instead the purpose of the letter seems to be to say that he had been awarded the Bruce medal of the Astronomy Society of the Pacific, for which he was duly congratulated by Veale. There followed a correspondence with the long-suffering Veale as to whether Milne would stay on the election board.

⁵⁶ For example Southwood (2015, 2017)

⁵⁷ N. Fukushima, Some topics and historical episodes in geomagnetism and aeronomy, *JGR*, 99 (1994), 19113-19142. N. Fukushima, Unreasonable discrimination of Birkeland's current system in the history of magnetic storm studies, *Proc. NIPR Symp. Upper Atmos. Phys.*, 4, (1991) 108-115. The former is a proper discussion of Birkeland, the latter is not.

⁵⁸ S. Borowitz, The Norwegian and the Englishman, *Phys. Perspectives*, 10 (2008), 287-294. Although he studied the ionosphere in his early career, the author had been an administrator for many decades. It is not clear what prompted his article.

⁵⁹ This section makes use of Oxford University and Queen's College documents.

⁶⁰ There is a memo written by Douglas Veale in 1933 following a meeting with F.A. Lindemann who argues for the possible appointment of a "foreigner" at the next Sedleian vacancy. Lindemann named this person, but Veale did not give the name in his memo. Lindemann would have had in mind someone escaping from Nazi Germany and was also perhaps trying to nudge Love out. One can speculate that this may have been Erwin Schrodinger who Lindemann had met in Germany, was in Oxford in 1933 at the time his shared Nobel prize (with Paul Dirac) was announced and held briefly a fellowship at Magdalen College. Nothing came of this and after a peripatetic few years Schrodinger ended up in Dublin, where the recently appointed Taoiseach Eamonn de Valera had founded the Dublin Institute for Advanced Study (DIAS). [Confidential note by Veale, July 6, 1933.]

Letters of reference were taken out for Robert Stoneley (1894-1976: University of Leeds: solid Earth science) and Leslie Howarth (1911 – 2001: University of Bristol: boundary layer theory). Nothing further is heard of these applicants. Another applicant was Sydney Goldstein (1903 – 1989: a fluid dynamicist and Howarth’s PhD supervisor) who then withdrew on moving to Manchester. On July 7, letters were sent to (Sir) Nevill Mott (1905 – 1995: University of Bristol and 1977 Nobel prize winner) and to (Sir) Harrie Massey (1908 – 1983: then occupied with the Manhattan Project at University of California Berkeley and due to return to UCL to become a pioneer of UK space research). The letter to Mott “invites him to accept the professorship” while that to Massey “whether you would be willing for your name to be considered”. Mott curtly declined on July 10. On August 20, 1945, a formal offer was made to Massey, who eventually declined in September 1945.

The next board of electors meeting in October 1945 asked Milne whether Chapman was interested⁶¹. They also authorised approaches to Drs Penney, Peierls and Heitler. These are Bill Penney (nominally at Imperial College but then the UK lead on the Manhattan project), (Sir) Rudolf Peierls (1907 – 1995: University of Birmingham also involved in the Manhattan project) and Walter Heitler (1904 – 1981: then in Dublin). Chapman, Penney and Heitler all responded positively, though later Penney and Peierls withdrew. So on 17 December 1945, the Sedleian chair was offered to Chapman who accepted and arranged to start on April 1, 1946. The difficulty in filling the Sedleian chair ought to have sent a warning to the University. A number of those approached expressed the view that they were happy where they were and, importantly, would have the resources they felt they needed, such as lectureships and facilities. Oxford was unwilling to offer any such carrots.

A different Chapman emerges after the war. His major monographs with Cowling and Bartels sum up many of his achievements between 1912 and 1940 and draw a line under the first part of his career. In the immediate post-war years and indeed up to roughly 1960, there are fewer long papers, more short notes and an increasing emphasis on what today one would call an “elder statesman” role. But, by most standards he was still a productive scientist, though Cowling comments on the lack of exciting advances. There is lack of strong collaborations such as he had enjoyed in the 1930s with Ferraro, Cowling and Bartels. He investigated a variety of problems, including Blackett’s hypothesis concerning the ratio of the angular momentum and magnetic moment of large rotating bodies being given by a combination of fundamental physical constants⁶². More important were his 1946 and 1950 paper on upper atmosphere nomenclature⁶³ (Chapman, 1946, 1950). The term “aeronomy”, now used to describe upper atmosphere and ionospheric studies originated in the first of these papers. Chapman in fact proposed that this term supersede “meteorology”, but that has not been adopted. The latter of these papers broke up these regions into “spheres”: thermosphere, mesosphere, ionosphere, though much of this was a summary of earlier proposals. Chapman’s main Oxford achievement, the International Geophysical Year (IGY) is discussed below.

While Cowling noted that Chapman “enjoyed the gracious living in Oxford”, especially welcome at a time of rationing, there were difficulties. At Imperial College, he had good secretarial support, not available at Oxford. In 1948 he approached the University through Veale about this. The bureaucracy rolled into action, precedent was scoured for advice, initially determining that only heads of department were entitled to secretarial support. Chapman was then asked to make his case to another committee, which he duly did, making the point that his time was being spent on minor matters that were a waste of his abilities. This committee then recommended to another committee that support for Chapman be

⁶¹ One wonders why Milne had not put Chapman forward sooner. See earlier text.

⁶² The hypothesis was nullified by, amongst other things, measurements from a coalmine in Leigh, Lancashire, not far from Chapman’s birthplace.

⁶³ S. Chapman, Some thoughts on Nomenclature, *Nature*, 157, (1946) 405. S. Chapman, Upper atmosphere nomenclature, *JGR*, 55, (1950), 395-399.

found within the general support of the mathematics department. Sadly the outcome is not recorded in the available files, but the prolonged nature speaks for itself.

Chapman soon found the need to travel to India and then in the winter of 1949-50, he wrote to Veale asking for approval to spend a year at Cal-Tech. A salary of \$10k and travel expenses of \$2k were offered. The visit was initiated by Cal-Tech⁶⁴ and was part of an effort by Vannevar Bush and Lloyd Berkner to strengthen US collaboration with western Europe: a scientific equivalent of the Marshall plan. This was one manifestation of the dominance that US science was to take on post-war. Much of continental Europe had been devastated: the UK, being obsessed with its delusional role as a great power, sterling as a reserve currency and, following the passage in 1946 by the US congress off the McMahon act, building its own atomic bomb, budgeted accordingly⁶⁵. Having seen the use of a strong science base in the war, the US military and then the US government (through the National Science Foundation and then NASA) invested in science.

The Cal-Tech visit went ahead and subsequently Chapman approached Veale about spending four weeks a year at the Institute of Geophysics in Fairbanks in the (then) territory of Alaska. The Institute had recently undergone some upheaval due to a dispute between its director and the head of the University, and was looking for a prestigious name to help it rebuild. In 1952 Chapman wondered if he was behaving properly with these absences and volunteered to resign from the Sedleian chair if needed. In the event, he retired from Oxford in 1953 and the Sedleian chair was filled by George Temple.

Chapman had no intention of going into a quiet retirement. A move to the USA was attractive for many reasons: no rationing (abolished in 1954 in the UK), travel without worrying about exchange controls (abolished in 1979) and the ability to actually observe the aurora. He subsequently spent winters in Alaska and summers at the High Altitude Observatory (HAO) in Boulder, Colorado where its founder Walt Roberts was beginning an expansion of activities⁶⁶. But there was more to it than that; after his death, the Alaska Geophysical Institute director Keith Mather wrote a very nice tribute including the words:

“He spent three months of each year at College (Alaska)—usually the winter months when the aurora was overhead, and he also preferred walking in the snow—and a goodly part of the remainder of the year at HAO/NCAR in Boulder, Colorado. But where Sydney Chapman might be at any given time involved an uncertainty principle of the Heisenberg kind, evidenced only by the postage stamps on the meticulously written letters that arrived from London, Cambridge, Moscow, New Delhi, Sydney, Tokyo, Ann Arbor, Minneapolis, Mexico or Ibadan.”⁶⁷

International Geophysical Year (IGY)

There had been International Polar Years (IPY) in 1882 and 1932. The birth of the IGY has been attributed to a dinner hosted by James Van Allen (1914 – 2006)⁶⁸ on 5 April 1950, attended by Chapman

⁶⁴ F. L. Korsmo, The Genesis of the International Geophysical Year, *Phys. Today*, 60 (2007), 38.

⁶⁵ C. Barnett, *The lost victory*, (1995), *The verdict of peace* (2001), McMillan

⁶⁶ HAO began in 1940 as an outgrowth of Harvard College Observatory with a site at 11,000 feet elevation at a molybdenum mine in Climax Colorado (see T.J. Bogdan, Donald Menzel and the beginnings of HAO, *J. Hist. Astron.*, 33 (2002), 157). It subsequently moved to Boulder, Colorado and became part of the National Center for Atmospheric Research (NCAR). The author spent 2½ years as a postdoc at HAO/NCAR in 1982 – 1984.

⁶⁷ See <https://www.alaska.edu/uajourney/notable-people/fairbanks/sydney-chapman/>

⁶⁸ After wartime navy service including a major role in the development of the proximity fuse, Van Allen was at the Applied Physics Laboratory until 1950 when he moved to the University of Iowa where he remained. He was one of the great scientific figures in the 2nd half of the 20th century.

and others, which led to the proposal of another IPY that subsequently developed into a broader IGY. Chapman credits the suggestion of the IPY/IGY at this dinner to Lloyd Berkner⁶⁹. However, in a 1998 interview⁷⁰, Van Allen thought that Chapman already had the idea of another IPY and was waiting for Berkner to suggest it! It was to be held in 1957 – 1958 (25 years after the last IPY and coinciding with the maximum of the 11 year solar activity cycle) with the aim of coordinating geophysics observations around the world. Korsmo provides a good and concise retrospective⁷¹. By 1953 roughly 12 people had formed a steering group, with Chapman and Berkner as president and vice president respectively. Uncertainty about a Soviet contribution was removed by 1955, although they did not commit to rockets or satellites. The USA invested extensively in IGY and committed to launching an IGY satellite. Beyond the USA and the USSR there was enthusiasm, but probably little money. Chapman's role can best be summarised by Van Allen:

“And so, all the rest of us spoke up (at the 1950 dinner) with the feeling that it sounds like a great idea to us and why don't we get going on this. Well after this occasion, Sydney Chapman took the international leadership in going ahead with it.... he sensed the timeliness and he had the international prestige of a geophysicist and scientist, very well earned clout to make it go. He spent a good part of his life for many years on the IGY..... For the next several years (after 1950), Chapman spent a good part of his time traveling internationally, going to various countries all over the world developing the idea of appointing committees for individual specialties, enlisting the support of influential scientists in all the major countries for this undertaking, and organizing the institutional structure for coordinating the work. He was, I think, essentially the unanimous choice for president of the IGY..... That was one of his major professional undertakings but he, of course, continued to do personal research throughout that period..... But certainly his most important contribution to the science of geophysics during that period was making a success of the IGY. It was a non-governmental enterprise. That was one of its distinguishing features. The adherent bodies were not government agencies in the countries, but independent scientific organizations of one kind or another.”⁷²

Two major achievements from the IGY changed the field of space science and solar terrestrial relations. One was the need for coordinated observations, both ground and (near-)space based. This was partly based on observing campaigns using rockets and rockoons. The “rockoon” was an ingenious idea due to Van Allen and others. In order to get further into space, a balloon was launched and, at a predetermined height, a rocket was launched from the balloon⁷³. It was also recognised that data should be shared between scientists. This led to the establishment of World Data Centres (WDC), locations where ranges of data were archived, and available to users. This pioneered the way to the present large-scale data archives.

The second major outcome was the launch of orbiting satellites. Sputnik 1 was launched in October 1957. The US response has been well documented: the Vanguard launch failures, and then the successful launch of Explorer-1 and -3 via a Juno/Jupiter rocket which led to the discovery of intense belts of charged particles (the Van Allen belts) by the small Geiger counter built by Van Allen⁷⁴. Figure 7 is a summary of the radiation belts: they are the shaded regions⁷⁵. An indication of the panic induced by Sputnik in the USA, and the relief when their own satellite programme was successful was the

⁶⁹ S. Chapman, The international geophysical year, 1957-1958, *Nature*, 172 (1953), 327-329.

⁷⁰ <https://kb.osu.edu/handle/1811/33918?show=full> This links to the oral interview and a transcript. I have quoted this at length because it conveys the sense of what Chapman accomplished with IGY.

⁷¹ Korsmo, (2007) op cit

⁷² From 1998 interview cited above.

⁷³ See Gillmor and Spreiter (2007)

⁷⁴ See for example, J.A. Van Allen, Energetic particles in the Earth's external magnetic field, in Gillmor and Spreiter (2007), page 235.

⁷⁵ J.A. Van Allen (1959) The geomagnetically trapped corpuscular radiation, *JGR*, 64 (1959), 1683

presence of Van Allen on the cover of *Time* magazine the week after the announcement of his discovery. NASA was formed in 1958, designated a civilian agency despite Eisenhower wishing “space” to remain largely in the military domain⁷⁶ (Ambrose, 1984). IGY was a major triumph for Chapman, perhaps his greatest. It changed the way that science was carried out.

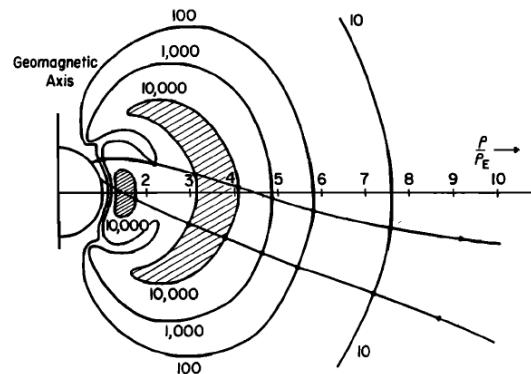


Figure 7 The ring currents (shaded regions) as summarised by Van Allen (1959). There is an inner belt near the Earth and an outer one at 3 – 4 R_E .

The Final Decade

We now return to the development of the Chapman-Ferraro model. In 1957 Chapman proposed that the outward extension of the hot solar corona into interplanetary space was a static, stratified atmosphere originating in a corona with a temperature of 1 MK⁷⁷. The temperature structure of this extended corona was determined by thermal conduction, which led to a temperature of 0.2 MK at the Earth. Thus the boundary in the Chapman-Ferraro model would be between this static, extended corona and the Earth’s magnetic field. There was no mention of an interplanetary magnetic field.

Motivated by observations of extended cometary tails, in 1958 Eugene Parker (1927 – 2022) at the University of Chicago proposed a different scenario, namely that the Sun’s corona extends into space as a dynamic highly supersonic wind, the solar wind with a velocity of hundreds of km/s⁷⁸. Parker excluded Chapman’s static solution since it predicted a pressure at large distances incompatible with those inferred for the interstellar medium. The story of the initial rejection of this paper by the *Astrophysical Journal* referees and subsequent editorial decision by S. Chandrasekhar to proceed with publication in that journal is well known⁷⁹. Parker also deduced that the solar magnetic field filled the interplanetary medium: the interplanetary magnetic field (IMF). His idea created controversy that was only resolved by the measurement of a supersonic wind and its magnetic field by spacecraft. Thus the Chapman/Ferraro model was modified again to have a highly distorted boundary between the magnetised wind and the terrestrial magnetic field, called the magnetopause. Tommy Gold named the cavity in space the “magnetosphere”⁸⁰.

⁷⁶ S. Ambrose, *Eisenhower Vol 2*, (1984) Simon and Schuster

⁷⁷ S. Chapman, Notes on the solar corona and the terrestrial ionosphere, *Smithsonian Contribution to Astrophysics*, Vol. 2, (1957) p.1 The detection of highly ionised iron in the Sun’s corona in the late 1930s had led to the realisation that it was hot, of order 1 MK. Akasofu (2011) notes that this paper was rejected by the first journal to which it was submitted, hence the unusual place of publication.

⁷⁸ E.N. Parker, Dynamics of the interplanetary gas and magnetic field, *ApJ*, 128 (1958), 664-677.

⁷⁹ E.N. Parker, Adventures with the geomagnetic field, in Gilmore and Spreiter (1997), 143-156

⁸⁰ T. Gold, Motions in the magnetosphere of the Earth, *JGR*, 64 (1959), 1219-1224

The second important paper was published by Jim Dungey in 1961⁸¹. This work was not entirely new, though the author's later claim that the inspiration arose "sitting at a café in Montparnasse" certainly accounted for many aspects. Its genesis lay in Dungey's PhD study with Fred Hoyle in the 1940s⁸² and as early as 1948 Dungey had realised that the IMF could play a crucial role in geomagnetic activity. The delay in publishing his work seems in part to have been due to the rejection by *Monthly Notices of the Royal Astronomical Society* referees of associated work⁸³. And, by 1960, the idea of the IMF filling the heliosphere was more palatable.

Dungey uses the dynamics of magnetic field lines in a plasma (as developed by Alfvén) to deduce how the solar and terrestrial magnetic fields interact with each other. In a highly conducting plasma, the magnetic field and plasma are said to be "frozen" to each other, so that when the solar wind and the IMF encounters the terrestrial magnetic field, solar wind plasma cannot enter the magnetosphere, the wind and IMF slides around the magnetosphere and the magnetopause is largely impermeable, though small-scale plasma instabilities may arise. An exception occurs in regions of strong current, such as can exist at the magnetopause, when (for simplicity) the solar wind and terrestrial fields are anti-parallel: i.e. a Southward-pointing IMF (Figure 8, upper left panel). Then the field line connectivity can change, so that plasma associated with the solar wind finds itself on magnetic field lines rooted in the Earth. Thus solar wind mass and energy can access the magnetosphere in a process known as "magnetic reconnection" and the magnetopause is then permeable. A similar process occurs downstream of the Earth, in the magnetotail.

Known as the "Dungey cycle", this work was of the highest importance and has concrete predictions: downstream there should be both Northward and Southward magnetic fields, solar plasma should be seen inside the magnetosphere, for example, energetic electrons and highly ionised states of ions (e.g. solar O^{5+} rather than terrestrial O^+) as well as a pattern of convection in the ionosphere. All have been confirmed. A more modern representation of the Dungey model is shown in the lower left panel of Figure 8. Note the survival of the Chapman/Ferraro boundary, albeit modified in nature, on the dayside. While Dungey's paper began to resolve many of the arguments of the past 30 years, acceptance of his ideas was slow⁸⁴. While others put some more flesh on the skeleton⁸⁵, it was not until spacecraft data became available, first slowly in the 1960s, and then in large quantities after 1978 that Dungey's work was widely accepted.

The right hand panel shows a contemporary sketch of the entire magnetosphere. The "magnetopause current" is that due to the Chapman and Ferraro cavity, the ring current, responsible for the main phase of the magnetic storm, is that discovered by van Allen, and the field-aligned currents those proposed by Birkeland.

⁸¹ J.W. Dungey, Interplanetary magnetic fields and the auroral zone, *PRL*, 6, (1961) 47-48

⁸² J.W. Dungey, Memories, maxims and motives, *JGR*, 99, (1994) 19189-19197. S.W.H. Cowley, in *Magnetospheric Plasma Physics: The Impact of Jim Dungey's Research*, Astrophysics and Space Science Proceedings 41, (2015), 1-32.

⁸³ J.W. Dungey, Conditions for the occurrence of electrical discharges in astrophysical systems, *Phil Mag*, 44, (1953) 725-738. P.J. Cargill, in *Magnetospheric Plasma Physics: The Impact of Jim Dungey's Research*, Astrophysics and Space Science Proceedings 41, (2015), 221-251

⁸⁴ See Cowley (2015, p 18) for a particularly egregious effort to denigrate his work

⁸⁵ R.H. Levy et al, Aerodynamic Aspects of the Magnetospheric Flow, *AIAA Journal*, 2 (1964), 2065

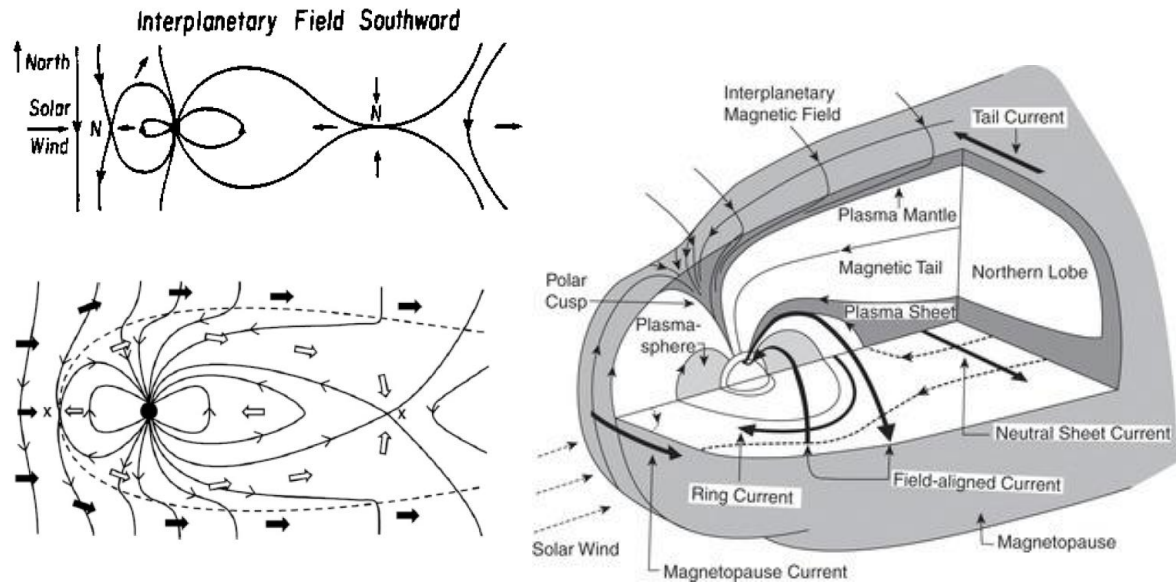


Figure 8 Upper Left: a sketch of magnetic field lines (from Dungey, 1961) showing the interplanetary magnetic field, magnetic reconnection when the IMF is southward (labelled N), and the formation of a tail on the nightside with a second reconnection site. The lower left panel shows a more modern two-dimensional projection of Dungey's work with field lines (solid) and plasma flow (arrows). The right panel shows a three-dimensional rendering of the magnetosphere with the various currents discussed throughout the text. Ancestry unknown.

Chapman was 70 in 1958 and continued as a productive scientist until his death in 1970, though the available space precludes an extensive discussion of his work in these years. A change in his circumstances in the 1960s was his renewed interaction with young scientists. S.I. Akasofu (1930 –) who arrived in Alaska at the end of 1958 and has remained there, was the primary collaborator. Peter Kendall (1934 – 2013) was another, a student of Ferraro's, based at Bedford College, London and then Sheffield, but making regular visits to HAO and Alaska. One does though have the impression that things were happening too quickly for Chapman to keep up. He seemed slow to catch on to the importance of a Southward IMF in instigating geomagnetic activity and a review paper given in late 1961 seems to be sitting on several fences, especially in the context of Parker's work⁸⁶. However by the time of the posthumous Akasofu and Chapman volume⁸⁷, both Parker and Dungey's work receive due recognition. There was also an insistence on persisting with overly simple current models when the wider community had moved on to more sophisticated and realistic models⁸⁸. And, as we have mentioned, the discovery of Birkeland currents in 1966 should finally have ruled out his approach. There was also a fair bit of tidying up old topics such as atmospheric and lunar tides, or invited introductions or summaries of conferences. On the other hand there are gems like the work with Peter Kendall on x-point collapse⁸⁹ which showed that Chapman had lost none of his mathematical skills. And he continued in the role of "elder statesman", perhaps for longer than he should have. He worked hard right to the end.

Conclusions

⁸⁶ S. Chapman, The extended solar corona, *IAU Symp.* 16, (1963) 235

⁸⁷ S.I. Akasofu and S. Chapman, *Solar Terrestrial physics*, (1972), Reidel.

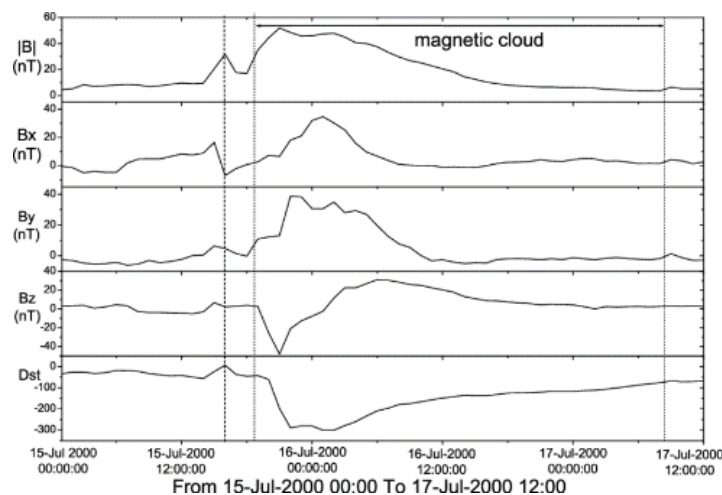
⁸⁸ A.J. Dessler and E. N. Parker, Hydromagnetic theory of geomagnetic storms, *JGR*, 64 (1959), 2239.

⁸⁹ S. Chapman and P.C. Kendall, Liquid Instability and Energy Transformation Near a Magnetic Neutral Line: A Soluble Non-Linear Hydromagnetic Problem, *Proc. Roy. Soc A*, 271 (1963) 435-448.

Sydney Chapman was one of the great scientific figures of the 20th century. He pioneered work into the nature of geomagnetic activity, the ionisation of the upper atmosphere, the kinetic theory of gases and atmospheric tides. His role in IGY perhaps showed him at his best. He was blessed by a long working life: he began by arguing with Jeans in 1912 and ended seeing the space age with his prediction of the magnetospheric cavity confirmed. His obituaries point to a kind and considerate colleague and friend, though tending to avoid controversy as in his arguments with Alfvén. There is also the feeling that there was a reluctance to absorb ideas that contradicted his own views⁹⁰. But he got a lot out of life, with travel and exercise being important to him.

Oxford perhaps did not see the best of Chapman and in my opinion would have been better off appointing a much younger person: Cowling for one would have been ideal. Chapman's pre-war collaborators had moved on to other things, and the lack of university support seems petty. The relative unimportance of non-military science for both the Attlee and Churchill governments must have been discouraging. So Chapman saw greater opportunities elsewhere, in particular in the USA, and as he envisioned working past his nominal retirement age, took steps to ensure his future although this led to lengthy absences from Oxford.

Since Chapman's death, ionospheric and magnetospheric physics have made huge advances such as the general acceptance of the open magnetosphere (Dungey) model, the identification of large solar eruptions (CMEs) as the cause of major storms, the ability to determine the origin (solar or terrestrial) of magnetospheric charged particles and, in the last two decades, the use of multi-point spacecraft measurements to determine the three-dimensional nature of space plasmas. Figure 9 summarises the present state of the link between the Sun and geomagnetic activity. The upper four panels show the magnetic field associated with the interplanetary manifestation of the Bastille day CME (called a magnetic cloud), starting on July 15, 2000. The field turns sharply Southward late on July 15, and the "storm" index (D_{st}) shows an immediate negative turning, peaking at ~ -300 nT. One can see the magnetic cloud as being equivalent to Chapman's stream (Figure 5) with the major difference that it has a magnetic field, the Southward orientation of which leads to the transfer of energy to the magnetospheric cavity, as Dungey proposed.



⁹⁰ Whether this was because he had no close collaborators after 1940 with the seniority to stand up to him and argue is unclear. However, in writing this chapter I have had some correspondence that suggests that there was rather too much unquestioning admiration from some.

Figure 9 The magnetic field associated with the CME in interplanetary space and (bottom) the D_{st} index. This CME and storm were the consequences of the Bastille day flare discussed in Section 2. Figure from S.M. Kang et al., JGR 111, (2006), A05102.

There are also societal implications. As an example, a series of large solar eruptions between March 9 and 12 1989, was followed by, early on March 13, a major failure of the power system in the Canadian province of Quebec. This arose due to induced ground currents associated with a large geomagnetic storm, and the effect was exacerbated by the nature of the rock which diverted the currents to long power lines, triggering circuit breakers. Expansion of the upper atmosphere caused severe difficulties with satellite tracking as well as with communications systems. These effects of a geomagnetic storm had long been appreciated, but the scale of disruption in March 1989 was new. So “space weather” was born. First in north America, then in Europe, funding became available for space weather: to understand its causes and consequences and to (if possible) predict its onset and severity. 100 years after Chapman’s initial speculations, the field is now well established in the scientific and public conscience.

Acknowledgements

I am grateful to Chris Hollings, Mark McCartney, Eric Priest and David Southwood for reading and commenting on a preliminary version of this chapter, to David Southwood for tracking down the written version of Chapman’s 1967 Birkeland symposium talk, Chris Hollings for obtaining the Oxford University records and Bernie Roberts for providing information about Peter Kendall.

Further reading

No formal biography of Chapman exists. His obituaries are:

T.G. Cowling, Sydney Chapman. 1888-1970, *Biographical Memoirs of Fellows of the Royal Society*, 17 (1971), 53-89.

T.G. Cowling and V.C.A. Ferraro, Obituary. Sydney Chapman, *QJRAS*, 13 (1972), 464-478.
as well as Cowling’s historical memoir:

T.G. Cowling: Astronomer by accident, *ARA&A*, 23, (1985), 1-18

and the reader should assume they are the source of factual material in the text.

In a series of short papers, Akasofu has provided important perspectives:

S.I. Akasofu, Chapman and Alfvén: A rigorous mathematical physicist versus an inspirational experimental physicist, *EOS* 84 (2003), 269-274

S.I. Akasofu, The scientific legacy of Sydney Chapman, *EOS*, 92 (2011), 281.

S.I. Akasofu, Space physics in the earliest days, as I experienced it, *Perspectives of Earth and space scientists* (2019), AGU.

S.I. Akasofu, A biographical sketch based on the book “Chapman Eighty, from his friends”, *Perspectives of Earth and Space scientists*, doi.org/10.1029/2020CN000135 (2020).

Southwood and de Moortel et al (2020) give a concise summary of the Chapman/Birkeland/Alfvén disputes:

D.J. Southwood, in *Magnetospheric Plasma Physics: The Impact of Jim Dungey’s Research*, Astrophysics and Space Science Proceedings 41, (2015), 253-271.

D.J. Southwood, The great Norwegian scientist that nobody knows, Norwegian Academy, (2017).

I. De Moortel et al., Alfvén on heating by waves, *A&G*, 61, (2020), 2.34-2.39.

The AGU has published a number of retrospective articles, especially in:

C.S. Gillmor and J.R. Spreiter, *Discovery of the magnetosphere*, (1997), AGU Publications

and articles in *Journal of Geophysical Research* October 1, 1994 and May 1, 1996, written by those participating in space science in the 1950s and 1960s.

