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Cover picture: Weber's 'Electrodynamische Maasbestimmungen' –
Poggendorf's Annalen LXXIII 1848 (see page 15)

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Guest Editorial

A European conference series?

The various congresses of history of science are popular and successful, but there is no regular large-scale meeting on the history of physics. Or is there? Corrections to Editor on a postcard, please, if any. Why not a European conference series, sponsored by EPS, IOP and others? If it arose out of the relevant groups of those societies, it might (among other things) form a bridge between the historians and the professional physicists, and help the latter to defend themselves from time to time, by recourse to the historical record.

This thought was provoked by a lecture at a recent meeting of the Academia Europaea, in which a distinguished historian played down the role of scientists in promoting economies. To his mind the technical advances of the 19th century were all the products of crafty entrepreneurs. He admitted the case of steam power could not be accounted for in such dismissive terms -but surely it is not atypical? The story of electricity and electromagnetism and their consequences, if appreciated, defeats such nihilistic attacks on the case for research support, based on a jaundiced view of the relevance of basic science.

So how about it? A splendid venue for the first meeting is already on offer.
Who wants to help?

D Weaire



There was a good turn out for the group's last meeting, held at the Clarendon in Oxford and we have two articles in this issue based on the lectures. The third lecture given by Dr. Daniel Mitchell on 'Controversies over electrical units in the late 19th century has had to be held over to a future issue as Dr. Mitchell is taking up a new post in Hong Kong and there was insufficient time to prepare the script. We wish him well in his new job.

Editor

News

New European history of physics centre opens

The idea had been brewing for years. It had given him more than enough headaches but finally on 29th May all his efforts came to fruition in the grand opening at Schloss Pöllau – a grandiose and magnificent building dominating the small market town of Pöllau in Steiermark, Austria.

It proved a triumph for its director Dr. Peter Maria Schuster who had been working frantically - especially during the last few months - to be the midwife and progenitor of the birth of his brainchild and to present it to the world in its resplendent new environment.

Among the many laudatory speeches to a packed audience, the opening talk giving the background to 'echophysics' and the Victor Franz Hess Society by Prof. Hartmut Kahlert was really outstanding, switching effortlessly as he did, from German to English. The occasion was marked by the opening of the first of many exhibitions, entitled 'Der ausgesetzte Mensch' - 'Radiation and Mankind'



Entrance Gallery of 'Echophysics'

The exhibition, which is a celebration of the Austrian physicists – Christian Doppler, Ludwig Boltzmann, Johann Loschmidt, Jožef Stefan and last but certainly not least Victor Franz Hess, will last at least a year and possibly beyond, but, says Dr. Schuster, 'we have plans already in hand to feature exhibits from Greece and Italy. When asked about the UK's future participation, Dr. Schuster was non committal but said it could be as early as 2013.

The exhibition features over 500 pieces of scientific apparatus being donated from the universities of Graz, Vienna and from many private collections. They covered a variety of areas but all focussing on radiation in all its guises.

Future plans include a strong educational element not only in the history of physics itself, but also in the associated social and cultural ethos which obtained. (A sentiment with which I'm sure all would agree – Ed).

Right, a party of students – early and enthusiastic visitors from a local school.



'Radiation & Mankind' was however, only part of the two day event. A symposium – the first of many with the generic title of 'The Roots of Physics in Europe' – an ambitious project indeed but one which had a flying start with speakers from 16 different countries (including two from our group I'm pleased to report) dealing with widely ranging topics and approaches to the history of physics in Europe. Some were biographical, (e.g. Günther Salcher on Peter Salcher – shock waves, ballistics; Ted Davis on Lord Rayleigh), some were subject based (e.g. Bruno Besser on planetary spectroscopy) and many gave a new insight into the physics history of their country (e.g. Rasa Kivilšienė on Lithuanian HoP; Ganka Kamisheva on theoretical physics in Bulgaria and Juraj Šebesta on HoP in Slovakia).

A great success! More info at www.echophysics.org

Malcolm Cooper

Is Poynting's energy flux real?

Controversies over Poynting, from Lodge to Feynman

Dr. John Roche

Linacre College, Oxford

1. Maxwell. In 1877, two years before he died, James Clerk Maxwell (1831-1879), at the height of his creative powers, published in *Matter and Motion*, an essay in *haute popularisation*, which provided a deep but puzzling assessment of the nature and distribution of energy:

'Energy is the capacity for performing work. The idea of work implies a fund of energy, from which the work is supplied.

Since all phenomena depend on the variations of the energy of the body, and not on its total value, it is unnecessary to form an estimate of the energy of the body in its standard state.

[The] absolute value of the energy of a body unknown. The energy of a material system can only be estimated in a relative manner.

We cannot identify a particular portion of energy, or trace it through its transformations. It has no individual existence, such as that which we attribute to particular portions of matter.'

Maxwell's reflections on the relative character of energy led him to regard energy as having no identity, nor individual existence, beyond its measured changes through the work done. But even here only the total energy change is known, and not its distribution. Maxwell's conclusion of our limited knowledge of energy at first provoked critical reactions.

2. Poynting. John Henry Poynting (1852-1914) in 1884, seven years later, attempts to address some of Maxwell's assertions:

'If we believe in the continuity of motion of energy, that is, if we believe that when it disappears at one point and reappears at another it must have passed through the intervening space, we are forced to conclude that the surrounding medium contains at least part of the energy, and that it is capable of transferring it from point to point.'

He then asks

'by what paths and according to what law does ...the energy...travel from the part of the circuit where it is recognisable as electric and magnetic to the parts where it changes into heat and other forms?'

Poynting first argues that the electromagnetic energy per unit volume

$\cdot KE^2/8\pi + \mu H^2/8\pi$...account as far as we know, for the whole energy'.

He then considers the case of a vacuum, integrates it 'within any fixed closed volume'. He differentiates it with respect to time. He then transforms it and, for a fixed conductor, interprets the result as a sum of a volume integral over the conductor, and a surface integral. The first he interprets 'as the energy transformed by the conductor into heat, chemical energy and so on'. For the second 'the energy flows perpendicularly to the plane containing E and H , the amount crossing unit area per second being $EH \sin\theta/4\pi$ '. In 1909, Lorentz provided a more rigorous derivation of this transformation as a continuity equation. In SI units it becomes

$$\int \mathbf{J} \cdot \mathbf{E} \, dv + \frac{\partial}{\partial t} \int \left[\frac{\epsilon_0}{2} \mathbf{E} \cdot \mathbf{D} + \frac{1}{2\mu_0} \mathbf{H} \cdot \mathbf{B} \right] dv + \int \mathbf{S} \cdot d\mathbf{a} = 0$$

Poynting argues that his flux vector [$\mathbf{S} = \frac{1}{\mu_0} \mathbf{E} \times \mathbf{B}$] represents the flow of

energy crossing unit area per second in the electromagnetic field in all cases, that is, not only for radiation but for any finite volume. Poynting's essay concentrates on Coulomb energy exchanges within steady or slowly changing circuits. But he also considered the transmission of light energy - two years before the discovery by Heinrich Hertz (1857-94) of electromagnetic radiation. He also makes a remarkable claim:

'According to Maxwell's theory currents consist essentially in a certain distribution of energy in and around a conductor accompanied by transformation and consequent movement of energy through the field...it seems that none of the energy of a current travels along the wire, but that it comes from the non-conducting medium surrounding the wire... In the electric light ...energy moves in upon the...filament from the surrounding medium, there to be converted and sent out again as [light]'

This is a surprising interpretation of Maxwell since, for him, electromagnetic energy has its 'seat' in the charges and currents, as well as in the medium. Is Poynting's flux theory compatible with Maxwell's theory, and can it be verified?

3. Sir J J Thomson. In 1885, one year after Poynting's paper, Joseph John Thomson (1856-1940) and Oliver Lodge (1851-1940) published opposing reactions to Poynting. According to J J Thomson

'It would seem to be impossible à priori to determine the way in which energy flows from one part of the field merely by differentiating a general

expression for the energy
$$\left[\frac{\partial}{\partial t} \int \left[\frac{\epsilon_0}{2} \mathbf{E} \cdot \mathbf{D} + \frac{1}{2\mu_0} \mathbf{H} \cdot \mathbf{B} \right] dv \right] \dots \text{without}$$

having any knowledge of the mechanism which produces the phenomena which occur in the electromagnetic field...The problem of finding the way in which the energy in a system whose mechanism is unknown seems to be an indeterminate one.'

He also points out that it is only the integral across the closed surface which is defined mathematically in Poynting's vector, and that it does not follow logically that $EH \sin\theta/4\pi$ is the 'flow of energy across each element'. Although 'Professor Poynting's description... is very instructive ...it is quite indeterminate'. J J Thomson's view seems very close to Maxwell.

4. Oliver Lodge. Oliver Lodge strongly supports Poynting's interpretation. On the conventional view (and on Maxwell's view):

'The conservation of energy was satisfied by the total quantity remaining unaltered; there was no individuality about it: one form might die out provided another form simultaneously appeared elsewhere in equal quantity. On the new plan we may label a bit of energy and trace its motion and change of form, just as we may ticket a piece of matter so as to identify it in other places under other conditions; and the route of the energy may be discussed with the same certainty that its existence was continuous as would be felt in discussing the route of some lost luggage which turned up at a distant station in however battered and transformed.'

But how is 'the route of energy' labelled in Lodge's interpretation?

Lodge also offers a gentle criticism of Maxwell's view of potential energy in gravitation and electrostatics. In *Matter and Motion* Maxwell writes that '[W]hen a stone has been lifted to a certain height above the surface of the earth, the system of two bodies, the stone and the Earth, has potential energy...'. According to Lodge,

'In the older and more hazy view of conservation of energy the idea of 'potential energy' has always been felt to be a difficulty. It was easy enough to take account of it in the formulas, but it was not easy or possible always to form a clear and consistent mental image of what was physically meant by it.'

He continues,

...the system of earth and stone possesses energy in virtue of its configuration. True, but foggy. The usual ideas and language current about potential energy are proper to notions of action at a distance. When universal contact action is admitted, the haze disappears; the energy is seen to be possessed, not by stone or by earth or by both of them, but by the medium which surrounds both and presses them together.

But Lodge's solution creates a difficulty far greater than Maxwell's 'foggy' interpretation. By 1877 Maxwell had already passed beyond a simple field interpretation for gravitational energy. Maxwell knew since 1865 that the potential energy between two masses is always negative, and that this cannot represent the absolute energy of the gravitational field. If the gravitational energy is located in the ether, then, in the absence of a gravitational field, this medium must have an

'intrinsic energy per unit volume greater than $\frac{1}{4\pi} R^2$ where R is the greatest

possible value of the gravitational [intensity] in any part of the universe.' Maxwell continues, 'The assumption...that gravitation arises from the action on the surrounding medium...leads to the conclusion [which] I am unable to understand'. The failure of a field explanation for gravitational potential energy led Maxwell, following 12 years of reflection, to suspend judgement about the exact distribution of gravitational energy, and to interpret it as a general property of the 'system of two bodies'. Furthermore, he did not regard it as implying action at a distance. Lodge seems to have missed all of these points. Sadly, Maxwell was not given enough time to explore the consequences of these insights for electromagnetism

It is difficult to look at J J Thomson's on the one hand, and Poynting's and Lodge's views on the other, as other than differences of physics 'styles', the first 'critical', the second 'romantic'. Of course, both of these styles in physics have been extremely fruitful.

5. Alexander McAulay. The most devastating criticism of Poynting's energy flux was by Alexander McAulay (1863 - 1931) of Melbourne in 1892. In Poynting's theory the energy flows laterally into the wire from the external field, and not along the wire, and then generates heat. Developing JJ Thomson's critique, McAulay was able, using Maxwell's equations, to transform the Poynting flux for steady circuits, from

$$4\pi\tau = \nabla EH \quad \left[\mathbf{S} = \frac{1}{\mu_0} \mathbf{E} \times \mathbf{B} \right]$$

into an equivalent flux vector v

$$v = \Psi C \quad [v = \phi J]$$

where Ψ is the scalar potential at a point in the current density C . McAulay's flux vector $v = \Psi C$ is sometimes called the 'Slepian vector', following the work in 1919 by the electrical engineer Joseph Slepian (1891-1961). McAulay then concludes:

'assuming v instead of τ to be the true time-flux of energy... C the current and Ψ the potential are the exact analogues of a liquid current and pressure. Compare this with the very different conclusion of Professor Poynting: "...If we accept Maxwell's theory of energy, we must no longer consider a current as conveying energy along the conductor". According to the present result, in a steady field the sole means of conveyance of energy would be precisely the means Prof Poynting warns us against, namely the electricity itself.'

Clearly, Maxwell's equations do not decide the physical status of the two flux vectors. McAulay also provides us the means to generalise his transformation, which becomes (borrowing Lorentz's phrasing, and for a linear quasi-static system)

$$\int \mathbf{J} \cdot \mathbf{E} dv + \frac{\partial}{\partial t} \int \frac{1}{2} \mathbf{A} \cdot \mathbf{J} dv + \int \phi \mathbf{J} da = 0$$

But what follows?

6. Lorentz, O'Rahilly and Feynman. Hendrik A Lorentz (1853-1928), in 1909 examined the controversy over what he called Poynting's 'beautiful theorem'. Supporting Maxwell and JJ Thomson against Lodge, he asks 'how far we can attach a definite meaning to a flow of energy' between circuit elements? This is because 'it will not be possible to trace the paths of parts or elements of energy in the same sense in which we can follow in their course the ultimate particles of which matter is made up'. Lorentz then employs a mathematical argument for the indeterminacy of the transfer path of energy within a circuit. Querying Poynting and supporting McAulay, he asks 'whether... the heat developed in the wire of an incandescent lamp is really due to the energy it receives from the surrounding medium...and not a flow of energy along the wire itself'. 'It all depends upon the hypotheses which we make concerning the internal forces in the system'. Supporting Poynting, he points out that is no ambiguity about the transfer of radiation energy in the 'ether [which] are entirely determined by the electric and magnetic force existing in that space.' He therefore insists that 'No one will deny that there is a flow of energy in a beam of light'. Lorentz, therefore, suggests that in a radiation regime Poynting's vector is physically defined, but in a circuit the transfer of energy is indeterminate.

Alfred O'Rahilly (1884-1969) in 1938 is even stronger, and argues that only the radiation component of \mathcal{S} is real, but the rest of \mathcal{S} is a 'mathematical fiction' and 'a mere mathematical transformation of the source energy'.

Richard Feynman (1918-1988), in 1964 states that:

'There are, in fact, infinite numbers of different possibilities for [the field energy] and for the [flow vector] and no one has thought of an experimental way to tell which one is right'.

7. Conclusions

Physics has never really settled this issue. In the bound regime, in which the fields are bound to the charges, it is very difficult, perhaps impossible, to decide between the Poynting and the McAuley interpretation. Does this mean that the two continuity equations and 'fluxes' in this regime, although numerically valid, are different analytical representations of an indeterminate energy transfer, as JJ Thomson suggests? But for the radiation regime, McAuley's expression does not apply. As Alfred O'Rahilly suggests, Poynting's interpretation may then be the only valid one?

Maxwell, sadly, had not the time to develop these issues, and physics is very slowly picking up the pieces of his extraordinary burst of late insights.

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Debates over Ampère's Law

*Dr. Gregor McLean
Harrow*

During the 19th century there were many approaches made to the question of the elemental interaction law, primarily between elements of current electricity, from Ampère to the end of the century, and it is this work which concerns us here.

It transpired that there are many plausible possibilities for an elemental interaction law, all compatible with the interactions involving closed currents.

A whole repertoire of choices, mathematical and/or physical were made by individual workers to achieve unique or possible interaction laws, starting with Ampère's requirement of equality of action and reaction at the elemental level, and Grassmann's requirement of mathematical simplicity leading to transverse forces on the elements.

Beyond Ampère and Grassmann, the contributions of Gauss and Weber are considered, leading to Clausius, Maxwell Stefan and Korteweg.

A propos the current being considered as electricity in motion, the contributions of Riemann, Betti, Carl Neumann, Kirchhoff and Ludwig Lorenz are described. All of them, dealing with velocity dependent forces, conclude that such forces imply propagation in time, provided that the velocity of the electricity is much less than that of the propagation. Considering that the force law represents the terms in a Taylor expansion of a force function [e.g. a potential], they are inexorably led to the consideration of differential equations of the d'Alembertian or diffusion type (what we might call field equations). A medium to enable a plausible physical interpretation of such equations becomes a consideration accordingly. Gauss was aware from the outset of this implication of a velocity dependent interaction.

Helmholtz chose a potential to underlie the interaction. His interaction can embrace that of Clausius and Maxwell in particular, and was the inspiration for the experiments of Hertz and Rowland.

André-Marie Ampère 1775 - 1836

Ampère envisages explicitly that, whether his formula be accounted for in terms of inverse square attractions, or of attractions which are exerted over no appreciable distance between particles in which there may exist vibrations of a space-filling fluid, the results of his work would not change although some further physical understanding might result.

He discusses the possibility that the force between “material particles traversed by the electric current” [deux particules matérielles traversées par le courant électrique] may be transmitted by an elastic space filling fluid, the latter being in particular that introduced by Euler and supporting the vibrations of light. This fluid was regarded as being none other than the combination of the two electricities, and as having no appreciable inertia. It is of interest to see this union of the electrical force with the material supporting the vibrations of light as early as 1823, and in the context of an author and a formula which tend to be regarded now as quintessentially action at a distance.

Carl Friedrich Gauss (1777 – 1855)

Gauss appears to have been the first (1835) to take Ampère's law further, and discuss it in relation to the concept that the current should be represented as electricity in motion. The latter is not a trivial procedure, but can readily be performed [as discussed for example by Maxwell in his treatise, Vol.II §846 et seq]. Gauss realised immediately that velocity dependent forces implied propagation in time. Because he could not conceive of the mechanism which could support this propagation, his contribution was not made known by him, being found out after his death. His force law depends only on the relative velocity of the electricities in the elements. This attitude of Gauss emphasises with greatest strength that focussing on force laws cannot be equated with action-at-a-distance. [Even though Maxwell in his treatise chooses to do so.]

Herman Grassmann (1809 – 1877)

Open Currents

Grassmann pointed out that if the observed interactions of magnets on magnets, currents on currents, or their mixed interactions are to be derived from a law between current elements, then there is freedom to select other requirements than equality of action and reaction and remain within the observations. Relinquishing the latter requirement, he provides mathematical argument which he interprets as proving that the force between a closed current and a current element is perpendicular to the latter. His interaction law predicts no force between parallel current elements. However he points out that any such elements must be part of a current system involving changes of direction and that his and Ampère's results are the same for closed circuits. An interesting aspect of his work is the consideration of genuinely "open" currents, i.e. those in which electricity can accumulate at the ends of an element.

Wilhelm Weber (1804 – 1891)

Charge in Motion

Weber is famed for his general electromagnetic interaction law, which formed a basis for virtually all further development of interaction laws in Germany, not least by providing a locus for criticism.

In going beyond Ampère's law involving otherwise undefined current elements, Weber's whole approach was predicated upon regarding the current as comprised of electrical masses in motion, in particular positive and negative electrical masses moving in opposite directions. With this choice for the current, Weber was able to add to his general law for such currents, the inverse square law for the electrostatic

interaction, in principle generating the velocity dependent law describing the interaction between two individual electrical masses. His force law was general enough to allow for varying currents, in particular leading to the law of induction, both for movement and for changing current without movement.

He appears to have abandoned any notion of a more general concept of current. One can only understand this as introducing charge as a fundamental irreducible concept, going far beyond anything which Faraday or Mossotti would have seen as legitimate.

Although Weber considered his force law as general, it was derived on the basis of experimental apparatus involving a pair of interacting coils, one of which was suspended on a bifilar arrangement echoing the structure of Gauss' magnetometer.* This allegation of generality evidently did not satisfy other workers, who approached the matter from a mathematical point of view. In this context, the attempts of Clausius, Maxwell, Stefan and Korteweg to establish general force laws are reviewed here. It may seem strange to see Maxwell here, but he had to ensure that his system conformed to the law for closed currents, and made selections compatible with his system.

* (See front cover)

Seeking General Force Laws: Clausius, Maxwell, Stefan and Korteweg.

It was clear that any conceivable forces and moments attributable to any particular current element had inevitably to be purely conjectural, provided the integrated consequences for entire circuits agreed with experiment. Researchers quickly attempted to formulate laws for current elements under these circumstances, which would be as general as the experiments would allow.

Rudolf Clausius (1822 – 1888)

Clausius takes the view that current involves the motion of only one electricity, which he takes to be positive, the negative electricity being bound to ponderable matter, and seeks the most general law between current elements, which depends only upon their separation, and time derivatives up to the second order in their coordinates, including products of first order time derivatives. In this manner, proceeding mathematically, he was led to what he considers to be the most general conceivable force law compatible with these assumptions.

Conditions are now applied to this force law:-

1. *A closed stationary current exerts no force on electricity at rest.*
2. *A closed constant current in a conductor at rest shows no tendency to change the current in another conductor at rest.*
3. *The inductive effect is the same whether one current is fixed whilst the other current grows to a given value; or the current in the latter is constant whilst the first moves from infinity to the position which prevailed in the first case.*
(*In each case, the inductive effect is represented as the time integral of the induced electromotive force, and the integrals resulting in the two cases are equated.*).

Added to these conditions, Clausius imposes the principle of conservation of energy upon the forces which two moving particles of electricity exert upon each other. This is expressed by the requirement that the work done by the forces between the particles in an element of time is expressed as the differential of a function of the particles' positions and movements. This function separates clearly into an electrostatic potential, and electrodynamic potentials.

The conclusion is that there are four possible forms for the electrodynamic potential, which can be reduced to two if it is assumed that the force as a function of distance is the same both for those coefficients which have already been defined and for those as yet undefined.

Clausius completes his work by giving what he alleges to be the only force law which is both compatible with the conservation of energy, and for which the currents are specified as having only one electricity in motion, the other electricity being at rest.

James Clerk Maxwell (1831 – 1879)

Maxwell considered the development of a general interaction law, on a basis comparable with that of Clausius, wrongly designating it "action-at-a-distance" [Treatise, Vol.II §509-527.] He dismisses the possibility of couples between elements from the outset, assuming that the force on an element ds is the resultant of three forces, in the direction of ds , in the direction of the other element ds' and in the direction of the line joining them, thus three arbitrary constants. He generates a potential for the interaction of two closed circuits, his criterion being that the action of a closed circuit on a moveable portion of another is perpendicular

to the moveable portion, which he regards as proved by Ampère. He requires the interaction between closed currents to conform to that between two magnetic shells. The result is that the elemental force law then depends on one unknown parameter. He shows that selection of this parameter can lead to Ampère's or Grassmann's law, and mentions two other plausible choices, a law depending on the angle between the elements, or on the angles between the elements and the line joining them. Of these four choices, he considers Ampère's law as "*by far the best*".

Jožef Stefan (1835 – 1893)

Stefan considers what forces are possible for elements in all possible parallel, transverse and longitudinally collinear positions. On the basis of these possible positions he develops what he sees as the most general force law, requiring neither central forces, nor the equality of action and reaction between elements.

He is led to a force law between elements, which has four arbitrary constants. Stefan shows that the interaction between two closed conductors under his presumptions leads to no conditions on the arbitrary constants. He shows that this force can be derived from the known potential for closed conductors. He now takes the view that the moment exerted by one closed conductor on another should be derivable from the angular dependence of this same potential, which leads him to one condition amongst the four constants. Ampère's law satisfies this condition as a special case, but the moments derivable from the potential do not then agree with those calculated from the forces themselves. Stefan reviews Ampère's various experiments involving closed circuits in which parts of one of the circuits or indeed entire circuits can move or rotate under constrained conditions, and shows that such experiments can lead to no further constraints on his constants. The same conclusion remains true when interactions between a closed circuit or a magnetic pole on a current element are considered.

Stefan notes that another relation between the four arbitrary constants can be achieved by specifying the unit of measurement for the currents, leaving in effect two arbitrary constants. Arbitrary choices can thus be made, without violating the two conditions amongst the four arbitrary constants. He considers the simple possibility of equating two of them to zero, or the imposition of equality of action and reaction for the current elements, leading to force laws, one of which is Ampère's, the other leading to a transverse interaction between elements one of which is longitudinal, the other transversal to the line joining them. The latter law

replaces the attraction between parallel transversal elements characteristic of Ampère's law. This latter has interesting topological implications, since the results for the closed circuits accessible to experiment is the same in both cases.

Should the condition of equality between action and reaction be relaxed, various choices allow for transverse forces only, or indeed attraction between parallel longitudinal elements. A further choice leads to the simplest realisation of Grassmann's law.

Johannes Korteweg (1848 – 1941)

Korteweg like Maxwell introduces as a fundamental principle, the idea that the force of a closed current on an open current element should be perpendicular to the latter. He also adds the condition that the force between two current elements should disappear for indefinitely large distances, the whole leading to four relations between seven arbitrary constants. A further relation can be derived under specific conditions for the closed current and an open element of semicircular form perpendicular to the plane of the former.



He points out that nothing is known experimentally for the case in which electricity can accumulate at the ends of an element (called by him the "closed" element), so that less definite constraints are applicable here. He gives no analytical argument in this case, but does argue that, plausibly, two of the above four conditions can be regarded as applying to the "closed" element.

For each of Ampère's and Grassmann's laws respectively, five of the seven arbitrary constants can be equated to zero, a different set of five in each case.

Retardation and Velocity Dependence: Riemann, Carl Neumann, Betti; Clausius' Criticism.

As mentioned above, Gauss appears to have been the first to consider the elemental force law as derived from electricity in motion, but realised that this necessarily implied propagation in time. Because he could conceive of no mechanism for this, he did not make his force law known. Later workers pursued this line, in particular Riemann, Carl Neumann and Betti.

Bernhard Riemann (1826-1866)

Riemann addressed the problem in 1858, in a work which he had previously withdrawn and was published in 1867 after his death. He assumes that the current interaction is described by a simple inverse distance potential law, the motion being reflected in the time dependence solely of the coefficient of $1/r$.

His crucial assumption is that this potential propagates in time at a very high speed, so that the simple potential takes a retarded form, meaning that the effect of the potential is governed by its value at a time earlier by the separation of the two elements divided by the speed of propagation, in practical situations a very small time interval.

In effect he now makes use of Taylor's expansion law to express the effect of the potential in terms of its value and derivatives at the beginnings of the time intervals. In effect, only the zero order and first order of the expansion need be retained, and the velocity dependent potential in the form evolved by Weber is derived. The procedure is not without criticism and this is thought to be the reason for its withdrawal, by Riemann, from publication in 1858.

Carl Neumann (1832-1925)

In 1868, Carl Neumann advanced Riemann's line of research. He took the view that Weber's potential law should be taken to be a discovery with its introduction of a general constant in the velocity dependent term.

In the context of a general discussion of the use of the potential function in generalised mechanics, with forces independent of velocity (as for example Newton's inverse square law) he interprets Riemann's work as an attempt to establish a formalism which retains the potential function in the previous form yet

sets out to explain the interaction of current elements by introducing the requirement that this potential function propagates in space at a certain constant speed, in a manner similar to light.

Neumann states his surprise that the use simply of the Newtonian potential with the propagation assumption leads exactly to the potential established by him for Weber's velocity dependent law, acknowledging the importance of this.

He develops a formalism which is general enough to embrace currents comprising the motion of either one, or of both positive and negative electricities. The moving force generated by the potential arising from his formalism is regarded as transferring from one mass point to the other, in space, with a definite speed. He introduces the concept of the emissive and the receptive potential, the separation between the mass points being a function of time. He argues that the potential itself propagates in time, and that this concept is distinct from that which would stem from a field equation describing dynamic qualities of a medium.

The receptive potential thus stems from the emissive potential at an earlier time equal to the separation divided by the speed of propagation imagined large. He too expands this receptive potential in terms of the value of the emissive potential and its derivatives at the time and place of emission, retaining terms only of the first order in the time difference [separation/propagation speed]. For the special case of the Newtonian potential, the result after propagation is just Weber's potential.

Enrico Betti (1823-1892)

Betti was also inspired, both by the remarks of Gauss as well as Riemann's work, to a similar line of research. Although he was aware that Riemann chose not to make his work public in his lifetime, Betti thought the ideas too important simply to languish, hence his own development also involving propagation.

He represents his current elements as periodic functions in time, reaching a mean value at repeated constant intervals, the motion representing the current being in the direction of the elements so that the current varies with the above period.

As his starting point, he takes a Newtonian type potential of one constant current on the other. With the currents varying as above he modifies the potential by associating the state of one current with that of the other at a later instant, the interval being equal to the propagation time from one current to the other at a constant speed. He too regards this interval as small, yet larger than the periods of the currents, and makes a Taylor expansion in powers of the propagation interval.

In this manner, he demonstrates that Weber's form of the potential can be retained also for his chosen form of changing current, provided that time for the propagation be allowed.

Rudolph Clausius agreed that these researches were significant, but found none of them satisfactory in the mathematical sense. In his critique of all three of them, he criticises Riemann's work on the ground that sequences of integration are reversed in an impermissible manner, yet praising him for originating a line of enquiry which he sees as inevitably significant.

With reference to Betti's work, he simply points out that the periodic functions which represent the current can be chosen to have frequencies which render the convergence of the Taylor expansion doubtful.

Clausius' criticism of Neumann's work rests on the incompatibility of the propagation of the latter's emissive potential with the propagation of light; a reform of Neumann's potential to be compatible with the propagation of light as he (Clausius) understands it, leads to a result at variance with Weber's law.

In a robust defence of his position, Neumann emphasises that his potential is to be understood as an order to an obeying body. The emissive potential at an earlier time becomes a receptive potential at a later time, and in practice depends on the relative velocities. He contrasts this concept with that involved in the propagation of light.

The works of Riemann, Neumann and Betti demonstrate that the association of "static" type potentials with the notion of propagation in time, or retardation, leads to force laws, which, if not unique, have precisely the form of Weber's law and can include the latter. Their work also demonstrates indirectly that the imposition of a field equation for the propagation of the potential (as for example in Riemann or indeed Maxwell) leads to an analytic completion of the potential in a way which goes far beyond any aspect which can be verified experimentally, in view of the enormous value of the velocity.

Gustav Kirchhoff (1824-1887)

In a work of 1857 Kirchhoff considered the movement of electricity in wires. He considers the possibility that the current, taken to be equal and opposite electricities in opposed motion, may accumulate at locations in the wire, such accumulation being resisted by normal electrostatic forces represented by an electrostatic potential. A second crucial aspect is his introduction of the notion of conservation, namely that any net accumulation of current into a region is equivalent to the increase of free electricity in that region.



Gustav Kirchhoff

Adding to these two the effect of Weber's electromagnetic potential between elements of the current, Kirchhoff deduces the presence of damped waves in the wire, the degree of damping being dependent upon the wire's resistance. He compares the propagation of electricity with that of longitudinal wave motions in a rod. He points out specifically that, for current in the wire, the wave velocity is just that of Weber's velocity constant, thus essentially the speed of light. He also discusses boundary conditions for both open and closed (re-entrant) wires, and indicates that reflections of current discontinuities traverse between the ends of the wire at the same high speed.

Although Kirchhoff's treatment involves equal and opposite electricities moving in opposed directions, he points out that the treatment can be applied for a general notion of electricity, provided the conservation law is appropriately amended.

In a further development, Kirchhoff extends his treatment to conductors of general form, using the conservation law, and the electrostatic and electromagnetic potentials in an extended three dimensional format. His primary conclusion, before the general equations are applied to the linear conductor is that, in general, free electricity is to be found within the conductor, by which he means electricity in dielectrics, a conclusion which is independent of the conductor itself.

Ludwig Lorenz (1829-1891)

It will be seen from the work of L Lorenz that this is crucially linked to the concept of displacement current.

With the insight gained in earlier work [for example Riemann's] and on the basis of the laws for the propagation of electricity under the influence of both the free and the current electricities in the surrounding medium, Lorenz shows that periodic currents are possible which behave in precisely the manner of light. He expresses with confidence *the vibrations of light are themselves electric currents*. He points out that closed changing currents induce parallel changing currents, just as the light vibrations induce parallel light vibrations. The absence of any longitudinal light vibrations argued, in his view, strongly against any other medium for the support of light vibrations.

His reasoning starts from precisely the position of Kirchhoff, working on the basis of the electrostatic potential whose effect is to resist any accumulation of electricity, of the Weber potential for current electricity, and accepting Kirchhoff's hypothesis of electricity conservation.

Lorenz's approach, reflecting that of Riemann, is to evaluate each of the potentials at the earlier time corresponding to propagation of the potential at a constant speed.

Reflecting the procedure introduced by Riemann, Lorenz expresses the potential at the earlier time in terms of the Taylor expansion of each potential in terms of its value and time derivatives at the time of its effect on the current components. Lorenz remarks that powers of the expansion higher than the first are utterly negligible. He regards it as compelling that the expansion can generate precisely the form of Weber's or Neumann's law of current interaction, and so hypothesises that the electrostatic and the electrodynamic potentials, propagated at a definite speed, express precisely the interaction between current components accordingly. On this basis he is led to a form of diffusion equation for each of the current components at a given point, the diffusion aspect relating to the conductivity.

Thus Lorenz arrives at the concept that light can be understood in terms of currents in a medium of definite conductivity, being in general dispersive. When the conductivity is high the medium becomes opaque. When it is vanishingly small, the medium ceases to be dispersive. It is clear that Lorenz's concept of current must embrace Maxwell's displacement current in the latter case.

Lorenz, in further development of his theory, generates equations which bear a strong resemblance to those found in Maxwell's treatise, yet are not identical to them. It is likely that they are the same as Maxwell's, subject to parametric transformations. In this, one can perhaps be guided by Maxwell himself, who refers to Lorenz's work in his treatise, noting Lorenz's conclusion that:

"the distribution of force in the electromagnetic field may be conceived as arising from the mutual action of contiguous elements; and that waves, consisting of transverse electric currents, may be propagated, with a velocity comparable to that of light, in non-conducting media. He therefore regards the disturbance which constitutes light as identical with these electric currents, and he shews that conducting media must be opaque to such radiation. These conclusions are similar to those of this chapter, though obtained by an entirely different method."

[Vol. 2 p450.]

In human terms, Maxwell was sensitive enough about the matter to add:

"The theory given in this chapter was first published in the Phil. Trans. for 1865, pp. 459-512."

[Lorenz's work was published in 1867, whereas the treatise is dated 1873.]

It is of interest to note that Maxwell describes Lorenz's theory as being:

"deduced from Kirchhoff's equations of electric currents, by the addition of certain terms which do not affect any experimental result...."

The addition corresponds precisely to the adjoining of all the neglected terms in the Taylor expansions of the electrostatic and electromagnetic potentials of Kirchhoff, to arrive at the corresponding propagated potentials.

One can remark from the work of Kirchhoff and Lorenz that the propagation requires the accumulation of electricity, a conservation law governing the accumulation and a medium, which can be matter itself as distinct from the ether, if the potentials are to be interpreted as physical.

The very general sense in which the German terms “Leiter” (conductor) and “Strom, Ströme (current, currents) are used in these works needs to be noted. The term “Leiter” specifically embraces what is called in English language works, the dielectric, the term “Strom” embracing not only current as such, but to some extent the sense of electricity in general as it was understood in the nineteenth century. Maxwell, perhaps unwittingly, captures these more general senses quite accurately in the above quotation when he describes Ludwig Lorenz’s work in terms of “waves consisting of transverse electric currents ... in non-conducting media” and describes Lorenz’s conclusion as “similar to those in this Chapter...”, the chapter in question [Maxwell Treatise, Vol2 Chapter XX] being that in which his electromagnetic theory of light is developed.

It is perhaps the failure of historians to appreciate this generality in the usage of “Strom” and “Leiter” which may have led them wrongly to dismiss the Continental work as irrelevant.

Hermann von Helmholtz (1821-1894) and the Potential Law

Taking a more abstract approach than those of the preceding authors, Helmholtz was led in 1870 to synthesise the laws between current elements in the form of a potential law with a free parameter. His preliminary constraints in leading him to this form of potential were that conservation of energy should apply, and that the distance dependence of the potential of one current on another should be the same for both closed and open currents. Although the matter was disputed by Poincaré, he argues that, for three values of his free parameter, the laws of F. Neumann (father of Carl Neumann), Maxwell and Weber are regained. The three potentials differ by a quantity whose contribution to the forces vanishes when integrated over closed currents. He claims that his potential leads to the most general law which applied to elements gives the verified result for closed currents. It represents the total amount of work which can be achieved changing the currents and their distribution in the body or surface of an extended conductor. Its form, integrated over the body and surface is composed of a quadratic form of the derivatives of the potentials of the currents and free electricity in the conductor, reminiscent of the quantity called the field energy in modern field theories.

As had Kirchhoff before him, Helmholtz considers the propagation of electricity in conductors. He proceeds on the basis of his general potential to derive expressions for the interactions between elements of the free and current electricities regarded as functions of the position within an extended conductor which may embrace the entire space. These expressions are in terms of the potentials of the free electricity, and in terms of the component directions of the current at each point. As had Kirchhoff before him, he introduces a conservation law for the free and current electricities, and can thus formulate equations relating to the potentials and the currents. The current itself is the consequence of spatial change in the potential of the free electricity and of the change in time of the current potentials, depending upon the conductivity of the medium, so that equations of motion can be derived for the potentials alone. These equations allow for, in general, damped wave motions of the currents in the extended conductor, the damping decreasing with the conductivity of the medium. The potentials are propagated in the same manner as in Maxwell's theory which is embraced as a special case.

Physically, Helmholtz's formalism, like that of Kirchhoff and Lorenz before him, is concerned with the propagation of current as the physical reality. In general, longitudinal as well as transverse waves are possible.

It seems that his perception of current and its accumulation at the surface of conductors did not allow him to perceive the continuation of the discharge from a discharge point as a continuation of a current which was thus closed. One would imagine that this would arise from the lack of any constraints in that part of the current, whose variations in the sense of the potential law would lead to a corresponding electrodynamic force.

A-propos the question as to whether or not electricity had to be moving or present within a conductor, before there were any electromagnetic effects, Helmholtz returned to an experiment involving a radial conductor and sliding contact, in which he attributed the electromagnetic force to end forces at the location of the sliding contact. The possibility of end forces could be eliminated by replacing the sliding contact with an arrangement in which a condenser whose plates were fixed or rotated with the radial conductor, could be charged if Faraday's law of induction applied, or would not be charged if the potential law applied. In the result, the condenser was charged. Helmholtz concluded from this that, as long as the potential law was confined to movements of electricity in conductors only, it conflicts with the facts.

He suggests that his use of the potential law has led him to experiments which would enable him to distinguish between the many possible interpretations of the electromagnetic interaction.

In this respect he points out that experiments with closed currents cannot distinguish the seven different laws:-

- i) Ampère – central forces;
- ii) Grassmann – transverse forces;
- iii) F. Neumann – potential law;
- iv) Weber – central forces dependent on absolute velocities;
- v) Gauss/Riemann – force dependent on relative velocity;
- vi) C. Neumann's propagated potential;
- vii) Maxwell – all forces dependent upon electric and/or magnetic polarisations in the ether:-

and considered that the charge experiment might do so.

He points out that the result of this experiment can be fully accounted for by the potential law if it is accepted, along with Faraday and Maxwell that movement of electricity in insulators (sic) also has an electromagnetic effect, in that the insulators can be polarised. Then there are no open currents at all, and any accumulation of electricity is immediately propagated further in the dielectric as the equivalent polarisation. Helmholtz even seeks to model the ether between the plates of the condenser, noting that acceptance of the approach would depend on further enquiry into the electromagnetic effects of insulators. It is in this context that he suggested to Heinrich Hertz (1857-1894) that he work on the electromagnetic effects of currents in dielectrics, leading to his famed researches in this area.

General Remarks

The above generalisations of the electrodynamic force law between current elements emphasise how far from experimental verification any law based upon the interaction of current *elements* must be. Each author, in imagining the most general law as a starting point, has chosen a distinct set of possible force and moment patterns for each of the elements, which, although overlapping to some degree, depend to a large extent on personal predilection and imagination.

Within the constraint of predicting the observed results for the action of closed circuits upon each other, considerable choice was possible, as each of the authors demonstrate. A definite force law for the elements was only possible on the basis of plausibility arguments, and the imposition of laws such as the conservation of energy and equality of action and reaction at the elemental level, for which no experimental justification was apparent.

Stefan and Korteweg are both silent as to what constitutes a current, apart from an unspecified notion of "electricity". Clausius explicitly assumes either motions of positive and negative electricities in opposite, not necessarily equal speeds, or motion of one electricity only, or a general circulating motion of one electricity of negligible mass about the opposite electricity comprised in a nucleus (Kern) carrying virtually the whole mass of the ponderable matter. The latter notion was introduced to allow for the existence of the Amperian currents considered to be the cause of magnetic phenomena.

Conclusion

It is clear that working with the elemental law itself cannot be dismissed as expounding the doctrine of "action-at-a-distance". If field equations are considered to be in the picture, so to speak, then, in speaking of the elemental law one is, logically, working with solutions to the field equations rather than the equations themselves. In this respect, all the researchers were perfectly aware that, as soon as the force law was contemplated to be velocity dependent, it was inevitable that the interaction propagated in time. Gauss was aware of this in the 1830's. Each of the workers Riemann, Carl Neumann, Betti, Clausius and Ludwig Lorenz performed what could be called a process of analytical completion to produce what we would see as field equations of the d'Alembertian or diffusion type to describe the propagation of a potential or (Neumann) an instruction in time.

There are two points to be noted here:-

1. There is no imperative whatsoever to regard what was propagated as other than purely mathematical, being capable of representing at each point, by a purely mathematical operation, what the elemental force would be if moving electricity or a current element were to be at that point. One could term this an extensive conditional interpretation, and is precisely the manner in which the field concept was first defined. Nonetheless,
2. There was a very strong feeling that what was propagated could be real, in a medium, and be associated with stresses and strains, that is to say "energy" in that

medium. This is specifically true of L.Lorenz. It is a small step from such a consideration to that in which the field is regarded as a physical density function (the modern concept), defining specific energy and momentum densities, and inevitably their fluxes, at each point where the field exists. However, this must be seen as an outright physical assumption dependent upon the existence of a medium, and quite distinct from the existence of the equations of propagation themselves.

Where the researchers referred to here have used potentials in the absence of a presumed medium, the potential has had no dynamic significance other than that its derivatives or variations (in the sense of the calculus of variations) lead to electromotive or ponderomotive forces and possibly moments. To extend such a potential by transforming the energies of interaction into local, intensive expressions for field energies and thus as an unwelcome by-product introducing the notion of "self" potentials, is a process of pure physical interpretation independent of any of the mathematical forms dealt with.

The works of Korteweg and of Helmholtz in particular show that any force law which does not take account of elemental moments which must arise due to the angular dependence in either a potential, or in a force law itself when applied to an element, is incomplete. In this context, it is well to bear in mind that, historically, current is an abstract concept defined by the interaction, which concept can embrace convection at a highly derived level but is not exhausted by it.

H.A. Lorentz chose to progress on the basis of convection of charged particles alone, to the exclusion of other possibilities which remained viable at the time, evolving a comprehensive theory of the electromagnetic interactions on the basis of this hypothesis. His whole work showed that convection of electrons associated with Maxwellian electrodynamics, in the form given it by Heaviside, could provide a sufficient means to explain a wide variety of electromagnetic phenomena.

In essence, the research into the most general possible natures of free and current electricity ended with Hertz's selection of Maxwell's theory of the electromagnetic field, and Lorentz's selection of convection as the sole form of current electricity.

Liénard, Schwarzschild, tautology, spin.

To turn the wheel full circle so to speak, we can work from a field equation, to a solution for the case of the Maxwell/Lorentz theory in general use today. This was done by Liénard in particular, via what is now called the Liénard/Wiechert potential. Liénard calculated the general interaction insofar as it related to the arbitrary motion of an extended “electron”, expressed in terms of the fields [interpreted in the extensive conditional manner] which this electron would produce. Is this law the ultimate, correct one? One could never say. The electron as we understand it is above all an artefact of the Maxwell/Lorentz theory, so that Liénard’s calculation should be seen as an interpretation of some of the electron’s properties, and to that extent, tautological. The calculation is cumbersome and requires considerable skill and physical intuition, to the extent of emphasising that, in the absence of a medium, there is little practical value in working with field equations, rather than their solutions. The calculation itself can be found in Feynman’s Lectures on Physics Vol.2 21-5. The calculation there is much the same as Liénard’s, although Feynman chooses a cubical electron as distinct from the more general form of Liénard.

A more interesting example is the calculation by Schwarzschild, also using the Maxwell/Lorentz electromagnetic theory, but allowing that his electron had angular momentum (perhaps spin in modern terminology). His result requires that such an electron moving in the absence of external forces would not move in a straight line, but would precess in a spiral path, with the directions of its centre of gravity motion, its axis of rotation and the axis of the spiral always in a plane. Can such a motion be possible? Yes because the equations, whatever else they may be, are equations of fluid mechanics. Schwarzschild points out that this spiral motion would not be perceptible under the observed conditions of cathode or of β rays (as in the Kaufmann experiment), as long as the peripheral velocity is not more than, for example $(1/5)c$.

BUT, we now know that the electron angular momentum is $h/4\pi$. this means, for an electron of (i) classical radius, of (ii) Compton wavelength and (iii) Bohr Radius, peripheral velocities of (i) 99.5% c , (ii) 0.7 c (iii) αc , where α is the fine structure constant. Only the third would meet Schwarzschild’s condition.

Schwarzschild's solution suggests that a far-reaching revision of the entire elemental interaction may be needed – much more than merely stitching electron spin onto an extant theory.

The work reviewed here is pretty much available only in German. However, much of it is covered in the text “Electrodynamics from Amperè to Einstein”, by Olivier Darrigol, OUP 2000.

The work “Electromagnetics, a Discussion of Fundamentals”, by Alfred O’Rahilly, Longmans 1938, brings out admirably the enduring conjectural status of electromagnetic theory. It is a work which is not “dated”, in spite of its age.

I am indebted to Dr. John Roche for bringing both of these works to my attention.

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Early Days In Particle Physics.

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The fifteen-year period 1932-1947 was of great significance in the early history of particle physics. It was then that many discoveries were made simply by observing the nuclear interactions of the cosmic radiation which, discovered earlier by Victor Franz Hess (1912), was at that time the only available source of high-energy particles. One serious consequence of the Second World War was that it seriously limited communication between physicists in different parts of the world; however, some progress was made then, using experimental techniques that were quite unsophisticated compared with those developed in dedicated particle-physics laboratories in subsequent years.

One of the most important lines of research during those early years in experimental particle physics was initiated by Hideki Yukawa (1935),



Hideki Yukawa

who suggested the existence of a particle that would have a role in relation to the nuclear-force field analogous to that of the photon in the electromagnetic field. Yukawa predicted that the particle should have three essential properties:-

- i) *It should have a mass about $200m_e$ where m_e is the mass of the electron: this followed from the argument based on the application of the Uncertainty Principle to the virtual particle in the nuclear field, that the range of the nuclear force, already established experimentally, should be given by \hbar/mc , where m is its mass.*

- ii) *It should be unstable with respect to β decay. If the exchange of Yukawa's particle (Y) between nucleons in the nucleus, $N \leftrightarrow N + Y$ (the hypothetical mechanism for the nuclear force) were to be accompanied by the β decay $Y \rightarrow \beta + \gamma$ (where γ is a neutrino), then the nett result should be $N \rightarrow N + \beta + \gamma$ the already well-known form of β decay: this would then suggest that the β -decay process for the free particle should occur if $m(Y) > m_e$, as originally proposed.*

- iii) *It should necessarily, as the mediator of the nuclear force, interact strongly with nuclear matter.*

The research that followed, during the period of interest here, may be seen as the search for a particle with all three of these essential properties.

The experimental technique that played the major part in the initial phase of the search for the Yukawa particle was the Wilson cloud chamber, in which particle tracks formed by condensation upon ions in a supersaturated vapour (produced by a process of rapid expansion) could be recorded photographically. It had been used by Carl Anderson (1933) in his discovery of the positron in the 'soft' (easily absorbed) component of the secondary cosmic radiation, which consisted of high-energy electrons and photons produced by interaction of the primary cosmic rays in the Earth's atmosphere. The main disadvantage of the cloud chamber as it was first operated lay in its inefficiency. With a sensitive time $T_s \sim 10\text{-}100\text{ms}$ following the initial expansion, compared with a recovery ('recycling') time $\sim 1\text{-}10\text{minutes}$, it was inevitable that random operation would record only a minute sample of the particles incident upon the chamber. A great improvement in that respect was the counter-controlled chamber devised by P M S Blackett and Giuseppe Occhialini (1933), in which the expansion of the chamber could be triggered by a coincidence pulse produced by the passage of a charged particle through two Geiger counters,

situated above and below the chamber. Such a device could be selective in its operation and take only those photographs in which the track of a particle, perhaps together with any interaction event which the particle might have produced, would have been recorded. This arrangement of counters and cloud chamber was employed by Blackett and Occhialini in their original observation of electron-positron pair production, the process that had earlier been predicted by Dirac (1928). In such a mode of operation, however, there was the disadvantage that the chamber expansion would take place $\sim 10ms$ after the passage of the particle that had defined its track, with the result that during that interval some diffusion of the ions (over $\sim 1mm$ in $15ms$) would occur, leading to a certain loss of track definition.

With the discovery of the positron by Anderson, followed by the observation by Blackett and Occhialini of electron-positron pair production, the soft component of the cosmic radiation could be explained as the 'cascade' that results from successive interactions of electrons and photons, and their interaction products, in the atmosphere. These processes were well described by the basic theory of the electromagnetic interaction of particles in matter (Bethe and Heitler, 1934).

Several years earlier, a counter experiment by Bruno Rossi (1932) had suggested that, in addition to the soft component, there were, in the cosmic radiation at ground level, particles capable of penetrating layers of dense absorber, even after their passage through the whole depth of the atmosphere. Further experiments, with a cloud chamber, carried out by Anderson and Neddermeyer (1934) clearly indicated the existence of this 'hard' (penetrating) component. It appeared that the penetrating power of the hard component, the particles of which were found to have both positive and negative charge, could be attributed to their having a much greater mass than the electron. The hard-component particles would, as electrons do, lose energy by the process of ionisation; but in the process of energy loss by radiation (the rate of which for a given particle energy is proportional to $1/m^2$, where m is the particle mass), their energy loss would be significantly less than it would be for electrons. Neddermeyer and Anderson (1937) came to the conclusion that, 'The experimental fact that penetrating particles occur both with positive and negative charge suggests that they might be created in pairs by photons, and that they might be represented as higher mass states of ordinary electrons'. This early speculation regarding the nature of the hard-component particles is especially interesting when seen in the light of much-later developments in particle physics; but the very fact that the particles had been shown to exist was sufficient alone for a significant advance in the search for Yukawa's particle to be made.

With the general realisation at about that time that the hard-component particles were of mass significantly greater than that of the electron, but less than that of the proton, the term 'meson'(alternatively 'mesotron') came into use to describe them.

Attempts were made by several research groups to measure the mass of the meson in cloud chamber experiments, two of the earliest having been carried out by Street and Stevenson (1937) and by Neddermeyer and Anderson(1938). The latter authors reported a value of $(220 \pm 35)m_e$, which agreed well with values obtained in subsequent experiments[See Thorndike (1952)]. The meson of the hard component therefore fulfilled the first essential property for its identification with Yukawa's particle.

The second predicted property, that Yukawa's particle should be unstable with respect to decay, was also demonstrated by the meson; this was first recognised by Helmuth Kulenkampff (1938) following experiments on the attenuation of the hard-component cosmic radiation carried out by several research groups using various arrays of counters and absorbers. Rossi et al.(1940) were able to show that anomalies apparent in the attenuation of the particle flux in absorbers of very different densities(e.g. air and carbon) could be resolved by allowing about equal importance to the processes of absorption and decay; the mean lifetime of the particles consistent with the experimental results was found to be $2.5 \mu\text{s}$. However, the nature of the decay process had not yet been determined, nor had the identity of any decay product been established.

In a theoretical study of the basic thermodynamics of the cloud chamber, which proved to be particularly relevant to further progress, EJ Williams (1939a) was able to calculate cloud-chamber sensitive time (T_s) from first principles. It could as a result be concluded that, when operated at atmospheric pressure, a sufficiently large cloud chamber (e.g. one with linear dimensions of about 30cm), which would also present a large cross-sectional area to the incident cosmic-radiation flux, could have $T_s \approx 0.4\text{s}$ and would be able to record meson tracks at the greatly improved rate of one or two per second.

It was with a chamber of this type, operated at atmospheric pressure in a magnetic field, that Williams (1939b) obtained the energy spectrum of both the soft (electron) and the hard cosmic-radiation components. (The chamber was randomly operated because counter control would have led to a bias against the detection of low-energy particles.) The electron component at ground level was found to be appreciably larger than expected. Because, as had already become apparent, the atmospheric absorption of the soft component as originally defined was so great,

the observed electron flux at ground level had to be explained as being partly due to a secondary effect, i.e. decay of the hard-component particles, the mesons. Euler and Heisenberg (1938) calculated, from the known flux of the mesons and their previously estimated lifetime, the flux of meson-decay electrons at ground level: this was found to be as much as 0.3 of the meson flux itself, which explained the electron excess that had been observed.

By means of the same cloud chamber, with its long sensitive time and large volume, Williams and Roberts (1940) successfully created the conditions under which a cosmic-ray meson ('mesotron') could be arrested in the chamber, so that any transformation that might occur at the end of its range could be observed. The first event of this kind to be identified is shown in Fig.1(a) and Fig.1(b). (below)

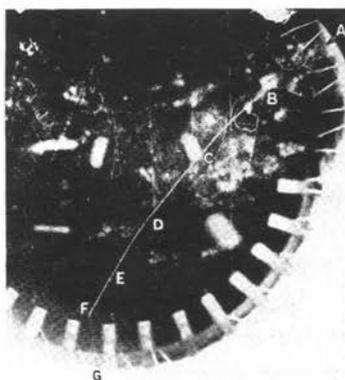


Fig.1(a)

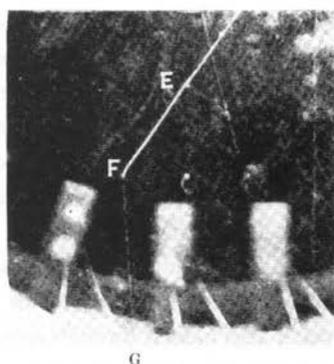


Fig.1(b)

Cloud chamber photographs showing the decay of the mesotron (Williams and Roberts, 1940).

As Williams and Roberts reported, 'A recent photograph taken with this [cloud chamber] shows a mesotron terminating in the gas as desired. From its end there emerges a fast electron track, the kinetic energy of which is very much greater than the kinetic energy of the mesotron, but is comparable with its mass energy. This indicates that the mesotron transforms into an electron, in which case the remarkable parallel between the mesotron and the Yukawa particle is taken one stage further. In terms of Yukawa's theory, the phenomenon observed may be described as a disintegration of the mesotron with the emission of an electron, thus constituting the most elementary form of β -disintegration'.

From the curvature of the meson track at the point B in the photograph of Fig(1a), and for the particular magnetic field applied, together with the residual range of the particle(in air at atmospheric pressure) measured from that point, a meson mass of $(250 \pm 70) m_e$ was obtained, in agreement with estimates from previous experiments. The direction of curvature of the track in the magnetic field indicated that meson and electron had positive charge. Observations were also made with a second chamber, containing argon at high pressure (80 atm), and two further examples of meson decay were observed (Williams and Evans, 1940).

Further experiments relating to the decay of the meson were carried out in subsequent years, in other laboratories. It became a matter of great interest to distinguish between the decay characteristics of positive and negative mesons, if these characteristics were indeed different. As pointed out by Tomonaga and Araki (1940), a negative meson that stops in a material should, after capture into bound atomic states, be attracted to a nucleus and should be absorbed by it before having an appreciable chance to decay, whereas a positive meson should be repelled by the nuclear charge and then decay without nuclear interaction. The definitive experiments on the absorption of mesons of both positive and negative charge in different materials were those devised by Conversi et al. (1945, 1947). In their apparatus two adjacent blocks of iron, magnetically polarised in opposite directions, served as a magnetic selector for either positive or negative incident particles, which also passed through a two-counter 'telescope', before finally stopping in an absorber; the decay electrons produced were then detected by counters. From the numbers of positive and negative decay electrons observed for absorbers made of different materials, Conversi et al. obtained a remarkable result: in their first experiment, with an iron absorber, the expected result was obtained, that is the negative mesons underwent nuclear absorption while the positive ones decayed; but in their second experiment, with a carbon absorber, both positive and negative mesons decayed. The conclusion was that for a material of low atomic number, a negative meson was able to decay even though it had spent a significant fraction of its lifetime within the atom in low-lying atomic states that would have allowed an appreciable overlap of its wave function with the volume of the nucleus. *'This observation was of great importance because it showed quite directly that [the meson] did not have the strong interaction with nuclei expected of the nuclear force particle predicted by Yukawa, and was actually the first experiment to show this in a completely unambiguous fashion'* (Thorndike, 1952).

This conclusion prompted the suggestion by Sakata and Inoue (1946) that the meson is 'an elementary particle which has close correlations to the Yukawa particle, but it should be considered as an elementary particle of a different sort'; Marshak and Bethe (1947) also made the clear distinction between the Yukawa particle and the meson of the cosmic radiation.

Further progress had to await the arrival of a new experimental technique: this was provided by the nuclear photographic emulsion. A solid material such as an emulsion had the advantages of high stopping power for particles and, under microscopic examination, high spatial resolution for any tracks that might be associated with particle interaction or decay events; emulsions were also robust, continuously sensitive and required the minimum of ancillary equipment. Nuclear emulsion plates could be exposed at great altitudes, nearer the presumed source of the secondary cosmic radiation (in the high atmosphere), including - a point that is particularly relevant - the source of the hard-component mesons. Plates exposed initially on mountain tops were processed and then examined at several laboratories; they gave results that were at once both revealing and conclusive.

Events discovered in nuclear emulsion plates by Perkins (1947) and by Occhialini and Powell (1947) clearly represented the interaction of negative particles, of mass similar to that of the meson, with nuclei in the emulsion plates; but unlike the mesons, these particles, which were also brought to rest and then (being negatively charged) captured into atomic states, experienced a strong interaction with nuclei, causing nuclear disintegration with the emission of energetic protons or other nuclear fragments (Fig.2).

The conclusion drawn from these events was that such particles fulfilled the essential condition required of Yukawa's particle, that of strong interaction with nuclear matter. Here was definite evidence that Yukawa's particle had at last been found.

Fig.2 Nuclear disintegration event caused by negative particle (curved track) captured in nuclear emulsion plate (Perkins, 1947)



The enigma of the meson itself was solved only by a further discovery. In a particular type of particle-decay event found in emulsion plates by Powell and his group (Lattes et al. 1947), there appeared to be two different kinds of particle involved: the lighter one, tentatively identified as the familiar cosmic-ray meson, arose from the heavier one, which had been brought to rest in the emulsion, by a spontaneous decay process. Later observations of events in more-sensitive emulsion showed that the lighter particle, at the end of its range in the plate, decayed in its turn to give rise to an energetic electron, the same process that had originally been observed by Williams and Roberts (1940) in their cloud-chamber experiment.

The most remarkable feature of the events recorded in the nuclear emulsion, however, was the unexpected appearance of the heavier particle, which was named by Lattes et al.(1947) the π (primary) meson, while the lighter particle was designated as the μ meson and now referred to as the muon. The whole decay sequence could then be written as $\pi \rightarrow \mu \rightarrow e$ (Fig.3).

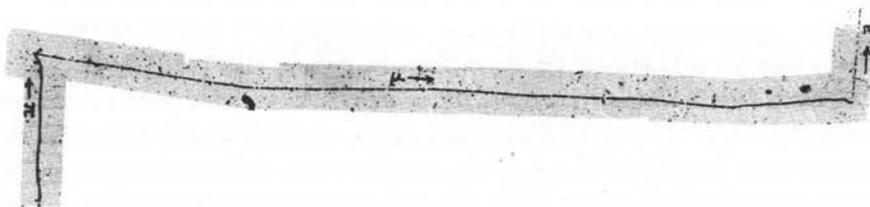


Fig.3 Decay of π^+ meson in nuclear emulsion plate, showing the decay sequence $\pi \rightarrow \mu \rightarrow e$
(Powell, Fowler and Perkins, 1959)

The π mesons observed to decay in this manner could be identified as positive particles (π^+); while those that had previously been observed to cause, at the end of their range, the disintegration of nuclei, were identified as negative (π^-).

The modern convention is to use the term 'meson' to refer only to strongly interacting particles. The muon is now classified together with the electron as a lepton and may be regarded as a heavy electron which, like the electron, experiences the weak but not the strong (nuclear) interaction. The experiment by Conversi et al.(1945) in which it was found that negative muons (μ^-) were absorbed by heavy(iron) nuclei indicated a process that may be described as a form of K-capture, $\mu^- + p \rightarrow n + \nu$, where ν is a neutrino(analogous to electron K-capture, $e^- + p \rightarrow n + \nu$), and which, being related to β decay, is a weak interaction rather than the strong interaction characteristic of the π meson.

With the discovery of the π meson, resulting from 'the beautiful and timely work of Powell's group', as described by Yang (1962), it was possible to gain a satisfactory understanding of the processes by which the hard component of the cosmic radiation is generated. Nuclear interactions of the primary cosmic rays in the high atmosphere produce energetic π mesons(both π^+ and π^-) which can then decay with a short lifetime(estimated to be $\approx 2 \times 10^{-8}$ s) to give the longer-lived muons, the hard-component particles, which can survive to be observed at ground level.

With this brief description of the generation of the hard component of the cosmic radiation we conclude our account of the discovery Yukawa's particle, having paid particular attention to the part played by the enigmatic muon, during those early days of particle physics when its existence was first realised. Why the muon, as a heavy electron, should exist at all remained a more profound enigma.

Acknowledgements

I would like to express my appreciation of the contribution to the contents of this article made by my late colleague Dr E.M. Job, in passing on to me valuable material bequeathed to him by Professor E.J. Williams, F.R.S., his former research supervisor in the Physics Department, University College of Wales, Aberystwyth.

I thank Professor D.H. Perkins, F.R.S., for permission to reproduce, as Fig.2 and Fig.3, illustrations from 'The Study of Elementary Particles by the Photographic Method' (Powell, Fowler and Perkins, 1959).

I would also like to thank Mr Adrian Potter for generous assistance in the preparation of this article for publication.



N.B. This article is based on a talk given at a joint IOP meeting of the HoP Group, High Energy Particle Physics Group and the South-West Branch, in October 2007 at the University of Bristol, to mark the 60th anniversary of the meson discoveries at Bristol and Manchester. - *Editor*

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Crystallography Before Computers: How We Summed Our Fourier Series.

Dr. Stephen C. Wallwork.

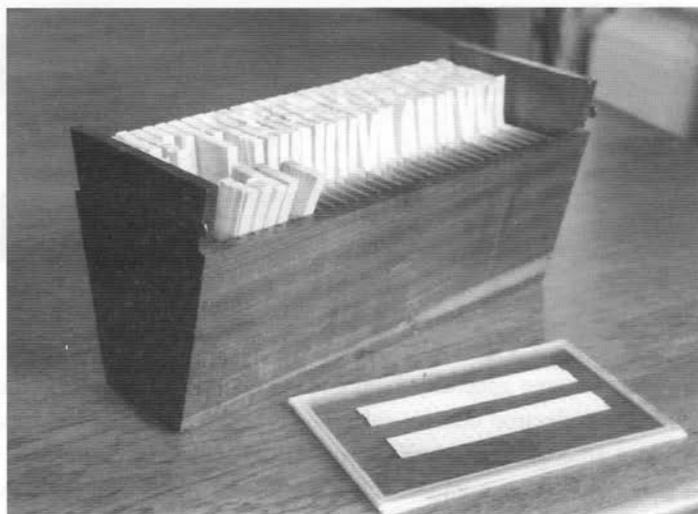
Department of Chemistry, University of Nottingham

100	OE 1	100	99	98	95	91	87	81	74	67	59	50	41	31	21	10	0	(100A)
24	OE 1	24	24	23	23	22	21	19	18	16	14	12	10	7	5	3	0	(24)
31	OE 3	31	29	27	18	10	0	10	18	29	31	29	25	18	10	0	(32)	
69	OE 2	69	67	65	56	46	34	21	7	7	21	34	46	56	65	67	69	(6)
8	OE 4	8	7	5	2	1	4	6	8	8	6	4	1	2	5	7	8	(8)
3	OE 6	3	2	1	1	2	3	2	1	2	3	2	1	1	2	3	(10)	

Fig. 1

Before computers became generally available to crystallographers after the Second World War, Fourier electron density calculations had to be carried out using desk adding machines (though some brave souls found that they could add mentally faster than they could enter numbers into the machines). In any case, evaluating the trigonometrical functions involved was very tedious until Arnold Beevers and Henry Lipson showed how the three-dimensional series for a 3D electron density distribution, or a two-dimensional series for an electron density projection, could be reduced to a sequence of one-dimensional series for evaluation with the aid of Beevers-Lipson strips. Their paper explaining the theory and describing the strips was published in the *Philosophical Magazine*, volume 17 (1934) 855-859. However, the practical sequence of operations in using the strips has hardly ever been described, apart from in a very informative article by Bob Gould in the December 1998 issue of *Crystallography News*. Even this omits one or two practical points that I felt should be mentioned before there were none of us left who actually used these strips. Since Bob's article is probably no longer available to most crystallographers, the whole method of calculation will be described.

The essence of the method is the reduction of the standard formulae for electron density to separate products of cosine and sine terms. Then the strips give the values of these trigonometrical functions at regular intervals along one crystallographic direction, originally at $1/60^{\text{th}}$ intervals but, in later boxes of strips, $1/60^{\text{th}}$ intervals on one side of each strip and at the intervening $1/120^{\text{th}}$ s on the other side (which could be ignored if $1/60^{\text{th}}$ s provided sufficient resolution.). They thus obviated the need to look up a large number of sine and cosine values in tables. To allow a whole group of strips to be turned over at once, I arranged them between two sheets of glass hinged together with cellotape (figure 2).



The strips are in two hopper-shaped boxes, one giving values of $F\cos 2\pi(hX)$ (figure 2) and the other $F'\sin 2\pi(hX)$, where F and F' represent the amplitudes of the cosine and sine waves, respectively, h indicates the frequency of the wave and X the distance along the wave in $1/60^{\text{th}}$ s or $1/120^{\text{th}}$ s of the unit cell length. The sum of the $F\cos$ or $F'\sin$ numbers on each strip is also noted in brackets (see figure 3) for the purpose of checking the arithmetic when the values on several strips are summed. The box is divided into sections, each corresponding to a given value of the frequency h , and the strips within each section have amplitudes -99 to $+99$, supplemented by $-900, -800, \dots, -100$ and $+100, +200, \dots, +900$ (so for amplitudes between 100 and 999 , two strips must be withdrawn from the same h section).

The hopper shaped boxes allow the strips to lean towards or away from the operator, making it easier to select the required strips and to save the places of withdrawn strips for replacement. The frequencies range from 0 to 30 and the distances along each wave range from 0 to 15/60ths of the unit cell dimension, corresponding to a quarter of a revolution ($2\pi/4$) in angular terms. This distance range often needs to be extended to 30/60ths ($2\pi/2$) which is done by recognising that, for cosines of even frequency h , the values from 16/60ths to 30/60ths are a reflection of the values from 14/60ths down to 0/60ths and, for odd h , the cosines are a similar reflection but with a change of sign. For sine values, the odd h values reflect with no change of sign and the even h values change sign.

These complications, and the way they affect the calculation procedure are best illustrated by a particular hypothetical example in two dimensions, corresponding to generating an electron density map in projection along a cell axis. In the plane group $p2$, the electron density expression simplifies to:

$$\rho = \text{constant} \left[\sum_h \sum_k [(F \cos 2\pi(hX) \cos 2\pi(kY)) - F' \sin 2\pi(hX) \sin 2\pi(kY)] \right]$$

where $F = (F_{hk} + F_{.hk})$ and $F' = (F_{hk} - F_{.hk})$. These combinations need only be made for half of the reciprocal lattice, since $F_{.h-k} = F_{hk}$ and $F_{h-k} = F_{.hk}$. In order to account for a multiplicity factor not shown in the above equation when including axial reflections in the summation, F_{h0} and F_{0k} must first be divided by 2.

The first stage, therefore, is to prepare a table of F values for the various values of h and k to use with the cosine strips and a corresponding table of F' values to use with the sine strips. A decision must then be made as to whether to calculate first $\sum_h F \cos 2\pi(hX)$ for the various 1/60ths of X at each value of k or to calculate $\sum_h F \cos 2\pi(hX)$ for the various 1/60ths of Y at each value of h . The choice is made on the basis of which of these will result in the smallest number of strips to sum (the smaller the number of the h or k frequencies to calculate for, or the smaller the number of X or Y points at which to do each summation) in the subsequent second stage calculations. In the present hypothetical example, we decide to calculate $\sum_h F \cos 2\pi(hX)$ for columns of constant k first and for rows of constant X second.

Suppose the $k = 0$ row has F values on an arbitrary scale 124, 69, -31, -6, 0, 3 for $h = 1$ to 6. (Absolute scale and addition of an F_{000} value was often only introduced at the point of calculating at which arbitrary-scale ρ values to draw the contours on the final electron density map.) These F values determine which strips are to be

The summations for each value of X and the check totals in brackets can be carried out mentally but were usually done with the aid of a mechanical or electrical adding machine. I used an electrical dollar accounting machine (one for adding sterling would have had the complication of pounds, shillings and pence) that printed a paper record of every number and operation. It was most convenient, therefore, to obtain a printed subtotal of all the $F\cos$ values for odd h , then continue adding those for even h to obtain the subtotal for both odd and even h values for $X = 0/60$ to $15/60$. Then finally subtract from this the odd h subtotal twice, to obtain the (even – odd) totals for $X = 16/60$ to $30/60$. Repeating this for each value of k allows a table to be constructed of the 31 $\sum_h F\cos 2\pi(hX)$ values for each k value. Summation of the subtotals over all the X values should equal the corresponding subtotals of the check totals in brackets.

A similar table of the 31 $\sum_h F'\sin 2\pi(hX)$ values for each X value and for each value of k is then obtained by summing the numbers on sine strips with F' values as the amplitudes and h values as the frequencies. This time the even h strips are summed first because they are the ones that change sign when extending the 0 to 15 1/60th calculations to 16 to 30 1/60ths of X by calculating the differences for odd – even h .

The next stage in the calculation uses the $\sum_h F\cos 2\pi(hX)$ values as the amplitudes in the selection of cosine strips for one value of X at a time, for the different values of the frequency k . The successive numbers on the strips summed at each X value give $\sum_h \sum_k F\cos 2\pi(hX)\cos 2\pi(kY)$ at the 31 1/60th values of Y . Again, odd and even k strips are subtotalled separately to obtain first the cosine contribution to ρ values from $Y = 0/60$ to $15/60$ and then from $Y = 16/60$ to $30/60$ by sign change. This completes the calculation of the contributions of cosine terms and they are listed in a 31 by 31 table of X versus Y , preferably leaving alternate lines blank to receive later the sine contributions.

These sine contributions to ρ are found in a second sine stage, similar to the second cosine stage. Strips are withdrawn for each of the 31 values of X at a time, using as amplitudes at each X the $\sum_h F'\sin 2\pi(hX)$ found in the first sine stage, the values of k being the frequencies. As in the first sine stage, the even k strips are summed first, since these are the ones that change sign in the range beyond 15/60ths, and the odd k strips are added after. At each of the 31 1/60th values of X , the totals now give the value of $\sum_h \sum_k F'\sin 2\pi(hX)\sin 2\pi(kY)$ for 31 values of Y .

These are then listed in either a second 31 by 31 table of X versus Y or, preferably, adjacent to the cosine terms in the same table as before. All that remains, to obtain the projected electron density map, is then to subtract the figure for each sine contribution from that for the cosine contribution at each XY point and to draw contours at appropriate values of the resulting arbitrary scale ρ values, calculated to show electron densities on an absolute scale. In spite of the simple nature of the operations when using Beevers-Lipson strips, it does not take much imagination to realise that a typical electron density projection would often take several days to compute.



Forthcoming meetings

The next meeting of the group will be on 20th November at the University of Birmingham. The provisional title was 'Physics at War' and was to have included lectures on the 'Frisch-Peierls Memorandum' and the history of the cavity magnetron. Unfortunately two speakers have dropped out and so other arrangements have to be made which we hope will include a lecture on Peierls and one on Chadwick and the Neutron.

The meeting will still include our AGM and we would urge members to attend if at all possible.



As you will know next year is the centenary of the publication of Rutherford's ground breaking model of the atom. The Nuclear Physics Group is holding their conference in August at Manchester University next year and they plan to have three high profile evening sessions of public lectures one of which will be devoted to the historical aspects of Rutherford's work. Also our group hopes to celebrate the centenary in a meeting in March/April 2011 but no details are yet available.

Liquefaction of Gases

Dr. Peter Ford
University of Bath

Towards the end of the eighteenth century two major areas of interest for scientists was whether all materials could exist in the form of solids, liquids and gases and also whether Boyle's law (pressure \times volume is a constant at a fixed temperature) was valid for all gases. It was during the early 1790s that the Dutch scientist Martinus van Marum, working in Haarlem examined the validity of Boyle's law for ammonia gas. He observed a dramatic decrease in the volume at a pressure of around 7 atmospheres. Van Marum had succeeded in liquefying ammonia by applying pressure. Martinus van Marum (1784-1837) deserves to be better known. He made important contributions in the study of the liquefaction of gases as well as in electrostatics and became the first director of the important Teyler Museum in Haarlem. This is a fascinating place with a famous Oval Room, which contains many early scientific instruments.

The English scientist Humphry Davy (1778-1829) worked at the important Royal Institution in London, founded in 1799 by that colourful character and excellent scientist Benjamin Thompson, Count Rumford (1753-1814). Davy was a charismatic lecturer attracting the fashionable London Society to the Royal Institution. He was also an outstanding experimental scientist and is well known for his pioneering work in electrochemistry, in the process of which he discovered the elements sodium, potassium, calcium, barium, strontium and magnesium. Davy was examining the properties of chlorine compounds and sometime in 1823 his laboratory assistant Michael Faraday (1791-1867) heated a chlorine compound in a sealed tube. Faraday was surprised to observe an oily liquid which appeared at the cold end of the tube. Further examination proved that this was liquid chlorine and Faraday realised that this had been liquefied through a combination of a high pressure existing in the sealed tube and a low temperature. By employing this technique of utilising high pressures and low temperatures Faraday was able to liquefy several gases which up to that time people had been unable to achieve. The history of the production of low temperatures is intimately connected with gas liquefaction. Faraday, who was undoubtedly one of the greatest experimental scientists of all time, is better remembered today for his work on the relationship between electricity and magnetism, leading ultimately to the worldwide use of

electrical energy, and his pioneering investigations into the conduction of electricity in solids, liquids and gases. Nevertheless, Faraday also made most important contribution to the liquefaction of gases.

Faraday was unable to liquefy nitrogen and oxygen, the two chief constituents of air, as well as hydrogen and suspected that the reason for this was that he had been unable to achieve a low enough temperature. His hunch was placed on a firm footing through a brilliant investigation by Thomas Andrews (1813-1885) at Queen's University, Belfast made between the years 1860-69. Andrews investigated the relationship between pressure and volume in carbon dioxide in a similar manner to the earlier studies of Boyle and van Marum. However, he also carried out the measurements at a variety of different temperatures and thereby demonstrated that it was only possible to liquefy carbon dioxide by applying pressure provided that the temperature was below a certain "critical temperature", which he found to be 31°C in this instance. This was the first investigation into an important and wide ranging branch of physics dealing with critical phenomena. The curves which Thomas Andrews obtained were explained theoretically by the Dutch scientist Johannes van der Waals (1837-1923). He realised that the results of Boyle were for an ideal gas, in which it was possible to neglect the effects of the size of the molecules and their interaction with each other. In reality both effects have to be taken into consideration and van der Waals derived his own equation, a modified form of Boyle's law, which more closely reflected the results obtained by Andrews, an achievement which gained him the Nobel Prize for Physics in 1910.

The first liquefaction of both nitrogen and oxygen took place at the end of 1877. Two scientists almost simultaneously reported their findings to the Academy of Sciences in Paris. One was Louis Cailletet (1832-1913), a mining engineer from Chatillon sur Seine outside Paris. He had been studying acetylene under very high pressures with a view to liquefying it. His apparatus suddenly sprung a leak and the gas escaped. Cailletet observed a thin mist which he concluded was probably due to liquid acetylene. High pressure equipment springing leaks or blowing up catastrophically were a fairly common event in this highly dangerous research. He concluded that the mist that he had observed had been formed as a result of an adiabatic expansion of the gas and set about trying to develop an apparatus with a controlled expansion. Using the expansion apparatus that he developed, Cailletet successfully produced liquid droplets of both oxygen and nitrogen.

At almost exactly at the same time, Raoul Pictet (1846-1929) a scientist from Geneva, telegraphed the Secretary of the Academy of Sciences in Paris to announce that he also had succeeded in liquefying oxygen. Raoul Pictet came from a prominent family living in Geneva. His technique was very different from that adopted by Cailletet and had a strong engineering approach. Pictet used a series of gases each having a progressively lower critical temperature. Each gas was liquefied by applying pressure and then allowed to evaporate cooling another gas to below its critical temperature so that it too could be liquefied by applying pressure. This "cascade" technique proved to be important in low temperature technology.

Cailletet was generous in providing information about the workings of his "expansion apparatus" and several were sent outside Paris. Among them, one went to the famous Jagiellian University in Cracow, which at the time was part of the Habsburg Empire, brought there by Sygmunt von Wroblewski (1845-88) who had worked in Paris and been present at the experiments of Cailletet. He teamed up with Karol Olszewski (1846-1915) from the chemistry department who had been trying to produce high pressure equipment and had developed considerable practical expertise in this area. Together they succeeded in producing easily visible quantities of liquid oxygen, although this only amounted to a few millilitres. Their work was the subject of the conference lecture by Maciej Kluza of the Jagiellonian University Museum and will not be discussed further in this article.

Another of Cailletet's apparatus reached James Dewar (1842-1923) of the Royal Institution in London. Dewar was said to be short in stature and in temper but was nevertheless a superb experimentalist with considerable manual dexterity. Dewar was very conscience of the legacy of his predecessors Davy and Faraday both of whom were said to haunt the Royal Institution. His research covered a wide range but he is best remembered today for his researches into low temperatures and for his superb demonstrations of these in his various "Friday Evening Discourse". Shortly after obtaining his equipment from Cailletet, Dewar succeeded in producing the first liquid oxygen within the United Kingdom and later he managed to solidify oxygen. He carried out important studies of the electrical behaviour of metals at low temperatures. Dewar appreciated the importance of "vacuum insulation", whereby the heating effects of conduction and convection could be largely eliminated while radiation was reduced by silvering his vessels. He developed the dewar or vacuum vessel for maintaining his liquid gases. An important spin-off from this work is the ubiquitous vacuum flask for providing

people who are travelling with hot water for tea or coffee. All of this work was a preliminary to the big challenge for Dewar, which was to liquefy hydrogen. He succeeded in 1898, using an apparatus which involved the Joule-Thomson effect whereby a gas at low temperatures is allowed to expand through a nozzle thereby cooling it and this low temperature gas in turn cools incoming gas using heat exchangers. There is no doubt that what Dewar had succeeded in doing was an outstanding experimental achievement and at the time he thought that hydrogen was the gas which boiled at the lowest temperature. However, this was not the case since the recently identified element helium had an even lower boiling point.

Major advances in the study of matter resulted from the development of the spectroscope during 1859-60 by Robert Bunsen (1811-1899) and Gustav Kirchhoff (1824-1887) working in Heidelberg. The spectroscope examines the individual wavelengths emitted from an incandescent source. Using it they discovered the elements caesium and rubidium. During the solar eclipse of 1869 the French scientist Pierre Janssen (1824-1907) and Sir Norman Lockyer (1836-1920) independently pointed a spectroscope towards the sun and observed a bright yellow spectral line, which they suspected was probably not due to the well known line of sodium. Further investigations mainly by Lockyer suggested that this spectral line was due to a new element and this was formally announced during the Presidential Address by Lord Kelvin (1824-1907) during the 1871 British Association Meeting. The new element was called helium. It required nearly a further twenty five years before helium was first detected on earth by Sir William Ramsay (1852-1916) of University College, London who was examining the spectral lines emitted by heating the mineral pitchblende. The previous year Ramsay and Lord Rayleigh (1824-1919) had discovered the element argon and Ramsay concluded that helium was another element in a new group of the Periodic Table called the noble elements, with a valence of zero and characterised by being extremely inert and not reactive. Helium occurs in hot springs such as those found in the city of Bath. It was fairly quickly established that helium had a critical temperature of less than ten degrees above the absolute zero and a much lower boiling point than hydrogen. Both Dewar and Ramsay were working in London at the time and if they had collaborated they might well have succeeded in being the first people to liquefy helium. However, they were not on speaking terms due to an earlier bitter argument resulting from Ramsay's allegation that Olszewski had already succeeded in liquefying hydrogen before Dewar, which proved not to be the case. Both tried independently to liquefy helium and both failed.

The person who succeeded was Heike Kamerlingh Onnes (1853-1926), a truly great scientist, who worked at the University of Leiden in Holland, and over many years painstakingly built up superb facilities for low temperature work. Onnes realised that this type of research needed highly trained glassblowers and technicians and began a famous school in Leiden to produce them. Onnes realised that modern science needed excellent facilities and technical support as well as large budgets. He and his research group represent the beginnings of the modern physics research environment and in this respect his laboratory in Leiden was the precursor of the large organisations of today such as CERN. By contrast, Dewar, Ramsay, Rayleigh and others represent the end of the classical period when a scientist worked alone or with very few assistants. During a monumental experiment in July 1908, Kamerlingh Onnes and his colleagues succeeded in liquefying helium, which was found to have a boiling point of 4.2K. A decisive factor in his success was in his use of a very pure form of helium which he was able to obtain from monazite gas. This contrasts with Dewar who used impure helium obtained from the mineral springs of the city of Bath, the contaminants of which blocked the tubes of his apparatus at low temperatures. The same day Onnes pumped the liquefied helium using powerful vacuum pumps, thereby further reducing the temperature of the liquid and becoming the first person to reach a temperature of around one degree above the absolute zero of temperature.

For some fifteen years after the first successful liquefaction of helium, the Leiden group were the only people in the world capable of producing such very low temperatures. During this period they made many important discoveries of which the best known is that of superconductivity. Kamerlingh Onnes was awarded the Nobel Prize for Physics in 1913 for his pioneering work in producing low temperatures.

Further Reading

K. Mendelssohn: *The Quest for Absolute Zero – The Meaning of Low Temperature Physics*; Taylor and Francis Press (1977)

P.J. Ford and G.A. Saunders: *The Rise of the Superconductors*; CRC Press (2004)



N.B This article is based on the lecture given at the first 'Roots of Physics in Europe' symposium in Poellau, Austria and is reproduced here by kind permission of Dr. PM Schuster

Notes

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