TWO RIVAL PROGRAMMES IN 19th. CENTURY CLASSICAL ELECTRODYNAMICS :

ACTION-AT-A-DISTANCE VERSUS FIELD THEORIES.

This thesis is submitted for the degree of DOCTOR of PHILOSOPHY

by HAWORTH MARTIN HARROP FRICKÉ, a candidate registered at the

LONDON SCHOOL OF ECONOMICS AND POLITICAL SCIENCE.

TWO RIVAL PROGRAMMES IN 19th.CENTURY CLASSICAL ELECTRODYNAMICS :

ACTION-AT-A-DISTANCE VERSUS FIELD THEORIES.

BY

HAWORTH MARTIN HARROP FRICKÉ

ABSTRACT

The thesis is a historical case-study in which I.Lakatos's Methodology of Scientific Research Programmes is applied to 19th. Century classical electrodynamics. Two research programmes are appraised. One, the Action-at-a-distance programme, had as its hard core the theory that electromagnetic phenomena were the outcome of sources acting at a distance across empty space on each other. Its rival, the Field programme, had the hard core that electromagnetic phenomena were the outcome of behaviour by the space between the apparent sources. It is argued that the Action-at-a-distance programme was always the superior one of the two. This revision in the standard historical appraisal results from the use of Lakatos's methodology. The Action-at-a-distance programme developed progressively, through the theories of Ampère, Weber, and their successors, to a satisfactory and fairly complete account of the phenomena of electrodynamics. In contrast, the Field programme degenerated as it consisted of a sequence of ad hoc or heuristically ad hoc theories. Faraday, Maxwell, and Helmholtz vigorously criticised the Action-at-a-distance programme. These criticisms were extremely influential and some historians regard them as persuasive today. It is shown that these criticisms are entirely without merit and further that they could easily have been seen to be without merit at the time of their proposal. Finally, many subsidiary theses, advocated by writers in the history and philosophy of the development of classical electrodynamics, are critically assessed.

CONTENTS

Chapter 1 : Introduction.

1. Introduction.

2. The Action-at-a-distance (A.A.D.) Programme : Its Hard Core and Heuristic.

3. The Philosophical Objection to A.A.D.

4. The Origins of Electrodynamics : Oersted and Ampère.

5. First Criticisms of A.A.D. : Faraday's Objections and His Foundation of the Field Programme.

6. The Rival Views on the Sources and Receivers of Force.

7. The Early Development of A.A.D. : Weber's Unification of Electrodynamics and Other Theoretical Advances.

8. The Growth of the Field Heuristic 1840-1860.

9. The Field Programme to 1860.

10. An Appraisal of the Two Programmes to 1860.

11. Maxwell's Theories of Electromagnetism.

12. The A.A.D. Theory of Light and an Appraisal of the Two Programmes from 1860 - 1900.

1. Introduction :

Imre Lakatos died in 1974. His contribution to the philosophy of science was twofold : a theory for the appraisal of scientific views and a theory on the relations between the history and the philosophy of science.[1] The former -- the Methodology of Scientific Research Programmes (M.S.R.P.) was complete; even so, like most theories, it was accompanied by an agenda of unsolved problems. The latter lay behind Lakatos's pleas for historical case-studies, and still only a few of these have been written.[2,3]

The Methodology of Scientific Research Programmes is well known.[4] In brief, it suggests that only a <u>series</u> of theories should be appraised. A series is characterized by a hard core which is a theory which runs through the series giving it continuity, and a heuristic which is the problem solving mechanism which dictates the lines of research. For example, the Newtonian research programme

---

1. The theories receive their fullest expression in I.Lakatos (1970), 'Falsificationism and the Methodology of Scientific Research Programmes', and I.Lakatos (1971),'History of Science and Its Rational Reconstructions'.

2. The ones in existence are collected together in C.Howson (ed.) (1976), <u>Method and Appraisal in the Physical Sciences,</u> and this volume also contains a reprint of Lakatos (1971).

3. My initial attraction to the topic of this dissertation arose as follows. Lakatos's (1971) suggests that case-studies should scrutinize rival research programmes which predict novel facts. Classical electro-dynamics had the rival research programmes of the Continental action-at-a-distance school and the British field theorists, and in the anticipation of propagated electromagnetic waves it had one of the most stunning novel facts ever. As Planck writes : '.... the criterion of the value of a theory, that it explains quite another phenomena besides those on which it was based, has never been so well satisfied as with Maxwell's theory ... This must for all time remain one of the greatest triumphs of human intellectual endeavour.' Maxwell (1931), <u>James Clerk Maxwell ·: a Commemorative Volume 1831-1931</u>, page 57.

4. See Lakatos (1970) and references therein.

had as its hard core the law of gravity and the three laws of dynamics and part of its heuristic was the instruction 'First treat the planets as mass points, then as mass balls, then as spinning mass balls, then as spinning mass balls with perturbations ... &c'. A research programme is good in so far as the members of the sequence predict novel facts, and bad in so far as the programme lags behind the facts and can explain them only in an ad hoc fashion. This notion of ad hoc covers two cases: the old standard use of ad hoc, and a new sense of heuristically ad hoc which occurs when the facts are actually explained but they are not explained in accordance with the plan of research. Such an appraisal is a function of time -- a programme may become better or worse -- and is in principle without end in that the value of it does not have to settle to a limit.[1] Occasionally, for clarity, I will avoid the jargon by talking of research programmes as consisting of a single theory and a heuristic -- the single theory referred to here is the hard core theory of the series; so I might describe the Newtonian research programme as being composed of the gravitational-dynamic theory plus its heuristic.

What does the appraisal indicate? I maintain, and this is argued in Appendix 1, that it measures three properties. First it shows the epistemological superiority of one theory in the series over its predecessor -- if one of two more or less similar theories makes a successful prediction which the other cannot account for, then that prediction can serve as objective grounds for preferring one theory. Thus, with a good developing programme, one can say that knowledge is growing. Second, and probably the most controversially, often it can show the epistemological superiority of one programme over its rival at a given

---

1. In connection with this, see Lakatos (1971) page 104 Note.

<u>time</u>. Finally, it indicates the heuristic power of a whole programme
-- for the continuing discoveries are good evidence for the potential
to discover.

Any author of a case-study ought to face the questions: Why
should the case-study be done? and How should the case-study be done?
These questions too are considered in Appendix 1. This case-study's
significance lies in its describing the growth of a sector of knowledge.
The answer proposed to the second question is that the history must be
approached theoretically and fallibly and -- given this -- it is
preferable to do so from a methodologically advanced standpoint which
is explicitly stated. Here the standpoint is Lakatos's M.S.R.P.

Descriptions of the growth of classical electrodynamics fall
into three alternative styles:

   a) The account given by most <u>physicists</u> is that all the develop-
      ments were due to field theories; as a result, the so-called
      Maxwell equations figure largely in physics textbooks;

   b) The account given by most <u>historians</u> acknowledges the existence
      of an alternative electrodynamics -- the action-at-a-distance
      theories -- and claims that both approaches made contributions
      and that the modern view is a synthesis of the two;

and finally,

   c) The account, generally considered to be <u>outrageous</u>, due to,
      and championed solely by, <u>A. O'Rahilly</u>, to the effect that the
      action-at-a-distance tradition was the important one.[1]

The physicists' story is implausible because half of Maxwell's
so-called equations were not due to Maxwell, and also the modern unified

---

1. A.O'Rahilly (1965), <u>Electromagnetic Theory</u>. First published in 1938
as 'Electromagnetics'.

view contains the Lorentz force law and the theory of electrons both
of which were alien to field methods.

The historian tells the tale as follows. There were field
ideas, which had their basis in Maxwell's translation into mathematics
of Faraday's work. Field theories had one major success and one major
failure; the success was in the propagated electromagnetic waves, whose
existence was suspected from early on; the failure was in the inability
to provide a coherent account of the source of the fields. Both
Faraday and Maxwell tended to identify charges with merely the term-
ination of lines of force, or -- more extremely -- with the result of
polarization of the medium; but then the polarization was supposed to
be caused by the charges and so it seemed that the polarization was
caused by itself. Action-at-a-distance theories, on the other hand,
also had one major success and one major failure. The success was the
discovery of electrons, whose existence was suspected from early on;
the failure was in the inability of the theories to anticipate the
propagation of electromagnetic waves. Eventually the field theories
were wedded to the theory of electrons and a coherent account
resulted.[1]

There is merit in the historian's view, but my sympathies and
my arguments lie with O'Rahilly for action-at-a-distance (A.A.D.)
electrodynamics has received harsh treatment from scientists and
historians. The field theories seemed unsatisfactory to O'Rahilly
but he lacked the philosophical sophistication to back up his instincts

_____

1. For examples of this, see Mary Hesse (1961) Forces and Fields;
W. Berkson (1974) Fields of Force; T. Hirosige (1962) 'Lorentz's Theory
of Electrons and the Development of the Concept of Electromagnetic
Field'; and A.F. Chalmers (1971) The Electromagnetic Theory of J.C.
Maxwell and Some Aspects of its Subsequent Development.

with fair and objective evaluation. His fault was that he was eclectic and inconsistent; he adjusted his grading criterion to yield the value judgements he had antecedently decided upon. For instance, Maxwell's equations were interpreted by Maxwell as being about a polarizable vacuum, and O'Rahilly savages Maxwell for this (see the Chapter 3 to his book), and Ludwig Lorenz's equations were interpreted by Lorenz as being about conducting matter distributed through empty space, yet O'Rahilly describes this interpretation as being an irrelevant addition (see page 183) and focusses on the equations -- but the two assumptions about space were similar, and their relations to their equations were the same. I will add a sound philosophical base to O'Rahilly's instincts.[1] I also add historical detail and an epistemological slant to O'Rahilly's case. However, I do not offer a critical comparison between O'Rahilly's work and mine, for my aim is to fortify his theses and to make our claims resilient to attack from outside.

The problem of this dissertation is to give an account of the growth of classical electrodynamics as knowledge, and this chapter is devoted to a sketch of the material to be covered. I argue in Appendix 1 that this problem of knowledge is equivalent to the question 'Which theory or theories should the scientists of the period have advocated as a description of the electrodynamic properties of the world?' and conclude that the scientists should advocate those theories judged best by Lakatos's M.S.R.P. As far as I have been able to find out, there have been no attempts to argue the superiority of one programme over the

_____

1. O'Rahilly frequently uses the modern textbook as the touchstone when judging historical theories. See, for example, page 83 of his (1965). This practice is unacceptable philosophically. For the basic arguments, see J. Agassi (1963), Towards An Historiography of Science.

other at a particular time, apart from those by O'Rahilly and by the scientific figures involved. Historians would rather describe theories than evaluate them. O'Rahilly was unorthodox, for the second time, in this -- his was an essay in 'constructive criticism',[1] and as such was historiographically superior to most works in this field.

My thesis, an epistemological variant of O'Rahilly's outrageous views, is that the A.A.D. programme was the superior one throughout the 19th century; the A.A.D. programme will be argued to be scientifically and philosophically better than the Field programme.

---

1. See the Preface to his book. O'Rahilly -- a Professor of Mathematical Physics -- must have been surprised at the curious reception of his book. Historians thought that the book was not history because they felt that histories should describe theories and not evaluate them. These feelings are responsible for the preponderance of book titles like 'A History of Theories of Electricity'. Scientists thought that the book was not science because scientists should evaluate modern theories not historical ones. My view is that O'Rahilly's work is a superior type of history.

2. The A.A.D. Programme : Its Hard Core and Heuristic :

The A.A.D. programme was the result of the attempt to harness electricity and magnetism to the sophisticated heuristics of Newtonian Gravitational theory. Such a union seemed natural once it had been found that electricity and magnetism were governed by inverse-square laws of the form :

$$F \propto \frac{M_1 M_2}{d^2}$$

, where F is the electric or magnetic force, M_1 and M_2 are the electric or magnetic 'masses', and d is the distance between the 'masses'.

The programme had as its <u>hard core</u> the thesis that <u>electromagnetic phenomena were the outcome of sources acting at a distance on each other</u>. Two further views were central though not part of the hard core. First, that the laws involved were inverse-square central force laws analogous to the law of gravity. The similarity here was not total -- there was only one form of gravitating matter, whereas there were positive and negative charges and North and South magnetic poles. Secondly, that the sources were discrete; A.A.D. electrodynamics, in common with most types of streamlined Newtonianism, contained atomism as a part.[1] An important decision to be made about the hard core concerns whether it contained the view that these distance forces act instantaneously. What rests on this is the truth of the suggestion that Field views had an intrinsic advantage because A.A.D. had to be

---

1. In connection with this, see R.Kargon (1969), 'Model and Analogy in Victorian Science', page 424 and f.

instantaneous whereas Field accounts had to have finitely propagated

effects and electromagnetic action actually does take time to spread.

Rosenfeld, to take a typical example here, writes :

> central force physics was doomed to ultimate failure in the
> domain of electromagnetic phenomena, where the basic idea
> of instantaneous action at a distance meets with an essential
> limitation of its validity.[1]

I reject this allegation.   I also refuse to accept Woodruff's

recommendation :

> We [confine] the term 'action at a distance' to forces acting
> instantaneously at a distance ... On the other hand, we take it
> as characteristic of field theories in their fully developed
> form ... that the propagation of the influence of a particle
> through space takes time, and occurs at a <u>finite speed.</u>[2]

Berkson too goes astray here :

> Field theories predicted that all actions of one body on
> another took time to move between bodies, while action-at-
> a-distance theories said the action was instantaneous.[3]

The question is whether it is more fruitful for historical purposes

to identify the A.A.D. programme by the hard core 'sources plus

empty space' or by the hard core 'sources plus empty space plus

instantaneous propagation'.   I favour the former.   Action-at-a-

distance forces do not have to act instantaneously, and Fields do not

have to lead to finitely propagated effects.   Newton himself, and

following him the other Newtonians such as Laplace, Ampère, and Gauss,

---

1. L.Rosenfeld (1957), 'The Velocity of Light and the Evolution of
Electrodynamics', page 1641.

2. A.E.Woodruff (1962), 'Action at a Distance in Nineteenth Century
Electrodynamics', page 440.

3. W.Berkson (1974), page 3.

thought it <u>impossible</u> that there could be instantaneous action at a distance.[1] That gravity was a function of distance at all and was perhaps to be explained by exchange particles or a medium carried the mild suggestion that gravity did not act instantaneously. As Whittaker puts it :

> That gravity is propagated by the action of a medium, and consequently is a process requiring time for its accomplishment, had been an article of faith with many generations of physicists. Indeed, the dependence of the force on the distance between the attracting bodies seemed to suggest this idea; for a propagation which is truly instantaneous would, perhaps, be more naturally conceived to be effected by some kind of rigid connection between the bodies, which would be more likely to give a force independent of the mutual distance.[2]

Laplace should be mentioned here. He used a propagated gravity to solve problems connected with the moon's orbit, but with his assumptions the speed of propagation had to be 100 million times the speed of light.[3] Instantaneous propagation was not a necessary property of gravitational force. And, as a contrast to this, fields did not have to lead to finitely propagated effects -- with perfectly rigid or incompressible media the transmission of some kinds of action is instantaneous, and the notion of being compressible is not intrinsic to that of a field. Descartes, to take an example, required an infinite velocity for light and used a rigid aether to obtain it.[4] I favour saying that the question of speed of action is open in both Field and A.A.D. accounts. And, as an additional argument, the alternative characterization excludes

---

1. See, for instance, Newton's <u>Opticks</u>, Query 21.

2. Sir E.T.Whittaker (1951), <u>A History of Theories of Aether and Electricity</u>, page 207.

3. P.S.Laplace, <u>Méchanique Céleste</u>, Book X, Chap. 7, § 22.

4. And he thought that an infinite velocity was essential 'I declare to you that if this lapse of time could be observed my whole philosophy would be completely ruined', Descartes (1634), 'Letter to Beeckman'.

the views of Weber, Gauss, Riemann, and Lorenz from being part of the

A.A.D. programme, but historically the debate was between those continental

scientists and the Field theorists Faraday, Maxwell, and Thomson.

The A.A.D. heuristic consisted of the   established Newtonian

techniques, which by this time included Potential theory.   The A.A.D.

programme was well developed even before the discovery of electrodynamic

phenomena.   Poisson had used potentials to transform electrostatics

into an advanced mathematical and physical theory, and in the early 1820's

he applied these methods to magnetism; important here is that he produced

the mathematical theory of polar forces which he used in the context

of induced magnetism.[1]   George Green also used A.A.D. and potentials

to make significant contributions to theoretical electrostatics and

magnetostatics.[2]

---

1. S.D.Poisson (1812), 'Mémoire sur la Distribution de l'Electricité à
la Surface des Corps Conducteurs', (1811, published 1812), and S.D.
Poisson (1821-7), 'Mémoire sur la Théorie du Magnetisme', (1821, and 1823
(published 1827)).

2. George Green (1828), 'An Essay on the Application of Mathematical
Analysis to the Theories of Electricity and Magnetism'.   See also
M.E.J.Carr (1949), The Development of Mathematical Theories of Electricity
Prior to Maxwell with Special Reference to the Concept Potential.

3. The Philosophical Objection to A.A.D. :-

The A.A.D. programme also inherited from Newtonianism a philosophical objection. This criticism was widely voiced by Field theorists and their use of it illustrates the philosophical essentialism adopted by followers of the Field programme.

I will describe the objection in terms of A.A.D. gravitation, then answer it. The reply involves the philosophy of explanation, and to avoid digressing too far I will merely state my views and use footnotes to cite the backing arguments.

Typical variants of the objection run :

Action at a distance does not explain any phenomena because no one understands how a body can act where it is not.

Action at a distance may describe the behaviour of bodies, but it does not explain their behaviour.

Action at a distance at best tells us how bodies behave, but it does not tell us why they do so.

The first presupposition often used here is that explanation should be aimed at subjective understanding -- that explanation is relative to an individual and that it should set the mind at rest by reducing the unfamiliar to the familiar.[1] This is a mistake.[2] Explanation should be aimed at objective understanding, and this is perhaps best achieved by the hypothetico-deductive model. So that, for example, relativity theory explains the bending of light around the sun, even though many people do not understand that explanation. (One can remark here on the accidental aspect of what it

---

1. For expressions of this presupposition in electrodynamics, see Maxwell, 'On Action at a Distance', page 302, and O.Lodge (1892), Modern Views On Electricity, pages 386-7 and f. Maxwell's article also reveals that he understood most of the points that I make in this section.

2. See J.Hospers (1956), 'What is Explanation ?' page 96 and f. This excellent paper contains a clear statement of almost all the views on explanation that I wish to defend.

is that we do understand. Traditionally people 'understand' action-by-contact because they observe it all the time, but do not 'understand' action-at-a-distance, because it is not part of their daily lives. But if we had been bigger or more massy, then we would use action-at-a-distance gravitational forces every day to move objects. Should this mean that whether A.A.D. explains depends on how massy we are?)

To reformulate one variant of the criticism, taking into account the objective requirement of explanation:

> Action at a distance does not explain any phenomena because
> no explanation has been given of how a body can act where
> it is not.

What lies behind this objection is Essentialism.[1] There will always exist a finite regress of available explanations : a phenomenon to be explained, an explanation of the phenomena, an explanation of the explanation, and so on. And one can ask of each of the links in this explanatory chain 'Why?' or 'What is the explanation of that?' and of most receive in answer the explanation represented by the immediately superior link. What should happen if the question is directed at the top link? Essentialism holds that the top link must not be open to the question 'Why?' -- it must not be in need of explanation. This is achieved by the top link being a statement of essence; that is, the definition of a defining property. Aristotle's example was 'Man is rational' -- if this appears at the top of an explanatory chain one cannot sensibly ask 'why?' because any object lacking rationality could not be a man for being rational is the essential property of man. Essentialism goes further than this. It holds that all the members of an upward explanatory chain which is not terminated by a statement of essence fail to explain anything at all. Unterminated chains simply do

---

1. See the pages cited in the Index to K.R. Popper (1945). The Open Society and Its Enemies.

not explain. And this is where A.A.D. was supposed to be at fault

-- A.A.D. was never a link to a terminated chain.[1] But essentialism

is mistaken.[2] And a case can be made that even the early Newtonians

knew this. The eventual early Newtonian reply to the objection came

in 1713 from Cotes in the <u>Preface</u> to the 2nd Edition of Newton's

<u>Principia</u>:

> either <u>gravity</u> must have a place among the primary qualities
> of all bodies, or <u>extension</u>, <u>mobility</u>, and <u>impenetrability</u>
> must not.

One reading of this is that Newton and Cotes thought that Descartes's

properties of extension, mobility, and impenetrability were, contrary

to widespread belief, <u>not</u> essential -- presumably they thought that

there were no such properties as essential ones.[3]

Essentialism arose to combat the apparent lack of explanatory

progress due to an infinite regress of 'why's?'. Campbell expresses

these fears thus:

> To say that all gases expand when heated is not to explain
> why hydrogen expands when heated; it merely leads us to ask
> immediately why all gases expand. An explanation which leads
> immediately to another question of the same kind is no
> explanation at all.[4]

---

1. Nor, for that matter, was any other explanation. There was a socio-
psychological asymmetry here. Field theorists did not explain their
mediums, but this was supposed to be of no consequence; A.A.D.
theorists did not explain A.A.D., but this was taken to be a failing.

2. Hospers (1956) page 115 and f., and K.R. Popper (1945).

3. Many read Cotes otherwise, so that gravity is essential. Thus
Maxwell describes the 'dogma of Cotes'. See J.C. Maxwell (1873),
<u>A Treatise on Electricity and Magnetism</u>,§ 865.

4. N.R. Campbell (1953), <u>What is Science?</u>, page 80.

But the regress is harmless. Newton's theory of gravity answers the question why the planets behave as they do, but to ask 'why?' or 'why is it true?' of Newton's theory is to ask a new and deeper question. And so progress is made.

In an explanatory chain, each link explains its immediate predecessor. So all links, other than the bottom one, are explanations; and all links, other than the top one, have explanations. But the links are also descriptions.[1] Newton's theory of gravity describes how the gravitational forces are functions of masses and distances. The theory explains the behaviour of the planets, but does not explain the workings of gravity; and the theory describes the workings of gravity but does not describe (or does not merely describe) the behaviour of the planets. The theory tells us how gravity works and why the planets behave as they do.[2]

The A.A.D. theorists should have either rebutted the objection using arguments similar to those sketched here or shown that the objection applied equally to Field theories, which are also not part of a terminated explanatory chain, and thus that no advantage accrued over this to either programme.[3]

---

1. A typical statement that I oppose is Tricker's 'Science, however, can never provide ultimate explanations but only describe', that is; there is a dichotomy -- either ultimate or descriptive. See R.A.R. Tricker (1966) The Contributions of Faraday and Maxwell to Electrical Science, page 130.

2. Jon Dorling has made a preliminary announcement of an interesting alternative account of explanation which may well bear on the issues discussed here. See his (1978) 'On Explanations in Physics: Sketch of an Alternative to Hempel's Account of the Explanation of Laws'.

3. One indirect argument that A.A.D. theorists did come up with was that at best Field theories replace macro-A.A.D. by micro-A.A.D. Fields were often mechanical mediums and inter-molecular A.A.D. was usually adduced to explain the properties of these. This argument -- as was pointed out in the well known objection by Hare -- hits Faraday's theories head on. Faraday would not tolerate A.A.D., yet his own theories required A.A.D. over distances of about 1/2 inch. (See R. Hare (1840), 'A Letter to Prof. Faraday, on Certain Theoretical Opinions'.)

4. The Origins of Electrodynamics: Oersted and Ampère:

Electrodynamics starts in 1820 with Oersted's discovery that current electricity affects a magnetic compass. This effect was independent of both programmes. The Field programme was not yet in existence, and the A.A.D. theorists had not considered whether the newly discovered current electricity behaved any differently from static electricity which they thought Coulomb had proved could not interact with magnetic poles.[1] Oersted's discovery was a real challenge to the A.A.D. programme for, prima facie, the interaction between electricity and magnetism must involve non-central forces because, in modern terms, the lines of force circulate a current carrying wire. As Pearce Williams explains:

> Hitherto only central forces ... had been known. A circular force was both unanticipated and inexplicable. The first 'skew' force in the history of mechanics threatened to upset the whole structure of Newtonian science.[2]

I will argue in Chapter 2 that Ampère produced a central force law which, when used with his simple idea that magnets were really currents, brought the interaction entirely within A.A.D. The law, which resulted from routine application of heuristic, was:

$$F \propto G \; \frac{ids_1 \; ids_2}{d^2}$$

where G is a geometrical factor, ids the product of the current and circuit element, and d the distance between circuit elements.

---

1. See L.P. Williams (1962), 'Ampère's Electrodynamic Molecular Molecule', page 113 and f.

2. L.P. Williams (1965), Michael Faraday, page 140. And Agassi thinks the same. See J. Agassi (1968), The Continuing Revolution, page 185.

Ampère's law passed all the tests that it was subjected to, subsumed all forces between currents and magnets under the one law, and predicted, for example, that Coulomb's law of magnetism should hold between magnets. This meant that it was a major triumph for the A.A.D. programme and counted as good evidence for that view. An open problem remained : static electricity and Coulomb's law of electrostatics were outside the reduction. Even at this stage it was clear how the problem was to be solved: current elements should be analysed ending up with a theory which would yield Coulomb's law as the static case and Ampère's law as the dynamic one; magnetism, static electricity, and current electricity would then all be united under the one law. Ampère wrote, after he had found his law:

> [supposing electric molecules in motion], it is no longer
> contradictory to admit that from the actions proportional
> to the inverse square of the distance which each molecule
> exerts, there can result between two elements of conducting
> wires a force which depends not only on their distance but
> also on the directions of the two elements ... If, starting
> from this consideration, it were possible that the mutual
> action of two elements is in fact proportional to the
> formula by which I have represented it, this explanation of
> the fundamental fact of the entire theory of electrodynamic
> phenomena should evidently be preferred to any other. But
> it would require investigations for which I lack time.[1]

---

1. A.M.Ampère (1826), <u>Théorie Mathématique des Phénomènes Électrodynamiques Uniquement Déduit de L'Expérience</u>, pages 96 and f.

5. First Criticisms of A.A.D.; Faraday's Objections and His

Foundation of the Field Programme:

One apparent defeat for A.A.D. was thus turned into a victory; what about other criticisms? A major critic of the A.A.D. enterprise was Faraday. The M.S.R.P. tells us what to look for in his attack on A.A.D. for it identifies two types of criticism: of particular theories in a programme, and of the programme as a whole. Faraday offered both types. I argue in Chapter 3 that his strictures against particular A.A.D. theories were well made, but that the theories' shortcomings did not affect the whole programme, and that his attack on the programme as a whole was ineffective.

To anticipate.

There were two main examples of local criticism. Faraday refuted Grotthuss's A.A.D. theory of electrolysis -- but Grotthuss's theory was not the result of routine use of A.A.D. heuristic. Secondly, Babbage and Herschel attempted to explain Arago's disc in A.A.D. terms using induced magnetism: Faraday, after he had discovered electro-magnetic induction, explained it successfully in terms of induced currents; again, more than routine development was involved.

Global criticism can be indirect, consisting of the production of a better rival programme, or direct. Direct scientific objection normally amounts to the claim that the programme cannot solve a given key problem. To argue this soundly the critic must not only appreciate the extant theories in the programme, but also the heuristic and its strengths and weaknesses. Take an example. Newton objected to the wave programme of light on the grounds that it could never solve the problem of dispersion. Newton knew not only the wave theories but also the ploys that the wave theorists used in solving problems. And he judged that the one plus the other could not yield an explanation of

dispersion. With Faraday there were important differences. He did

not know what the A.A.D. theories really were and, as a result of

knowing no mathematics at all, had no idea of the limitations of the

heuristic. For instance, he wrote to Ampère in 1825:

> With regard to your theory, it so soon becomes mathematical
> that it quickly gets beyond my reach ...[1]

In short, he was not well qualified as a critic. Nevertheless he

offered objections and these have to be judged in their own right.

I list the major ones here and show in detail in Chapter 3 that they

are not damaging:

A   the phenomena of curved lines of force -- A.A.D. has straight

line central forces, whereas lines of magnetic or electro-

static force can be curved, hence electromagnetic phenomena

cannot be A.A.D.,[2]

B   that the forces were not independent of the medium as they

perhaps should have been given A.A.D.,[3]

C   the detailed behaviour of dielectrics -- Faraday altered

the force between two charges by putting a layered pile of

mica discs between them and also found that when the layers

were separated out the discs carried + or - charges on

their surfaces; thus, the action took place in the

medium and was not primarily concerned with the source charges

themselves.[4]

---

1. L.P. Williams (1965), page 143.

2. M. Faraday (1839) Experimental Researches in Electricity, Vol.1,
§ 1224 and also see references cited in the Index to Vol.3.

3. See, for instance, Mary Hesse (1961) pages 198 and f. and Mary Hesse
(1955) 'Action at a Distance in Classical Physics' page 342.

4. M. Faraday, Diary, Vol.III, page 72 and f.

<u>D</u> that lines of force in space were primary and existed on

their own independently of their 'sources'[1]

and <u>E</u> That A.A.D. violated the conservation of energy.[2]

Faraday's indirect criticism amounted to the foundation of the Field

programme. He proposed the thesis which became the <u>hard core</u> of the

programme: <u>electromagnetic phenomena were the outcome of behaviour</u>

<u>by the space between the (real or apparent) sources.</u> As Maxwell

described it:

> For my own part, I look for additional light on the nature of
> electricity from a study of what takes place in the space inter-
> vening between the electrified bodies. Such is the essential
> character of the mode of investigation pursued by Faraday ...[3]

At this time the heuristic of the programme was extremely weak. True,

there was the general directive 'look at the intervening space', but there

was no mathematical knowledge and no body of problem solving skills that

could be learned and passed on. Faraday once wrote:

> I do not think that I could work in company, or think aloud,
> or explain my thought at the time.[4]

As a result, he had no students, no followers, and no one thought his

theories sound. Faraday kept his theories to himself; indeed Pearce

Williams claims to be the first person since Faraday to have any real

idea what his theories were.[5] Faraday's reticence is easy to explain

-- he judged his theoretical speculations to be insufficiently

supported to be put before a wider audience. He published all the views

---

1. L.P. Williams (1965) page 204.

2. See L.P. Williams (1966) <u>The Origins of Field Theory</u> page 116.

3. J.C. Maxwell (1873) <u>A Treatise on Electricity and Magnetism</u> §37.

4. Quoted from J. Agassi (1971) <u>Faraday as a Natural Philosopher</u>
page 199.

5. L.P. Williams (1975), 'Should Philosophers be Allowed to Write
History', page 250.

that he thought would withstand critical scrutiny. To quote Pearce
Williams:

> The experimental results were clearly and firmly reported; the
> theoretical aspect was hedged with fuzzy and tentative language;
> was hesitantly and sometimes confusedly presented. It is, I
> think, fair to say that no one in the 1830's took the theory
> seriously.[1]

The Field programme has to be appraised by the discoveries which it leads
to. Faraday made five major discoveries -- electromagnetic induction,
the laws of electrolysis, dielectrics, the rotation of the plane of
polarization of light by a magnetic field, and diamagnetism -- but none
of these was a credit to the Field programme. Faraday searched for
correlations of forces, and his discoveries were loosely connected with
that pursuit. This means that, if anything, the discoveries were
evidence for the thesis that all forces are correlated. The other idea
-- that the space plays a key role in all electrodynamic phenomena --
was proposed only at the end of his career after all the discoveries
had been made.

---

1. L.P. Williams, 'Faraday' article, Dictionary of Scientific Biography,
page 537.

6. The Rival Views on the Sources and Receivers of Force:

Some advanced warning should be given of the difficulties
to be faced over sources and receivers of force.

The A.A.D. view is crystal clear. In contrast the Field view
is a morass -- so much so that many experts regard it as intrinsically
inconsistent and beyond comprehension. Duhem tells us that Maxwell's
theories are:

> compromised by contradictions which are not contingent ...
> but essential and inseparable from the totality of the work.[1]

And O'Rahilly writes:

> Never did a great physicist throw out such a mass of incoherent
> ideas, calmly pursuing his course with intuitive genius amid
> a welter of discrepant theories.[2,3]

For electrostatics, the A.A.D. theories tell us that there
are source and receiver electrical fluids[4] which act at a distance on
each other so that with no medium the circumstances may be depicted:

A: (the vacuum case)   (empty space)

If there is a medium present then the Poisson-Mossotti-Thompson analysis

---

1. P. Duhem (1902), Les Théories Electriques de J.C. Maxwell, page 223.

2. O'Rahilly (1965), page 79.

3. There are many passages similar to these for example, Ehrenfest's
'kind of intellectual primeval forest, almost impenetrable in its
fecundity' or Boltzmann's 'So soll ich den mit saurem Schweiss, Euch
lehren, was ich selbst nicht weiss.' -- see also H. Hertz (1892),
Electric Waves, Introduction, H. Poincaré (1901), Electricité et Optique,
viii, J.J. Thomson (1885), 'Report on Electrical Theories', page 125 and
O. Heaviside (1893), Electromagnetic Theory, Preface.

4. These electrical fluids were generally considered to be atomic in
constitution; that is, the sources and receivers of force were taken
to be positive or negative 'electrons'.

of polar forces and dielectrics applies[1] -- the bounding fluids

attract and repel fluids in the medium with the result that the medium

becomes polarized and shows polarization fluids on its boundary

surface, thus:

B: (the dielectric case)
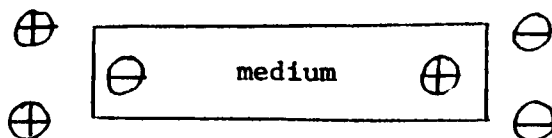
There is here free charge and polarization charge, and the positive

fluids in the medium move away from the positive free charge so that

the polarization current[2] or displacement is in the diagram from left

to right:

C: (the dielectric case)
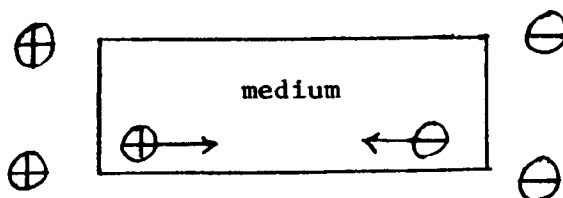
It is the free source fluids which cause the polarization and

polarization current in the medium.

For electrostatics, the Field view denied that there were

electrical fluids acting at a distance.[3] There were to be no electrical

fluids and no action at a distance.. Instead the medium was all

pervasive, and it was occasionally under stress and this resulted in

polarization. As Maxwell puts it:

---

1. Poisson (1821-7), F.O. Mossotti (1847) in Archives de Sciences Physiques et Naturelles, 6, (1847), page 193, and W. Thomson (1845), 'On the Mathematical Theory of Electricity in Equilibrium'.

2. I use the direction of travel of the positive fluids to identify the direction of the polarization current.

3. For a more detailed account of the Field views, see J. Bromberg (1968), 'Maxwell's Electrostatics', P.M. Heimann (1971), 'Maxwell, Hertz, and the Nature of Electricity', and A. Chalmers (1971), Chapter 4.

> ... we must regard electrification as a property of the dielectric medium rather than of the conductor which is bounded by it.[1]

Or again:

> The charge therefore at the bounding surface of a conductor and the surrounding dielectric, which on the old theory was called the charge of the conductor, must be called in the theory of induction the surface-charge of the surrounding dielectric. According to this theory, all charge is the residual effect of the polarization of the dielectric.[2]

Typically:

A': (the 'vacuum' case)
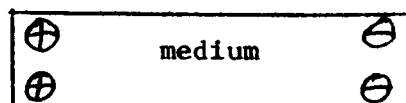
B': (the dielectric case)
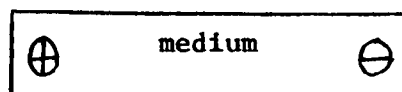
There is here only polarization charge, and the positive apparent charge in the medium moves towards the boundary where it is manifested so that the 'polarization current' in the diagram is from right to left:

C': (the dielectric case)
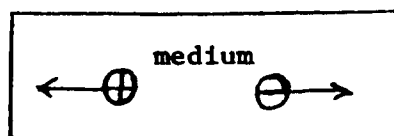
There is one minor variation that should be mentioned. The Field theorists discussed polarization as a mechanical stress, and they also discussed lines of force which mapped this stress. Faraday and Maxwell vacillated over the question of whether it was the polarization that was primary or whether it was the lines of force -- Faraday eventually favoured the lines of force, and Maxwell eventually favoured

---

1. Maxwell (1881), An Elementary Treatise on Electricity, §62.

2. Maxwell (1873), §111.

mechanical stress.[1] Under either view there were to be no genuine

sources. A preliminary difficulty concerns the causes of the stress or

apparent charge. A rubber band will become stretched and remain so

only if there are forces to hold it out. Similarly polarization or

line of force stresses seem to require forces to implement them. What

are these forces? No satisfactory answer was given. One possibility

would be to take the line of force as a primitive notion -- Faraday

did this; but Maxwell favoured mechanical stresses and taking them as

primitive would have run up against Newton's third law.[2] Maxwell

describes this:

> It must be carefully borne in mind that we have made only
> one step in the theory of the action of the medium. We have
> supposed it to be in a state of stress, but we have not in
> any way accounted for this stress, or explained how it is
> maintained ...
>   I have not been able ... to account by mechanical
> considerations for these stresses in the dielectric. I
> therefore leave the theory at this point ...[3]

I think that it was the desire for a unified view that lay

behind the Field approach to sources. If there are several non-inter-

acting substances, then there is the seemingly paradoxical problem of

how they interact. Source fluids interact under Coulomb's law;

masses interact under Newton's law, but masses presumably do not inter-

act with source fluids. How then do bodies retain the fluids to become

charged? A unified Field view avoids this problem. I do not know of

---

1. See P.M. Heimann (1970), 'Maxwell and the Modes of Consistent
Representation' for a further discussion of this topic.

2. C.W.F. Everitt, the recognized authority on Maxwell's views, argues
that for Maxwell electric force is primitive (see his (1975), James
Clerk Maxwell, page 124 and f.). I agree entirely that such an inter-
pretation makes sense of Maxwell's views, but I think that Maxwell's
mechanical essentialism made this interpretation unavailable to him.

3. Maxwell (1873) §110-111.

the Field theorists using this argument.[1] Instead they conjured up

weaker ones. Maxwell's favourite was the conservation of substance

together with the cancelling out of electrical fluids. The optical

phenomenonof interference was to him incontrovertible proof that light

was not a substance:

> We cannot suppose that two bodies when put together can
> annihilate each other; therefore light cannot be a
> substance.[2]

In a similar vein he argued:

> it is difficult to conceive how the combination of the
> two fluids can have no properties at all.[3]

(But having zero resultant effect does not mean that the causes them-

selves add to non existence; there is zero resultant gravitational

force at the midpoint between two equal masses.) Maxwell and Faraday's

other plea was that electrical fluids had not been proved by experiment

to exist -- an objection that applied universally to theories, including

their own ones of stresses and lines of force.

The A.A.D. account was always perfectly satisfactory. And

this is where the difficulties originate. The Field theorists accepted

the A.A.D. view of dielectrics as a mathematical and phenomenological

description. Indeed it was the Field theorist Thomson who brought the

theory to the attention of British scientists:

> Poisson has investigated the mathematical laws of [magnetic
> polarity]. These laws seem to represent in the most general
> manner the state of the body polarized by influence; and
> therefore ... we may make use of them to form a mathematical
> theory of electrical influence in dielectrics the truth of
> which can only be established by a rigorous comparison of its
> results with experiment.[4]

---

1. The problem -- of the possible interactions of 'imponderable'
fluids -- was considered mainly by A.A.D. scientists. And, by the way,
was solved by them for electromechanical interaction by means of the electron.

2. Scientific Papers, II, page 764.

3. Maxwell (1873) §36. See also, for instance, §63.

4. W. Thomson (1845), page 55.

And Maxwell says of the Poisson-Mossotti theory that it:

'may well be true.'[1]

But rethinking the Poisson-Mossotti-Thomson analysis in terms of 'apparent charge' as an epiphenomenon of stress led to immense confusions. These confusions are most marked over questions of the directions of the 'polarization' or 'displacement' or 'polarization currents' or 'displacement currents'. This is why Maxwell made many inconsistent uses of signs. And he ends up with such puzzles as 'all electricity results from polarization' (i.e. div $\underline{P} \neq 0$), yet 'polarization is the motion of electricity which behaves like an incompressible fluid' (i.e. div $\underline{P} = 0$).

For magnetism, the early A.A.D. view was that there were source and receiver magnetic fluids which act at a distance on each other, and this idea was complemented with the Poisson account of induced magnetism.[2] Then Ampère substituted circulating microcurrents and current shells for magnetic fluids so that the later A.A.D. view was that magnetic fluids could be used as a convenient representation, but really only electricity was needed to explain electromagnetic behaviour. The A.A.D. account here fared reasonably well. The magnetic vagaries of materials posed problems, but the Ampère-Weber tradition provided better explanations than any rival of diamagnetism and similar oddities.

---

1. Maxwell (1873), §62.

2. The Poisson theory of polar forces was first applied to magnetism then, as Maxwell once remarked, Mossotti translated French into Italian and magnetism into electricity to obtain the (Poisson)-Mossotti theory of dielectrics.

For magnetism, the Field theorists generally discussed magnetic poles acting at a distance, and adopted the Poisson theory of induced magnetism. I think they wished to reach an acceptable view of apparent electrical sources before attempting to give a similar account of apparent magnetic sources.

Current electricity was discovered at the beginning of the nineteen century and initially there were two theories as to its nature: that it was the flow of electrical fluid, and that it was a vibration or oscillation in the wire.

The A.A.D. scientists took the view first that current was the flow of electrical fluids, then that current was the flow of atoms of these electrical fluids, and finally with Weber that current was the flow of inertial atoms of electrical fluids.

The Field theorists remained silent as to the nature of currents. Again, phenomenologically the A.A.D. account was perfectly satisfactory -- it explained Faraday's results that galvanic electricity and flow of initially static electricity produce exactly the same effects, and much more besides.[1] But since the Field theorists denied that there were electrical fluids, they could hardly accept that such fluids flowed in wires. Instead they issued warnings about what had not been proved by experiment. Faraday writes:

> There are many arguments in favour of the materiality of electricity and but few against it; but still it is only a supposition; and it will be as well to remember, while

---

1. For instance, it explained electrolysis, Ohm's law, convection current effects, and the Hall effect.

pursuing the subject of electro-magnetism, that we have no proof of the materiality of electricity, or of the existence of any current through the wire.[1]

And Maxwell warns:

Electricity, Fluids, and Heat all tend to pass from one place to another ... A fluid is certainly a substance heat is as certainly not a substance ... we must be careful not to let the one or the other analogy suggest to us that electricity is either a substance like water, or a state of agitation like heat.[2]

_____

1. Faraday (1821), 'Historical Sketch of Electromagnetism', page 196.

2. Maxwell (1873), §72. See also §574, §243, §244, and §355.

7. The Early Development of A.A.D.: Weber's Unification of Electro-
   dynamics and Other Theoretical Advances:

By 1930, the A.A.D. programme contained three prominent

theses:

1. That static electricity was governed entirely by Coulomb's
   A.A.D. Law.

2. That, in a manner of speaking, there was no such thing as
   magnets or magnetism; instead there were currents which
   produced the effects.

3. That current electricity (and thus magnetism) was governed
   entirely by Ampère's A.A.D. law.

During the following twenty years Weber and his colleagues

developed these three into five replacement theses:

1'. That electricity is atomic in structure.

2'. That currents are streams of electrical 'atoms'.

3'. That the forces acting operate directly between electrical
    atoms and not between conductors.

4'. That the forces acting do not do so instantaneously.

5'. That all electrodynamic phenomena (that is, all forces,
    inductions, etc.) may be deduced, by statistical summation,
    from an A.A.D. force formula for electrical atoms.

These developments -- due to Ohm, Fechner, Kirchoff, Gauss,

Riemann, and Weber -- are considered in Chapter 4.

Electrodynamic induction was the major problem facing electro-

dynamics in the early 1830's. It was a new problem and was not

anticipated by either of the two Programmes. But the A.A.D. programme

still had the unfinished task of unifying static and current electricity

by analysing current elements. Weber completed this by deducing a

force law from Ampère's law and a reasonable theory of currents. The

law was:

$$F = \frac{q_1 q_2}{r^2} \left[1 - \frac{1}{c^2} \left[\left(\frac{dr}{dt}\right)^2 - 2r \frac{d_2 r}{dt^2} \right] \right]$$

where r is the distance between charges, c is a constant of propor-
tionality, and q_1 and q_2 are the charge magnitudes. Weber's force law
not only superseded Ampère's and Coulomb's laws but also it predicted
all the various forms of electrodynamic induction. Thus an A.A.D. theory
explained induction, and the explanation was produced independently of
Faraday's discovery of induction -- had Faraday not discovered induction,
A.A.D. would still have predicted it theoretically.[1] Weber's law, when
proposed in 1846, accounted for all known electrodynamic phenomena.

Far from being well received, Weber's law was subjected to
severe criticism which apparently damned it for once and for all.
Helmholtz stated that the law violated the conservation of energy, and
this allegation was used by Field theorists (and many historians) as a
fatal criticism of A.A.D. electrodynamics. Here we see aspects of the
harshness of A.A.D.'s fate. For not only was Helmholtz mistaken, but
also his arguments were transparently fallacious.

The other replacement theses fared rather better. The analysis
1'-3' predicted the Hall effect,[2] and the outcome of Rowland's
convection current experiments.

4' -- that the forces should be retarded -- was first
advocated in earnest by Gauss in 1835. Gauss made progress but did

---

1. The priority of discovery here is of no significance, since none
of the theories predicted the effect. In fact, Ampère himself nearly
discovered induction (see S.P. Thompson, Phil Mag., 1895): and also a
case can be made that Henry was an independent and simultaneous
discoverer of it, and Henry was an A.A.D. theorist. (Henry certainly
discovered self-induction.)

2. It anticipated a Hall-type effect – the exact effect depends on
whether it is the positive electrical fluids that move in the
conductor or the negative or both. In contrast, the Field programme
made no predictions regarding this kind of phenomenon.

did not solve the problems involved. 4' yielded A.A.D.'s next problem

for Weber's force law used instantaneous transmission and thus was

inadequate. Yet another force law was needed, using retarded forces,

from which Weber's law, or an approximation to it, could be derived.

Riemann made a brave but unsuccessful attempt to solve this in 1857.

8. The Growth of the Field Heuristic 1840-60:

Maxwell and Thomson constructed the Field heuristic out of three components: the re-interpretation of A.A.D. mathematics as Field mathematics, the search for a single mechanical aether, and the use of mechanical models and analogies.[1]

The first constituent of the Field heuristic is what we would now call vector analysis.

Most of the essential results concerning potential fields and force fields had by this time been derived by Gauss, Poisson, Laplace, Green, and other A.A.D. theoreticians. The potentials and forces were written both as functions of the sources and in terms of partial derivatives as functions of the local fields. The two approaches were interchangeable for most mathematical purposes, but physically the source or cause of the fields was in both cases the distant electrical fluids.

Thomson, and later Maxwell, made discoveries, re-discoveries, and re-interpretations concerning this localized mathematics.[2] The distant source fluids were quietly forgotten, and the local mathematics in terms of partial differential equations were taken as the true expression of Faraday's intuitions that electrodynamics was concerned essentially with the behaviour of the space. Thus Maxwell writes:

---

1. See for additional background J. Turner (1955a), 'Maxwell on the Method of Physical Analogy'; J. Turner (1955b), 'A Note on Maxwell's Interpretation of Some Attempts at Dynamical Explanation'; J. Turner (1956), 'Maxwell on the Logic of Dynamical Explanation'; and Jed. Z. Buchwald (1977), 'William Thomson and the Mathematization of Faraday's Electrostatics'.

2. See W. Thomson (1872) Papers on Electrostatics and Magnetism, pages 1, 15, 42, 52, 340; W. Thomson (1882) Mathematical and Physical Papers page 76; and Maxwell's view of Thomson's (1872) in J.C. Maxwell Collected Papers, II, pages 301-7. Thomson was to find out that George Green had anticipated many of his results.

... Faraday, in his mind's eye, saw lines of force tra-
versing all space where the mathematicians saw centres of
force attracting at a distance: Faraday saw a medium where
they saw nothing but distance: Faraday sought the seat of
the phenomena in real actions going on in the medium, they
were satisfied that they had found it in a power of action at
a distance impressed on the electrical fluids.

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.

I also found that several of the most fertile methods of
research discovered by the mathematicians could be expressed
much better in terms of ideas derived from Faraday than in
their original form.

The whole theory, for instance, of the potential,
considered as a quantity which satisfies a certain partial
differential equation, belongs essentially to the method
which I have called that of Faraday. According to the other
method, the potential, if it is to be considered at all,
must be regarded as the result of a summation of the electrified
particles divided each by its distance from a given point.
Hence many of the mathematical discoveries of Laplace, Poisson,
Green and Gauss find their proper place in this treatise, and
their appropriate expressions in terms of conceptions mainly
derived from Faraday.[1]

(I cite this passage only as evidence of the aims and methods of the

Field programme, I certainly do not wish to defend some of the theses

expressed in it — for instance, it is clear that the A.A.D. theoret-

icians would have disputed Maxwell's claim that the _true expression_ of

their ideas was the Faraday one.)[2]

---

1. J.C. Maxwell (1873), page ix.

2. Notice the inconsistency in the Field theorists' interpretation of
mathematics. Electric currents and heat flow were governed by similar
equations -- the Field theorists warned against concluding from this
that electric current was a _thing_ that flowed. The electric field and
heat flow were governed, as the young Thomson showed, by similar
Laplacian equations - the Field theorists concluded from this that
the electric field was a _thing_ going on in the apparently empty space.

The second constituent -- the search for a mechanical aether -- represents an addition that Thomson and Maxwell made to Faraday's original venture.

A criticism that Faraday made of A.A.D. was the standard philosophical one that matter cannot act where it is not, and this type of objection applied to gravity and electromagnetism.[1] Faraday quoted with approval the Third letter of Newton to Bentley in which it is denied that gravity is essential and asserted that there must be mutual contact between the distant matter.[2] As we have seen, Faraday's eventual suggestions here involved non-mechanical unified fields of force. Initially he regarded the electromagnetic space as being stressed to produce polarization, and it is important to note that with this view a wire about to have current induced in it was in a special state of stress the 'electrotonic state'. This account is mechanical and thus involved, in an informal way, an aether. He later took forces and lines of force to be primary and then induction became the cutting of lines of force or the change in the flux threading a circuit. Under this view there is no aether -- there is only a field of force.

Maxwell favoured Faraday's _first_ view.[3] Basically he thought that mathematically lines of force may describe the phenomena including induction, but that the real explanations would have to be in terms of mechanics:

---

1. See also Section 3 above.

2. M. Faraday, Experimental Researches, III, §532n., §571.

3. Maxwell changed his mind several times. This complex issue is discussed in P.M. Heimann (1970), 'Maxwell and the Modes of Consistent Representation'.

When any phenomenon can be described as an example of some
general principle which is applicable to other phenomena, that
phenomenon is said to be explained. Explanations, however, are
of very various orders, according to the degree of generality
of the principle which is made use of ... when a physical
phenomenon can be completely described as a change in the
configuration and motion of a material system, the dynamical
explanation of that phenomenon is said to be complete. We
cannot conceive any further explanation to be either necessary,
desirable, or possible, for as soon as we know what is meant
by words configuration, motion, mass, and force, we see that
the ideas which they represent are so elementary that they
cannot be explained by means of anything else.[1]

This later was to mean that even though he was content to describe

Faraday's 'electronic state' in terms of a vector potential he still

wished to explain it mechanically. Maxwell's mechanical essentialism

has the internal difficult that often one branch of mechanics is

explained in terms of another; thus it needs supplementing by the

identification of the ultimate mechanical explanations. In particular

Maxwell was puzzled over the question of gravitational A.A.D. He too

quotes with approval Newton's letter to Bentley,[2] and he also tried on

occasions to introduce a mechanical medium to explain gravitational

A.A.D. On the other hand he must have known that A.A.D. was part of

Newtonian mechanics and thus may not be in need of explanation.[3]

And the attempts he made to explain gravity ran into severe difficulties.

He writes, for example:

---

1. Scientific Papers, II, page 418. Maxwell uses the word 'dynamics'
for 'mechanics'. See also II, page 592. The quoted passage also
appears in the draft of his (1864), 'A Dynamical Theory of the Electro-
magnetic Field' -- see University Library Cambridge Add. MSS §7655 'On
the Dynamical Explanation of Electric Phenomena'.

2. Scientific Papers, II, page 316.

3. Chalmers makes a mistake here in his (1971), page 47. He quotes
Maxwell's presentation of Maxwell's opponents views as if it were
Maxwell's own view.

the assumption, therefore, that gravitation arises from the action of the surrounding medium in the way pointed out, leads to the conclusion that every part of this medium possesses, when undisturbed, an enormous intrinsic energy, and that the presence of dense bodies influences the medium so as to diminish this energy whenever there is a resultant attraction. [!]

As I am unable to understand in what way a medium can possess such properties, I cannot go any further in this direction searching for the cause of gravitation.[1]

The troubles over the constitution of the mechanically ultimate did not impinge on electromagnetism - the preliminary step for electromagnetism was to explain it in terms of any branch of Newtonian mechanics.[2]

Thomson and Maxwell thought that the finite velocity of light and heat, and the transversality of light waves should be explained by means of a mechanical aether possibly involving rotational or vortex elements.[3] The vortices were introduced because magnetism appeared to be genuinely rotational in character -- as is evinced by Oersted's results, and the magnetic rotation of the plane of polarization of light -- and consequently Thomson postulated vortex mechanical mediums.[4] I suggest that they always considered that one aether would be

---

1. Maxwell (1864), 'A Dynamical Theory of the Electromagnetic Field', §82.

2. Mechanical essentialism was the dominant thought in 19th Century science. As Hertz put it, 'all physicists agree that the problem of physics consists in tracing the phenomena of nature back to the simple laws of mechanics', H. Hertz (1899), The Principles of Mechanics, author's preface.

3. See Maxwell (1877), Matter and Motion §108 for the proof of the existence of aether from the finite velocity of light. See also Maxwell (1864), Sections 5 and f., for the objective reasons behind the postulation of aether.

4. Thomson's and Helmholtz's mathematical results suggested that lines and vortices are duals so that either electricity was a line phenomenon and magnetism a rotational one or vice-versa. That electrolysis appeared to be in a straight line, and the plane of polarization of light appeared to rotate absolutely motivated the former choice.

sufficient for light, radiant heat, electricity, magnetism and gravity.

Faraday writes:

> it is not at all unlikely that, if there be an aether,
> it should have other uses than simply the conveyance of
> [light] radiations.[1]

And the developing background knowledge -- such as that on the interaction between magnetism and light -- supported this idea. Thomson writes to Faraday:

> I enclose the paper ... giving an analogy for electric and
> magnetic forces, by means of the strain propagated through
> an elastic solid. What I have written is merely a sketch
> of the mathematical analogy. I did not venture even to
> hint at the possibility of making it the foundation of a
> physical theory of the propagation of electric and magnetic
> forces, which, if established at all, would express as a
> necessary result, the connection between electrical and
> magnetic forces, and would show how the purely statical
> phenomenon even of magnetism may originate either from
> electricity in motion, or from an inert mass such as a
> magnet. If such a theory could be discovered, it would
> also, when taken in connection with the undulatory theory
> of light, in all probability explain the effect of magnetism
> on polarized light.[2]

And Maxwell thought that the task was to discover the exact properties of this aether.[3]

The third constituent was the postulation of mechanical models and analogies.

There appear to have been several ideas behind this. One role it had was that of an existence or consistency proof -- a mechanical model demonstrated that a particular process could be mechanical. (Then the problem became to show that it was mechanical

---

1. Experimental Researches, III, page 330.

2. In S.P. Thompson (1910), The Life of William Thomson, I, page 203.

3. For detailed argument, see A.F. Chalmers, (1973a), 'Maxwell's Methodology and his Application of It to Electromagnetism', Section II, page 154 and f.

and to identify the unique mechanism associated with it.) Another

purpose models had was that of a mathematical or physical heuristic.

Technical skill in Newtonian mechanics could be transferred to other

domains by means of the models, and the British scientists seemed to be

more adept at mechanics than at other branches of mathematics.[1]

Maxwell writes:

> We must retranslate [symbols] into the language of dynamics.
> In this way our words will call up the mental image, not of
> certain operations of the calculus, but of certain character-
> istics of the motion of bodies.[2]

The models were intended also to be a physical heuristic. It was thought

that the failing of standard approaches was that scientists advocated

theories they hoped were true -- and this meant that physicists were

constrained by their pet theories or prejudices. Maxwell writes:

> If we ... adopt a physical hypothesis, we see the phenomena
> through a medium, and we are liable to that blindness to facts
> and rashness in assumption which a partial explanation
> encourages. We must therefore discover some method if invest-
> igation which allows the mind at every step to lay hold of a
> clear physical conception, without being committed ...[3]

Models were to be the means of liberation -- a scientist was to be

free to propose any model merely to see if the properties it possessed

suggested unsuspected physical properties in the archetype. A

---

1. Duhem's well known joke that the British scientists lead us not into
the tranquil abode of reason but into a factory is not that far wide of
the mark. There are external factors here. It was the England of the
Industrial Revolution and many scientists -- Rankine, for example --
were engineers for whom mechanics had pride of place.

2. Scientific Papers, II, page 308. See also the opening few para-
graphs of his (1856), 'On Faraday's Line of Force'.

3. Maxwell (1856), page 155.

criticism here is that if a process has a modelling mechanism, then
-- as Maxwell knew[1] -- it will have an infinite number of mechanisms;[2]
consequently the physically suggestive role of the mechanism is _prima_
facie no better than that of arbitrarily adding an unknown property
to the original phenomena.[3]

---

1. See Maxwell (1873), §831.

2. This may be seen if the system is considered in terms of Lagrangian
mechanics.

3. We may set this difficulty aside if we admit that Maxwell and Thomson
were 'committed' in so far as they held that some of their models --
though perhaps false -- had verisimilitude and thus could be a guide as
to the nature of the world. This admission, though, puts Maxwell and
Thomson back among the ordinary scientists who propose theories that they
hope will be something like the world.

9. The Field Programme to 1860 :-

The research of Thomson and Maxwell prior to 1860 predicted
no new results, and thus the Field programme was degenerating during
this time.

It is important to stress this. Thomson and Maxwell worked
to show that the Field programme could offer mathematical derivations
leading to descriptions of phenomena discovered by the rival A.A.D.
programme. And they argued that success in this task would mean
that the two programmes had equal merit. Most historians agree, and so
direct their attention to the question of whether Maxwell and Thomson
did succeed. But the M.S.R.P. offers an improved system of appraisal,
under it only new predictions count. Consequently my concern is
with whether Maxwell and Thomson produced new results, not with whether
they re-cast old ones.

The papers of Thomson relevant to the Field programme are
those concerned with its heuristic, discussed in the last section, and
those concerned with energy and energy density. Thomson made a suggestion,
later adopted by Maxwell, to locate the energy of electromagnetic
interactions throughout space.[1] The choice here appears to be letting
the energy arise from the configuration of the sources, or letting it
be distributed throughout space.[2] For example, the potential energy of
a pendulum bob above the surface of the earth could be interpreted as being

---

1. See W.Thomson (1882), Mathematical and Physical Papers, 1, page 447.
This is the origin of one common modern method of using all space integrals
of E^2 and B^2 to calculate energy.

2. See A.Shadowitz (1975), The Electromagnetic Field, page 192 and f.

due to the configuration of the bob and the earth and thus have no
location or it could be interpreted as having a location in the
gravitational field.   Field theorists favoured distributing the energy
as it seemed that this would require a medium as the store of the
energy; and also the alternative meant sources and configurations of
sources, which they were trying to avoid.   There were two physical
arguments against the Field approach.   Distributing energy should work
also for gravity but, since the energy of a medium would have to be
positive definite, absurd results are obtained -- for example, that
in regions where there are no masses there is infinite gravitational
potential energy.[1]   The other argument -- not so strong I feel -- is
that potential energy is about differences in energy, yet a distribution
involves an absolute value.   Mathematically, if the energy does
arise from the configuration of the sources then it can be transformed
into a surface or volume integral of an energy density.[2]   Thomson's
interpretation needs independent evidence in its favour, none was
forthcoming in the period before 1860.

One early paper of Maxwell's should be discussed -- his
(1856) 'On Faraday's Lines of Force'.   He proves in this, by means

---

1. See Maxwell (1864), § 82.   This difficulty with gravitation defeats
Faraday's intuition on local energy, mentioned in my Chapter 1 Section 5
and explained in Chapter 3, which was later expressed by Maxwell thus :
    We are dissatisfied with the explanation founded on the hypothesis
    of attractive and repellent forces directed towards the magnetic
    poles, even though we may have satisfied ourselves that the
    phenomenon is in strict accordance with that hypothesis, and we
    cannot help thinking that in every place where we find these
    lines of force, some physical state or action must exist in
    sufficient energy to produce the actual phenomenon.
(See his (1862), 'On Physical Lines of Force' page 452.)

2. See R.P.Feynmann (1964), Lectures on Physics, II, 8.5.

of a hydrodynamical analogue :

> a ... system of propositions ... which is in itself
> a collection of purely geometrical truths ...[1]

He also introduces the vector potential as an analytic measure of

Faraday's electrotonic state; however, as he himself writes, this

mathematical description does not constitute an explanation :

> I do not think that it contains even the shadow of a
> true physical theory; in fact, its chief merit as
> a temporary instrument of research is that it does not,
> even in appearance, _account for_ anything.[2]

There are no discoveries in this paper.

---

1. Maxwell (1855), _Letter to William Thomson_, page 17.

2. Maxwell (1856), 'On Faraday's Lines of Force', page 207.   His italics.

10. An Appraisal of the Two Programmes Until 1860 :

The Field programme had no evidence in its favour before
1860. No electrical or magnetic effect could even be shown to be
a consequence of the thesis that the medium was the seat of electromagnetic
behaviour let alone was predicted from this base. There are two
possible exceptions to my claim. There was a period of twenty years
during which some scientists argued that the existence of dielectrics
was evidence for a medium -- this ran from the time that the Field
theorists Faraday and Snow Harris stated *that* dielectrics defied Coulomb's
law until the Field theorist Thomson showed that they did not. Throughout
this time the A.A.D. programme was able to explain dielectrics.[1]
Electromagnetic induction also seems to merit further discussion. The
Field explanations developed in two stages. Faraday described induction
in terms of flux-cutting and flux-threading -- these descriptions were
unsatisfactory, as I shall show in Chapter 3. Then Maxwell described
induction in terms of the rate of change of a vector potential. Does
this vector potential description constitute a Field explanation of
induction ? Maxwell says not.[2] And, for other reasons, I too say not.
An analogue is this. Say the problem is to explain the observations of
an orbiting satellite, then Newton's theory explains that the orbit
should be an ellipse, and an elliptic path 'describes' or 'weakly
explains' the observations. The last qualification arises because

---

1. See my Chapter 1, Section 3.

2. In the passage quoted a few pages ago -- Maxwell (1856), page 207.

our observations, though multitudinous, are finite whereas ellipses

are continuous curves of continuum many points; thus strictly speaking

the ellipse hypothesis does explain the observations in that it both

has the observations as consequences and is independently testable.

But once Newton's theory has been proposed it seems more appropriate

to treat the ellipse hypothesis as a description of a general fact

which is in turn explained by Newton's theory.   With electromagnetic

induction the sequence was as follows.   In 1835 Gauss showed that

induction could be described mathematically in terms of the rate of

change of a vector potential; in 1845 F.E.Neumann published this result;

in 1846 Weber's force law had been shown to explain induction and to

predict that a vector potential should describe the effect; and in the

early 1850's Weber's derivations had been published in English.[1]   In

1856 Maxwell'discovered' that a vector potential could be used to

describe induction.   My view is that either Maxwell's vector potential

theory is not an explanation, or it is an A.A.D. explanation.

In contrast with the Field programme, the A.A.D. view of

no medium and distant sources could satisfactorily explain all electromagnetic

phenomena known before 1860.

_____

1. These results are discussed further in Chapter 4.

11.Maxwell's Theories of Electromagnetism :

By 1860 the Field programme had acquired the ability to solve problems. But did it ever surpass its rival ? The key issue here is the electromagnetic theory of light. Most historians would regard the thesis that the A.A.D. programme was the better one as unusual, but probably they would admit that the thesis was sound until 1860. But they would qualify their admission. Surely, they would add, the electromagnetic theory of light, developed during the 1860's, gave the Field programme its decisive victory ?

I think not, and I argue the point in Chapters 5 and 6.

It was Maxwell who developed the Field theories of light and he did so in two stages : the 'early' theory of 'On Physical Lines of Force'[1], and the 'later' theory of 'A Dynamical Theory of the Electromagnetic Field'[2] and subsequent publications.

The early theory sets out to solve the problem of electromagnetic induction using an extremely natural mechanical model which filled the intervening space with a mechanism. This model apparently has the independent and unexpected consequence that it supports transverse waves and further these waves travel at the speed of light, so that :

> we can scarcely avoid the inference that <u>light consists in</u>
> <u>the transverse undulations of the same medium which is the</u>
> <u>cause of electric and magnetic phenomena.</u>

Thus the model seems to solve the outstanding problem of the Field programme while predicting a rudimentary electromagnetic theory of light. It also

---

1. Maxwell (1862), 'On Physical Lines of Force'.

2. Maxwell (1865), 'A Dynamical Theory of the Electromagnetic Field'.

3. Maxwell (1862), page 500, his italics.

seems to link electrical and optical properties by predicting that in dielectrics the refractive index is proportional to the square root of the dielectric constant.

In reality, though, the model is _ad hoc_ and _heuristically_ _ad_ _hoc_. It has an open texture and indeterminateness with regard to the values of the crucial parameters, and is _heuristically_ _ad_ _hoc_ in its shift from being a hydrodynamical model to being an elastic solid model (the latter being needed to support a transverse wave). As it stood, with the parameters settled upon by Maxwell, it predicted that the velocity of a transverse disturbance should be $\frac{1}{\sqrt{2}}$ times the velocity of light. The model was refuted by the actual velocity of light, not confirmed by it. And the other 'predicted' relationship -- between refractive index and dielectric constant -- also failed experimentally.

The later theory consists of purely electrical postulates and has as its main feature a derivation that there should exist transverse electromagnetic waves in dielectrics. The derivation is then coupled with a key thesis of the Faraday-Maxwell tradition -- that the vacuum is a dielectric -- to reach the conclusion that there should be transverse electromagnetic waves in a vacuum. Light was suggested to be such a wave. Then the later theory is interpreted in terms of the Lagrangian methods of analytical mechanics, and this interpretation is taken to signify that the postulates describe a mechanical aether.

The later theory was _heuristically_ _ad_ _hoc_. Its origins lay with purely electrical arguments based on A.A.D. background knowledge and not with the Field programme's heuristic. The use of Lagrangian

mechanics did not improve the pedigree of the theory. The later
theory did make predictions. Many of these were consequences of
background knowledge, and the other novel predictions -- generally
about the vacuum -- were either untried or unsuccessful. The later
theory made no successful novel predictions within decades of its
proposal in 1865.

The early and later theories are components of a degenerating
research programme.

_____

12. The A.A.D. Theory of Light and An Appraisal of the Two Programmes from 1860 - 1900 :

The A.A.D. programme also had a theory of light -- one that postulated retarded scalar and vector potentials emanating from electron sources.

The major theoretical problem facing the A.A.D. programme from 1840 on was that of modifying the instantaneous force laws so that electrical forces took time to spread through space. An answer to this came in 1867 from Ludwig Lorenz who proposed a retarded force conservative generalization of the A.A.D. electrodynamic equations.[1]   In his theory the scalar potential ϕ and the vector potential $\underline{A}$ propagate at the speed of light.   The theory is conservative in the sense that it does not contradict the established experimental base of quasi-stationary phenomena; but Lorenz was not seeking merely to generalize the A.A.D. equations, he was also searching for a theory of light.

He had earlier put forward a desideratum for a theory of light in the form of a differential equation to be satisfied by the light vector $\underline{u}$.   In Lorenz's electrodynamic system Ohm's law is stated as the current density vector $\underline{j}$ being equal to the product of the electric vector $\underline{E}$ and the reciprocal of the resistance; and $\underline{E}$ satisfies the desideratum; but $\underline{j}$ does not, although it nearly does so.   For reasons to be explained in Chapter 6, Lorenz identified the light vector with the current density

---

1. L.Lorenz (1867), 'On the Identity of the Vibrations of Light and Electrical Currents'.

vector $\underline{j}$. He writes :

'the vibrations of light are themselves electrical
currents'[1]

This identification is incorrect from his own point of view, and

further the interpretation it leads him to is in direct conflict

with the A.A.D. programme. He then argued that the current

density vector must be able to be non-zero in a vacuum to permit

the propagation of light, and in turn that this meant that the

vacuum must contain electrons or conducting matter; he writes :

... there is scarcely any reason for adhering to the
hypothesis of an aether; for it may well be assumed
that in the so-called vacuum there is sufficient matter
to form an adequate substratum for the motion [electric current] .[2]

This interpretation must be incorrect or incomplete. The vacuum

is empty, and so the current density vector must be zero there --

Lorenz's identification might be defensible for conducting matter,

but it cannot hold for the vacuum.

The light vector should be identified with the electric

vector $\underline{E}$, and consequently the A.A.D. programme should adopt Lorenz's

basic idea, but not the strict form of his theory and not his

interpretation. The need for this identification is made even

clearer by Hertz's 1884 paper.[3] In this paper Hertz proves the

formal equivalence of the retarded scalar and vector potentials

of Riemann and Lorenz and the electromagnetic axioms used by

Maxwell in his derivations; and thus the retarded potentials

---

1. Lorenz (1867), page 288.

2. Lorenz (1867), page 301.

3. H.Hertz (1884), 'On the Relations Between Maxwell's Fundamental
Equations and The Fundamental Equations of the Opposing Electromagnetics'.

are shown to entail the propagation of transverse electric waves.

Although the two sets of equations are formally equivalent, they have different interpretations. Maxwell's theory concerns propagating transverse electric waves in dielectrics, and this account is extended to the vacuum by the postulate that the vacuum is itself a dielectric. The theory was heuristically ad hoc, and in the 1880's neither the means of production and detection of the waves nor the boundary conditions between media for the waves were understood. The theory of retarded potentials applies primarily to the vacuum (and actually needs development to apply to dielectrics, as dielectrics supply secondary sources of the waves). The theory was heuristically acceptable, and Lorenz had given the means of production, detection, and the boundary conditions, for the waves. None of the predictions of either theory was confirmed before Hertz's well known experimental work of the late 1880's.

These A.A.D. theories, and the A.A.D. theory of light seem to have been understood best by the Field theorists in Britain and in particular by Maxwell. The majority of continental scientists were misled by Helmholtz and his accounts of electrodynamics -- as I will show in Chapters 4 and 6. Maxwell, though, had a good appreciation of the strengths of A.A.D. methods and emphasizes them

repeatedly throughout his publications.   For example, he writes :

> According to a theory of electricity which is making
> great progress in Germany, two electrical particles
> act on one another directly at a distance, but with a
> force which, according to Weber, depends on their
> relative velocity, and according to a theory hinted at
> by Gauss, and developed by Riemann, Lorenz, and Neumann,
> acts not instantaneously, but after a time depending on
> the distance.   The power with which this theory, in
> the hands of these eminent men, explains every kind of
> electrical phenomena must be studied in order to be
> appreciated.
> Another theory of electricity, which I prefer, denies
> action at a distance and attributes electric action
> to tensions and pressures in an all-pervading medium,
> these stresses being the same in kind with those familiar
> to engineers, and the medium being identical with that
> in which light is supposed to be propagated.
> Both these theories are found to explain not only
> the phenomena by the aid of which they were originally
> constructed, but other phenomena, which were not thought
> of or perhaps not known at the time; and both have
> independently arrived at the same numerical result,
> which gives the absolute velocity of light in terms
> of electrical quantities.

But he did not think that the A.A.D. theory of light was the
equal to his own.   The reason for this was the mechanical essentialism
discussed in Sections 3 and 8 of this Chapter.   Maxwell rebuts
the retarded potential theory on these grounds.   I should explain
that for Maxwell the vector potential was an analytic measure of
Faraday's electrotonic state and was thus a mechanical state of
the aether, but the scalar potential :

> ' ... is a mere scientific concept; we have no reason to
> regard it as denoting a physical state.'[2]

---

1. W.D.Niven (ed.) (1965), The Scientific Papers of James Clerk-Maxwell, Vol. 2, page 228.

2. Maxwell (1881), page 53.

Maxwell writes as his rebuttal :

> Now we are unable to conceive of propagation in time,
> except either as the flight of a material substance
> through space, or as the propagation of a condition
> of motion or stress in a medium already existing in space.[1]

To sum up; the retarded potentials fail to explain the behaviour

of light because he, Maxwell, cannot understand how it is that

a propagated scalar potential is itself to be understood in

mechanical terms.[2]

The pure A.A.D. programme led to no new theoretical

predictions in the thirty years from 1870 to 1900.   There simply

were no scientists working within the A.A.D. tradition.   But many

of the earlier predictions of theories in the programme were

confirmed during this period, principally those relating to the

atomic nature of the sources of electrical force.   Most of these

predictions are discussed in Chapter 4.   The A.A.D. programme

continued into the twentieth century and through to the present

day -- contributors here range from Ritz[3] and Wiechert[4] working

at the turn of the century through to Wheeler and Feynmann and

---

1. Maxwell (1873), $\int$ 866.

2. This hesitancy about the scalar potential makes Maxwell adopt
the Coulomb gauge, and this leads to the curious result that the
scalar potential acts instantaneously at a distance, whereas the
vector potential propagates.

3. See W.Hovgaard (1932), 'Ritz's Electrodynamic Theory'.   Ritz
claims allegiance to Gauss in his (1908b), 'A Critical Investigation
of Maxwell's and Lorentz's Electrodynamic Theories', page 231.
Modern physics students are told that ballistic theories like Ritz's
are refuted by the transverse Doppler effect.   Apparently though
it is not clear that the experimental results do mean that Ritz's
theory is faulty.   Ritz's theory is explained in A.O'Rahilly (1965).

4. See T.Hirosige (1966), 'Electrodynamics Before the Theory of
Relativity 1890-1905', pages 18 and f.

other modern theoreticians.

Research continued in the no-source Maxwellian aether Field programme until 1905. The main scientists involved were Heaviside, Poynting, Fitzgerald, and Lodge (although the latter two did on occasions discuss atomic sources). And the work was fruitful. The results were mostly of a theoretical kind, not leading to empirical discoveries; and the most important one concerned the standard Field technique of locating energy in the field. I argued in Section 9 that the Thomson-Maxwell energy density method of calculating the energy of electromagnetic interaction was without independent evidence before 1870. Also I mentioned that during this time the Field theorists were unable to explain what it was for a current to flow through a wire. Poynting and Heaviside's work changed all this. Using the Poynting vector in the way that is now standard, they showed : that when a current flows through a wire one can maintain that nothing does happen in the wire, instead there is an energy flow through the space around the wire (possibly a flow of kinetic energy of the aether), that a travelling electromagnetic wave carries energy (and momentum), and that the locations and movements of energy could be described consistently. These interpretations led to no empirical discoveries, but they were certainly novel theoretical unifications. One other theoretical consequence should be mentioned

for its somewhat ironic connection with Helmholtz's reasons for
preferring the Field programme;   Helmholtz rejected the A.A.D.
theories because they employed velocity dependent forces, yet
Heaviside used velocity dependent forces as a natural part of the
Field programme.

Chapter 2 : The Origins of Electrodynamics : Oersted and Ampère

1. Introduction.

2. Agassi on the Philosophy of Discovery of General Facts.

3. Oersted's Discovery of the General Fact that Current Electricity
   Produces a Magnetic Field.

4. The Production of Ampère's Law by Rational Problem Solving
   within the A.A.D. Programme -- Dorling on Demonstrative
   Induction.

5. The Evidence for Ampère's Law and the Significance of Oersted's
   and Ampère's Results for the A.A.D. and Field Programmes.

6. Ampère's Theories of Magnetism and the Epistemological
   Interpretation of Them.

7. The Origin, Validity, and Epistemological Status of the Biot
   and Savart Law.

8. Tricker on the 'As if' Interpretation of Ampère's Theories.

9. Summary.

1. Introduction

Classical electrodynamics originates with Oersted's discovery in 1820 that current electricity affects a magnetic compass, and it was this interaction which constituted the first challenge to the A.A.D. programme.

The discovery is clearly an important one and has to be discussed. It turns out, though, that Oersted's theories are uninteresting in as much as they are not rationally defensible nor even are they pretenders to knowledge. In short, primary source material is unexciting, although the question does remain of how Oersted made a discovery that others failed to make. Secondary literature consists of Meyer's biography, Dibner's and Stauffer's historical studies, and Agassi's philosophical and historical work.[1] Agassi gets very agitated about discoveries (which is defensible in that discoveries advance knowledge) and put his ideas into action on Oersted.

It seems useful to relate what little I claim about Oersted to Agassi's philosophical and historical assertions. I maintain that Agassi's philosophy is unfruitful and, more importantly, his history is mistaken.

The challenge posed by Oersted's discovery was successfully met by Ampère. He used one simple idea and routine application of heuristic to produce a law which brought the interaction entirely within A.A.D. The idea was that of simulating or replacing magnets by loops of current. There appear to have been two strands of thought here. From the fact

---

1. Kirstine Meyer (1920), H.C. Oersted, Scientific Papers, Collected Edition with Two Essays on His Work; B. Dibner (1961), Oersted and the Discovery of Electromagnetism; R.C. Stauffer (1953), 'Persistent Errors Regarding Oersted's Discovery of Electromagnetism', Isis 1953; R.C. Stauffer (1957), 'Speculation and Experiment in the Background of Oersted's Discovery of Electromagnetism', Isis 1957; J. Agassi (1963), Towards an Historiography of Science.

that a current affects a compass and that the magnetic Earth affects

a compass to the guess that the magnetic Earth was really a current[1] --

then only a little geometrical intuition is required to see that the

current must be in the form of a loop or solenoid to mimic a magnet.

And from the realization that the earlier claims of Coulomb were really

to the effect that only likes interact, and that Oersted had refuted this

unless currents were really magnets or magnets really currents. Thus

Ampère suspected, and then found, that two current carrying wires would

interact on their own.[2] This is the background to his well-known

experiments on the magnetic forces between two parallel wires each

carrying a current.[3] The substitution of loops of current for magnets

---

1. He used a thought experiment. Imagine, he suggested, that the development of electromagnetism had occurred in reverse: that first Oersted had discovered that a current could align a magnetic needle, and that later it was found that a magnetic needle would point to the North Pole; what would be a reasonable guess as to what was happening? See Ann.Chim.Phys., XV, 1820, Section II, pp.59-76 and 177-208.

2. Arago described Oersted's results to the French Academy on 11th September 1820 ('Expériences sur l'effet du conflict electrique sur l'aiguille aimantée, par M.H. Chr.Oersted', Ann De Chimie, VIX, 1820, p.417). There was quite a reaction -- especially since Coulomb had 'proved' some forty years earlier that there could be no effects between electricity and magnetism. Within a week Ampère had stated publicly that there was a force between two parallel current carrying wires. His achievement was belittled on the grounds that it was a consequence of Oersted's discovery. Ampère rebutted this: 'When M.Oersted discovered the action which a conductor exerts on a magnet, it really ought to have been suspected that there could be interaction between two conductors; but this was in no way a necessary corollary of the discovery of this famous physicist. A bar of soft iron acts on a magnetised needle, but there is no interaction between two bars of soft iron.' (Memoires de L'Academie Royale des Sciences 1823 (issued 1827), probably written in 1826.)

3. I offer a minor historical conjecture here. Contrary to most histories, Ampère discovered first that the two current carrying helices would interact. Such an experiment makes better sense than the parallel wires one, and the documents reveal that in the Academy meeting of 25th September 1820 Ampère announced both the interaction of helices and the interaction of parallel wires.

changed the problem raised by Oersted's discovery into that of giving an account of interacting currents, and this Ampère solved by an inverse-square central force law which was produced by standard textbook methods. The law was:

$$dF = (\sin \theta \sin \theta' \cos \omega - \tfrac{1}{2}\cos \theta \cos \theta')\frac{\text{ids i 'ds'}}{r^2}$$

where dF is a differential of force acting centrally, ids and i'ds' are products of currents and differential circuit elements, r is the distance between these circuit elements, θ is the angle between one element and r in their plane, θ' is the angle between the other element and r in their plane, and ω is the angle between two planes. Historians have never made sense of what Ampère was doing. He presented his work as inductivist deduction from the phenomena, but yet he used an explicit assumption -- that the phenomena were governed by a central force law. This use of an unproven and untested assumption means that Ampère's claim is false historically as a description of the process of discovery and false logically as a description of the status of his law. Typical here are Pearce Williams's:

> Ampère first described the law of action of electric currents, which he had <u>discovered</u> from four extremely ingenious experiments.[1]

and Whittaker's:

> The weakness of Ampère's work evidently lies in the assumption.[2]

How was the law discovered and what is its status? One philosopher -- Jon Dorling -- has an answer.[3] It was discovered by Demonstrative

1. 'Ampère' article, <u>Dictionary of Scientific Biography</u>, page 145. My italics.

2. Sir E.T. Whittaker (1951), <u>A History of Theories of Aether and Electricity</u>, page 86.

3. J. Dorling (1973), 'Demonstrative Induction: Its Significant Role in the History of Physics'.

Induction and had the status of a plausible hypothesis. I agree with Dorling over the question of discovery, and I agree that the law had the status of a plausible hypothesis, but I will express reservations about Dorling's arguments on status. The M.S.R.P. also suggests a solution -- according to it most laws are discovered by the evolution of a programme under its heuristic. I agree with this solution too. In this case, Demonstrative Induction was the heuristic tool which suggested that certain experiments be performed and then enabled the law to be deduced from the facts.

Ampère's law was a great triumph for A.A.D. It reduced to one law all known electromagnetic forces except the static electrical one. This omission left the problem of bringing the Coulomb electrical force into the scheme.

The reduction was accomplished with the aid of the elimination of magnetic poles as an ontological category. I briefly explain this and discuss its epistemological significance. In particular, the main secondary source on Ampère -- Tricker -- urges an instrumentalist inter-pretation here. I criticize his view in Section 8 and discuss the instrumentalism versus fallibilist realism debate in Appendix 2.

Modern scientists do not use Ampère's law when calculating forces between circuits, instead they use a law which was produced at about the same time as Ampère's law by Biot and Savart:

$$dB = \frac{ids \times r^0}{r^2}$$

(where B is the magnetic field and r^0 is a unit vector)

Prima facie, then, this was a rival to Ampère's law.[1] I consider this in Section 7.

---

1. Similar theories -- that is, theories based on forces between current elements and derived from assumptions and geometrical considerations -- were offered by Grassman in 1845, Stefan in 1869, and Korteweg in 1881. (See J.J. Thomson (1885) Report on Electrical Theories.) I do not discuss these because I hold that the A.A.D. tradition started with Ampère's current elements and developed to the electron theories of Weber by 1845. Later current element theories were a retrograde step.

2. Agassi on the Philosophy of Discovery of General Facts:

Agassi offers a bold thesis on the discovery of general facts: behind every such discovery there exists a theory which the discovery refutes.[1]

He arrives at this view as follows: He considers the problems:

a) How are discoveries made?

b) Why are discoveries not made earlier than they are? and, subsidiarily,

c) How can we make discoveries?

The type of discovery at issue here is that of general facts for example, that water boils at 100°C. Agassi then looks at the possible logical relations between the discovery and scientific theories. He maintains that either:

a) the discovery is independent of all theory,

or b) the discovery is predicted by a theory,

or c) the discovery is forbidden by a theory (that is, the discovery refutes a theory),

and he treats these three as exclusive and exhaustive categories. In his (1975), page 79, he writes:

I should stress again that the choice is only between inter-
preting a discovery to be a verification, or a refutation,
or an accident.

That discoveries are, or should be, independent of all theories Agassi calls Bacon's view. Under this, all true discoveries are accidental. Their novelty consists in not being known previously; that is, in being independent of all prior knowledge. Many observers, however, do entertain theories and become blinded by these 'prejudices' and that is why discoveries are not made earlier than they are. The

1. J. Agassi (1963), pp.60-67, and J. Agassi (1975), 'On Novelty', Chapter 3 of Science in Flux (see especially the Appendix on page 73f.)

remedy, and the recipe for making discoveries, is for searchers to purge their minds.

That discoveries are predicted by theories Agassi calls Whewell's view. Whewell agrees with Bacon that factual novelty lies in being independent of _existing_ theory, but argues that if the effects are not expected then they would not be noticed. New factual occurrences are predicted on the basis of _new_ ideas. Scientists produce new theories and on the rare occasions that these work a factual discovery may result. Discoveries are not made earlier because they need the advent of the theory which predicts them. And the advice to the discoverer is to think up new ideas.

Finally, that discoveries refute theories is Agassi's view. He aligns himself with Whewell against Bacon over the impossibility of recognizing independent happenings but attacks Whewell's account with the point that many discoverers just plain did not believe their own eyes when making a discovery and so they could not have expected their discoveries. The only other possibility is that discoveries refute theories. The discoveries are not made earlier because they have to wait for the proposal of the theory which they refute. Further, the neophyte discoverer is to actively criticize or try to refute theories.

Agassi's case is not strong.

The first point to be made against Agassi (and Whewell also) is that their views are practically irrefutable. The closure of proposed scientific theories is hard to delimit, and the consequence classes of members of this closure will be recursively enumerable but not recursive; that is, it is easier to show that a discovery refutes or confirms an existing theory when it does, than it is to show that no such theory exists when indeed no such theory exists. This means that

the onus should shift to Agassi to show the virtue of his irrefutable

suggestion. We can look only at the arguments he uses and at the

fruitfulness as a historical tool of his search for the refuted theory

behind each discovery. The arguments are lacking and in the case of

Oersted, which is Agassi's favourite, the search is unrewarded.

A second point to be made about the discovery of general facts

is that even these 'facts' have a theoretical content. True a rough

distinction can be made between factual and theoretical, or items to

be explained and explanations; but such a demarcation will shift

through time and will be only a relative one so that 'facts' will have

a certain theoretical backdrop. For instance, that water boils at 100°C

could hardly have been discovered before notions of temperature,

Centigrade temperature scales, boiling, and so on were familiar.[1] This

consideration partly explains why many discoveries were not made earlier

and it is also a serious objection to Bacon's atheoretical facts.

Agassi's own account is sloppy logically, weak heuristically,

and is founded largely on armchair psychology of discovery.

The categories he considers are not exclusive -- an effect may

well be predicted by one theory but yet forbidden another. The logic of

1. One approach here is to reduce all discoveries to discoveries that
... where the blank space is filled in by a proposition. So, for
example, instead of talking about the discovery of oxygen we talk of the
discovery that there was a gas with atomic weight 16 (or whatever). This
move brings the theoretical content out into the open and permits
conclusive debate of rival assertions. Whereas discussing issues like
'Who discovered oxygen and when was it discovered?' is hopeless because
the discoverer has both to encounter oxygen and to know or to recognise
what he had encountered, and the last requirement is just too vague.
A.E. Musgrave has done some preliminary work here -- see page 195 of
A.E. Musgrave (1976), 'Why did oxygen supplant phlogiston? Research
Programmes in the Chemical Revolution', in C. Howson (Ed.) (1976),
Method and Appraisal in the Physical Sciences.

the matter is complex. There is an amorphous background containing all sorts of views ranging from specific theories, rationally acceptable at the time or not rationally acceptable, to vague expectations, both fashionable and unfashionable; and this conglomerate will be inconsistent. A scientist should hold a consistent selection of rationally defensible views, but he will be aware of many more ideas than these, and in particular will often be able to recognize that the prediction of a crankish idea has actually occurred.

Much of Agassi's discussion concerns in essence _psychology_ of discovery. We are offered:

a) Bacon's claim that if you hold no theories you will observe all facts whereas if you adopt a theory (or are prejudiced) you can see only confirmations of it.

b) Whewell's claim that if you hold no theories you will be 'swamped' by possible perceptual information and will not be able to see anything of significance in it, whereas a theory serves to focus interests; then you notice occurrences you expect to happen.

c) Agassi's claim that provided what happens refutes a decent theory then it will not slip by; you notice things you do not expect.

I do not know what is the right answer here and frankly I doubt the ability of philosophers doing _a priori_ research to find it. Even so, Agassi's account is not the right one. He supposes that refutations are few and far between and thus their significance is manifest. But, as we will see in Appendix 1, any decent scientific theory has a plethora of (real or apparent) refutations and thus the Aggasi-ite would find himself as swamped as the Baconian. Historical example runs against Agassi here. Thermodynamics, for instance, was 'refuted' by Brownian motion even before it was proposed, yet there was a wait of eighty years until statistical

mechanics plucked the refutation from the background noise of exceptions and gave it significance.[1]

Finally, the heuristic advice seems unlikely to result in a rash of discoveries. There is no need to try to refute theories -- theories apparently go wrong all over the place. What is required is some sifting and fortifying of these exceptions and that, as Feyerabend has carefully argued, is better achieved by proliferating and developing rival ideas and explanations.[2]

_____

1. This example, and the general idea of proliferation, run through many of Feyerabend's papers of the late 1960's. See, for instance, P.K. Feyerabend (1968), 'How to be a Good Empiricist -- A Plea for Tolerance in Matters Epistemological', in P.H. Nidditch (Ed.) (1968) The Philosophy of Science.

2. See Feyerabend (1968).

3. <u>Oersted's Discovery of the General Fact that Current Electricity</u>

   <u>Produces a Magnetic Field</u>:

In July 1820, Oersted announced that an electric current
and the magnetic needle of a compass can interact. His findings pose
several historical problems; primarily, Why did he succeed where
others had failed?

My view is that Oersted held several unorthodox theories
and as a result had a rough idea as to how the interaction should
occur, even so he was lucky to find it. Others failed because in
the main they were not looking, as Coulomb had convinced orthodox
science that there could be no interrelation.[1] The argument will be
developed by criticizing two descriptions of Oersted's work and
extracting from each an important unresolved question which my
account answers.

As we have just seen in the last section, some hold that
true discoveries are accidental and in particular the discovery of a
completely new effect, like Oersted's, has to be accidental. The
first of these myths was provided by Ludwig Wilhelm Gilbert when he
translated Oersted's initial announcement into German[2] and published

---

1. Ampère wrote to M. Roux-Bordier February 21, 1821: 'You are quite
right in saying that it is inconceivable that for twenty years no one
tried the action of the voltaic pile upon a magnet. I believe, however,
that one can assign a cause for this; it was Coulomb's hypothesis on
the nature of magnetic action. People believed this hypothesis was
a fact and discarded any idea of an action between electricity and
the so-called magnetic wires ... Everyone had already decided that
[interaction] was impossible.' Quoted from L. Pearce Williams
(1962), 'Ampère's Electrodynamic Molecular Model', page 114.

Arago tells us in his <u>Oeuvres Complètes</u>, (1854), Vol.2, page 50
that Ampère used to announce in his 1802 lectures that he would:
'DEMONSTRATE that the electrical and magnetic phenomena are due to
<u>two different fluids</u> which act independently of each other.'

2. 'What every search and effort had not produced, came to Professor
Oersted ... by accident ...', <u>Annalen der Physik</u>, <u>66</u>, (1820), page 292.

it in his <u>Annalen der Physik</u> in 1820.

Oersted himself quickly produced a defense:

> All my auditors are witnesses that I mentioned the result
> of the experiment beforehand. The discovery was therefore
> not made by accident, as Professor Gilbert has wished to
> conclude from the expressions I used in my first announce-
> ment.[1]

The background here was Oersted's belief in the unity of the forces

of nature and an acceptance of the widespread scientific view that

only likes interact.[2] He was sympathetic to F.W.J. Schelling's

Naturphilosophie. This discipline was a type of mystic, Kantian,

<u>a priori</u> study of the universe which stressed the role of intuition

and the unity of physical forces.[3] The opinion that only likes

interact was undermined by the discovery that frictional or static

electricity could produce chemical effects such as dissociation, and

this difficulty was further emphasized by the discovery of electrolysis

in 1800. This suggested an analogy between static and current

electricity but did nothing to connect the pair with chemical forces.

One idea was to assume that there was one primordial force which could

take on different aspects; then electrolysis does not refute the view

---

1. Footnote to H.C. Oersted (1821), 'On Electromagnetism (A.) The
History of my previous Researches on this Subject.', new translation
in Stauffer (1957).

2. 'Throughout his literary career, he adhered to the opinion, that
the magnetical effects are produced by the same powers as the
electrical. He was not so much led to this, by the reasons commonly
alleged for this opinion, as by the philosophical principle, that all
phenomena are produced by the same original power.', H.C. Oersted
(1830), 'Thermo-Electricity', <u>The Edinburgh Encyclopaedia, XVIII</u>
(1830). Oersted writes about himself in the third person in this
paper.

3. See, for instance, I. Kant (1786), <u>Metaphysical Foundations of
Natural Science</u>. (Translation, J. Ellington (1970).)

that only likes interact since chemical forces and electrical forces are likes in so far as they are both transformations of the primordial force. This modification was confronted by electricity and magnetism which are naturally occuring forces but yet which were not known to interact -- there should be some way of either transforming the primordial force into electricity and magnetism or of converting the latter pair into each other.[1] Thus Oersted's problem was to find the conditions of transmutation, and it is this that he refers to when he says that he expected the result.

This background forces a modification of the accidental discovery story and a much more cogent version was given by Professor Hansteen in a letter to Michael Faraday:

> Already in the former century there was a general thought that there was a great conformity, and perhaps identity, between the electrical and magnetical force; it was only a question of how to demonstrate it by experiments. Oersted tried to place the wire of his galvanic battery perpendicular (at right angles to) over the magnetic needle, but marked no sensible motion. Once, after the end of his lecture, as he had used a strong galvanic battery in other experiments he said, 'Let us now try once, as the battery is in activity, to place the wire parallel to the needle'; as this was made, he was quite struck with perplexity by seeing the needle making a great oscillation (almost at right angles with the magnetic meridian). Then he said: 'Let

---

1. Oersted describes this: '... electrical bodies act upon magnetic bodies as if they were not animated by any particular force whatsoever. To remove this difficulty completely would be very interesting for science; but, since the present state of physics has not yet furnished facts sufficient for that, we shall show at least that this involves merely a difficulty, not a fact absolutely contrary to the identity of the electrical and magnetic forces ... The galvanic mode of activity lies midway between the magnetic mode and the electrical. There the forces are more latent than in electricity and less than in magnetism.
...Magnetism exists in all the bodies of nature ...For this reason it is felt that magnetic forces are as general as electrical forces. One should test whether electricity in its most latent form has any action on the magnet as such. This experiment would offer some difficulty because electrical effects are always likely to be involved, making the observations very complicated.' H.C. Oersted (1813), 'Recherches sur l'identite des forces chemiques et electriques.'.

us now invert the direction of the current' and the needle
deviated in the contrary direction. Thus the great detection
was made; and it has been said, not without reason, that 'he
tumbled over it by accident'. He had not before any more idea
than any other person that the force should be transversal.[1]

The account seems inaccurate. If, as Hansteen states, the magnetic

force was strong enough to produce a ninety degree deflection then, in

the original perpendicular case, it would have been perfectly obvious

that the needle was adjusting itself perpendicular to the wire and

not aligning itself in the magnetic meridian; again, if Oersted had

chosen the wrong direction of East-West or West-East he would have seen

the spectacular occurence of the needle rotating through 180 degrees.[2]

Hansteen's letter, which was written 37 years after the event,

probably contains imaginary embellishments.[3]

It does, though, highlight one question which any viable

reconstruction should answer. Most historical accounts of the

discovery have this reference to parallel and perpendicular placements

of the needle. It must therefore be asked: Why was it that the

needle was placed initially perpendicular to the wire? This is the

first unresolved question that I mentioned earlier.

Agassi takes it on, and as a result has a twofold problem:

why the perpendicular placement and what is the theory that the

discovery refutes? He starts with the important insight that:

---

1. Hansteen (1857), Letter to Michael Faraday 30th December 1857.

2. It is not impossible to obtain the results described by Hansteen,
but it is unlikely that such should occur. The directions 'perpendicular
to the wire' and 'the magnetic meridian' are identical if perpendicular
is exactly ninety degrees. But setting the wire perpendicular would in
practice usually mean setting it roughly perpendicular and then there
would be a vibration of the needle when the current is switched on. So,
if the 50 : 50 chance of having the current in the correct direction
favoured you, and you had the wire within a few degrees of perpendicular,
and you failed to notice the vibration -- you could produce the results
described by Hansteen.

3. Stauffer argues in his (1953) that Hansteen did not witness the
discovery, and he develops this theme in his (1957).

[Oersted's theories] led him to introduce the electric current into the investigation ... the current itself was not there accidentally nor was its role predicted by anyone but Oersted.[1]

Oersted did not hold the received view that an electric current is the flow of electric matter; instead, Agassi suggests, Oersted thought that currents were transformations of forces, that:

> ... in an electric discharge the electric force is transformed into other kinds of force; namely, heat and light. And ... that if the current is sufficiently strong it might also turn electricity into magnetism.[2]

So that if a more powerful cell were used -- which, by the way, Oersted invented in 1816 -- the current carrying wire would become a weak magnet. However, Agassi continues, Oersted did not know any more about this magnet and, in particular was ignorant of the position of the poles; but Oersted believed, being a Newtonian, that the magnetic forces involved were central. Agassi goes on:

> Now, if one has a long weak magnet, if one does not know where its poles lie or which is North and which South, and if one wishes it to interact with a compass, some knowledge of Newton's theory of force will tell one to place the magnet in the East-West direction. One does so and sees no result. Hence one appears to have made a mistake. One concludes that either (a) the long weak magnet is weaker than thought, or (b) that it is not a magnet after all, or else (c) that the Newtonian hypothesis concerning forces is false.[3]

When eventually Oersted tested the third assumption:

> he gasped; he saw at once how much more important his discovery was than he had ever hoped.[4]

Therefore Oersted's 'accidental' discovery was a refutation of the Newtonian hypothesis that all forces are central.

Agassi's account is interesting but not satisfactory. It relies on the hypothesis that Oersted held that the current-carrying wire was a longitudinal magnet. Oersted states categorically in his

---

1. Agassi (1963) page 69.

2. Agassi (1963) page 71.

3. Agassi (1963) page 72.

writing that he thought that the wire was <u>not</u> a magnet at all;

rather he thought that magnetic influence would emanate from the wire

in all directions. For example, Oersted wrote:

> As the luminous and heating effect of the electrical current,
> goes out in all directions from a conductor, which transmits a
> great quantity of electricity; so he thought it possible that
> magnetical effect could likewise eradiate. [1]

Agassi dismisses this on the grounds that it was in Oersted's

interest to invent <u>post hoc</u> a theoretical background. But all

Oersted's descriptions -- both before and after the event -- cohere

together well.

Besides, there are objective reasons why the assumption

that the wire was a longitudinal magnet would be the very last one

that Oersted would make. There had been plenty of thought about

the connection between electricity and magnetism before Oersted's

discovery. At first there seemed to be many similarities between

the unusual attractive and repulsive powers of lodestone and amber,

and in particular that inverse-square force laws ruled. The com-

parison here was between <u>static</u> electricity and magnetism.

The major disanalogy was that no matter how a magnet was made, or

cut up after manufacture, it always had two poles; whereas the two

forms of static electricity were easily available independently of

each other. The galvanic cell, when invented, provided the natural

analogue of the magnet for it too was <u>dipole</u>. But the analogy was

sustained <u>only as long as the cell was on open circuit</u>. As soon

as the terminals were connected there was a galvanic current which

was an entirely new effect apparently not connected with either

electricity or magnetism. By analogy then, a cell on open circuit

should be a longitudinal magnet. Accordingly there were many

---

1. Oersted (1830).

experiments trying to align suspended cells in the earth's magnetic
field.[1] All produced negative results, so a cell was not a magnet
whose line of action was that joining the terminals. Apparently,
perhaps due to frustration with the negative results, the experiment
was also tried with a straight piece of wire connecting the terminals
— again there was no success.[2] Oersted certainly did not hold that
the current-carrying wire was a longitudinal magnet; to emphasize
that he wrote:

> ... he conjectured, that if it were possible to produce any
> magnetical effect by electricity, this could not be in the
> direction of the current, since this had been so often tried
> in vain, but that it must be produced by a lateral action.[3]

Furthermore, if Oersted actually had refuted the Newtonian assumption
one would expect him to claim this very important discovery with some
vigour; in fact, he always regarded the interaction between electricity
and magnetism as the discovery and merely mentions without special
comment that the forces appeared to be rotational.

Agassi's account is mistaken.

The value of Agassi's work is that it draws attention to the
question of why Oersted employed galvanic electricity. This is the
second problem mentioned earlier. Oersted had an unorthodox view of
current. The transmission of current was oscillatory and the
electricity possessed great activity. When a wire was being heated
electrically the electric forces combined together becoming neutral

---

1. See P.F. Mottelay (1922), <u>Bibliographical History of Electricity
and Magnetism</u> page 376. Hachette and Désormes's experiment on align-
ing an insulated pile was widely known.

2. Oersted's friend Johann Wilhelm Ritter claimed success, but the
experiment was not reproducible. Oersted was wary of freak effects.
Earlier he had been castigated by the <u>Anneles de Chime et de Physique</u>
for enthusing baselessly on the results of Ritter's imagination (See
L.P. Williams, 'Oersted', article in <u>Dictionary of Scientific Biography</u>.)

3. Oersted (1830).

and yet still showed great activity by reappearing in an entirely different form as heat. To effect this transformation and produce heat, thin wires of high resistance are needed. Oersted also knew that if the cells were strong enough and the wires thin enough, the current undulations could be converted into light. And next comes his conjecture. If the wires are yet thinner still, and the cells yet stronger still, perhaps the current undulations will produce magnetism as well as heat and light.

The experiment was tried in the spring of 1820:

> Since I expected the greatest effect from a discharge
> associated with incandescence, I inserted in the circuit
> a very fine platinum wire above the place where the needle
> was located. The effect was certainly unmistakable, but still
> it seemed to me so confused that I postponed further invest-
> igation to a time when I had more leisure.[1]

Two remarks are called for here. The small effect was due to the low current flowing because of the high resistance of the platinum wire. This is where Oersted's luck comes in -- the conditions which he insisted upon were those which produced the weakest magnetic field, so he was fortunate to observe it. Second, he did not become excited at the mild positive result because he had often experienced disappointment with effects that were not reproducible.[2]

Three months later:

> In the month of July 1820, he again resumed the experiment,
> making use of a much more considerable galvanical apparatus.
> The success was now evident, yet the effects were still feeble
> in the first repetitions of the experiment, because he employed
> only very thin wires, supposing that the magnetical effect
> would not take place, when heat and light were not reproduced
> by the galvanical current; but he soon found that conductors

---

1. Oersted (1821).

2. See footnote 2 on page 79.   Besides cells lasted only a few minutes, so reproducible effects were difficult to obtain.

of a greater diameter gave much more effect; and then he discovered by continued experiments during a few days, the fundamental law of electromagnetism, viz., that the magnetical effect of the electric current has a circular motion round it.[1]

To sum up. The needle was placed perpendicular to the wire because the magnetic influence was expected to emanate like heat, and the parallel placement had apparently failed to detect this emanation. Oersted stresses these objective grounds during his own reconstructions, and this is why the stories arose about him not using the parallel placement.[2] The expectation of magnetic emanation was a loose consequence of the non-standard oscillatory view of electric current and its role as the link in the transformations of the primordial force.

Oersted's results were given public expression in the well-known Latin paper.[3]

---

1. Oersted (1830).

2. My view apparently does not explain what it should. Under my account the wire should be placed in any direction other than parallel -- there is no requirement that it be placed perpendicular. I suggest that commentators used 'perpendicular' to describe any set up in which the needle crossed the wire -- that is, 'perpendicular' means 'any direction other than parallel'.

3. H.C. Oersted (1820), 'Experimenta circa effectum conflictus electrici in acum magneticam'. Translated in Thomson's Annals of Philosophy, Oct. 1820, XIV first series pp273-6.

4. The Production of Ampère's Law by Rational Problem Solving within

A.A.D. -- Dorling on Demonstrative Induction :

In the early 1820's, Ampère put forward a law which encompassed

all steady current and magnetic phenomena known at the time.   The

law was :

$$dF = G \frac{ids. \ i'ds'}{r^2}$$

where dF is a differential of force acting centrally, ids and i'ds'

are products of currents and differential circuit elements, r is the

distance between these circuit elements, and G is a geometrical factor.

I maintain that the law was a hypothesis produced by rational

problem solving within the A.A.D. programme.   I support this thesis

by reconstructing the heuristic path to the law.   My reconstruction

will be objective and such that any Newtonian would find plausible

reasons for making each of the assumptions or decisions in it -- no

innovations are required.   This means that the law is tightly bound

to the A.A.D. programme and reflects favourably on it.   After presenting

my reconstruction, I will consider the arguments of Jon Dorling to the

effect that it was Demonstrative Induction which yielded and fortified

the law.

The problem is to find a central force law which gives the

force between two current carrying circuits.

The total force is considered to be the resultant of the forces

due to the elements of the circuits, and these elementary forces are

are assumed to be central.

A decision is required at this point. What should be considered as the _elements_ of a circuit ? Wires or currents differ from masses in that they have direction as well as position, and thus it seems that a slight departure from standard A.A.D. methods is required. But this difficulty is not new. Magnets are directed line segments, and the technique with them is to look into their inner structure and to regard their behaviour as being the resultant of the effects of the two individual poles. This apparently is the best way of analysing wires; an alternative is to try the calculation using directed line segments as elementary.

If we follow the latter line, the question becomes : what is the central force between two arbitrarily orientated circuit elements separated by a distance r :
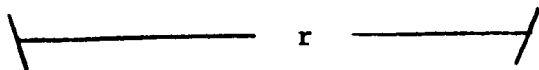
A Newtonian force is central and has magnitude proportional to $\dfrac{M_1 M_2}{r^n}$ where M_1 and M_2 are some factors of the sources and n is an integer, usually 2. With circuits, the force is zero with no current, its direction reverses if either current is reversed, and is

greater the longer the wire. M is guessed to be ids so that the force
is proportional to[1] :

$$\frac{ids. \; i'ds'}{r^n} \qquad \text{(n usually 2)}$$

The next step is to impose some order on the arbitrary
orientations. It is assumed that these can be split up into cases.
Say the first element is in the x-y plane, thus :
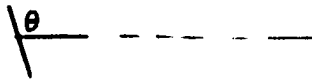
then it will have a projection onto the x-axis of dx = ds cos θ and
onto the y-axis of dy = ds sin θ . The second element is placed in a
plane with r at angle ω to the x-y plane and θ' is measured in i'ds's
own plane, then dx' = ds' cos θ' ,dy' = ds' sin θ' cos ω , and dz = ds' cos ω

There are thus five cases :

a) dx with dy' :

b) dx and dz' :

c) dy " dx' :

d) dx " dx' :

e) dy " dy' :

There can be no forces in cases (a), (b), and (c), by symmetry.

---

1. Ampère's own reasoning on this point may be paraphrased thus :
   The mutual action of two elements of electric current is
   proportional to their length; for, assuming them to be
   divided into infinitesimal equal parts along their lengths,
   all the attractions and repulsions of these parts can be
   regarded as directed along one and the same straight line,
   so that they necessarily add up.

Only (d) and (e) are left :

d) →——   ......   →——

e) ⅄   ......   ⅄
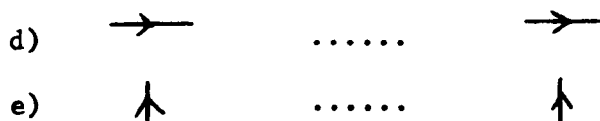
A constant k can be used to denote the ratio between the force in (d)

and that in (e), with the currents flowing as drawn.

Finally, n, the exponent in the denominator, is assigned

its expected value of 2.

The result is Ampère's law :

$$dF = (\sin\theta \, \sin\theta' \cos\omega \ + k \cos\theta \cos\theta') \ \frac{ids. \ i'ds'}{r^2}$$

Ampère gave this law public announcement on December 4th

1820, a little over two months after he had heard of Oersted's

discovery.   k was later found to be - ½.

The discussion so far has concerned the origin of the law, and the

argument has been that it was produced by 'text-book' application of

A.A.D. techniques.   Nothing has been asserted as to the truth or

validity of the law -- that is discussed in the next section.

My reconstruction should be compared with Ampère's writings

and in particular with the experiments he drew attention to.   I say

that he should have performed two experiments -- one to see if the

arbitrarily orientated element can be projected on to the axes, and the

other to find k -- and maybe he should perform a third experiment to find n.

————————————————

What is uncontroversial historically is that Ampère drew attention to four experiments and held that he had proved from them that his law was true.[1] The experiments were of a sophisticated and elegant 'null' variety.[2] Even so they do not support an inductive proof. Ampère's derivation used the Newtonian format and central force assumption and this strips the proof of its certainty. Further, much to the disgust of most scientists and historians, the fourth experiment was not even performed, as Ampère freely admits. He 'knew' how it would turn out and describes it merely to allow others to complete the inductive proof. Two steps are required in such a proof; to show that the law can be of the form Ampère gave

---

1. There are two reasons for being even more cautious than usual about the words of the great man. First, the documents. Ampère used his position as secretary of the Academy to amend his papers and keep them in line with his thought. The results of this stand unused in Paris. Many of the transcriptions of his sources have been added to or altered by the transcribers. (See, L.P. Williams, 'Ampère' article, Dict. Sci. Biog.) Fortunately there is a reasonable version of Mémoire sur la théorie mathématique des phénomènes électrodynamiques, uniquement déduite de l'expérience (1827) in Mémoires sur l'électrodynamique (Paris 1885-7). All that is readily available in English is R.A.R. Tricker (1965), Early Electrodynamics about which Bromberg writes '... it is dangerous to discuss Ampère on the basis of translations in Tricker' (see Joan Bromberg (1976), Review of W. Berkson's Fields of Force, page 133). Second, the false consciousness. Ampère describes his task as proving from experience that his law was certainly true (that is why his book has in its title '...uniquement deduite de l'experience'). Since laws cannot be so proven, Ampère was not doing what he said he was doing.

2. At first sight these null experiments, in which one force is balanced against another so that there is no resultant force to move a magnet or conductor, are extremely sound. Indeed most commentators remark on their accuracy and conceptual elegance. In fact they are not especially reliable. Weber pointed out that if there were friction there might not be movement even if there was a small resultant force. Weber redesigned the experiments and put them on a firm basis. See his (1848), 'On the Measurement of Electrodynamic Forces', page 491.

it, and to show that the law cannot be of any other form. I hold

that the second requirement is always unsatisfiable and no attempt

should be made to meet it. Ampère tried to meet it, and that

explains the divergences from my reconstruction.

The first experiment was that a wire doubled back on

itself exerts no magnetic effect when a current is put through it:[1]

This is to show that if the current is reversed the magnetic force

is reversed. This experiment was unnecessary as Ampère knew from

other experiments what its outcome would be. But _his_ aim was to use

the experiment to sharpen up his proof by eliminating one of the

assumptions, and this experiment validates the use of 'null' methods.

It also backs up the symmetry arguments: - a current element cannot

exert a force on another element in a plane at right angles to itself

because, considering one element, the current approaches the common

perpendicular for one half of the element and recedes from it for the

other half and so the two halves produce equal and opposite forces

which cancel.

The second experiment was that a wire doubled back on

itself, but with the outgoing segment straight and the return segment

bent into arbitrary sinuosities, also exerts no magnetic effect:

---

1. Fuller descriptions of the inessential experimental details are
available in Ampère (1827), Tricker (1965), or A.E. Woodruff (1962),
'Action at a Distance in Nineteenth Century Electrodynamics'.

This is a key experiment. It shows that a small straight piece of circuit produces exactly the same magnetic effect as another piece with identical end points but of arbitrary path. This allows the replacement of a straight piece of random orientation by three other pieces running parallel to the axes, with the same end points. For example, a current element, entirely in the x-y plane, with ends (0,0) and (1,1) is completely equivalent to a current element running from (0,0) to (0,1) connected to an element from (0,1) to (1,1). Note the absurdity of the Pearce Williams's suggestion, quoted in the Section 2 .1, that Ampère's law was discovered inductively from experiment. An eternity would elapse before anyone would merely happen to try an experiment of this kind; just as an eternity would elapse before anyone just happened to take themselves off to South America and look at the stars behind the sun during an eclipse, as in the Eddington eclipse experiment. Ampère's second experiment was a deliberate probe of Nature prompted by the A.A.D. heuristic.

The third experiment was that a movable circular arc of a circuit cannot be put in motion by magnetic interaction with a second circuit of any shape:

What this shows is that there is a mathematical constraint on k (or on the relation between k and n). For if the force is summed around one complete circuit it can exert no tangential force on a circuit element. Ampère integrated by parts the force around one circuit and showed that the no tangential force condition is equivalent to:

$$n + 2k = 1 \quad (\text{i.e. if } n \text{ is } 2, \text{ then } k \text{ is } -\tfrac{1}{2}).$$

The fourth experiment, which was not performed, was to the effect that the linear dimensions of the circuit are irrelevant, provided solid angle proportions are maintained. The magnetic force between two circuits A and B was exactly balanced by that between A and another circuit C which was of similar shape to B but of, say, half the size of B and half the distance from A as B; C, of course, was in reverse orientation to B. This result means that the force is an inverse-square one so that n is 2.

Experiments three and four were a sophisticated way of fixing n as 2 and k as $-\frac{1}{2}$ and were an attempt to rule out other possibilities. Experiment four was unnecessary (except perhaps as a good test of the law, once it was available, n could have been guessed as 2 (which, after all, is what Ampère did)). Experiment three was an elegant, maybe too elegant, way of finding k. A more natural way to have done this would have been to have simply measured the ratio of forces in the (d) and (e) cases, but Ampère also wanted a null experiment for accuracy. (Actually, it took him seven years to think of this way of determining k, as compared with under two months to find the rest of the law.)

As far as I am aware only one philosopher -- Jon Dorling -- has looked in detail at Ampère's deduction.[1] Dorling's arguments exhibit one way in which the positive heuristic functions, and so his paper is of value here. Dorling's thesis is that the valid argument form of Demonstrative Induction (D.I.) is valuable for discovery and justification. My view is that D.I.'s main merit is for discovery.

---

1. J. Dorling (1973), ' Demonstrative Induction: Its Significant Role in the History of Physics'.

Demonstrative Induction has the feature of the explanans

being deduced from one of its own explananda:

> [The principle schema] ... is one in which a universal
> generalization is deduced from one of its own particular
> instances. Of course this deduction involves the use
> of additional theoretical premises. The important thing
> about these additional premises is that they must not
> themselves imply the universal generalization in question
> and that they be such that, in a realistic situation, we
> could have more initial confidence in them than in the
> universal generalization which we propose to deduce with
> their help.[1]

And a typical D.I. might proceed:

1. A universal law of specified form characterized by the
   value of a parameter covers the phenomena. (Existence Assumption)
2. This parameter has at most one value.   (Uniqueness Assumption)
3. In a specific measured (or observed) instance of
   the law-schema the parameter has value k. (Experimental Result)

Therefore

4. A universal law of the appropriate form characterized
   by parameter value k covers the phenomena. (Specific Law)

W.E. Johnson gives a clear illustration of a simple type of D.I.:

> Every specimen of argon has some the same atomic weight.
> This specimen of argon has atomic weight 39.9.

Therefore

> Every specimen of argon has atomic weight 39.9.[2]

---

1. J. Dorling (1973), page 360.

2. Quoted from J. Dorling (1973), page 370.

Dorling reconstructs Ampère's argument as follows :

THÉORIE MATHÉMATIQUE DES PHÉNOMÈNES ÉLECTRO-DYNAMIQUES
UNIQUEMENT DÉDUITE DE L'ÉXPÉRIENCE
A.-M. Ampère 1827.

A rational reconstruction of Whittaker's rational reconstruction of Ampère's deduction
(The English quotations are from Whittaker [27], p. 85. Notice that what he describes as Ampère's experimental results are really low level generalizations from them. Unbroken arrows signify *deductive* inferences, broken arrows hypothetico-deductive inferences or inductive inferences according to your philosophical fancy.)

Force Law

Ampère's Experiments

1. $dF = dF(i, i', ds, ds', r)$ ←—experiments the force depends ←————— specific experiments unnecessary
   Under the conditions of Ampère's (perhaps only *inter alia*) on these variables.

   ←—"Expt. 1: The effect of a current is reversed when the direction of the current is reversed" ←————— actual Expt. 1

   ←—proportionality to i by definition, to i' by equality of action and reaction ←

2. $dF \propto i i' dF(ds, ds', r)$
   force is along line ←————— All forces reduce to joining current elements ·····Newtonian mechanics ←·· all physical experience
   inter-particle forces ←————"rien ne s'oppose" (central dogma)

3. $dF \propto i i' df(ds, ds', r)$
   ←————invariance under translations and rotations ←——————common experience
   ←————invariance under reflections ←———————————?

4. $dF \propto i i' df(ds.ds, ds'.ds', ds.ds', r.r, ds.r, ds'.r)$
   "Expt. 2: The effect of a current flowing in a circuit twisted into small sinuosities is the ←———— actual Expt. 2
   linearity and homogeneity in ds and ds' same as if the circuit were smoothed out"
   ←—equality of action and reaction ←—Newtonian mechanics ←·· all physical experience

5. $dF \propto i i'(A(r)(ds.ds') + B(r)(ds.r)(ds'.r))$
   All forces reduce to inter-particle forces ←—"rien ne s'oppose"
   ←— dF proportional to $1/r^2$ ←————————————?

6. $dF \propto \frac{i i'}{r^2}(a.(ds.ds') + b.(ds.r)(ds'.r))$
   "Expt. 4: The force between two elements of currents is
   ←—unaffected when all linear dimensions are increased ←— actual Expt. 4
   proportionately, the current strengths remaining unaltered"

7. $dF \propto \frac{i i'}{r^2}(a.(ds.ds') + b.(ds.r)(ds'.r))$
   "Expt. 3: The force exerted by a closed circuit on an ←—actual Expt. 3
   element of another circuit is at right-angles to the latter"

8. $dF \propto \frac{k i i'}{r^2}(2(ds.ds') - 3(ds.r)(ds'.r))$
   The sign of k is −ve if two parallel currents attract; ←—additional
   ←—Set the magnitude of $k = 1$, by an appropriate defn. of current strength   unspecified experiment

9. $dF \propto \frac{i i'}{r^2}(3(ds.r)(ds'.r) - 2(ds.ds'))$

(dF is the force exerted by circuit element ds (current strength i) on circuit element ds' (current strength i', relative position r)).

A TYPICAL CASE OF A DEDUCTIVE JUSTIFICATION OF A NEW FUNDAMENTAL HYPOTHESIS

D.I.'s of the parameter fixing type occur from 6 to 7 and from 7 to 8. In the main the other steps are D.I.'s of the form:

$$\exists n \, \forall x \, [\, Fnx \, \& \, Gx \,]$$

$$\forall x \, Fkx$$

Therefore

$$\forall x [\, Fkx \, \& \, Gx \,]$$

Notice here that the conclusion is logically equivalent to the conjunction of the premises.

I will consider three questions: Is Demonstrative Induction acceptable? Does it occur in science? and What are its merits? and I will argue that it is acceptable, it does occur, and that its main merit is that of solving problems for a research programme.

Demonstrative Induction is a valid argument form and is thus acceptable.

It occurs frequently in science. It would probably be more recognizable if we called it parameter fixing instead of Demonstrative Induction. Parameter fixing is common, for it is the main task of normal science.

What are the merits of Demonstrative Induction?

It can be an aid to discovery. A research programme usually assumes that laws have a characteristic form; its positive heuristic therefore directs the scientist to perform specific parameter fixing experiments[1]; and thus laws can be found by Demonstrative Induction.

---

1. Dorling does not state this, although it is clear that he would do so. He tends, in his less explicit moments, to reconstruct the situation as that of a scientist just merely happening to perform an experiment and then using general principles to Demonstratively Induce a general law. I think he would articulate the heuristic steps as follows. It is the general principles which direct the scientist to perform experiments, then a Demonstrative Induction is made to discover a law.

Both Ampère's law and Weber's law were discovered in this way by parameter fixing within the A.A.D. programme.

Dorling argues that it is an aid to justification -- with the proviso that Demonstrative Induction does nothing to solve the problem of induction since there are general principles among the premises. Dorling writes:

> A hypothesis is placed at a considerable advantage if it can be shown to be required by the facts provided we assume certain plausible general principles.[1]

and

> the naive hypothetico-deductivist [might] treat [formula 9] as Ampère's hypothesis and ... ignore the deductive steps which led to it. However such a construction of Ampère's theory would lead to the mistaken inference that any experiments which later threw doubt on Ampère's formula merely called into question a single rather arbitrary-looking hypothetical force formula, whereas in fact, had such an experimental refutation been devisable, it would have called into question some of the most fundamental assumptions of classical physics.[2]

and

> The importance of Weber's formula is ... that ... its experimental refutation would have called in question either the quite plausible assumptions on which Weber's deduction of it rests, or Ampère's formula and the assumptions on which that rests.[3]

So Dorling thinks that:

A D.I.'s include among their premises theoretical principles in which we have a relatively high initial confidence and from these a specific law is deduced in which our confidence is not so high;

B this means that if the particular law fails, then deeper principles in which we have more confidence are called in

---

1. Dorling (1973) page 371.

2. Dorling (1973) page 364.

3. Dorling (1973) page 366.

question;

so that,

    <u>C</u> the particular law is placed at a considerable advantage

by being tied to more general principles in which we have

more initial confidence.

I will scrutinize the notions: 'relative initial confidence',

'calling in question', and 'being placed at an advantage'.

'Relative initial confidence' is an undefined and

unexplained notion for Dorling. One might seek a concept of

absolute initial confidence, then obtain relative initial

confidence by comparing absolute initial confidences. However, I

have a proposed desideratum on relative initial confidences which

will clarify matters without introducing absolute initial

confidences:

> If $A \models B$ then one should be not less initially confident in
> B than in A.

I defend the principle on the grounds that in a valid argument if

the conjunction of the premises is true then so is the conclusion,

and even if the conjunction of the premises is not true, the

conclusion may be true. What are the relative initial confidence

relations in Demonstrative Induction? We have:

1. Existence.
2. Uniqueness.
3. Instance.
Therefore
4. Specific Law.

And $4 \models 1$, $4 \models 2$, $4 \models 3$, and $1 \& 2 \& 3 \models 4$. That is: more

confident in any of the premises individually than in the conclusion,

but equally confident in the conjunction of the premises and the

conclusion.

One result needs discussing; that is: $4 \models 2$. The

question here is whether inferences like

       Every specimen of argon has atomic weight 39.9.
Therefore
       Every specimen of argon has the same atomic weight.

                                                    are valid.

On the face of it they are, but a natural symbolization can render

them invalid for validity requires a uniqueness condition. But

atomic weights (and other parameters for that matter) are the

sorts of things that atoms (or whatever) have only one of -- that

is why scientists talk of the atomic weight of argon. So the

more proper statement 'Every atom of argon has the atomic weight

39.9' has an implicit uniqueness condition. And this also applies

to other parameters. In short, 4 $\neq$ 2 provided that the pre-

suppositions are spelled out.

          What now about Dorling's 'calling into question' notion?

What is the principle that lies behind it? Clearly it is the

following: If the conclusion of a valid argument is false, then

all the premises are 'called in question'. Is this principle

sound? If the conclusion of a valid argument is false, then the

conjunction of the premises is false or, to put it another way,

at least one premise is false. Whether this 'calls in question'

each individual premise is another matter. Intuitions suggest

that it need not. Take the example,

       2 + 2 = 4
       If 2 + 2 = 4, then 2 + 2 = 5.
Therefore
       2 + 2 = 5.

Does this valid argument with a false conclusion call in question

the arithmetical truth 2 + 2 = 4? I think not. Further, I would

be surprised if the Nobel prize were forthcoming for the scientist

who called in question Einstein's Theory of Relativity as follows:

Einstein's Theory of Relativity is true.
If Einstein's Theory of Relativity is true, then 2 + 2 = 5.
Therefore
2 + 2 = 5.

What Dorling tends to do here is to surreptitiously discard the experimental result and imagine that the deduction proceeds from only general principles ('fundamental assumptions of ... physics') with the result that failure of a law refutes a general principle.[1] He is mistaken -- the failure of a law might equally well refute the experimental statement.

Finally, what about 'being placed at an advantage'? Presumably here the advantage accrues to a hypothesis which can be Demonstratively Induced over one that cannot. There are no such advantages. All experimentally tested hypotheses can be Demonstratively Induced -- here is the prescription:

Take your specific law:
a) Existentially quantify over any parameter to obtain
your 'General Theoretical Principle'   (Existence),
b) Infer uniqueness from the uniqueness presupposition
of your specific law   (Uniqueness),
c) Infer from the specific law the practical
experimental result that you have tried.   (Experimental
                                               Result).
Then from (a), (b), and (c) Demonstratively Induce your law.
Notice that since (a), (b), and (c) individually follow from
your specific law, you must have higher initial confidence
in them than in the law (by the relative confidence
desideratum); also (a) and (b) alone do not imply the
specific law; consequently all the preconditions of a
Demonstrative Induction are satisfied.

No doubt Dorling's best response to this is to emphasize that he has strengthened his initial requirement on general principles from 'additional theoretical premises ... such that we could have more initial confidence in them than [in the conclusion of a D.I.] ' to, in the case of Ampère, 'fundamental assumptions of ... physics'. In other words, my concocted existential generalizations, although in receipt of relatively more confidence, are not fundamental enough. What then is? In the Ampère deduction it is the major

_____

1. See again the Ampère quote -- quote 2 page 93.

theoretical assumption that $dF \propto \frac{1}{r^n}$, dF here is the differential

of force between directed line segments. Is this one of 'the

most fundamental assumptions of classical physics'? There was

one precedent. The force between two magnets -- for magnets are

directed line segments; and this force was known <u>not</u> to be of

the form $dF \propto \frac{1}{r^n}$ (The dipole field was known to be approximately

inverse cube, but it was also known to be not exactly inverse

anything.)[1]

What then are my views on the justificatory role of D.I.?

First, since the experimental result can be deduced from the

specific law <u>a D.I. seems to show that the law has passed an</u>

<u>experimental test</u>. But if the law is discovered by D.I., this is

not so. The experimental result dictates the specific form of the

law and so the law does not run the risk of being refuted by it --

there is no test. Second, since the 'fundamental assumption of

physics' can be deduced from the specific law <u>a D.I. seems to show</u>

<u>that the law has passed a theoretical test</u>. The general principle

here is usually one championed by a research programme in which

case the theoretical test shows that the law is not heuristically

<u>ad hoc</u>. But since the law is usually discovered by a D.I., this

'theoretical test' is also no test.

So, when D.I.'s are used for discovery -- as they usually

are -- they play no justificatory role.

I maintain that Ampère's law may well have been discovered

by Demonstrative Induction or a process akin to it, but -- unlike

Dorling -- I do not hold that it was thereby placed at an advantage.

_____

1. Jon Dorling has satisfied me in a private communication that he
has an answer to my criticisms. It seems that my arguments exploit
an incompleteness in the expression of his ideas, rather than expose
an inherent weakness in them.

5. The Evidence for Ampère's Law and the Significance of Oersted's and Ampère's Results for the A.A.D. and Field Programmes:

Ampère's law was a great triumph for A.A.D. Maxwell, a Field theorist, writes:

> The experimental investigation by which Ampère established the laws of the mechanical action between electric currents is one of the most brilliant achievements in science. The whole, theory and experiment, seems as if it had leaped, full grown and full armed, from the brain of the 'Newton of electricity'. It is perfect in form, and unassailable in accuracy, and it is summed up in a formula from which all the phenomena may be deduced, and which must always remain the cardinal formula of electro-dynamics.[1]

The law quantitatively accounted for all known current-electric, magnetic, and electromagnetic forces, passed all the tests that it was subjected to, and predicted novel facts like that of a current-bearing helix orientating itself in the Earth's magnetic field.[2] It also immunized apparent counter-examples. For instance, in 1821 Faraday made the first conversion of electric force into continuing mechanical work with his 'electromagnetic rotations' experiments; these allegedly showed the vortex nature of electromagnetic phenomena; but Ampère pointed out that his law predicted this exact occurrence, and Faraday concurred.

Magnetism, and in particular Coulomb's law of force between magnetic poles, was brought into the reduction by means of the substitution of current shells for magnets. This is discussed further in the next section.

The statical electrical force was the only known electromagnetic force omitted from the reduction. This then was a problem to be solved.

---

1. J.C. Maxwell (1873), A Treatise on Electricity and Magnetism, § 528.

2. The demonstration of this was the favourite laboratory 'party piece' of the time.

There is a contrast here between my views and those of Dorling on the relationship between Ampère's law and the A.A.D. programme. His interest is in the epistemological strength of Ampère's law and he argues that it was fortified by being linked by Demonstrative Induction to 'fundamental assumptions' (that is, to the general principles of A.A.D.). My interest is in the epistemological strength of the A.A.D. programme and I argue that the A.A.D. programme is fortified by its ability to generate Ampère's law by Demonstrative Induction. For me, the strength that Ampère's law has -- given that it was not heuristically ad hoc -- derives only from its ability to survive experimental test.

The Field programme was not in existence when Oersted and Ampère made their discoveries, and when in existence was never able to explain Ampère's law.

---

6. Ampère's Theories of Magnetism and the Epistemological Interpretation of Them:

As has been mentioned several times, Ampère eliminated magnets as an ontological category by replacing them with equivalent current shells:[1]

The reduction leads to an important philosophical and scientific question about which Ampère and I are in disagreement with Tricker, the main secondary source, and most modern scientists.[2] The problem is that of realism versus instrumentalism.

Ampère's first theory of magnetism, just described, was in terms of macro-currents. Ampère took the theory as a realistic description of the world, and consequently had to resolve some difficulties. Ordinary currents need a source to drive them and give out heat when flowing through iron. Whereas magnets are not hot[3] and apparently have no means of supporting perpetual currents.

What Ampère did was to offer a second theory in terms of micro-currents, which again he interpreted realistically. When

---

1. The mathematics of this is given in most modern textbooks.

2. Ampère's philosophy of science is as follows. He distinguished phenomenal laws, which were proven certain truths, from hypotheses. His law of current elements was the former, whereas his theory of magnetism was the latter. Both types were interpreted as realistic descriptions of the world. I deny his distinction, but defend his interpretation.

3. At first he thought that the currents in the Earth would explain the Earth's heat, but later realized that this account would run into trouble with iron magnets.

molecules were aligned suitably these molecular currents cancelled across adjacent boundaries but yet had a resultant around the edge of the material (rather like what happens in popular proofs of Stokes's Theorem $\iint \text{curl } Y \, dA = \oint Y.dS$). These molecular currents were subject to Ampère's law, but yet were perpetual and did not give off heat. At this stage, the second theory represents a degenerate step; however it opened up a whole line of research -- that of explaining gross magnetic properties in terms of molecular currents -- which was later successful when developed by Ampère and Weber.

Tricker suggests that what Ampère should have done was to avoid the criticism by retreating into an instrumentalist interpretation:

> Though Ampère would like to go further there is, in fact, no compulsion to look upon his theory as more than an interpretation of magnetic phenomena in terms of the mutual action of electric currents and thus unifying them by means of one system. His theory actually necessitates only the adoption of the principle that magnetic materials behave, when magnetized, as though there were electric currents circulating round them ...[1]

This is bad advice and runs contrary to my view of science as an epistemological venture. I will criticize Tricker's suggestion after I have described the Biot and Savart law and Tricker's instrumentalist interpretation of that.

---

1. Tricker (1965) page 87, his italics.

7. The Origin, Validity, and Epistemological Status of the Biot and Savart Law:

Modern physicists do not use Ampère's law when calculating forces between circuits instead they use a law which was produced by Biot and Savart in the early 1820's:

$$dB = \frac{ids \times r^0}{r^2}$$

(where B is the magnetic field and r^0 denotes a unit vector)

The Biot and Savart law is formally inconsistent with Ampère's law but the two are practically equivalent in that they give the same values for fields and forces for complete circuits; (we now know that this is because they are provably equivalent for closed circuits). An unusual feature of the Biot and Savart law is that it contradicts Newton's third law. In the illustrated orientation:

A ↑   B ←

A exerts a magnetic force on B, but B does not exert a force on A.

Three problems crop up with the Biot and Savart law:

a) How could early 19th century scientists have arrived at a law which contradicts Newtonian physics?

b) Was the Biot and Savart law part of a better electro-dynamic world view than A.A.D.?

and c) How should the law be interpreted?

The historical problem turns out to be uninteresting. Biot and Savart, with some mathematical help from Laplace, 'inferred' the law from experience via a low-level experimental generalization. They placed a current-bearing straight wire in a vertical position and used a magnet suspended on a torsion pendulum to determine that the force per magnetic pole was inverse as the

distance:
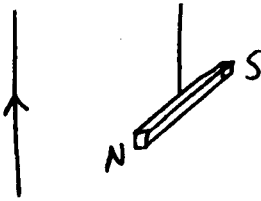
(that is, roughly $B = \dfrac{is \times r^0}{r}$)

Then Laplace told them that the required form for infinitesimals

able to integrate to an inverse-distance law was inverse-square:

$$d\vec{B} = \frac{ids \times r^0}{r^2}$$

The law was not as well tested as Ampère's one -- there

were non-uniformities and background magnetic fields which were

known to interfere.

The Biot and Savart law was just an experimental law

and was not produced as part of a research programme, further --

because of its denying that action equals reaction -- there were

good reasons for thinking it false. In other words, scientists

of the time should not have maintained that the electrodynamic

world was as Biot and Savart described it.

As to the interpretation of the law, I hold that it

should have been taken as a putative realistic account. Tricker,

in contrast reverts here to the main philosophical theme of his

(1965). He commences by telling us what Newton did:

> He [Newton] is content to work out the consequences of
> the fact that bodies behave as if they attracted each
> other by a force proportional to their masses and
> inversely proportional to the square of the distance
> between them.[1]

Then we are given the same interpretation for magnetism:

> So long as it is known that electric circuits and magnets

---

1. Tricker (1965) page 35, his italics.

behave <u>as if</u> magnetic poles existed, it would be perfectly legitimate to employ the concept in magnetic theory.[1]

And then he urges it for the Ampère and Biot and Savart laws:

It is, however, surely only sensible not to complicate out calculations unnecessarily and, so far as is known, the assumption that steady currents in closed circuits behave <u>as if</u> their constituent elements obeyed Ampère's law (in whatever form we choose to employ it) is perfectly adequate to describe the phenomena.[2]

I will take issue with Tricker in the next section.

_____

1. Tricker (1965) page 41, his italics -- see also page 87.

2. Tricker (1965) page 105, his italics. The phrase in parentheses -- 'in whatever form we choose to employ it' -- means that the Biot and Savart law is included.

8. Tricker on the 'as if' Interpretation of Ampère's Theories:[1]

I regard the best scientific theories as defensible views on the world's structure; that is, scientific theories are given epistemological weight by being taken as realistic descriptions of the world. Such a view is controversial. There are arguments that theories be interpreted instrumentally -- that scientific theories do not describe instead they are mere classificatory systems or 'rules of inference' which serve to generate the appropriate predictions. This issue burns hot in classical electrodynamics. First, because A.A.D. makes extensive use of potentials and potentials are apparently just mathematical contrivances not intended to be real and descriptive of the actual world. Second, because commentators -- Tricker is the first -- urge Instrumentalism.

This then is a philosophical issue that needs to be discussed and I do so in a general context in Appendix 2. Here I restrict myself to criticizing Tricker's precepts to Ampère.

Tricker's prescriptions have two faults: they are likely to be unfruitful, and they are difficult to apply consistently. (In addition, they conflict with the spirit of this dissertation because I hold that the aim of science should be knowledge.)

---

1. One of the intellectual forerunners of Tricker is H. Vaihinger with his (1924) The Philosophy of 'As If'. The key point about Vaihinger's work is that the admittedly false may be practically valuable (and we can all agree with him - for instance, over earth-stationary astronomy being useful for celestial navigation). But Vaihinger claims that all scientific theories are admittedly false but are none the worse for that. And Tricker has really much the same view: 'as ifs' are fictions, that is, they are factually false. So whereas I wish scientific theories to be like the world, Vaihinger and Tricker prefer them not to be like the world.

The instrumentalist restricts the problem agenda, and thus he runs the risk of excluding a fruitful problem. This is best illustrated by an example.[1] In Ptolemaic astronomy, eccentric circles and the appropriate epicycles and deferents are mathematically equivalent:

Ptolemy knew this and chose to employ the eccentric as it was simpler using one circle rather than two. The instrumentalist approves of this.[2] And Tricker would like it too. He would say that, for instance, the sun behaved 'as if' it was on an eccentric and it also behaved 'as if' it followed an epicycle and deferent; and he would continue that the scientist had free choice between the two and that any discussion of what was 'really' happening was unfruitful (and possibly meaningless). But consider the issue from a realist point of view. The two hypotheses are physically different. These circles are actually spheres and the planets are mounted on the spheres. Then the eccentric circle predicts that the planet always presents the same face to the Earth, whereas the epicycle and deferent predicts that the planet rotates presenting all faces to the Earth. So, while the instrumentalist sleeps the realist looks for evidence of rotation and perhaps even finds the moving sunspots in the case of the sun. In brief, a realist can rationally appraise these physically different hypotheses, and may make discoveries as a result. To put the whole argument as

---

1. This example is due to A.E. Musgrave.

2. See, for instance, P. Duhem (1969), <u>To Save the Phenomena</u>, Chapter 1.

two rhetorical questions on other scientific matters. How could anyone following Tricker's precepts have discovered X-ray diffraction? How could anyone following Tricker's precepts have discovered Einstein's theory of Brownian Motion?

Tricker's prescriptions are difficult to apply because they rely on a demarcation between observable and theoretical, and --- as is well known -- such a demarcation is difficult to draw. This consideration can be used to force Tricker into a solipsistic idealism. He would hold that in the world there are, for example, tables and that these behave 'as if' they were made of atoms and electrons. But should he be allowed to draw the line there? Must not he say that in the world there are sensations and that these behave 'as if' there were tables which produced them? And so on.

9. Summary:

In this Chapter, I have:

a) criticized Agassi's philosophy of the discovery of general facts,

b) refuted Agassi's historical account of Oersted's discovery,

c) described Oersted's discovery and solved certain historical problems concerning it,

d) argued that Ampère's law was produced by problem solving within the A.A.D. programme and critically discussed Dorling's account of Demonstrative Induction,

e) argued that the A.A.D. programme was massively corroborated by Ampère's law, and

f) criticized Tricker's philosophy of the 'as if'.

Chapter 3 : Early Criticism of the A.A.D. Programme : Faraday's Objections and His Foundation of the Field Programme :

1. Introduction.

2. Faraday's Criticism of Particular A.A.D. Theories.

3. Faraday's Direct Criticism of the A.A.D. Programme.

4. Faraday's Indirect Criticism : The Field Programme and Faraday's Major Discoveries.

5. Faraday's Discoveries and Their Relations to the Field Programme.

1. Introduction :

In the early 1820's, then, scientists should have held that the electrodynamic world was as it had been described by the A.A.D. programme. At this point Faraday criticized the enterprise and tried to persuade scientists that the A.A.D. account was unsatisfactory.

The main problem of this Chapter arises from this. How should Faraday's objections have been appraised ? This is a problem which has not been considered before. Historians either follow their usual practice of reporting without judging or slip into some philosophical naivety such as Pearce Williams's :

> My estimate of the relative worth of the contributions
> of Faraday and Maxwell to the development of Field theory
> will also, I suspect, meet with opposition. Here my defense
> is somewhat stronger; I have only followed Maxwell's own estimate.[1]

that is, Faraday's and Maxwell's contributions were great because Maxwell said they were ! My view on the objections is that there is no substance in them.

Once again historians have not made much sense of what Faraday was doing here and once again the M.S.R.P. tells us what to look for in his onslaught on A.A.D. The M.S.R.P. identifies two types of criticism : of particular theories in a programme, and of the programme as a whole. Faraday's strictures against specific A.A.D. theories were well made, but were confined locally -- I argue this in Section 2. Criticism of a programme as a whole can be direct

---

1. L.P.Williams (1966), The Origins of Field Theory, page x.

or indirect.   I show in Section 3 that the direct criticisms were
not damaging.   The indirect criticism consisted of the foundation
of the rival Field programme, and this is considered in Section 4.
The Field programme has to be appraised by the discoveries that it
leads to.   And Faraday made many discoveries.   However, the
discoveries were not the result of his holding the Field programme;
so at this stage the Field programme was poor and thus the indirect
criticism had no force.[1]

        This argument of mine leaves a subsidiary problem.   After all,
Faraday did make many discoveries -- if he did not make them by virtue
of the Field programme, how did he make them ?   As Helmholtz writes :

> 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.[2]

I look at this in Section 5.   There is no monolithic answer.   From
early in his scientific career Faraday held the metaphysical view
that the world was constituted of interconvertible forces.   This
view -- similar to that of Oersted, Kant, and the Naturphilosophers --

---

1. I must qualify the negative tone of what I have to say about Faraday.
I have immense admiration for Faraday, both as a person and as a scientist.
But my concern in this dissertation is solely with research programmes
and their appraisal.   My guess is that Faraday, who was always perfectly
honest and fair, would have said the following : 'At this time there is
more evidence for A.A.D. than for any other rival view.   However, I am
convinced that A.A.D. has shortcomings and cannot be right, so I have given
my life to the search for a viable alternative.   I think that I have
found that alternative in Fields.   These show great promise and I think
that with more work the balance of evidence can be tipped in their favour.'

2. H.Helmholtz (1881), 'On the Modern Development of Faraday's Conception
of Electricity', page 278.

led him to seek correlations of forces. But the view did not tell him
the conditions of transmutation and thus did not immediately direct
his research. For the same reason the metaphysical view was only
weakly confirmed when he discovered successful relationships between
forces. The metaphysics required supplementing with ideas on how the
forces were to be converted, but it was not the Field programme that
provided the subsidiary ideas. Fields played a different role.
Faraday made primary discoveries by luck or by metaphysics together
with a variety of inspirations, he then described these in his evolving
'Field' terms and made secondary discoveries concerning similar issues.
For example, although he expected magnetic forces to be able to correlate
with or produce electric forces his actual discovery of electromagnetic
induction was little more than an accident; he then described the
process of induction as one where a current is produced when lines
of magnetic force are cut by a conductor, and he then discovered other
unsuspected cases of induction. I think that the key point here is
that Faraday knew no mathematics, and the Field descriptions were his
surrogate heuristic. Faraday's life work was not that of announcing
the Field programme and then following its heuristic; rather his life
work culminated in the foundation of the programme. Faraday was great
friends with Thomson and they had lengthy discussions on the significance
of his discoveries and how best to describe them.[1] It was these that

---

1. See also Jed Z.Buchwald (1977), 'William Thomson and the Mathematization
of Faraday's Electrostatics', and Barbara Giusti Doran (1975), 'Origins
and Consolidation of Field Theory in Nineteenth-Century Britain : From
the Mechanical to the Electromagnetic View of Nature', page 163 and f.

led Faraday to articulate the hard core : <u>electromagnetic phenomena are</u> <u>the outcome of behaviour by the space between the (real or apparent)</u> <u>sources</u>, and the positive heuristic : <u>look at the intervening space,</u> <u>describe phenomena in terms of lines of force, and ...</u>

A remark should be made on the sifting of conjectures in this Chapter. I use L.P.Williams's book <u>Michael Faraday</u> critically and with caution as a source on Faraday's writings and thoughts.[1] Pearce Williams claims, probably rightly, that he is the only person other than Faraday to have <u>any idea</u> what Faraday's theories were.[2] The temptation is to become independently the second person with sound ideas on Faraday. But two considerations encourage me to resist. First, it can be seen without too much difficulty that Faraday held an unorthodox world view roughly about the convertability of <u>all</u> forces -- light, heat, sound, electrical, magnetical, chemical, gravitational, and so on; this has little to do with electrodynamics and will not be relevant to this dissertation. Second, only rarely did Faraday vent his speculations in public, so one possible guide for the historian is missing. Fortunately, Pearce Williams's book -- with two suspect areas -- is now taken as the body of historical background knowledge on Faraday's views.[3] The exceptions concern a dispute about the influence of Boscovich on Faraday and that is of no interest here[4],

---

1. The reader can be assured that I am critical where Pearce Williams's work is concerned -- substantial theses of Pearce Williams's are refuted in Chapters 2, 3, and 5 of my dissertation.

2. L.P.Williams (1975), 'Should Philosophers be Allowed to Write History', page 250.

3. B.S.Finn provides a typical reviewer's assessment : 'It is unlikely ever to be surpassed in its clear account of his work' -- ISIS (1965), page 485.

4. See J.Brookes Spencer (1967), 'Boscovich's Theory and its Relation to Faraday's Researches : An Analytic Approach' and P.M.Heimann (1971), 'Faraday's Theories of Matter and Electricity'.

and a dispute about whether it was Faraday's theories or his discoveries which came first. The latter disagreement is important. Agassi and Berkson maintain that Faraday's discoveries were predicted consequences of his theories.[1] Were this so, the M.S.R.P. deems those discoveries evidence for the theories. On the other hand, Pearce Williams holds that the theories came after the discoveries -- in which case they are not evidence. I think that what settles this is the decisive arguments given by Pearce Williams in his review article 'Should Philosophers be Allowed to Write History'.[2] The discoveries came first.

---

1. J.Agassi (1971), Faraday as a Natural Philosopher, and W.Berkson (1974), Fields of Force.

2. L.P.Williams (1975).

2. Faraday's Criticism of Particular A.A.D. Theories :

Faraday successfully attacked specific A.A.D. theories about electrolysis and Arago's disc, but without thereby discrediting the A.A.D. programme. Not every failure of an A.A.D. theory is a black mark against the A.A.D. programme -- for were it so, a critic could destroy the programme merely by arbitrarily concocting a pathetic A.A.D. theory. What is important is the relation between the theory and the programme -- whether the theory is heuristically ad hoc or whether it is heuristically generated by the programme. The theories of electrolysis were heuristically independent of the programme. The theory of Arago's disc was weakly heuristically generated, but it did not represent the straightforward A.A.D. approach.

Electrolysis -- discovered around 1800 -- was an extremely important phenomenon for it seemed to represent a point at which electrical and chemical forces were connected. It was also complex and difficult to explain. There was no natural way for A.A.D. electrodynamics to approach it simply because chemistry and chemical forces were involved. There were individual scientists who did look at electrolysis in A.A.D. terms -- Grotthuss was one, and he started a tradition of theories in which the electrolytic poles acted-at-a-distance on polarized molecules, or polarized-and-sheared molecules, in solution. Faraday refuted many of these theories,[1] and thus showed that A.A.D. electrodynamics had to that time failed to succeed in electrochemistry. This failure to succeed should not have reflected adversely on the A.A.D. programme

---

1. See L.P.Williams (1965), Michael Faraday, pages 227 and f.

since there was no reason to suppose that the programme should apply

unproblematically in this domain.   As Pearce Williams explains :

> Electrochemistry, before Faraday's researches, was in
> a state of almost total confusion.   Theoretical models
> abounded and, most importantly, the phenomena had not been
> successfully subjected to mathematical analysis.   Electrochemical
> action at a distance had only been suggested as an analogy
> with electrostatic action at a distance with the hopes that
> this would help clarify matters.   Certainly the mathematical
> physicist felt that almost anything could happen in chemistry
> which steadfastly refused to bow before the analytical powers
> of his mathematical tools.[1]

Also, once Faraday had to some extent found out what occurred

experimentally during electrolysis, the A.A.D. programme was well suited

to explaining his discoveries -- as I shall show in Section 4.

Arago discovered in 1824 that if a copper disc is spun above

a magnetic compass then the compass itself turns sluggishly following

the disc.[2]   Arago's disc involves only magnets and forces and is thus

a phenomenon that any adequate electrodynamics should explain.   I

suggest, though, that there was no clear way for the A.A.D. programme

to account for it.   Let us try to reason out a solution using the

A.A.D. techniques of 1825.   The problem involves : a) the forces and

motions as described, b) the fact that there are no forces when the

disc is at rest, and c) the fact that copper cannot be magnetized.

One must postulate sources acting between the needle and the copper,

and the choice of type of source is between 'magnetic'[3], electrostatic,

and current.   Electrostatic looks unlikely, since the magnetic compass

---

1. L.P.Williams (1965), page 283.

2. Ann.Chim.Phys. XXVIII, (1825), page 325 and see also Oeuvres Complètes Vol. IV, (1854), page 424.

3. 'Magnetic' is in inverted commas because according to the A.A.D. programme there were no magnets in the world -- 'magnets' were current shells. See my Chapter 2 Section 6.

is not charged -- unless the motion charges the compass. Magnetic

looks unlikely, since copper cannot be magnetized -- unless the

motion gives copper the ability to become magnetized.   Current

looks unlikely, since the copper has no currents -- unless the

motion creates currents in the copper.   There is no obvious path

to follow.   Two additional facts were known in the mid-1820's.

The magnitude of the effect depends on the conductivity of the disc,

and the effect can be made to disappear by cutting radial slits in

the disc.   The best hope of an explanation does seem to be currents

in the copper.

Babbage and Herschel tried to account for the disc's

behaviour using <u>induced magnetism</u> and a time lag ![1]   They were

mistaken.   Faraday later discovered electrodynamic induction and

then successfully explained Arago's disc in terms of <u>induced currents</u>.

What is the significance of Babbage and Herschel's failure

for the appraisal of A.A.D. ?   I think that the failure was a failure

only in so far as A.A.D. did not anticipate the new effect of electro-

dynamic induction.   Once induction was known as a phenomenon it fitted

in naturally with the A.A.D. programme to provide an explanation of

the disc.   Further, I argue in Chapter 4 that the A.A.D. programme

would have predicted, and then discovered, induction, if Faraday, Henry,

and Lenz had not the fortune to accidentally discover it first.

---

1. C.Babbage and J.F.W.Herschel (1825), 'Account of the Repetition of
M.Arago's Experiments on the Magnetism Manifested by Various Substances
During the Act of Rotation'.

3. Faraday's Direct Criticism of the A.A.D. Programme :

Faraday attacked the A.A.D. programme as a whole. As I mentioned in Chapter 1, he was not well qualified as a critic. Nevertheless he offered criticisms and these have to be judged in their own right. I list the major ones and then give an appraisal:

A <u>the phenomena of curved lines of force</u> -- A.A.D. has straight line central forces, whereas lines of magnetic or electrostatic force can be curved, hence electromagnetic phenomena cannot be A.A.D. Faraday writes that this was :

> <u>strong proof</u> [that induction is] an action of contiguous particles affecting each other in turn, and not an action at a distance.[1]

And Pearce Williams tells us :

> the lines of transmission of this action were curves, whereas action at a distance took place in straight lines. From this [Faraday] concluded, and was to insist upon it time and time again, that when it could be shown that force was transmitted in curved lines <u>it must be the result of the action of contiguous particles.</u>[2]

B <u>that the forces were not independent of the medium as they should have been given A.A.D.</u> Faraday discovered dielectrics and these meant that Coulomb's force law was false. Pearce Williams describes this :

> Faraday also, with astonishing calm, and almost in passing, demolished the experimental basis of electrostatics. Coulomb's law relating charge, force, and distance was discovered to be only a quite special case of the action

---

1. <u>Experimental Researches</u> § 1224, and see the references cited in the Index to Volume 3. See also Mary Hesse (1961), <u>Forces and Fields</u>, pages 198 and f., Mary Hesse (1955), 'Action at a Distance in Classical Physics' page 342, and L.P.Williams (1965) page 296.

2. L.P.Williams (1965) page 250.

of contiguous particles. If it were still to be retained, it would have to be restated in terms which took into account the nature of the medium or media through which the force was propagated. The force varied inversely as the square of the distance only under special conditions which now had to be stated explicitly.[1]

And Agassi writes :

He [Faraday] refutes Coulomb's theory of electrical action at a distance by showing how decisive is the function of the material medium in electrostatic interaction.[2]

Instead of the force being $\dfrac{m.m'}{r^2}$, it equalled $\mu \dfrac{m.m'}{r^2}$ where μ

is a function of the medium, being perhaps 1 in a vacuum but

less than 1 in air or in wax. A similar result applied to

magnetism. Mary Hesse writes :

nothing in the intervening medium had been found to affect the propagation of gravity, whereas the effect of the intervening medium was one of the main reasons for asserting the reality of the electromagnetic field.[3]

C further evidence that the medium was all important was provided

by the detailed behaviour of dielectrics. Faraday placed a

layered pile of mica discs between two charges -- this altered

the force; and when the layers were separated out the discs

were found to carry + or - charges on their surfaces.[4] Thus

the action took place in the medium and was not concerned primarily

with the source charges themselves.

---

1. L.P.Williams (1965), page 298.

2. Agassi (1971), page 234.

3. Mary Hesse (1955), page 353.

4. Diary, V.III, page 72 and f. See also W.Grove (1867), The Correlation of Physical Forces, page 108.

<u>D</u> lines of force were a property of space and not of 'sources'.

Some examples of electromagnetic induction showed that <u>the</u>

<u>line of force in space was primary and existed on its own</u>

<u>independently of its'sources'</u> -- and therefore the A.A.D.

programme, with its sources and empty space, could not explain

these cases. Faraday used two experiments here :[1]

(a) sliding contacts   (b)   sliding contacts
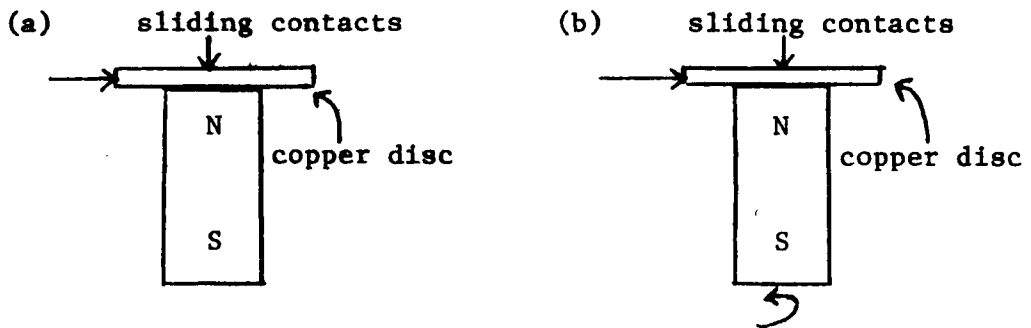
In (a) the disc and magnet are fixed together, and a current

is induced when they rotate.   In (b) the disc is held stationary

while the magnet is rotated, and no current is induced.   Faraday

used a flux-cutting explanation of induction under which a current

is induced when a conductor cuts lines of force; the magnet is

accompanied by lines of force which are either like hairs and

rotate when the magnet does or are fixed in space and do not

move when the magnet is spun; experiments (a) and (b) show that

lines of force are not 'hairs', but rather must be a property of

space.   Faraday writes :

> Thus a singular independence of the magnetism and
> the bar in which it resides is rendered evident.[2]

1. See L.P.Williams (1965), page 204 for a discussion of this.

2. L.P.Williams (1965), page 204.

And Pearce Williams informs us :

> It goes without saying that this new property of
> magnetism appeared incompatible with Ampère's theory,
> for there the magnetic forces were tied to the molecules
> of the magnet; when these molecules moved the lines of
> force had to move with them.[1]

E <u>A.A.D. violated the conservation of energy</u>. Faraday always

felt that there should be enough 'energy' locally to produce any

actions or forces. He coupled this intuition with a thought

experiment. If only one body exists, and another is then

created, the second one is immediately attracted by the first;

but there is no local energy around the second body, therefore

the conservation of energy is violated.[2]

What should have been made of these criticisms ? First,

curved lines of force. It was well known that gravitational lines of

force could be curved; and gravitational lines are structurally similar

to electrostatic lines for attracting charges. Faraday just did not

have the scientific knowledge to realize this, but the other major

scientists would have been aware of it. Besides, Ampère had just

shown that Oersted's curved lines could result from straight line

forces ! Faraday's thesis that curved lines cannot be the outcome

of straight line forces was known to be false. Second, dielectrics.

Indeed A.A.D. forces are independent of the medium, but the case is

subtle. The <u>real</u> gravitational force between two masses is independent

---

1. L.P.Williams (1965), page 204.

2. L.P.Williams (1966), page 116.   See also L.P.Williams (1965), page 458.

of the space between them, but the apparent gravitational force

between the two masses can change if further gravitating matter

is introduced into that space.   (It may be clearer to discuss

the case in terms of component and resultant forces : the component

force on a mass A due to a mass B is unaffected by the presence of

other masses, but the resultant or total gravitational force on A

depends on how many masses are present to be sources of the component

forces.)   The A.A.D. programme is unequivocal about this : the force

is unaffected by the medium, unless the medium introduces new sources

of force.   Dielectrics are new sources.   Faraday, Pearce Williams,

and Hesse are just wrong.   Listen again to Pearce Williams :

> Note the change that [Faraday's electrical theory] forces upon
> Newtonian physics.   Previously, one needed to know only
> the position and momentum of bodies to determine their future
> positions.   The forces acting upon them were assumed simply
> to act at a distance.   Now one also had to ask what the medium
> was in which these bodies existed for this affected the forces
> acting upon them.   The space between bodies had previously
> been measured merely by a mathematical line; it now became a
> physical entity to be ignored only with great risk of inaccuracy.[1]

Needless to say : Pearce Williams is mistaken; before Faraday's theory

was proposed one had to know the sources, after Faraday's theory

had been proposed one had to know the sources, nothing changes.   There

are, of course, slight differences between the gravitational and the

electrostatic cases.   With gravity, anything material in the intervening

space affects the apparent force and anything non material does not;

---

1. L.P.Williams (1966), page 87.

and nothing 'non-material' becomes 'material' as a result of its insertion. There is no real analogue of the 'creation' of sources by polarization. However, induced magnetism was well known at this time, and Poisson had given the full mathematical theory of it using magnetic fluids acting-at-a-distance; and so these subtle disanalogies were peripheral. Third, the mica discs.   These really do show that there are sources in the medium.   Not only did the A.A.D. programme predict sources, but it also had an explanation of how they worked for all that was needed was an application of Poisson's theory of polar forces to electrostatics. Faraday himself on occasions offered Poisson-type explanations of dielectrics :

> The particles of an insulating dielectric whilst under
> induction may be compared to a series of small magnetic needles...[1]

And the Field programme simply adopted the A.A.D. view on dielectrics.[2] Fourth, the induction counter-examples.   The conflict here is between Faraday's explanation of these cases and A.A.D., not between experimental reports and A.A.D. theories.   But should Faraday's explanation have been accepted ?   I think not.   First, Faraday knew full well that in general lines of force did move with the magnet, as he would have seen iron filings following a magnet moving transversely.   Second, his 'explanations' of induction were awry.   He used the now familiar flux-cutting and flux-threading explanations interchangeably.   Flux-cutting is defeated by transformer action in the case where a solenoid (with no external magnetic field, and therefore no external flux) is used

---

1. Experimental Researches §1679

2. See my Chapter 1 Section 6.

to induce a current in a surrounding wire.   Flux-threading works

here, but only by acting at a distance on the surrounding wire.   It

is doubtful whether Faraday could explain his own experiments (a)

and (b).   The dynamo, an invention of Faraday's, is a third

variation :

sliding contacts

(c)   rotating copper disc

N

stationary magnet

S
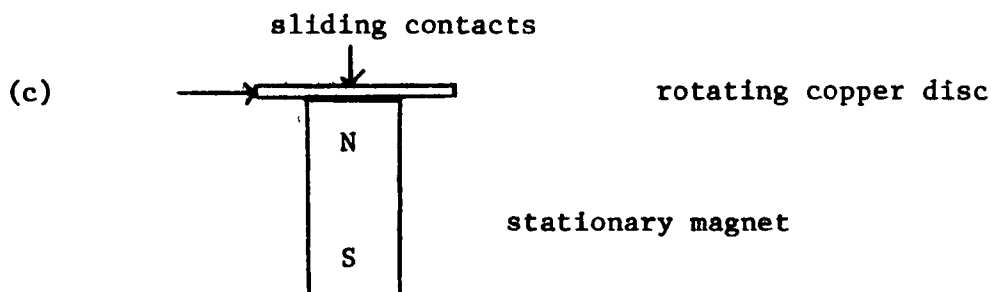
This -- the homopolar generator -- is a well-known puzzle for 'flux-

threaders' (so much so that school textbooks falsify Faraday's

discovery by giving the disc radial slots and thus converting it

into a Barlow wheel which is amenable to flux-threading.)   In contrast,

the A.A.D. theory of inter-charge forces gives a direct account of all

these oddities of induction.   Fifth,and finally, the conservation of

energy.   Faraday did not know what energy was, and his objection was

made some years before the idea of conservation of energy arose.   So

understanding him literally, which I will do first, will be unfaithful

to his intentions.   Faraday's thought experiment is weak.   If we

can suppose that a mass is created, then we can equally well suppose

that energy is created with it.   Most Newtonians would have said that

potential energy was simply a matter of configuration, then indeed

if the configuration is created the energy is created also. And

Faraday's intuitions on the necessity of local energy led into

difficulties.[1] Really, though, Faraday's objection is not about

energy. His thought experiment amounts to a plea for an explanation

of the workings of gravity together with a blind faith assertion

that none can be given. The response to it is to reject the

essentialist presupposition on which it is based. The theory

of gravity is a sound explanatory theory on its own; and as to the

explanation of gravity itself, Faraday offers no arguments that no

explanation <u>can</u> be given.[2]

My view on Faraday's criticisms is summed up by the field

theorist Thomson :

> ... [Liouville] ... asked me to write a short paper ...
> explaining the phenomena of ordinary electricity observed
> by Faraday, and supposed to be objections fatal to the
> mathematical theory [i.e. A.A.D.] . I told Liouville
> what I had always thought on the subject of these objections
> <u>(i.e. that they are simple verifications).</u>[3]

---

1. See Maxwell (1864), 'A Dynamical Theory of the Electromagnetic Field',
§ 82, my Chapter 1 Section 5, and. Chapter 1 Section 9.

2. See my Chapter 1 Section 3 for a discussion of the philosophical
objection to action at a distance.

3. Thomson's March 1845 letter to his father quoted from S.P.Thompson (1910),
<u>The Life of William Thomson</u>, Vol. 1, page 128, my italics.

4. Faraday's Indirect Criticism: The Field Programme and Faraday's Major Discoveries:

Faraday's indirect criticism amounted to the foundation of the Field programme. Towards the end of his career he proposed the thesis which became the hard core of the programme:

> electromagnetic phenomena were the outcome of behaviour by the space between the (real or apparent) sources.

At this time the heuristic of the programme was extremely weak. True, there was the general directive 'look at the intervening space', but there was no mathematical knowledge and no body of problem solving skills which could be learned and passed on. As a result, Faraday had no students, no followers, and no one thought his theories sound.

The Field programme has to be appraised by the discoveries which it led to. Faraday made five major discoveries: electrodynamic induction, the laws of electrolysis, dielectrics, the rotation of the plane of polarization of light, and diamagnetism. But they are individually either independent of the Field programme or are only weakly linked with it. Electrodynamic induction was not a complete surprise to Faraday as he thought that all forces were interconvertible; however this metaphysical view gave him no indication of the conditions of transformation. Its occurrence was unexpected and unexplained on the basis of the Field programme. The laws of electrolysis too were not expected on the basis of his theories. Further, the second law -- that electrochemical equivalents are liberated -- seemed to ask for explanation in terms of atoms of electricity. As Faraday put it:

> If we adopt the atomic phraseology, then the atoms of bodies which are equivalent to each other in their ordinary

chemical action, have equal quantities of electricity naturally associated with them.[1]

But, as I have explained, electrical fluids and atoms of electricity were foreign to his approach and he resisted this interpretation. This awkwardness over electrolysis was inherited by the Field programme. Maxwell later defined a 'molecule of electricity' and then wrote:

> This phrase, gross as it is, and out of harmony with the rest of this treatise, will enable us at least to state clearly what is known about electrolysis, and to appreciate the outstanding difficulties.[2]

This is a tactful admission that A.A.D. is more suited to explaining electrolysis than is the Field programme.[3] The existence of dielectrics was to be expected on the basis of embryonic Field ideas -- so here is evidence for the value of Faraday's methods. But the victory was limited since, as I have described, dielectrics were _explained_ by A.A.D. The rotation of the plane of polarization of light in a magnetic field was unexpected; so too were diamagnetics, although initially they were explained well by Faraday in terms of abilities to conduct lines of force.[4]

In short, in the early 19th Century the newly born Field programme was poor.

------

1. _Experimental Researches_ §869

2. J. C. Maxwell (1873) § 260.

3. Helmholtz later wrote: 'If we accept the hypothesis that the elementary substances are composed of atoms, we cannot avoid concluding that electricity also, positive as well as negative, is divided into definite elementary portions which behave like atoms of electricity'. Helmholtz (1881) page 277.

4. See L.P. Williams (1965) p438f. It is to be noted though that most of the progressive work in the 19th Century on the magnetic vagaries of materials resulted from the Ampere - Weber tradition using electrons, currents, and orientation of micro-currents.

5. Faraday's Discoveries and Their Relation to the Field Programme:

In this section I look at how Faraday made his discoveries.

Faraday discovered electrodynamic induction while experimenting with a powerful electromagnet made to a design of Henry. I think that there was nothing magnificent about this, since Henry and Lenz -- both A.A.D. theorists -- discovered the effect at more or less the same time. Now our knowledge of the immediate background to Faraday's experiments is extremely hypothetical. Pearce Williams warns:

> The sources ... are extremely meagre. The attempt is to reconstruct Faraday's thoughts from the hints thrown out in the laboratory diary and follow him as he struggled towards success ... This account contains a good deal more conjecture than is desirable. The result is a coherent tale, but perhaps not the only nor the correct one; unfortunately, it seems unlikely that new evidence will be uncovered and we must make do with what we have.[1]

Pearce Williams' story is this: Faraday held that currents consisted of an oscillatory wave which was not specifically confined to its conductor ; he had observed _accoustical_ induction where one vibrating object starts another body oscillating; hence he expected electrodynamic induction. Let us accept the story. Then, first, the key to induction lies in the vibrating currents and not in the medium or field; that wires interact via their electric, magnetic, or electromagnetic forces was well known since Coulomb, Ampère, and Oersted; so it was not the interaction that was important, it was the _vibration_. And second, induction should occur with steady currents, _which it does not_. So, if induction was discovered this way, it was the refutation of a view about currents!

---

1. L.P. Williams (1965) page 169.

The laws of electrolysis were discovered as a result of research on the identity of electricities.[1] There was the question of whether current electricity, produced by a cell, and a flow of initially static electricity were the same. Faraday tried to find out if all known effects of the one could be produced by the other, and eventually found that they could. One property that the electricities had was that of deflecting a galvanometer needle a set amount according to their quantity. And voltaic electricity could be used to decompose electrolytes. Faraday considered whether flow of static electricity could decompose electrolytic solutions and whether the amounts produced by decomposition were a function of the quantities of electricity; and thus he discovered the laws of electrolysis. Again, this has no relation with Fields.

Dielectrics were discovered as a result of paying attention to the intervening space. Faraday had early Field-style explanations of electrolysis and suspected correctly that substances could be polarized without shearing.[2]

---

1. L.P. Williams (1965), pages 211 and f.

2. Actually, it was the A.A.D. theorists who first predicted the existence of dielectrics. One problem for A.A.D. was that of keeping the source fluids with their conductors; one answer was the theory that air was an insulator and then fluids stayed with the conductor due to hydrostatic pressure; further it was this pressure that caused the electrostatic force; then it was reasoned that if air was replaced by another substance less impermeable to electrical fluids, the electrostatic force must change. See R. Murphy (1833) Elementary Principles of the Theories of Electricity, Heat and Molecular Action.

The rotation of the plane of polarization of light by a
magnetic field was discovered by luck. Faraday thought of electro-
static lines of force as a stress or line of polarization in the
medium, and it was well known that mechanical stress on glass
rotates the plane of polarization of light passing through it. So
Faraday, encouraged by Thomson, sought to find his electrostatic
stress by investigating glass and polarized light. He failed to
find it. Having failed, he switched from electric lines to magnetic
lines produced by a powerful electromagnet. Lo and behold, there
was the rotation! This was pure luck. Pearce Williams disagrees,
he says that electric forces were weak, perhaps too weak, but electro-
magnets exerted immense forces so it was natural to try them after
the initial failure.[1] This is not so. Consider the theories.
Electric lines are stresses and glass is a dielectric so electric
lines should stress glass and rotate polarization. Magnetic lines
-- under Faraday's theory -- were not mechanical stresses.
Magnetic lines were non-divergent, there was no 'free' pole, and so
the lines were nothing to do with mechanical stresses. Further,
glass was not a magnetic material and so could not have been stressed
by magnetic lines even if magnetic lines had been stresses.
(Diamagnetism was not known at this time.) So, since the glass was
not under stress, background knowledge plus Faraday's theories
predict no rotation. So, if anything, this discovery was a refutation
of Faraday's theories on the nature of lines of force.

These rotations pointed the way to the discovery of dia-
magnetism. The rotations showed that magnets or magnetic fields
could affect (apparently) non-magnetic materials. It was but a short

---

1. See 'Faraday' article, Dictionary of Scientific Biography,
   page 538. And L.P. Williams (1965) page 386.

step from there to finding diamagnetism

      To sum up. With the exception of dielectrics, none of Faraday's discoveries were evidence for the Field programme.

Chapter 4 : The Development of the A.A.D. Programme 1830-1860 :

Weber's Unification of Electrodynamics and Other Theoretical Advances.

1. Introduction.

2. Sources and Receivers of Force.

3. Gauss -- The Unification of Electrodynamics and the
   Retardation of Forces.

4. Weber's Deduction of His Law.

5. The Significance of the Law for the A.A.D. Programme.

6. Criticisms of the Law and Their Evaluation.

7. Riemann's Attempt to Deduce Weber's Law from a Propagated
   Force Law.

8. Electrical Actions Propagated at the Speed of Light.

9. Summary.

1. Introduction :

By 1830, the A.A.D. programme contained three prominent
theses :

1. That static electricity was governed entirely by Coulomb's
   A.A.D. law.

2. That, in a manner of speaking, magnets did not exist; instead
   there were currents which produced the effects.

3. That current electricity (and thus magnetism) was governed
   entirely by Ampère's A.A.D. law.

During the following twenty years Weber and his colleagues
developed these three into five replacement theses :

1'. That electricity is atomic in structure.

2'. That currents are streams of electrical 'atoms'.

3'. That the forces acting operate directly between electrical
    atoms and not between conductors.

4'. That the forces acting do not do so instantaneously.

5'. That all electrodynamic phenomena may be deduced, by statistical
    summation, from a force formula applied to electrical atoms.

This chapter looks at these advances.

A few remarks are in order on the origins and advantages of
these theories.

1' results from the common association of Newtonianism and
atomism and it has the merit, as I explained in Chapter 3, of being
in harmony with Faraday's laws of electrolysis.

2' is a routine development of 1' and background knowledge
on voltaic and frictional currents. The principal scientists involved

were Ohm, Fechner and Kirchoff. In the mid-1820's Ohm offered his application of Fourier conduction-analysis to currents in conductors - the work was almost entirely ignored.[1] The one man to take an interest was Fechner -- later to become Weber's colleague at Leipzig. Fechner carried out the experiments and favoured Ohm's theoretical analysis.[2] Ohm once wrote:

> My theory has found in him [Fechner] alone, if I am not mistaken, a very gallant defender; and I have found into the bargain an honest friend...[3]

The main difficulty with Ohm's theory was in identifying the physical counterpart of the mathematical 'electric tension' function -- that is, the question was what is the true electrical analogue of 'temperature' used in the Fourier heat case. In 1849 Kirchoff identified 'electric tension' with Poisson's electrostatic potential function V, and this completed his earlier extension of Ohm's theory to three dimensions.[4] Thus the advanced A.A.D. theoretical electrostatics of Poisson (and George Green, and others) became linked with Ohm's theory of electric circuits and conductors. Needless to say the Ohm-Fechner-Kirchoff analysis fared well when tested by experiment. Ohm's research remained unknown to the Field theorists in England for near twenty years. But Weber and Gauss used it as early as 1833 for their research on terrestrial magnetism and for their construction of electromagnetic instruments.[5]

---

1. See R. Taylor (1841), Scientific Memoirs, Vol II, page 401 for a translation of one of Ohm's papers and see also Morton L. Schagrin (1963), 'Resistance to Ohm's Law'.

2. See, for instance, G.T. Fechner (1831), Massbestimmungen uber die galvanische Kette.

3. Quoted from H.J. Winter (1944), 'The Reception of Ohm's Electrical Researches by His Contemporaries', page 378, my italics.

4. Ann. de Phys. LXXV (1848) page 189 and Ann.de Phys. LXXVIII (1849) page 1.

5. Actually, Gauss derived most of the network analysis results twenty years before Kirchoff. See Schaefer (1931), 'Gauss's Investigations on Electrodynamics', page 340.

Ohm's theories themselves in no way stem from the A.A.D. programme; however, the Ohm-Fechner-Kirchoff analysis placed the A.A.D. theorists in a strong position to unify electrodynamics. The analysis leaves open the mechanism of flow in ordinary circuits, excepting that conductors carrying voltaic currents have to be electrostatically neutral. Fechner did some further theoretical work using one of the three possible assumptions: the voltaic currents consist of equal and opposite flows of positive and negative electrical fluids.

3' arises from the attempt to unify static and current electricity. I discuss it in Section 2.

4' was developed by Gauss, Weber's fellow researcher at Gottingen, and Riemann, who was Gauss's student. One point to note is that the retardation of forces also suggests a revision in the Newtonian central force assumption. If a force takes time to travel across space, then it becomes an open question as to whether the force should act along the line joining the particles when the forces set out, or along the resultant of the directions of the retarded forces, or along some other direction. This important area of retarded forces is considered in Sections 3 and 7.

5' is the outcome of Gauss's and Weber's approach to electro-dynamics using a force law between electrical atoms. It is discussed in Sections 3 and 4.

The major problem facing electrodynamics in the early 1830's was that of explaining electrodynamic induction. This new effect was unexpected, except under the metaphysical view that all forces were inter-convertible, and it manifested itself in a myriad of forms including Arago's disc, dynamos, and self and mutual induction.

The problem, being new, was not on the agenda of either the Field or the A.A.D. programmes. However the A.A.D. programme did have

a pressing problem -- that of unifying static and current electricity

by combining Coulomb's and Ampère's laws -- and it did have a

prescription for a solution -- analyse current elements, then combine

with charge fluids.

Weber solved this problem by deducing a force law from

Ampère's law and a reasonable analysis of currents -- as I shall show

in Section 4. Weber's law superceded Ampère's and Coulomb's laws. It

also predicted that there should be electrodynamic induction -- so, if

Faraday had not discovered induction, Weber would have done so. The

existence of induction was good evidence for Weber's law, and in turn

the A.A.D. programme. The most conservative claim that I make here is

that Weber's law, when it was proposed in 1846, accounted for all known

electrodynamic phenomena.

Far from being well received, Weber's law was subjected to

severe criticism which apparently damned it for once and for all. I

will show in Section 5 that not only was this criticism without foundation

but also that either it was known to be false or should have been known

to be false when first offered.

The A.A.D. heuristic suggested that Weber's law should be

replaced by a retarded force law. This line was followed by Riemann

and is discussed in Section 7.

2. Sources and Receivers of Force :

The thesis 3' -- that the forces acting operate directly between electrical atoms and not between conductors -- was naturally assumed to be necessary for relating the laws of Coulomb and Ampère.[1] And as a result of 3', the A.A.D. programme took a new view of a standard distinction. It was usual to distinguish ponderomotive forces, which act between current carrying conductors, from electromotive (which were often inductive) forces, which act on a current in a conductor. The A.A.D. theorists regarded this distinction as spurious -- only the one 'electric' force was needed.

Maxwell, on behalf of the Field programme, specifically denied 3' :

> It must be carefully remembered, that the mechanical force
> which urges a conductor carrying a current across the
> lines of magnetic force, acts, not on the electric current,
> but on the conductor which carries it.[2]

However, 3' was a theory which was consistent with the known properties of electro-mechanical interaction and which steadily produced novel facts in the years following its proposal. Everyone knew that conductors are the receivers of magnetic force only if they are bearing currents, and that a conductor bearing a current is affected in direct proportion to the strength of that current while the size and material of the conductor is a matter of indifference. And the bending of a spark discharge by a magnetic field,[3] and the properties of

---

1. See, for example, Weber (1848), 'On the Measurement of Electrodynamic Forces', page 511.

2. J.C.Maxwell (1873), A Treatise on Electricity and Magnetism, § 501.

3. Davy discovered this in 1821 -- see Phil.Trans. cxi, (1821), page 425.

fluorescent discharge -- both known to Faraday -- become clearer under 3'.

Then there were the truly novel discoveries such as the Hall effect.

Let me quote again from Maxwell :

[the distribution of currents in conductors is independent of magnetic forces.]

The only force which acts on electric currents is electromotive force ...

To this J.J.Thomson added in 1891 the editorial revision :

Mr.Hall has discovered .... in [1880] ... a steady magnetic field does slightly alter the distribution of currents in most conductors, so the statement ... must be regarded as only approximately true.

In real English, 'only approximately true' means 'false'.

Hall himself describes the matter :

Sometime during the last University year, while I was reading Maxwell's 'Electricity and Magnetism' in connection with Professor Rowland's lectures, my attention was particularly attracted by the following passage in vol ii p. 144 :-
'It must be carefully remembered ... [etc.] ...'
This statement seemed to me to be contrary to the most natural supposition in the case considered ....
Soon after reading the above statement in Maxwell I read ... in which the author evidently assumes that a magnet acts upon a current in a fixed conductor ...
Finding these two authorities at variance, I brought the question to Prof. Rowland. He told me he doubted the truth of Maxwell's statement ...
I ... hit upon a method that seemed to promise a solution to the problem ...

A.A.D. electrodynamics, even in the form it was with Weber in the 1840's, predicts the Hall effect.

---

1. Maxwell (1873), § 501.   My italics.

2. Maxwell (1873), § 501.

3. E.H.Hall (1879), 'On a New Action of the Magnet on Electric Currents'.

The analysis 1'-3' as a whole predicted novel results, such as the outcome of Rowland's convection current experiments. Rowland explains :

> The experiments described in this paper were made with a view of determining whether or not an electrified body in motion produces magnetic effects. There seems to be no[1] theoretical ground upon which we can settle the question ....

Rowland was mistaken -- the A.A.D. programme predicted that there would be 'magnetic' effects. But Helmholtz -- ever the vigourous critic of A.A.D. electrodynamics -- was aware of the theoretical relations :

> I understand by electric convection the conveyance of electricity by the motion of its ponderable bearers. In my last memoir on the theory of electrodynamics, I proposed some experiments (which were then carried out by Herr N.Schiller) in which the question came into consideration whether electric convection is dynamically equivalent to the flow of electricity in a conductor, as W.Weber's theory assumes. Those experiments might possibly have been decisive against the existence of such an action. They were not so; but, on the other hand, through this negative result the existence of the action in question remained unproved. Mr.Rowland has now carried out a series of direct experiments, in the physical laboratory of the University here, which give positive proof that the motion of electrified ponderable substances is also electro-magnetically operative.[2]

And he sums up :

> As regards the signification of these experiments for the theory of electrodynamics, they correspond to the hypotheses of the theory of W.Weber; but they can also be referred to Maxwell's or to the potential-theory which takes account of the dielectric polarization of the insulators.[3]

That is : the convection current experiments start life as an intended crucial experiment against Weber's theory, but when the theory

1. H.A.Rowland (1878), 'On the Magnetic Effect of Electric Convection'.

2. H.Helmholtz (1876), 'On the Electromagnetic Action of Electric Convection', page 233.

3. Helmholtz (1876), page 237.

passed the test Helmholtz argued that the Field theories can be modified so as to entail the result. He concluded that the convection current experiments become confirming instances of the Field theories also. The M.S.R.P. imposes an entirely different appraisal on the same theoretical relations -- the experiments are evidence for the A.A.D. view but they are not evidence for the Field view.

There were also genuine experimental difficulties which appeared to beset 1'-3'. These too resulted in the discovery of novel facts. Maxwell pointed out that the entire analysis faced anomalies : accelerating conductors should exhibit inertial effects such as 'inertial currents', but no such effects were known.[1] However, mere anomalies do not affect the appraisal of a research programme, and even Maxwell acknowledged that the expected effects were small. Later, as experimental methods became more refined, all of Maxwell's predictions, derived from A.A.D. views, were discovered to occur.[2] The anomalies became triumphant confirming instances.[3]

---

1. Maxwell (1873) § 574-577.

2. See S.J.Barnett (1933), 'Gyromagnetic Effects : History, Theory, and Experiments'.

3. 3' also led to fruitful research outside the strict domain of electrodynamics. Weber worked extensively on the magnetic, electrical, and thermal properties of materials. In particular he tried to explain electrical resistance in terms of lattice molecular models. Weber's own research was not notably successful, but Weber's assistant Eduard Riecke developed the electron theory of metals and this approach led to the Drude-Lorentz electron gas models of conduction.

3. Gauss -- The Unification of Electrodynamics and the Retardation of Forces:

Gauss developed one key suggestion of the A.A.D. heuristic and he highlighted another. The first was to unify electrodynamics by rationally guessing a force law between electrical particles which would link Coulomb's and Ampère's laws. Gauss produced such a force law in 1835.[1] The law, in addition to providing the link, explained some but not all the cases of electrodynamic induction -- so it predicted novel facts. Gauss, though, regarded the law as provisional and to be replaced. He thought it temporary because of the second key idea: that A.A.D. inverse distance forces should propagate in space and not be instantaneous:

> I would doubtless have published my research long ago, if only, at the time I interrupted my work, what I considered to be the cornerstone had not still been missing.
>
> Nil actum reputans si quid superesset agendum [unfinished work counts for nothing]
>
> And by that I mean the derivation of the additional forces (which are in addition to the reciprocal effect of passive electric particles, when they are in relative motion) from the effect which is not immediate, but (in a similar manner as with light) which propagates in time. I did not manage to do this then: but as far as I remember, I turned away from the investigation at that time not entirely without hope, that I would perhaps be successful later, although - if my memory is correct -- with the subjective conviction, that it would be first necessary to arrive at a constructible representation how this propagation takes place.[2]

---

1. Gauss (1867), Werke, Vol. V, page 616. The law was:

$$F = \frac{q_1 q_2}{r^2} \left[1 + \frac{1}{c^2} \left[u^2 - \frac{3}{2} \left(\frac{dr}{dt} \right)^2 \right] \right]$$

where u is the relative velocity of the two 'charges' and r their distance apart.

2. Letter to Weber, Werke, V, page 629. Translated by Professor E.W. Herd. The German is convoluted. The Latin is a misquote of Lucan's description of Julius Caesar which emphasizes his demonic energy. It means literally: considering that nothing had been done, if anything remained to be done.

Gauss devoted most of his work in electrodynamics to appraising the consequences of a propagated force.

It is fair to say that the A.A.D. heuristic did not originally contain the strict instruction to retard the forces so that they propagated. But all A.A.D. theorists -- working on gravity or on electrodynamics -- thought that instantaneous propagation was impossible.[1] All that was required was for someone to transform the underlying assumption 'instantaneous propagation is impossible' into the heuristic hint 'evaluate the consequences of a finite propagation'. It was Gauss who provided that service for electro-dynamics.

Gauss's ideas were to remain unpublished. In his eyes they were incomplete -- he was an essentialist and regarded the fact that he had not been able to explain the propagation itself as a shortcoming.[2] However, published or not, Gauss's ideas were influential. Weber, Riemann, and others knew both that Gauss regarded electrodynamics as being governed by a propagated force acting between particles, and that Gauss had been to some extent successful in these researches.

Gauss's deduction of his law was the pattern that Weber followed in making his derivation. I give a full account of Weber's derivation in Section 4, and point out there the alterations needed to obtain Gauss's law.

---

1. In connection with this, see also my arguments in Chapter 1 Section 2.

2. See the letter to Weber quoted earlier.

Gauss did one other important piece of research in electro-
dynamics. He showed in 1835 that the phenomena of electrodynamic
induction could be described mathematically in terms of the rate of
change of a (nowadays the) vector potential A. This result is
generally attributed to Franz Neumann who published it in 1845.[1]
This mathematical description enables us to see that Weber's law
does indeed yield all the types of electrodynamic induction -- this
is discussed further in Section 5.

---

1. Berlin Abhandlungen. (1845) page 1. F.E. Neumann produced
several mathematical results of value to the A.A.D. programme -- the
most important one was the connecting induction with variation in
a vector potential. He first used Lenz's law to arrive at a flux-
cutting description of electromagnetic induction for the case of a
conductor moving in a magnetic field. He then considered the
electrodynamic potential of two closed circuits under Amperian forces
and found that the variations of this potential would yield an
account of induction. In turn, this closed circuit potential could
be split up into a 'vector potential' at a point of one circuit
due to the other circuit. Thus mathematically induction was a
function of variations in vector potential. Neumann was an A.A.D.
theorist -- for him electrodynamics was about direct action at a
distance -- but yet his work was away from the main line of
development. He analyzed complete circuits, yet microanalysis of
currents or current elements was required to link the laws of
Coulomb and Ampere.

4. Weber's Deduction of His Law:

My aim in this section is to show that, given the problem situation and the problem solving techniques, A.A.D. naturally led to Weber's law:

$$F = \frac{q_1 q_2}{r^2} \left[1 - \frac{1}{c^2} \left[\left(\frac{dr}{dt}\right)^2 - 2r \frac{d_2 r}{dt^2} \right] \right]$$

where r is the distance between charges, c is a constant of proportionality,[1] q_1 and q_2 are the charge magnitudes, and Coulomb's law for static charges has been added. Weber's theory was the third and most complete in a sequence of A.A.D. attempts to solve the problem by substituting for the trigonometrical functions in Ampère's law. Gauss's law of 1835 was the first. Fechner's theory of 1845 was the second.[2] And both of these predicted some, but not all, cases of induction.

The heuristic generation starts with Ampère's law and Fechner's account of currents and proceeds by deduction. At one point an obvious guess has to be made -- so that while not logically inevitable the process may be described as being heuristically inevitable. Weber himself claims a necessary and sufficient link between his law and Ampère's law.[3] This claim is false, but could justifiably have been thought to be true in the days before modern logic.

---

1. c appears because the force law combines Coulomb's force law for static charges, which uses one system of units, with Ampère's law for moving charges, which uses a different system. c, the ratio of one unit to the other, was known to have the dimensions of a _velocity_. There is the further inessential complication that there were two units for currents, one being $\sqrt{2}$ as big as the other. The result is that many of the historical equations have apparently mysterious factors of 2 which appear due to the units being switched. It was important for Weber to determine c but there were technical difficulties which prevented him (and Kohlrausch) from succeeding until 1855. Their value for c was $\sqrt{2}$ times the speed of light.

2. G.T. Fechner, _Ann de Phys & Chim_, _64_ (1845), pp.337-345.

3. I quote this claim at the end of my account of Weber's derivation.

The deduction runs as follows[1]

Weber starts with Ampère's law for the case where the elements are in the same plane:[2]

$$dF = - \frac{ids\ i'ds'}{r^2}\ [sin\ \theta\ sin\ \theta' - \frac{1}{2}\ cos\ \theta\ cos\ \theta'] \qquad (1)$$

using the notation of Chapter 2.

Partial derivatives are substituted for the trigonometrical functions

$$cos\ \theta\ =\ (\frac{\partial r}{\partial s})_{s'}\ =\ \frac{\partial r}{\partial s}$$

$$cos\ \theta'\ =\ -\ (\frac{\partial r}{\partial s'})_{s}\ =\ -\ \frac{\partial r}{\partial s'}$$

$$sin\ \theta\ sin\ \theta'\ =\ r\ \frac{\partial_2 r}{\partial s\ \partial s'}\ \ (because\ \ \frac{\partial}{\partial s}\ cos\ \theta'\ =\ -\ sin\ \theta'\frac{\partial \theta'}{\partial s}\ \ and$$

$$-\ r\ \delta \theta'\ =\ \delta s\ sin\ \theta\ ,\ \ therefore$$

$$r\ \frac{\partial}{\partial s}\ (\frac{\partial r}{\partial s'})\ =\ -\ sin\ \theta'\ sin\ \theta).$$

so that:

$$dF\ =\ -\ \frac{ids\ i'ds'}{r^2}\ \left[\frac{1}{2}\ \frac{\partial r}{\partial s}\ \frac{\partial r}{\partial s'}\ -\ r\ \frac{\partial_2 r}{\partial s\ \partial s'}\right]\ \ldots \qquad (2)$$

---

1. I reconstruct Weber (1848) 'On the Measurement of Electrodynamic Forces'. I have changed and modernized notation for clarity -- any more substantial alterations are indicated in footnotes. Weber (1848) is a translation of his German (1848), which in turn is a short version of his (1846).

2. Actually Weber uses:
$$dF = - \frac{ids\ i'ds'}{r^2}\ [cos\ t\ -\ \frac{3}{2}\ cos\ \theta\ cos\ \theta']$$
t here is the angle of intersection of the extension of the positive flow of the current elements. That is, $t = (\theta - \theta')$ and cos t = cos θ cos θ' + sin θ sin θ', so the two expressions are identical.

There are two 'currents' in each wire; if λ and λ' are the linear charge densities and v and v' the velocities of the charges, then:

$$i_1 = \lambda v$$
$$i_2 = -\lambda v$$
$$i_3 = \lambda' v'$$
$$i_4 = -\lambda' v'$$

There are four current pairs between the wires which provide forces - name the forces dF_1, dF_2, dF_3 and dF_4.

The line elements ds and ds' now become split in half: ds_1 and ds_1' are the positive current line elements and ds_2 and ds_2' are the negative current line elements.

Applying equation (2) to each force pair:

$$dF_1 = -\frac{\lambda v\,\lambda v'\,ds_1\,ds_1'}{r_1^2}\left[\frac{1}{2}\frac{\partial r_1}{\partial s_1}\frac{\partial r_1}{\partial s_1'} - r_1\frac{\partial_2 r_1}{\partial s_1\partial s_1'}\right] \quad \ldots (3)$$

$$dF_2 = +\frac{\lambda v\,\lambda v'\,ds_1\,ds_2'}{r_2^2}\left[\frac{1}{2}\frac{\partial r_2}{\partial s_1}\frac{\partial r_2}{\partial s_2'} - r_2\frac{\partial_2 r_2}{\partial s_1\partial s_2'}\right] \quad \ldots (4)$$

$$dF_3 = -\frac{\lambda v\,\lambda v'\,ds_2\,ds_2'}{r_3^2}\left[\frac{1}{2}\frac{\partial r_3}{\partial s_2}\frac{\partial r_3}{\partial s_2} - r_3\frac{\partial_2 r_3}{\partial s_2\partial s_2'}\right] \quad \ldots (5)$$

$$dF_4 = +\frac{\lambda v\,\lambda v'\,ds_2\,ds_1'}{r_4^2}\left[\frac{1}{2}\frac{\partial r_4}{\partial s_2}\frac{\partial r_4}{\partial s_1} - r_4\frac{\partial_2 r_4}{\partial s_1\partial s_2'}\right] \quad \ldots (6)$$

At the moment the electrical masses referred to are all in ds and ds',

$$r_1 = r_2 = r_3 = r_4 \left(= r\right)$$

so the total force is, from adding (3) - (6)

$$dF = -\frac{\lambda v\,\lambda v'\,ds\,ds'}{4r^2}\left[\frac{1}{2}\left[\frac{\partial r_1}{\partial s_1}\frac{\partial r_1}{\partial s_1'} - \frac{\partial r_2}{\partial s_1}\frac{\partial r_2}{\partial s_2'}\right.\right.$$

$$\left.+ \frac{\partial r_3}{\partial s_2}\frac{\partial r_3}{\partial s_2'} - \frac{\partial r_4}{\partial s_2}\frac{\partial r_4}{\partial s_1'}\right]$$

$$\left.- r\left[\frac{\partial_2 r_1}{\partial s_1\partial s_1'} - \frac{\partial_2 r_2}{\partial s_1\partial s_2'} + \frac{\partial_2 r_3}{\partial s_2\partial s_2'} - \frac{\partial_2 r_4}{\partial s_1\partial s_2'}\right]\right] \quad \ldots (7)$$

Now, r_1 *is a function of* s_1 *and* s_1'

$$S_1 \nearrow \vert \underbrace{\hspace{4cm}}_{r_1} \vert \nwarrow \uparrow S_1'$$

so $\quad \dfrac{dr_1}{dt} = \dfrac{\partial r_1}{\partial s_1}\dfrac{\partial s_1}{\partial t} + \dfrac{\partial r_1}{\partial s_1'}\dfrac{\partial s_1'}{\partial t}$

but $\dfrac{\partial s_1}{\partial t}$ *is* v *and* $\dfrac{\partial s_1'}{\partial t}$ *is* v'

therefore $\quad \dfrac{dr_1}{dt} = v\dfrac{\partial r_1}{\partial s_1} + v'\dfrac{\partial r_1}{\partial s_1'}$ $\qquad \ldots (8)$

and similarly

$$\dfrac{dr_2}{dt} = v\dfrac{\partial r_2}{\partial s_1} + v'\dfrac{\partial r_2}{\partial s_2} \qquad \ldots (9)$$

$$\dfrac{dr_3}{dt} = v\dfrac{\partial r_3}{\partial s_2} + v'\dfrac{\partial r_3}{\partial s_2'} \qquad \ldots (10)$$

$$\dfrac{dr_4}{dt} = v\dfrac{\partial r_4}{\partial s_2} + v'\dfrac{\partial r_4}{\partial s_1'} \qquad \ldots (11)$$

Further

$$\dfrac{d_2 r_1}{dt^2} = v^2\dfrac{\partial_2 r_1}{\partial s_1^2} + 2vv'\dfrac{\partial_2 r_1}{\partial s_1 \partial s_1'} + v'^2\dfrac{\partial_2 r_1}{\partial s_1'^2} \qquad \ldots (12)$$

$$\dfrac{d_2 r_2}{dt^2} = v^2\dfrac{\partial_2 r_2}{\partial s_1^2} + 2vv'\dfrac{\partial_2 r_2}{\partial s_1 \partial s_2'} + v'^2\dfrac{\partial_2 r_2}{\partial s_2'^2} \qquad \ldots (13)$$

$$\dfrac{d_2 r_3}{dt^2} = v^2\dfrac{\partial_2 r_3}{\partial s_2^2} + 2vv'\dfrac{\partial_2 r_3}{\partial s_2 \partial s_2'} + v'^2\dfrac{\partial_2 r_3}{\partial s_2'^2} \qquad \ldots (14)$$

$$\dfrac{d_2 r_4}{dt^2} = v^2\dfrac{\partial_2 r_4}{\partial s_2^2} + 2vv'\dfrac{\partial_2 r_4}{\partial s_2 \partial s_1'} + v'^2\dfrac{\partial_2 r_4}{\partial s_1'^2} \qquad \ldots (15)$$

Now, $\dfrac{\partial_2 r_1}{\partial s_1^2} = \dfrac{\partial_2 r_2}{\partial s_1^2} = \dfrac{\partial_2 r_3}{\partial s_2^2} = \dfrac{\partial_2 r_4}{\partial s_2^2} \left(= \dfrac{\partial_2 r}{\partial s^2}\right)$

because they depend merely on the position and form of the first

conducting wire. Similarly for $\frac{\partial_2 r}{\partial s'_2}$

Therefore from (12) - (15)

$$\frac{d_2 r_1}{dt^2} - \frac{d_2 r_2}{dt^2} + \frac{d_2 r_3}{dt^2} - \frac{d_2 r_4}{dt^2} = 2vv'\left[\frac{\partial_2 r_1}{\partial s_1 \partial s_1'} - \frac{\partial_2 r_2}{\partial s_1 \partial s_2'}\right.$$

$$\left. + \frac{\partial_2 r_3}{\partial s_2 \partial s_2'} - \frac{\partial_2 r_4}{\partial s_2 \partial s_1'}\right] \quad \dots (16)$$
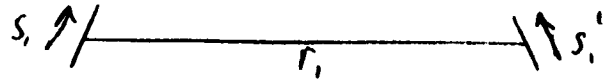
and squaring (8) - (11)

$$\frac{dr_1}{dt}^2 = v^2 \frac{\partial r_1}{\partial s_1}^2 + 2vv' \frac{\partial r_1}{\partial s_1}\frac{\partial r_1}{\partial s'} + v'^2 \frac{\partial r_1}{\partial s'} \quad \dots (17)$$

$$\frac{dr_2}{dt}^2 = v^2 \frac{\partial r_2}{\partial s_1}^2 + 2vv' \frac{\partial r_2}{\partial s_1}\frac{\partial r_2}{\partial s_2'} + v'^2 \frac{\partial r_2}{\partial s_2'} \quad \dots (18)$$

$$\frac{dr_3}{dt}^2 = v^2 \frac{\partial r_3}{\partial s_2}^2 + 2vv' \frac{\partial r_3}{\partial s_2}\frac{\partial r_3}{\partial s_2'} + v'^2 \frac{\partial r_3}{\partial s_2'} \quad \dots (19)$$

$$\frac{dr_4}{dt}^2 = v^2 \frac{\partial r_4}{\partial s_2}^2 + 2vv' \frac{\partial r_4}{\partial s_2}\frac{\partial r_4}{\partial s_1'} + v'^2 \frac{\partial r_4}{\partial s_1'} \quad \dots (20)$$

Now $\quad \dfrac{\partial r_1}{\partial s_1}^2 = \dfrac{\partial r_2}{\partial s_1}^2 = \dfrac{\partial r_3}{\partial s_2}^2 = \dfrac{\partial r_4}{\partial s_2}^2 \left(= \dfrac{\partial r}{\partial s}^2 \right)$

and $\quad \dfrac{\partial r_1}{\partial s_1'}^2 = \dfrac{\partial r_2}{\partial s_2'}^2 = \dfrac{\partial r_3}{\partial s_2'}^2 \quad \dfrac{\partial r_4}{\partial s_1'}^2 \left(= \dfrac{\partial r}{\partial s'}^2 \right)$

because of their dependence on the conducting wires.

Therefore, from (17) - (20)

$$\frac{dr_1}{dt}^2 - \frac{dr_2}{dt}^2 + \frac{dr_3}{dt}^2 - \frac{dr_4}{dt}^2 = 2vv'\left[\frac{\partial r_1}{\partial s_1}\frac{\partial r_1}{\partial s_1'} - \frac{\partial r_2}{\partial s_1}\frac{\partial r_2}{\partial s_2'}\right.$$

$$\left. + \frac{\partial r_3}{\partial s_2}\frac{\partial r_3}{\partial s_2'} - \frac{\partial r_4}{\partial s_2}\frac{\partial r_4}{\partial s_1'}\right] \quad \dots (21)$$

Substituting (16) and (21) into (7)

$$dF = - \frac{\lambda \, ds \, \lambda \, ds'}{16r^2} \left[\left(\frac{dr_1}{dt}^2 - \frac{dr_2}{dt}^2 + \frac{dr_3}{dt}^2 - \frac{dr_4}{dt}^2 \right) \right.$$

$$\left. - 2r \left(\frac{d_2 r_1}{dt^2} - \frac{d_2 r_2}{dt^2} + \frac{d_2 r_3}{dt^2} - \frac{d_2 r_4}{dt^2} \right) \right] \qquad \ldots (22)$$

Rewriting

$$dF = - \frac{q_1 q_2}{16 r_1^2} \left[\frac{dr_1}{dt}^2 - 2r_1 \frac{d_2 r_1}{dt^2} \right]$$

$$- \frac{q_1 (-q_2)}{16 r_2^2} \left[\frac{dr_2}{dt}^2 - 2r_2 \frac{d_2 r_2}{dt^2} \right]$$

$$- \frac{(-q_1)(-q_2)}{16 r_3^2} \left[\frac{dr_3}{dt}^2 - 2r_3 \frac{d_2 r_3}{dt^2} \right]$$

$$- \frac{(-q_1) q_2}{16 r_4^2} \left[\frac{dr_4}{dt}^2 - 2r_4 \frac{d_2 r_4}{dt^2} \right] \qquad \ldots (23)$$

Each of the four members in this expression refers exclusively to

two of the four electric masses e.g. the first to the two positive

masses forming the positive currents .

Weber then continues :

Hence it is evident that if the entire expression of the
electrodynamic force of two elements of a current be considered
as the sum of the forces, which each two of the four electric
masses they contain exert upon each other, this sum would be
decomposed into its original constituents, the four above
members representing individually the four forces which the
four electric masses in the two elements exert in pairs upon
each other.
   Hence also the force with which any positive or negative
mass E acts upon any other positive or negative mass E' ,
at the distance R , with a relative velocity of $\frac{dR}{dt}$
and acceleration $\frac{d_2 R}{dt_2}$ may be expressed by

$$- \frac{a \, a}{16} \frac{E E'}{R R} \left(\left(\frac{dR}{dt} \right)^2 - 2R \frac{d_2 R}{dt_2} \right)$$

for this fundamental principle is necessary and at the same
time sufficient to allow of the deduction of Ampère's electro-
dynamic laws...[1]

---

[1] Weber (1848) page 518

The addition of Coulomb's law gives the total result :

$$F \propto \frac{q_1 q_2}{r^2} \left[1 - \frac{1}{c^2} \left(\frac{dr}{dt}\right)^2 + \frac{2r}{c^2} \frac{d_2 r}{dt^2} \right]$$

[*To derive Gauss's law, velocities and relative velocities (not partial derivatives) are substituted for cos t in Ampère's law (see 145 footnote 2) :*

$$\underline{u} = \underline{v} - \underline{v}'$$

$$u^2 = v^2 - 2vv' \cos t + v'^2$$

$$\cos t = \frac{v^2 + v_1{}^2 - u^2}{2vv'}$$

. *Then the derivation*

proceeds as before, but concludes :

$$F = \frac{q_1 q_2}{r^2} \left[1 + \frac{1}{c^2} \left[u^2 - \frac{3}{2} \left(\frac{dr}{dt}\right)^2 \right] \right]]$$

I will now run over the strategy of the proof, make some remarks on the assumptions, and refute Weber's claim of a necessary and sufficient link between his law and Ampère's.

The proof opens with Ampère's law used in a plane. This is not a special case since Amperian forces exist only for coplanar elements. (Non-coplanar elements are projected onto planes and non-coplanar projections exert no forces.)

An assumption is made about 'macro' currents being twin 'micro' currents. This -- Fechner's idea -- seems perfectly reasonable. Equal quantities of positive and negative electricity must be assumed, then the question is 'which of them move?' Both is the best answer, since, at this time, there were no known asymmetries between the electricities.

Then the force between macro-currents is taken to be the sum of four forces between micro-currents.

Then Ampère's law is applied to each micro-current pair. This step is a guess. And it is a little unusual in that micro-

currents have a resultant charge and so one would expect Ampère's law _not_ to apply. However, there was no other law -- Ampère's was the only known current element law. The rational stance here is to use Ampère's law and to expect that it needed correcting. Notice that Weber's law cannot be proved from Ampère's law in its micro-current form. For example,

Ampère's micro law $\quad dF_1 = f[\frac{1}{2} \frac{\partial r_1}{\partial s_1} \frac{\partial r_1}{\partial s_1'} - r_1 \frac{\partial_2 r_1}{\partial s_1 \partial s_1'}]$

$$and \quad \left(\frac{dr_1}{dt}\right)^2 = v^2 \left(\frac{\partial r_1}{\partial s_1}\right)^2 + vv' \frac{\partial r_1}{\partial s_1} \frac{\partial r_1}{\partial s_1'} + v'^2 \left(\frac{\partial r_1}{\partial s_1'}\right)^2$$

$$\frac{d_2 r_1}{dt^2} = v^2 \frac{\partial_2 r_1}{\partial s^2} + 2vv' \frac{\partial_2 r_1}{\partial s_1 \partial s_1'} + v'^2 \frac{\partial_2 r_1}{\partial s_1'^2}$$

$$and\ Weber's\ law\ is \quad dF_1 = f'[\left(\frac{dr}{dt}\right)^2 - 2r_1 \frac{d_2 r_1}{dt^2}]$$

so Ampère's = Weber's iff

$$v^2 \left(\frac{\partial r_1}{\partial s_1}\right)^2 + v'^2 \left(\frac{\partial r_1}{\partial s_1'}\right)^2 - v^2 \frac{\partial_2 r_1}{\partial s_1^2} - v'^2 \frac{\partial_2 r_1}{\partial s_1'^2} = 0$$

which in general it will not.

Then the four micro forces are summed to obtain the macro force.

The macro force is manifestly separable into four micro components of identical form each linked to a micro-current pair. The natural guess here is that each micro-current pair is governed by a law of that form (Weber's law, in fact). There is no proof at this point and Weber is mistaken in claiming that there is. (Justification of my statement: Amperian micro-current forces are consistent with the macro force but inconsistent with Weber's

law, hence Weber's law cannot be a logical consequence of the macro force expression.) However, even though Weber's law is not provable from the macro-force expression it is the most rational guess as to an explanation of the macro force.

Notice that the rational guessing here is genuinely content increasing. It starts with the special case of currents in wires, and ends with a law that is intended to apply to all moving charges whether or not they are confined to wires.

5. The Significance of the Law for the A.A.D. Programme :

Weber's law united the laws of Coulomb and Ampère, and it was not heuristically ad hoc. Further, it predicted the stunning novel fact that there should be electrodynamic induction.[1]

Electrodynamic induction phenomena, say between two wires, may be considered as combinations of two basic types : where there is only relative motion between the wires ('motional action'), and where there is only variation in current ('transformer action').

Qualitatively, Weber's law accounts for these as follows. The positive and negative electricities in the current carrying wire move in opposite directions in the conductor and so, when the relative motions between conductors is added on, do not move equal and oppositely relative to the initially stationary positive (or negative) electricity in the current-less wire; this means that there is a resultant force on the positive (and negative) electricities in the initially current-less conductor and so a current is induced. With transformer action, it is the acceleration terms that lead to a resultant force. When the current increases in the first wire the positive electricity accelerates in one direction and the negative electricity accelerates in the opposite direction -- consequently, as the force depends both on the sign of the charge and the first power of the acceleration, there is a resultant force on the electricities in the second wire.

Quantitatively, Weber's law was shown to predict that induction would be fully described by the rate of change of a vector potential.[2]

---

1. 'Novel fact' is used here in the sense of Zahar. See E.G.Zahar (1973), 'Why did Einstein's Programme Supersede Lorentz's ?', and A.E.Musgrave (1974), 'Logical Versus Historical Theories of Confirmation'.

2. See Weber (1848) pages 521-9, and Maxwell (1873) §856 and f.

This means that Weber's law exactly accounts for all electrodynamic induction phenomena.

To emphasize the strength of the A.A.D. programme here, I will quote, then analyse, a section from Maxwell :

> 856.) After deducing from Ampère's formula for the action between the elements of currents, his own formula for the action between moving electrical particles, Weber proceeded to apply his formula to the explanation of the production of electric currents by magneto-electric induction. In this he was eminently successful, and we shall indicate the method by which the laws of induced currents may be deduced from Weber's formula. But we must observe, that the circumstances that a law deduced from the phenomena discovered by Ampère is able also to account for the phenomena afterwards discovered by Faraday does not give so much additional weight to the evidence for the physical truth of the law as we might at first suppose.
> For it has been shown by Helmholtz and Thomson (see Art. 543), that if the phenomena of Ampère are true, and if the principle of the conservation of energy is admitted, then the phenomena of induction discovered by Faraday follow of necessity. Now Weber's law, with the various assumptions about the nature of electric currents which it involves, leads by mathematical transformations to the formula of Ampère. Weber's law is also consistent with the principle of the conservation of energy in so far that a potential exists, and this is all that is required for the application of the principle by Helmholtz and Thomson. Hence we may assert, even before making any calculations on the subject, that Weber's law will explain the induction of electric currents. The fact, therefore, that it is found by calculation to explain the induction of currents, leaves the evidence for the physical truth of the law exactly where it was.[1]

The suggestions here are twofold : (a) that, ceteris paribus, the fact that Weber's predicts induction constitutes good evidence for its truth, and (b) that the ceteris paribus clause in (a) is violated because any law 'equivalent' to Ampère's and consistent with the conservation of energy must explain induction and so no special merit attaches to

---

1. Maxwell (1873) § 856.

Weber's law by virtue of this feature.   And many secondary sources

share this view; Woodruff, for example, writes :

> With this law of force, Weber proceeded to explain the
> phenomenon of electromagnetic induction, and thereby to
> unify the treatment of all the electrostatic and electromagnetic
> effects known in his day.   However, Helmholtz, ... and
> William Thomson demonstrated the intimate connection
> between the Ampèrian force, the conservation of energy
> and electromagnetic induction, by deriving the last (with
> some restrictions) from the first two.   Thus any theory
> which satisfied the energy principle, and yielded the
> ponderomotive force expressed in Ampère's law, would be
> expected to imply the inductive effects.

I maintain that the suggestion (b) is false, and that Maxwell

should have been aware of the insufficiency of his argument.

Induction cannot be explained by 'Ampère's law' and the

conservation of energy because induction generally involves two or

more circuits, and thus the division of energy between two circuits,

and the conservation of energy imposes only one constraint.   No

predictions can be made, and thus no satisfactory explanations can

be offered.[3]

Maxwell should have known this -- he was the most accomplished

mathematical physicist of his day and would have known full well the

analogous triviality that the conservation of energy alone cannot

determine the equations of motion of a system with two degrees of freedom.[2]

Maxwell must have had some misgivings about his argument.   He

appeals in the quoted passage to papers by Helmholtz and Thomson.

---

1. A.E.Woodruff (1962), 'Action at a Distance in Nineteenth Century
Electrodynamics', page 445, his italics; and see also C.W.F.Everitt's
'J.C.Maxwell' article in the Dict.Sci.Biog., page 206.

2. It seems that Maxwell had a blind spot here.   On the one hand, he
must have known that the conservation of energy alone would not determine
the behaviour of simple systems like colliding billiard balls.   But see
his (1877), Matter and Motion IX, 9, and Larmor's Appendix II to that volume,
especially page 158 and f.

3. See also A.O'Rahilly (1965), Electromagnetic Theory, Ch.4, Section 4.

These papers offer energy equations to account for induction, but these are successful only if all the forms of energy that are involved are known and can be summed. In this application some of the types of energy were newly discovered : the chemical energy of the batteries, and the heat energy from the wires, for example. It is easy to produce an energy equation to 'account for' induction, once induction is known; in contrast, it is formidable to sum over types of energy and to predict, and thus genuinely explain, that energy re-appears as the energy of induced currents. Take, as an illustration of the difficulties, the theory of the electric cell as it was in 1855 : the cell loses energy and the circuit gains energy but the one does not equal the other; the cell is a heat pump and works, using energy, to pump energy from the surroundings into the circuit. Maxwell would have been aware of the problems. And he would have known that Thomson, in his paper, proves that Helmholtz's paper is fallacious as Helmholtz had omitted the energy stored in the magnetic field.[1,2]

The rational scientist of the mid-nineteenth century should have been wary of the connection between energy arguments and induction; and he should have appreciated that Weber's force law operates from first principles to predict all the effects.

---

1. Thomson's reasoning -- which approximates to the modern theory -- appears in four main papers which are reprinted in W.Thomson (1872), Papers on Electrostatics and Magnetism.

2. Helmholtz himself was later to describe the electrodynamic section of his paper as follows : 'The chapter on electro-dynamics in my treatise was written under great difficulties. At that time I scarcely had access to any mathematical and physical literature [He had seen only isolated portions of the works of Poisson, Green, and Gauss] , and was almost wholly confined to what I could discover for myself.' Quoted from L. Koenigsberger (1906), Hermann von Helmholtz., page 119.

6. Criticisms of the Law and Their Evaluation :-

         The M.S.R.P. evaluates merit primarily by the presence of

positive success and not by the absence of failure, anomaly, or

inconsistency -- it directs the historian's attention away from

criticisms of laws or programmes.   However, I wish to leave the

direction of the M.S.R.P. for a moment.   I have the view that the

A.A.D. programme has been harshly treated and in further defence of

that thesis I will now look at criticisms of Weber's law.

         The law was not well received.   Helmholtz stated that the

law violated the conservation of energy,[1] and this allegation was

used by Field theorists (and many historians)[2] as a damning criticism

of A.A.D. electrodynamics.   In 1855 Maxwell wrote :

> There are objections to making any ultimate force in nature
> depend on the velocity ... the principle of Conservation of
> Force requires that these forces ... be functions of distance only.[3]

and in 1864 he wrote :

> ... apparatus may be constructed to generate any amount of work
> from its own resources ... I think that these remarkable deductions
> from ... Weber['s] theory ... can only be avoided by recognizing
> the action of the medium in electrical phenomena.[4]

Similar statements can be found in the publications of most of the

---

1. H.Helmholtz (1847), 'On the Conservation of Force', page 114.

2. For example, Berkson in his (1974), Fields of Force, writes on page
131 : 'One of the chief difficulties of the action-at-a-distance theories
was their violation of the conservation of energy'.

3. Scientific Papers, v.1, page 208.   It took a long while for Maxwell
to change his mind over the question of whether Weber's law contradicted
the conservation of energy.   That is why these early passages are incons-
istent with the later passage that I quoted in the last section.

4. I have quoted this from O'Rahilly (1965), page 529.   Actually O'Rahilly
has misquoted and concatenated two sections.   In 1864 Maxwell tells us that
Weber's law faces mechanical difficulties (Scientific Papers, v.1, page 527),
and in 1868 Maxwell tells us that these mechanical difficulties are in fact
those of leading to a perpetual motion machine (Scientific Papers, v.2,
page 137).   See also Scientific Papers,v.1, page 488.

English Field theorists.[1]

I will expose much of this for the nonsense that it is.

I will show :

a) that Helmholtz's refuting principle is false and his argument for it invalid;
b) that Weber's argument for the consistency of his law with the conservation of energy is a good argument;
c) that Weber's argument can no longer be taken as being persuasive;
d) that Whittaker's (and O'Rahilly's) modern proof that the law and the conservation of energy are consistent is again a good argument;
e) that Whittaker's (and O'Rahilly's) proof is faulty.

The clearest approach is the one used by Maxwell in the last quote : discuss perpetual motion machines and their impossibility.[2] Perpetual motion machines may be roughly characterized as isolated systems which start in one state, produce work, and yet return to exactly that state; then the system can be repeatedly run through the cycle to produce work _ad infinitum_. Isolation prevents the system from doing work at the expense of energy that it obtains from elsewhere; cyclical operation prevents the system from working indefinitely by virtue of special initial conditions.

Now let us consider the arguments.

Helmholtz based his on the principle 'All velocity dependent forces must violate the conservation of energy' which he claimed to

---

1. See O'Rahilly (1965), page 530.

2. I do this because in the mid-nineteenth Century the Conservation of Force was a mechanical principle without direct application to electrodynamics, and the Conservation of Energy was a new principle which, in a manner of speaking, was regularly being refuted with the discovery of new forms of energy. What lay behind these conservation laws was the thesis that there could be no perpetual motion machine.

have the backing of his 'Conservation of Force' paper.[1] But Helmholtz's

principle is false -- a trivial counter-example occurs when the force

is always perpendicular to the velocity, such a force can do no work

and thus cannot form the basis of a perpetual motion machine. If

the English Field theorists Maxwell and Thomson had considered whether

Helmholtz's principle was true, they could have hardly failed to see

that it was false.[2] It was even not at all unusual to argue that the

velocity dependent frictional forces were consistent with the conservation

of energy. What about Helmholtz's proof ? In the 'Conservation of

Force'paper he shows that from assumption A 'all forces depend only

on distance' conclusion B 'the Conservation of Force' follows. He

then tries to argue that from not-A 'not all forces depend only on distance'

(for example, there are velocity dependent forces) not-B 'violation of

the Conservation of Force' follows ! Maxwell's later reconstruction of

this reads :

> ... in establishing by mathematical reasoning the well-known
> principle of the conservation of energy, it is generally
> assumed that the force acting between the two particles is
> a function of the distance only, and it is commonly stated
> that if it is a function of anything else, such as the time,
> or the velocity of the particles, the proof would not hold.
>   Hence a law of electrical action, involving the velocity
> of the particles, has sometimes been supposed to be inconsistent
> with the principle of the conservation of energy.[3]

In short, the condemnation of Weber's law rested on the logical blunder

-- 'If A, then B' _therefore_ 'If not-A, then not-B'.

---

1. Helmholtz (1847).

2. The case is more complex than I have suggested. Maxwell and Thomson
may have assumed that the forces are central and acting between two particles
if so the $\underline{F} \times \underline{v}$ counter-example would have been obscured. However, Weber
himself'refuted' the principle, as I show in the next paragraph of the text,
and Maxwell and Thomson must have known this.

3. Maxwell (1873), § 852.

Weber's response to Helmholtz's attack was to publish immediately -- in 1848 -- the spatial integral for his force :

$$\frac{1}{r} \left[1 - \frac{2}{c^2} (\frac{dr}{dt})^2 \right]$$

The existence of this potential is a strong argument that the law cannot lead to a perpetual motion machine. The work done by this force on a charged particle is linked to the particle's position and velocity in such a way that if a particle starts in one state (presumably here this means that the particle has a specific charge, position, and momentum (velocity)) and finishes in the same state, there is no change in potential. The kinetic energy and the potential energy of the particle sum to a constant. Once this potential was known -- in the absence of other arguments -- all criticism concerning violation of conservation of energy should have stopped. But it took nearly thirty years for the Field theorists to acknowledge their errors.[1]

Looking briefly at the problem in modern terms. If we consider path integrals of force laws in a distance, velocity, acceleration phase-space, then the existence of a scalar ψ such that $F = - \text{grad } \psi$ is neither necessary nor sufficient for the work done by F around an arbitrary loop to be zero. This seems to mean that in absolute terms Weber's argument is not strong enough. But there are many subtleties here. Whittaker and O'Rahilly have proved that the law does not violate the conservation of energy, using Lagrangian and variational methods.

---

1. The English Field theorists presumably learned of Weber's law from the 1852 translation of his paper. This paper contains the spatial integral (on page 520) -- yet the Field theorists still alleged that there was violation of the conservation of energy. Secondary sources also go astray here. For instance, Everitt holds that Weber did not refute the energy arguments until 1869 and by then Maxwell had produced a better theory of electromagnetism; see his 'Maxwell' article in the Dict.Sci.Biog., page 205.

Their proofs too are not strong enough. In essence the method is to

show that there is a precisely analogous mechanical system for which

the conservation of energy holds.[1] This means that a system of

particles moving in the phase space in any closed path available to

them under Weber's law do no work. But this is insufficient. There

may be other closed loops in the phase space around which work may be

done. Let me explain. A force must be used to make a particle travel,

and in general the assumption is made that there is a suitable driving

force to give access to all paths in the phase space. But if restrictions

are imposed on the driving force, certain paths may become inaccessible.

For example, if there were only two particles and they were governed

by a purely repulsive force, then if one is released it will travel to

infinity and will not return under its own force -- there will be no closed

path in the phase space, and so no path around which work is done; but

the particle may be brought back *from a distance* to its original position,

momentum, and acceleration using another type of driving force, and then

it becomes an open question as to whether the repulsive force does work

around the loop. Whittaker's and O'Rahilly's proof restricts the

driving forces to Weberian forces, and this is too much of a special case.

Their proof shows that if there are only Weberian forces, then no perpetual

motion machine can be constructed on the basis of Weber's force law.

But the A.A.D. theorists always assumed that gravitational forces could

---

1. The proofs appear in Sir E.T.Whittaker (1951), <u>A History of Theories of</u> <u>Aether and Electricity</u>, page 203 and O'Rahilly (1965), page 530 and f.

be used on the electrical fluids, either directly, by virtue of the

fluids having mass, or indirectly, by virtue of the fluids being able

to attach to ponderable bodies. Whittaker's and O'Rahilly's proof is

insufficient. And in fact, if one particle is fixed at the origin,

and another travels from x = 1 to x = 2 and back under Weber's law,

then conservation of energy _can be_ violated.[1]   (Whittaker's proof shows

that there is no way that Weberian forces could produce such a motion.)

  To return to the history. Helmholtz continued and diversified

his opposition to Weber's law. He blindly carried on asserting that

physics allowed of no velocity dependent forces, and he offered the new

objection that Weber's law led to absurd instabilities.[2]   This had little

influence historically as it was always in the shadow of the major objection

that conservation of energy was violated. But it is worthwhile to

consider it briefly. Helmholtz put forward a sequence of thought-experimental

absurdities. In the main these consisted of initial conditions which

would provide a large (possibly infinite) amount of energy, usually by

virtue of the negative term in Weber's force law which can lead to a particle

having apparently negative mass and thus accelerating itself in a

resisting medium. (As a parenthetical remark, the last paragraph _may_

clarify _an_ apparent inconsistency in Helmholtz's position : by this time

he accepted Weber's proof that no perpetual motion machine could be

constructed from the law, and yet he was offering thought-experimental

----

1. Such violations are not always significant. Accelerating electrons
radiate energy and so driving an electron around a loop should be
inconsistent with the conservation of energy, but any inconsistencies should
be accounted for by the radiated energy.   I think that Weber's law is
still deficient here (unless, of course, there is a new type of radiated
energy which has not yet been discovered.)

2. See A.E.Woodruff (1968), 'The Contributions of Hermann von Helmholtz
to Electrodynamics', pages 304-6, and references therein.

perpetual motion machines relying on the law -- these latter machines used forces in addition to the Weberian one.)   Weber objected to the initial conditions in some of these examples -- one required sub-atomic dimensions and speeds over half that of light.   I suggest that mere absurdities are not sufficient, they should be converted into failed experiments.   I offer two arguments.   It is one of the glories of science that many 'absurdities' have actually occurred, so theories should not be ruled out simply because they suggest new effects -- our limited imagination should not act as a constraint on a theory.   Listen to Ritz discussing Relativity :

> The result is that it has been found necessary to abandon the classical concept of universal time, to make simultaneity a quite relative notion, to suppress the concept of the invariability of mass as well as that of rigid bodies, to abandon the axioms of kinematics and the parallelogram of velocities, ... It is curious and worth noting that a few years ago it would have been thought sufficient, in order to refute a theory, to show that it entailed only one or other of the consequences here enumerated.[1]

Secondly, the attempt at realizing an absurdity may disarm it.   For example, Coulomb's law on its own leads to 'absurd' instabilities -- there can be no stable equilibrium in a charge-free electric field, so a charge released there may produce an infinite amount of energy.[2] (But, to realize the situation experimentally charges must be used to produce the field and then the roaming charge will not deliver an infinite amount of energy.)

---

1. W.Ritz (1908b), 'A Critical Investigation of Maxwell's and Lorentz's Electrodynamic Theories'.

2. For stability, displacement must produce a restoring force. That is, flux $\underline{E}$ over a surface containing the point cannot be zero; so div $\underline{E}$ cannot be zero, which is impossible in a charge-free region.

7. Riemann's Attempt to Deduce Weber's Law from a Propagated Force Law :

The history of Riemann's contribution to electrodynamics is briefly told. In 1858 he offered the world his 'discovery of the connection between electricity and light'[1], and this consisted of the retardation of the Coulomb scalar potential ϕ.[2] But no sooner had he submitted the paper to the Gottingen Royal Society than he withdrew it, possibly because he realized that it contained a mathematical error. The paper was published posthumously in 1867 and was then criticized by Clausius for the mathematically incorrect permutation of two integrals.[3] In the same year -- 1867 -- Ludwig Lorenz presented his theory in which both the scalar potential ϕ and the vector potential $\underline{A}$ are retarded.[4]

Riemann's theories were inadequate. He retarded the Coulomb potential and incorrectly derived from this a force law similar to Weber's force law. The Riemann force law made no successful predictions to make it preferable to that of Weber.

However, Riemann's paper is still important. Its value lies as an easily understood piece of evidence for the objective problem situation and the techniques that would be used for solving

1. B.Riemann, Letter to his Sister, quoted from Rosenfeld (1957), page 1634.

2. See B.Riemann (1867), 'A Contribution to Electrodynamics'.

3. R.Clausius (1869), 'Upon the New Conception of Electrodynamic Phenomena Suggested by Gauss'. A distance integral and a time integral are permuted and in this case, with a moving particle, the order of integration is important.

4. L.Lorenz (1867), 'On the Identity of the Vibrations of Light and Electrical Currents'.

that problem.   Of course, one swallow does not a summer make -- but

Riemann was not alone.   Gauss tried to retard forces, Carl Neumann

retarded potentials to obtain his father's vector potential induction

laws, Betti retarded potentials, and Ludwig Lorenz retarded the scalar

and vector potential.[1]   And Weber himself wrote to Gauss in 1845 :

> ... the nicest solution to the puzzle [of electrodynamic
> action-at-a-distance] would be its explanation on the basis
> of a gradual propagation of the force.[2]

The problem was to produce a retarded force law which would

yield Weber's law or an approximation to it.   Such a theory might be

expected to provide an electric or 'electromagnetic' theory of light.

A remark is called for here on the relationship between electricity

and light.   I think that it was virtually part of background knowledge

at this time that the two would be connected, for such a link opens

up the possibility of an explanation of Faraday's results of the 1840's

on the magnetic rotation of the plane of polarization of light.[3]

Light was known to be propagated with speed c, electrical action was

---

1. C.Neumann (1868), Betti (1868), and Lorenz (1867).

2. This letter of the 31 March 1845 is quoted on page 68 of Karl
Heinrich Wiederkehr, Wilhelm Webers Stellung in der Entwichlung der
Elektrizitatslehre, (diss. University of Hamburg, 1961).   I read it in
K.L.Caneva (1978), 'From Galvanism to Electrodynamics : The Transformation
of German Physics and Its Social Context', page 100; but so late as to
prevent checking with the original source.

3. Note, for instance, the Riemann quote on the previous page, the Gauss
passage quoted in Section 4.3, and the Thomson passage quoted in Section 1.8.

thought to be propagated -- a reasonable guess was that the latter

also travelled at speed c and that light was electrical in nature.[1]

Instantaneous and retarded functions can often be integrally

transformed one to another -- for example, if the present position

of a particle is given as an explicit function of its past position

and its acceleration as a function of time. In these cases, A.A.D.

theorists would regard the retarded law as the physically significant

one -- that is, the one in need of further explanation. In the 1840's

Weber's instantaneous law should have been taken as an approximation to

the truth about electrodynamic phenomena -- the search was for a more

fundamental retarded law that would underpin it.

I suggest that Riemann's idea was this. Weber's law is

merely Coulomb's law with additional velocity and acceleration terms,

and these terms are significant only if there is relative motion between

the particles. Further, there must be relative motion or change to

distinguish between an instantaneous and a retarded force function --

an instantaneous signal and a signal taking time can be told apart only

if a signal is sent. Riemann's idea was that Coulomb's law required

the additional velocity and acceleration terms because it was instantaneous

whereas it should have been retarded.

Riemann took Coulomb's law stated in the scalar potential form

governed by Laplace's equation :

$$\nabla^2 \phi = -4\pi\sigma$$

and retarded

---

1. The constant c appears as the velocity of light, it also appears in Weber's law and similar laws as a ratio of dynamic and static electrical units having the dimensions of a velocity -- that the two 'c's' were identical (except for, perhaps $\sqrt{2}$ factors) becomes apparent only with the work of Riemann, Weber, and Kirchoff.

it at speed c, substituting a D'Alembertian for the Laplacian :

$$\Box^2 \phi = -4\pi\sigma$$

Riemann then incorrectly derived from this D'Alembertian equation an approximation to Weber's law.[1]

In heuristic reconstruction the history would have developed as follows. Sound derivation would have shown that an electric current or charge in uniform motion produces forces over and above a retarded Coulomb force -- Riemann's error in supposing that a retarded scalar potential ϕ was equivalent to an instantaneous scalar potential ϕ together with an instantaneous vector potential $\underline{A}$, which were then assumed to be the complete basis of electrodynamics, would have been uncovered. Some vector potential or remnants of a vector potential would have to be used. The second step would be to extend the retardation to the existing vector potential $\underline{A}$ so that it too propagated in space -- this would yield all the known results, and in addition would have predicted the full electromagnetic theory of light.

The actual history is slightly deviant. Ludwig Lorenz omitted one of the intermediate steps. He sought an electromagnetic theory of light and a desideratum he imposed was that such a theory should yield the Fresnel formulas for reflection and refraction, which he had described by means of a vector equation governing the boundary conditions of the light vector. He then asked, 'How can the Weber-Kirchoff equations be modified to yield that vector equation ?' and

---

1. These mathematical equations introduce an extra physical solution. The idea of retarding forces is that the force propagates outward from a source, but a D'Alembertian has advanced solutions as well as retarded ones and so is not true to the physical ideas. That is, it is not ad hoc for A.A.D. theorists to discard the advanced solutions.

he noticed that this question had a unique answer -- by retarding ϕ and $\underline{A}$ at speed c. A retarded ϕ and $\underline{A}$ are consequences of knowledge on reflection and refraction and A.A.D. background knowledge on electrodynamics.

---

8. Electrical Actions Propagated at the Speed of Light :-

During the 1850's Weber and Kirchoff developed the theory of transmission lines.[1] Their approach is similar to the modern treatment except that Weber's force law and vector potential are used to eliminate self-induction and capacitance from the calculations. The outcome was a set of equations, depending on Weber's theory, which predicted the velocity of an electrical disturbance down a wire. These equations constitute part of the problem situation for Ludwig Lorenz's theory of light and they will be discussed further in that context in Chapter 6.

What has to be noted is that Weber and Kirchoff knew that the velocity of current waves (and so on) in suitable wires was that of light, and they also pointed out that under their theory this velocity was the ratio of the electrical units.[2]

---

1. This appears in Kirchoff (1857a), 'Uber die Bewegung der Electricitat in Drahten', Kirchoff (1857b), 'Uber die Bewegung der Electricitat in Leitern', and Weber (1864), Electrodynamische Maassbestimmungen, IV, page 105.

2. Some idea of the subtlety of Weber's investigations may be gained from his suggestions for determining a link between charge and mass of electrons. For him a current disturbance was akin to a wave in a plasma and its velocity of propagation was damped by a factor of frequency related to the charge and mass of ions.

9. Summary :

In this Chapter I have :

a) shown that the A.A.D. views on the sources and receivers of force
were superior to those of the rival Field programme,

b) argued that the A.A.D. programme united Ampère's and Coulomb's
laws by a process akin to deduction to yield Weber's law -- the objective
problem situation and the problem-solving techniques were such that
A.A.D. theorists would arrive at Weber's law or a law similar to it,

c) concluded that since Weber's law was produced in this way and it
predicted electrodynamic induction, it constituted good evidence for
the A.A.D. programme,

d) refuted Maxwell, Woodruff, and Everitt on the evidential relationship
between electrodynamic induction, Weber's law, and the conservation
of energy,

e) refuted Maxwell, Helmholtz, Berkson, and others over their arguments
concerning Weber's law and the conservation of energy,

and

f) argued that the A.A.D. programme would lead to a retarded potential
approach to electrodynamics, independently of developments elsewhere.

Chapter 5 : Maxwell's Theories of Electromagnetism.

1. Introduction.

2. The Early Theory of 'On Physical Lines of Force'.

3. A Critical Appraisal of 'On Physical Lines of Force' and of the Theses of Heimann and Bromberg Concerning It.

4. The Later Theory.

5. A Critical Appraisal of the Later Theory.

6. Summary.

1. Introduction:

By 1860 the Field programme had acquired the ability to solve problems. But did it ever surpass its rival? The key issue here is the electromagnetic theory of light. Most commentators would regard the thesis that the A.A.D. programme was the better one as somewhat unusual but on reflection, and perhaps on consideration of my arguments, they would admit that the thesis was sound until 1860. But they would qualify their admission. Surely, they would add, the electromagnetic theory of light, developed during the 1860's gave the Field programme its decisive victory?

I think not, and I argue the point in the next two Chapters.

It was Maxwell who developed the Field theories of light, and he did so in four publications: 'On Physical Lines of Force' (1862), 'A Dynamical Theory of the Electromagnetic Field' (1865), 'On a Method of Making a Direct Comparison of Electrostatic with Electro-magnetic Force; with a Note on the Electromagnetic Theory of Light' (1868), and the Treatise on Electricity and Magnetism (1873). I regard the theories here as being in two groups. There is what I will call the early theory of 'On Physical Lines of Force', and there is the later theory which was proposed and refined in the other publications. The theories will be assessed carefully in this Chapter using the M.S.R.P. -- particular attention will be paid to the questions of whether the theories were heuristically integrated to the Field programme, and whether they predicted novel facts.

My approach -- that of assessing Maxwell's theories in their own terms -- may be contrasted with the method used by most historians. Many writers are led by excessive hindsight into insoluble problems. The modern theory of electrodynamics yields an electromagnetic theory of light and it does so by virtue of the

term involving the time variation of the electric field (the 'dis-

placement current'). Some historians search for the discoverer of

the displacement current and think their venture is satisfied by

Maxwell. But Maxwell's 'displacement current', which was not much

more than the aether stepping sideways, has very different properties

to the modern 'displacement current'. At this point these writers

either do an injustice to Maxwell or retreat into such empty phrases

as 'the germ of the modern idea' or the 'glimmer of the displacement

current'. And they invent stories as to why Maxwell introduced the

'displacement current'. Two such myths can be briefly disposed of.

He did not realize that the equations:

$\quad$ curl $\underline{B}$ = $\underline{j}$ (Ampère's equation relating magnetic force and

$\qquad$ current)

$\quad$ div $\underline{j}$ = - $\dfrac{\partial \rho}{\partial t}$ (Continuity)

were inconsistent and seek to remedy this by adding the displacement

current term.[1] And it was not a desire for symmetry that led him

to introduce the displacement current.[2] The truth is -- as we shall

see -- that there is nothing original to Maxwell that is closely

related to the modern 'displacement current'.

---

1. The inconsistency is that div curl $\underline{B}$ = 0 so div $\underline{j}$ = $-\dfrac{\partial \rho}{\partial t} \neq 0$. Physicists offer the rational reconstruction that Maxwell added $\dfrac{\partial E}{\partial t}$ so that curl $\underline{B}$ = ($\underline{j}$ + $\dfrac{\partial E}{\partial t}$). This is historically incorrect. See J. Bromberg (1967), 'Maxwell's Displacement Current and His Theory of Light'.

2. In a charge and current free region the pre-Maxwell versions of 'Maxwell's equations' were:
$\qquad$ div $\underline{E}$ = 0 $\quad$ curl $\underline{E}$ = - $\dfrac{\partial B}{\partial t}$

$\qquad$ Div $\underline{B}$ = 0 $\quad$ curl $\underline{B}$ = 0
and so, it has been said, Maxwell made curl $\underline{B}$ = + $\dfrac{\partial E}{\partial t}$ to obtain symmetry. This is mistaken. See A.M. Bork (1963), 'Maxwell, Displacement Current, and Symmetry', and J. Bromberg (1967).

Maxwell's theories -- especially the later one -- are easier to understand if viewed in the light of three theses which I maintain form the skeleton of his ideas :

i) that all charge is polarization charge,

ii) that polarization is a mechanical stress in the aether, and

iii) that the vacuum is a polarizable dielectric.

Thesis (iii) is the Helmholtz A.A.D. interpretation of Maxwell's theories.[1] It enables the Maxwell view to be expressed in terms of the then existing A.A.D. electrodynamics, and in fact most scientists of the period understood Maxwell's theories in this way. (That is why Hertz's experiments on radiated electromagnetic waves were taken as a proof that the vacuum is a polarizable dielectric.[2,3]) One consequence of the A.A.D. theories using source fluids, which was presumably unknown until Maxwell derived it in his (1865), is that there can be transverse electromagnetic (e.m.) waves in dielectrics. For Maxwell these waves should also occur in the vacuum, since the vacuum is a polarizable dielectric. This consequence, although previously unknown, was in harmony with background knowledge. No scientist acquainted with the then standard result that conductors support longitudinal current waves propagating at the speed of light would have been surprised to be told that dielectrics should support transverse current waves propagating at the speed of light.

---

1. See Helmholtz (1870), 'Ueber die Theorie der Elektrodynamik'.

2. Historians, with hindsight sharpened by relativity theory, have re-written this story.

3. See, for instance, G.F.Fitzgerald's opening address to the annual meeting of the British Association for the Advancement of Science, 1888, reprinted in Fitzgerald (1902) pages 229 and f.

But thesis (iii) does not do full justice to Maxwell's views. Maxwell did not hold merely that the vacuum was polarizable. Had he done so there would still be the source charges to cause this polarization and then, for instance, a charged capacitor with empty space between its plates would carry both a free charge and a polarization charge. No, polarization was not to be the result of free charges acting on a vacuum, it was instead to replace free charges. Electrical sources such as fluids or electrons were not to be permitted. As Maxwell put it, in a revealing passage:

> Bodies ... are said to be electrified, or charged with electricity. These words are mere names given to a peculiar condition of matter ...
> In speaking of a quantity of electricity, we need not conceive of it as a separate thing, or entity distinct from ponderable matter, any more than in speaking of sound we conceive it as having a distinct existence. Still it is convenient to speak of the intensity or velocity of sound, to avoid tedious circumlocution; and quite similarly we may speak of electricity, without for a moment imagining that any real electric fluid exists.[1]

It should be noted that this was written in a British Association for the Advancement of Science Report and consequently represents Maxwell's judgement as to the objective truth and not merely a speculation that he thought merited public attention. For Maxwell, apparent sources were the outcome of a mechanical 'displacement' in the medium. Polarization currents, in one direction, then become displacement currents, possibly in the opposite direction.[2] Some

---

1. J.C. Maxwell and Fleeming Jenkin (1863), 'On the Elementary Relations between Electrical Measurements', page 136. Fleeming Jenkin was one of Maxwell's students.

2. See also my Chapter 1 Section 6. Passages expressing this may be found in all of Maxwell's later publications on electromagnetism -- see, for instance, his (1868) page 139 or his (1873) § 62, § 111. And see also J. Bromberg (1968), 'Maxwell's Electrostatics'.

deficiencies in his theory originate here. For example, Maxwell never derived the Fresnel formulas for the reflection and refraction of light; this is no accident -- he was unable to; all three components of a 'displacement' strain must be continuous across the boundary of two media or else the two media lose contact with each other; with this condition the Fresnel formulas are unavailable; one might say that the Fresnel formulas 'refute' Maxwell's electro-mechanical theory of light.[1] Another instance is the persistent difficulty with signs -- his (1862) has two complementing errors of sign, his (1865) contains a formal contradiction, and his subsequent publications inherit the ghost of this contradiction. These all arise as follows. The displacement strain, or polarization, or displacement must be in the same direction as the electric force, that is $\underline{D} \propto + \underline{E}$, then for charge to be the result of displacement div $\underline{D} \propto - \rho$, but the standard Coulomb law of electrostatics makes div $\underline{E} \propto + \rho$; and these three conditions are incompatible.

It is often stated that Maxwell's novel contribution was his postulation of a displacement current which was akin to the conduction current in producing magnetic effects. In this vein Simpson writes:

> ..it is bold hypothesis to assert, as Maxwell did without empirical evidence even at the time of the Treatise, that these hypothetical momentary currents would produce the same magnetic effects as conduction currents in wires.[2,3]

---

1. See Sir E.T. Whittaker (1951), A History of Theories of Aether and Electricity, page 266.

2. T.K. Simpson (1966), 'Maxwell and the Direct Experimental Test of His Electromagnetic Theory', page 413.

3. See also, for example, J.J. Thomson (1893), Notes on Recent Researches in Electricity and Magnetism, page vii.

This is not true.[1]   For Maxwell the displacement current is a polarization current and polarization currents -- as transient flows of electrical fluids -- had been part of A.A.D. background for at least twenty years.[2]   That these polarization currents should produce magnetic effects was also part of background.   Simpson is completely mistaken when he writes:

> ...it would be the truly crucial evidence for Maxwell's
> displacement current, namely, the direct demonstration
> of a magnetic field produced by varying polarization
> of a dielectric.[3]

All A.A.D. theorists regarded polarization currents and conduction currents as being identical; and specifically under Weber's force law conduction currents, polarization currents, and equal and opposite convection currents, all merited the same treatment.   I must emphasize that the A.A.D. view was not one account among many -- it was the only common view.[4]

More sophisticated authors argue that Maxwell's contribution was in identifying the static electrical force with the induced electrical force in so far as both were able to cause polarization in a dielectric.   This also is not true.[5]   A.A.D. theorists had been identifying ponderomotive, inductive, and electromotive electric

---

1. See also H. Hertz (1884), 'On the Relations between Maxwell's Fundamental Electromagnetic Equations and the Fundamental Equations of the Opposing Electromagnetics'.

2. The twenty years run back to Weber and Fechner -- it can be argued that the figure should be forty years, which run back to Poisson.

3. T.K. Simpson (1966), page 429.

4. See my Chapter 1 Section 6.   Maxwell and Faraday remained silent on the questions of the nature of conduction current and of how dielectrics work.   In contrast, the A.A.D. theorists explained conduction currents as a flow of electrical fluids, and they explained the surface charge of dielectrics as the outcome of a momentary flow of electrical fluids.

5. See H. Hertz (1884).

forces for again at least twenty years.[1]

What is novel to Maxwell, or to the Faraday-Maxwell tradition, is the thesis that the vacuum is a dielectric.[4]

The views that I have expressed in the last paragraphs are not entirely new.   Hertz argued them in 1891.   He mentions three hypotheses:

1. that changes of dielectric polarization produce the same electromagnetic forces as do the currents which are equivalent to them;
2. that electromagnetic [electrodynamic] forces as well as electrostatic are able to produce dielectric polarizations;
3. that the vacuum is a dielectric;[2]

and he writes:

> But while I was at work it struck me that the center of interest in ... [Maxwell's] theory did not lie in the consequences of the two hypotheses.   If it were shown that these were correct for any given insulator, it would follow that waves of the kind expected by Maxwell could be propagated in this insulator, with a finite velocity which might perhaps differ widely from that of light.   These however could not be very surprising, not more than the circumstance, known long since then, that in wires electric perturbations propagate with a great but finite velocity.   I felt that the third hypothesis contained the gist and special significance of Faraday's and therefore Maxwell's view, and that it would thus be a more worthy goal for me to aim at.[3]

Not many English readers are aware that Hertz thought this.   The translator, after assuring us that he has made only minor changes only to the title and some footnotes, omits   the entire sentence that starts 'These however could not be very surprising ....'.

---

1. See also my Chapter 4, Section 2.

2. H. Hertz (1891), <u>Electric Waves</u>, Introduction, page 6.

3. See S.D'Agostino (1975), 'Hertz's Researches on Electromagnetic Waves', page 310.   I should explain the reference to varying velocities.   In wires, the velocity of transmission can be that of light or can differ widely from it -- depending on the properties of the wire; dielectrics are similar.

4. See also P.Drude (1897), 'Ueber Fernwirkungen', page xxiv.

2. 'On Physical Lines of Force' (1862) :

At first glance this paper achieves the following. It sets out to solve the problem of electromagnetic induction using an extremely natural mechanical model which filled the intervening space with a mechanism. This model has the independent and unexpected consequence that it supports transverse waves and further these waves travel at the speed of light, so that:

> we can scarcely avoid the inference that <u>light   consists in the transverse undulations of the same medium which is the cause of electric and magnetic phenomena.</u>[1]

Thus the model solves the outstanding problem of the Field programme while predicting a rudimentary electromagnetic theory of light.

As Everitt writes:

> Maxwell's ... paper ... began as an attempt to devise a medium occupying space which would account for the stresses associated by Faraday with lines of magnetic force. It ended with the stunning discovery that vibrations of the medium have properties identical with light.[2]

Such an achievement would be an impressive victory indeed for the Field programme.   And some think it so.   Pearce Williams writes:

> [the model] had an amazing ability to account for observed electrical and magnetic phenomena.   Using this model as a starting point for his mathematics, Maxwell was able to explain a host of facts.   Magnetic attractions and repulsions could be derived by some elementary mathematical operations from the assumed tension and hydrostatic lateral pressure of the rotating vortices.   More dramatically, the electrical effects of the disturbance of magnetic lines of force followed so naturally from his model ....[3]

I, of course, will argue that the achievement is not real, but I will

---

1. Maxwell (1862), page 500, his italics.

2. C.W.F. Everitt (1975), <u>James Clerk Maxwell</u>, page 93.

3. L. Pearce Williams, (1966), <u>The Origins of Field Theory</u>, pages 131-2.

give a more detailed account before offering criticism.

We may take it that the problem was that of explaining induction for that was the major unsolved one of the Field programme, and also Maxwell tells us of the unfinished task of his previous paper:

> The idea of the electro-tonic state, however, has not yet presented itself to my mind in such a form that its nature and properties may be clearly explained without reference to mere symbols ... By a careful study of the laws of elastic solids and of the motions of viscous fluids, I hope to discover a method of forming a mechanical conception of this electro-tonic state adapted to general reasoning.[1]

Induction involves the magnetic field so that heuristically the first task is to model magnetism. Faraday had argued in 1852 that the behaviour of magnetic lines of force could be described completely by supposing that they were trying to shorten in length and expand laterally away from each other. Tubes of rotating fluid have exactly the property of shortening longitudinally and expanding laterally, and further the Field theorists had always supposed that magnetism was rotatory or vortex in character. In short:

'The explanation which most readily occurs to the mind ..'[2] was that of using vortex filaments to model magnetism. The filaments or tubes of force run through space as a substitute for lines of force. Thomson treated magnetic energy as an all space integral of the magnetic energy density B^2. With vortex filaments, the total energy is an all space integral of $d.v^2$ where d is the density of the fluid (which we can set on one side for the moment) and v^2 is the square of the tangential velocity of the fluid.[3] Maxwell made

---

1. Maxwell (1857), pages 187-8.

2. Maxwell (1862), page 455.

3. I am presenting here a simplified reconstruction. I will in general use modern vector notation throughout this dissertation, and discuss the primary fields of E and B, not D and H.

the obvious identification of magnetic force and tangential velocity

so that the tangential velocity v mimics the axial magnetic force B.

There is a mechanical difficulty in filling space with these

vortices.   Adjacent filaments cannot move in the same sense and

maintain mechanical contact:
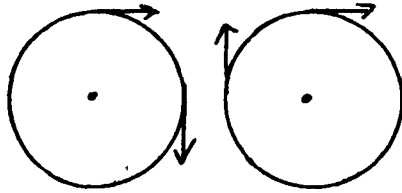
To overcome this Maxwell introduced idler particles, which he

called particles of 'electricity'.   These electric 'ball-bearings'

are distributed throughout space -- they are free to rotate, and they

can also have translatory motion.   Flow of these particles --

being flow of 'electricity' -- constitutes a current.   And if an

electric force produces such a flow, say in a conductor, it will set

the vortices in motion; in this way an electric current creates a

magnetic field.   Also, if there is a variation in current, this

alters the speeds of rotation of the adjacent vortices, and the

disturbance is passed from vortex to vortex by the idler particles

until it encounters particles in a conductor, which are free to move

in translation, and then a current is induced.   Now the model

needed to be adapted to account for static electricity.   As Maxwell

states it :

> If we can now explain the condition of a body with respect
> to the surrounding medium when it is said to be 'charged'
> with electricity, and account for the forces acting between
> electrified bodies, we shall have established a connexion [1]
> between all the principal phenomena of electrical science.

And he did so by permitting the aether cells to distort and using a

---

1. Maxwell (1862), page 490.

displacement of the aether.   The attempt here is at a mechanical

interpretation of the existing A.A.D. theory of dielectrics:

> In a dielectric under induction, we may conceive that
> the electricity in each molecule is so displaced that
> one side is rendered positively, and the other negatively
> electrical ...
>   The effect of this action on the whole dielectric
> mass is to produce a general displacement of electricity
> in a certain direction ...
> These relations are independent of any theory about the
> internal mechanism of dielectrics, but when we find ...
> electric displacement in a dielectric ... we cannot help
> regarding the phenomena as those of an elastic body,
> yielding to a pressure.[1]

Such a distortion enables one to introduce a coefficient of rigidity

m of the medium, and this can be evaluated in terms of electrical

quantities.[2]   Maxwell then calculates the velocity of a transverse

wave in the medium using the formula $v = \sqrt{\dfrac{m}{d}}$ where v is the velocity,

m the coefficient of rigidity, and d the density of the medium

(mentioned earlier).   And the velocity had the value of the ratio of

the electrical units, which is numerically equal to the speed of

light.

There is one further consequence.   In 'genuine'

dielectrics like glass or paraffin the coefficient rigidity m' is

proportional to the dielectric constant of the substance.   This

means that the velocity of the disturbance in dielectrics (and hence

the refractive index) is proportional to the square root of the

dielectric constant.   Thus electrical and optical quantities are

related and genuine tests of the theory may be performed.

---

1. Maxwell (1862), pages 491-2.   See also my explanations in
Chapter 1 Section 6, and Chapter 5 Section 1.

2. Berkson's long explanation of this in his (1974) Fields of Force
has it that the idler particles of electricity distort, not the aether
cells.   This account of Maxwell's theory is simply mistaken.

3. A Critical Appraisal of 'On Physical Lines of Force' and of the Theses of Heimann and Bromberg Concerning It:

The achievement of 'On Physical Lines' is illusory.

I will drive a logical wedge between the independent test and the model, and a heuristic and historical wedge between the wave model and the induction model.

Maxwell employs what was known to be an incorrect formula for the velocity of a transverse wave -- the correct formula is $v = \sqrt{\frac{m}{2d}}$.[1]   It is a logical consequence of his model that the velocity of a transverse wave be the speed of light <u>divided by $\sqrt{2}$</u>, not the speed of light.   The model <u>fails</u> the independent test.

This mistake has occasioned much comment.   Scientists usually manage to make omissions from the penultimate line of a calculation only when they know exactly what they are trying to obtain for the last line.   Certainly Maxwell has been accused of deception.[2]   My concern, though, is solely with the logical and heuristic structure of his paper.   I suggest that the model was designed to ensure the existence of a transverse wave, and that there was enough openness in the determination of the parameters to ensure the right value for the velocity.   There were no novel predictions.

How might we explain Maxwell's mistake?   If the use of the wrong velocity formula was a chance error, then either the model is just false -- as above -- or if basically sound there must be a compensatory error of $\sqrt{2}$ elsewhere.   Some commentators favour this

---

1. See P. Duhem (1902), <u>Les Théories Electriques de J.C. Maxwell</u>, page 62.

2. See, for example, P. Duhem (1905), <u>The Aim and Structure of Physical Theory</u>, page 98.

story of compensating errors.[1]   But did Maxwell insert a $\sqrt{2}$ by

chance, then later omit a $\sqrt{2}$ by chance, to obtain the right answer?

I can think of two better explanations and both suggest that the

objective problem was to model the propagation of light.   Maxwell

was familiar with the work of Kohlrausch, Kirchoff, and Weber.   And

in 1857 Kirchoff and Weber predicted the existence of electrical

actions travelling at the speed of light, and that this speed was

proportional to the ratio of the electrical units.[2]   But they used

electrodynamic units of current, whereas Maxwell used <u>electromagnetic</u>

units of current, and there is a conversion factor of $\sqrt{2}$ between

these two units.   Had Maxwell been trying to model light, and had

he used Weber's numerical relationships as a pattern, he could easily

have overlooked a $\sqrt{2}$.   My other idea concerns waves.   The

appropriate formula for speeds of waves depends on how many types

of wave the medium will support -- if a medium has two degrees of

freedom and permits longitudinal as well as transverse waves then a

factor of $\sqrt{2}$ can appear in the velocity expression.   Had Maxwell

been aiming at a theory of light -- which would have to allow only

transverse waves -- then his use of an elastic solid, which permits

both longitudinal and transverse waves, leaves him open to making a

mistake of a $\sqrt{2}$ in a velocity.   Either way here one mistake is made,

and an <u>ad hoc</u> adjustment is made elsewhere to ensure that the correct

speed is obtained.   There is no genuine test.

My view does not stand unopposed.   There is a body

---

1. See, for example, L. Rosenfeld (1957), 'The Velocity of Light and
   the Evolution of Electrodynamics', page 1658.

2. See, for instance, G. Kirchoff (1857), 'On the Motion of
   Electricity in Wires'.

of historical folk lore that the model must have been genuinely
predictive because there could have been no 'fudging'. The
argument is that Maxwell did not know anything about the ratio of
the electrical units and the connection between this ratio and the
velocity of light. Helmann sums up this evidence:

> Maxwell formulated his theory of light without being aware
> of a paper by W. Weber and R. Kohlrausch, ... in which the
> ratio ... was determined. ... That Maxwell was unaware of
> this paper is clear from a letter to Faraday of 19 October
> 1861 in which he stated that 'I have determined the velocity
> of transverse vibrations .... The coincidence between
> velocities is not merely numerical. I worked out the
> formulae in the country before seeing Weber's number ...
> and I think we have now strong reason to believe, whether
> my theory is a fact or not, that the luminiferous and
> electromagnetic medium are one' ... This statement, and
> the way in which his wave-equation was derived from the
> model, clearly show the unexpectedness of the result. See
> also a letter to Thomson of 10 December 1861 where he repeated
> this statement: 'I made out the equations in the country
> before I had any suspicion of the nearness between the two
> values of the velocity of propagation of magnetic effects
> and that of light' ... The first indication of the numerical
> equivalence of the velocities of propagation of light and
> electricity was by G. Kirchoff in 1857, ...Maxwell seems to
> have been unaware of this paper, in which little significance
> is attached to the numerical equivalence. ...[1]

But this is not the full story. There is a paper of
Maxwell's that few historians know of -- his (1863) 'On the
Elementary Relations between Electrical Measurements'. This paper
is usually overlooked because it was omitted from the collected
edition of Maxwell's scientific papers. In it Maxwell discusses the
determination of the ratio of electrical units, and in particular he
refers to an attempt by Thomson in 1860 to determine this quantity.
Thomson read a paper to the Royal Society on the topic and the paper
was published in the 1860 Proceedings of the Royal Society.[2] In turn,

---

1. P.M. Helmann (1970), 'Maxwell and the Modes of Consistent
   Representation', footnote 130.

2. W. Thomson (1860), 'Measurement of the Electrostatic Force
   Produced by a Daniell's Battery'.

Thomson in his paper refers to Weber's determination of the ratio

of the units.   I say that Maxwell knew of Weber's calculations

before he started to write 'On Physical Lines'.   What about

Kirchoff's paper?   Heimann does not offer any evidence here -- he

merely states that Maxwell 'seems unaware' of it.   Kirchoff's paper

was reprinted in English, being published in the widely read

Philosophical Magazine, and part of it reads :

> The velocity of propagation of an electric wave is here
> found to be $= \dfrac{c}{\sqrt{2}}$, ...: its value is 41950 German
> miles in a second, hence very nearly equal to the velocity
> of light in vacuo.[1]

The paper was not some obscure work.   Its subject was transmission

lines, and the major research area in electrodynamics at this time

was the application of the theory of transmission lines to cable

telegraphy.   And Maxwell does tell us in 1854 that he has read

Kirchoff's earlier papers on currents in conductors.[2]   Further, it

is clear from the references that Maxwell makes throughout his work

that he read all the publications of the Continental A.A.D. school[3] --

are we to believe that he omitted from his readings a paper of

Kirchoff published in English in the Philosophical Magazine?   The

balance of evidence is that Maxwell knew of Weber's and Kohlrausch's

determination of the ratio of the units, that he knew that this ratio

had the dimensions of a velocity, that he knew of Kirchoff's

prediction of electrical actions travelling at the speed of light,

and that he knew of the connection between this speed and the ratio

of the units.   I think that Maxwell may not have known, or had total

----

1. G. Kirchoff (1857), page 406.

2. See page 10 of his November 1854 letter to Thomson in Larmor
   (1937).

3. And see also L.Campbell and W.Garnet (1884), The Life of James Clerk
   Maxwell.

recall, of the exact figure (and experimental error) that Weber and

Kohlrausch arrived at.

What about Heimann's other assertion that 'the way in

which his wave-equation was derived from the model, clearly show[s]

the unexpectedness of the result'?   I maintain that Heimann is

mistaken.   Looking more closely at the 'On Physical Lines' paper.

It is in four parts.   Part 1 is introductory, and part lV is on

the rotation of the plane of polarization of light -- neither concern

us.   Part II is on the vortex induction model and Part III is on

the static electricity-transverse wave model.   Maxwell intended

the paper to end with Part II.   He wrote, completed, and published

that much of it before he had the afterthought of writing a

Part III.[1]   This historical division is reflected heuristically.

The induction model is hydrodynamical, whereas the static electricity

one is an elastic solid.   It is true that Maxwell links one to the

other -- he stops the vortices rotating and assumes that they have

'elasticity of figure' so that they may distort.   Why does Maxwell

do this?   He tells us:

> ... it is necessary to suppose, in order to account for the
> transmission of rotation from the exterior to the interior
> parts of each cell, that the substance in the cells possesses
> elasticity of figure, similar ... to that observed in solid
> bodies.[2]

It is unlikely that this is the real reason for the velocity

---

1. He wrote Part III while in Scotland during the summer of 1861.
(See, for instance, Maxwell's 1861 letter to Thomson in Larmor
(1937) page 34.)   Six months elapsed between his writing Part II
and Part III.

2. Maxwell (1862), page 489

distribution within a vortex is of no interest.   Maxwell continues:

> The undulatory theory of light requires us to admit this
> kind of elasticity in the luminiferous medium, in order
> to account for transverse vibrations.   We need not then
> be surprised if the magneto-electric medium possesses the
> same property.[1]

That is, the model was heuristically ad hoc relative to the induction

model, and the speed of propagation was not a genuine prediction.

One expert -- Joan Bromberg -- tries to rescue Maxwell thus.

She suggests that it was supposed that there were two elastic solid

aethers -- one optical and one electromagnetic -- and the novel

prediction was that the two were identical.   She writes:

> [in this early section] there is no mention of the optical ether.
> Were it the case that MAXWELL wanted to offer a physical theory
> of electromagnetism, one would expect the optical ether to enter.
> For then his task would have been to look at that medium already
> thought to fill space, and to investigate whether it had, or
> could be given, properties which would also give rise to
> electromagnetic effects.   In this case, the identity of the
> optical and electromagnetic ethers would not have been the final
> and unexpected result, but the starting assumption for "On
> Physical Lines".   As it was, however, the theory developed
> differently.   Maxwell first invented a mechanical electro-
> magnetic medium, and subsequently discovered it could be
> identified with the optical ether.
>   When we come to the pages of Part III of "Physical Lines",
> we see, as the argument is developed, a gradual growth of a
> conviction of this identity.   The first mention of the optical
> ether is on the first page .... After he endows the electro-
> magnetic ether with the additional property of elasticity of
> figure, MAXWELL brings in the optical medium to support the
> plausibility of this idea .... Now the sense here is of the
> electromagnetic and optical ether as two similar but distinct
> media.   In his next mention of the light-bearing medium,
> however, three pages later, MAXWELL reports he has shown its
> elasticity to be the same as that of his electromagnetic ether,
> and strongly raises the question whether "these two coexistent,
> coextensive, and equally elastic media are not rather one
> medium" ... Finally, ... at the end of the velocity computation,
> he concludes, ... that the two are identical.[2]

This is grossly implausible.   How could a scientist

mindful of Faraday's discovery that magnetism rotates polarized light

---

1. Maxwell (1862), page 489

2. Bromberg (1967), page 226.

have supposed that there were two similar but distinct media?[1]   And

Maxwell was mindful of the discovery -- he devotes Part IV of

'Physical Lines' to rotation of polarization, he tells in it that the

vortex approach is developed from Thomson's and Faraday's ideas on

the subject, and he re-affirms this in letters to Thomson.[2]

Maxwell set out in Part III to investigate whether the one

aether had, or could be given, properties to produce electromagnetic

effects, and there is no independent way to test the construction he

produced.

The relationship between refractive index and dielectric

constant remains to be discussed.   The problem for Maxwell here is

that this relationship failed badly for all the substances that were

considered.

Maxwell later wrote:

> The only dielectric of which the capacity has been hitherto
> determined with sufficient accuracy is paraffin, for which ...
> ... K = 1.975 ...
> ... the index of refraction ... would be about 1.422.
> The square root of K is 1.405.
> The difference between these numbers is greater than can be
> accounted for by errors of observation, and shews that our
> theories of the structure of bodies must be much improved
> before we can deduce their optical from their electrical
> properties.[3]

So if the model had genuinely entailed this relationship, the model

would have been refuted by it.[4]

Both of Maxwell's models have numerous difficulties of

their own.   Even at the qualitative level the induction model is

---

1. See also my Chapter 1 Section 8.

2. See, for instance, the 1861 letter to Thomson in Larmor (1937),
   page 34.

3. Maxwell (1873) $\oint$ 789.

4. Maxwell was unlucky.   The relationship is accepted nowadays.   The
   difficulty results from the static dielectric constant not being
   the same as the high frequency dielectric constant.

baffling. The magnetic field around a current carrying wire is not uniform, so the peripheral velocities of the vortices must not be uniform, so the idler electrical particles must travel in space -- does this mean that a steady current induces a current in space? Also the idler particles cannot travel freely in space for otherwise the vortex motion would not penetrate the surrounding space -- there must be resistance to translatory motion. Turning now to a second conductor. There must in it be resisted translatory motion -- if there was no translation there would be no induction, if there was no resistance a steady current in the first wire would induce a current in the second. How then is the conductor to be distinguished from the space so that all the phenomena occur where they should? Maxwell does not tell us.[1] And what are these 'electrical particles' whose motion constitutes a current but which themselves are electrically neutral. Maxwell writes of their role:

> I do not bring it forward as a mode of connexion existing in nature, or even as that which I would willingly assent to as an electrical hypothesis. It is, however, a mode of connexion which is mechanically conceivable, and easily investigated, and it serves to bring out the actual mechanical connexions between the known electromagnetic phenomena; so that I venture to say that anyone who understands the provisional and temporary character of this hypothesis, will find himself rather helped than hindered by it in his search after the true interpretation of the phenomena.[2]

Finally, the wave model seems to suggest that a transverse wave could be initiated merely by moving an uncharged, current free, conductor, and such a wave could presumably be detected by any other conductor -- no such sympathetic vibrations were known.

---

1. But Everitt does, on page 96 of his (1975) -- there is resistance in space, and none in the conductor. This interpretation cannot be right.

2. Maxwell (1862), page 486.

4. The Later Theory :

Many historians relate that Maxwell was dissatisfied with some of the mechanical details of the theory of 'On Physical Lines of Force' -- for instance, those of the particulate electricity -- and that he wished to restructure the theory on a firmer base.   The result was the later theory, which on this account becomes a sophisticated version of the early theory and a natural heuristic development of it.[1]

I disagree with this view.   The later theory does not have the early theory as an ancestor -- it is instead descended from accepted electrical science.   The early theory is a mechanical model which is heuristically faithful to the aims of the Field Programme; in contrast, the later theory is axiomatic and has as its main feature a phenomenological and electrical derivation of the existence of transverse e-m waves in dielectrics.   The derivation -- essentially the one used today -- required exceptional mathematical and physical skills on Maxwell's part; but I must emphasize that its deductive base was A.A.D. background. Maxwell then tried to impose on the electromagnetic postulates a mechanical interpretation -- on some occasions he insisted that the equations were known to be about certain definite mechanical properties of a medium,[2] and on other occasions he applied the Lagrangian methods of generalized coordinates to electromagnetism and inferred from this application that electromagnetism concerned a mechanical aether.[3]

---

1. See, for example, L.P.Williams (1966) or R.A.R.Tricker (1966), The Contributions of Faraday and Maxwell to Electrical Science.

2. See, for instance, Maxwell (1873) $\oint$ 831.

3. See Maxwell (1873) or Maxwell (1873b), 'Electromagnetism'.

Maxwell offered several alternative derivations of travelling waves from 1865 through to 1873. He first found that a transverse magnetic field could be propagated, later he was able to show that a transverse electrical field could travel, and also (after some difficulties with the gauge condition[1]) that the vector potential could be propagated.[2] I will explain the postulates and show how the first derivation was made.

The theory and derivation occur first in his (1865) 'A Dynamical Theory of the Electromagnetic Field'. I will quote a section of that paper in full, then, as I discuss the equations, translate the component notation into modern vector notation.

> In these equations of the electromagnetic field we have assumed twenty variable quantities, namely,
>
> For Electromagnetic Momentum .... F G H
>   " Magnetic Intensity .... $\propto$ β γ
>   " Electromotive Force .... P Q R
>   " Current due to true Conduction .... p q r
>   " Electric Displacement .... f g h
>   " Total Current (including variation of
>   displacement).................... p'q'r'
>   " Quantity of Free Electricity .... e
>   " Electric Potential .... Ψ
>
> Between these twenty quantities we have found twenty equations, viz.
>   Three equations of Magnetic Force .... (B)
>   "   Electric Currents .... (C)
>   "   Electromotive Force .... (D)
>   "   Electric Elasticity .... (E)
>   "   Electric Resistance .... (F)
>   "   Total Currents .... (A)
>   One equation of Free Electicity .... (G)
>   "   Continuity .... (H)
>
> These equations are therefore sufficient to determine all the quantities which occur in them, provided we know the conditions of the problem. In many questions, however, only a few of the equations are required.[3]

---

1. Maxwell found that a Coulomb gauge, in which div $\underline{A}$ = 0, suited his derivations, but he was not satisfied with his own physical arguments for the truth of this condition. See P.F.Cranefield (1954),'Clerk Maxwell's corrections to the page proofs of "A dynamical theory of the electromagnetic field"'.
2. For a discussion, see A.M.Bork (1967), 'Maxwell and the Electromagnetic Wave Equation'.
3. Maxwell (1865) $\S$ 70.

Equation B relates Magnetic Intensity -- $\underline{H}$ -- to the

Electromagnetic Momentum (the vector potential), it is

$$\mu\underline{H} = \text{curl } \underline{A} \qquad \cdots\cdots\cdots\cdots\cdots\cdots \quad B'$$

where μ is the coefficient of magnetic induction of the particular

circuit.[1]   This equation is a particular case of a standard mathematical

result related to a consequence of Ampère's law that div $\underline{H}$ = 0.

Equation C connects Magnetic Intensity $\underline{H}$ to Total Current :

$$\text{curl } \underline{H} = 4\pi \underline{i}_{total} \qquad \cdots\cdots\cdots\cdots\cdots\cdots \quad C'$$

This postulate, which was discussed in the Introduction, is part of the

Ampère-Weber A.A.D. background, if the Total Current is understood to

mean all flows of electrical fluids.

Equation D relates Electric Force $\underline{E}$ found in a moving

conductor to the sum of the induced e.m.f.,arising (a) from its

movement and (b) from change of magnetic field ('transformer action'),

and the static electric field :

$$\underline{E} = \mu(\underline{v} \times \underline{H}) - \frac{\partial \underline{A}}{\delta t} - \nabla \psi \qquad \cdots\cdots\cdots \quad D'$$

Here the rate of change of vector potential has been taken as

an unanalysed description of Faraday's results on electromagnetic

induction and it has, by mathematical manipulation for a particular case,

been divided up into motional action and transformer action.   Finally the

static electric field has been added.

---

1. I have used the standard modern symbols which I consider portray most
aptly the quantities of Maxwell.

Equation E equates Electromotive Force and Electric Displacement :

$$\underline{E} = k \underline{D} \quad \dots\dots\dots\dots\dots\dots\dots\dots\dots\dots \quad E'$$

Equation F is Ohm's law for isotropic substances :

$$\underline{E} = -\rho \underline{i}_{conduction} \quad \dots\dots\dots\dots\dots\dots\dots\dots\dots \quad F'$$

Equation A governs total motion of electricity as a source of magnetic intensity and relates total current to conduction current and 'displacement current' :

$$\underline{i}_{total} = \underline{i}_{conduction} + \frac{\partial \underline{D}}{\partial t} \quad \dots\dots\dots\dots\dots\dots\dots \quad A'$$

Equation G relates free positive electricity e to Displacement :

$$div \underline{D} = -e \quad \dots\dots\dots\dots\dots\dots\dots\dots\dots\dots \quad G'$$

Equation H is the continuity equation for conduction current :

$$div \underline{i} = -\frac{\partial e}{\partial t} \quad \dots\dots\dots\dots\dots\dots\dots\dots\dots\dots \quad H'$$

The first derivation of the wave equation is carried out in Sections 93-5 of the (1865) paper. The two assumptions made are that the medium is a perfect dielectric in which there are no true conduction currents, and that there are no conductors or motions of conductors. The relevant equations then become :

$$\mu \underline{H} = curl \underline{A} \quad \dots\dots\dots\dots\dots\dots\dots\dots\dots\dots \quad B'$$

$$curl \underline{H} = 4\pi \underline{i}_{total} \quad \dots\dots\dots\dots\dots\dots\dots\dots\dots \quad C'$$

$$\underline{E} = -\frac{\partial \underline{A}}{\partial t} - \nabla \psi \quad \dots\dots\dots\dots\dots\dots\dots\dots\dots \quad D''$$

$$\underline{E} = k \underline{D} \quad \dots\dots\dots\dots\dots\dots\dots\dots\dots\dots \quad E'$$

$$\underline{i}_{total} = \frac{\partial \underline{D}}{\partial t} \quad \dots\dots\dots\dots\dots\dots\dots\dots\dots\dots \quad A''$$

B, C, and A" combine to give :

$$\mu 4\pi \frac{\partial \varrho}{\partial t} = \nabla \nabla . \underline{A} - \nabla^2 \underline{A} \qquad \ldots\ldots \text{ (1)}$$

Differentiating the combination of D" and E' yields :

$$\frac{\partial \varrho}{\partial t} = \frac{1}{k} \left[-\frac{\partial_2 A}{\partial t^2} - \nabla \frac{\partial \psi}{\partial t} \right] \qquad \ldots\ldots \text{ (2)}$$

(1) and (2) combine to give :

$$\mu 4\pi \left[\frac{\partial_2 A}{\partial t^2} + \nabla \frac{\partial \psi}{\partial t} \right] + k \left[\nabla \nabla . \underline{A} - \nabla^2 \underline{A} \right] \ldots\ldots \text{ (3)}$$

Curl of (3) provides :

$$\mu 4\pi \left[\frac{\partial_2 (\nabla \times \underline{A})}{\partial t^2} \right] + k \left[-\nabla^2 \nabla \times \underline{A} \right] = 0 \qquad \ldots\ldots \text{ (4)}$$

then substituting from B' :

$$\mu 4\pi \frac{\partial_2 \mu \underline{H}}{\partial t^2} - k \nabla^2 (\mu \underline{H}) = 0$$

which is a standard wave equation for $\mu\underline{H}$, (and Maxwell showed also

that the wave is purely transversal).   The velocity of such a wave .

is $\sqrt{\dfrac{k}{4\pi\mu}}$; this retains the connection with Weber and Kohlrausch's

velocity figure, and the relationship between refractive index and

dielectric constant.

5. A Critical Appraisal of the Later Theory :

I will argue that the later theory was heuristically ad hoc, and that it did not predict novel facts. Thus, the advent of the later theory did not make the Field programme progressive.

There are two unsatisfactory heuristic aspects to the later theory. The theory was not directed at explaining induction, which was the unsolved problem of the early theory; instead the later theory postulated the rate of change of the vector potential as an unanalysed description of induction.[1] Secondly, the later theory was not the byproduct of the search for the properties of a single mechanical aether; the heuristic path to it used purely electrical arguments and only later was an aether interpretation applied to it. As Chalmers states, in my view correctly :

> Maxwell's successful innovations in electromagnetism were not
> occasioned by his desire to reduce that branch of science to
> mechanics. The displacement current did not emerge as a result
> of his attempts to cast electromagnetism in the framework
> of Lagrangian mechanics, nor did it emerge as an inevitable
> consequence of his attempts to construct a mechanical model.
> Its introduction was supported by electrical rather than
> mechanical arguments.[2]

Maxwell's much admired use of Lagrange's equations needs further discussion.[3] Some argue that Maxwell's electrodynamic equations were derived from a Lagrangian application of general dynamics.[4] If indeed there was a mechanical aether, then it would be governed by

---

1. And actually the Field programme was never able to explain induction.

2. For the sixty page argument that Chalmers uses in support of his thesis, see A.F.Chalmers (1973a), 'Maxwell's Methodology and His Application of It to Electromagnetism'.

3. The admirers include H.Poincaré in his (1905), Science and Hypothesis, pages 216, 222, and 223 and R.T.Glazebrook in his (1896), J.C.Maxwell and Modern Physics, page 179.

4. See, for example, T.K.Simpson (1970), 'Some Observations on Maxwell's Treatise on Electricity and Magnetism', page 249.

Lagrange's equations -- which is to say that if Maxwell's discoveries

resulted from the use of Lagrange's equations then the discoveries

would be evidence for the Field programme's thesis that there was

a mechanical aether, and furthermore Maxwell's theories would be

acceptable heuristically.

I maintain that as a matter of fact Maxwell's equations were

not the outcome of a Lagrangian analysis -- Maxwell first produced the

electromagnetic equations and then cast them in a Lagrangian form.

And I suggest also that Lagrangian analysis is not a powerful heuristic

aid in this type of case.

Lagrange's equations, in their original role in analytical

dynamics, are useful transformations of Newton's equations for systems

of particles. The transformation is usually made from Cartesian

coordinates to generalized coordinates, and in most cases of interest

there are constraints which enable the number of generalized coordinates

to be reduced to the number of degrees of freedom of the system.

For example, a wheel free to rotate on a fixed axle has only one degree of freedom and is thus governed by one Lagrange equation[1]; whereas many Newtonian equations are required to describe the dynamics of the vast number of particles which constitute the wheel. In addition to the generalized coordinates of Lagrange there are generalized velocities, generalized forces, and generalized momenta -- these concepts are given purely analytical definition, for example the time derivative of a generalized coordinate is a generalized velocity. The final component of the Lagrangian method is energy.   There is the kinetic energy of the system which is the sum of the kinetic energies of the individual particles, and of lesser importance is the potential energy (if there is one) which, when differentiated with respect to the appropriate generalized coordinate, yields the generalized forces.   Lagrange's equations relate the generalized forces to derivatives of the kinetic energy, and they are sufficient to predict the time development of the system of particles.   From a mathematical point of view the equations achieve no more and no less than Newton's equations, if the focus of interest is total information about all the individual particles.   But often the focus of interest is limited, and then Lagrange's equations may have an advantage.   A typical case is where the behaviour of the generalized coordinates is all important, and where there are known constraints.   Maxwell's

---

1. I assume here that the interest is confined to rotatory motion of the wheel.   Engineers may be concerned whether flywheels disintegrate, and to calculate forces of constraint larger numbers of Lagrangian equations would have to be used.   (In these cases the number of generalized coordinates is larger than the number of degrees of freedom of the system.)

favourite example was that of church bells[1] -- in his example there is

a bell mechanism which is inaccessible in the bellfry, and there are

exposed bell ropes which operate the mechanism; the investigator's concern

is solely with the behaviour of the ropes -- how they respond to forces

and displacements -- and Lagrangian analysis is used to yield all the

information that can be known about the behaviour of the ropes.

This type of problem is one where there is a hidden Newtonian

mechanism with observable parameters, and the interest is confined to the

observables. Lagrangian analysis will apply to these cases if the

observables determine the state of the mechanism, and if the kinetic

energy of the hidden machinery can be evaluated.

Maxwell's wish was to analyse electromagnetism in this fashion.

Electromagnetic effects were the observable epiphenomena of the aether

and thus for him played the role of the observable parameters of a

hidden Newtonian mechanism. Accordingly Lagrange's equations should be

able to yield all the information that can be known about electromagnetic

effects.

I maintain that there is no rational procedure for applying

Lagrangian techniques to the aether to produce descriptions or explanations

of electromagnetic effects or to make discoveries about electromagnetic

effects. Consequently Maxwell's electromagnetic postulates could not

have had their origins in rational application of Lagrange's equations.

I offer three arguments; these concern the observable-unobservable

---

1. Maxwell (1868b), 'Thomson and Tait's Natural Philosophy', page 783.

distinction, the generalized coordinates, and the kinetic energy.

Maxwell's bell example is unfairly favourable to his enterprise -- in it there is a clear distinction between the hidden mechanism and the observable ropes. Such clarity does not in general exist and does not exist in the case of the aether. With the aether, it is not obvious what the candidates for the observables are. For example, Maxwell takes electric current (and even displacement current) as an observable parameter partially determining the state of the aether, but electric current was discovered only around 1800, prior to that date it was an unobserved observable; there may yet be undiscovered observables. The appropriate bell analogy would be the following. There is a hidden bell-mechanism and there are some observable bell-ropes in the bell-ringers' room; furthermore, there may be other undiscovered bell-ropes perhaps in another unlocked, but as of yet unopened, bell-ringers' room. The investigator must make a conjecture as to the candidates for observables, but -- as I shall show -- Lagrangian techniques offer no rational way of refining such conjectures.

Maxwell's bell example is also unfairly favourable over the constitution of the generalized coordinates. In that example the coordinates of the ropes are guessed to be the generalized coordinates, the velocities of the ropes then become the generalized velocities, and the forces on the ropes are guessed to be the generalized forces.

But mathematically the generalized concepts are given a purely analytical definition and usually these generalized concepts will not have the same dimensions as the ordinary concepts; for instance, generalized forces will often not have the dimensions of a force. The investigator must make a conjecture as to the constitution of the generalized coordinates, and it is not permissable for him to assume that these must be positions or that the generalized velocities are the velocities of moving objects or that the generalized forces are pushes or pulls.

Of crucial importance in Lagrange's equations is the kinetic energy <u>of the hidden mechanism</u>. In standard applications, where the mechanism is not hidden, the kinetic energy can be determined -- with the spinning wheel example there is a Newtonian formula for the kinetic energy of a wheel. In these cases, where the kinetic energy is known, there can be a independent check of the ongoing Lagrangian analysis of generalized coordinates, and certain types of errors can be rectified. And in other cases, where there is a hidden mechanism with known generalized coordinates and unknown kinetic energy -- as in Maxwell's bell example -- the kinetic energy of the hidden mechanism can be operationally defined in terms of the behaviour of the generalized observables, and again certain types of false conjectures can be improved upon. But with the aether, both the generalized coordinates and

the kinetic energy are unknown.   The investigator must make a

conjecture about the kinetic energy of the hidden mechanism of the

aether, and since he is unsure of the generalized coordinates he has no

independent test of his conjecture.

A failed Lagrangian analysis of the aether would not indicate

whether the conjectured generalized coordinates were incorrect, whether

they were incomplete, or whether the conjectured formula for the kinetic

energy of the aether was false.

Simpson completely misleads his audience when he writes :

> The relevance of $\lfloor$Lagrangian analysis$\rfloor$ to the problem of
> electromagnetism is immediately apparent : if indeed the field
> is to be regarded as a connected mechanical system, the positions
> and velocities of the conductors, together with the currents
> and integral-currents associated with them, constitute the
> generalized coordinates, and determine the configuration of the
> field at any moment.

Maxwell's electrodynamic equations and the existence of

Maxwell's displacement current were not produced by means of Lagrangian

analysis,[2] and from a mathematical point of view could not have been

produced effectively in this way.

The later theory was thus heuristically <u>ad hoc</u>.   But was it

testable ?   Did it predict novel facts ?   The postulates that Maxwell

favoured had several disadvantages.   They were formally inconsistent;[3]

they did not explain electromagnetic induction;[4] and -- with the

---

1. Simpson (1970) page 253.

2. The displacement current was always included at the beginning of the
analysis, not discovered by virtue of the analysis.   See, for example,
Maxwell (1873) $\S$ 604.

3. The derivation is given in Chalmers (1973a) pages 141 and f.

4. See also my Chapter 4 Section 10.

interpretation of the vacuum as a dielectric -- they abandonned a satisfactory theory of dielectrics. In compensation, the postulates offered a unified view of travelling electromagnetic waves. These elegant theoretical results were to the effect that there would be transverse but no longitudinal electromagnetic waves in dielectrics, including the vacuum. Light was suggested to be such a wave.

The obvious direct test of the electrical axioms is to produce and detect a travelling wave. Maxwell never tried this, and there is good evidence that he had no idea of how it might be done, even as an in principle thought experiment. His postulates obscured the nature of the sources and receivers of the waves; his derivations -- being plane-wave solutions with sources and receivers at infinity -- left him in ignorance. Furthermore the theory was directed at the electromagnetic band around visible light, which has a relatively high frequency -- it seems that Maxwell judged that the frequency of the travelling waves was so high as to defy artificial production in the laboratory. The whole question of a Maxwellian direct test of the existence of travelling waves is curious. Many scientists had produced, observed, and reported non-optical electromagnetic <u>radiation</u> before Maxwell's time. They did not know what it was that they were observing -- neither, it seems, did Maxwell know. Furthermore, all the technological and technical materials needed for a test were available to Maxwell, but he did not use them.[1]

---

1. See C.Susskind (1964), 'Observations of Electromagnetic-Wave Radiation before Hertz', T.K.Simpson (1966), 'Maxwell and the Direct Test of His Electromagnetic Theory' and A.F.Chalmers (1973b), 'The Limitations of Maxwell's Electromagnetic Theory'.

Maxwell did propose two tests -- the first his theory failed,
and the second, though not attempted, would not have distinguished
his theory from background knowledge.   The first test was to evaluate
the relationship between dielectric constant and refractive index -- the
equations failed this test, as Maxwell admitted.[1]   The second test
was to construct a sensitive galvanometer and to try to detect a current
within a   genuine 'dielectric' such as solid paraffin :

> According to this view [Maxwell's own] , the current produced
> in discharging a condenser is a complete circuit, and might be
> traced within the dielectric itself by a galvanometer properly
> constructed.   I am not aware that this has been done, so that
> this part of the theory, though apparently a natural consequence
> of the former, has not been verified by direct experiment.   The
> experiment would certainly be a very delicate and difficult one.[2]

This is a poor suggestion.   All the background theories asserted the
existence of polarization currents in dielectrics.   So the mere
deflection of a galvanometer needle in these circumstances -- had it
been produced -- was hardly a novel fact predicted by Maxwell's theory.
Maxwell seems to have been unable to suggest demanding tests of his
theory.   The second unperformed experiment could have been developed
into two reasonable tests.   It should have been tried in a vacuum,
since a vacuum current is a peculiarity of Maxwell's theory.   And
attention should have been directed to the magnitude of these currents
-- Maxwell's currents into dielectrics are circuital, whereas the
background theories' polarization and conduction currents are not --

---

1. See Section 3 of this Chapter.

2. Maxwell (1868), page 139.

so the important factor is the magnitude of the current within the dielectric, not its mere existence.

And there is a third test that Maxwell could easily have thought of. For him, displacement currents were exactly the same as transient conduction currents. He writes :

> Whatever electricity may be and whatever we may understand
> by movement of electricity, the phenomenon which we have
> called electric displacement is a movement of electricity
> in the same sense as the transference of a definite quantity
> of electricity through a wire is a movement of electricity;
> the only difference being that in the dielectric there is
> a force which we have called electric elasticity, which
> acts against the electric displacement ....[1]

In which case, since conduction currents -- even transient ones -- produce heat, so should displacement currents. A capacitor in a vacuum should generate heat while being charged. This test is simple to perform (and Maxwell's theory would have failed it).

_____

1. Maxwell (1873) § 62.

6.Summary :

In this Chapter I have :

a) argued that Maxwell's 'early' theory of electromagnetism, while initially heuristically acceptable, became heuristically ad hoc and simply ad hoc as it was developed into a theory of light,

b) concluded that Maxwell's 'later' theory of electromagnetism was heuristically ad hoc and made no successful novel predictions,

c) pointed out that the originality in Maxwell's theories lies in their suggestion that the vacuum is a dielectric,

d) shown that Maxwell's use of Lagrangian Mechanics did not make his later theory heuristically acceptable,

e) made suggestions regarding the source of the $\sqrt{2}$ error in his early theory,

f) refuted two theses of Simpson -- one concerning polarization currents producing magnetic effects and the other concerning Lagrangian analysis,

g) refuted the standard view, as expressed by Heimann, that there could have been no 'fudging' in the case of Maxwell's derivation in his early theory,

h) refuted Bromberg's thesis that the novel outcome of the early theory was the discovery that the electromagnetic and optical aethers were one and the same,

and

i) refuted minor theses of Everitt, Pearce Williams, Tricker, and others.

Chapter 6 : The A.A.D. Theory of Light.

1. Introduction.

2. Ludwig Lorenz's Theory of Light.

3. Hertz's 1884 Paper, 'On the Relations Between Maxwell's
   Fundamental Equations and The Fundamental Equations of
   the Opposing Electromagnetics'.

4. A Comparison of the A.A.D. Theory of Light and Maxwell's
   Theory of Light.

5. The Theory of Helmholtz.

1. Introduction :

The question of whether the electromagnetic theory of light gave the Field programme a decisive victory has still to be given a final answer. This will involve discussing the A.A.D. electromagnetic theory of light, and the development of the two programmes after 1860. The A.A.D. electromagnetic theory of light is that of postulating retarded potentials emanating from electron sources. The theory receives partial expression in Ludwig Lorenz's theory of light, but from the point of view of the A.A.D. programme Lorenz's theory must be incorrect and incomplete. The necessary refinements are indicated in Hertz's theoretical paper of 1884.[1]

It was Ludwig Lorenz who proposed the prototype of the A.A.D. theory of light, and his problem situation and the solutions he offered, in terms of retarding the scalar potential ϕ and the vector potential $\underline{A}$, are discussed in Section 2.

Little research was carried out in the pure forms of either of the two programmes between 1870 and 1900. Instead new hybrid programmes arose. Helmholtz developed a general potential theory which he maintained encompassed both Maxwell's and Weber's theories. Indeed it did so, but at the cost of disfiguring them -- Maxwell's theory became an A.A.D. theory with genuine sources, and Weber's theory became endowed with current element sources as a

---

1. H.Hertz (1884), 'On the Relations Between Maxwell's Fundamental Equations and the Fundamental Equations of the Opposing Electromagnetics'.

replacement for the atomic ones. Helmholtz favoured his own variant

of Maxwell's theory. His preference was based in part on good

reasons; and it was also based in part on bad reasons (as we have seen

in Chapter 4). There was a bitter dispute between Helmholtz and

Weber -- which extended as far as Helmholtz opposing the proposal,

made at the First International Congress on Electricity, to use

the name 'weber' to denote a unit of current.[1] The core of the

intellectual disagreement was that Weber's force law used velocity

dependent forces and Helmholtz held as a basic principle that

physics allowed of no such forces. Helmholtz writes in 1872 that

Maxwell's theory :

> proves that there is nothing in electrodynamic phenomena
> to compel us to attribute them to an entirely anomalous
> sort of natural forces, to forces depending not merely
> on the situation of the masses in question, but also
> on their motion.[2]

and he writes in 1873 :

> all the known effects of electro-dynamic action are subject
> to the great principle of conservation of energy, although
> a theoretical deduction of this universal principle of
> nature can be given only for forces ..... which are
> independent of motion.[3]

and in 1881 he writes :

> Nobody can deny that this new theory of electricity
> and magnetism, originated by Faraday and developed
> by Maxwell, is in itself well consistent, in perfect
> and exact harmony with all the known facts of experience,

---

1. See A.E. Woodruff (1968), 'The Contributions of Hermann von
Helmholtz to Electrodynamics', footnote 20.

2. H. Helmholtz (1872), 'Ueber die Theorie der Elektrodynamic, II',
page 532.

3. H. Helmholtz (1873), 'On Later Views of the Connection of
Electricity and Magnetism', page 248, my italics.

> and does not contradict any one of the general axioms of
> dynamics ..... Other eminent men have tried to reduce
> electromagnetic phenomena to forces acting directly
> between distant quantities of hypothetical electric
> fluids, with an intensity which depends not only on
> distance, but also on the velocities and accelerations ....
> All these theories explain very satisfactorily the
> phenomena of closed galvanic currents. But applied to
> other electric motions, they all come into contradiction
> with the general axioms of dynamics.[1]

Helmholtz should have given Weber's law a fairer hearing -- Helmholtz's

principle is false, and in the mid-19th. century there were many good

arguments for thinking it false and none for thinking it true.

Helmholtz's papers were extremely influential and in my view had a

detrimental effect on the perception of many scientists of

electrodynamics, for example on that of Hertz. The theory of Helmholtz

is considered in Section 5.

The younger continental scientists -- notably Hertz and

H.A. Lorentz -- used Helmholtz's theory as a starting point. Hertz

was one of Helmholtz's students[2]; and H.A. Lorentz's doctoral

dissertation was directed at the electromagnetic boundary conditions

between media, a problem that he had learned of from a footnote to

one of Helmholtz's papers.[3]

Hertz's research is important. Its significance lies not

with his well know experimental production of finitely propagating

electromagnetic waves in space, but instead with the lesser known

theoretical paper of 1884. The experiments are of value, they

---

1. H.Helmholtz (1881), 'On the Modern Development of Faraday's
Conception of Electricity', pages 280-1.

2. For the Helmholtz-Hertz relationship see S.D'Agostino (1975),
'Hertz's Researches on Electromagnetic Waves', section 2.

3. H. Helmholtz (1870), 'Ueber die Theorie der Elektrodynamic'
footnote to page 558.

did show the direct production of travelling electromagnetic

waves. But they are of limited value because their outcome did

not serve to confirm a novel prediction of a theory -- theories

in both the A.A.D. and Field programmes anticipated the result.

The experiments, which compared a wave in space with one in a

wire, also had shortcomings. Poincaré observed immediately

that Hertz had used the incorrect theoretical value for the velocity

of propagation in a wire and so there was a missing factor of $\sqrt{2}$;

Hertz himself knew that his results were awry due to the spatial

wave reflecting off objects in his laboratory; and he freely

admitted to falsifying the values he obtained, selecting those

that theory demanded.[1] In the 1884 theoretical paper Hertz

proves the equivalence of the retarded potentials of Ludwig

Lorenz and the equations of Maxwell. This meant that the elegant

mathematical derivations of Maxwell were available to A.A.D.

theoreticians for the vacuum case. The 1884 paper received a

hostile reception from Helmholtz and his followers, and it was

never again mentioned in print by Hertz.[2]

H.A.Lorentz's Electron Research Programme was a hybrid

programme which employed both atomic sources and a rest aether.

In the years around the turn of the century Lorentz's programme

was more popular than either the Field or the A.A.D. programmes.

Discussing the intellectual merits of Lorentz's programme is

---

1. See Hertz (1892), Electric Waves. p.

2. See D'Agostino (1975), pages 293 and 295.

not within the province of this dissertation, but I should remark

that Lorentz's programme can be seen naturally as a continuation

and development of the A.A.D. programme.[1]   Lorentz would have

described his own work as being initially in the A.A.D. tradition

and then later in the Field programme (and he meant by the Field

programme : Helmholtz's version of it).   What caused him to transfer

allegiance was his feeling that electrical actions should have

a finite velocity of propagation and that this required contiguous

action.[2]   Lorentz writes :

> I have tried to reduce all the phenomena to one, the
> simplest of all : the motion of an electrified body ...
> We see then that Maxwell's theory, in the new form I
> am about to give it, approaches the old ideas. ...
> [The] simple formulae regulating the motion of charged
> particles ... [can be regarded] as expressing a fundamental
> law comparable with those of Weber and Clausius.   But
> these equations continue to bear the impress of Maxwell's
> principles ....   In general terms we can say that [electrical
> actions] are propagated with a velocity equal to that of
> light.   Thus we return to an idea already expressed by
> Gauss in 1845, according to which the electrodynamic actions
> require a certain time to propagate themselves from the acting
> particle to the particle which experiences their effects.[3]

Lorentz's characterization of the difference between the programmes

is inappropriate -- as I argued in Chapter 1 Section 2 -- and

---

1. H.A.Lorentz's research is described in H.A.Lorentz (1909), Theory of
Electrons, T.Hirosige (1962), 'Lorentz's Theory of Electrons and the
Development of the Concept of Electromagnetic Field', T.Hirosige (1966),
'Electrodynamics before the Theory of Relativity 1890-1905', T.Hirosige
(1969), 'Origins of Lorentz's Theory of Electrons and the Concept
of the Electromagnetic Field', and R.McCormmach (1970), 'H.A.Lorentz
and the Electromagnetic View of Nature'.

2. See Hirosige (1962), Section 6.

3. H.A.Lorentz (1892), 'La Theorie Electromagnetique de Maxwell et son
Application aux Corps Mouvants', page 432 and f.   And McCormmach,
for example, writes on page 462 of his (1970) : ' In his [Lorentz's]
view electrodynamics should return to the theories of Weber ... while
at the same time retaining the core of Maxwell's theory -- the finite
propagation of electrical action'.

thus it indicates a false consciousness on Lorentz's part.

The affinity between Lorentz's theories and the A.A.D. programme

is shown by Lorentz's use of electron sources and of the key

unifying idea that static and dynamic electrical phenomena were

the outcome of electrons interacting under the one force law.

Lorentz solved an important problem for the A.A.D. programme.

Forty years earlier the A.A.D. electrodynamics of sources and

empty space had apparently been questioned by the existence of

dielectrics, but it had been shown that empty space and sources

governed by Coulomb's law were sufficient to explain the behaviour

of materials.[1] Now a similar problem had arisen. It appeared

that the A.A.D. view of sources, empty space, and one finite

velocity of propagation cannot explain the fact that light has

a different velocity in a dielectric to its velocity in a vacuum.

Lorentz showed that sequences of charged harmonic oscillators

respond to an impinging electromagnetic wave so that the manifold

resultant wave travels with a different velocity to its component

members. Dielectrics contain sequences of electron sources.

Thus Lorentz gave an explanation of why light travels slower in

a dielectric than it does in a vacuum and furthermore Lorentz's

explanation was independently testable, and actually confirmed,

as it related the velocity to the frequency of light and the

density of the dielectric. The aether that Lorentz invoked in his

---

1. See my Chapter 1 Section 6 and Chapter 3 Section 3.

theories was imponderable and at rest -- in contrast to that of

Maxwell[1] -- and as such was little more than a picturesque rep-

resentation of the electric and magnetic forces in space.   Lorentz

did allow for the propagation of energy across space and thus

his aether was akin to Maxwell's in being a seat of energy, but

for Maxwell the energy was strictly mechanical energy whereas for

Lorentz the energy was non-mechanical.   This feature of the location

of energy in space prevents Lorentz's work from being classified

as part of the A.A.D. programme.   It perhaps should be mentioned

that Lorentz incurred Helmholtz's ire for using a velocity

dependent force -- the Lorentz force law.[2]

1.He defined aether as a material substance, see his 'Aether' article
in Niven (ed.) (1965),and for him the relationship between aether
and moving matter was always problematic.

2. Heaviside used velocity dependent forces as a natural part of the
Field programme -- such forces were in use in the A.A.D. programme,
the Field programme, and Lorentz's hybrid programme.

2. Ludwig Lorenz's Theory of Light :

Ludwig Lorenz has never been widely known. This fact has often been lamented, mainly by those few historians sympathetic to the A.A.D. programme -- they claim that Lorenz proposed an electromagnetic theory of light equivalent, or superior, to that of Maxwell.[1] They support their view by pointing out that Lorenz suggested that the scalar and vector potentials be retarded and that this idea is the full modern electromagnetic theory of light. Lorenz's obscurity is explained by the fact that he was Danish and had :

> 'great difficulties in presenting his ideas and
> calculations in an accessible form.'[2]

This simple view and its subsidiary explanation are unacceptable -- it contains too much hindsight. Lorenz's own theory was bound to an interpretation in terms of contiguous action in a full space; and this means that as it stood it was not an A.A.D. theory. And the subsidiary explanation is just false. Lorenz wrote only one major paper in electrodynamics[3], and it was clear and it was published in German and in English in the major physics periodicals.

Lorenz's work is important, but not because he proposed a theory of light superior to that of Maxwell for neither Lorenz's

---

1. See A.O'Rahilly (1965), M.Pihl (1962), 'The Scientific Achievements of L.V.Lorenz', and R.W.P.King (1949), 'Review of Mogens Pihl : Der Physiker L.V.Lorenz ...'

2. Pihl (1962), page xxi.

3. L.Lorenz (1867), 'On the Identity of the Vibrations of Light and Electrical Currents'.

theory nor Maxwell's contributed to progression in a research

programme. Lorenz's theory has to be considered in the context

of the A.A.D. programme. Two aspects of Lorenz's research are

isolated by the A.A.D. programme as it was in the 1860's : that

he invented a new heuristic tool, and that he emphasized the

connections between retarded potentials and the theory of light

especially those relating to the boundary conditions between media

and to the velocity of light.

      Ludwig Lorenz almost certainly knew nothing of Maxwell's

papers in electrodynamics or of the equivalence between their two

sets of equations. (Most scientists, both on the Continent and in

England, did not become aware of Maxwell's work until Helmholtz

drew attention to it, and Hertz 'proved' the existence of Maxwell's

mechanical aether.) Lorenz took as electrodynamic background

the Weber-Kirchoff equations[1], which can be written in modern

notation :

$\underline{j} = \sigma \underline{E}$ 1		(A localized Ohm's law for a conducting medium, $\underline{j}$ is the current density vector, σ the conductivity, and $\underline{E}$ the electric force.)
$\underline{E} = \underline{E}_{el} + \underline{E}_{ind}$ 2		(The total electric force is the sum of the induced electric force and the electric force due to the static charge.)
$\underline{E}_{el} = - \operatorname{grad} \phi$ 3		(ϕ is the scalar potential)

---

1. See my Chapter 4 Section 8.

$$\text{div grad } \phi = -\frac{\rho}{e_\bullet} \quad \ldots \quad 4$$

(ρ is the charge density and $e_\bullet$ a constant.)

$$\underline{E}_{ind} = -\frac{1}{c^2}\frac{\partial \underline{A}}{\partial t} \quad \ldots \quad 5$$

($\underline{A}$ is the vector potential and c is the ratio of the electrical units.)

$$\text{div grad } \underline{A} = -\mu_\bullet \underline{j} \quad \ldots \quad 6$$

(μ is a constant.)

$$\text{div } \underline{j} = -\frac{\partial \rho}{\partial t} \quad \ldots \quad 7$$

(Continuity.)

$$\text{div } \underline{A} + \frac{\partial \phi}{\partial t} = 0 \quad \ldots \quad 8$$

(This was an auxiliary condition for Kirchoff.)

Earlier Lorenz had considered the question of the boundary conditions between media needed to yield the Fresnel formulas for reflection and refraction of light. He, in common with many other scientists, became convinced that no elastic solid aether could yield the Fresnel formulas (because of difficulties, mainly with the longitudinal wave); and so he sought as an intermediate step a condition on the light vector. The differential equation he proposed was :

$$\text{curl curl } \underline{u} = \frac{1}{a^2}\frac{\partial^2 \underline{u}}{\partial t^2} \quad \ldots \quad *$$

(where $\underline{u}$ is the light vector and a is the velocity of light.)

and he showed that this equation guaranteed transverse waves and that the Fresnel formulas would hold at an interface.[1] The equation was a desideratum for a theory of light.

He -- also in common with many other scientists -- assumed that light would be electrical in nature and involve propagated

---

1. This had been shown independently by MacCullagh in 1863.

effects.[1]   He writes :

> ... the entire action between the free electricity and the
> electrical currents requires time to propagate itself
> -- an assumption not strange in science, and which may in
> itself be assumed to have a certain degree of probability.[2]

In his earlier research in elasticity he had described propagated

effects by retarded potentials and he emphasized that a propagated

force function can be expanded as a Taylor series which, given

a reasonably high velocity of propagation, would remain consistent

with its experimental base of quasi-stationary phenomena.   He

had drawn attention here to a new heuristic tool.   He writes :

> It is at once obvious that the equations .... are not
> necessarily the exact expression of the actual law;
> and it will always be permissable to add several members,
> or to give the equations another form, always provided
> these changes acquire no perceptible influence on the
> results which are established by experiment.   We shall
> begin by considering the two members on the right-hand
> side of the equation as the first members of a series ...[3]

Equations 4 and 6 thus can be modified to :

$$\text{div grad } \phi \; - \; \frac{1}{v^2}\frac{\partial_2 \phi}{\partial t^2} \; = \; -\frac{\rho}{\epsilon_0} \quad \ldots\ldots \quad 4'$$

(where v is the velocity of propagation)

$$\text{div grad } \underline{A} \; - \; \frac{1}{v^2}\frac{\partial_2 \underline{A}}{\partial t^2} \; = \; -\mu_0 \underline{i} \quad \ldots\ldots \quad 6'$$

Such a modification is conservative in the sense that if v is infinite

the original equations are obtained, and if v is high the resulting

equations are not contradicted by experimental data.

Lorenz's problem is now in view.   How is an equation with

the form of * obtained from 1, 2, 3, 4', 5, 6', 7, and 8 ?   What

---

1. See my Chapter 4 Section 7, especially page 165.

2. Lorenz (1867), page 291.

3. Lorenz (1867), page 289, his italics.

has to be identified with $\underline{u}$, the light vector, and what with a, the velocity of light ?   Such a problem is to be solved by evaluating curl curl $\underline{x}$ for each of the vectors $\underline{x}$ which occur in the modified equations 1 - 8' and seeing if the form $\frac{1}{a^2}\frac{\partial^2 \underline{x}}{\partial t^2}$ can be obtained. Lorenz starts on this path but, as we will see, encounters an unfortunate success.

He shows that :

$$\text{curl curl } \underline{j} = -\frac{1}{c^2}\frac{\partial^2 \underline{j}}{\partial t^2} - \sigma\frac{\partial \underline{j}}{\partial t}$$

provided that he makes the heuristically determined identification of the velocity of propagation v of the retarded potentials and the value of the ratio of the electrical units c.   And he emphasizes that if c, v, and a (the velocity of light) are all identified then

$$\text{curl curl } \underline{j} = -\frac{1}{a^2}\frac{\partial^2 \underline{j}}{\partial t^2} - \sigma\frac{\partial \underline{j}}{\partial t} \quad \cdots \quad **$$

is obtained, and ** is similar to * but not identical to it.

At this point the problem solver's strategy is clear : he either discards ** as not having identical form to * and proceeds to evaluate curl curl $\underline{E}$, curl curl $\underline{A}$, and so on, or he scrutinizes the additional term $\sigma\frac{\partial \underline{j}}{\partial t}$ and ponders on its significance.   The best move is to do both -- Lorenz did only the latter.

The factor σ is the conductivity and Lorenz realized immediately first that in free space σ would be low and thus **

would approach the form :

$$\text{curl curl } \underline{j} = - \frac{1}{a^2} \frac{\partial^2 \underline{j}}{\partial t^2}$$

and second that the $\sigma \frac{\partial \underline{j}}{\partial t}$ term is a damping term which if σ is not

low rapidly removes a sinusoidal $\underline{j}$ solution with the result that

good conductors cannot support this type of transverse wave. And

Lorenz knew that good conductors like metals are opaque to light,

and that transparent materials like glass are poor conductors.

Lorenz simply identified the current density vector $\underline{j}$ with the

light vector $\underline{u}$ and concluded :

> 'the vibrations of light are themselves electrical
> currents'

Lorenz's theory may be summarized :

i) the potentials ϕ and $\underline{A}$ are retarded so that they propagate

at the velocity of light (which is the equivalent to the ratio of the

electrical units);

ii) the light vector is identified with the current density

vector;

iii) the problem of the boundary conditions for the reflection

and refraction of light are solved by means of (i) and (ii);

iv) in a laboratory vacuum (or interstellar space) the current

density vector must be non-zero and so the vacuum cannot be empty --

there must be in it electrons or conducting matter, light is

propagated through a vacuum by virtue of the contiguous action

---

1. Lorenz (1867), page 228.

of conducting matter, Lorenz writes :

> ... there is scarcely any reason for adhering to the
> hypothesis of an aether; for it may well be assumed that
> in the so-called vacuum there is sufficient matter to
> form an adequate substratum for the motion [electric current] [1]

Lorenz's theory fits well into the A.A.D. programme. For some time the task of the programme had been to find a retarded force conservation generalization of the Weber-Kirchoff equations.[2] And Lorenz had done just that, and in addition he had given the boundary conditions for an electromagnetic theory of light.   But what has to be rejected by A.A.D. theoreticians is the suggestion that the vacuum contains electrons.   The vacuum is just empty space and this means that the current density vector $\underline{j}$ has to be zero, and so the light vector cannot be the current density vector -- Lorenz's identification might hold in conducting matter, but it could not hold in empty space.   But since $\underline{j} = \sigma \, \underline{E}$, by the Weber-Kirchoff equation 1, the light vector could have been identified with $\underline{E}$ and $\underline{E}$ does not have to be zero in empty space.

----

1. Lorenz (1867), page 301.

2. See my Chapter 4 Section 7.

3. Hertz's 1884 Paper, 'On the Relations Between Maxwell's Fundamental Equations and The Fundamental Equations of the Opposing Electromagnetics' :

The thesis of Hertz's 1884 paper is that Maxwell's equations of electromagnetism are the best available.[1] The thesis develops from a premise in two stages, by means of a subsidiary argument and a separate proof. The premise is that the only two rival equations of electromagnetism are those of the instantaneous force law of Weber and those of the equations of Maxwell. The subsidiary argument is that Weber's law, when properly applied, leads to the retarded potentials of Riemann and Lorenz, and the separate proof is that the retarded potentials of Riemann and Lorenz are identical to the equations of Maxwell. Thus, Maxwell's equations follow from Weber's law, and Maxwell's equations follow from Maxwell's equations, therefore Maxwell's equations are the best available.

The merits of the subsidiary argument need not be discussed. The desire of A.A.D. theoreticians to replace instantaneous forces with retarded ones had been prominent for some time -- further motivation, whether persuasive or not, was unnecessary.

The proof of the equivalence of Maxwell's equations and the retarded potentials of Lorenz and Riemann is important. It means that the elegant mathematical derivations of Maxwell are available to the A.A.D. theoreticians for the vacuum case.

---

1. Hertz actually argues the stronger claim that Maxwell's equations are necessarily true.

Maxwell had shown that his axioms had as a consequence that there should be propagated transverse electric and magnetic waves; the retarded potentials of Lorenz and Riemann, which travel across empty space, also predict the existence of transverse electric and magnetic waves. It was now manifestly clear that for the A.A.D. theory the light vector has to be identified with the electric vector and not, as Lorenz had done, with the current density vector.

An unusual feature of Hertz's paper is that he does not compare the equations of Lorenz and Riemann with those of Maxwell. It is true that the two sets of equations are formally equivalent, and so have the same consequences; but they are embedded in separate programmes and have different interpretations -- as we shall see in the next section. I think that the reasons for this are that Hertz did not understand Maxwell's theory[1] -- his knowledge of it was from Helmholtz's generalized potential theory which transmogrified Maxwell's research; and Hertz seemed to believe in the strict identification of theories which had formally equivalent equations -- he writes :

> To the question 'What is Maxwell's Theory ?' I know of no
> shorter or more definite answer than the following : Maxwell's
> theory is Maxwell's system of equations. Every theory which
> leads to the same system of equations and therefore comprises
> the same possible phenomena, I would consider as being a form
> or special case of Maxwell's theory .....[2]

---

1. For the demands that Maxwell's theory places on the comprehension, see my Chapter 1 Section 6.

2. H.Hertz (1891), _Electric Waves_, page 21.

4. A Comparison of the A.A.D. Theory of Light and Maxwell's Theory of Light :

In this section I summarize the two theories of light. The M.S.R.P. does not provide the means of appraising individual theories from different programmes -- theories have to be considered as components of programmes and programmes are appraised. It is my contention that there was no decisive victory to the Field programme by virtue of its theory of light.

Maxwell's theories of light have already been explained at some length in Chapter 5. The theory is in essence that of transverse mechanical stresses in an all pervading medium. As J.J.Thomson, a British scientist sympathetic to the Field programme, writes in his B.A. report on Electrical Theories :

> This theory [Maxwell's] , which is called the electro-
> magnetic theory of light, might almost as justly be
> called the mechanical theory of dielectric polarization.[1]

The theory has no genuine electrical sources or receivers, and there was some indefiniteness over the questions of how to produce electromagnetic waves and how to obtain the Fresnel formulas for reflection and refraction of light. The prominent attraction of the theory was its unified approach -- no distinction was made between the vacuum and dielectrics and in consequence one theory applied directly to both.

Theories of the A.A.D. programme had acknowledged the existence of current waves in dielectrics long before the advent

---

1. J.J.Thomson (1885), Report on Electrical Theories, page 132.

of Maxwell's ideas. What was denied was the existence of this type of wave in a vacuum, since the vacuum was not a dielectric. The theory of retarded potentials constituted an account of electric waves in a vacuum (and it also produced a revision of the theory of current waves in dielectrics). The means of producing and detecting such waves was manifest -- Lorenz had given sine wave solutions for oscillating currents and charges -- and the problem of the Fresnel boundary conditions had been solved. The theory was heuristically acceptable as it was the end result of a thirty year search for an account in terms of retarded forces; but the theory, like Maxwell's, did not predict any novel facts.

In the late 1880's Hertz detected propagated electromagnetic waves. There were flaws in his experiments, but these need not concern us. The question is : does Hertz's result constitute a decisive victory for the Field programme ? Hertz and many other scientists thought so -- they all thought that the existence of a mechanical aether had been proved. But what are the objective relations between the experiments and the programmes ? Propagated waves were predicted by both programmes -- the experiments were a decisive victory to neither.

_____

5. The Theory of Helmholtz :

Helmholtz's generalized potential theory has been mentioned several times, and it is to be explained further in this section. Helmholtz saw his own paper[1] as a survey paper -- a'tour d'horizon' of the 'pathless wilderness' of competing electromagnetic theories. In this role the paper was hailed by later scientists -- like Hertz -- and by historians -- like Berkson.[2] Many researchers did learn electrodynamics from Helmholtz's paper, it was the only discussion of Maxwell's theories available on the Continent. And undeniably the paper was a stimulus to Hertz and to his experimental production of travelling electromagnetic waves. But Helmholtz's theory was not a good theory -- it did not provide a fair representation of the rivals -- and the scientists who learned electrodynamics from it were misled as to the characteristic features of the theories.

Helmholtz's theory was a general Neumann-type A.A.D. potential theory, using current element sources and a dielectric with parameters that could be varied to yield Weber's theory, Maxwell's theory, or the other theories. This dielectric was shown to support transverse and longitudinal current waves, the nature of which depended on the value of the parameters.

Philosophical considerations of the M.S.R.P. warn against the effectiveness of this. Weber's theory and Maxwell's theory are in separate research programmes -- a theory could encompass both only

---

1. Helmholtz (1870). There is an accessible account of Helmholtz's theory in Woodruff (1968).

2. W.Berkson (1974), Fields of Force.

by some misrepresentation.   And indeed this is what occurs.

Maxwell's theory is a unified no-source contiguous action

mechanical theory.   Helmholtz endows it with electrical sources

and makes it into a non-unified A.A.D. theory.[1]   The Field

theorists explicitly rejected Helmholtz's account of their views.

Heaviside writes :

> I made acquaintance with it in about 1886, and concluded
> that it would not do, being fundamentally in conflict
> with Maxwell's theory.[2]

And Larmor writes :

> [Helmholtz's] so-called extension of Maxwell's theory ...
> being based on distance actions is in conception entirely
> foreign to Maxwell's view of transmission by a medium.[3]

And the secondary source Rosenfeld writes :

> [Helmholtz's theory was not only] entirely alien to
> its [Maxwell's theory's] spirit, but it tended to obscure
> its characteristic features and to make the theory appear
> as a somewhat limiting case of the scheme.[4]

Weber's theory loses its atomic sources and gains as

a replacement current element sources.   One key idea that runs

through the A.A.D. approach is that electrostatics and

electrodynamics are to be united by means of one force law applied

to atomic sources.   And it is this that leads to an

explanation of electrodynamic induction and to the dissolution

of the distinction between inductive and ponderomotive electric

---

1. See also my Chapter 5 Section 1.

2. O.Heaviside (1912), _Electromagnetic Theory_, v.3, page 504.

3. J.Larmor (1900), _Aether and Matter_, page 274.

4. L.Rosenfeld (1957), 'The Velocity of Light and the Evolution of Electrodynamics', page 1665.

forces.   The Helmholtz version of Weber's theory obliterates
that idea -- the Helmholtz variant does not explain induction,
does not explain why Coulomb's law holds between charged bodies,
and re-introduces the distinction between inductive and ponderomotive
forces.   No A.A.D. theoretician should have accepted Helmholtz's
generalized view, and none did so.

_____

APPENDIX 1 : History and Philosophy of Science

1. *Introduction.*

2. *Inductivist versus Hypothetico-Deductivist Historiography : The Problem of Selection.*

3. *The Growth of Scientific Knowledge : The Problem of the History of Science.*

4. *Methodologies of Science : The Problem of Appraisal.*

5. *Methodological Bias : The Problem of Objectivity.*

6. *Lakatos's Suggestion : History of Science as a Test of its Methodology.*

7. *Rejection of Lakatos's Views.*

8. *Conclusion.*

1. *Introduction:*

What does the M.S.R.P.'s appraisal indicate? I maintain that it measures three properties. First it shows the <u>epistemo-logical</u> superiority of one theory in the series over its predecessor -- if one of two more or less similar theories makes a successful prediction which the other cannot account for, then that prediction can serve as objective grounds for preferring one theory. Thus, with a good developing programme, one can say that knowledge is grow-ing. Second, and probably the most controversially, often it can show the <u>epistemological superiority of one programme over its rival at a given time</u>. Lakatos regarded this question as a major pro-blem. The difficulty is that under the M.S.R.P. science seems to become a trivial game with good and bad moves, but what we wish is for science to give us knowledge, so there is the problem of arguing that good moves actually mean increase in knowledge. Lakatos's own answer was to postulate an inductive principle stating, roughly, 'good moves increase knowledge'. Postulation is not argument, though; but what is worse is that Lakatos states that <u>only</u> postulation can solve the problem. Let me quote him on this issue:

> We should here at least refer to the main epistemological
> problem of the methodology of scientific research programmes.
> As it stands, like Popper's methodological falsificationism,
> it represents a very radical version of conventionalism.
> One needs to posit some extra-methodological inductive principle
> to relate - even if tenuously - the scientific gambit of
> pragmatic acceptances and rejections to verisimilitude.
> Only such an 'inductive principle' can turn science from a
> mere game into an epistemologically rational exercise; from
> a set of lighthearted sceptical gambits pursued for intellectual
> fun into a - more serious - fallibilist venture of approximating
> the Truth about the Universe.

I maintain that Lakatos is wrong here. I will propose, without postulating, an answer using one of Popper's and Musgrave's ideas.

---

1. *Lakatos (1971) p. 101.*

*Finally, it indicates the heuristic power of a whole programme --
for the continuing discoveries are good evidence for the potential
to discover.*

*And there is another outstanding problem.   Any author of
a case-study ought to face the questions: Why should the case-study
be done? and How should the case-study be done?   These questions
give rise to Appendix 1.   This case-study is done because it des-
cribes the growth of a sector of knowledge and such accounts are
important.   (One can jokingly, but not entirely incorrectly, claim
that classical electrodynamics is one third of all knowledge.)   It
may also have value as a weapon for criticizing philosophies of
science, if Lakatos's thesis on the function of case-studies is
correct.   I will explain Lakatos's ideas on this later -- sadly I
think that there is not much in them.   The second question
occupies most of this Appendix.   The answer proposed is that the
history must be approached theoretically and fallibly and -- given
this -- it is preferable to do so from a methodologically advanced
standpoint which is explicitly stated.*

*(As further methodological remarks:-   I consider that the
positions advocated throughout this dissertation were defended, and
should be defended by arguments.   And the best arguments are those
which are valid and have true premises.   Ensuring that arguments
are valid is not difficult, although many of my arguments were en-
thymemic forms and thus had to be understood as though they have
their hidden premises made explicit.   Ensuring that premises are
true is another matter.   If the premises are logically true, then
in the main their truth can be recognized without further ado.   But
if the premises are intended to be true non-logically, then usually
more argument is needed.   And thus there is the possibility of an*

infinite regress. My way out of this was to regard all the positions advocated as conjectures to solve problems. Then my concern was only that of showing that conjectures were better than other known or easily imaginable conjectures to solve the same problem. Generally this process involved a series of valid arguments leading eventually to one or more premises tentatively accepted by all parties to the debate -- then, hopefully, these premises settled the matter. This means that the regress was taken back only as far as the supposed common ground, so all claims that were defended here were guessed to be uncontroversial or were derived from premises guessed to be uncontroversial. Another point on arguments is that stating the identity of the original proposer of an argument -- if such a person can be found -- adds nothing to an argument's strength; it is mere appeal to authority. Consequently in general I gave only the argument and did not try to re-inforce it by stating its 'source'. Of course, when I expounded or criticized existing interpretations, I first made clear the objective claims, which stood or fell on their own, and I then held that these objective claims were my sympathetic characterizations of the author's views.)

2. *Inductivist versus Hypothetico-Deductivist Historiography: The Problem of Selection:*[1]

How should one write history of science?

At first sight the answer is obvious: write a true and complete description of the historical events. Sadly the quick answer faces a devasting criticism: its design is Utopian because the end

---

1. *The contents of my brief introduction to some problems in historiography are dealt with at greater length in the standard texts on hypothetico-deductivism and historiography. See, for example, J. Agassi (1963), Towards an Historiography of Science, and p. 10f. of C.G. Hempel (1966) Philosophy of Natural Science.*

is impossible to achieve.

There have been men that have attempted to 'tell it like it was'. A famous one was Tristram Shandy.[1] He resolved to write in his diary everything that happened to him each day so that there would be a comprehensive and true description of his life. The trouble was that it took him a year to write up each individual day! And thus his diary was never finished. There is a lot to be learned from Tristram Shandy (but little from his diary). The key point is that the quantity of possible information far excedes the amount that can be written in a book.

Authors must therefore choose what is deemed to be the useful true information and include it, and they must omit the useless true information. This choice might be made in one of two styles: at random or under some principle.

A random choice amongst historical facts would lead to a 'shopping-list' history. What of value could come of this? What could emerge from a hotch-potch of facts about Thales, the Battle of Trafalgar, and Maxwell's displacement current? The main argument for this Inductivist Historiography is a criticism of the alternative idea that the selection should be made under some principle. It rests on the inductivist theory of error under which errors are the result of prejudice or preconception. The argument runs as follows. If the historian made a choice under some principle then he would be bringing some antecedently adopted point of view to bear on the history and this would mean that the historian's approach was biased and thus, as likely as not, that the history itself was error-ridden. But the criticism is unsound. The fault with this argument is that the adoption of a point of view does not have to mean bias. If the points

---

1. *See the novel by Sterne. This, and Swift's Gulliver's Travels, are, in part, criticisms of the inductivist orientated Royal Society.*

of view are made explicit and thus criticizable and the historian is permitted to choose which of the alternatives he adopts, there is no bias. The historian may be under the directives of his point of view but he is not enslaved by it -- one is not a slave if one can choose a master.[1]

Making a choice under a principle requires a principle which divides the world of facts up into one manageable portion which can be used and into another which can be discarded: it must lay down what is relevant. Excellent candidates for principles are hypotheses. A hypothesis partitions the world of facts into those which it permits or forbids and to those which are irrelevant to it. Facts can be said to be relevant only in respect of hypotheses and hypotheses can then do the job of selection procedures. Consequences can be drawn from a hypothesis and then the historical facts consulted to see if the conjecture is corroborated or refuted. And history books would consist of reports of such tests.

In a sense, this Hypothetico-Deductivist Historiography has a difficulty which is only one stage removed from the difficulty of selecting facts. How are hypotheses chosen? Where do hypotheses come from? In answer to these questions. A finite, and usually small, number of hypotheses are proposed as 'happy guesses' to solve problems, and in respect of one problem that hypothesis is defended which critical scrutiny reveals to be the best among the available

---

1. *For a fuller version of this counter see H. Poincaré (1905), Science and Hypothesis (Dover reprint 1952) page 143.*

explicit alternatives.[1]   *As critical scrutiny consists largely in*
subjecting a hypothesis to the trial by historical facts, the best
hypothesis can be recognized often only after the history has been
done.   In which case a history book should consist of a problem,
several tentative solutions to it, and a report of the test of the
solutions.

3. *The Growth of Scientific Knowledge: The Problem of the History*
   of Science:

     The Hypothetico-Deductivist historiography favoured here
requires one more item: problems for the historian.

     Which problems should the history of science address it-
self to?

     This is a difficult question to answer for in general it
is impossible to judge the value of a problem without solving it.[2]
Thus, the historian seems to be confronted with an existentialist
choice -- he must just pick a problem and hope that it leads to
something valuable.   However, while this is roughly correct, there
is one exception and this will provide our answer.   There is one
problem that is important and should be of concern to all historians
of science: it is the problem of the growth of scientific knowledge.

---

1. *At first sight facts can be relevant directly to problems; for*
example, the question 'Did Napoleon win the battle of Borodino?'
immediately points to the appropriate facts.   However, this sort
of problem does so only because it incorporates all the hypotheses
which can be used to solve it.   Most problems do not do this.   Take
'why-problems' -- for example, 'Why did Napoleon win?' -- these do not
have hypotheses attached and therefore do not indicate relevant
facts.

2. *And we cannot predict future solutions, or else we would have them*
now.   A fuller argument is available in Preface v of K.R.Popper
(1957), The Poverty of Historicism.   Of course, there can be a
rational debate as to the possible value of open problems.   But it
is as well to remember that these can go hopelessly awry.   Kepler's
problem, for instance, was 'Why are there six planets?' (see I.B.
Cohen (1960), The Birth of a New Physics, page 135) -- but there are
not six planets.

In turn this generates the historian's problem agenda and further actually provides a host of types of solution to these problems.[1] For example, say we hold that the growth of scientific knowledge consists of conjectures and experimental refutations, then some -- at least -- of the components of the agenda will be: Who put forward which conjecture and why and when was it proposed? Who refuted it and when and how did they do it? And, as an example of a solution type: say scientists abandoned a conjecture and we wished to explain this, the trial answer must be that the conjecture was refuted and so we search for an experimental refutation.

The problem of scientific knowledge is important for two reasons. It is interesting on philosophical grounds. Epistemology is the central issue of philosophy; many epistemological theories assert that current knowledge is so only in so far as it bears the correct relation to past views; so a knowledge claim requires an investigation of pedigree to see if the title is warranted. In short, epistemology needs history; the philosopher would like the historian to chart the development of views. The second reason is that scientific knowledge is used as a basis for technology, actions, and decision making. This means that a historian will be able to explain past technology, past actions, and past decision making only if he appreciates what was known at the time. A history of knowledge will thus be presupposed by other histories.

There have been two types of hypotheses as to the nature and growth of knowledge: dogmatic and fallibilist. The first type

---

1. See page 173f. of J. Worrall (1976), 'Thomas Young and the 'refutation' of Newtonian Optics' in C. Howson (ed.) (1976).

will be rejected and the second defended.[1]

Dogmatist epistemologies analyse 'P is known' as 'P is a proven certain truth'. For this view knowledge tends to be a black or white affair -- either P is or is not a proven certain truth; there is no middle ground, for there are no degrees of proven certainty.

The sceptic accepts the analyses and tenets of dogmatism but shows that when these are combined with his standard arguments they lead to the stunning conclusion: <u>There is no knowledge</u>. My position is that the sceptic's attack cannot be repulsed: there is no <u>certain</u> knowledge. The sceptic always uses the same strategy -- he takes a knowledge claim, say 'P is known', and then frustrates attempts at certain justifications of P; <u>he does not criticise P, he criticises only putative proofs of P</u>. The weapons used are the infinite regress argument, the invalidity of induction, the unreliability of authorities, and the possibility of perceptual or intellectual error. The sceptic asks 'Where is the proof of the certainty of P?'; and in reply he usually receives a justification of P on the basis of other statements; he then switches his attention to the other statements 'How are these known?' or 'Where is the proof of the certainty of these?'; thus either there is an infinite regress and no proof of certainty or the chain is stopped by a proven certain truth which is somehow guaranteed and needs no justification by other statements. Finally, the last door is closed. Typical candidates for proven certain truths not in need of justification by other statements are truths of tradition, the senses,

---

1. *Of value here is Lakatos's extensive classification and argument as used in I.Lakatos (1962), 'Infinite Regress and the Foundations of Mathematics', Section 1.*

or the intellect. All three are debarred by the possibility of error -- the existence of error does not mean that these alleged truths have to be false, but it does mean that these alleged truths are not certain truths. For example, on occasions the senses are mistaken, this is not to say that they are always mistaken, but it does imply that there is nothing intrinsic to the perceptual situation which allows you to tell when they are mistaken and when they are not -- so any truth of sense is not going to be certain. Similarly authorities, books, tradition, and intellectual intuitions can be in error; so there is no certainty. Usually the dogmatist takes a wrong step as he is chased up the ladder of proofs -- he decreases content in the hope of increasing certainty. Thus, if he justifies P by Q, he aims to give Q less content and more certainty than P -- for instance, perceptual statements about physical objects which are generally taken to be pretty uncertain are often justified by statements about sense-data which say less and are presumably more certain. The trouble here is that the steps have to be reversed for Q to justify P and so a content-increasing logic is needed. But induction and all such content-increasers are invalid; we have only to recall that Descartes required both God and Induction to reverse the steps from Physics to the Cogito. In short, under dogmatist standards there is no certain knowledge.

All views are thus on a par in so far as they are all not proven certain truths. Does this mean that all views are on a par full-stop? Does this mean that all views are equally good? Some have thought so. Russell tells the tale of Pyrrho, the founder of scepticism:

> He maintained that we never know enough to be sure that one
> course of action is wiser than another. In his youth, when
> he was taking his constitutional one afternoon, he saw his
> teacher in philosophy (from whom he had imbibed his principles)

*with his head stuck in a ditch, unable to get out. After
contemplating him for some time, he walked on, maintaining that
there was no sufficient ground for thinking that he would do any
good by pulling the old man out. Others, less sceptical,
effected a rescue, and blamed Pyrrho for his heartlessness.
But his teacher, true to his principles, praised him for his
consistency.*[1]

But Pyrrho and his teacher were mistaken.

The question 'Are all views equally good?' is wrongly put
for it is ambiguous; it covers 'Are all views equally good as des-
criptions of the world?', 'Are views equally good as guides for
action?', 'Are all views equally good for making the one who holds
them happy?', 'Are all views equally good as bases for explanation?',
and so on. The ambiguity is removed only if we reformulate the
question as: 'Are all views equally good _for purpose_ Z?' and fill
in the end 'Z'. It is a general tenet of mine that any system for
appraising theories or views should grade relative to an end. I
see no point in merely _labelling_ theories 'good' or 'bad'; instead
I feel that philosophers should argue that theories are better or
worse for achieving a specified end, then others seeking that end

---

1.   *B. Russell, (1941), 'On the Value of Scepticism', page 1.*

receive helpful advice.[1]

What are the goals or purposes that I intend some views to achieve? Well, they are similar to those ends that the dogmatist was trying to attain with his notion of certain knowledge: to tell us what the world is like, to be a basis for explanation in science, to be a guide for action in daily life, and suchlike. Unfortunately these ends are incompatible. A theory stands a better chance of describing the world if it is timid and commits itself to little -- for instance, 'Some animals are coloured' is more likely to be true than 'All swans observed until now are white' which is in turn more likely to be true than 'All swans are white'; whereas for a

---

1. In normative ethics it is common to distinguish intrinsic value and instrumental value: one is good in itself and the other good-in-so-far-as-it-fulfils-its-intended-purpose. Items which have a definite and manifest function naturally lend themselves to the second sense; for instance, a screwdriver just would not be a good screwdriver if it could not be used for driving in screws. Beliefs, though, do not have a single definite and manifest function -- they can be used for all sorts of purposes -- and consequently there is no one obvious meaning to statements like 'some beliefs are better than others'. However, the force of these assertions can be recognized by choosing a goal or goals and relativizing the claims to these goals. This is what I do. One result is that whenever I argue that one view is better than another my conclusion has minimal commendatory content -- it is just a roundabout way of saying that one belief fulfils the function. The situation is similar in logic; the word 'valid' has primarily the descriptive meaning that an argument transfers truth; the recommendatory meaning of 'valid' is minimal -- logicians wish neither to praise nor to exhort; however if anyone shares the goal of using an argument to transfer truth then he ought to use valid arguments since they are best for that purpose; so we can say that the valid arguments are the good ones so long as we remember that 'good' here has a hidden purpose-operator. There is the further point that theories do have at least one function, for theories are always theories as to what something is like; this means that it is easier to suggest an appropriate end for theories than it is for beliefs, namely to be like what they intend to be like.

theory to serve as an explanation it has to maintain that one

property must be connected with another property, that is -- it has

to be bold and commit itself to much by attempting to describe the

structure of the world [1]*; again, to serve as a guide to action a*

theory must make assertions about the future -- it must be bold.

to reconcile these differences, I propose that the primary purpose

of scientific views is to describe the structural properties of the

world.   Some views are better than others for fulfilling this role.

For a start, some views are true and some are false.   Of course,

the dogmatist and the sceptic would retort that we are not helped

by this as we do not know for certain which is which.   But not

knowing for certain need not prevent us from being inspired by the

existence of the ideal to argue about the merits of rival views.

If this can be done satisfactorily -- and I will argue in the next

section that it can -- then what will emerge is the idea of a

critically preferred view or a rational view.   And we can label any

---

*1. By this I mean only that the theories should be true unrestricted
universal statements.   There is fierce debate at this point con-
cerning accidental and nomic universality.   It would take me too
far afield to enter it -- however, I will state my views.   I place
myself firmly in the Hume, Frege, Wittgenstein, Popper tradition of
claiming that the only necessity is logical necessity (See K.R.
Popper (1934 -- English edition 1959), Logic of Scientific Discovery,
page 438).   If 'A is B' is explained by 'All A's and B', then we can
say that A must be B (in virtue of the universal truth 'All A's are
B') -- the statement 'A is B is, if you like, physically necessary.
However, the statement 'All A's are B' is not itself physically (or
nomically) necessary.   If 'All A's are B' is in turn explained by
a deeper generalization, say 'All A's or C's and B and D' then we
can say, if you like, it must be the case that 'All A's are B.'.
But then the deeper generalization itself is not in any sense nec-
essary.   For an example -- imagine ourselves back in Newtonian days.
Bodies must fall as they do, and planets must orbit as they do (in
virtue of Newton's Law of Gravity).   But Newton's law of Gravity,
which is at the top of the explanatory tree, is just true; it is not,
in any sense, necessarily true.   (It is definitely not logically
true, then all it appears to be is 'true in the actual world and true
in all possible worlds in which it is true' which gives it the same
status as any other true statement.)*

theory so favoured at time t the <u>scientific background at time t</u> and <u>scientific knowledge</u> is composed of the modern background.[1]

Finally, to return to an earlier point, each epistemology tells the historian what to do: he must find the theories and track the critical discussion. These are the primary aims, but there are also secondary problems.[2] Say the scientist's behaviour is at odds with the epistemologist-historians account -- maybe the scientists <u>said</u> that A was better than B, acted as if B was better than A, and the historian assesses B as better than A; then the scientist's utterances pose a problem: why didn't the scientists admit explicitly what actually was the case and what they acted as if were the case?, were they subject to external pressures?, did the state or the church intimidate them?, and so on. To conclude, the historian should tackle the problem of knowledge and the problems generated thereby.

---

1. I was tempted to call the theories so favoured at t the <u>scientific knowledge at time t</u>. This would mean that, for example, the Ptolemaists knew that the earth was stationary and the Copernicans knew that the earth moved, and it might even mean that certain modern primitives know that the earth is flat. Nothing turns on words, but this terminology gives credence to a relativism to which I am opposed -- therefore it was not adopted.

2. These considerations lay behind Lakatos's re-defining 'internal' and 'external' history. See T.S. Kuhn, 'Science: History of Science', article in the <u>International Encyclopaedia of Social Science</u>, Lakatos (1971), and the criticism and Lakatos's 'Replies to Critics' in the last mentioned volume.

4. _Methodologies of Science: The Problem of Appraisal:-_

Thus far the sceptic has taught us that any attempt to prove the certainty of a theory will be in vain.

Where does that leave the status of theories ? There are two views on this: Pyrrho's -- that, no matter what the end, all theories have the same status, and the optimist's -- that some theories are better than others for achieving some ends.

The second view encompasses the rational tradition of optimistic epistemologies. These assert that under explicit standards some theories are better than others. I will mention four such methodologies to appraise theories: probabilism, conventionalism, falsificationism, and research programmism.[1] I advocate the last two to solve our problem of finding a rational view as to the structure of the world. The first pair enter only to illustrate two points: that often the goals of different systems of appraisal are different -- this means that were you to attempt the ardous task of evaluating systems of appraisal you must first argue as to whether the ends are appropriate and then consider if the methodology achieves those ends, and that the key terms like 'science' and 'evidence' have a methodological content which varies with system. I need both results later -- the first to criticize Lakatos's suggestion on evaluating systems of appraisal, and the second to argue that a methodology fundamentally colours the history

---

1. These are names for objective philosophical positions which are third world objects. The Popper-Musgrave theory of objective knowledge is presupposed in this thesis. (See K.R. Popper, 'Epistemology Without a Knowing Subject' and 'On the Theory of the Objective Mind' reprinted in K.R. Popper (1972) and A.E. Musgrave (1968), _Impersonal knowledge_
The named objects are characterized sufficiently for my purposes in the text, for a fuller treatment see Lakatos (1970), (1971), and I. Lakatos (1968), 'Changes in the Problem of Inductive Logic'

for which it is used.

Probabilism replaces the dogmatist's aim of certainty by the weaker requirements of probability. Usually this probability is understood as being in the sense of the mathematical calculus of probabilities, although probabilism can be set up with other confirmation functions. In effect, then, the probabilist takes the primary end of science to be that of being right -- explanation, and action become subsidiary. Some theories are better than others in so far as they have a higher probability. Presumably 'science' is composed of all statements with probability over one half, and the 'evidence' for a statement are all those things which raise its probability.

Conventionalism is not really an epistemology in the same manner as the other three -- generally its aim is not to say which theories tell us what the world is like -- but nonetheless it is a grading system for theories. It arose not to solve the problems of epistemology, action, and explanation, but instead to answer a sub-problem of these -- the invalidity of induction. Experimental reports cannot prove theories because induction is invalid; the conventionalist sidesteps this by arguing that theories are not intended to be proven anyway. Their purpose, it is said, is solely to order, summarize, classify, or act as inference rules between experimental reports. Observations are taken for granted and the goal of science is that of producing theoretical frameworks to systematize these. The grading arises in that simple theories order well and that means that the best theories are the simple ones. Conventionalism has no notion of experiments being evidence for theories -- the sole item favourable to a theory is its elegance. Also there is no proper definition of science -- any

attempt to link up the data would be 'science'.

Falsificationism, and its sophisticated variant the theory of research programmes, result from consideration of one important feature of the sceptic-dogmatist debate. The sceptic does not criticize a statement, he criticizes only attempted proofs of a statement. Together with the simple fact of logic that the conclusion of a valid or invalid argument can be true even if the premises are false, this means that even if the dogmatist's premises are false his conclusion may be true. Throwing doubt on proofs of T, say, therefore does not throw doubt on T. The falsificationist recommends that instead of trying to prove T we should criticize T. Take T for granted, in other words, unless we can prove it false.

There seems to be a difficulty here, for proving T false is exactly the same as proving not-T true so we are apparently back to square one.

It is at this point that the decisive break with dogmatic justificationism is made. The dogmatist distorts the problem and as a result cannot answer it -- he thinks that, for instance, action is possible only if what the agent intends to do is certainly justifiable. But this warps the set up -- the agent is actually aware of only a small number of courses of action, he can act only in the light of the alternatives before him, and so action is possible if he can make a rational choice among the alternatives; in other words, he does not have to justify the view that he chooses, he has to justify only his choice. Where scientific background and explanation are concerned, there are never more than a few rival theories as to the structure of the world -- our problem is to say which is the best, not to attempt the impossible by trying to show that one theory truly and with certainty represents the state of

affairs. The problem of action can then be solved if the agent uses the scientific background to decide which is the best choice among the alternatives.[1] *What Pyrrho should have argued was:*

> *Either I will walk off down the road, or I will stand here mesmerized by my philosophical predicament, or I will pull the old man out. My guesses as to the world's structure provide sufficient ground for thinking that the last course of action is best so I will pull the old man out. It is true that there is no guarantee that good will result, but that does not worry me for I know that equally there is no guarantee that good will result from my walking off or that good will result from my twiddling my thumbs. The onus on me is merely to establish a preference -- there is no requirement that I should be awed because no guarantees are given.*

---

1. *The problem of action is formidable and here is not the place to go into it in depth. But I feel that science is the answer, for if the problem is formulated in terms of instances then it is insoluble. My approach will be to use past experience to weed out unsound theories -- this is logically impeccable in that if a theory has a false past consequence then it is false and that means false for the past, present, and future. But for action an agent can maintain that he is not interested in whether or not a theory is false but rather that his concern is whether or not the very next intended exemplification of the theory will occur. Given that theories can have instrumental value -- that is, false theories can have true consequences -- there seems to be no reason why the agent should opt for the conjectural theory over a theory which has been falsified. For example, the theory 'The sun always rises' is conjectural whereas the theory 'The sun never rises' is false , but the second may be right in predicting that the sun will not rise tomorrow and the first may be wrong in predicting that it will - so why shy away from the second theory for the next instance prediction unless for the inductive reason that false in the past means false in the future? I think that there is no answer to this if one sticks to instances, for all experience is past experience and thus is consistent with any future instances. But one should not stick to instances, for rational action is possible only if the world is law-governed -- random and chaotic action would be the only policy for a random and chaotic universe. Why not use our best guesses as to what the laws are as guides for action? If we demand that each of the two statements be deduced from purported scientific laws, then past experience may be able to settle the matter. This will also mean that, for example, that the sun has always risen is not the only evidence for its rising tomorrow, for we know why the sun appears to rise and there is much past evidence for our explanation. I regard the fact that past pendulums have swung slower at the equator than at the pole (due to the oblateness of the spinning earth) as evidence for the view that the sun will rise tomorrow.*

In short, a theory T cannot be absolutely justified, but there is no need for it to be; T has only to be preferable to its rivals.

This answer is a variant of the Popper-Musgrave approach, Musgrave explains it thus:

> Popper sums up all this in the following formula: 'We cannot justify our theories but we may, by considering the present state of the critical debate about them, be able to justify our preference for one theory over some others.'
> One can say this only with the full realization that (a) to have made a justified choice of a theory does nothing to justify the theory itself, so that (b) we can justifiably choose a theory which is false, and which we may have good reason to think is false, and finally that (c) these choices are not so important because the state of the critical discussion may change tomorrow and an opposite choice become reasonable.[1]

What should not be accepted for espistemology is clause (b). In the case where we have good reason to think that the critically preferred view is false, we must withhold judgement and modestly state that we do not know. There cannot be good grounds for supposing that the critically preferred view describes the world if there are good grounds for supposing that it does not. For example, around the turn of the century the Rayleigh-Jeans account was the best theory of radiation but in view of the behaviour of black bodies no knowledge claims could be made.

The ideal method for establishing a preference is that of choosing the best corroborated hypothesis.[2] In the clearest case this will involve logically-crucial experiments in which two theories make contradictory empirical predictions one of which experiment will show to be mistaken. There is nothing certain about this procedure because the empirical test is not certain, but the method

---

1. A.E. Musgrave (1968, page 302. Popper argues this line in 'Conjectural Knowledge: My Solution to the Problem of Induction' in Popper (1972) -- see especially page 21.

2. The basic Popperian theory of corroboration -- which is the one adopted here -- is explained in K.R. Popper (1963): Conjectures and Refutations.

is sufficient to give the agent reason for preferring one theory
over the other.   If a theory fails a test then, as far as the
agent knows, it cannot be a law; whereas if another passes the same
test it might well be a law -- so, from the agents point of view the
second theory is preferable to the first.   In a case of moderate
clarity we have to look at *potential* logically-crucial experiments.
It may be that the theories concerned make their predictions only
when conjoined with auxiliary theories, and it may be that one
theory makes a successful prediction whereas the other lacks suit-
able auxiliary theories to link it with the phenomena -- so that the
second theory says nothing about the phenomena rather than is mis-
taken about it.   Here, the first theory should be preferred;   it
explains the phenomena whereas were the second to attempt an ex-
planation it would fail.   Finally, there is a bizarre case which
apparently can bring the whole programme to a halt.   It arises
from the curve fitting problem: no matter how well any particular
hypothesis is corroborated in the above senses it is always possible
to devise an infinite number of hypotheses which are equally well
corroborated and thus to render empty the instruction to choose the
best corroborated one.   This arises because any theory can be
represented as a (generally continuous) curve in an infinite dimen-
sional space, and the finite known data points in that space can be
used to choose between curves which forbid or fail to predict
particular points, but what the data points cannot do is to distin-
guish between curves which account for them all and -- as every
mathematician knows -- an infinite number of curves can be drawn
through any finite number of points.[1]   The Popperian theory of

---

1. The much vaunted 'New Riddle of Induction' is actually only this
old as the hills curve-fitting problem.

corroboration provides a perfect answer here: only <u>genuine</u> tests should count. If a curve <u>predicts</u> a point, then that point corroborates the curve; but if a curve is drawn merely <u>post hoc</u> through a point then -- since that point cannot potentially falsify and thus genuinely test the curve -- that point does not corroborate the curve. This is an excellent theory of evidence in that it solves the key problems in confirmation theory -- namely, the paradoxes of confirmation, the curve-fitting problem, and the problem of action [1] -- and it accords with our intuitions on evidence. But it does not satisfy our epistemological qualms. On the face of it, the extent to which a theory <u>describes</u> the world will be merely a timeless relation between the theory and the world; whereas this account of corroboration embodies a time variable.[2] This, then, is a problem to be solved.[3] I can say in mitigation only that all the 'curves' in science that I will consider will be non-bizarre: there will be actual or potential crucial experiments between them.

In this way, experience -- that is, past experience -- can be used to make a rational choice between competing views on the world's structure.

How might the debates about merit develop?

---

1. *See J.W.N. Watkins (1964), 'Confirmation, the Paradoxes, and Positivism'*
and A.E. Musgrave (1974), 'Logical versus Historical Theories of Confirmation'.

2. *This time variable may be a pseudo variable -- the prediction testing the theory may be known before the theory is proposed. For the intricacies of this, see A.E. Musgrave (1974).*

3. *In my opinion the problem here cannot be overstated. I feel that 'prediction-orientated' corroboration theory is required to solve confirmation problems, yet my intuitions on truth and verisimilitude have it that they are not 'prediction-orientated' -- so how can we link corroboration and verisimilitude in these difficult cases?*

I will discuss two cases -- where T has rival theories and where T does not -- and will argue that the dialogue proceeds in much the same way.   All that has to be invoked is the suggestion that we use corroboration to establish a ranking among the views between us. But before that I will explain the relation between fallibilism and the logical models that I adopt.   This is an important issue to settle because Lakatos uses an argument here as the main foundation for the M.S.R.P. and if the argument is sound all of Popperian falsificationism (and much of what I intend to do) would be incorrect. Popper, for instance, writes of it:

> *if [it] were true, then my [Popper's] philosophy of science would not only be completely mistaken, but would turn out to be completely uninteresting.[1]*

My view is that the argument is faulty and thus falsificationism survives.

Ohm's Law, to start with an example, forbids certain combinations of voltage current and resistance.   Scientists can determine, fallibly, these types of combination in the laboratory, so the logic of a test can be a monotheoretical one with one fallible theory and one fallible experimental report about voltage, current, and resistance.   Equally well, one could be more explicit about experimental technique by saying that when a scientist measures voltage, current, and resistance, all he does is to determine, fallibly, pointer readings and these require observation theories for their interpretation.   Then the logic of the test becomes a multitheoretical one with many fallible theories including Ohm's law, and theories on ammeters, voltmeters, & c., and one fallible

---

1.   K.R. Popper (1974), The Philosophy of Karl Popper, page 1005.

experimental report about *pointer readings*. Then the question
arises: should the logical model for the testing of, say, Ohm's law,
be a monotheoretic one or a multitheoretic one?

There have been two firm champions of multitheoretic
models: Duhem and Lakatos. Duhem's case arises because he was not
a fallibilist -- his argument was that in a refuting situation you
have to rely on theories other than the one under test, namely those
governing voltmeters and the like, and so there are these extra
possibilities of error which should be listed in the test model.[1]
This in itself, though, does no**t** force the adoption of a multi-
theoretic model. If you are a fallibilist -- as I am -- many, and
perhaps all, of Duhem's possibilities of error can be swallowed up.
Intuitively speaking, scientists can determine voltages at least as
well as, to take a philosophical chestnut, human beings can deter-
mine that the tables in their rooms are rectangular and brown. So,
we can forget about pointers and hold that scientists measure,
fallibly, voltage. Lakatos, though, goes one step further than
Duhem. He takes the view that even with fallibilism a multi-
theoretic model is mandatory. Then falsificationism fails because
nothing can be learned, even tentatively and conjecturally, from
tests. With multiple premises, the failure of the test itself
cannot isolate a guilty premise and the success of a test may be
fortuitous. Lakatos's position is expressed in the assertion:

> *exactly the most admired scientific theories simply fail to
> forbid any observable state of affairs.*[2]

---

1. See P. Duhem (1905), *The Aim and Structure of Physical Theory*
(P.P. Weiner translation 1954) especially pages 180 ff.

2. Page 100, Lakatos (1970), italics throughout in the original.

and the reasoning behind it is that admired scientific theories have
to be conjoined with _other_ theories to entail predictions.   Lakatos
gives no argument for his claim.[1]   However, I will give a counter-
example to it.   The first point to be cleared up is a terminolog-
ical one.   Lakatos writes of 'observable states of affairs' and
takes fallibilism to be the view that 'observations' do not with
certainty represent the 'observable states of affairs'.

_____

1. The argument seems to be that all theories must be conjoined with
ceteris paribus clauses -- see page 101, Lakatos (1970).   But then
there is a footnote which reads:
> [Added in press]: This '_ceteris paribus_' must not normally be
> interpreted as a separate premise.   For a discussion, cf.
> _below_, page. 186.

And on page 186 there is a muddle and the claim is made, in footnote
2, that the defect in the argument is 'easily repairable'.   The
'easy repair' shows a subtle change of emphasis.   In his (1971) he
states on pages 111 and 112:
> 'What kind of observation would refute to the satisfaction of
> the Newtonian not merely a particular Newtonian explanation but
> Newtonian dynamics and gravitational theory itself?   And have
> such criteria ever been discussed or agreed upon by Newtonians?'
> The Newtonian will, alas, scarcely be able to give a positive
> answer.

(And this same passage appears verbatim in pretty well all of
Lakatos's later papers; for example, in his paper in the Popper
(1974) Schilpp volume).   The reformulation makes an entirely
different point to the archetype.   First let us distinguish between
observations and observable states of affairs.   Lakatos's original
states categorically that Newtonian theory fails to forbid any
observable states of affairs.   The 'improvement' states that
Newtonian theory fails to forbid observations specifiable in
advance.   The original is false, as I will show; the improvement is
possibly false.   It is a statement about the minds of Newtonians:
it says that they are so dogmatic about their theory that they are
willing to take advantage of the fallibility of any experimental
report.   What arguments are there that Newtonians are as dogmatic
as this?   Lakatos does not resort to psycho-sociological evidence,
he simply refers us back to the original (now refuted) argument!!!
(See footnote 83 of Lakatos (1971).   What arguments are there that
Newtonians are not as dogmatic as this.   One can name a large
number of people who held Newton's theory and gave it up.   Or one
can perform a thought experiment: wake Newton from the dead, in-
struct him in relativity theory, Eclipse experiments and the like,
and ask him 'What observation would refute to your satisfaction
your dynamics and gravitational theory?', would he not say 'The
precession of Mercury's perihelion, when I see it, will satisfy me'?

But I have not dwelled on the word 'observation'; to me, it is too anthropocentric and I feel that for science 'measurable' or 'determinable' are better -- in other words, I put the limits of observation with the limits of measurement. Then Lakatos is wrong in the following way.[1] Most of the most admired scientific theories involve fundamental constants; one aspect that fundamental constants have is that they are measurable (how else do scientists determine them to so many significant figures?); then the theories _alone_ will forbid measurable states of affairs involving these constants. For example, Newton's theory rules out the measurable state of affairs that there exist two one kilogram masses one metre apart which attract each other with a force of 0.5 G Newtons. Then, to turn this around. Newton's theory on its own predicts that all pairs of kilogram masses a metre apart attract each other with a force of G Newtons. So, Lakatos's arguments do not force the adoption of a multitheoretic model: we still have the choice. Which choice should we make?

There is one advantage in multitheoretic models -- they make more of the fallibility explicit and thus identify targets for criticism. But there are limits. As the model expands the additions to it have less content. Many 'observation theories' have never been articulated and do not amount to much more than the assumptions 'The instrument works', or 'The observations mean what we think they do', or 'Our eyes are not deceiving us'. These 'theories' are not specific enough to help criticism.

To sum up. Generally either monotheoretical or multitheoretical models can be used: one talks of more 'theoretical'

---

1. Popper rebuts Lakatos in an alternative way in the Popper (1974) volume -- I feel that my argument is stronger than Popper's.

notions like voltage and force, the other of more 'observational'
notions like pointer readings.   There is an advantage in expanding
a model, but not without limit.   A recipe for producing a model is
as follows.   Choose the type of statement that, for these purposes,
is regarded as being determinable by experimental technique.   See
if the theory under discussion yields that type of statement.   If
not, add in all the observation theories, initial conditions,
ceteris paribus clauses, and the like, which are necessary for the
derivation to go through.   The result is one of the many suitable
logical models -- usually we will be able to expand or contract the
model, if we wish to.   I tend to use minimal models to discuss
logic and expanded models to discuss the dynamics of criticism.

       To return to the main theme.   Say T has no rival theories.
Even in this null case, T has so to speak one rival: not-T, although
not-T will not be a universal theory.   Should we try to prove T true
or should we try to prove not-T true? My view is that this depends
on the form of T; in science, where T is universal, we should try to
prove not-T true -- that is, we should criticize T by trying to show
that it is false.   I argue this for two reasons.   First from a
desire to make observation an arbiter -- I feel that if we want to
find out what the world is like we ought to have a look at it to see.
And secondly from considering the form of theories and observations;
the scientific theories that we are trying to assess are universal
in form whereas any observations we make are of particular places
and times, therefore we can never prove a theory but we might refute

it.[1]

To take the argument further let us assume that a theory *T* has been put forward and criticized successfully in that an experimental consequence of it, say *P*, has apparently failed.   There is now a refuting situation and some decision will have to be made between two logically incompatible conjectures: one the guess that the theory is true and the other the guess that the experimental consequence is false.   No simple directive can be given; the rule always trust the experimental test contravenes fallibilism; on the other hand, always overruling the test contravenes empiricism.

Let us look at the possibilities of relinquishing the observation.

What arguments can be used here?   Of the many, two types are prominent: from initial conditions, and from instruments.   The first arises as follows.   Most scientific theories are expressed as differential equations; a differential equation connects the value of a function at one point in space or time with the value at the next point in space or time: it connects initial conditions with predictions or causes with effects.   The experimental consequence *P* that has been referred to therefore actually consists of two components which are connected by a conditional: If the initial

---

1. Much of what has been argued about dogmatism and fallibilism could have been expressed as theses about language.   Language is a system of conventional signs and it is a fact that the community of speakers do use the same of similar linguistic units to apply to (presumably similar) aspects of different situations.   Thus, in a way, an aspect of the situation itself justifies or motivates, in the light of the community's conventions, the use of the linguistic unit to describe it.   Then, all fallibilism amounts to is the acknowledgement that the labels are not sacrosanct and that they may even, for one reason or another, be retroactively changed.   And the importance of observation as an arbiter is that it is here that the conventions are the most widespread, uniform, and entrenched.
    Mary Hesse in her (1974), *The Structure of Scientific Inference*, makes rapid and deep advances which throw light on this line.

conditions hold, then the prediction must occur.   And the guess
that the consequence fails is actually a double guess: that the
initial conditions hold, and the prediction fails.   The falsifica-
tionist thus has the opportunity to argue against the initial cond-
itions.   He cannot merely say that the initial conditions may be
false; we know that already, for we know that there is no certainty.
What he must do is to devise and present a rival view on the initial
conditions which, when we come to judge between the alternatives,
turns out to be better corroborated than the original view.   Take
the example of Leverrier.   At first it was the orbit of Uranus
that worried him; in 1846 he announced:

> I have demonstrated ... a formal incompatibility between the
> observations of Uranus [the prediction] and the hypothesis
> that this planet is subject only to the actions of the sun
> and of the other planets [the initial conditions] acting in
> accordance with the principle of universal gravitation.
> [the theory under test.]

He offered a rival view on the initial conditions by postulating
the existence and position of a new planet the actions of which
affected Uranus.   It took the observers just one hour to find
Neptune, and thus the argument against the initial conditions was
successful.   Leverrier then moved on to the next great astronomical
problem: the observed perihelion of Mercury was incompatible with
the supposed initial conditions and Newton's theory of gravity.
Again Leverrier offered an argument against the initial conditions
by postulating a new planet 'Vulcan' which affected Mercury.   But
there is no 'Vulcan' and consequently the original view remained the
best corroborated one.   The second type of argument concerns

---

1. See   N.R. Hanson (1962), 'Leverrier: The Zenith and
Nadir of Newtonian Mechanics', page 361.
This article provides the historical background for the example.

instruments. Few predictions of scientific theories can be tested without the use of measuring devices or experimental apparatus, but such instruments tacitly presuppose theories in addition to the one under test, and so there is opportunity to take issue with these observation theories. For instance, Galileo claimed to have observed the phases of Venus by means of a telescope.[1] If Venus shines solely by the light of the sun then, under the Ptolemaic system, its face should never by fully lit up; to the naked eye, though, Venus appears to be a shapeless point; however, Galileo asserted that he has seen with his telescope the completely illuminated disc of Venus and that consequently the Ptolemaic system was refuted. As you would expect, the Ptolemaists tried to direct the arrow of modus tollens into the observation theories. Galileo said that his telescope was a 'superior and better sense' than the eye, but this seems to have been a bluff for the weight of the arguments were against Galileo. To start with, he had no idea how his telescope worked: there was no observation theory available to him under which it could have been subsumed. The instrument itself did not perform very well on the earth -- producing chromatic and other aberrations; and it seemed not to work at all when used on the heavens -- it magnified the planets, but diminished the size of the stars; the image produced by the telescope appeared to be within the telescope and so, in the absence of a satisfactory optical theory, it was reasonable to assume that some, if not all, of the observed images, double-images, and triple-images, were produced by the telescope itself; finally, through Galileo's telescope the

---

1. *The historical basis for this example is provided in P.K. Feyerabend (1970), 'Problems of Empiricism, Part II'.*

planets appeared to be coloured squares. Clearly the Ptolemaists had some grounds for questioning whether a coloured square really was the fully illuminated disc of a spherical Venus. But again, this debate can be conducted satisfactorily within the framework that we have adopted. Let us look anew at this test. What the Ptolemaic theory actually forbad was Venus being further away from the Earth than the Sun was, and at that time measuring the Earth-Sun and Earth-Venus distances was a taxing problem at the frontiers of science. Galileo claimed to have done it qualitatively with his 'coloured squares' observation, clearly the onus is on Galileo to produce the back up arguments as to why the observation meant what it did. And it is at this point that the Ptolemaists can offer a rival, and presumably better corroborated, interpretation of the 'coloured squares'. As time goes by the task of overthrowing the observation theory becomes more arduous. Nowadays we can measure these distances it a thousand-and-one ways and so faulting a particular observation theory achieves nothing; instead a reinterpretation of a factor common to all these theories is required; this is not impossible (look at relativity, for example) but it is difficult. To sum up, then, the observation can be overthrown -- all that is needed is an explanation of what is wrong with it.

There is nothing final about the overthrow of the observation -- overthrows can be overthrown, and so on. But what is final and generally unambiguous is the state of the critical discussion at a particular time: usually there is no doubt at all as to which is the best guess about the observation at a particular time. As an illustration, there is an embellishment to the Leverrier story. Several people actually did 'find' the predicted 'Vulcan'; in fact, one Dr. Lescarbault was awarded the Legion of Honour by the French

Academy for discovering it.   At this point, then, it may have been

that the best view was that the initial conditions of the Mercury

prediction were at fault; but eventually good arguments arose that

Lescarbault was mistaken and so the debate swung the other way.   So

much for the case where T has no rivals.

The same approach can be adopted when T does have rivals.
All we have to do is to look at how well the various views are

corroborated.   If necessary we can carry out a series of crucial

experiments, this will establish a ranking and the grading obtained

will be absolute for a particular time, although it will usually

vary through time.[1]

To find out what advice follows from this appraisal, we

have only to recall the end for which the appraisal was made.   With

these in mind, my thesis is that a scientist should maintain of the

best theory, 'This is what the world is like.   This is the explan-

ation of such-and-such a phenomena.   And this is what to use as a

---

1. I have devised an objection to my account.   If we assume that
all scientific theories are false -- an assumption that I for one
am happy to make -- then if a logically crucial experiment favours
T over T' there will be other crucial experiments that favour T'
over T.   So what advantage is there in being victorious in com-
petitive tests? The proof goes through as follows: if $T \not\vdash p$ and $T' \vdash$
$-p$ and p is true, then $T \not\vdash (p \& f)$ and $T' \vdash -(p \& f)$ where f is any
false consequence of T and, of course, (p & f) is false and -(p & f)
is true.
    I do not know the answer to this and consequently regard the
objection as a problem to be solved.   But it does not look too
serious.   There is something a little strange about regarding
(-p v -f) for all f as genuine predictions of a theory which pre-
dicts -p.   Consider relativity and Newtonian science and say
gravitational red shift did not occur -- then relativity predicts
Mercury's perihelion correctly and the conjunction Mercury's peri-
helion and red shift incorrectly, whereas Newtonian science is wrong
about Mercury but right about the disjunction Mercury does not pre-
cess or gravitational red-shift does not occur.   But does Newtonian
science really have anything to say about gravitational red shift?

guide to action'. I must stress that on this account no advice

follows on other matters such as which theory or group of theories

a scientist or community of scientists should work on.[1] If I wish-

ed to offer advice on, say, this I would consider first what ends

---

1. *It was at this point that another wrong turn was made in the development of the M.S.R.P. Lakatos was challenged to say what the consequences of his appraisals were -- what were the repercussions for scientists of their hearing that a theory or research programme was 'good'? He, following a suggestion by John Worrall, then made a distinction between appraisal and advice and claimed that his evaluations were appraisal only and that, more or less, no advice followed from them (see page 174 of Lakatos (1971) -- 'Replies to Critics' section). This caused uproar. Among the first to bring the obvious into the open was J.J.C. Smart in his (1972) Review of Lakatos's papers, "Science, History, and Methodology",*
He wrote on p. 269:
What is the point of appraisal as such? Surely appraisal is valuable only if it is a guide to decision. In footnote 18 to Chapter 5 of his Open Society and its Enemies Popper remarks: 'But it is clear that moral judgments are absolutely irrelevant. Only a scandalmonger is interested in judging people on their actions...' Analogously, if Lakatos's methodological principles are not meant as heuristics, what is the point of them? What is the point of saying that a scientific research programme is a good one if this is not meant as advice to follow it or to do likewise?
And thus Lakatosians were faced with putting some bite into the appraisals. One suggestion came from A.E. Musgrave -- that the advice should be not to individual scientists but instead to the community of scientists, that they should work on programmes in accordance with the programme's worth, that a division of labour should be effected guided by the appraisals (see Section 3 of A.E. Musgrave (1976), 'Method or Madness'.
This is completely wrong. If a theory is 'good', the advice that follows is simply this: scientists should advocate the theory as an explanation of the appropriate phenomena (and, in turn, there is much advice which follows from that instruction). To be fair to Musgrave, (a) he does offer arguments for his suggestion -- I have not considered these; (b) he is not the only one to make this sort of mistake, indeed, I myself in my (1976) 'The Rejection of Avogadro's Hypotheses'   tended to use appraisals to explain why scientists ignored (i.e., refused to work on) theories, whereas now I would use appraisals to explain why scientists ignored (i.e., refused to advocate as explanations) theories.
*   John Worrall now advocates explicitly the view that I hold -- see Section 5 of J. Worrall (1976).*

the scientist was trying to achieve by working on a theory -- was he trying to find out what the world was like? was he trying to make a contribution and become famous? was he trying to make money by producing a technological innovation? and so on. I do not do this here because I consider such issues not to be in the province of epistemology.

To return to the key terms 'science' and 'evidence'. For a falsificationist, 'science' consists of those theories which are falsifiable, and the 'evidence' for a theory is simply the set of those items which corroborate it.

The M.S.R.P. -- our final methodology -- is an extension of falsificationism and consequently has a similar view on 'science' and 'evidence'. It is enough for my purposes to say that falsificationism appraises only a theory whereas the M.S.R.P. appraises a theory together with its heuristic. This seemingly small change causes large differences in emphasis. Lakatos suggested that scientific theories should be considered not merely as abstract logical systems, instead they should be looked upon as systems plus associated research policies or 'local logics of discovery', and thus there arose the notion of a research programme which consists of a deep theory together with a heuristic.

Earlier I discussed clashes between theory and observation and the possibility of abandonning an observation in favour of a rival interpretation and the possibility of relinquishing the theory in favour of a better rival theory. But I did not discuss what is to be made of the clash before the rivals have been proposed. It is important to deal with this because of the empirical fact all theories have difficulties in so far as they all have (real or

apparent) exceptions or (real or apparent) inconsistencies.[1] This
means that we are always in the circumstance of having clashes with-
out rivals. If corroboration is to be used, it needs to be
supplemented by some guide as to which evidence is merely apparent.
Which 'exceptions' can be ignored and which not? How are the 'ex-
ceptions' to be weighted? Lakatos has given an answer. He argues
that all major theories have accompanying problem-solving techniques
which consist of mathematical methods, planned simulation by
sequences of models, and overall research policies for exposing
'exceptions' and other matters. These heuristics weight the
'exceptions'. If the heuristic is powerful, it in itself constit-
utes a good argument that an objectively sound case will be made that
'exceptions' of a familiar type and merely apparent for it actually
provides the means for showing them to be so. A good example of a theory
facing up to its anomalies is that of Hertz's mechanics, as described by
Hertz:

> At first it might have appeared that the fundamental law was
> far from sufficient to embrace the whole extent of facts which
> nature offers us and the representation of which is already
> contained in the ordinary system of mechanics. For while the
> fundamental law assumes continuous and normal connections, the
> common applications of mechanics bring us face to face with
> discontinuous and abnormal connections as well. And while the
> fundamental law expressly refers to free systems only, we are
> also compelled to investigate unfree systems. Even all the
> normal continuous, and free systems of nature do not conform
> immediately to the law, but seem to be partly in contradiction
> to it. We saw, however, that we could also investigate
> abnormal and discontinuous systems if we regarded their abnor-
> malities and discontinuities as only apparent; that we could
> also follow the motion of unfree systems if we conceived them
> as portions of free systems; that, finally, even systems
> apparently contradicting the fundamental law could be rendered
> conformable to it by admitting the possibility of concealed
> masses in them. Although we have associated with the

---

1. As Lakatos often wrote 'all theories are born refuted' or 'all
theories are submerged in an ocean of anomalies'. See, for example,
Lakatos (1970) page 133.

> *fundamental law neither additional experiential facts nor*
> *arbitrary assumptions, yet we have been able to range over the*
> *whole domain covered by mechanics in general.[1]*

If we assessed Hertz's mechanics, or any other theory, merely by looking at

its prima facie corroboration, we would evaluate it as being very

poor for it is massively refuted; but once we take its problem-solving

power into account it fares reasonably well for much of its troubles

can be branded 'apparent'.

Heuristics themselves are graded in accordance with how

well they are functioning. A research programme has a plan and is

good at a given point in time in so far as it is solving its problems

according to the plan and bad in so far as it either is not solving

its problems or is solving the problems but not according to the plan,

and heuristics are good in so far as they are associated with good

programmes.[2] Lakatos's appraisals have epistemological import for

isolated programme: a good programme is likely, at that time, to

develop into a defensible view on the world's structure. For rival

programmes, the case is more involved.

The natural way within my approach to argue the epistemo-

logical superiority of one programme over another one is to hope for

a logically crucial experiment between the two. But there are

difficulties. The M.S.R.P. was intended as a theory of super

science: of the deepest and most profound theories only. With

these crucial experiments become ineffective because the theories

under test are each embedded in a plethora of other theories -- mere

---

1. H. Hertz (1899), *Principles of Mechanics*, Book II, page 735.

2. *My view is that there is more to the evaluation of heuristics than this. One tactic in science is to improve heuristics so as to improve a programme. This seems to suggest that the heuristics can be appraised independently of the programme.*

mention of initial conditions or observation theories fails to do

justice, for auxiliary theories and *ceteris paribus* clauses abound

and so multitheoretical logical models are necessary.[1]   The most

that can be counted on is the existence of potential crucial

experiments between programmes: that one predicts novel facts which

the other cannot satisfactorily explain.   Sound judgements on

potential crucial experiments can be made only if the heuristics

are taken into account for what has to be defended is the assertion

that a programme cannot solve a given problem and that requires an

assessment of the programmes's ideas *and* its problem solving tech-

niques.   To sum up. Actual and potential crucial experiments are

still able to establish the epistemological superiority of one pro-

gramme over its rival, and these 'experiments', are an expression of

Lakatos's appraisals (that a programme is good if and only if it pre-

dicts novel facts and these are facts which are unexplained or

forbidden by a rival programme).

---

1. This point comes out in most of the existing case-studies, and in
I.Lakatos (1974), 'The Role of Crucial Experiments in Science',

   Lakatos was always wary of interprogrammatic crit-
icism (see the 'Replies to Critics' in his (1971)), to the extent of
virtually forbidding major crucial experiments between programmes.
It is true that major crucial experiments are not easy to perform for
the people attempting it would have to be masters of the scientific
and mathematical techniques of *both* programmes; and even then the
outcome may be inconclusive.   Most ordinary mortals would be better
to exploit an alternative pattern of growth by rapidly producing
novel facts in one programme which, hopefully, the rival will not be
able to explain.   Even so, I think that the methodologist should
not restrict any form of criticism.   Many scientists make good use
of interprogrammatic debate.   For example, talented scientists often
explain their allegianace to one programme by claiming that the rival
together with its heuristic cannot solve a particular key problem.
This is extremely valuable for, if sound,it tells the workers on the
rival that they must produce a 'creative shift' in heuristic.
(See Lakatos (1971) page 176 and references in footnote 9 for an
account of this technical term.)

5. *Methodological Bias: The Problem of Objectivity:*

History must be tackled theoretically and philosophically. In particular a historian should be looking at the growth of scientific knowledge; and he can hardly do that without some views of what constitutes scientific knowledge and what constitutes evidence for a scientific theory; and finally philosophical theories intrude into those concepts. The historian will use more tainted terms than 'science' and 'evidence', but these key ones are sufficient for my purpose.

This seems to leave us with radical methodological bias and relativism. A sentence like 'Faraday's scientific theory was supported by such-and-such experiments' means different things to different philosophers and some would judge it true where others would claim it to be false.

To avoid this, the historian should (a) declare his interests by being explicit about the philosophical stance he adopts and (b) use an advanced philosophy.

Parenthetically, it may be remarked that these methodological bias considerations indicate that testing historical theses may be extremely difficult. It is only artefacts that chance has permitted to survive that can be used;[1] and documents of these might be ruled out on the grounds of methodologically biased design -- for example, Faraday's own statement 'These experiments support my scientific theory' might be no evidence at all for the historians claim that those same experiments supported Faraday's scientific theory.

---

1 See page 797 of H. Guerlac (1963), 'Some Historical Assumptions of the History of Science'.

We are left then with the problem of determining which philosophies of science should be preferred.   There are logico-epistemological arguments between the various philosophies, but it is not in the province of this thesis to go into these.   I simply assert that there are objective arguments to defend the view that falsificationism and research programmism are the best among the available explicit philosphies.[1]   There is, however, Lakatos's suggestion that there is a completely new style of argument using history that will grade philosophies, and that bears on the possible value of any case study -- I will consider this in the next two sections.

6. Lakatos's suggestion: History of Science as a Test of its Methodology:

I have followed Lakatos in arguing the thesis that methodologies grade scientific theories and steps made in scientific debate.   Further, we all know that scientists themselves grade scientific theories and steps made in scientific debate; they make basic value judgements like 'Newton's theory was good'.

Given this, Lakatos has made a proposal which is really part of a general theory of norms.[2]   He suggests that the grading theory should explain the grading judgements of the experts; here this means that the methodology should explain the basic value judgements of the scientists.   The value judgements which are explained by a methodology count in its favour, and those which it fails to explain are arguments against it.

Three points about explanation should be made.   To explain a value judgement means merely to be able to derive it from the

---

1. *Lakatos and Popper argue to this end.   See, for instance, Lakatos (1970).*

2. *This is the main thesis of Lakatos's (1971).*

methodology and the appropriate facts about the scientific gambit. An explanation is a good one if it is independently testable -- so a good methodology should predict unmade or unexpected judgements. Finally, and this is most important, in an explanation the explicandum is a statement and it is possible that this does not correspond with the world and thus is false; indeed, the explanation itself may highlight the falsehood by correcting the explicandum while explaining it; what this implies is that there is no assumption that the experts have to be right, they are fallible -- they may judge a theory bad, the methodology may constitute an argument that the same theory is good and the argument may win them over, in which case the methodology corrects their judgement while explaining it.

As an example, I use Lakatos's favourite one. According to the scientists, Newton's theory was good; according to falsificationism, any unfalsifiable theory is bad; according to Lakatos, Newton's theory is a matter of fact unfalsifiable[1] -- as a result, Lakatos argues that either the scientists should revise their judgement or falsificationism is inadequate at this point.

History of science is thus used, in conjunction with the scientists' value judgements, to test the methodology used to generate it.

7. Rejection of Lakatos's View:-

My thesis in this section is that Lakatos's new critical weapon has limited strength. All criticism, even weak criticism, is valuable; but it is as well to be aware that not much can be achieved with this new approach. The argument proceeds in two stages:

a) to the conclusion that the introduction of norms and value

---

1. This assertion, as we have seen, is mistaken.

judgements leads to a blind alley, for in this sort of

grading there is a hidden purpose operator and the judge-

ments and grades can and should be translated back into

ordinary descriptive language.

and b)   to the conclusion that there are vicious feedback loops and

that these vitiate the whole enterprise;[1] one might expect

that some circularity would arise in using philosophy to

produce the history which tests that philosophy; however the

loops become manifest only when it is realised that scient-

ist's value judgements are required and that philosophy is

also used to identify who the 'scientists' really are.

Lakatos does have a point.   Taken in the widest possible

sense, scientists know much better than anyone else what the world is

like, but this must be balanced against the possibility that part-

icular scientists, particular scientific groups, or even particular

periods of all science are degenerate.   The philosopher must retain

his role as a critic -- he must be able to argue that some science

just is not knowledge; for example, a philosopher in the middle ages

should have been able to point out that much of what was done in the

name of science was without value.   So, experts know better than

philosophers, but experts are fallible and their judgements should

be open to criticism.   What can be made of this?

Not much, I am afraid.   Lakatos maintains:

What the scientists tell us is 'good' is, fallibly, good.

---

*1. T.S. Kuhn in his (1971) 'Notes on Lakatos' in the Lakatos (1971)
Volume makes this sort of charge, but in a less explicit and exten-
sive fashion.   John Worrall produces a reply to this on page 164
of Worrall (1976), his response ultimately amounts to testing
methodologies against 'general opinion' -- but then do we need history
and all of Lakatos's complex suggestions concerning it?*

*This type of statement has characteristic difficulties -- of ident-
fication, of unanimity, and of correctness -- not all of which
Lakatos's view answers   We have to identify the scientists -- for
the view to be useful one has to know who the scientists are.
Generally this has to be done philosophically -- the scientists are
the people who do 'science' and a philosophical theory lays down what
'science' is.   Again, for the view to be useful, the scientists
must agree, otherwise a statement might end up being both good and
bad.   Finally, even if the scientists do agree, we still must know
why it is that they are right.*

*Taking these difficulties in reverse order, Lakatos's view
starts to founder with the second one.   The problem of correctness
is answered satisfactorily: there is no assumption of correctness,
all there is is the reasonable claim that the experts guess better
than ordinary people.   As to the second difficulty, there certainly
is no unanimity over the basic normative judgements.   This is be-
cause the normative judgements have a hidden purpose operator and
consequently should be unravelled so their meaning is exposed.   A
scientist might tell you in the one sentence that Newtonian mechan-
ics was bad and good and bad, and mean that Newtonian mechanics is a
poor description of the world, is good for calculating how to put a
man on the moon, and is a poor bet for a research student to devote
his life to.   The prospects of real comparison deteriorate even
further when my earlier result that different philosphies grade
relative to varying goals is brought in -- conventionalism and
falsificationism, say, just do not make the same claim by calling
a theory 'good'.   To compare scientists' judgements we have first to
translate back into the descriptive mode.   This means here that
Lakatos's assertion becomes:*

What the scientists tell us is 'scientific knowledge' is,

fallibly, scientific knowledge.

Is there now unanimity or near unanimity? Well, there might be. But there are two feedback loops that cause problems. First, the scientist would apply some prior (and usually unsound and out of date) theory of scientific method in order to make his judgement on 'scientific knowledge'. Secondly, we -- the philosophers -- would use our philosophical theories to identify who the 'scientists' were; namely, those who espoused and practiced our philosophy. And thus there would be two bootstrap lifts. A typical result might be: an inductivist would define a scientist as being a member of the Royal Society (since the rules of that body demand adherence to inductivism), in turn a typical member would judge Ampère's work to be scientific knowledge since Ampère had explicitly used the inductive method; finally inductivism appraises Ampère's work as knowledge -- thus the grading theory fits the judgements of the experts. Clearly this is a cheap victory.

8. Summary

The important consequences of the previous seven sections are:

> (a) *History of science should be concerned primarily with the problem of knowledge and then with any other questions which that problem generates.*

> (b) *An advanced philosophy of science should be made explicit and used -- in this case it will be the M.S.R.P.*

and (c) *A case study will have value as history of science and it may have value for criticizing philosophies of science.*

and the subsidiary achievements of this appendix are:

> (a) *to make a case for methodologies (in particular the M.S.R.P.) being appropriate for grading epistemological ventures and for not being confined to being fancy labelling systems for*

past socio-psychological trends in what is commonly called 'science'.

(b) to stress that grading here is grading relative to an end and thus to make some sort of sense of the appraisal/advice distinction and its associated flocculent literature.

(c) to refute the linch-pin argument for the M.S.R.P.

This is the argument of page 100-101 of Lakatos (1970) which is cited, in every Lakatos methodological paper, as being the basis for the MSRP. (Need I add that refuting the argument for the M.S.R.P. does not refute the M.S.R.P.)

(d) to refute the main tenets of Lakatos (1971) -- for example, that history tests the philosophy which generates it.

APPENDIX 2 : *Fallibilist Realism versus Instrumentalism.*

R.A.R.Tricker, the major secondary source on Ampère and the earlier scientists researching in electrodynamics, urges an instrumentalist interpretation of scientific theories. This was discussed briefly in Chapter 2, here I defend at greater length the view that scientific theories can and should be interpreted realistically.[1]

Instrumentalism has traditionally had as its rival dogmatic realism which is the view that : a) scientific theories should aim to be true, and b) the true ones are known, or can be known, to be so. Then clause (b), and with it dogmatic realism, is defeated by means of the arguments outlined in Appendix 1; and consequently instrumentalism has been dominant in this two-cornered fight.

But Popper introduced a third category -- fallibilist realism -- under which : a) scientific theories should aim to be true, and b) we can never know for certain that a true one is so. It is this view that I contrast with instrumentalism.

The standard arguments in favour of instrumentalism, and my replies may be reviewed as follows. Economy. Putting the argument as a question : if we can never know that a theory is true, then why make the unnecessary and superfluous demand that it should

---

1. This issue was brought into prominence by K.R.Popper in his 1956 paper 'Three Views Concerning Human Knowledge' which is reprinted in his (1963), *Conjectures and Refutations.* The basic arguments are there; but the paper suffers from two defects : that of identifying dogmatic realism with essentialism, and that of using some weak arguments (for instance, the major argument against instrumentalism is that it does not account for the actual scientific practice of testing -- in other words, our philosophy is to be ruled by what scientists do.)

be so ?   The counter is that other benefits outweigh the loss
of economy.   _The Lewis Carroll_ argument.   Lewis Carroll showed
that genuine rules of inference cannot be conjoined as extra
descriptive premises in a deductive argument; hence -- it is said --
scientific theories must not be taken as descriptive major premises
in the hypothetico-deductive explanatory model, they must instead
by understood as rules of inference.   This argument is invalid.
Indeed if scientific theories actually were rules of inference,
they could not be interpreted as descriptive major premises; but
the question is whether they are rules of inference, and this
invalid argument throws no light on that.   _Craig's Theorem and_
Ramsey Sentences.   These technical results in logic show that
the theoretical terms in some artificial idealized theories are
for certain purposes eliminable.   Thus -- it is argued -- theoretical
terms are not necessary, they are merely a convenience and a
fictional convenience at that.   With Craig's theorem, a formal
theory with a recursively enumerable set of theorems and two
recursive categories of predicate or term (theoretical and observable,
say) can be converted into another axiomatized theory which yields
all and only the pure observation terms of the first theory.   Therefore,
as far as the observational consequences are concerned, the theoretical
component is superfluous.   The theorem is not profound.   It takes
an existing result that a recursively enumerable set (of axioms or

theorems) can be recursively axiomatized, and then adds a filter to let through only the observational axioms.[1] Usually the resulting observational theory will have an infinite number of axioms -- one axiom for each observational consequence of the first theory. To sum up, if the interest is solely in observational predictions and certain artificial conditions obtain, the theoretical terms are superfluous. But our interest should not be solely in observational predictions -- our theories should aim to describe the structual properties of the world so that they _explain_ why certain things happen. The Craigized observational theory does not explain why its consequences occur. For instance, Newtonian physics explains why the moon and an apple fall with the same acceleration, whereas the conjunction 'the moon and an apple fall with the same acceleration _and_ the moon and an apple fall with the same acceleration _and_ the moon and an apple fall with the ....' does not explain that observation. Consequently, if explanation be our aim, Craig's theorem does not show that theoretical terms are eliminable. With Ramsey sentences the theoretical properties

---

1. The existing result is proved as follows. (I use here Church's thesis to make the theorem more accessable.) Given an effectively enumerable set of theorems, the axioms are taken to be the formulas which (a) are repeated conjunctions of a given formula, say A, and (b) the number of occurrences of '&' in the conjunct is the code number of the theorem A in the enumeration of theorems. For example, say B is the second enumerated theorem, then B & B & B is an axiom, whereas B & B or B & B & B & B or C & C & C are not. Clearly, i) the axioms are decidable, ii) a formula is one of the effectively enumerable theorems if and only if it is a consequence of the axioms. For Craig's theorem, we add (c) A must contain only observable predicates/terms. Then the second theory will still be decidable and have as its theorems all and only the observational theorems of the first theory.

are existentially quantified over in second order logic and

are thus apparently eliminated. But they disappear only in so

far as they either lose or change their <u>name</u>, and this is not

enough to eliminate theories. In first order logic the inference

from Fa to $\exists x\, F(x)$ is valid, but the inference $\exists x\, F(x)$ to Fa

is not (were it so then $\exists x\, F(x)$ & - Fa would be inconsistent);

but clearly an existential quantifier can be instantiated, all that

is required is the use of a suitable instantiating constant (usually

this demand is made in the form that the constant be <u>new</u>) so $\exists x\, F(x)$

does entail Fb for suitable b; now, say F is the only property we

have to discuss the world, and the world has certain objects in it

which we name by our constants a, b, c and one object has the

property F so we commence with Fa from which we validly infer $\exists x\, F(x)$

from which in turn we validly infer Fb; all that has happened here

is that the object in the world which first had the name 'a' now

has the name 'b' so the real situation would be best expressed by

saying that if we forget about names, $\exists x\, F(x)$ and Fa are logically

equivalent in this case. On now to second order logic and to

Ramsey sentences : at the local level these work as follows. Say

we have a theory that 'a is a red magnet' symbolized as $\exists x\, (\, x = a\, \&$

M(x) & R(x)), and here we take being red as an observational property

and being magnetic as theoretical; this theory has one informative

observational consequence, namely Ra (a is red); the Ramsey trick

is to existentially quantify over the theoretical property,
thus $\exists \phi \exists$ *x(x = a &* ϕ *a & R(x)), so that the transformed*
theory reads 'There is a property which a has and also a is red';
this Ramsey sentence has the same observational consequence as
the original theory; and, by existential instantiation, it also
has an informative non-observational consequence, namely a has a
property, N say, so that Na follows from the Ramsey sentence.
Ramsey sentences do not eliminate theoretical properties while
retaining the observational ones -- they merely refuse to name the
theoretical properties.   There are two cases -- local and global
Ramseyfication.   With local Ramsey sentences a theoretical property
(say magnetism) in one theory (the above one, for example) is
eliminated by quantification.   But that property also appears in
other theories (for instance, 'All magnetic substances align
themselves along a line of force when freely suspended above the
Earth'); in which case the Ramsey sentence loses observational
information over its original (for example, the original with
background knowledge has the consequence 'a points to the North'
whereas the corresponding Ramsey sentence lacks this.)   So local
Ramsey sentences are not strictly observationally equivalent to
their archetypes.   With global Ramsey sentences every single accepted
theory in which the given theoretical property appears are conjoined,
and then the conjunction is Ramseyfied.   In this case the global

theory and the Ramsey sentence have identical observational

consequences.   But -- I maintain -- the relation between them is

stronger : they are now logically equivalent except for the

change of name.   Consequently the theoretical properties have

not disappeared -- we are merely refusing to call them what they

are.   For these purposes let us say that the meaning of a term

is fully known if a procedure exists which yields a 'Yes/No'

answer to those situations in which the term does or does not apply,

and also that the meaning of a term is to some extent known if

a procedure exists to answer 'Yes' to some of those cases in which

the term applies or to answer 'No' to some of those cases in which

the term does not apply.   Then take a global Ramsey sentence for,

say, the property 'magnetic'.   The name 'magnetic' disappears with

the existential quantification, but the quantifier can be instantiated

to give the property instance, say N.   N and 'magnetic' name the

same property, so that Ramseyfication at best changes or hides

names in much the same manner as what happened in the given example

in first-order logic.   Every 'Yes/No' or 'Yes' or 'No' procedure

for 'magnetic' has the identical procedure for N so to at least

some degree their meanings coincide; but the 'magnetic' sentence,

the corresponding Ramsey sentence, and the N sentence are all global,

so there can be no extra meaning left over.   In short, Ramsey

sentences do not both eliminate theoretical terms and fail to lose

observational consequences. Local Ramsey sentences lose observational

consequences, and global Ramsey sentences merely hide the terms

and do not eliminate them. Duhem's fruitfulness. *Duhem argues*

that (dogmatic) realism is not as fruitful as instrumentalism. He

claims that realists have dogmatic (usually metaphysical) views

about the world and are thus prevented from expounding possibly

fruitful scientific theories which conflict with these prejudices.

For example, a realist might hold that 'God does not throw dice'

and thus refuse to entertain fruitful scientific theories which are

probabilistic. Duhem's objection does not hit fallibilist realists.

For these, any view on the world is just a guess and so they have to be

tolerant of rival views. Quantum theory. *Modern science —*

especially quantum theory (Q.T.) — seems to favour instrumentalism.

In Q.T. a complex wave contains all the information which is possibly

knowable about a system or systems. This wave is usually manipulated

in a highly mathematical fashion — it is governed by the

Schrodinger Equation and is generally considered to develop not in

our ordinary physical space but instead in an abstract Hilbert

space. Information is extracted from the wave by subjecting

it to the appropriate mathematical operations. Any other attempts

at extracting knowledge, or asking how the process works, will

almost certainly lead to contradictions. To sum up, the theory is

mathematics only and these yield the experimental or observable

predictions, and the mathematics apparently <u>cannot</u> be further explained or taken realistically.   What is the fallibilist realist's reply ?   I make three points.   The argument, although good, is not a knock-out one in favour of instrumentalism; Q.T. is merely a theory -- as such, it may well be mistaken and be replaced by a theory not inimical to realism.   Point two.   Fallibilist realism seeks ever deeper explanations : it wants explanations, then explanations of those explanations, and so on.   Whereas with instrumentalism the quest for theories is satisfied at the first level.   In other words, realism is question amplifying and instrumentalism is question damping.   Q.T. as presented above seems to halt all further questions and is instrumentalist.   But actually the above presentation is not entirely accurate.   Q.T. does not bar all questions : some are permitted both without and within Q.T.   Certain will lead to contradictions, but these can still be asked and answered consistently if the interrogator is willing to abandon some current <u>interpretations</u> of Q.T.   For example, with some 'hidden variable' theories all of Q.T. is retained within its empirical limits but Heisenberg Uncertainty is held to be false in its extrapolated and untested domains, the result is an attempt at consistent explanation of 'empirical' Q.T. itself.   Other questions can still be asked without contradiction within Q.T. -- the difficulty here is that the sensible physical

questions become obscured by the current mathematics. In short,
Q.T. does not have to be instrumentalist. Point three. No
realist denies that theories are subjected to mathematical
manipulation or that they have purely mathematical portions. However,
he will wish to separate the mathematics from the physical theory,
to make the theory as extensive as possible, to interpret it realistically,
and to claim that there are advantages in doing all this. Look
at the example of the interpretation of Fourier Analysis. Often
a complicated electromagnetic wave can be Fourier analysed into a
sum of a fundamental wave and harmonics. One should ask in each
case : is this merely mathematics or is it indicative of the
physical situation. The answer matters. Electromagnetic waves
have causes, so if the complicated wave really is composed of a
fundamental and harmonics then it may well have originated in a
set of oscillators behaving in a specific fashion. As a piece
of physics, Fourier analysis has further ramifications. With
Q.T., there is an abundance of mathematics, but much of it --
Hilbert spaces, complex waves, and mathematical operators --
is fashionable rather than essential. There might one day be a realist Q.T.

Thus there appears to be no compelling argument to
regard scientific theories instrumentally.

But there are good arguments for interpreting theories

*realistically -- these have been described in Chapter 2 and Appendix 1.
In brief, science should be conceived of as an epistemological
venture aimed at discovering ever deeper explanations, and the
problem agenda of science should not be restricted.*

APPENDIX 3 : Weber's Law and the Conservation of Energy.

For an argument[1] that Weber's law violates the conservation of energy consider the following motion in one dimension.

Take any curve γ (t) such that :

γ (0) = 1

γ (1) = 2

$\dot\gamma$ (0) = 0

$\dot\gamma$ (1) = 0

that is, a particle following the curve would go from 1 to 2 with initial and final velocities zero.

Let the particle follow trajectory r(t) such that :

$$r(t) = \begin{cases} \gamma(t) & 0 \le t \le 1 \\ \gamma(2-t) & 1 \le t \le 2 \end{cases}$$

The particle goes from 1 to 1 with initial and final velocities zero -- it traverses a closed loop in the phase space.

The limits are :

$t = 0 \Rightarrow \gamma = 1 \Rightarrow r = 1$

$t = 1 \Rightarrow \gamma = 2 \Rightarrow r = 2$

$t = 2 \Rightarrow r = 1$

Weber's law is $F = \frac{q_1 q_2}{r^2}\left[1 - \frac{1}{c^2}\left[(\dot r)^2 - 2 r \ddot r \right]\right]$

Work done $= \int F.dr$.

----

1. At the time of submission I have not convinced myself as to the conclusiveness of this argument.

$$= q_1 q_2 \int_{t=0}^{t=2} \left[\int \frac{1}{r^2} dr - \frac{1}{c^2} \int \frac{(\dot{r})^2}{r^2} dr + \frac{2}{c^2} \int \frac{\ddot{r}}{r} dr \right]$$

$$= \frac{q_1 q_2}{c^2} \left[-\int_{t=0}^{t=1} \frac{(\dot{r})^2}{r^2} dr - \int_{t=1}^{t=2} \frac{(\dot{r})^2}{r^2} dr + 2\int_{t=0}^{t=1} \frac{\ddot{r}}{r} dr + 2\int_{t=1}^{t=2} \frac{\ddot{r}}{r} dr \right]$$

$$= \frac{q_1 q_2}{c^2} \left[-\int_{t=0}^{t=1} \frac{(\dot{\gamma})^2}{\gamma^2} d\gamma - \int_{t=0}^{t=1} \frac{(\dot{\gamma})^2}{\gamma^2} d\gamma + 2\int_{t=0}^{t=1} \frac{\ddot{\gamma}}{\gamma} d\gamma - 2\int_{t=0}^{t=1} \frac{\ddot{\gamma}}{\gamma} d\gamma \right]$$

$$= \frac{2 q_1 q_2}{c^2} \left[-\int_{t=0}^{t=1} \frac{(\dot{\gamma})^3}{\gamma^2} dt \right] \neq 0, \text{ if } \dot{\gamma} \geq 0.$$

Bibliography :

ACHINSTEIN P. (1968), Concepts of Science.

AGASSI J. (1963), Towards an Historiography of Science.

AGASSI J. (1964), 'Scientific Problems and Their Roots in Metaphysics',
        in M.Bunge (ed.) (1964), The Critical Approach to Science
        and Philosophy.

AGASSI J. (1968), The Continuing Revolution : A History of Physics
        from the Greeks to Einstein.

AGASSI J. (1971), Faraday as a Natural Philosopher.

AGASSI J. (1975), Science in Flux.

D'AGOSTINO S. (1975), 'Hertz's Researches on Electromagnetic Waves',
        in R.McCormmach (ed.) (1975), Historical Studies in the
        Physical Sciences, Vol. 6.

AMPERE A.M. (1826,7), Théorie Mathématique des Phénomènes Electrodyn-
        amiques Uniquement Deduit de L'Expérience, Paris 1826, Tenth
        Edition Paris 1883, and republished with foreword by E.Bauer
        Paris 1958.

ARAGO D.F.J. (1825), 'L'Action Que Les Corps Aimantés ....', Ann.Chim.
        Phys., 28, (1825), p.325, and in Quart. J. Sci., 19, (1825),
        p.336.

ARAGO D.F.J. (1854), Oeuvres Complètes, Paris 1854.

ARAGO D.F.J. (1872), 'Eulogy on Ampère', Reports of the Smithsonian
        Institute, (1872), p.141.

BABBAGE C. and J.F.W.HERSCHEL (1825), 'Account of the Repetition

    of M.Arago's Experiments .....', Phil.Trans,18, (1825),

    p.484.

BARNETT S.J. (1933), 'Gyromagnetic Effects : History, Theory, and

    Experiments', Physica, 13, (1933), p.241.

BENJAMIN P. (1898), A History of Electricity from Antiquity to the

    Days of Benjamin Franklin.

BERKSON W. (1974), Fields of Force -- The Development of a World View

    from Faraday to Einstein.

BETTI E. (1868), '....', Nuovo Cimento,27. (The paper's reference

    is missing from the Royal Society Catalogue of Scientific

    Papers, and the Nuovo Cimento volume is difficult to obtain --

    the paper is referred to in Clausius (1869) and Maxwell (1873).)

BORK A.M. (1963), 'Maxwell, Displacement Current, and Symmetry',

    Amer.J.Phys., 31, (1963), p.854.

BORK A.M. (1967), 'Maxwell and the Electromagnetic Wave Equation',

    Amer.J.Phys., 35, (1967), p.844.

BORK A.M. (1967b), 'Maxwell and the Vector Potential',Isis, 58, (1967),

    p. 210.

BROMBERG J. (1967), 'Maxwell's Displacement Current and His Theory

    of Light', Archives for the Hist. of Exact Sci., 4, (1967),

    p.218.

BROMBERG J. (1968), 'Maxwell's Electrostatics', Amer.J.Phys., 36, (1968),

    p. 142.

BROMBERG J. (1976), 'Review of W.Berkson's Fields of Force', Isis, 67,

    (1976), p. 132.

BRUSH S.G. and C.W.F.EVERITT (1962), 'Maxwell, Osborne Reynolds, and
the Radiometer', in R.McCormmach (ed.), Hist.Stud.in the Phys.
Sci., Vol.1.

BUCHWALD J.Z. (1977), 'William Thomson and the Mathematization of
Faraday's Electrostatics', in R.McCormmach and L.Pyenson (eds.),
Hist.Stud.in the Phys.Sci., Vol.8.

CAMPBELL L. and W.GARNETT (1884), The Life of James Clerk Maxwell.

CAMPBELL N.R. (1953), What is Science ?

CANBY C. (ed.) (1963), History of Electricity.

CANEVA K.L. (1978), 'From Galvanism to Electrodynamics : The Transformation
of German Physics and Its Social Context', in R.McCormmach
et al.(eds.), Hist.Stud.in the Phys.Sci., Vol. 9.

CARR M.E.J. (1949), The Development of Mathematical Theories of Elect-
ricity Prior to Maxwell with Special Reference to the Concept
Potential., Thesis, M.Sc., London.

CHALMERS A.F. (1971), The Electromagnetic Theory of J.C.Maxwell and
Some Aspects of its Subsequent Development., Thesis, Ph.D.,
London.

CHALMERS A.F. (1973a), 'Maxwell's Methodology and His Application of
It to Electromagnetism', Studies in Hist. and Phil. of Sci.,
4, (1973), p.107.

CHALMERS A.F. (1973b), 'The Limitations of Maxwell's Electromagnetic
Theory', Isis, 64, (1973), p.469.

CLAUSIUS R. (1869), 'Upon the New Conception of Electromagnetic
Phenomena Suggested by Gauss', Phil.Mag., 37, (1869), p.445.

COHEN I.B. (1956), Franklin and Newton.

COHEN I.B. (1960), The Birth of a New Physics.

CRANEFIELD P.F. (1954), 'Clerk Maxwell's Corrections to the Page Proofs of "A Dynamical Theory of the Electromagnetic Field"', Ann.Sci., 10, (1954), p.359.

CROWTHER J.G. (1935), British Scientists of the Nineteenth Century, Pelican Ed., 1940-41, 2v.

DAVY H. (1821), 'Further Researches on the Magnetic Phenomena Produced By Electricity ....', Phil.Trans., 111, (1821), p.425.

DESCARTES R. (1634), 'Letter to Beeckman', in Oeuvres, ed. Tannery-Adam, 6, p.307.

DIBNER B. (1961), Oersted and the Discovery of Electromagnetism.

DICTIONARY OF SCIENTIFIC BIOGRAPHY.

DOMB C. (ed.) (1963), Clerk Maxwell and Modern Science.

DORAN B.G. (1975), 'Origins and Consolidation of Field Theory in Nineteenth-Century Britain : From the Mechanical to the Electromagnetic View of Nature', in R.McCormmach (ed.), Hist.Stud.in the Phys.Sci., Vol.6.

DORLING J. (1973), 'Demonstrative Induction : Its Significant Role in the History of Physics', Phil. of Sci., 40, (1973), p.360.

DORLING J. (1978), 'On Explanations in Physics : Sketch of an Alternative to Hempel's Account of the Explanation of Laws', Phil. of Sci., 45, (1978), p.136.

DRUDE P. (1897), 'Ueber Fernwirkungen', Ann.der Phys., 62, (1897), p.1.

288

DUHEM P. (1902), <u>Les Théories Electriques de J.C.Maxwell</u>.

DUHEM P. (1905), <u>The Aim and Structure of Physical Theory</u>, (tr.
P.P.Wiener, 1962).

DUHEM P. (1969), <u>To Save the Phenomena</u>.

EVERITT C.W.F. (1975), <u>James Clerk Maxwell</u>.

FARADAY M. (1821, 2), 'Historical Sketch of Electro-Magnetism',
<u>Annals of Phil.</u>, <u>2</u>, (1821), p.195, p.274, and <u>3</u>, (1822),
p.107.

FARADAY M. (1832-6), <u>Diary</u>.

FARADAY M. (1839-55), <u>Experimental Researches in Electricity</u>, 3v.

FECHNER G.T. (1831), <u>Maassbestimmungen Uber Die Galvanische Kette</u>.

FECHNER G.T. (1845), 'Ueber die Verknupfung der Faraday'-schen
Inductions ....', <u>Ann.der Phys.</u>, <u>64</u>, (1845), p.337.

FEYERABEND P.K. (1968), 'How to be a Good Empiricist -- A Plea
for Tolerance in Matters Epistemological', in P.H.Nidditch
(ed.), (1968), <u>The Philosophy of Science</u>.

FEYERABEND P.K. (1970), 'Problems of Empiricism, Part II', in R.G.
Colodny (ed.) (1970), <u>The Nature and Function of Scientific
Theory</u>.

FEYNMANN R.P. (1964), <u>Lectures on Physics</u>, 3v.

FITZGERALD G.F. (1902), <u>The Scientific Writings of G.F.Fitzgerald</u>,
ed. J.Larmor.

FRICKE M. (1976), 'The Rejection of Avogadro's Hypotheses', in C.
Howson (ed.) (1976).

GARDINER K.R. and D.L. (1965), 'André-Marie Ampère and His English
Acquaintances', <u>B.J.Hist.Sci.</u>, <u>2</u>, (1965), p.235.

GAUSS C.F. (1877), Werke, 5.

GILLISPIE C.C. (1960), The Edge of Objectivity.

GLAZEBROOK R.T. (1896), James Clerk Maxwell and Modern Physics.

GREEN G. (1828), 'An Essay on the Application of Mathematical Analysis
to the Theories of Electricity and Magnetism', rep. in
The Mathematical Papers of the Late George Green.

GROVE W. (1867), The Correlation of Physical Forces, 5th.Ed.

GUERLAC H. (1963), 'Some Historical Assumptions of the History of
Science', in A.C.Crombie (ed.), (1963), Scientific Change.

GUILLEMIN E.A. (1932), 'Early Developments in Electromagnetic Theory',
Isis, 18, (1932), p.118.

HAAS-LORENTZ G.L. De (ed.) (1957), H.A. Lorentz -- Impressions of His
Life and Work.

HALL E.H. (1879), 'On a New Action of the Magnet on Electric Currents',
Amer.J.Math., 2, (1879), p.287.

HANSON N.R. (1962), 'Leverrier : The Zenith and Nadir of Newtonian
Mechanics', Isis, 53, (1962), p.359.

HANSTEEN (1857), Letter to Michael Faraday, 30th.Dec.1857, in H.
Bence Jones, Life and Letters of Faraday, 2, p.395.

HARE R. (1840), 'A Letter to Prof.Faraday, on Certain Theoretical
Opinions', rep. in Phil.Mag., 17, (1840), p.44.

HEAVISIDE O. (1893-1912), Electromagnetic Theory, 3v., Dover reprint
1950, 1v.

HEIMANN P.M. (1970), 'Maxwell and the Modes of Consistent Representation',
Archives for History of Exact Sciences, 6, (1970), p.171.

HEIMANN P.M. (1971), 'Maxwell, Hertz, and the Nature of Electricity',
    Isis, 62, (1971), p.149.

HEIMANN P.M. (1971b), 'Faraday's Theories of Matter and Electricity',
    B.J.Hist.Sci., 5, (1971), p.235.

HELMHOLTZ H. (1847), 'On the Conservation of Force', in R.Taylor,
    Scientific Memoirs, 5, (1853), p.114.

HELMHOLTZ H. (1870), 'Ueber die Theorie der Elektrodynamik, I',
    Wissenschaftliche Abhandlungen, 1, (1882), p.545.

HELMHOLTZ H. (1872), 'Ueber die Theorie der Elecktrodynamik, II',
    rep. in English in Phil.Mag., 44, (1872), p.530.

HELMHOLTZ H. (1873), 'On Later Views of the Connection of Electricity
    and Magnetism', Smithsonian Reports, (1873), p.246.

HELMHOLTZ H. (1876), 'On the Electromagnetic Action of Electric
    Convection', Phil.Mag., 2, (1876), p.233.

HELMHOLTZ H. (1881), 'On the Modern Development of Faraday's Con-
    ception of Electricity', J. of the Chem.Soc., 39, (1881),
    p.

HEMPEL C.G. (1966), Philosophy of Natural Science.

HERTZ H. (1884), 'On the Relations Between Maxwell's Fundamental
    Electromagnetic Equations and The Fundamental Equations of
    the Opposing Electromagnetics', rep. in Hertz (1896).

HERTZ H. (1892), Electric Waves.

HERTZ H. (1896), Miscellaneous Papers, Landmarks of Science Micro-card.

HERTZ H. (1899), The Principles of Mechanics.

HESSE M.B. (1953), 'Models in Physics', B.J.P.S., 4, (1953), p.198.

HESSE M.B. (1955), 'Action at a Distance in Classical Physics',

Isis, 46, (1955), p.337.

HESSE M.B. (1961), Forces and Fields.

HESSE M.B. (1963), Models and Analogies in Science.

HESSE M.B. (1973), 'Logic of Discovery in Maxwell's Electromagnetic

Theory', in R.N.Giere and R.S.Westfall (eds.) (1973)

Foundations of Scientific Method : The Nineteenth Century.

HESSE M.B. (1974), The Structure of Scientific Inference.

HIROSIGE T. (1962), 'Lorentz's Theory of Electrons and the Development

of the Concept of the Electromagnetic Field', Jap.Stud.Hist.

Sci., 1, (1962), p.101.

HIROSIGE T. (1966), 'Electrodynamics before the Theory of Relativity,

1890-1905', Jap.Stud.Hist.Sci., 5, (1966), p.1.

HIROSIGE T. (1969), 'Origins of Lorentz's Theory of Electrons and the

Concept of the Electromagnetic Field', in R.McCormmach (ed.),

Hist.Stud.in the Phys.Sci., Vol.1.

HOPLEY I.B. (1959), 'Maxwell's Determination of the Number of Elect-

rostatic Units in One Electromagnetic Unit of Electricity',

Ann.Sci., 15, (1959), p.91.

HOSPERS J. (1956), 'What is Explanation?', reprinted in A.G.N.Flew,

(ed.), (1956), Essays in Conceptual Analysis.

HOVGAARD W. (1932), 'Ritz's Electrodynamic Theory', Stud. in Applied

Math., (1932), p.218.

HOWSON C. (ed.) (1976), Method and Appraisal in the Physical Sciences.

HUMPHREYS A.W. (1937), 'The Development of the Conception and
        Measurement of Electric Current', Ann.Sci., 2, (1937),
        p.164.

HUTTEN E.H. (1948), 'On Semantics and Physics', Proc.Arist.Soc., 49,
        (1948), p.115.

HUTTEN E.H. (1953), 'The Role of Models in Physics', B.J.P.S., 4,
        (1953), p.284.

JONES R.V. (1973), 'James Clerk Maxwell at Aberdeen 1856-60', Notes
        and Records of the Royal Society of London, 28, (1973),
        p.57.

KANT I. (1786), Metaphysical Foundations of Natural Science,
        translated by J.Ellington (1970).

KARGON R. (1969), 'Model and Analogy in Victorian Science, Maxwell's
        Critique of the French Physicists', Journal of the Hist. of
        Ideas, 30, (1969), p.423.

KING R.W.P. (1949), 'Review of Mogens Pihl : Der Physiker L.V.Lorenz.
        Eine Kritische Untersuchung', Isis, 40,(1949), p.64.

KIRCHOFF G. (1849), 'Ueber eine Ableitung der Ohm'schen Gesetze ...',
        Ann.der Phys., 78, (1849), p.506, and reprinted in
        Phil.Mag., 37, (1850), p.463.

KIRCHOFF G. (1857), 'On the Motion of Electricity in Wires', Phil.
        Mag., 88, (1857), p.393.

KIRCHOFF G. (1857a), 'Uber die Bewegung der Electricitat in Drahten',
        this is the German original of his (1857).

KIRCHOFF G. (1857b), 'Uber die Bewegung der Electricitat in Leitern', Ann.der Phys., 102, (1857), p.529.

KOENIGSBERGER L. (1906), Hermann von Helmholtz, translated by F.A. Welby.

KUHN T.S. (1971), 'Notes on Lakatos', in R.C.Buck and R.S.Cohen (eds.), Boston Studies in the Philosophy of Science, Vol.8.

LAKATOS I. (1962), 'Infinite Regress and the Foundations of Mathematics', Arist.Soc.Supp.Vol., 36, (1962), p.155.

LAKATOS I. (1968), 'Changes in the Problem of Inductive Logic', in I.Lakatos (ed.), The Problem of Inductive Logic.

LAKATOS I. (1970), 'Falsificationism and the Methodology of Scientific Research Programmes', in I.Lakatos and A.E.Musgrave (eds.) (1970), Criticism and the Growth of Knowledge.

LAKATOS I. (1971), 'History of Science and Its Rational Reconstructions', reprinted in Howson (1976).

LAKATOS I. (1974), 'The Role of Crucial Experiments in Science', Studies in the History and Philosophy of Science, 4, (1974), p.357.

LAKATOS I. and A.E. MUSGRAVE (eds.) (1970), Criticism and the Growth of Knowledge.

LAPLACE P.S. (1829-39), Méchanique Céleste.

LARMOR J. (ed.) (1937), Origins of Clerk Maxwell's Electric Ideas.

LENZ E. (1834), 'Ueber die Bestimmung der Richtung ....', Ann.der Phys., 31, (1834), p.483.

LODGE O.J. (1892), Modern Views on Electricity, 2nd.Ed.

LORENTZ H.A. (1909), Theory of Electrons.

LORENTZ H.A. (1923), Clerk Maxwell's Electromagnetic Theory, The
     Rede Lecture for 1923.

LORENZ L. (1867), 'On the Identity of the Vibrations of Light and
     Electrical Currents', Phil.Mag., 34, (1867), p.287.

MAGIE W.F. (1935), A Source Book in Physics.

McCORMMACH R. (1970), 'H.A.Lorentz and the Electromagnetic View of
     Nature', Isis, 61, (1970), p.459.

McGUIRE I.E. (1968), 'The Origin of Newton's Doctrine of Essential
     Qualities', Centaurus, 12, (1968), p.233.

MAXWELL J.C. (1855), 'Letter to William Thomson', in Larmor (ed.),
     (1937), p.17.

MAXWELL J.C. (1856), 'On Faraday's Lines of Force', reprinted in
     Niven (ed.) (1965), The Scientific Papers of James
     Clerk-Maxwell.

MAXWELL J.C. (1862), 'On Physical Lines of Force', reprinted in
     Niven (ed.) (1965).

MAXWELL J.C. (1865), 'A Dynamical Theory of the Electromagnetic
     Field', reprinted in Niven (ed.) (1965).

MAXWELL J.C. (1868), 'On a Method of Making a Direct Comparison
     of Electrostatic with Electromagnetic Force; with a Note
     on the Electromagnetic Theory of Light', reprinted in
     Niven (ed.) (1965).

MAXWELL J.C. (1868b), 'Thomson and Tait's Natural Philosophy',
     reprinted in Niven (ed.) (1965).

MAXWELL J.C. (1873), A Treatise on Electricity and Magnetism, in

general I have used the 1891 3rd. edition which is

available in a 1954 Dover reprint.

MAXWELL J.C. (1873b), 'Electromagnetism', English Cyclopaedia, Arts

and Sciences Supplement, (1873), p.854.

MAXWELL J.C. (1877), Matter and Motion.

MAXWELL J.C. (1881), An Elementary Treatise on Electricity.

MAXWELL J.C., 'On Action at a Distance' reprinted in Niven (ed.)

(1965).

MAXWELL J.C. (1931), James Clerk Maxwell : a Commemorative Volume

1831-1931, (Essays by J.J.Thomson, Planck, Einstein, etc.)

MAXWELL J.C. and FLEEMING JENKIN (1863),'On the Elementary Relations

between Electrical Measurements', App.C to the 2nd. B.A.

Report on Electrical Standards, (1863), p.130.

MEYER H.W. (1971), A History of Electricity and Magnetism.

MEYER K. (1920), H.C.Oersted,Scientific Papers, 3v.

MOTTELAY P.F. (1922), Bibliographical History of Electricity and

Magnetism, Chronologically Arranged.

MURPHY R. (1833), Elementary Principles of the Theories of Electricity,

Heat, and Molecular Actions.

MUSGRAVE A.E. (1968), Impersonal Knowledge, Thesis, Ph.D., London.

MUSGRAVE A.E. (1974), 'Logical Versus Historical Theories of Confirmation',

B.J.P.S., 25, (1974), p.1.

MUSGRAVE A.E. (1976), 'Method or Madness', in R.S.Cohen et.al.(eds.),

Essays in Memory of Imre Lakatos, (Boston Stud. in the P. of S.).

NEUMANN C. (1868), 'Die Principien der Elektrodynamik', Math. Annalen, 70, (1880), p.400.

NIDDITCH P.H. (ed.) (1968), The Philosophy of Science.

NIVEN W.D. (ed.) (1965), The Scientific Papers of James Clerk-Maxwell, Dover reprint.

OERSTED H.C. (1813), 'Ansicht der Chemischen Naturgesetze ...' translated into 'Recherches sur l'Identité ...', Fahie, Hist. of Electric Teleg., (1884), p.270.

OERSTED H.C. (1820), 'Experimenta Circa Effectum Conflictus Electrici in Acum Magneticam', Ann.Phil., 16, (1820), p.273.

OERSTED H.C. (1821), 'On Electromagnetism (A) The History of My Previous Researches on this Subject', new translation in R.C.Stauffer (1957).

OERSTED H.C. (1830), 'Thermoelectricity', article in The Edinburgh Encyclopaedia (1830).

OHM G.S. (1827), 'Die Galvanische Kette ....', translated in vol.2 of R.Taylor, Scientific Memoirs.

PEIERLS R.E. (1963),'Field Theory since Maxwell', in Domb (ed.) (1963).

PIHL M. (1962), 'The Scientific Achievements of L.V.Lorenz', in E.C.Jordan (ed.) (1963), Electromagnetic Theory and Antennas; Proceedings.

POINCARE H. (1901), Electricité et Optique, 2nd ed., 2v.

POINCARE H. and F.K.VREELAND (1904), Maxwell's Theory and Wireless
Telegraphy.

POISSON S.D. (1812), 'Mémoire sur la Distribution de l'Electricité
à la Surface des Corps Conducteurs', Mem. de l'Inst.,
(1811), p.1.

POISSON S.D. (1821-7), 'Mémoire sur la Theorie du Magnetisme', Mem.
Acad. Sci., 6, (1823), p.441.

POLYA G. (1954), Mathematics and Plausible Reasoning, 2v.

POPPER K.R. (1934), Logic of Scientific Discovery.

POPPER K.R. (1945), The Open Society and Its Enemies, 2v.

POPPER K.R. (1957), The Poverty of Historicism.

POPPER K.R. (1963), Conjectures and Refutations.

POPPER K.R. (1972), Objective Knowledge.

POPPER K.R. (1974), The Philosophy of Karl Popper.

O'RAHILLY A. (1965), Electromagnetic Theory, Dover reprint, 2v.,
first published in 1938 as Electromagnetics.

RANDALL Sir J. (1963), 'Aspects of the Life and Work of James Clerk
Maxwell', in Domb (ed.) (1963).

RIEMANN G.F.B. (1867), 'A Contribution to Electrodynamics', Phil.
Mag., 34, (1867), p.368.

RITZ W. (1908a), 'Recherches Critique sur l'Electrodynamique
Générale', Ann.Chim.Phys., 8 Series, 13, (1908), p.145.

RITZ W. (1908b), 'A Critical Investigation of Maxwell's and Lorentz's
Electrodynamic Theories', translated in Hovgaard (1932),
p.225 and f.

RIVE A. de la (1853), Treatise on Electricity, 2v.

ROLLER D. and D.H.D.ROLLER (1954), The Development of the Concept
Electric Charge.

ROMER A. (1942a), 'The Speculative History of Atomic Charges 1873-
1895', Isis, 33, (1942), p.671.

ROMER A. (1942b), 'The Experimental History of Atomic Charges, 1895-
1903', Isis, 34, (1942), p.150.

ROSENFELD L. (1957), 'The Velocity of Light and the Evolution of
Electrodynamics', Il Nuouvo Cimento, 5, Supp. Vol.4 Series
10, (1957), p.1630.

ROWLAND H.A. (1878), 'On the Magnetic Effect of Electric Convection',
Amer.J.Sci., 15 (1878), p.30.

RUSSELL B. (1941), 'On the Value of Scepticism', in Let the People
Think.

SCHAEFER W. (1931), 'Gauss's Investigations On Electrodynamics',
Nature, 128, (1931), p.339.

SCHAGRIN M. (1963), 'The Resistance to Ohm's Law', Amer.J.Phys., 31,
(1963), p.536.

SCHUSTER A. (1910), 'The Clerk-Maxwell Period' in A History of the
Cavendish Laboratory, 1871-1910.

SCOTT W.T. (1963), 'Resource Letter FC-1 on the Evolution of the
Electromagnetic Field Concept', Amer.J.Phys., 31, (1963),
p.819.

SHADOWITZ A. (1975), The Electromagnetic Field.

SIMPSON T.K. (1966), 'Maxwell and the Direct Test of his Electro-
        magnetic Theory', Isis, 57, (1966), p.411.

SIMPSON T.K. (1970), 'Some Observations on Maxwell's Treatise on
        Electricity and Magnetism', Stud. in Hist. and Phil. of
        Sci., 1, (1970), p.244.

SMART J.J.C. (1972), 'Science, History, and Methodology', Brit.J.
        Phil.Sci., 23, (1972), p.266.

SMITH-ROSE R.L. (1948), James Clerk Maxwell, F.R.S. 1831-1879.

SNOW HARRIS Sir W. (1849), Rudimentary Electricity.

SNOW HARRIS Sir W. (1867), A Treatise on Frictional Electricity.

SPENCER J.BROOKES (1967), 'Boscovich's Theory and its Relation to
        Faraday's Researches : An Analytic Approach', Archive
        for History of Exact Sciences, 4, (1967), p.184.

STAUFFER R.C. (1953), 'Persistent Errors Regarding Oersted's
        Discovery of Electromagnetism', Isis, 44, (1953), p.307

STAUFFER R.C. (1957), 'Speculation and Experiment in the Background
        of Oersted's Discovery of Electromagnetism', Isis, 48,
        (1957), p.33.

STINE W.M. (1903), 'The Contribution of H.F.E.Lenz to the Science
        of Electromagnetism', Journal of the Franklin Institute, 155,
        (April-May 1903), p.301 and p.363.

SUSSKIND C. (1964), 'Observations of Electromagnetic-Wave Radiation
        Before Hertz', Isis, 55, (1964), p.32.

TAYLOR R. (1837), Scientific Memoirs, Vol.1.

TAYLOR R. (1841), Scientific Memoirs, Vol.2.

THOMPSON S.P. (1895), 'Mirrors of Magnetism', Phil.Mag., 39, (1895), p.213.

THOMPSON S.P. (1910), The Life of William Thomson.

THOMSON J.J. (1885), 'Report on Electrical Theories', Report of the 55th. Meeting of the British Association, (1885), p.97.

THOMSON J.J. (1893), Notes on Recent Researches in Electricity and Magnetism.

THOMSON W. (1846), 'On the Mathematical Theory of Electricity in Equilibrium', Camb. and Dublin Math. J., 1, (1846), p.75.

THOMSON W. (1860), 'Measurement of the Electrostatic Force Produced by a Daniell's Battery', Proc.Roy.Soc., 10, (1860), p.319

THOMSON W. (1872), Reprint of Papers on Electrostatics and Magnetism.

THOMSON W. (1882), Mathematical and Physical Papers.

TRICKER R.A.R. (1955), 'On the Teaching of Electricity -- III', School Science Review, 36, (1955), p.213.

TRICKER R.A.R. (1962), 'Ampère as a Contemporary Physicist', Contemporary Physics, 3,(1962), p.453.

TRICKER R.A.R. (1965), Early Electrodynamics.

TRICKER R.A.R. (1966), The Contributions of Faraday and Maxwell to Electrical Science.

TURNER D.M. (1927), Makers of Science : Electricity and Magnetism.

TURNER J. (1955a), 'Maxwell on the Method of Physical Analogy', Brit. J.Phil.Sci., 6, (1955), p.226.

TURNER J. (1955b), 'A Note on Maxwell's Interpretation of Some
Attempts at Dynamical Explanation', Annals of Science,
2, (1955), p.238.

TURNER J. (1956), 'Maxwell on the Logic of Dynamical Explanation',
Phil. of Sci., 23, (1956), p.36.

TYNDALL J. (1868), Faraday as a Discoverer.

VAIHINGER H. (1924), The Philosophy of 'As If'.

WATKINS J.W.N. (1964), 'Confirmation, the Paradoxes, and Positivism',
in M.Bunge (ed.), The Critical Approach to Science and
Philosophy.

WEBER W. (1846), Electrodynamische Maassbestimmungen, partial reprint
in Ann.der Phys., 73, (1848), p.193, and this is translated
in R.Taylor, Scientific Memoirs, Vol.5, p.489.

WEBER W. (1848), 'On the Measurement of Electrodynamic Forces', this
is the partial reprint referred to in the previous entry.

WEBER W. (1864), Electrodynamische Maassbestimmungen, Vol.IV.

WHITTAKER Sir E.T. (1951), A History of Theories of Aether and
Electricity.

WILLIAMS L.PEARCE (1960), 'Michael Faraday and the Evolution of the
Concept of the Electric and Magnetic Field', Proc.Roy.
Inst., 38, (1960), p.235.

WILLIAMS L.PEARCE (1962a), 'Ampère's Electrodynamic Molecular
Model', Contemporary Physics, 4, (1962), p.113.

WILLIAMS L.PEARCE (1962b), 'The Physical Sciences in the First Half

    of the Nineteenth Century : Problems and Sources',

    History of Science, 1, (1962), p.1.

WILLIAMS L.PEARCE (1965), Michael Faraday.

WILLIAMS L.PEARCE (1966), The Origins of Field Theory.

WILLIAMS L.PEARCE (1975), 'Should Philosophers be Allowed to Write

    History', Brit.J.Phil.Sci., 27, (1975), p.241.

WINDRED G. (1932), 'The Relation Between Pure and Applied Electrical

    Theory : with Special Reference to Mathematical Methods',

    Isis, 18, p.184.

WINTER H.J.J. (1944), 'The Reception of Ohm's Electrical Researches

    by His Contemporaries', Phil.Mag., 35, (1944), p.371

WOODRUFF A.E. (1962), 'Action at a Distance in Nineteenth Century

    Electrodynamics', Isis, 53, (1962), p.439.

WOODRUFF A.E. (1968), 'The Contribution of Hermann von Helmholtz to

    Electrodynamics', Isis, 59, (1968), p.300.

WORRALL J. (1976), 'Thomas Young and the 'Refutation' of Newtonian

    Optics', in C.Howson (ed.) (1976).

ZAHAR E.G. (1973), 'Why did Einstein's Programme Supersede Lorentz's ?',

    reprinted in C.Howson (ed.) (1976).