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# EVOLUTION OF THE CONCEPT OF THE VECTOR POTENTIAL IN THE DESCRIPTION OF FUNDAMENTAL INTERACTIONS

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Modern theories of fundamental interactions are based on the concept of "connections," a generalization of the vector potential **A**. How did this concept evolve in physics? We attempt in the present paper to trace this evolution, starting from the early 19th century. A schematic diagram which charts this evolution is exhibited (Fig. 1).

*Keywords*: Vector potential; Faraday; Maxwell; $(\mathbf{p} - e\mathbf{A})$; action principle; gauge theory; symmetry dictates interaction.

## Contents

## 1. Introduction

Profound changes took place in the 20th century in physicists' understanding of the concepts of *space, time, motion, energy* and *force*. These are primordial but essential concepts important for the very survival of early man. They must therefore have entered, in one form or another, into the early vocabulary of every primitive human civilization. That these elementary but essential concepts should now acquire such precise and sophisticated mathematical meaning in physics will probably forever remain one of the great mysteries of nature, and of the epistemological history of mankind.

The profound changes over our understanding of *force*, or *interaction* in today's terminology in physics, took place gradually. It had started with the idea of action at a distance. Then in the mid-19th century, through the works of M. Faraday and J. C. Maxwell, one type of force, the electromagnetic force, became described by a *field theory*. It is no exaggeration to say that field theory of electromagnetism has proved enormously successful in the 20th century. But what about the other types of forces discovered in the 20th century, such as nuclear forces and the weak forces? What kind of field theory do they obey? It turns out that these forces are also

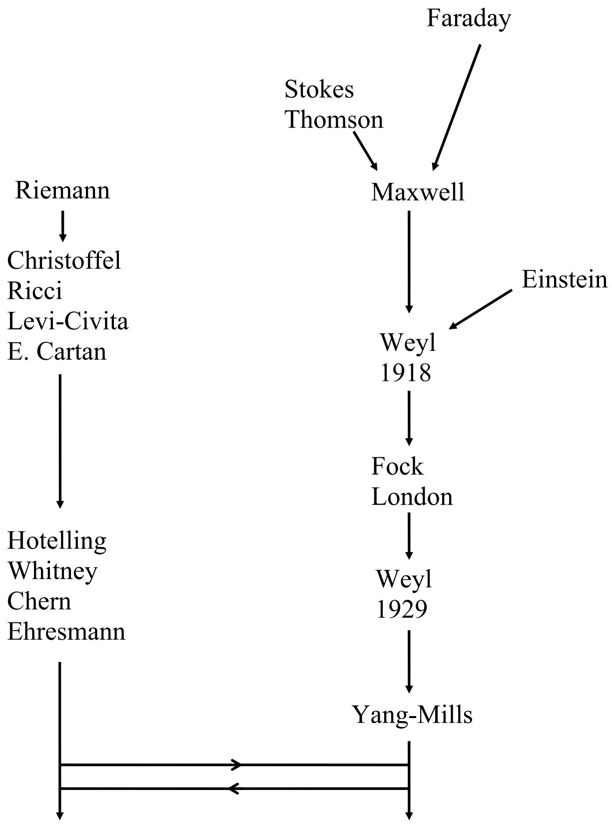


Fig. 1. Flow of ideas in the evolution of the concept of the vector potential.



describable beautifully and precisely by field theories, and that all these theories have mathematical structures required by the concept of symmetry. Hence the principle: *symmetry dictates interaction*. The conceptual history of this remarkable development is the subject of the present paper.

Playing an important part in this history is the vector potential **A**, which first made its appearance in the 19th century. There was certain freedom, now called gauge freedom in its definition, which was early recognized as a simple but somewhat annoying mathematical property. It is this freedom which has now metamorphosed into the key symmetry principle that dictates the exact equations describing the fundamental forces of nature.

Very remarkably, the mathematics of this symmetry principle was in the meantime developed by geometers in the theory of *fiber bundles*, entirely independently of the developments in physics. When this became known, a renewed cross-fertilization of basic ideas between the disciplines of physics and mathematics happily resulted.

Throughout this paper our emphasis is on the early motivation and evolution of the key ideas. There is a vast literature about various aspects of the history we try to cover. We shall thus not be able to include many important developments. On the other hand, we cite many references for the convenience of the readers, even though we do not necessarily agree with the views expressed in some of them.

## 2. Early Work on the Vector Potentials: Neumann, Weber, Kirchhoff and Thomson

### 2.1. *On the European continent: Franz Ernst Neumann (1798–1895); Wilhelm Eduard Weber (1804–90); Gustav Robert Kirchhoff (1824–87)*

The vector potential was first introduced to describe the magnetic field produced by an electric loop or a circuit element. E. T. Whittaker[132] credits F. E. Neumann[91] (in 1845) as the one who, in discussing the induced electrical currents from Ampère's law,[2] extracted from the interaction energy (between two current elements $ds$ and $ds'$ carrying current $i$ and $i'$) the expression for a vector potential

$$\mathbf{a} = \int \frac{i'\, d\mathbf{s}'}{r}\,. \tag{2.1}$$

Whittaker (Ref. 132, p. 230) attributed another expression to W. E. Weber (1846):[124]

$$\mathbf{a}' = \int \frac{i'(\mathbf{r}\cdot d\mathbf{s}')\mathbf{r}}{r^3}\,. \tag{2.2}$$

The difference $\mathbf{a} - \mathbf{a}'$ is a gradient of a scalar on account of an identity attributed to H. von Helmholtz (1870):[43]

$$\{(d\mathbf{s}\cdot d\mathbf{s}') - (\mathbf{r}\cdot d\mathbf{s})(\mathbf{r}\cdot d\mathbf{s}')/r^2\} = (d^2 r/ds\, ds')ds\, ds' \tag{2.3}$$

This is seen by noting that

$$\frac{\partial r}{\partial s} = \cos\theta\,, \quad \frac{\partial r}{\partial s'} = \cos\theta'\,,$$

where $\theta$, $\theta'$ are the angles between $(\mathbf{r}, d\mathbf{s})$, $(\mathbf{r}, d\mathbf{s}')$ respectively. Equation (2.3) is the result by performing the second derivative $\partial^2 r/\partial s \partial s'$. Alternatively, a direct calculation gives

$$\mathbf{a} - \mathbf{a}' = \int i'\, d\mathbf{s}' \cdot \nabla\left(\frac{\mathbf{r}}{r}\right). \tag{2.4}$$

It should be noted that Ampère[2] had worked out expressions very similar to (2.3). Maxwell gave an account of Ampère's theory in his *Treatise*[88] (see Ref. 88, Vol. 2, articles 502–527). The right-hand side would vanish when integrated over a closed loop. By hindsight, this is a historically interesting first example of gauge freedom.

In his work on the propagation of the electrical disturbance in the telegraph wire and later for a general conductor, Kirchhoff (1857)[65,66] had worked with Weber's vector potential (2.2).

### 2.2. *William Thomson* (*Lord Kelvin*) (*1824–1907*)

Among W. Thomson's many contributions, two of his papers[113,114] are of particular interest to us.

(I) In a 1847 paper,[113] "On a Mechanical Representation of Electric, Magnetic and Galvanic Forces," Thomson considered the cases when the expression (in modified notation, $\mathbf{u} = (\alpha, \beta, \gamma)$, $d\mathbf{r} = (dx, dy, dz)$)

$$\mathbf{u} \cdot d\mathbf{r} = df = \text{a complete differential}\,. \tag{2.5}$$

The three examples discussed by Thomson are:

(i) Electric case: $\mathbf{u} = \mathbf{E}$ field of unit point charge $= \mathbf{r}/r^3$; $-f =$ the corresponding electrostatic potential $= 1/r$.
(ii) Magnetic case: $\mathbf{u} = \mathbf{B}$ field of unit dipole with the unit normal $\mathbf{n}$ parallel to the dipole moment;
$$\mathbf{B} = \operatorname{curl}\mathbf{A}\,, \quad \mathbf{A} = \mathbf{n} \times \mathbf{r}/r^3\,, \quad f = \mathbf{n} \cdot \mathbf{r}/r^3\,.$$
(iii) Galvanic current case: $\mathbf{n}$ is parallel to the current, $\mathbf{u}$ in Eq. (2.5) is replaced by the Laplacian $\nabla^2\mathbf{u}$, $\mathbf{u} = \frac{1}{2}\nabla(\mathbf{n}\cdot\mathbf{r}/r) - \mathbf{n}/r$, $f = -\mathbf{n}\cdot\mathbf{r}/r^3$. Here $\operatorname{curl}\mathbf{u} = \mathbf{A}$ of (ii), and $\operatorname{div}\mathbf{u} = 0$.

Thomson cited the influence of Stokes' 1845 work on the equation of equilibrium of an elastic solid, and the statement (2.5) is related to the idea behind the so-called "Stokes' Theorem" (see the Appendix).

(II) In an extensive 1851 study, "A Mathematical Theory of Magnetism,"[114] Thomson, while working with quantities associated with magnetization distributions, had written down the solenoidal condition for a generic vector, say $\mathbf{B}$,

$$\operatorname{div}\mathbf{B} = 0\,, \quad \mathbf{B} = \operatorname{curl}\mathbf{A}\,. \tag{2.6}$$

Thomson clearly recognized that such vector $\mathbf{A}$ is to some extent arbitrary and is determined *modulo a gradient of a scalar*.

It is interesting to note that Thomson in 1850 while working on magnetism had discovered the integral identity which has came to be known as the Stokes' theorem. For a brief historical note, see the Appendix. In his letter to Stokes dated July 2, 1850 in which he communicated the integral theorem to Stokes, Thomson cited the influence of Stokes' papers;[108,109] in particular the dilatation wave (velocity with zero-curl) and the distorted wave (velocity with zero-divergence) are discussed in Ref. 109. (See J. Larmor's footnote to *Stokes Papers* **5**, Appendix, Question 8 of 1854 Smith Prize, quoted in our Appendix.)

## 3. Maxwell's Work on Field Theory

Historically the three monumental papers (1855–1865) of Maxwell (1831–1879), which we shall refer to as 1, 2 and 3, founded the field theory of electromagnetism. For references, the three-volume set,[31–33] *The Scientific Letters and Papers* *of James Clerk Maxwell*, edited by P. M. Harman and the paper by C. N. Yang[141] are especially useful. Before we discuss Maxwell's papers in detail in this section, we shall first summarize what Faraday had done to inspire Maxwell.

### 3.1. *Michael Faraday (1791–1867)*

Faraday had become interested in electricity and magnetism after Örsted had discovered in 1820 that electric currents generate magnetism, and after Ampère had carefully and quantitatively studied the subject. Faraday's great contribution was the discovery of induction in 1831. Unable to describe the phenomena mathematically, he used his geometrical intuition to envisage space around a magnet filled by lines of force. There is a "tension" in the space which he called an "electrotonic state." Variation with time of the tension generates electricity, according to Faraday's ideas.

Among the many readable accounts of Faraday's life and work, besides Faraday's *Experimental Researches in Electricity* (Ref. 19) and *Faraday's Diary* (see Ref. 79), we mention (a) *Faraday as a Discoverer* by his friend J. Tyndall;[122] (b) *The Life & Letters of Faraday*, by H. B. Jones;[62] and (c) *Michael Faraday*: *A Biography*, by L. P. Williams.[134]

In Faraday's *Experimental Researches* (three volumes on electricity and one volume on chemistry and physics),[19] he recorded meticulously his work from 1831 to 1856. His private notes of the *Experimental Researches* contained entries of 16,041 paragraphs (see Ref. 122, p. 149).

### 3.2. *Faraday's work on electromagnetic induction*

On August 28, 1831, using two insulated copper coils wound on an iron ring, Faraday made the historic observation of the *transient* effect of the volta-electric induction. In a letter to his friend Richard Phillips (1778–1851, chemist and editor of *Philosophical Magazine*) dated September 23, 1831 (see Ref. 62), Faraday said:

> "*I am busy just now again on electromagnetism, and think I have got hold of a good thing, but can't say. It may be a weed instead of a fish that, after all my labour, I may at last pull up.*" (Faraday obviously remembered his previous unsuccessful attempts in 1824, 1825 and 1828.)

On October 17, 1831, Faraday made another historic discovery on the magneto-electric induction (see Fig. 2). In his diary entry on that date, Faraday wrote:

> "*A cylindrical bar magnet,* $\frac{3}{4}''$ *in diameter and* $8\frac{1}{2}''$ *in length, had one end just inserted into the end of the helix cylinder, then it was quickly thrust in the whole length, and **the galvanometer** needle moved; then pull out, and again the **needle moved but** in the opposite direction. This effect was repeated every time the magnet was put in or out, and therefore a wave of Electricity was so produced from **mere approximation of a magnet**, and not from its formation **in situ**.*" (*Emphasis*: original.)

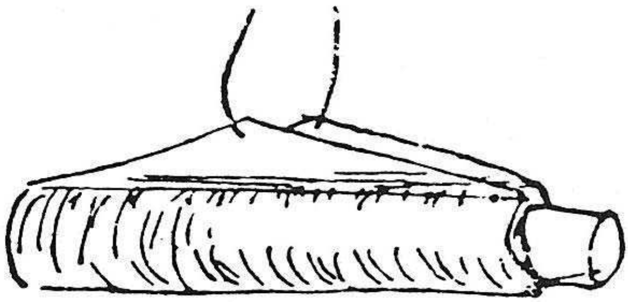

Fig. 2. A diagram from *Faraday's Diary* (October 17, 1831) (see Ref. 79). It shows a solenoid with coil attached to a galvanometer. Moving a bar magnet in and out of the solenoid generates electricity.



### 3.3. *Faraday's statements on the electrotonic state*

In his first paper on induction which appeared in the first series of Ref. 19, Vol. 1 and which was read before the Royal Society on November 24, 1831, Faraday pondered upon a new electrical state or condition of matter:

> "*Whilst the wire is subject to either volta-electric of magnetoelectric induction, it appears to be in a peculiar state; for it resists the formation of an electric current in it, whereas, if in its common condition, such a current would be produced; and when left uninfluenced it has the power of originating a current, a power which the wire does not possess under common circumstances. This electrical condition of matter has not hitherto been recognized, but it probably exerts a very important influence in many if not most of the phenomena produced by currents of electricity. For reasons which will be immediately apparent* (*paragraph* 71), *I have, after advising with several learned friends, ventured to designate it as the* ***electrotonic*** *state.*" (paragraph 60)
>
> "*This peculiar state appears to be a state of tension, and may be considered as* ***equivalent*** *to a current of electricity, at least equal to that produced either when the condition is induced or destroyed.*" (paragraph 71)

In a letter to R. Phillips dated November 29, 1931 (cited in Ref. 62, Vol. 2, p. 9) in which Faraday summarized his latest discovery, he said:

> "*The effects are a current in the same direction when the induction is established; a reverse current when the induction ceases, and a* ***peculiar state*** *in the interim .... The condition of matter I have dignified by the term* ***Electrotonic, The Electrotonic State****. What do you think of that? Am I not a bold man, ignorant as I am, to coin words? But I have consulted the scholars.*"

In the same letter, Faraday offered an interesting observation:

"*It is quite comfortable to me to find that experiment need not quail before mathematics, but is quite competent to rival it in discovery.*"

Faraday was cognizant of the interplay between electricity and magnetism:

"*If we endeavour to consider electricity and magnetism as the results of two faces of a physical agent, or a peculiar condition of matter, exerted in determinable directions perpendicular to each other, then, it appears to me, that we must consider these two states or forces as convertible into each other in a greater or smaller degree; i.e. that an element of an electric current has not a determinable electric force and a determinable magnetic force constantly existing in the same ratio, but that the two forces are, to a certain degree, convertible by a process or change of condition at present unknown to us.*" (paragraph 1114)

Faraday asserted that the idea of electrotonic state should apply to the nonconductor as well:

"*Again and again the relation of conductors and nonconductors has been shown to be one not of opposite in kind, but only degree, and therefore, for this, as well as for other reasons, it is probable, that what will affect a conductor will affect an insulator also; producing perhaps what may deserve the term of the electrotonic state.*" (paragraph 1661)

In his 1852 paper *On Physical Lines of Magnetic Force*,[20,21] Faraday revisited his ideas of the electrotonic state and the physical lines of force:

"*I incline to the opinion that (the lines of magnetic force) have a physical existence correspondent to that of their analogue, the electric lines, and having that notion, am further carried on to consider whether they have a probable dynamic condition, analogous to the axis to which they consist in a state of tension round the electric axis, and may therefore be considered as static in their nature. Again and again the idea of an* ***electrotonic*** *state has been forced on my mind; such a state would coincide and become with that which would then constitute the physical lines of force.*" (paragraph 3269)

"*The motion of an external body, . . . , could not beget a physical relation such as that which the moving wire presents. There must, I think, be a previous state, a state of tension or a static state, as regards the wire, which, when the motion is superadded, produces the dynamic state or current of electricity. This state is sufficient to constitute and give a physical existence to the lines of magnetic force.*" (paragraph 3270)

It is interesting to point out that many years later, in 1881, in his Faraday Lecture before the Fellows of the Chemical Society of London, Helmholtz summarized Faraday's idea of the electrotonic state (see Ref. 41, pp. 401–436; Ref. 50, pp. 52–87).

> "(*Faraday*) *found that an electromotive force striving to produce these currents arises wherever and whenever magnetic force is generated or destroyed. He concluded that a part of space traversed by magnetic force there ought to exist a peculiar state of tension and that every change of this tension produces electromotive force.... This unknown hypothetical state he called provisionally the electrotonic state, and he was occupied for years and years in finding out what this electrotonic state was.*"

### 3.4. *Faraday's ideas on lines of force*

Faraday spoke of the metal cutting the magnetic curves (see first paper cited above, paragraph 114), and in a footnote, he visualized:

> "*By magnetic curves I mean the lines of magnetic force,... which would be depicted by iron filings, or those to which a very small magnetic needle would form a tangent.*"

Jones (Ref. 62, Vol. 2, p. 5) had this to say:

> "*This is the germ of those* ***lines of force****, which arose up in the mind of Faraday into* ***physical*** *and almost tangible matter. The influence which they had upon his thoughts and experiments will be seen from this time up to the date of the last researches which he sent to the Royal Society.*"

Twenty years after his 1831 discovery of electromagnetic induction, Faraday summarized his ideas in an important paper, "*On Lines of Magnetic Force.*"[20,21] In Faraday's own words, we read:

> "*It is also evident, by the results of the rotation of the wire and magnet* (paragraphs 3097 and 3106)*, that when a wire is moving amongst equal lines* (*or in a field of equal magnetic force*)*, and with an uniform motion, then the current of electricity produced is proportional to the* ***time****; and also to the* ***velocity*** *of motion.*" (paragraph 3114)
>
> "*They also prove, generally, that the quantity of electricity thrown into a current is directly as the amount of curves intersected.*" (paragraph 3115)

Faraday had the idea of superposition (paragraphs 3076 and 3082); he much preferred field lines to action at distance (paragraph 3074). The direction of the induced current relative to the field was discussed (paragraphs 114, 3079 and 3089).

Faraday clearly stated that the magnetic field lines are closed:

"*Every line of force, therefore, at whatever distance it may be taken from the magnet, must be considered as a closed circuit, passing some part of its course through the magnet, and having an equal amount of force in the every part of its course.*" (paragraph 3117)

He went on to expand on the idea of flux:

"*On the other hand, the use of the idea of* ***lines of force****, which I recommend, to represent the true and real magnetic forces, makes it very desirable that we should find a unit of such force, if it can be attainable.... In the mean time, for the enlargement of the utility of the idea in relation to the magnetic force, and to indicate its conditions graphically, lines may be employed as representing these units in any given case. I have so employed them in former series of these Researches* (paragraphs 2807, 2821, 2831, 2874, etc.)*, where the direction of the* ***lines of force*** *is shown at once, and the relative amount of force, or of lines of force in a given space, indicated by their concentration or separation, i.e. by their number in that space.*" (paragraph 3122)



### 3.5. *Tribute to Faraday*

In the 1881 Faraday Lecture cited above at the end of Sec. 3.3, Helmholtz gave the following tribute:

"*Now that the mathematical interpretation of Faraday's conceptions, regarding the nature of electric and magnetic forces has been given by J. C. Maxwell, we see how great a degree of exactness and precision was really hidden behind the words which to Faraday's contemporaries appear either vague or obscure*; *and it is in the highest degree astonishing to see what a large number of general theorems, the methodical deduction of which requires the highest powers of mathematical analysis, he found by a kind of intuition, with the security of instinct, without the help of a single mathematical formula.*"

"*A single remarkable discovery may, of course, be the result of a happy accident and may not indicate the possession of any special gift on the part of the discoverer, but it is against all rules of probability that the train of thought which has led to such a series of surprising and unexpected discoveries as were those of Faraday should be without a firm, although perhaps hidden, foundation of truth.*"

Faraday was a kind and humble man, unconcerned with honors. He declined the offers of the presidency of the Royal Society in 1857, and the presidency of the Royal Institution in 1864, and a knighthood by Queen Victoria. Yet he treasured the love and sympathy of fellow man and prized it more than the renown which his science had brought him. After Faraday had read Tyndall's review of Faraday's *Experimental Researches* in the *Philosophical Magazine*, Tyndall recounted Faraday's remark to him:

> "*The sweetest reward of my work is the sympathy and good will which it has come to flow in upon me for all quarters of the world*" (Ref. 122, p. 150)

In a letter to Tyndall dated June 28, 1854, Faraday wrote (see Ref. 122, p. 152):

> "*... You are young, I am old.... But our subjects are so glorious that to work at this rejoices and encourages the feeblest, delights and enchants the strongest.*"

Maxwell's tribute to Faraday is cited toward the end of Sec. 9 in this paper.

### 3.6. *Maxwell's preparation*

Maxwell in his student days already was familiar with the concept of Potential Functions. In a "Manuscript on the Potential Function (Mathematical Theory of Polar Forces)" (circa 1851) (Ref. 31, pp. 210 and 211), Maxwell gave a then conventional view (apparently as a sophomore) of the potential:

> "*The most remarkable example of the use of a mathematical abstraction of this kind in mechanical science is the function used by Laplace* (*note added*: see Ref. 67, 1825) *in the theory of attractions and to which Green in his* (*note added*: see Ref. 23, 1828) *essay on Electricity has given the name of the Potential Function. We have no reason to believe that anything answering to this function has a physical existence in the various parts of space, but it contributes not a little to the clearness of our conceptions to direct our attention to the potential function as if it were a real property of the space in which it exists.*"

It is obvious from this paragraph that to introduce the potential function, which was considered *nonreal* at that time, required imagination and perhaps courage.

Upon his graduation from the Trinity College in 1854, Maxwell wrote to Thomson in February 20, 1854 (Ref. 31, No. 45, p. 237), asking for his advice toward readings in Electricity and specifically regarding how to follow in order the works of Ampère, Faraday and Thomson. Thomson apparently wrote a long letter to Maxwell on Electricity (not preserved), as acknowledged by Maxwell's letters to Thomson dated March 14 1854 and November 13, 1854. It is amazing how rapidly Maxwell has mastered the subject in eight months. In Maxwell's own words (Ref. 31, No. 51, p. 254):

> *Dear Thomson*:
>
> *I soon involved myself in that subject, thinking of every branch of it simultaneously, and have been rewarded of late by finding the whole mass*

> *of confusion beginning to clear up under the influence of a few simple ideas. . . . I got up the fundamental principles of electricity of tension* (*note added by Harman*: *i.e. potential*) *easily enough. I was greatly aided by the analogy of the conduction of heat, which I believe is your invention at least I never found it elsewhere . . . . Now I have heard you speak of* "*magnetic lines of forces*" (*note added by Harman*: Ref. 116) *and Faraday seems to make great use of them* (*note added by Harman*: Refs. 20 and 21), *but others seem to prefer the notion of attractions of elements of currents directly . . . .*

Maxwell was aware of Weber work[124] through Thomson (see Ref. 31, No. 66, May 15, 1855).

It is interesting to note that, many years later, in his *Preface* to the first edition of *A Treatise on Electricity and Magnetism*,[88] Maxwell stated that

> "*. . . before I began the study of electricity I resolved to read no mathematics on the subject till I had first read through Faraday's Experimental Researches on Electricity.*"

Clearly the ideas of Faraday's caught Maxwell's attention. One may surmise that in the period before his first great paper of 1855, Maxwell was trying hard to absorb both the physical intuitions of Faraday and the mathematical language of potential functions.

### 3.7. *Maxwell's Paper 1*[a]

Borrowing from Thomson's earlier papers on magnetism (see Subsec. 2.2 above) where the vector potential **A** was introduced (called three functions $F$, $G$ and $H$), in paper 1, Maxwell *identified this vector potential with Faraday's intuitive idea of "electrotonic intensity."* In today's notation, Maxwell found

$$\mathbf{E} = -\dot{\mathbf{A}}\,, \tag{3.1}$$

which is Faraday's law. This is the most important result of Maxwell's paper 1.

With (3.1), Maxwell has expressed Faraday's law in the form of a partial differential equation, an achievement of the greatest importance. In his letter dated September 13, 1855 (Ref. 31, No. 71, p. 322), months before paper 1 was read at a meeting, Maxwell wrote:

> *Dear Thomson*
>
> "*. . . I intend next to apply to these facts Faraday's notion of an electrotonic state.* (*Harman's note*: *M. Faraday, Electricity,* **1**, 16 (Sec. 60). *I have worked a good deal of mathematical material out of this vein and I believe I have got hold of several truths which will find a mathematical expression in the electrotonic state. . . .*"

[a]Paper 1 refers to the paper published in 1856, Ref. 80.

That Maxwell regarded his interpretation of Faraday's intuitive idea of an electrotonic intensity (which Maxwell sometimes called electrotonic function) as an important discovery is evident from the following passage at the end of paper 1, which indicated his misgivings that Thomson might take offense:

> "*With respect to the history of the present theory, I may state that the recognition of certain mathematical functions as expressing the "electrotonic state" of Faraday, and the use of them in determining electrodynamic potentials and electromotive forces is, as far as I am aware, original*; *but the distinct conception of the possibility of the mathematical expressions arose in my mind from the perusal of Prof. W. Thomson's papers....*"

We shall make the following remarks about paper 1:

(a) Paper 1 (which consists of two parts) is a long paper. Reprinted in Niven's volume,[95] it ran 75 pages from pp. 155–229. The important part of the volume is in the middle, from pp. 188–205, after the subtitle "Part II — On Faraday's 'Electrotonic State'."

(b) Maxwell adopted a left-handed $x$, $y$, $z$ system (p. 194), and a left-handed definition of the operator curl (e.g. bottom of p. 194). He used Stokes' theorem repeatedly. (Please see the Appendix in this paper.)

(c) On p. 192, Maxwell explained that subscript 1 referred to magnetic quantities and subscript 2 to electric questions. Thus

$$\begin{aligned} a_1,\ b_1,\ c_1 &= \mathbf{B} && \text{magnetic induction}\,,\\ \alpha_1,\ \beta_1,\ \gamma_1 &= \mathbf{H} && \text{magnetic force}\,,\\ a_2,\ b_2,\ c_2 &= \mathbf{j} && \text{current}\,,\\ \alpha_2,\ \beta_2,\ \gamma_2 &= \mathbf{E} && \text{electromotive force}\,,\\ \alpha_0,\ \beta_0,\ \gamma_0 &= \mathbf{A} && \text{electrotonic intensity (or function)}\,. \end{aligned}$$

(d) On p. 195, Maxwell stated

> "*Our investigations are therefore for the present limited to closed currents*; *and we know little of the magnetic effects of any currents which are not closed.*"

In this connection, he wrote down explicitly the equation for closed currents, which is in today's notation:

$$\boldsymbol{\nabla}\cdot\mathbf{j} = 0\,. \tag{3.2}$$

(e) In today's understanding, Faraday's law is expressed usually not as (3.1); but as

$$\oint \mathbf{E}\cdot d\boldsymbol{\ell} = -\iint \dot{\mathbf{H}}\cdot d\boldsymbol{\sigma}\,. \tag{3.3}$$

While the two are identical, (3.3) is certainly simpler. How did Maxwell arrive at the less simple form in this first attempt? To try to answer this question, it is important to realize that Faraday, in his intuitive discussions of the electrotonic state, was very far from such quantitative results as (3.1) or (3.3). It was Maxwell's great contribution to have arrived at (3.1). He did this in the four pages, 201–204 of paper 1.

First he integrated over the whole 3-space the equation

$$\nabla \cdot (\mathbf{A} \times \mathbf{H}) = \mathbf{H} \cdot (\nabla \times \mathbf{A}) - \mathbf{A} \cdot (\nabla \times \mathbf{H}) \tag{3.4}$$

obtaining

$$0 = \iiint \mathbf{H}^2 \, d\tau - \iiint \mathbf{A} \cdot \mathbf{j} \, d\tau \,. \tag{3.5}$$

He then wrote:

> "*Let us now consider the conditions of the conduction of the electric currents within the medium* ***during changes in the electrotonic state****. The method which we shall adopt is an application of that given by Helmholtz in his memoir on the Conservation of Force.* (*Note added*: Refs. 40 and 41)." (***Bold***: added.)

Finally using (3.5) he showed that Helmholtz's Conservation of Force (i.e. energy principle) led to

$$\iiint \mathbf{E} \cdot \mathbf{j} \, d\tau + \frac{d}{dt} \iiint \mathbf{A} \cdot \mathbf{j} \, d\tau = 0 \,. \tag{3.6}$$

He then derived (3.1) from (3.6).

(f) Maxwell had understood very well the gauge freedom in the definition of $\mathbf{A}$, as is evident from his Theorem V (for which he gave credit to Thomson[114,115] for the modulo gradient term). So which of the gauge choices did he take in (3.1)? He did not enter into a discussion of this point in paper 1. We can assume, perhaps, that he realized that when integrated over a closed circuit the additional term in gauge freedom, being a gradient, would contribute nothing. So he did not comment on this freedom.

Maxwell remarked on p. 203 that it is better to use the vector potential which he called functions $\alpha_0$, $\beta_0$, $\gamma_0$:

> "*We have now obtained in the functions $\alpha_0$, $\beta_0$, $\gamma_0$ the means of avoiding the consideration of the quantity of magnetic induction which* ***passes through*** *the circuit. Instead of this artificial method we have the natural one of considering the current with reference to quantities existing in the same space with the current itself. To these I give the name of* ***Electrotonic functions, or components of the Electrotonic intensity***." (*Emphasis*: original.)

What did he mean by quantities "existing in the same space with the current itself"? Did he mean the vector potential can be chosen to be in the space where there is a current? Is this the reason that he did not comment on any gauge freedom?

### 3.8. *Maxwell's Paper 2*[b]

In paper 2 Maxwell introduced the displacement current, thereby reaching the historical conclusion that light is an electromagnetic wave. But what had led him to the displacement current is not clear. The paper had begun with the following declaration:

> "*My object in this paper is to clear the way for speculation in this direction, by investigating the mechanical results of certain states of tension and motion in a medium, and comparing these with the observed phenomena of magnetism and electricity.*"

There followed Part II — "Theory of Molecular Vortices applied to Electric Currents" with beautifully intricate lattices of vortex motion. To illustrate Maxwell's style of writing in his three monumental papers, we quote from paper 2:

> "*Let $AB$, Plate VIII,* p. 488, Fig. 2, *represent a current of electricity in the direction from $A$ to $B$. Let the large, spaces above and below $AB$ represent the vortices, and let the small circles separating the vortices represent the layers of particles placed between them, which in our hypothesis represent electricity.*"

which refers to the diagram reproduced here on the right side of our Fig. 3. How the diagram is related to electromagnetism was not explained by Maxwell.

Then, suddenly without warning, in Part III of the paper, Maxwell wrote:

> "*Prop XIV — To correct Eq.* (9) (*of Part I*) *of electric currents for the effect due to the elasticity of the medium.*"

The correction was to add the displacement current. With this historic addition Maxwell then proved charge conservation and wrote down as Eq. (113) what would be, in today's notation,

$$\boldsymbol{\nabla} \cdot \mathbf{j} + \dot{\boldsymbol{\rho}} = 0\,. \tag{3.7}$$

He then derived the existence of electromagnetic waves.

Did Maxwell arrive at the concept of the displacement current through his complicated lattices of vortex motion? Through the elasticity of the medium? We cannot be sure. But it is worth noticing that Part III was published ten months after Part I, and eight or nine months after Part II, during which time he must have been working hard on the subject, arriving finally at the "correction." What had led to the "correction" we do not know, but he had written already at the beginning of Part I, months before he landed on the "correction,"

[b]Paper 2 refers to the papers published in 1861 and 1862, Refs. 81–85.

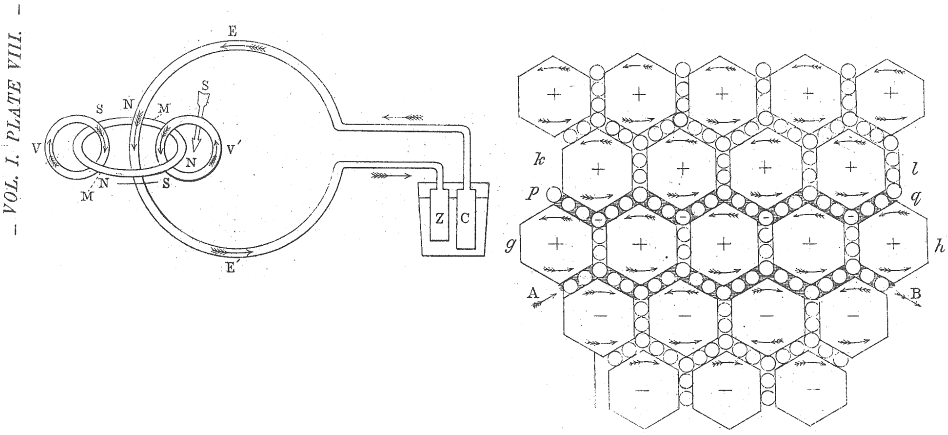


Fig. 3. A diagram in Maxwell's paper 2 as a model of the electromagnetic fields. This figure was also quoted in Ref. 6 with the following caption: Helmholtz (Ref. 44) describes it as "a systems of cells with elastic walls and spherical cavities . . . in which elastic balls can rotate and be flattened out by the centrifugal forces. In the wall of cells there must be other balls, of invariable volume, as friction rollers . . . their center of gravity . . . would merely be displaced by elastic yield of the center-wall . . . displacement of (the friction rollers) gives dielectric polarization of the medium; streaming of the same, as electric currents; rotation of the elastic balls corresponds to the magnetizing of the medium, the axis of rotation being the direction of the magnetic force."

> "*. . . I have found the geometrical significance of the 'Electrotonic State', and have shown how to deduce the mathematical relations between the electrotonic state, magnetism, electric currents, and the electromotive force, using the mechanical illustrations to assist the imagination, but not to account for the phenomena.*"

Thus he was constructing mechanical illustrations to "assist the imagination." Could it be that his lattices of vortex motion did assist him to incorporate the concepts of charge conservation and of the displacement current? If so, how?

But it is also possible that in the period after Part II of paper 2, Maxwell had decided to consider more than just "closed currents," and therefore had to use (3.7) rather than (3.2) (see (d) in Subsec. 3.7 above). It is easy to believe that once he embarked on this more general problem he would discover the displacement current.

We mention here in passing that for the mathematical backgrounds regarding the use of curl and divergence of vectors (in components), Maxwell cited the influence of Thomson[113–115] and Stokes.[108]

### 3.9. *Maxwell's Paper 3*[c]

Three years after the appearance of the last parts of paper 2 (Parts III and IV), Maxwell published paper 3 (which consists of six parts) with the title "A Dynamical

[c]Paper 3 refers to the paper published in 1865, Ref. 86.

Theory the Electromagnetic Field," in which he collected and tabulated the equations developed in the two earlier papers. It is especially noteworthy that he emphasized

> "*I have on a former occasion*[d] *attempted to describe a particular kind of motion and a particular kind of strain, so arranged as to account for the phenomena. In the present paper I avoid any hypothesis of this kind*; *and in using such words as electric momentum and electric elasticity in reference to the known phenomena, of the induction of currents and the polarization of dielectrics, I wish merely to direct the mind of the reader to mechanical phenomena which will assist him in understanding the electrical ones. All such phrases in the present paper are to be considered as illustrative, not as explanatory.*"

It is clear that Maxwell realized now that the mechanical models were irrelevant. It was the *field* that was the real essence. Also noteworthy was the following words in Section (74):

> "*In speaking of the Energy of the field, however, I wish to be understood literally. All energy is the same as mechanical energy, whether it exists in the form of motion or in that of elasticity, or in any other form. The energy in electromagnetic phenomena is mechanical energy. The only question is, Where does it reside? On the old theories it resides in the electrified bodies, conducting circuits, and magnets, in the form of an unknown quality called potential energy, or the power of producing certain effects at a distance. On our theory it resides in the electromagnetic field, in the space surrounding the electrified and magnetic bodies, as well as in those bodies themselves, and is in two different forms, which may be described without hypothesis as magnetic polarization and electric polarization, or, according to a very probable hypothesis as the motion and the strain of one and the same medium.*"

Here the fundamental spirit of the concept of the field, so dominant in the physics of the 20th century, was for the first time clearly and emphatically enunciated.

### 3.10. *Maxwell's statements about* A *being a momentum*

There were many places where Maxwell stated that **A** is a momentum. Some examples:

(a) In Part II of paper 2, we read (Ref. 95, Vol. 1, pp. 478 and 479)

> "*The electrotonic state, whose components are* $F$, $G$, $H, \ldots$ *correspond to the* ***impulse*** *which would act on the axle of a wheel in a machine*

[d]Here Maxwell referred to his paper 2.

*if the actual velocity were suddenly given to the driving wheel, the machine being previously at rest....*
*This impulse may be calculated for any part of a system of mechanism, and may be called the* ***reduced momentum*** *of the machine for that point.*" (*Emphasis*: original.)

(b) In Part III of paper 3 (Ref. 95, Vol. 1, p. 555) Maxwell stated that

Electromagnetic Momentum $\mathbf{A}$ = total impulse of the $\mathbf{E}$-field:

$$-\mathbf{A} = \int \mathbf{E}\, dt$$

or

$$\mathbf{E} = -\frac{d\mathbf{A}}{dt}\,.$$

(c) In Part III of paper 3, we read, "this total electromagnetic momentum is the same thing to which Professor Faraday has applied the name of the electrotonic state" (Ref. 95, Vol. 1, p. 556).

(d) In a more extended account given in his 1873 *Treatise* (Ref. 88, Vol. 2, articles 585–603), Maxwell stated quite clearly (article 590),

"*The vector* $\mathbf{A}$ *represents in direction and magnitude the time-integral of the electromotive force which a particle placed at the point* $(x, y, z)$ *would experience if the primary current were suddenly stopped. We shall therefore call it the Electrokinetic Momentum at the point* $(x, y, z)$*. It is identical with the quantity which we investigated in article* 405 *under the name of the vector-potential of magnetic induction.*"



## 4. The Action Principle

### 4.1. *Explicit versus implicit references to the expression* $(\mathbf{p} - e\mathbf{A})$

Today, because of its relationship to covariant differentiation, the expression

$$\mathbf{p} - e\mathbf{A} \to -i\boldsymbol{\partial} - e\mathbf{A}$$

has become a primary concept in physicists' description of the forces of nature. We want to trace in this section the early appearance in the literature of this fundamental expression.

### 4.2. *Explicit expression of* $(\mathbf{p} - e\mathbf{A})$

One often quoted reference where the expression

$$\mathbf{p} = m\mathbf{v} + e\mathbf{A}$$

makes its appearance is the Max Born's 1923 Göttinger Lectures[7] discussing the Zeeman effect, as cited, e.g. in a Dirac's 1926 article.[15] J. H. Van Vleck (1926)[123] gave some interesting references to works on the modifications in the Bohr–Sommerfeld quantization to include relativity correction and magnetic fields. For example, the equivalent forms for the $(\mathbf{p} - e\mathbf{A})$ term appeared in the Hamilton–Jacobi formalism for the Zeeman effect treated by A. Sommerfeld[106] and by P. Debye[12] in 1916.

Among other references cited by Van Vleck, the earliest work where the expression $(\mathbf{p} - e\mathbf{A})$ can be explicitly found was George A. Schott's 1912 book, *Electromagnetic Radiation* (*and the Mechanical Reactions arising from it*).[101] This book is the revised version of his 1908 Essay which was awarded the 1909 Adams Prize at the University of Cambridge. Additional materials beyond the 1908 Essay are contained in seven appendices. The expression $(\mathbf{p} - e\mathbf{A})$ is found in App. F, "*The Mechanics of the Lorentz Electron*." In the context of the Lorentz's relativistic mass correction (1895),[76] Schott used the Lagrangian written down in 1903 by K. Schwarzschild.[102–104] Schott wrote down the explicit Hamilton–Jacobi equations and the Hamiltonian, thereby exhibiting the explicit expression of $(\mathbf{p} - e\mathbf{A})$. So far, much to our surprise, we have not been able to find any explicit expression of $(\mathbf{p} - e\mathbf{A})$ prior to this work of 1912.

### 4.3. *Implicit expression of* $(\mathbf{p} - e\mathbf{A})$ *and the action principle*

On the other hand, one could ask instead the question: where was it *first* written down, the Lagrangian containing the interaction terms equivalent to $\mathbf{j} \cdot \mathbf{A}\, d^3x$, or $e\mathbf{v} \cdot \mathbf{A}$, which imply the expression $(\mathbf{p} - e\mathbf{A})$ in the Hamiltonian? To answer this question we have to examine the role of Thomson, Helmholtz and Larmor, etc. about their work on the Lagrangian and the action principle. We shall work our way backward in time.

#### 4.3.1. *Joseph Larmor* (*1857–1942*)

W. Pauli[98] in his Handbuch article credited $(\mathbf{p} - e\mathbf{A})$ to Larmor's (1900b) book, *Aether and Matter*,[71] where for the Lagrangian for an electron moving with velocity $\mathbf{v}$, Larmor had included the magic term $\mathbf{v} \cdot \mathbf{A}$. Actually the relevant expressions in Larmor's 1900 book have already appeared in his earlier articles during 1895 to 1900[70,71] where the Lagrangian for a charged particle can be found. (See Eq. (4.1) below.)

Larmor wrote an article, "*On Least Action as the Fundamental Formulation in Dynamics and Physics*" in 1894. As a commentary on this article, Larmor gave an interesting "*Historical Note on Hamiltonian Action*" in a 1927 appendix to his collected papers (see Ref. 68, pp. 640–643):

> "*The original ideas of William Rowan Hamilton, nurtured, like all the Dublin school of his time, on the writers of the great age in France, have constituted an epoch in fundamental mathematical physics. They began with consideration of the properties of what he at first named on historical*

*grounds the "Action function" for a System of Optical Rays, in a memoir communication in June* 1824 *to the Royal Irish Academy when he was an undergraduate of nineteen years of age."* (*Hamilton's work appeared in Trans. R.I.A.* ***15*** (1828), *followed by three supplements in* 1830, 1831 *and* 1832 *respectively. A "magnificent synthesis," "On a General Method in Dynamics," appeared in two memoirs, Phil. Trans.* 1834, 1835).) *"After this date* (1835) *Hamilton seems to have parted company with fundamental dynamics and optics, devoting the remaining twenty-five years of his life largely to the domain of symbolic algebras, and particularly to the development, and the detailed application over wide ranges of subjects, of his system of quaternions, being the precursor in this extensive and imaginative field, on the future practical importance of which he laid great stress.*"

Larmor then went on to trace the historical development of the action principle:

"*The fundamental character of his* (*Hamilton's*) *culminating doctrine of Varying Action, as implicating the whole range of physical science, seems to have been first emphasized for a wider audience in Thomson and Tait's classic book on Natural Philosophy* (1867). *It appears from the surviving letters that Tait had a large share of the merit of this recognition, by insistence on his more physical colleague's study of the work of his own Irish friend, of which the development, apart from applications in dynamical astronomy natural to the time, had hitherto, following the lead of Jacobi, been in the direction of abstract analysis of the implications of systems of partial differential equations.... From Thomson and Tait the illumination passed on to Helmholtz, culminating in his* [1886] *extensive memoir*[45] *Über die physikalische Bedeutung des Princips der kleinsten Wirkung....*"

Here Larmor regarded his own 1894 paper on Least Action as of "preliminary and analogical scope as contrasted with Helmholtz's bolder formulations developed soon afterwards."

Larmor in fact gave an interesting prehistory of the action principle:

"*As a matter of scientific history, a germ of the earlier Hamiltonian developments for rays may be found, already in Newton's days, in the principle of Cotes, expanded by R. Smith in his Systems of Opticks and recalled to modern attention by Rayleigh. It asserts that the apparent distance of an object at A, seen across any optical instrument by an observer at any other place B, is equal to the apparent distance of the same object located at B as seen from A. For the special case when A and B are conjugate foci, the relations of object and image, as earlier generalized for all systems by Huygens, emerge from this principle....*"

In a footnote added in 1927 to his 1895 paper in his collected papers (Ref. 69, p. 414), Larmor acknowledged the influence of Thomson's work on dynamics:

"*The main idea of these three memoirs was to express and extend the electrical theory by reference, under direct inspiration mainly from Lord Kelvin's writing, to a guiding working model framed as far as possible directly on dynamical principles....*"

Larmor's 1895 (see Ref. 70, p. 568) version of the kinetic energy of a moving electron (in slightly modified notation) reads:

$$T = \frac{1}{2} m\mathbf{v}^2 + e\mathbf{v} \cdot \mathbf{A} + \cdots . \tag{4.1}$$

4.3.2. *William Thomson (1824–1907)*

Thomson and Tait's *Treatise on Natural Philosophy*[117] gave "an account of the splendid dynamical theory founded by d'Alembert and Lagrange." In article 310, one finds the equations of impulsive motion:

"*The effect of any stated impulses, applied to a rigid body, or to a system of material points or rigid bodies connected in any way, is to be found most readily by the aid of d'Alembert's principle* (*note added*: *in* 1742) *according to which the given impulses, and the impulsive reaction against the generation of motion, measured in amount by the momenta generated, are in equilibrium*; *and are therefore to be dealt with mathematically by applying to them the equations of equilibrium of the system.*"

Thomson and Tait of course did not have the expression $(\mathbf{p} - e\mathbf{A})$. But their work was important historically because Maxwell (i) attributed his dynamics theory to Thomson–Tait's treatment of the impulsive forces (Ref. 88–90, Vol. 2, article 554); and (ii) appreciated from his study of Faraday's law of induction that the electrotonic state (namely, the vector potential) is an impulse. (See Subsec. 3.10 above.)

There are several dynamical examples discussed in Thomson–Tait's *Natural Philosophy* which contain linear terms in the velocities (besides the quadratic terms). This would give rise to a difference between the *canonical* momentum $\mathbf{p}$ and the *kinematical* momentum $m\mathbf{v}$, and such difference could be interpreted as that due to the existence of a vector potential.

4.3.3. *Hermann von Helmholtz (1821–1894)*

During 1886–1892, Helmholtz wrote three papers on the principle of least action. The 1892 paper has the Lagrangian with the $\mathbf{v} \cdot \mathbf{A}$ term, and the difference between the canonical momentum and the kinematical momentum is clearly recognized. Thus Helmholtz's work preceded Larmor's by some two to eight years. Indeed, in his 1894 paper, Larmor acknowledged (see Ref. 73, p. 447):

"*It may be mentioned that a scheme for expressing the equations of electrodynamics by a minimum theorem analogues to the principle of Least Action has recently been constructed by von Helmoltz* (1892).[47,48]"

A. Einstein (1879–1955) was deeply interested in electrodynamics since the age of 16.[100] As revealed by his letters to M. Maric,[100] Einstein was expressing both his admiration for and frustration with Helmholtz's work:

(a) Early August 1899 letter:

> "*I admire the originality and independence of Helmholtz's thought more and more.*"

(b) 10(?) August 1899 letter:

> "*I returned the Helmholtz*[50] *volume and am now rereading Hertz's*[54,55] *propagation of electric forces with great care because I didn't understand Helmholtz's*[47] *treatise on the principle of least action in electrodynamics. I'm convinced more and more that the electrodynamics of moving bodies as it is presented today doesn't correspond to reality, and that it will be possible to present it in a simple way.*"

On the other hand, perhaps not surprisingly, Oliver Heaviside (Ref. 38, Vol. 2, p. 509) had a very disparaging opinion on the least action principle:

> "*Finally, there is the Principle of Least Action. Now, Least Action has no more to do with the matter than the man in the moon, as far as I can see. It is quite unnecessary, to begin with. Next, it obscures and complicates the matter, so much so as sometimes to lead to serious error. I made this remark advisedly, remembering previous applications of the Principle of Least Action to electromagnetics, which is much clearer without it.*"

#### 4.3.4. *William Rowan Hamilton* (*1805–1865*)

Hamilton's work was briefly alluded to in Subsec. 4.3.1 above. It is interesting to trace how Hamilton's idea on the least action principle evolved from geometrical optics to dynamics. Hamilton[24] dealt with the properties of systems of rays under reflection. Hamilton[25] treated refraction. Here he derived the Euler–Lagrange equations for the rays from the principle of least action (generalizing Fermat's principle of least time). We note that Fermat (1601–1665) stated his least-time principle first for mirror reflection. Huygens (1629–1695) verified Fermat principle for refraction. Pierre-Louis Moreau de Maupertuis (1698–1759) stated the least action principle in 1744. Euler (1707–1793) had his least action (in the form of integral of $v\,ds$) principle using calculus of variation in 1744. Subsequently Lagrange (1736–1813) applied Euler's principle of least action to problem in dynamics.

Hamilton[26] contained the interesting observation:

> "*Meanwhile, it appears that if a general method in deductive optics can be attained at all, it must flow from some law or principle, itself of the highest generality, and among the highest results of induction.... The answer, I think, must be, the principle or law, called usually the Law of*

*Least Action*;... *that this linear path of light, from one point to another, is always found to be such, that if it be compared with the other infinitely various lines by which in thought and in geometry the same two points might be connected, a certain integral or sum, called often Action, and depending by fixed rules on the length, and shape, and position of the path, and on the media which are traversed by it, is less than all the similar integrals for the other neighboring lines, or, at least, possesses, with respect to them, a certain stationary property.... From this Law, then, which may perhaps, be named the Law of Stationary action, it seems that we met most fitly and with best hope set out, in the synthetic or deductive process, and in search of a mathematical method.*"

In paragraphs following the above quote, Hamilton noted that

"*Newton, however, by his theory of emission and attraction, was led to conclude that the velocity of light was directly, not inversely, as the index, and that it was increased instead of being diminished on entering a denser medium*; *a result incompatible with the theorem of shortest time in refraction. The theorem of shortest time was accordingly abandoned by many, and among the rest by Maupertuis, who, however, proposed in its stead, as a new cosmological principle, the celebrated law of least action which has since acquired so high a rank in mathematical physics, by improvements of Euler and Lagrange.*"

Hamilton[27] was the turning point on the application to dynamics of a general mathematical method previously applied to optics, while Hamilton[28] and Hamilton[29] gave the fully developed general method in dynamics.

### 4.4. *Little emphasis on the Hamiltonian and* $(\mathbf{p} - e\mathbf{A})$ *until quantum theory*

The history of post-Maxwell electromagnetism is described in many books, such as *The Maxwellians* by Hunt;[60] *From Maxwell to Microphysics* by Buchwald;[8] and *Intellectual of Mastery of Nature*: *Theoretical Physics from Ohm to Einstein* by Jungnickel and McCormmach.[63,64] The two encyclopedia articles by Lorentz[77,78] give a technical summary of the development up to and around the turn of the century. O. Darrigol[11] has given a historical account, *Electrodynamics from Ampère to Einstein*.

From our discussion of the history of $(\mathbf{p}-e\mathbf{A})$, we see that while the least action principle was invoked by a number of people, it appears that not much emphasis was placed on the use of the Hamiltonian $H = (\mathbf{p} - e\mathbf{A})^2/2m$ before the advent of quantum theory and quantum mechanics. This fact had been explicitly stated in Whittaker[133] in his article on Hamilton.

## 5. Heaviside–Hertz's Elimination of A

### 5.1. *Heaviside's view on the role of the potentials*

O. Heaviside (1850–1925) seemed to have derived great satisfaction in 1885 in eliminating the vector potential **A** from the Maxwell equations. Disagreeing with Maxwell's elevation of **A** to the rank of a fundamental quantity, Heaviside regarded "**A** and its scalar potential *parasite* $\psi$ sometimes causing great mathematical complexity and indistinctiveness; and it is, for practical reasons, best to *murder* the whole lot, or, at any rate, merely employ them as subsidiary functions." (Ref. 35 or 36, Vol. 2, p. 482.) "Thus $\psi$ and **A** are murdered, so to speak, with a great gain in definiteness and conciseness." (Ref. 35 or 36, Vol. 2, p. 468.)

In a PostScript to the 1889 *Phil. Mag.* article referred to above, Heaviside stated,

> "*According to the way of regarding the electromagnetic quantities I have consistently carried out since January 1885, the question of the propagation of, not merely the electrical potential $\psi$, but the vector potential* **A**, *does not present itself as one for discussion; and, when brought forward, proves to be one of a metaphysical nature.*"

In his Preface to his two-volume set of *Electrical Papers*,[36] Heaviside reiterated,

> "*The duplex method* (*referring to his recasting of the Faraday's law as* curl **E** *in parallel to the* curl **B** *for the Ampère's law as Maxwell has it*) *eminently suited for displaying Maxwell's theory, and brings to light many useful relations which were formerly hidden from view by the intervention of the vector potential and its parasites.*"

In his Preface to his three-volume set of *Electromagnetic Theory*,[37–39] again he stated, referring to "an outline scheme of the fundamentals of electromagnetic theory from the Faraday–Maxwell point of view, with some small modifications and extensions upon Maxwell's equations,"

> "*... it is also done in the duplex form I introduced in* 1885, *whereby the electric and magnetic sides of electromagnetism are symmetrically exhibited and connected, whilst the 'forces' and 'fluxes' are the objects of immediate attention, instead of the potential functions which are such powerful aids to obscuring and complicating the subject, and hiding from view useful and sometimes important relations.*"

In a similar vein, Heaviside (Ref. 37, Vol. 1, p. 383) has this to say:

> "*The reader who is acquainted with the* (*at present*) *more 'classical' method of treating the electromagnetic field in terms of the vector and scalar potentials cannot fail to be impressed by the difference of procedure and of ideas involved. In the present method we are, from first to last, in contact with those quantities which are believed to have physical significance* (*instead of*

*with mathematical functions of an essentially indeterminate nature), and with the laws connecting them in simplest form.*"

What did Heaviside mean by "indeterminate nature"? Whatever he meant, we observe that *the flexibility of the potential functions is precisely the concept essential to today's understanding of the fundamental forces of nature. This flexibility is what is meant by the technical term: gauge invariance.*

### 5.2. *Hertz's view on the role of the potentials*

H. Hertz (1857–1894) wrote, among others, three interesting theoretical papers on electrodynamics. According to Hertz (Ref. 51, footnote, p. 289), Helmholtz in 1847 and Thomson in 1848 deduced induction from the electrodynamic action.

Hertz (Refs. 51 and 59, p. 286) did away with the vector potential:

"*The vector potentials of electric and magnetic currents have hitherto occurred as quite separate, and from them the electric and magnetic forces were deduced in an unsymmetric manner. This contrast between the two kinds of forces disappear as soon as we attempt to determine the propagation of these forces themselves, i.e. as soon as we eliminate the vector potentials from the equation...*"

Hertz (Refs. 53 and 55, p. 209) regarded the potentials as *scaffolds*:

"*Als eine rudimentare Erscheinung mathematischer Natur nenne ich das Vorherrschem des Vectorpotentiales in den Grundgleichchungen. Bei dem Aufbau der neuren Theorien dienten die Potentiale als Gerüst... Nachdem wir aber gelernt haben, die Kräfte welber als Grössen der letzteren Art anszusehen, hat ihr Ersatz durch Potentiale nur dann einen Zweck, wenn damit ein mathematischer Vorteil erreicht wird. Und ein solcher scheint mir mit der Einführung des Vectorpotentiales in die Grundgleichungen nicht verbunden, in whelchen man ohnehin erwarten darf, Beziehungen zwischen Grössen der physikalischen Beobachtung, nicht zwischen Rechnungsgrössen zu finden.*"

Hertz (Refs. 53 and 55, pp. 209 and 210) acknowledged that Heaviside had worked along the same direction (in eliminating the vector potential) since 1885.

### 5.3. *Maxwell*

Less well known than the elimination of the vector potential in the hands of Heaviside and Hertz, Whittaker (Ref. 132, p. 287) asserts that Maxwell himself did it in 1868:

"*As the potentials do not possess any physical significance, it is desirable to remove them from the equations. This was done afterwards by Maxwell* *himself, who in* 1868 *proposed to base the electromagnetic theory of light solely on the equations*

$$\operatorname{curl}\mathbf{H} = 4\pi\mathbf{S}\,, \quad \operatorname{curl}\mathbf{E} = -\frac{\partial\mathbf{B}}{\partial t}$$

*together with the equations which define* **S** *in terms of* **E**, *and* **B** *in terms of* **H**." (See Ref. 87.)

## 6. A Cannot be Eliminated: Aharonov–Bohm Experiment

Although Maxwell himself in all of his papers and in his *Treatise* always explicitly used the vector potential in his equations, throughout most of 20th century the Heaviside–Hertz form of Maxwell's equations were taught to college students all over the world. The reason is quite obvious: the Heaviside–Hertz form is simpler, and exhibits an appealing near symmetry between **E** and **H**.

With the widespread use of this vector-potential-less version of Maxwell's equations, there arouse what amounted to a dogma: that the electromagnetic field resides in **E** and **H**. Where both of them vanish, there cannot be any electromagnetic effects on a charged particle.

This dogma explains why when the Aharonov–Bohm[1] article was published it met with general disbelief. A year later, Chambers[9] claimed to have verified experimentally the *A*–*B* effect. But he used a tapered magnetic needle instead of a long solenoid, and the leakage magnetic field led to many confusing discussions about whether he had really seen the effect. Fortunately, beautiful quantitative experiments were finally performed by Tonomura and collaborators,[120,121] showing conclusively the existence of the *A*–*B* effect, which means that **E** and **H** together do not completely describe the electromagnetic field, and that the vector potential cannot be totally eliminated in quantum mechanics. In the words of Wu and Yang:[136] the field strengths *underdescribe* electromagnetism.

## 7. Gauge Theory

The history of gauge theory has been described in detail in several publications in the last 30 odd years.[13,141,143–145,147–150,96,97,61] We shall only outline here the main conceptual steps in this history.

### 7.1. *H. Weyl (1918)*

In 1918 and 1919 three papers appeared that served to start in time a great transformation of fundamental physics in the latter part of the 20th century. Yet these papers were not written by a physicist, nor were they based on experimental facts. And they were immediately objected to by Einstein. The author was H. Weyl (1885–1955), a distinguished mathematician who liked to dabble in philosophy. What were his motivations? How did he land on the chief ideas behind these papers?

We do not know the full answers to these questions. But we shall make a few remarks as follows.

(1) Weyl was very much motivated by Einstein's geometrization of gravitational forces through a quadratic infinitesimal form $ds^2$. He wanted to geometrize electromagnetic interactions through a linear form $d\varphi = \varphi_\mu \, dx^\mu$.

(2) He was impressed by the ideas of Levi-Civita, Hessenberg and himself about the parallel displacement of a vector in Riemannian geometry. He asked, if after going around a closed loop in space–time, the parallel displaced vector may not return to its original direction, why not also its length? ("Warum nicht auch seine Länge?") (See Ref. 145, footnote 14.)

He seemed to try to apply this length-changing idea by making a conformal dilation which varies from point to point in space–time. To this end he introduced in the middle of his first paper[125] a "Proportionalitätsfaktor"

$$\exp\left(\int d\varphi\right), \quad d\varphi = \varphi_\mu \, dx^\mu = \boldsymbol{\phi} \, d\mathbf{x}$$

in which he later identified $\varphi_\mu$ with the vector potential $A_\mu$ times a numerical constant. Thus we read in that paper

$$(ds')^2 = \lambda (ds)^2 \,, \quad d\varphi' = d\varphi - \left(\frac{d\lambda}{\lambda}\right), \tag{7.1}$$

where $\lambda$ is the conformal factor. These two equations later evolved, in 1929, into the gauge transformations of the first and second kind, by which time he had also adopted the insertion of the factor $i = \sqrt{-1}$ in the numerical constant relating $\varphi_\mu$ with $A_\mu$. He wrote in 1929:[74]

> "*By this new situation, which introduces an atomic radius into the field equations themselves — but not until this step — my principle of gauge-invariance, with which I had hoped to relate gravitation and electricity, is robbed of its support. But it is now very agreeable to see that this principle has an equivalent in the quantum-theoretical field equations which is exactly like it in formal aspect; the laws are invariant under the simultaneous replacement of $\varphi$ by $e^{i\lambda}\varphi$, $\phi_\alpha$ by $\phi_\alpha - \partial\lambda/\partial x_\alpha$, where $\lambda$ is an arbitrary real function of position and time. Also the relation of this property of invariance to the law of conservation of electricity remains exactly as before… the law of conservation of electricity*
>
> $$\frac{\partial s_\alpha}{\partial x_\alpha} = 0$$
>
> *follows from the material as well as from the electromagnetic equations. The principle of gauge-invariance has the character of general relativity since it contains an arbitrary function $\lambda$, and can certainly only be understood in terms of it.*"

Please notice that he has changed notation here. The new $\lambda$ is the old $(\ln \lambda)$, and the old conformal factor is now a phase factor, multiplied not onto the metric $ds^2$, but onto the wave function $\psi$.

Thus the key ideas of Weyl were already in his first paper.[125] The latter two papers of 1918–1919, for our present investigation of the evolution of ideas, did not really add anything of importance.

Weyl's papers on physics were rambling, discursive and philosophical. They were also extraordinarily original. He was exploring new ideas, so there was great fluidity in his style. He frequently changed names for key concepts. Proportionalität later became Strecke, so that when Schrödinger in 1922 referred to the Proportionalitätsfaktor, it became Streckenfaktor (see Yang Ref. 146).

When the concept of gauge invariance first came up, he called it Massstab-Invarianz. But later he settled on Eichinvarianz. English translation of this term is now gauge invariance, but had been at times calibration invariance and measure invariance.

(3) Why did Weyl first consider a Proportionalitätsfaktor, and not a phase factor? The answer is simple: (a) Weyl had started his thinking with Riemannian geometry, not waves. So there were only real numbers. (b) The importance of phase factors in physics only began with de Broglie and Schrödinger a few years later when wave mechanics was born.

(4) Weyl strongly associated gauge-invariance (measure-invariance) with the conservation of electricity. A paragraph in Ref. 126 (in the 1923 translation) reads:

> "*For we shall show that as, according to investigations by Hilbert, Lorentz, Einstein, Klein, and the author, the four laws of the conservation of matter* (*the energy–momentum tensor*) *are connected with the invariance of the action quantity* (*containing four arbitrary functions*) *with respect to transformations of coordinates, so in the same way the law of the conservation of electricity is connected with the 'measure-invariance'.*"

(5) Starting from the Proportionalitätsfaktor there arose an operator

$$(\boldsymbol{\partial} - e\mathbf{A})$$

which played an important part in Weyl's theory. In today's terminology, Weyl was dealing with connections on a $R_1$ bundle where the fiber is the noncompact additive group of real numbers.

### 7.2. *Einstein's objection*

Einstein objected to Weyl's idea in a postscript published in 1918 together with Ref. 126. He said if Weyl's idea were correct, "*The length of a common ruler (or the speed of a common clock) would depend on its history.*" (Our translation.)

which was deeply perspective and killed the original version of Weyl's idea.

As we shall see below, the idea was later resurrected in 1927–1929 by the insertion of a factor $i = \sqrt{-1}$ in Weyl's Proportionalitätsfaktor, converting it to a phase factor: a change of the quantum mechanical phase factor does not affect the length of a ruler, so Einstein's objection was rendered nonoperative.

It was pointed out in a paper by Yang in 1983 (Ref. 144) that although Einstein's objection is nonoperative in the sense that the phase factor does not affect the length of a ruler, but one could raise the question whether the phase, or difference between phases, can be observed. Had Einstein or Weyl revisited in 1929 the original Einstein's objection, the $A$–$B$ effect would have been proposed some thirty years before Aharonov and Bohm came upon it in 1959.

### 7.3. *V. Fock (1898–1974) and F. London (1900–1954) (1926 and 1927)*

After quantum mechanics was developed in 1925 and 1926, Fock and London independently pointed out that in quantum mechanics,

$$(\mathbf{p} - e\mathbf{A}) \rightarrow (-i\boldsymbol{\partial} - e\mathbf{A}) = -i(\boldsymbol{\partial} - ie\mathbf{A})$$

which is very similar to Weyl's $(\boldsymbol{\partial} - e\mathbf{A})$ except for the replacement of $\mathbf{A}$ with $i\mathbf{A}$. Of course with this replacement, Weyl's factor becomes

$$\exp \int [-ie\mathbf{A}\, d\mathbf{x}]$$

which is a phase factor. Thus "gauge theory" is a misnomer. It should have been called a "phase theory."

### 7.4. *H. Weyl (1929)*

Weyl came back in 1929 in a paper that incorporated the factor $i = \sqrt{-1}$. It is a very remarkable paper which for the first time explicitly discussed the concept of gauge transformations.

We had mentioned above in Subsec. 7.1 that Weyl had strongly associated gauge invariance with charge conservation. This association found its way into Pauli's review article of 1941 which was the "bible" in field theory for the graduate students of the 1940's. We shall see in Sec. 8 that this association turned out to be important for later developments.

Throughout the 1930's and 1940's gauge invariance was well known to theoretical physicists, but it produced nothing really new. Its only use seemed to be for a young theorist to spring a "smart" question at the end of a seminar: "Is your result gauge invariant?"

## 8. A Principle for Interactions: Yang and Mills (1954); Symmetry Dictates Interaction

The discovery of many "strange" particles in the late 1940's and early 1950's was the background motivation for finding a general *principle* for interactions between particles. (See Ref. 143, p. 19.) In 1954 Yang and Mills published a short abstract[138] and a short paper[139] generalizing Weyl's gauge invariance to non-Abelian gauges. It is interesting today to see how the authors stated the motivation for this generalization.

We reprint here the full text of the short abstract:

"***Isotopic Spin Conservation and a Generalized Gauge Invariance***

> *The conservation of isotopic spin points to the existence of a fundamental invariance law similar to the conservation of electric charge. In the latter case, the electric charge serves as a source of electromagnetic field; an important concept in this case is gauge invariance which is closely connected with* (1) *the equation of motion of the electromagnetic field,* (2) *the existence of a current density, and* (3) *the possible interactions between a charged field and the electromagnetic field. We have tried to generalize this concept of gauge invariance to apply to isotopic spin conservation. It turns out that a very natural generalization is possible. The field that plays the role of the electromagnetic field is here a vector field that satisfies a nonlinear equation even in the absence of other fields.* (*This is because unlike the electromagnetic field this field has an isotopic spin and consequently acts as a source of itself.*) *The existence of a current density is automatic, and the interaction of this field with any fields of arbitrary isotopic spin is of definite form* (*except for possible terms similar to the anomalous magnetic moment interaction terms in electrodynamics*)."

The focus here is to look for a *principle* for interactions through using conserved quantities as sources of vector transmitting fields.

In the paper, however, which was submitted a few months later, a different motivation was summarized in the first sentence of the abstract of the paper:

> "*It is pointed out that the usual principle of invariance under isotopic spin rotation is not consistent with the concept of localized fields.*"

This sentence is certainly overstated: it would be better to read

> "*It is pointed out that the usual principle of invariance under isotopic spin rotation is not consistent with* ***the spirit of*** *the concept of localized fields.*"

Be that as it may, for the purpose of the present paper, the important point about the generalization was the replacement of $(\boldsymbol{\partial} - ie\mathbf{A})$ with $(\boldsymbol{\partial} - ie\mathbf{B})$ where $\exp(-ie\mathbf{B})$ is a matrix representation of a Lie group.

| Gauge Field Terminology | Bundle Terminology |
|---|---|
| Gauge (or global gauge) | principal coordinate bundle |
| Gauge type | principal fiber bundle |
| Gauge potential $b_\mu^k$ | connection on a principal fiber bundle |
| $S$ | transition function |
| Phase factor $\Phi_{QP}$ | parallel displacement |
| Field strength $f_{\mu\nu}^k$ | curvature |
| Source (electric) $J_\mu^k$ | ? |
| Electromagnetism | connection on a $U_1$ bundle |
| Isotopic spin gauge field | connection on a $SU_2$ bundle |
| Dirac's monopole quantization | classification of $U_1$ bundle according to first Chern class |
| Electromagnetism without monopole | connection on a trivial $U_1$ bundle |
| Electromagnetism with monopole | connection on a nontrivial $U_1$ bundle |

Fig. 4. "Dictionary" in the Wu–Yang paper.[136] What was particularly interesting to the mathematicians was the ? mark, i.e. the concept of the electric source 4-vector $J_\mu$, which was among the basic concepts in physics. This is a good example of how a fundamental and subtle mathematical concept could be discovered by very different routes, in mathematics and in physics, through very different traditions and very different value judgments. Please compare with Ref. 149.



Thus given a Lie group associated with a conservation law, i.e. an invariance, the new principle of interaction gives rise to a gauge theory that incorporates that invariance, in a beautiful and unique way. Invariance, of course, means symmetry, hence the statement *symmetry dictates interaction* (C. N. Yang, *Phys. Today* **33**, 42 (1980)).[142]

This gauge theory of interactions, however, did not find acceptance among the physicists in the 1950's and in much of the 1960's, despite its appealing elegance. The reason is the bothering question about whether the vector field in the theory should have mass zero. It was only in the late 1960's that the idea of *symmetry breaking* was applied to non-Abelian gauge theories to remove this bothersome question. This development led to the *Standard Model*, which has been spectacularly successful in describing essentially all aspects of particle physics in the last 30 years. The exciting history of the *standard model*, however, lies outside of the considerations of the present paper.

## 9. Connections on Fiber Bundles

The mathematical meaning of the matrix $-ie\mathbf{B}$ in $(\boldsymbol{\partial} - ie\mathbf{B})$ as a "connection" was not understood by Yang and Mills in the 1950's. In 1983 Yang described (Ref. 143, p. 73) how during 1967–1968 he finally noticed the similarity between the nonlinear terms in the Yang–Mills paper and the nonlinear terms in the definition of the Riemann curvature tensor, and thereby began to understand the geometrical meaning of gauge theory. He then learned the rudiments of fiber bundle theory from his mathematician colleague Jim Simons, and wrote a paper in 1975 with T. T. Wu[136] which contained a "dictionary" of the corresponding concepts in physics and in mathematics (Fig. 4). They discussed this "dictionary" with I. M. Singer when the latter visited Stony Brook in the summer of 1976. Singer then lectured on gauge theory at Oxford and wrote an article with Atiyah and Hitchin. He later described this experience in 1988:[105]

> "*Thirty years later, I found myself lecturing on gauge theories at Oxford, beginning with the Wu and Yang dictionary and ending with instantons, i.e. self-dual connections. I would be inaccurate to say that after studying mathematics for thirty years, I felt prepared to return to physics. Instead, elementary particle physics turned to modern mathematics, and some of us found the interplay full of promise.*"

With the 1978 article by Atiyah, Hitchin and Singer,[5] an intense period of interest among mathematicians in gauge theory began.



## 10. The Concepts of Connections and Fiber Bundles in Mathematics

The concept of connections in mathematics had started with the works of Riemann, Christoffel, Ricci, Levi-Civita and E. Cartan. (We have not studied this evolution in detail.) For obvious reasons the concept had started with the geometry of the tangent bundle of a manifold. Extension to other bundles took place in the 20th century. According to Dieudonné (Chapter III of Ref. 14), this was first done in a paper by Hotelling in 1925. The word fiber (Faser) later appeared in the work of Seifert in 1932. Then, "*The first genuine 'fiber space' was only defined by Hassler Whitney in* 1935 *under the name 'sphere-space'.*"

According to Spivak,[107] connections on principle bundles were first developed in Ehresmann.[17]

The field of global differential geometry, which had profoundly influenced every major branch of mathematics in the latter half of the 20th century, was ushered in by the works of Chern. (See Ref. 14.)

It is interesting to note here that an example of a nontrivial bundle had already occurred in Dirac's 1931 paper[16] on magnetic monopoles. (See also the paper by Wu and Yang, Ref. 137.)

Also interesting is the fact that Maxwell was keenly aware of the possible origin of deep mathematics in the examination of the physical world. Some 50 years before Maxwell was born, Lagrange (1736–1813) had written in 1781 to his friend and mentor d'Alembert (1717–1783), lamenting the dearth of new developments in mathematics and in mechanics:

"*I also think that the mine has become too deep and sooner or later it will be necessary to abandon it if new ore-bearing veins shall not be discovered. Physics and chemistry display now treasures much more brilliant and easily exploitable, thus, apparently, everybody has turned completely in this direction, and possibly posts in geometry in the Academy of Sciences will some day be like chairs in Arabic Language in universities at present.*"

(See Arnold in Ref. 4, and the discussion by Yang in Ref. 149).

In the event, "new ore-bearing veins" did get discovered — by Faraday (1791–1867), whose ideas led Maxwell to his equations and to the evolution of concepts outlined in the present paper. Faraday knew little mathematics. He was a great experimentalist, but had profound geometrical intuition. He conceived of the "lines of force" and "the electrotonic state." He had results expressed neither in the form of the differential equation

$$\nabla \times \mathbf{E} = -\dot{\mathbf{H}} \tag{10.1}$$

nor in the form of the integral equation

$$\oint \mathbf{E} \cdot d\boldsymbol{\ell} = -\iint \dot{\mathbf{H}} \cdot d\boldsymbol{\sigma} \,. \tag{10.2}$$

Rather his results were in terms of concepts *equivalent* to this integral equation, and Maxwell was the one who understood this equivalence. In the preface to the first edition of *Treatise*, published years later, Maxwell characterized Faraday's intuitive results as follows:

"*As I proceeded with the study of Faraday, I perceived that his method of conceiving the phenomena was also a mathematical one, though not exhibited in the conventional form of mathematical symbols*....

*When I had translated what I considered to be Faraday's ideas into a mathematical form, I found that in general the results of the two methods coincided, so that the same phenomena were accounted for, and the same laws of action deduced by both methods, but that Faraday's methods resembled those in which we begin with the whole and arrive at the parts by analysis, while the ordinary mathematical methods were founded on the principle of beginning with the parts and building up the whole by synthesis*...."

Maxwell was really saying here that Faraday had used the integral form (10.2), and ordinary mathematical methods had used the differential form (10.1), of the same physical law.

The equivalence of the two was via Stokes' theorem (see Appendix) which today has evolved into a key idea in differential geometry, assuming the wonderfully simple and potent form:

$$\int_{\partial R} \mathbf{A} = \int_R d\mathbf{A} \,.$$

Upon Faraday's death on August 25, 1867, Maxwell wrote a tribute which was published in *Nature*. Two passages in this tribute were as follows:

> "*The way in which Faraday made use of his idea of lines of force in co-ordinating the phenomena of magnetoelectric induction shews him to have been in reality a mathematician of a very high order — one from whom the mathematicians of the future may derive valuable and fertile methods....*
>
> *From the straight line of Euclid to the lines of force of Faraday this has been the character of the ideas by which science has been advanced, and by the free use of dynamical as well as geometrical ideas we may hope for further advance.*"

Maxwell concluded his tribute to Faraday with the following paragraph:

> "*We are probably ignorant even of the name of the science which will be developed out of the materials we are now collecting, when the great philosopher next after Faraday makes his appearance.*"

Today, more than 135 years later, it is perhaps appropriate to repeat these words to conclude our sketch of the evolution of the physicists' description of the fundamental forces of nature.

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## Appendix. Historical Notes on Stokes' Theorem

This theorem was first stated in a letter, dated July 2, 1850, from Thomson to Stokes. Stokes acknowledged on July 5, 1850, that it was elegant and new. Stokes used this as question No. 8 on the Smith Prize examination in 1854 (the year Maxwell took it), which has since come to be known as the Stokes' Theorem. In 1864, Maxwell used this theorem without attribution. The theorem was first published in Thomson–Tait's *Treatise of Natural Philosophy*.[117] It appeared in Thomson's paper, "On Vortex Motion."[118] In 1873, the theorem appeared in Maxwell's *Treatise*, Vol. 1, Sec. 24. Prior to that, Maxwell wrote to Stokes on January 11, 1871, and to Tait on April 4, 1871, inquiring about the history of this theorem. Stokes' reply is lost, but Tait's reply credited this theorem to Thomson. For an interesting historical account of the Stokes' Theorem, the reader is referred to the article by J. J. Cross[10] and subsequently published volumes of the correspondence between Stokes–Thomson,[135] and *The Scientific Letters and Papers of J. C. Maxwell*[31–33] and references therein. A summary account is given below.

**July 2, 1850**

Letter from Thomson to Stokes (1819–1903) (Ref. 135, Vol. 1, p. 63):

> My Dear Stokes
>
> "... Do you know that the condition that $\alpha\,dx + \beta\,dy + \gamma\,dz$ may be the diff$^{\rm l}$ of a function of two indep$^{\rm t}$ variables for all points of a surface is
>
> $$\lambda\left(\frac{d\beta}{dz} - \frac{d\gamma}{dy}\right) + m\left(\frac{d\gamma}{dx} - \frac{d\alpha}{dz}\right) + n\left(\frac{d\alpha}{dy} - \frac{d\beta}{dx}\right) = 0\,?$$
>
> I made this out some weeks ago with ref$^{\rm ce}$ to electromagnetism. With ref$^{\rm ce}$ to an elastic solid, the cond$^{\rm n}$ may be expressed thus — the resultant axis of rotation at any point of the surface must be perp$^{\rm r}$ to the normal.
>
> Yours very truly
> William Thomson

P.S. The following is also interesting, and is of importance with reference to both physical subjects.

$$\int(\alpha\,dx + \beta\,dy + \gamma\,dz)$$
$$= \pm\iint\left\{\lambda\left(\frac{d\beta}{dz} - \frac{d\gamma}{dy}\right) + m\left(\frac{d\gamma}{dx} - \frac{d\alpha}{dz}\right) + n\left(\frac{d\alpha}{dy} - \frac{d\beta}{dx}\right)\right\}dS\,,$$

where $\lambda$, $m$, $n$ denote the dir$^{\rm n}$ cosines of a normal through any el$^{\rm t}$ $dS$ of a [limited] surface; and the integ$^{\rm n}$ in the sec$^{\rm d}$ member is performed over a portion of this surface bounded by a curve round w$^{\rm h}$ the int$^{\rm n}$ in the 1$^{\rm st}$ member is performed.

**July 5, 1850**

Stokes' prompt reply to Thomson reads (Ref. 135, Vol. 1, p. 64):

> Dear Thomson,
>
> "... The theorems which you communicated are very elegant and are new to me. I have demonstrated them for myself, the first by the calculus of variations, the second (which includes the first as a particular case) by simple considerations like what I have employed at the beginning of my paper. I suppose from what you say that you arrived at these theorems by working with magnetic ideas....
>
> Yours very truly
> G. G. Stokes

**1854:** Stokes used this as Question No. 8 in the February 1854 Smith's Prize Examination, the year Maxwell took it.

**1864:** Maxwell used this theorem in his paper without attribution.

**1867:** The theorem was first published in Thomson–Tait's *Treatise of Natural Philosophy* (Ref. 117, article 190j).

**1869:** It appeared in Thomson's paper, Ref. 118. There Thomson states that "In a purely analytical light the result has an important bearing on the theory of the integration of complete or incomplete differentials. It was first given, with the indication of a more analytical proof than the preceding, in Thomson and Tait's *Natural Philosophy*, Sec. 190(j)."

**1871:** January 11, Maxwell wrote to Stokes: (Ref. 32, p. 351)

My dear Stokes

. . . Did not you set the theorem about the surface integral

$$\left(\left(\frac{d\beta}{dz} - \frac{d\gamma}{dy}\right) dy\, dz + \cdots + \right)$$

over the surface bounded by the curve $s$ being equal to

$$\left(\frac{\alpha dx}{ds} + \frac{\beta dy}{ds} + \frac{\gamma dz}{ds}\right) ds\,.$$

I have had some difficulty in tracing the history of this theorem. Can you tell me something about it.

Yours truly
J. Clerk Maxwell

I hope you saw the eclipse well.

Unfortunately, Stokes' reply to Maxwell's inquiry has not been preserved. However, on April 4, 1871, Maxwell wrote to Tait on a postcard (Ref. 32, p. 366), asking the same question:

Dr. T′. . . . But the history of

$$\iint \left\{\lambda\left(\frac{dZ}{dy} - \frac{dY}{dz}\right) + m\left(\frac{dX}{dz} - \frac{dZ}{dx}\right) + n\left(\frac{dY}{dx} - \frac{dX}{dy}\right)\right\} dS = \int \left(\frac{X dx}{ds} + \frac{Y dy}{ds} + \frac{Z dz}{ds}\right) ds$$

ascends (at least) to Stokes Smiths Prize paper 1854 and it then not altogether new to yours truly. Do you know its previous history? Poisson? on light???. . . .

Yrs $dp/dt$

(Note added: T′ is the nickname of Tait, and $dp/dt$ is the nickname of Maxwell in reference to a statement in thermodynamics: $dp/dt =$ JCM, the initial of Maxwell.)

Tait's reply to Maxwell is fortunately available (*Manuscripts in the University Library*, Cambridge, cited by Cross and as footnote 5 to Ref. 32, p. 366 by Harman)

> O $dp/dt, \ldots$ As to $\iint SUv\nabla\sigma\, ds = \int S\sigma\, d\rho$ I really thought it due to T and first published by the Archiepiscopal pair. . . .
>
> T′

(Note added: Here Tait wrote the integral theorem in the quaternion form. T is the nickname of Thomson. The "Archiepiscopal pair" refers to Thomson and Tait — with the Archbishops of York and Canterbury having the same surnames. See, footnote 5 of Ref. 32 cited above.)

**1873:** In Maxwell's *Treatise*, Vol. 1, Sec. 24, the theorem is stated and proved. A footnote reads:

> "This theorem was give by Professor Stokes, *Smith's Prize Examination*, 1854, Question No. 8. It is proved in Thomson and Tait's *Natural Philosophy*, Sec. 190(j)."

**1888:** The 1888 edition of Thomson and Tait's *Natural Philosophy*, Sec. 190(j) contains the following footnote:

> "This theorem was given by Stokes in his Smith's Prize paper for 1854 (Cambridge University Calendar, 1854). The demonstration in the text is an expansion of that indicated in our first edition (1867). A more synthetic proof is given in Sec. 60(q) of Sir W. Thomson's paper on "*Vortex Motion*," *Trans. R.S.E.* 1869. A thoroughly analytical proof is given by Prof. Clerk Maxwell in his *Electricity and Magnetism* (Sec. 24)." (1873, note added)

**1905:** Some of the *Math. Tripos Examinations* and the *Smith's Prize Examinations* at Cambridge University were reprinted in the Appendix of Vol. 5 (1905) of the *Mathematical and Physical Papers of G. G. Stokes* (1819–1903), edited by J. Larmor, who added the following footnote to Question No. 8 of 1854:

> "This fundamental result, traced by Maxwell (*Electricity*, Vol. 1, Sec. 24) to the present source, has of late years been known universally as Stokes' theorem. The same kind of analysis had been developed previously in particular cases in Ampère's memoirs on electrodynamics of linear electric currents. And in a letter from Lord Kelvin, of date July 2, 1850, relating to such transformations, which has been found among Stokes' correspondence, the theorem in the text is in fact explicitly stated as a postscript. The vector which occurs in the surface integral had been employed by MacCullagh, who recognized its invariance, about 1837, in optical dynamics. It reappeared in Stokes' hands in 1845 (*ante*, Vol. I, p. 81) as twice the differential rotation in the theory of fluid motion and formed the basis of the mathematical

theory of viscosity of fluids, as also at a later time of Helmholtz's theory of vortex motion; its application to vibrations of solid elastic media was developed by Stokes in 1849 (*ante*, Vol. II, p. 253). Thus the theorem, though first stated by Lord Kelvin, relates to a quantity which, as regards physical applications, may be claimed to be Stokes' vector."



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