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SIR EDWARD APPLETON AND EARLY IONOSPHERE RESEARCH

by

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The earliest suggestion that there might be a conducting layer in the earth's atmosphere was made in 1839 by Gauss, who was concerned to explain the diurnal geomagnetic phenomena, and who pointed out that a possible hypothesis was that they arose from electric currents encircling the earth somewhere in the atmosphere. However, at that time, no mechanism to account for the existence of a conducting layer could be suggested, nor was there any knowledge of how air could become conducting; indeed, until ionizing radiation began to be studied, it was even suggested that air naturally became conducting at low pressure.

It was soon apparent that the propagation of radio waves beyond the horizon could not be explained as the result of diffraction. Heaviside and Kennelly, at about the same time, both proposed that a conducting layer in the atmosphere was responsible. It was Appleton who first showed, by an elegant and well planned series of experiments, that this was indeed the case; or to put it more precisely, that at a point distant from the transmitter there was, in addition to a wave received along the surface of the earth, a second wave descending from the sky.

EARLY LIFE

Edward Appleton was born in Bradford, Yorkshire, in 1892. He received his early education at Hanson Secondary School, and went up to St John's College, Cambridge, in 1911, to read for the Natural Sciences Tripos. He studied physics, geology, and mineralogy in Part 1 and physics in Part 2. Appleton stayed on in Cambridge, beginning work at the Cavendish Laboratory under W.L. Bragg, helping to determine the structure of some metallic crystals.

The First World War started before Appleton could become fully launched on an academic career, and he instantly applied for a commission in the Royal Engineers through the University Officers Training Corps. Like many other young men of his generation, he could not bear to wait until his commission came through and he joined an infantry battalion (6th West Yorks) as a private soldier. By time he was gazetted an officer in December 1914 he had reached the rank of corporal. From the point of view of survival, he was fortunate to leave the battalion when he did, as soon afterwards it was ordered to France and suffered heavy casualties.

On joining the Royal Engineers, Appleton was posted to the signals section. Whether he applied for this or whether it was a choice made for him I do not know,

Figure 1. E.V. Appleton (seated) photographed in army uniform early in World War 1.

but it was a fortunate occurrence as it introduced him to the field of radio communication and the newly invented vacuum tube. The Royal Engineers provided signalling services for the Army until towards the end of 1920, when what eventually became the Royal Corps of Signals was established.

However, before Appleton could begin regimental duty, he underwent a rigorous course of training, including equitation to full regular army standards. The above photograph, which shows him (seated) with a brother officer belonging to a different arm of the service, was taken when he was on this course, or so I would judge from the slightly disheveled condition of their uniforms.

The instructor on the signals course on which Appleton was sent next, fell seriously ill just as Appleton was passing out. The colonel, following regular practice in the Army, sent for Appleton who was top of the class just ended and told him to 'carry on'. So it came about that he spent the entire war at the signals depot, training successive waves of signals personnel, and engaging in some minor experimental work. Things were very nearly otherwise, as at one point he was ordered to France on a technical mission, with the understanding that once it was completed he would remain as a regimental officer. However, within a few weeks he was recalled to the depot. During his period at the front, he picked up in a captured pill box a vacuum triode of German manufacture.

Interest in radio

Appleton returned to Cambridge as soon as he could after the war was ended. His interest in radio had been stimulated by what he had learned as a signals officer, and his desire was to do research in that subject, in particular to study the non-linear properties of vacuum tubes. However, he was not at all sure that this would be agreeable to Rutherford, who he felt might very reasonably wish to concentrate the limited resources then available for research on the very promising field of radioactivity. However, when Rutherford heard what Appleton had to say, he was impressed by his enthusiasm and generously said he would support him. Rutherford, it will be remembered, had himself done some work on radio waves at the outset of his own career and had even thought of exploiting it commercially.

Appleton had the captured German tube and he was able to obtain some more as gifts from various British manufacturers. For a long time, these were precious possessions. Appleton is on record as instructing a student who had ridden out to his house on a bicycle to collect some tubes for an experiment, not to ride his bicycle on the way back, but to push it, for fear of an accident.

Appleton's interest in non-linear phenomena was shared by another student of roughly his own age, namely Balth van der Pol from the Netherlands. Together they undertook fundamental research on non-linear vacuum tube oscillators that has stood the test of time. Van der Pol later joined the Philips Company in Eindhoven and had a distinguished career with them.

The experiments on non-linear oscillations, along with work on the nature of atmospherics, took up Appleton's time until 1924 when, as we shall see, he turned his attention to the propagation of radio waves.

The discovery of the electron in 1897 by J.J. Thomson led to great advances in physics, particularly as regards the understanding of the optical properties of matter. The leaders in this new field were H.A. Lorentz in the Netherlands and J. Larmor in Cambridge. Both had published highly influential books on the subject. When Appleton became a student in Cambridge, Larmor was giving regular class lectures on electron theory. Appleton attended these lectures together, I suppose, with most of the more able physics students of his year.

As soon as he became a fellow of St John's College (in October 1919) Appleton would meet Larmor on a regular basis at dinner. He would also meet Ebenezer Cunningham, a man some 10 years older than himself. Both Larmor and Cunningham had been Senior Wranglers in their respective years, 1880 and 1902.

One of the successes of the Lorentz–Larmor theory—now often known as the classical electron theory—was the partial explanation of the Zeeman effect, namely, the splitting of the spectral lines in the radiation from an ionized gas that occurs when a magnetic field is applied.

Electron theory did not lead, as was hoped, to a comprehensive theory of matter, but it did lead to the theory of relativity. This began when it was discovered that Maxwell's equations were invariant under a certain transformation of the space and time variables. At first, this was thought to be an approximate result, but as soon as the Lorentz transformation—as it came to be called—was properly formulated, it was seen to be exact. Strictly, the invariance was proved only for electromagnetic phenomena, but it began to be suspected that it might apply more generally. This led Einstein to propose the Special Theory of Relativity.

These developments fired the interest of Cunningham. In 1914, he published the first monograph in English on relativity. He followed this in 1915 with a similar book addressed to a wider readership and entitled *Relativity and the Electron Theory*. In 1921, a second edition of this book appeared with the enhanced title *Relativity, the Electron Theory, and Gravitation*; it included a new part containing an introduction to the General Theory of Relativity. The book is in some ways comparable to the better known *Space, Time, and Gravitation* by A.S. Eddington. Neither book gives a formal development of the mathematical theory, but Cunningham's book is less discursive and assumes more mathematical knowledge on the part of the reader. Of particular interest, in the present context, is that the preface contains an acknowledgement as follows:

> Acknowledgement is due to the author's friends Prof H.F. Baker and Mr E.V. Appleton, who kindly read through the proofs of the new matter.

It was natural that Larmor should turn his mind to possible explanations of how radio waves could be reflected from the upper atmosphere. He drafted a paper in which he discussed the mechanism of reflection from an ionized gas, without making any conjecture about what the ions might be. He showed the manuscript to Appleton, who pointed out that, if the ions were electrons, the influence of the earth's magnetic field would have to be taken into account. Larmor confessed to having overlooked this point altogether. He said that in view of this he would not modify his draft, but leave the way open for Appleton to publish a paper of his own.

Appleton did this, and in the paper he sketched the essentials of the magneto-ionic theory, giving the formula for the refractive index for plane waves travelling in the direction of the magnetic field.[1] Shortly afterwards, he published another paper on the same topic jointly with M.A.F. Barnett, a young research student from NewZealand.[2] They mention early work by W.H. Eccles and go on: 'Further contributions to the subject have been made by Sir Joseph Larmor, and the theory discussed in the present paper must be regarded merely as a slightly modified form of these original theories'. In saying this, Appleton was not being modest, but was taking a perfectly correct view of the situation.[3] The paper contains a brief statement of the magneto-ionic theory, and includes the formula for propagation across the field as well as along it.

Larmor began to include material on the magneto-ionic theory in his class lectures. In these, he gave expressions for the refractive index of the medium for the two cases of plane waves travelling along and perpendicular to the earth's field.

Experimental proof of reflection

Appleton was already studying the fading of signals received in Cambridge from a broadcast transmitter in London. Such fading could, of course, be explained by assuming that interference was taking place between a ground wave and a down-coming wave of varying amplitude or phase.

It was not long before Appleton felt able to take up, also with Barnett's help, the important challenge of attempting to prove, by direct experiment, that the hypothetical reflecting layer in the atmosphere really existed. The method he proposed was to vary the frequency of a transmitter by a suitable amount while recording the strength of the signal received at a place where the magnitude of the reflected wave was expected to be a substantial fraction of that of the ground wave. If the reflecting layer really existed, interference would be observed and it would be possible to make an estimate of the height of the layer from the spacing of the maxima and minima.

The experiment required the use of a powerful transmitter whose frequency could be varied over the necessary range. Appleton was fortunate in securing the cooperation of the BBC, whose transmitter at Bournemouth was suitable for the purpose and could be made available late at night when broadcasting for the day was over. He borrowed space in a laboratory at Oxford—which was at a suitable distance from the transmitter—and set up a temporary receiving station.

From the beginning, the experiment was entirely successful. The expected interference fringes were observed, and Appleton was able to show that they were consistent with the theory that there was a down-coming wave reflected at a height of about 90 km. One point did not escape Appleton's attention. It might be objected that, while the experiment proved the existence of a second wave, it did not show that this wave came from the sky; it could, theoretically at least, be a second ground wave reflected from high ground or from some other feature on the earth's surface. Appleton therefore performed a careful series of experiments on which the fringes received on a loop aerial were compared with those received on a vertical wire. The latter would be expected to be smaller than the former by the sine of the angle of incidence on the reflecting layer. This expectation was borne out, and the deduced value for the angle of incidence was consistent with the height of reflection deduced from the spacing of the fringes. The work was published under the names of Appleton and Barnett; J.A. Ratcliffe, a recent Cambridge graduate, also assisted.

Appleton realized that he had made a breakthrough and had opened up a new and rich field of research. He dropped his other interests and concentrated on propagation studies.

Within a few months of the publication of the paper by Appleton and Barnett, Breit and Tuve, working at the Carnegie Institution of Washington, announced similar results obtained by a pulse method, using a cathode ray tube to display the received pulses. Unlike Appleton, neither of these workers devoted themselves to ionospheric studies on a long-term basis.

Appleton, still using the frequency change method, went on to perform, with Ratcliffe's aid, a further series of experiments designed to enable the state of polarization of the down-coming wave to be determined. They found it to be circular in a left-handed sense and they were able to show that this was what would be expected from the theory. This was a well-planned experiment, skillfully carried out.

By the time the above results were obtained, Appleton had moved to King's College, London, to take up an appointment as Professor of Physics. One of his students has recorded how one morning he came into the laboratory in great glee. He told the class about the experiment he had just done and showed them how an explanation of the results could be given along the same lines as the explanation of the Zeeman effect, which they had been studying earlier.[4]

The equations of the magneto-ionic theory

As mentioned above, Appleton could hardly have had better sources of information on electron theory than Larmor and Cunningham. He also had another contact of a very different kind, and this brings us up against an issue on which there is a difference of opinion. C. Stewart Gillmor, who comes to the subject as a historian of science, found among Appleton's papers preserved at the University of Edinburgh some papers relating to Wilhelm Altar, an Austrian born in 1900, who had taken a PhD in theoretical physics at the University of Vienna, and had simultaneously qualified as an engineer.[5] Altar was having difficulty in obtaining suitable employment in Austria, and had come to try his luck in England. He arrived in late summer 1925 and left in mid-February 1926.

Altar sought to interest Appleton in a very different mathematical treatment from that given by Larmor and Lorentz, namely, one based on the theory of crystal optics and expressed in terms of matrices or, to be more accurate, in terms of tensors. Appleton may have been exposed to the theory of crystal optics at the outset of his career, when he worked briefly with W.L. Bragg. He was familiar with tensors from talking to Cunningham and reading the proofs of his book on relativity.

According to Altar,[6] Appleton was fascinated by the way in which equations that interested him came out of the complex analysis, and he began to work on a draft in German that Altar had given him. It appears that they were contemplating a joint paper. However, Altar left England and the paper was not written. Appleton cannot have regarded the crystal optic approach as superior to the standard Larmor–Lorentz treatment. It has since been found useful for general theoretical investigations,[7] but at the time, Appleton would have been right in regarding it as like using a sledgehammer to crack a nut.

At the same time as he was seeing Altar frequently, Appleton was also in touch with Cunningham, whom he saw on trips to Cambridge. There is no evidence that Appleton told Cunningham about the conversations with Altar, or vice versa. This is indeed what would be expected by anyone who knew Appleton.

One can only speculate about what was going on in Appleton's mind at this time and what his priorities were. He conducted his business with both Cunningham and Altar mainly through personal contact. In these circumstances one would not expect the archive to provide a clear story, and this, judging by Gillmor's account, appears to be the case. Gillmor mentions a few letters (not all unambiguously dated) from Cunningham written while Appleton was on holiday. Similarly, there are a few letters from Altar written after he had returned to Vienna, but none of the letters that Appleton wrote to Altar appear to have survived.[8]

Gillmor[6] quotes from a letter Appleton wrote to van der Pol on 27 December 1925, in which he says that he expects to become very busy with a paper on the magneto-ionic theory, and adds in parentheses the words 'very general'. This may have been the joint paper with Altar, but it could well have been that Appleton was planning a more general paper that would present the magneto-ionic theory both as a straightforward development of the Larmor–Lorentz theory and also as a development of crystal optics. Such a paper would have been an undoubted coup.

However, I suggest that, in either case, when he set to work he found that his mathematical powers were being stretched to the limit,[9] and that he came to realize that his wiser course would be to concentrate on the practical side where his true strength lay and where many important problems were waiting to be solved. He was always very good at seeing where he might hope to make major progress and directing his efforts accordingly. A contributory, or even main, factor could have been that he became aware that H. Lassen was also working on a tensor treatment of the theory. There would be no point in writing anything on that subject if it would not come out until after Lassen had published his paper.[10]

Although he wrote no comprehensive paper, Appleton spoke on the subject in the Presidential Address that he delivered to the General Assembly of the International Union of Radio Science, which met in Washington, D.C. in December 1927. A summary only of the address was published (in 1928),[11] and in it Appleton gave the general formula for the refractive index. This was a generalization of the formulae for the two special cases of plane waves travelling respectively along and parallel to the magnetic field. It was clearly going to be important for the discussion of radio wave propagation, and it was characteristic of Appleton that he should have been the first to give it in print.

In this publication, Appleton gave no acknowledgements to the people with whom he had worked, namely Larmor, Cunningham and Altar. This was unusual for him, as he was normally careful about such things; he did give a reference to Nichols and Schelling, whose paper independently proposing the magneto-ionic theory had been published a month or two after his own. The reason for this lapse must be that Appleton did not regard the derivation of the general formula, given the two special cases, as being in itself a major achievement, although the formula itself was of importance. Certainly, it must have been abundantly clear to him that as an achievement it would be nothing to boast about at the high table at St John's College.[12]

Gillmor, on the other hand, appears to take it for granted that the derivation of the formula was a significant piece of mathematical research; he plays down inputs from Larmor himself and from Cunningham and suggests that Appleton depended for the result on Altar's draft paper. He sees Appleton as being guilty of a major piece of scientific plagiarism with Altar as the wronged party; he implies that a major part of the work on which Appleton's early reputation was based should really be ascribed to Altar.

I cannot agree with this judgement. As I have said, Appleton made it quite clear in his joint paper with Barnett, written before he met Altar, that the magneto-ionic theory was no more than a slightly modified form of earlier theory. All that was new in the Presidential Address, as far as the magneto-ionic theory goes, was the formula for the refractive index for a general direction of propagation. Even if this had been Appleton's own unaided work, it could not have accounted for more than a tiny element in his early reputation. If Appleton learned other things from Altar, he did not include them in his Presidential Address. The true basis for his reputation was that he had recognized the seminal importance of the theory for the study of radio wave propagation and put it to work in explaining the observed phenomena.[13]

While I do not see how Appleton can be accused of serious plagiarism, I nevertheless agree with Gillmor that Appleton was at fault in not giving acknowledgements to the people, including Altar, with whom he had had technical discussions.

A derivation of the general formula appeared in a paper by Sydney Goldstein[14] published on 1 November 1928. Goldstein, then a research student, had become interested in the subject by attending Larmor's class lectures. Larmor encouraged him and communicated his paper. Goldstein had taken the Mathematical Tripos and the derivation can have given him little trouble. Starting from Maxwell's equations and the equation of motion of an ion, it occupied less than two pages of the *Proceedings of the Royal Society*. Most of the paper was taken up with a discussion of the properties of the formula and how it might be used to explain the experimental observations. On 16 August 1928, while the paper was in proof, Goldstein added a note to the effect that, since writing it, he had learned that Appleton had given the general formula—in a different notation—in the Proceedings of the Washington Conference. Goldstein's paper contains several references to Ratcliffe and it was presumably from him that he obtained the above information. It seems that Appleton had been unaware that Goldstein was working on the subject.

In 1931 D.R. Hartree, then a professor at Manchester University, wrote a paper[15] in which he developed the magneto-ionic theory with the inclusion of what became known as the Lorentz polarization term and thereby started a long controversy. In spite of these papers no complete presentation of the formulae of the magneto-ionic theory appeared and Appleton took the opportunity of filling the gap when he was preparing a survey paper for publication by the Institution of Electrical Engineers in 1932. When I started doing ionospheric research under Ratcliffe in 1934, this was the most complete account available.

Further contributions to ionospheric research

Appleton went on, with the aid of his students, to make a number of important discoveries and to interpret them in the light of the magneto-ionic theory. He discovered the existence of the F region and the magneto-ionic splitting of echoes returned by it when the frequency of the waves was raised towards the point at which penetration of the region took place. This work settled the question of the nature of the ions responsible for radio wave reflection, showing beyond any doubt that they were electrons. Appleton became the undoubted leader in ionospheric studies and various independent groups, drawing their original inspiration from him, were established by his students.

Appleton played a major role in the inception and organization of the Second International Polar Year of 1932. He went with a team of observers to Tromsö in Norway, just within the Arctic Circle. He had the good fortune to be operating the equipment when a surprising and important discovery was made, namely that during a magnetic storm it often happened that all reflections from the ionosphere disappeared.

In 1938 Appleton left academic life to become the head of the Department of Scientific and Industrial Research, the agency of the British government responsible for supporting scientific research over a wide field, including the ionosphere. In that capacity he contributed in many ways to the British war effort. Later a new career opened up to him when he became Principal and Vice-Chancellor of the University of Edinburgh. He did not lose interest in the ionosphere, however, and he continued to work on it as far as time permitted.

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The photograph accompanying this paper was first published in ‘ Sir Edward Appleton’ by R. Clark. I am grateful to Mr R.D. Fulton, of the late Lady Appleton’s trust, for giving me permission, on behalf of himself and his co-trustees, to reproduce it here.

Notes

1 The paper was presented at a Physical Society Symposium held in November 1924. See *Proc. Phys. Soc. Lond*. **37**, 16D (1925).

2 *Electrician* **94**, 398 (1925).

3 In spite of Appleton’s emphatic disclaimer, there has been a tendency to credit him with contributing to the mathematical development of the magneto-ionic theory. For example, J.A. Ratcliffe, in his obituary notice (see note 4) states that Appleton had extended Lorentz’s theory to cover the case of free electrons, whereas in fact he heard about this when he attended Larmor’s lectures.

4 J.A. Ratcliffe, 'Edward Victor Appleton', *Biogr. Mem. Fell. R. Soc.* **12**, 1-21 (1966). See p. 4.

5 C.G. Gillmor, 'Wilhelm Altar, Edward Appleton, and the magneto-ionic theory', *Proc. Am. Phil. Soc.* **126**, 395 (1982).

6 *Ibid.*, p. 405, para. 4.

7 See for example K.G. Budden, *Radio Waves in the Ionosphere* (Cambridge, 1961).

8 The letters from Cunningham contain references to verifying the general equation. Apart from this it would appear that Appleton was having difficulty in understanding what the reflection condition was for rays entering the ionosphere at oblique incidence. This is a matter for which a wave theory, rather than a ray theory, is required, and Cunningham was well qualified to help.

9 This would certainly have been true as far as Altar's work was concerned. Several people who have seen Altar's manuscript as transcribed by Gillmor (Lady Jeffreys, F. Smithies, K.G. Budden) have remarked to me that it is poorly arranged and that a lot of work by a skilled mathematician would have been needed to put it into a publishable form.

10 H. Lassen, *Elektrische Nachrichten Technik* **4**, 324 (1927).

11 Reprinted by H.G. Booker in *Journal of Terrestrial and Atmospheric Physics* **36**, 2135–2136 (1974).

12 In this he was right. Anyone who had been through the rigours of the Mathematical Tripos and had achieved distinction would have had no difficulty in arriving at the general formula.

13 Gillmor draws attention to a variety of apparently innocent circumstances which, he seems to imply, support the hypothesis that Appleton had something to hide. For example, it is to him a suspicious circumstance that no administrative record survives in the King's College archives of Altar's visit and that Appleton should not have invited him to the laboratory tea parties. If an explanation is needed, it may perhaps be found in the fact that the administrative practices and customs prevailing in King's College in the 1920s differed from those current in an American University in the 1980s. Similarly, in my view, Gillmor makes too much of the fact that Appleton did not keep in touch with Altar and that they did not meet when Altar was briefly in London in 1935. Only if one starts out with the assumption that Appleton had a guilty conscience, do these various circumstances appear sinister.

14 S. Goldstein, 'The influence of the earth's magnetic field on electric transmission in the upper atmosphere', *Proc. R. Soc. Lond.* A **121**, 260–285 (1928).

15 D.R. Hartree 'The propagation of electromagnetic waves in a refractive medium in a magnetic field', *Proc. Cam. Phil. Soc.* **27**, 143–162 (1931).