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HOW WE KNOW ABOUT ELECTRONS

1. INTRODUCTION

In 1997 we celebrated the centenary of Thomson's (1897) 'Cathode Rays' that is conveniently taken as marking the discovery of the electron, our first fundamental particle. The electron is not just our first fundamental particle, but one of the earliest microphysical entities to acquire secure status in modern physics. We see how early electrons acquired this status if we recall the status of the humble atom at this same time. While atoms had been a subject of interest in science and natural philosophy for millennia, their existence and properties remained clouded in debate and clear demonstrations of their existence and properties only emerged in the early part of this century when the same occurred for electrons.

While the existence and properties of electrons stood at the forefront of physical research at the start of the twentieth century, any doubts about the electron's existence and basic properties soon disappeared. So physicist and historian Edmund Whittaker could review Thomson's investigations of the electron and conclude without apology:

> Since the publication of Thomson's papers, these general conclusions have been abundantly confirmed. It is now certain that electric charge exists in discrete units, vitreous [positive] and resinous [negative], each of magnitude 4.80×10^{-10} electrostatic units or 1.6×10^{-19} coulombs. (Whittaker 1951, p. 365)

Whittaker's confidence reflect a widespread certainty in the physics community about electrons. If the existence and properties of electrons were not assured in 1897, then this assurance arose in the years that followed, so that doubt over the existence of electrons has now moved beyond the realm of normal scientific prudence.

My concern in this paper is to understand the stratagems used by physicists to arrive at this assurance. I will visit two general argument forms that have been used to affirm the existence and properties of electrons. The first, to be reviewed in section 3, has been brought to the notice of philosophers of science by Wesley Salmon in the corresponding analysis of the reality of atoms. It requires the determination of numerical properties of electrons in many different circumstances. That these properties invariably prove to have the same values – that is, their massive over determination by observation and experiment – is taken as evidence for the existence and properties of electron and that the parameters

Robert Nola and Howard Sankey (eds.), After Popper, Kuhn and Feyerabend, 67–97.
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computed are not just accidental artefacts of experiment. In the second, to be discussed in section 4, I will review a strategy for inferring theory *from* phenomena that has entered into the literature under many names including 'demonstrative induction' and 'eliminative induction'. These inferences are especially strong since they proceed from phenomena. Their inductive risk resides principally in the general hypotheses needed to enable the inferences so that security of the inference depends in great measure on our warrant for these general hypotheses. Before proceeding to these two stratagems, in section 2 I will review various traditions of scepticism that might lead us to disavow the existence and properties of electrons in spite of their entrenchment in modern physics. I will indicate why I find each tradition unsuccessful in sustaining scepticism about electrons. Concluding remarks are offered in section 5.

2. VARIETIES OF SCEPTICISM ABOUT MICROPHYSICAL ENTITIES

While the physics community may have harboured no real doubts over electrons for many years, several traditions of criticism have maintained and some continue to maintain that, at best, theories of microphysical entities cannot be taken at face value or, at worst, can in principle never succeed in their goal of revealing the nature of matter on a submicroscopic scale. I have divided these traditions into three classes in increasing order of the severity of their scepticism.

2.1. Evidential Insufficiency

This most modest of sceptical positions merely asserts that there happens to be insufficient evidence to warrant belief in the microphysical entity. While this attitude is so straightforward as to need little elucidation, it is helpful to review one of the most celebrated instances of this sceptical position: Wilhelm Ostwald's rejection of atomism in favour of his energeticism. More precisely, his attitude was that chemical thermodynamics simply did not need the hypothesis of atoms.[1] Those results that were usually thought to require atomism could be secured directly from the phenomena of chemical thermodynamics. For example he could recover stoichiometric laws such as the law of definite proportions. As he explained to an audience assembled in the inner sanctum of British atomism in his (1904, pp. 363, 364) Faraday Lecture, one distinguishes a chemical compound as those solutions for which the 'distinguishing point in the boiling curve'[2] is independent of pressure. He concluded his lecture with a flourish. His suggestions 'are questions put to nature. If she says Yes, then we may follow the same path a little further. If she says No – well, then we must try another path' (p. 522). One might suspect him of a feigned modesty given the history of polemical confrontation with atomists. But subsequent events proved otherwise and showed that Ostwald's scepticism was of the contingent nature appropriate to this category of evidential insufficiency. Famously, in the preface of the 1909 4th edition of his *Grundriss der Allgemeinen Chemie*, he announced:[3]

> *I have convinced myself that we have recently come into possession of experimental proof of the discrete or grainy nature of matter, for which the atomic hypothesis had vainly sought for centuries, even millennia.*

He reflected on J.J. Thomson's successful isolation and counting of gas ions and Perrin's accommodation of Brownian motion to the kinetic theory, concluding

> ... this evidence now justifies even the most cautious scientist in speaking of the *experimental* proof of the atomistic nature of space-filling matter. What has up to now been called the atomistic hypothesis is thereby raised to the level of a well-founded theory, which therefore deserves its place in any textbook intended as an introduction to the scientific subject of general chemistry.

Is scepticism based on evidential insufficiency appropriate in the case of the electron? Whether it is must be decided by an investigation of the evidence available and its interpretation. The material in section 3 and after shows that the existence and basic properties of electrons lies well within the reach of current evidence as long as we are allowed standard stratagems for interpretation of this evidence. Needless to say this sort of scepticism is warranted concerning interactions sustained by electrons in exotic domains for which we have scant or no evidence.

2.2. Programmatic Restrictions

A stronger form of scepticism attempts to avoid entirely the issue of the evidential warrant for microphysical entities. Instead it asserts that the establishment of the existence and properties of microphysical entities is simply not the business of science. Its goals lie elsewhere and suppositions about microphysical entities are perhaps at best an intermediate convenience or a temporary delusion that will pass as the true purpose of science is realised. This attitude is exemplified in positivism or instrumentalism. Ernst Mach is the best known proponent of this attitude, asserting (1882, pp. 206, 207; Mach's emphasis):

> ... it would not become physical science to see in its self-created, changeable, economical tools, molecules and atoms, realities behind phenomena, forgetful of the lately acquired sapience of her older sister, philosophy, in substituting a mechanical mythology for the old animistic or metaphysical scheme, and thus creating no end of suppositious problems. The atom must remain a tool for representing phenomena, like the functions of mathematics. Gradually, however, as the intellect, by contact with its subject-matter, grows in discipline, physical science will give up its mosaic play with stones and will seek out the boundaries and forms of the bed in which the living stream of phenomena flows. The goal which it has set itself is the *simplest* and most *economical* abstract expression of facts.

When this goal is realised, we shall need talk of atoms no more since atoms do not figure in the facts of experience whose simple expression is sought.

This viewpoint promotes a scepticism about micro-entities not by directly casting doubt on our knowledge of them but by suggesting that all consideration of them is, in the last analysis, irrelevant to the true purposes of science. If this tradition of scepticism is taken on its face, it need not concern us here directly, for it amounts to a self-imposed decision not to entertain the existence of entities that are not directly part of the observed phenomena. Why should we impose this on ourselves? There are two cases to consider. Either micro-entities such as electrons lie within the reach of evidence or they do not. In the first case, programmatic scepticism seems wholly unwarranted. We can know about electrons. Why would we choose to know less than we can? Why would we think this a virtue? That

Mach would urge this in the case of atoms suggests that his programmatic restrictions are rooted in a deeper form of scepticism belonging to the second case indicated, in which micro-entities such as electrons lie beyond the reach of evidence. That Mach did harbour the ensuing blanket disbelief in micro-entities is suggested by his dismissal of molecules as things which 'only exist in our imagination' and are 'valueless images'.[4] But now our variety of scepticism is deepened and is seen to rest on presumptions about the methods of science. We must ask how these presumptions can be sustained.

2.3. Methodological Limitations

This deepest form of scepticism asserts that knowledge of micro-entities is something that simply extends beyond the reach of the methods of science. The roots of this form of scepticism can lie in several areas: philosophical, historical and sociological.

In its philosophical form, this version depends on a pessimism concerning the reach of evidence. The underdetermination thesis asserts that no body of evidence, no matter how extensive, will ever be able to determine a unique theory. So no matter how strong the evidential case may appear for some theory, other comparably viable competitors assuredly wait in the wings. The related Duhem–Quine thesis asserts that evidence must confront theory as whole; any particular hypothesis in a theory can be protected from falsification by suitable adjustment of other parts of the theory. For our purposes an immediate corollary is that the empirical success of any theory of micro-entities cannot assure us of the correctness of any particular hypothesis of the theory, so that while we may have an empirically successful theory of electrons, we cannot know as an independent fact that the electron charge is about 1.6×10^{-19} coulombs. I have argued elsewhere (Norton 1993, 1994) that both of these theses are false and that their failure can be shown by looking at the use of a particular strategy of inductive inference, demonstrative induction. I will describe in section 4 below how demonstrative induction was used in the case of the electron.

The historically founded scepticism derives from a recognition of the pervasiveness of change in theories in the history of science. The electron is a clear example. In the course of the century since Thomson's 'Cathode Rays' paper, theories of the electron have undergone near constant revision. Thomson's classical electron is not the electron of Einstein's 1905 relativity theory, which is not the jumping electron of Bohr's old quantum theory, which is not the wave of Schrödinger's wave mechanics, which is not the excitation of a Fermion field in quantum field theory – and so on in multiple subtler variations. The moral, according to the so called 'pessimistic meta-induction,' is that none of the superseded theories was correct, so, by induction, we can have no confidence that our latest theory is correct. Now the existence of a sequence of theories through time may be a manifestation of a pathology: massive, repeated, inexorable error and misconstrual of evidence. Or it may be evidence of great health: a tradition of theory which grows richer by the appropriation of new evidence and in which earlier theories are preserved in limiting form and corrected. In joint work with

Jon Bain (Bain and Norton, forthcoming) we have investigated this meta-induction in the case of the electron. We conclude the latter is the case and that the meta-induction fails for the electron. What the history of the electron reveals is a vigorously growing body of theory concerning the electron, in which the evidential successes of the early theories of the sequence are largely preserved as our understanding of the properties of the electron are refined and expanded. Thus our estimates of the charge and mass of the electron have scarcely altered in over eighty years while we have learned of properties of the electron unanticipated by Thomson: its quantum character, its possession of an intrinsic, quantized spin, that it obeys a Fermi–Dirac statistics and that its electromagnetic interactions may be unified with its interactions in the weak force. Within the sequence is a growing core of stable properties for which physicists have good evidence and which point to the existence of a single stable structure whose existence and nature is revealed in growing detail by the development of theories of the electron. We cannot conclude from this that physicists make no errors or that our latest theory is incorrigible. But optimism and not pessimism is surely licensed, for we are assured that the inevitable errors are sought, found and corrected leaving us with an ever more secure image of the electron.

Finally a sociologically based scepticism seeks to undermine the evidential warrant of scientific theories by examining the social structures and processes that produce the theories. Such seems to be the goal of the 'strong programme in sociology of knowledge' of Bloor (1991) which is intended, apparently, to answer affirmatively the question (p. 3) 'Can the sociology of knowledge investigate and explain the very content and nature of scientific knowledge?'. Insofar as the very content of scientific theories can be explained solely in terms of the social interactions of scientists, then that content can reflect only the conventional agreement of scientists and not an agreement of the theories with nature.[5]

The strong programme embodies a very strong form of scepticism. That the scepticism is justified remains entirely unclear. In evaluating it, we must guard against a simple error. We cannot conclude that a theory only reflects agreement between scientists merely because of the possibility in principle of giving a detailed reconstruction of the social processes that lead to its acceptance. Scientific theories are generated and validated by the communal effort of scientists. Thus it will always be possible to discern and describe the social process that lead to the communal acceptance of this or that theory. But offering such a purely sociological description, as is invited by the strong programme, cannot by itself decide whether a theory agrees with nature or fails to agree or to what degree. The community of astrologers has failed to discern causal influences from sun and moon to the earth; the community of astrophysicists has succeeded in discerning the gravitational influence of sun and moon that raises the earth's tides. That one has failed and one succeeded cannot be revealed merely by noting, even in painstaking detail, the exact course of social interaction that led to acceptance of this or that theory. Such judgement can only be provided by testing the evidential warrant offered by the communities against good epistemic standards, but such comparison has no place in the strong programme. More simply, whether a community has succeeded or failed in its goal of describing nature can only be determined if one is willing to consider what it takes to be successful, but such considerations have been eschewed in the strong programme.[6]

It may well be the case that the manoeuverings of particular scientists are driven by social factors: their needs for wealth or power or the jealous defeat of a rival. But that does not establish that the arguments they mount for the bearing of the theory on evidence are defective. Indeed sound arguments of this type would appear to be the most effective weapons in these battles. Again, as Forman (1971) suggests, the quantum physicists of the 1920s may well have found it expedient to hawk a new physics that emphasised chance and indeterminacy since those characteristics were welcomed by the chaotic society of Weimar Germany. But that would not preclude the possibility that these physicists had in addition good reasons and evidence for the indeterminism of their theory.

Needless to say, there are cases in science in which social factors have illegitimately determined the cognitive content of a scientific theory. A strong candidate is Cyril Burt's investigations of the inheritance of intelligence by means of identical twin studies. The posthumous discovery of anomalies in the statistics of his papers showed that his claims could not be read at face value and raised the question of whether his data has been faked to fit Burt's expectations.[7] Just one such case is needed to refute the view that the scientific endeavour is perfect and invariably offers theories with proper evidential warrant. But refuting that view is of little interest since it is not one that could ever be taken seriously. Rather we need assistance in deciding between two views: the complete scepticism of the strong programme or a more sober view which allows that some scientific theories enjoy proper evidential warrant whereas others do not. Cases such as Burt's do not allow us to distinguish these two views. But what would refute the first view, the complete scepticism of the strong programme, is even one case of a scientific theory with proper evidential warrant. There are many such cases. That of the electron is just one. The nature of the evidential warrant for the existence and properties of electrons will be reviewed below.

While these traditions of scepticism entail that scientists do not, should not or cannot establish the existence and properties of electrons, the broad consensus of physicists is that they long ago succeeded in doing just that. An enormous array of strategies and techniques of great complexity and ingenuity have been employed to this end and, in principle, there may be no common ground between the different arguments used to extract this or that property from the various items of observational evidence. It turns out, however, that we can discern two particular strategies that have been used very effectively to establish the existence and properties of electrons. I will review them in the sections that follow.[8]

3. OVERDETERMINATION OF CONSTANTS

3.1. Reality of Atoms and the Quantum

The great debate over the reality of atoms was resolved with some speed in the first decade of this century. Many contributed to the victory of the atomists, but their undisputed leader was Jean Perrin. His case for atoms was reduced to a single grand argument that was brought to the attention of the modern philosophical community by Salmon (1984, pp. 213–227). Perrin's argument was very simple in concept. Atomism is predicated on the idea that atoms are so small that matter appears continuous on the macroscopic scale. In earlier years atomists were unable to give reliable estimates of the sizes of atoms; they had to content themselves with the assertion that these sizes must be exceedingly small since they had transcended all attempts at measurement. With the coming of the twentieth century this situation changed. Through many different phenomena and experimental techniques, it became possible to estimate the size of atoms. The quantity computed in estimating this size is Avogadro's number N, the number of atoms or molecules in a gram mole.

Perrin himself had worked experimentally on determining the magnitude of N. When this work was drawn together with the work of others, Perrin was able to report roughly a dozen different methods for estimating N and they all gave values of N in close agreement. In the conclusion to *Les Atoms*, Perrin tabulated the resulting estimates of N from methods based on:[9] viscosity of gases (kinetic theory), vertical distribution in dilute emulsions, vertical distribution in concentrated emulsions, Brownian movement (displacement/rotations/diffusion), density fluctuations in concentrated emulsions, critical opalescence, blueness of the sky, diffusion of light in argon, black body spectrum, charge as microscopic particles, radioactivity (projected particles/Helium produced/Radium lost, energy radiated). The methods agreed in giving values of N in the range 60–69 $\times 10^{22}$ (with one exception, critical opalescence, that returned 75×10^{22}). The case for the reality of atoms and molecules lay in this agreement as Perrin explained (p. 215):

> Our wonder is aroused at the very remarkable agreement found between values derived from the considerations of such widely different phenomena. Seeing that not only is the same magnitude obtained by each method when the conditions under which it is applied are varied as much as possible, but that the numbers thus established also agree among themselves, without discrepancy, for all the methods employed, the real existence of the molecule is given a probability bordering on certainty.

The agreement of all these different methods for estimating N is to be expected if matter has atomic constitution. If, however, matter were not to have atomic constitution, then it would be very improbable that all these estimates of a non-existent quantity would turn out to agree.

In his analysis, Salmon (1984, pp. 213–227) has characterized the argument as employing the common cause principle. I do not wish here to pursue the connection to causation and the common cause principle since it seems to me that the essential result is secured already by a simple feature in the logic of the agreement. The agreement between the various estimates of the parameter is expected if the relevant theory is true, but it is very improbable if the theory is false.[10] In this form, the important result resides in an overdetermination of a parameter by many different methods. This overdetermination has been exploited quite frequently in the history of science – more examples follow. Since the parameter determined in these examples is always a constant of a theory, I have called the approach the method of overdetermination of constants.

The method was used by James Jeans when he sought to justify the then emerging quantum theory of the early 1920s in a new chapter added to the 1914 first edition in the 1924 second edition of his *Report on Radiation and the Quantum Theory*. Jeans noted that he had reviewed four phenomena that revealed the failure of quantum theory and the need for a new quantum theory (p. 61) '(i) Black Body Radiation; (ii) The spectra of the elements; (iii) The photoelectric effect; (iv) The specific heats of solids.' In the atomic theory, the size of atoms is set by the magnitude of N with the limit of a continuum theory approached with infinite N. The magnitude of the deviation from classical physics of quantum theory is set by Planck's constant h, with the classical limit arising when h vanishes. So Jeans proceeded to tabulate the values of h derived from the phenomena in these four areas, recovering values in very close agreement; they varied from 6.547×10^{-27} to 6.59×10^{-27}. According to Jeans (p. 61), these concordances demonstrate that the four phenomena 'agree in pointing to the same new system of quantum-dynamics.'

3.2. The Mass to Charge Ratio and the Charge of the Electron

The method of overdetermination of constants, as we shall now see, played an important role in the early history of the electron and much of importance was shown for the electron by demonstrating that the same constant values were recovered for each of the mass to charge ratio of the electron and the charge of the electron in many different circumstances.

When Thomson wrote his 1897 'Cathode Rays' the problem he addressed was not simply the issue of whether there are electrons. The issue was to decide between two theories of the nature of cathode rays. The theory favoured 'according to the almost unanimous opinion of German physicists' is that these rays are 'due to some process in the aether' (1897a, p. 293), that is, 'some kind of ethereal vibration or waves' (1906, p. 145). Thomson, along with his British colleagues, favoured the view that cathode rays consisted of charged corpuscles. More precisely, over the course of the following decade or two, Thomson and others sought to establish a series of properties for cathode rays:

(a) Cathode rays consist of a stream of corpuscles (electrons).
(b) Electrons are negatively charged.
(c) The universality of electrons: all cathode rays consist of electrons of just one type and these electrons are constituents of all forms of matter.
(d) Electrons are much less massive than atoms and molecules.

To make his case, Thomson sought to show that cathode rays had just the properties that would be expected of a stream of negatively charged corpuscles. Thus he recalled that Perrin had shown that cathode rays could impart a negative electric charge to an electroscope and then he reported his own improvement on the experiment.

The bulk of his paper was given over to reporting on two types of experiments: the deflection of cathode rays by magnetic fields and the deflection of cathode rays by electrostatic fields. Qualitatively, these experiments already yielded results indicating that cathode rays consisted of a stream of negative corpuscles. The rays were deflected along the direction of the electric field as expected for negatively charged particles and deflected perpendicular to the direction of the magnetic field as expected for negative charges in motion. The most telling result was that Thomson recovered the same value of *m/e*, the mass to charge ratio, for both magnetic and electric deflection. For the first case of magnetic deflection, he reported 26 values recovered from three cathode ray tubes operated under different circumstances and they lay in the small range of 0.32–1.0×10^{-7} – although Thomson doubted the accuracy of the tubes that gave the smaller results. For the second case of electric deflection, Thomson reported 7 values of *m/e* in the range 1.1–1.5×10^{-7}.

The overdetermination of this constant *m/e* was a strong test of the electron hypothesis. One might imagine that, through some fortuitous agreement of effects, an aetherial wave could be deflected by electric and magnetic fields in directions akin to that of deflected electric particles. But the concordance of the computed values of *m/e* showed quantitative agreement between the observed deflections and the properties of charged particles that transcends such chance. If a ray is deflected by a magnetic field, one can perhaps choose a value for *m/e* so that the deflection is compatible with the assumption that the ray consists of charged particles deflected by a magnetic field of strength **H** that will deflect the particles with acceleration $\mathbf{a} = -(e/m)\, \mathbf{v} \times \mathbf{H}$. But once this value of m/e is set, no further adjustment is possible to accommodate the deflection due to an electric field strength **E**. That deflection is just to be measured and it must agree with the acceleration $\mathbf{a} = -(e/m)\mathbf{E}$. That both series of experiments returns the same value of (*m/e*) assures us that this necessary compatibility has been secured.

While the evidence of this quantitative agreement is strong. Thomson already felt that the qualitative result made the electric nature of cathode rays inescapable. His computation of the ratio *m/e* was intended to answer further questions. He wrote (1897a, p. 302):

> As the cathode rays carry a charge of negative electricity, are deflected by an electrostatic force as if they were negatively electrified, and are acted on by a magnetic force in just the way in which this force would act on a negatively electrified body moving along the path of these rays, I can see no escape from the conclusion that they are charges of negative electricity carried by particles of matter. The question next arises. What are these particles? are they atoms, or molecules, or matter in a still finer state of subdivision? To throw some light onto this point, I have made a series of measurements of the ratio of the mass of the particles to charge carried by it.

So the result that Thomson emphasised was that the value of *m/e* recovered was independent of the variation of many factors in his experiment. It did not vary appreciably if he used different gases in his tubes: air, hydrogen, carbonic acid; or if the electrodes were iron, aluminium or platinum. Thomson summarised the agreement in his 1906 Nobel Prize speech (Thomson 1906, p. 148):

> The results of the determinations of the values of *e/m* made by this method are very interesting, for it is found that, however the cathode rays are produced, we always get the same value for *e/m* for all the particle in the rays. We may, for example, by altering the shape of the discharge tube and the pressure of the gas in the tube, produce great changes in the velocity of the particles, but unless the velocity of the particles becomes so great that they are moving nearly as fast as light, when other

> considerations have to be taken into account, the value of e/m is constant. The value of e/m is not merely independent of the velocity. What is even more remarkable is that it is independent of the kind of electrodes we use and also of the kind of gas in the tube. The particles which form the cathode rays must come either from the gas in the tube or from the electrodes; we may, however, use any kind of substance we please for the electrodes and fill the tube with gas of any kind and yet the value of e/m will remain unaltered.

This invariability of the ratio demonstrated the universality of electrons; they were the same whatever may be the matter from which they were derived. Finally the value of m/e of 10^{-7} was significantly smaller than even the smallest value then known for a charge carrier, the hydrogen ion of electrolysis, whose value in 1897 was estimated as 10^{-4}. From the constancy of m/e and its magnitude, Thomson drew his major conclusion (1897a, p. 312):

> ... we have in the cathode rays matter in a new state, a state in which the subdivision of matter is carried very much further than in the ordinary gaseous state: a state in which all matter – that is, matter derived from different sources such as hydrogen, oxygen, &c. – is of one and the same kind; this matter being the substance from which all the chemical elements are built.

From examination of the ratio m/e for electrons moving freely as cathode rays, Thomson had inferred to the universal presence of electrons in all matter. But are electrons identifiable within matter itself? As it turned out, in 1897, Thomson could report another determination of the ratio m/e from quite a different source. Zeeman (1897) had investigated experimentally the splitting of emission spectra by magnetic fields. As Zeeman explained (p. 232), H.A. Lorentz had communicated to him a theoretical analysis of the splitting. If the emitting atoms were modelled as bound, vibrating ions, the splitting could be accounted for by the magnetically induced alterations in the frequency of vibration. Working back from the magnitude of the shift in the spectral lines, Lorentz's model enabled Zeeman to estimate the charge to mass ratio of the ions: 'It thus appears that e/m is of the order of magnitude 10^7 electromagnetic C.G.S. units.' Thomson (1897b, p. 49) could not resist concluding another briefer treatment of his work on cathode rays by reporting this happy agreement of the value of e/m for charges bound within matter:[11]

> It is interesting to notice that the value of e/m, which we have found from the cathode rays, is of the same order of magnitude as the value 10^{-7} deduced by Zeeman from his experiments on the effect of a magnetic field on the period of the sodium light.

Thomson's demonstration of the constancy of m/e had allowed him to mount a good case for the properties (b)–(d) listed above. But his analyses had not established (a), the corpuscularity of electrons. He could not preclude the possibility that cathode rays and the matter of electrons are a continuous form of matter with a uniform mass and charge distribution so that any portion of the matter would present a constant ratio m/e. The possibility was eliminated by experiments aimed at directly determining the charge of the electron e. The celebrated experiments and analysis is due to Millikan (1913, 1917). But already a decade before Langevin (1904) had assembled an argument for the corpuscularity of the electronic matter that used the overdetermination of constants, that constant being, of course, the electric charge. He reviewed a series of methods then available for estimating electron charge. They were: measurement by many investigators (including Thomson) of charged water droplets condensed from supersaturated water vapour; investigations by H.A. Lorentz of the emissive and absorptive power of radiation by metals; and investigation by Townsend of the diffusion of ions in an electric field. These methods all produced values in agreement for the charge of the electron; the values ranged from 3.1×10^{-10} to 4×10^{-10} esu. Townsend's investigations also enabled another deduction of Avogadro's number in agreement with values then accepted. Langevin (p. 202) concluded:

> Here is an important group of concordant indications, all of absolutely distinct origin, which show without doubt the granular structure of electric charges, and consequently the atomic structure of matter itself. The measurements which I have just enumerated allow us to establish, in great security, the hypothesis of the existence of molecular masses.
>
> I seek to point out here this extremely remarkable result, which belongs without doubt to some fundamental property of the ether and of the electrons, that all these electrified centres, whatever may be their origin, are now identical from the point of view of the charge which they carry.

3.3. Limitations

The strength of the method of overdetermination of constants is that it allows comparison and combination of evidence from very diverse domains and, should the evidence disagree, that disagreement will be revealed clearly. The weakness of the method is that the significance of agreement need not always be apparent. We can infer to our intended hypothesis only if we can be assured that the concordance of results is very unlikely to arise if that hypothesis is false. But that assurance may be elusive. For example, the wave theory of light, as expressed in Maxwell's electrodynamics, famously predicts a velocity of propagation for light of 3×10^8 m/s. So, if we find numerous independent measurements of the speed of light returning this value, are we allowed to infer to the wave theory of light and not to a corpuscular theory? This agreement might well eliminate a Newtonian emission theory of light in which the velocity of light will vary with the velocity of the emitter. But it cannot preclude a theory that merely asserts that light consists of non-quantum, relativistic particles of zero rest mass, for all such particles will propagate at 3×10^8 m/s.

An almost exactly analogous problem arose for Thomson's 1897 argument. Recall that his original purpose was to decide between the corpuscular theory of cathode rays that he favoured and the aetherial wave theory. In introducing his paper (1897, pp. 293, 294), he complained of the difficulty of deciding between the two theories since 'with the aetherial theory it is impossible to predict what will happen under any given circumstances, as on this theory we are dealing with hitherto unobserved phenomena in the aether, of whose laws we are ignorant'. His remarks proved prescient. With the emergence in the 1920s of de Broglie's matter wave hypothesis and then Schrödinger's wave mechanics, it became quite apparent that at least some sort of wave-like theory of the electron would be adequate to the phenomena known to Thomson – although the form of the theory is of a type that we can scarcely fault Thomson for failing to anticipate. As it turns out, however, the bulk of Thomson's conclusions remains unaffected by this development. Electrons are negatively charged systems of just one type that inhere in all ordinary forms of matter at a subatomic level. Thomson also correctly concluded that individual electrons have a definite mass and charge and, at least in the form of cathode rays, do comprise independent systems. These conclusions do not, however, eliminate the possibility that electrons have a wavelike character. That possibility would have appeared *remote* in 1897, however, when no wavelike form of matter was known to which quite specific, discrete quantities of mass and charge could be assigned. The quantity of energy or momentum assignable to a light wave depended on the intensity and spatial extent of the wave.

4. EVIDENCE AS AN IMAGE OF THEORY

4.1. The Many faces of Demonstrative Induction

These deficiencies of the method of overdetermination of constants can be ameliorated by a stronger technique that gives a far more definitive verdict on the import of evidence. The penalty for this added strength is that situations in which this second method can be used are more contrived and harder to find. In its most general form, it is very simple. One starts with evidence statements, be they observations or experimental reports. From them, with the assistance of some general hypotheses, one deduces a theory or hypothesis within a theory. Several points are important. First, the inference is deductive. So there is no longer any inductive risk associated with the use of an inductive argument form. That risk has been relocated into assertions (the more general hypotheses) and the risk associated with accepting them usually proves easier to assess and control. Second, the direction of the deductive inference is *from* evidence *to* theory. This fact almost immediately de-fangs the underdetermination thesis since the item of evidence is seen to point to a particular theory or even particular hypothesis.

The method has recently been rediscovered by a number of philosophers and it goes under several names.[12] This multiplicity of names is unfortunate since it masks the fact all of these philosophers are discussing essentially the same method. The method was used by Newton in his *Principia* so it is easy to see why it is often called 'Newtonian deduction from the phenomena.' Again, since the arguments employed are deductive (i.e. demonstrative) yet serve a function usually reserved for inductive arguments, it is also natural to label the approach as using 'demonstrative induction'. We might also view the general hypotheses of the arguments extensionally as defining the largest class of theories in which we expect the true theory to be found. The observations or experimental reports then eliminate all but the viable candidates from this universe. In this view, the method employs 'eliminative induction'. Finally, if the universe of theories admits of parameterisation, by far the most common case, then the method has been called 'test theory methodology'.

On first acquaintance, it seems dubious that there might be non-trivial instances of these deductions. A few simple examples dispel this impression. The most straightforward is a simplification of a deduction used repeatedly by Newton in his *System of the World* to recover the inverse square law of gravitational attraction from the phenomena of planetary motion. For simplicity, assume that planetary orbits are circular (as they nearly are) and recall Kepler's third law of planetary motion which relates the period T of a planet's motion to the radius R of its orbit

$$T^2 \propto R^3.$$

This is the phenomenon whose theoretical significance is sought. Newton's laws of motion contribute to the general hypotheses through which this phenomenon will be interpreted. More precisely, his mechanics give us the result that a planet moving at velocity V in a circular orbit of radius R is accelerated toward the centre of the orbit with acceleration $A = V^2/R$. Also we have from simple geometry that the velocity V and period of orbit T are related by $V = 2\pi R/T$. Using these two results we now deduce from Kepler's third law that the planets are accelerated towards the centre of their orbits with an acceleration A that varies with the inverse square of the distance from the centre. For we can write

$$A = V^2/R = (2\pi)^2(R/T)^2(1/R) = (2\pi)^2(R^3/T^2)(1/R^2)$$

and note that (R^3/T^2) is a constant from Kepler's third law so that we have

$$A \propto 1/R^2.$$

In short, we have inferred from the phenomena of planetary orbits to the inverse square law of gravitation, even if only in a special form.

This is a simple example and quite transparent. See the literature cited earlier in this section for more substantial examples drawn from quantum theory, general relativity and other branches of modern physics. Norton (1993), for example, presents a very striking instance. In the ten years following Planck's 1900 analysis of black body radiation, the principal result came to be understood to be a somewhat weak and puzzling one: one could save the phenomena of black body radiation if one presumed some kind of quantum discontinuity, that is, that thermally excited systems could adopt only a discrete set of energy levels. While this result was clearly of some significance, it did not suffice to establish as aberrant a hypothesis as quantum discontinuity. That this hypothesis saved the phenomena did not preclude the possibility that other, more conservative hypotheses might not also suffice. These hopes were dashed in 1911 and 1912, when Ehrenfest and Poincaré showed in a most robust demonstrative induction that one could infer *from* the phenomena of black body radiation *to* quantum discontinuity. They thereby demonstrated the power of evidence to determine theory and, moreover, to force a particular hypothesis and one that was then strenuously resisted by the physics community.

4.2. Bohr's 1913 Atomic Theory

Niels Bohr's (1913) 'On the Constitution of Atoms and Molecules' developed a theory of atomic structure that was as bold as it was successful. Einstein reserved the highest praise for Bohr's achievement when he wrote in his *Autobiographical Notes* (p. 43) that it '... appeared to me as a miracle – and appears to me a miracle even today. This is the highest form of musicality in the sphere of thought'. The core of Bohr's theory was an account of the behaviour of electrons bound in orbit around the nuclei of atoms. Famously he supposed that the electrons could persist in a discrete set of stationary states governed by the electrostatic interaction between the electron and the nucleus. When electrons dropped from higher to lower energy states, however, they would emit a quantum of light radiation, thereby enabling Bohr to account for the discrete lines characteristic of atomic emission spectra.

Bohr's first published development of his theory was in the first section of his paper ('Part 1 – Binding of Electrons by Positive Nuclei: Section 1, General Considerations'). In recounting it, I will group and label Bohr's results to aid in later description of his arguments. To begin Bohr laid out the results that govern the orbit of a negatively charged electron around a positively charged nucleus on the assumption that the electron and nucleus interact only electrostatically. These results were standard and comprise:

> *A: The electrostatic model of electron orbits*
> An electron of negative charge of magnitude e and mass m much smaller than the nucleus orbits a nucleus of positive charge of magnitude E in a closed elliptical orbit with major semi-axis a and eccentricity ε. The energy W released in forming the orbital state is.[13]
>
> $$W = eE/2a \tag{1}$$
>
> and the frequency ω of the orbit (in cycles per second) is
>
> $$\omega = \frac{\sqrt{2}W^{3/2}}{\pi eE\sqrt{m}}. \tag{2}$$

That orbiting electrons conformed to this electrostatic model was a startling aspect of Bohr's theory – perhaps even as surprising as the quantum discontinuity about to be introduced. Classical electrodynamics was then very well developed. One of its incontrovertible results was that a negatively charged electron in orbit about a positively charged nucleus is accelerated and therefore must radiate its energy and spiral into the nucleus, so that no stable orbit is possible. In considering only an electrostatic interaction, Bohr chose to ignore this prediction.

The next component of Bohr's theory was a restriction to a discrete set of the energy levels admissible for bound electrons. I will express the restriction in two forms:

> *B: Quantization of energy levels*
> The stationary electron orbits are restricted to those whose energy W and frequency ω are related by the condition

$$W = \tau h\omega/2 \tag{3}$$

for τ a positive integer $1, 2, 3, \ldots$ and h Planck's constant. In the context of the electrostatic model, this restriction (3) induces equivalent restrictions on the energy W, frequency ω and major semi-axis a. We recover the first by using (2) to eliminate ω from (3)

$$W = \frac{2\pi^2 me^2 E^2}{\tau^2 h^2}. \tag{4}$$

Bohr's justification of (3) was somewhat tenuous. He recalled Planck's then latest development of his theory of black body radiation and that it was based on the assumption that an oscillator with natural frequency ν would radiate energy in integral amounts $\tau h\nu$ where as before $\tau = 1, 2, 3, \ldots$ Bohr next considered the process of binding the electron to the atom. In falling from a great distance to a stationary orbit with frequency ω, Bohr simply assumed that Planck's frequency ν would be replaced by *half* the corresponding frequency ω of the orbit so that final energy of the orbit would be given by (3).

Finally, Bohr's (3) proves puzzling to every reader of Bohr's paper if the reader tries to fit the result with the mechanisms proposed in the remainder of the paper. It is easy to interpret (3) as deriving from a sequence of τ emissions as the electron drops to stationary states of successively lower energy. These successive states would differ in energy by the same amount, $h\omega/2 = h\nu$, and the radiation emitted with each transition would be of energy $h\nu$ at frequency ν. The catch is that (4) does not supply such equally spaced energies for the stationary states, so that (3) cannot be justified by the supposition that it represents τ distinct emissions. I will return to this rather unsatisfactory situation below, where we will see that Bohr himself abandoned his justification of (3) in terms of Planck's theory.

In section 2 of his paper, Bohr turned to a more successful application of Planck's notion:

> *C: Emission of light by quanta*
> When an electron drops from a stationary state with quantum number τ_1 to a state of lower energy with quantum number τ_2, energy is emitted as homogeneous radiation with frequency ν given by
>
> $$W_{\tau_2} - W_{\tau_1} = h\nu. \tag{5}$$

The combination of (4) and (5) gave Bohr the great success of his theory. It now followed that the emission spectra of an excited atom would contain lines with the frequencies

$$\nu = \frac{2\pi^2 me^2 E^2}{h^3}\left(\frac{1}{\tau_2^2} - \frac{1}{\tau_1^2}\right). \tag{6}$$

Bohr could now report near perfect agreement with the observed emission spectrum of hydrogen.

D: Observed emission spectrum of hydrogen
The lines of the spectrum are given by the formula

$$\nu = R\left(\frac{1}{\tau_2^2} - \frac{1}{\tau_1^2}\right) \tag{7}$$

where τ_1 and τ_2 are positive integers and the value of the constant R is given as

$$R = 3.290 \times 10^{15}. \tag{8}$$

Bohr's predicted functional form (6) matched exactly the functional form fitted to the observed spectral lines (7). Moreover Bohr could recover the value of the constant R to within plausible experimental error by substituting appropriate values for the hydrogen atom into the constant of his expression (6). For the hydrogen atom, Bohr reported, we have $e = E = 4.7 \times 10^{-10}$, $e/m = 5.31 \times 10^{17}$ and, with Planck's constant $h = 6.5 \times 10^{-27}$, we have

$$\frac{2\pi^2 me^4}{h^3} = 3.1 \times 10^{15} \tag{9}$$

so that the theory predicts a hydrogen spectrum governed by

$$\nu = \frac{2\pi^2 me^4}{h^3}\left(\frac{1}{\tau_2^2} - \frac{1}{\tau_1^2}\right). \tag{6'}$$

To summarise, by the close of section 2, the case that Bohr could mount for his theory resided in two arguments. The first, a deductive argument, captures the remarkable fact that the principles of Bohr's theory were able to save the phenomena of the hydrogen emission spectrum:

A: The electrostatic model of electron orbits
B: Quantization of energy levels
C: Emission of light by quanta
———————————————— (Deduction)
D: Observed emission spectrum of hydrogen.

On the strength of this argument, we can then say that Bohr's entire theory enjoys inductive support by the hypothetico-deductive scheme. I pass over the question of whether the scheme supports one or other of A, B or C in lesser or greater degree and represent this argument as

D: Observed emission spectrum of hydrogen.
———————————————— (Inductive support
– Hypothetico-Deduction
scheme)
A: The electrostatic model of electron orbits
B: Quantization of energy levels
C: Emission of light by quanta

4.3. Bohr's Demonstrative Induction

That so simple a theory should succeed in saving the phenomena of the hydrogen spectrum lends strong support to Bohr's theory. But against it remains the problem that Bohr's suppositions depend on very arbitrary elements. Why are we licensed to revert to simple electrostatics in selecting our stationary orbits? Are the quantum conditions imposed the only ones that will work? *Might* we not find an account of hydrogen spectra that does not require such wholesale departure from classical physics? The success of the hypothetico-deductive induction sketched above gives us no direct grounds for expecting that other less controversial analyses might not meet with comparable success. Anyone who has worked with Bohr's theory, however, rapidly loses such hopes. One quickly develops an intuitive sense that Bohr's principles, or something of comparable nature, are unavoidable if we are to give an adequate treatment of atomic spectra. His principles are in some sense re-expressions of the information already given us in the discreteness of the spectra. We shall soon see how these intuitions can be put in more precise form.

These intuitions certainly seem to be expressed by Millikan in his summary of Bohr's theory, written a few years after Bohr's paper. Millikan (1917, pp. 207–209) represented Bohr's theory as based on three assumptions, essentially comparable to A, B and C above, with A specialised to the case of circular orbits alone. Apparently wishing to assure the reader that these assumptions were not arbitrary flights of fancy, he announced (p. 209, Millikan's emphasis):

> It is to be noticed that, if circular electronic orbits exist at all, no one of these assumptions is arbitrary. Each of them is merely the statement of the existing *experimental* situation. It is not surprising, therefore, that they predict the sequence of frequencies found in the hydrogen series. They have been purposely made to do so. But they have not been made with any reference whatever to the exact numerical values of these frequencies.

Bohr also clearly sensed the artificiality of his initial development of the theory. His introduction of the condition B: Quantization of energy levels, in form of (3) contained an arbitrary deviation from the theory of Planck, justified only by its success in giving the right result. Planck's theory required emission of light energy in integral multiples of $h\nu$, where ν is the frequency of the emitting oscillator; Bohr based his theory on the supposition that stationary states with frequency ω are formed by the emission of light energy in integral multiples not of $h\omega$ but of $h\omega/2$, when an electron is captured by a nucleus. At this point in his development in his section 1, Bohr promised that all would soon be put right. Reflecting on the assumptions from which he was proceeding, he wrote (p. 5):

> The question, however, of the rigorous validity of both assumptions, and also of the application made of Planck's theory, will be more closely discussed in §3.

In returning to this question in section 3, Bohr (p. 12) immediately retracted one essential element of his argument for (3):

> ... we have assumed that the different stationary states correspond to an emission of a different number of energy-quanta. Considering systems in which the frequency is a function of the energy, this assumption, however, may be regarded as improbable; for as soon as one quantum is sent out the frequency is altered. We shall now see that we can leave the assumption used and still retain the equation [(3)], and thereby the formal analogy with Planck's theory.

Bohr could easily retract this element and with it his earlier justification for the quantum condition (3) for he was about to offer a far stronger derivation. That derivation lay in an inference that proceeded *from* the functional form of the observed hydrogen emission spectrum to the quantum condition (3). In reflecting on the derivation after it was complete, Bohr made explicit that his starting point now lay in observation and the inferences proceeded to theory. He wrote (section 3, p. 14):

> ... taking the starting point in the form of the law of the hydrogen spectrum and assuming that the different lines correspond to a homogeneous radiation emitted during the passing between different stationary states, we shall arrive at exactly the same expression for the constant in question as that given by [(6′)], if only we assume (1) that the radiation is sent out in quanta $h\nu$, and (2) that the frequency of radiation emitted during the passing of the system between successive stationary states will coincide with the frequency of revolution of the electron in the region of slow vibration.

In order to lay out concisely the argument Bohr develops in his section 3, I will again group and label Bohr's results. To begin, Bohr retained A: The electrostatic model of electron orbits, so that stationary electron states are possible. But he made essentially no assumptions about the further character of these stationary states other than:

> *E: Indexing of stationary electron states*
> These stationary states are indexed by a parameter τ and governed by the relation
>
> $$W = f(\tau)h\omega \qquad (10)$$
>
> where f is an undetermined function.

Since the function f is undetermined, this condition places very little restriction on the stationary states. The explicit presence of $h\omega$ in the formula is unnecessary. It is only there to simplify the final expression for $f(\tau)$, which would otherwise end up containing a factor of $h\omega$. The sole content of Equation (10) is an indexing of the energies W by a parameter τ. The real restriction brought through (10) comes into force when a form for the function f is determined and the parameter τ is shown to admit only integral values. Nothing at this point in Bohr's argument requires τ to be integer valued; it could adopt continuous values. Thus Bohr no longer assumes the quantization of energy levels of the bound electron; he is about to derive it. Combining (10) with Equations (1) and (2) from the electrostatic model, Bohr now inferred that the stationary states satisfy not (4) but the generalised relation

$$W = \frac{\pi^2 me^2 E^2}{2h^2 f^2(\tau)}. \qquad (4')$$

The assumptions of C: Emission of light by quanta, now enabled Bohr to translate this relation into a condition on the expected emission spectra.

Proceeding as before, these spectra will have lines at frequencies

$$\nu = \frac{\pi^2 m e^2 E^2}{2h^3}\left(\frac{1}{f^2(\tau_2)} - \frac{1}{f^2(\tau_1)}\right). \tag{6''}$$

Comparison with the functional form (7) of D: Observed emission spectrum of hydrogen, now fixed the undetermined function f as

$$f(\tau) = c\tau \tag{11}$$

where c is some constant and τ must be restricted to positive integer values alone. It is this last restriction on the range of values of τ that introduces the quantization of the bound electron's energy levels. That is, the crucial discreteness of the energy levels is now inferred from the observed spectrum and not posited.

All that remains to complete recovery of the quantization of the energy levels is to determine the value of the constant c. Of course that value could be fixed by employing the observed numerical value of R (8) in the formula for the observed spectrum. Bohr, however, proceeded to show that he had no need of this observation to fix the value of c. He could recover it merely by requiring that his theory behave classically in the domain of large quantum numbers.

> *F: Classical electrodynamics governs emission for large quantum numbers*
> An electron bound in orbit about a nucleus will emit its energy in light with a frequency equal to the momentary frequency of the orbit. If the electron energy is W, then that frequency is given by (2), $\omega = \sqrt{2}W^{3/2}/(\pi eE\sqrt{m})$.

In the domain of very large quantum numbers – say $\tau = N$ – this classical behaviour is to be imitated by an electron dropping from the $\tau = N$ energy state to the $\tau = N - 1$ state. The transition will generate a quantum of radiation of frequency

$$\nu = \frac{\pi^2 m e^2 E^2}{2h^3 c^2}\left(\frac{1}{(N-1)^2} - \frac{1}{N^2}\right) \approx \frac{\pi^2 m e^2 E^2}{2h^3 N^3}\cdot\frac{2}{c^2} \tag{12}$$

where the approximation introduced is for large N. The condition F requires that there be an emission of radiation at the frequency given by (2). Substituting the expression (4′) for W and the functional form $f(\tau) = c\tau$ into (2), we recover a frequency

$$\nu = \frac{\pi^2 m e^2 E^2}{2h^3 N^3}\cdot\frac{1}{c^3}. \tag{13}$$

Comparison of the two expressions sets the value of c at[14]

$$c = \tfrac{1}{2}$$

so that the expression for the admissible energy levels (4′) reverts to Bohr's original (4) $W = 2\pi^2 m e^2 E^2/(\tau^2 h^2)$. Bohr's deduction of the condition

B. Quantization of energy levels is now complete. Cast as a demonstrative induction, it can be summarised as:

Observation
  D: Observed emission spectrum of hydrogen
    (functional form (7) only)
General Hypotheses
  A: The electrostatic model of electron orbits
  C: Emission of light by quanta
  E: Indexing of stationary electron states
  F: Classical electrodynamics governs emission
    for large quantum numbers
——————————————————— (Deduction)
B. Quantization of energy levels

What continues to be noteworthy is that Bohr's argument required only the functional form fitted to the observed spectrum. Bohr's argument allows him to calculate the functional form's constant R and his predicted form is in close agreement with the observed value as we saw in (8) and (9) above.

4.4. A Reduced Form of Bohr's Demonstrative Induction

Impressive as Bohr's argument is, it still retains some features that are troubling to modern readers. The most significant is the continued dependence of the argument and theory on A: The electrostatic model of electron orbits. This model retains entities, such as elliptical orbits of electrons, and quantities such as the orbit's major semi-axis a and frequency ω that have been expunged from the ontology of modern, standard quantum theory. Of course we could not expect Bohr to foresee this. In 1913, with the amazing success of his analysis, it would be entirely reasonable to expect that quantum theory would settle on an ontology of discrete elliptical orbits for electrons with some as yet unknown theoretical element bringing about stochastic jumps between the admissible orbits.

Therefore it is interesting to notice that this electrostatic model is actually inessential to Bohr's demonstrative induction. Essentially the same results can be recovered from a reduced form of Bohr's argument that is compatible with the new quantum theory about to emerge in the 1920s. We will review the reduced version for the special case of an electron bound in the hydrogen atom and infer to the quantization of its energy levels.

The reduced form eschews all talk of elliptical orbits other than in the domain of correspondence with classical theory. Outside this domain, it posits only a stripped down version of the ontology of Bohr's 1913 theory:

A': Existence of stationary electron states
Electrons bound in an atom can persist in a variety of stationary states with energies $W(\tau)$, where τ is an index of these states of undetermined character and W is a strictly decreasing[15] function of τ, so that all states of equal energy are assigned the same value of τ.

No assumption is made or needed that these stationary states are elliptical orbits of some definite size and frequency of localised electrons. What is retained is that these states possess a definite energy. This condition will replace both A and E in Bohr's demonstrative induction. With this replacement, we can proceed as before. We invoke C: Emission of light by quanta, to arrive at the conclusion that the atomic emission spectra will contain frequencies

$$\nu = (W(\tau_2) - W(\tau_1))/h$$

for all admissible values of τ_1 and τ_2, such that $\tau_1 > \tau_2$. Comparing this expression with the functional form (7) of the observed emission spectrum for hydrogen, we conclude that the indices τ_1 and τ_2 adopt only positive integer values, $1, 2, 3, \ldots$ and the functional dependence of W is given as

$$W(\tau) = Rh/\tau^2 + \text{constant}. \tag{14}$$

We have now inferred the quantization of energy levels.

The constant R is still undetermined as is the additive constant in (14). Both values are set by invoking F: Classical electrodynamics governs emission for large quantum numbers. We will use a classical electrodynamic analysis in which the energy of an electron spatially very remote from the hydrogen nucleus is set to zero. Such an electron arises in the limit of infinitely large τ, so that we correspondingly set the additive constant of (14) to zero. We now take the case of a large value of $\tau = N$ for the hydrogen atom in which $E = e$ and substitute the simplified expression for $W = Rh/N^2$ into the expression $\omega = \sqrt{2}W^{3/2}/(\pi e^2\sqrt{m})$ for the orbital frequency of an electron with energy W in the classical analysis. We recover an expression for both orbital frequency ω and frequency of emitted radiation ν

$$\nu = \omega = \frac{\sqrt{2}}{\pi e^2\sqrt{m}} \cdot \frac{R^{3/2}h^{3/2}}{N^3}. \tag{15}$$

This classical process will be imitated by the light emitted in the transition from the state with $\tau = N$ to $\tau = N - 1$. We have from (7) that the frequency of light emitted in this process will be

$$\nu = R\left(\frac{1}{(N-1)^2} - \frac{1}{N^2}\right) \approx R \cdot \frac{2}{N^3}. \tag{16}$$

Setting the two frequencies of (15) and (16) equal we solve for an expression for $R = 2\pi^2 me^4/h^3$ which is just the expression for R in Bohr's theory. That is, we have recovered B: Quantization of energy levels, expression (4) restricted to the special case of the hydrogen atom for which $e = E$:

$$W(\tau) = \frac{2\pi^2 me^4}{h^2\tau^2}. \tag{4''}$$

We can summarise this reduced demonstrative induction as follows:[16]

Observation
D: Observed emission spectrum of hydrogen.
(functional form (7) only)
General Hypotheses
A′: Existence of stationary electron states
C: Emission of light by quanta
F: Classical electrodynamics governs emission
for large quantum numbers
———————————— (deduction)
B: Quantization of energy levels (for electron
in hydrogen atom, (4″))

This reduced demonstrative induction recovers the quantization of electron energy levels from a strict subset of Bohr's commitments. Bohr's A and E have been replaced by A′ which is itself entailed by A and E. The inference now only returns results for the hydrogen atom. The argument can be readily modified to allow recovery of the corresponding results for other atoms if we are able to affirm in D that the emission spectra of these other atoms are also governed by the functional form (7).

It is also noteworthy that all the assumptions of this reduced demonstrative induction are compatible with the new quantum mechanics that emerged in the 1920s. The stationary states of A′, for example, would simply correspond to the energy eigenstates of a bound electron. Therefore we would expect the conclusion to remain valid in the new quantum mechanics. And it does, of course. The energies of (4″) are simply the energy eigenvalues of an electron bound in a hydrogen atom.

This reduced demonstrative induction also gives us some insight into the much discussed logical inconsistency of Bohr's theory. That inconsistency lay in the presumption of the electrostatic model for electron orbits. That model provided for no radiation and thus had to be arbitrarily suspended as expedience required. In addition, one needed to ignore the massive body of evidence in other domains that showed that the behaviour of electrons was governed not merely by electrostatics but by electrodynamics. The reduced demonstrative induction shows us that Bohr's use of the electrostatic model was inessential for his celebrated account of atomic spectra. A subset of his commitments, free of manifest inconsistency, suffices for recovery of atomic spectra. This resolution is compatible with my earlier analysis (Norton 1987) of the logical inconsistency of the old quantum theory of black body radiation. There I urged that the viability of the theory depended on the existence of a consistent subtheory from which the essential results of the theory could still be recovered. We have now seen that the same strategy succeeds with Bohr's 1913 theory.

4.5. The Strength of Bohr's Demonstrative Induction

Bohr's theory provided a greatly deepened understanding of the properties of electrons bound in atoms. How strong was Bohr's evidence for these properties?

We have seen that Bohr's theory did not merely depend on its success in saving the phenomena of atomic spectra. There was a sense in which Bohr's theory was inferred from that phenomena. Thus Bohr's results are as secure as the reports of the phenomena and the demonstrative inductions that take us from them to the theory. In this section, I will assess the strength of the demonstrative induction. Since the argument itself is deductive, we need not torment ourselves with an evaluation of the degree of inductive risk introduced by an inductive argument form.[17] We have relocated all our inductive risk in the premises of the arguments. I will take the reports of atomic spectra as unproblematic and consider the general hypotheses that allow us to translate them into Bohr's theory. Bohr's case for his theory is made insofar as we can establish these general hypotheses.

There is very strong evidence for these general hypotheses. The evidence for them is of two types. The first is external and stems from other results in physics. The second is internal and derives from the way that the demonstrative inductions succeed.

To begin, we can review the external evidence by considering the general hypotheses individually. The condition C: Emission by light quanta, as Bohr makes clear, is imported from Planck's treatment of black body radiation. The general result – that systems of atomic size would emit energy in quanta of magnitude $h\nu$ – had become a fixture of the physics of the preceding decade. The result was difficult to interpret for there was no classical account of it, but it had repeatedly proved its utility . It was the core of Planck's original 1900 analysis of black body radiation and continued to feature in his more recent theories. Einstein has also developed analogous notions extensively commencing with his celebrated 1905 introduction of the notion of the light quantum. Finally the analyses of Ehrenfest and Poincaré of 1911 and 1912 (see Norton 1993) had shown the unavoidability in treatments of black body radiation of energy discontinuities associated with quanta of energy of size $h\nu$.

Soon after Bohr's investigations, Einstein's (1916a,b) celebrated '*A* and *B* coefficients' papers gave even more secure foundation to C: Emission of light by quanta. Einstein pictured molecules with discrete energy levels in thermal equilibrium with radiation. He supposed that energy exchanges were governed by just three probabilistic processes: spontaneous and induced emission and absorption. From this extraordinarily simple foundation, he recovered Planck's formula for the distribution of energy in heat radiation. In the recovery, he compared his formula with that of the Wien displacement law and *concluded* that the frequency of light ν emitted or absorbed when the molecule alters its energy between energies ε_m and ε_n is given by the formula $\varepsilon_m - \varepsilon_n = h\nu$, remarking immediately that this result is just 'the second rule in Bohr's theory of spectra' (Einstein 1916b, p. 69). That is, the formula (5) of C: Emission by light quanta, was derived by Einstein along with the Planck formula.

The condition F: Classical electrodynamics governs emission for large quantum numbers, was easier to understand and virtually impossible to avoid. It required only that the behaviour of electrons revert to classical behaviour when they are no longer closely bound to atomic nuclei. A full and very secure account of the behaviour of such free electrons was provided by the crown jewel of ninteenth century physics, Maxwell's electrodynamics, as perfected by the turn of the century.

Most troublesome is A: The electrostatic model of electron orbits, which provides for the existence of stationary electron orbits governed by electrostatics.[18] To begin, Rutherford's experiments had shown that atoms consisted of very small positively charged nuclei and associated negatively charged electrons. Thus something like the electrostatic model with electrons orbiting a small nucleus was suggested. It was clear that the model could not be governed by classical electrodynamics in its entirety, for then the electron must radiate its energy and spiral into the nucleus. As Bohr (1913, p. 4) observed, this prediction of classical electrodynamics was not in accord with observation:

> A simple calculation shows that the energy radiated out during the process considered will be enormously great compared with that radiated out by ordinary molecular processes.

The simplest response is just to switch off that component of the classical theory that leads to radiation, that is, to revert to electrostatics. But how can we be assured that we have preserved the correct component of electrodynamics? As it turns out, according to the new quantum mechanics developed in the 1920s, Bohr preserved too much of the classical theory in continuing to represent bound electrons as possessing definite positions, elliptical orbits and the like. The reduced demonstrative induction of the preceding section shows, however, why this excess ontology was not fatal to Bohr's theory: it was simply superfluous to the treatment of atomic spectra. We would now locate the essential component of A merely in the supposition of the existence of stationary states. As the reduced demonstrative induction shows, the electrostatic quantities Bohr introduces through the model A can be introduced instead through the condition F: Classical electrodynamics governs emission for large quantum numbers.

In spite of the obviously problematic character of a theory that embodies the electrostatic model A, Bohr could be assured that there was still something very right about the theory. This assurance would come in the way that his demonstrative induction succeeded. This yields the internal evidence for the general hypotheses foreshadowed above. In brief, the demonstrative induction's result is massively overdetermined. Just as the overdetermination of constants gives inductive support for the theory in which they arise, so this overdetermination gives inductive support for the soundness of the demonstrative *induction* and the general hypotheses in particular.

The way in which this inductive support arises is strongly analogous to a more familiar circumstance, which I will use to elucidate the support. This analogy can be shown by introducing yet another way of describing demonstrative induction. As the title of section 4 indicates, in a demonstrative induction, we can conceive of the evidence as an image of theory, much as cameras and other optical instruments provide images of objects. Some of the structural properties of the objects are encoded within the image and, by suitable analysis, we can recover these properties from the image. For example, by stereoscopic analysis of aerial photographs, we can determine the heights of objects on the ground, although these heights might not be apparent on a casual scan of the photographs. Correspondingly, the evidence of Kepler's third law is a kind of image of the law of gravitation and encodes within it information about the structure of the law. The demonstrative induction sketched above allows us to extract that structure. Similarly, observed atomic emission spectra are images of the theoretical structure that interests us, the energy spectrum of the bound electron. We read the image and recover that energy spectrum with a demonstrative induction.

When we interpret an image produced by an optical instrument, we are inferring from a two dimensional image to aspects of the full structure of the three dimensional object that produced the image. To begin, we have some confidence in the interpretation if any simple reading at all of the image is possible; that is, if the image is not just noise. Correspondingly, we have some initial confidence in Bohr's demonstrative induction simply because it is possible at all and as simply as it is. But the mere fact that an interpretation of the image is possible, cannot give final assurance of its correctness. Optical systems are typically troubled by aberrations. How do we know that we are not mistaking such an aberration for a real feature of the original object, much as Galileo misinterpreted the distorted images of the rings of Saturn and inferred that the planet had ears?

That assurance comes when we procure multiple images of the same object, taken, for example, from many different angles. If we reconstruct the same object from each image, we become very confident of our interpretation. The multiplicity of images overdetermines the character of the object; each image provides a test of the interpretation of the other images. Correspondingly, Bohr's observation report on atomic spectra massively overdetermine his resulting theory. Only a small part of the spectral observations catalogued by (7) are needed to complete his demonstrative induction. These spectral observations are customarily divided into series with frequencies

$$R\left(1-\frac{1}{n^2}\right) \quad \text{with } n = 2, 3, 4, \ldots \quad \text{(Lyman series)},$$

$$R\left(\frac{1}{2^2}-\frac{1}{n^2}\right) \quad \text{with } n = 3, 4, 5, \ldots \quad \text{(Balmer series)},$$

$$R\left(\frac{1}{3^2}-\frac{1}{n^2}\right) \quad \text{with } n = 4, 5, 6, \ldots \quad \text{(Paschen series)},$$

$$R\left(\frac{1}{4^2}-\frac{1}{n^2}\right) \quad \text{with } n = 5, 6, 7, \ldots \quad \text{(Brackett series), etc.}$$

It is easy to see that just the first of these alone, the Lyman series, provides an observational premise rich enough to support Bohr's complete demonstrative induction. That is, with Bohr's other premises, it is sufficient to force the functional form $f(\tau)$ to adopt the value (11) with integer values for τ. Thus the remaining series serve as tests, akin to the image of the same object from a different angle. If the theory were baseless, we would not expect each series to yield results concordant with the others when interpreted through the demonstrative induction. But it is already a part of the Bohr's induction that they all yield the same result.[19] The spectra of substances other than hydrogen could in principle supply more observation-images that would serve to overdetermine the theory further. This possibility was hard to realise in 1913. Bohr's (1913) discussion of the helium spectrum (pp. 10, 11) and of other substances (pp. 11, 12) was too hesitant to admit this possibility. Again, Bohr's treatment had concerned emission spectra only. In principle similar determinations would be possible for absorption spectra, but Bohr's (1913) discussion (pp. 15, 16) shows that such efforts would then have been premature.

There was another way in which the observations overdetermine the resulting theory. Bohr clearly took some pride in the power of his theory to give a definite value for the constant R of the formula (7), even though the value of the constant was already known from observation. In the analogy of the optical images, the value of the constant is another image of the energy spectrum of the hydrogen atom. On the basis of earlier images (the functional form of (7)) we predict what that new image must be. When the prediction matches the observation, we are assured again of our interpretation of the images.

5. CONCLUSION

Our present knowledge of the electron is the result of a century of vigorous investigation. While electrons are almost unimaginably small and abstruse in character, we can come to know of their existence and properties at the highest level of confidence. This paper has illustrated two of the stratagems used to reach this level of confidence. It also illustrates the utility of history of science in philosophy of science. That utility has become a commonplace of the last few decades of research. However I believe that it has often been misused. With talk of revolution and incommensurability widespread, it has been used to emphasise the irrational and the accidental in the history of science. While we must never lose sight of the highly contingent and often erratic character of science, it is all too easy to see nothing but this character in the history of science. One result is the pessimistic meta-induction discussed in section 2.3, which erroneously purports to establish the failure of all scientific theories without any explanation of how the failure arises.

When one approaches a speculative theory as bold as Bohr's 1913 theory, we add an easy drama to our histories if we overemphasise the irrational and accidental. Even the best of historians of science can be lured to do so. Thus Pais (1991, p. 148) calls Bohr's derivation of his expression for the constant in the spectral law (7) '... the most important equation that Bohr derived in his life. It represented a triumph over logic'. Pais found this notion so congenial that this section of his text is entitled 'Triumph over logic: the hydrogen atom'. If this is all we see in Bohr's achievement and others like it, then we end up with an image of science as a collection of imaginative, speculative leaps, untempered by prudence or reason. It is hard to have confidence that such an endeavour can supply us with an accurate and stable picture of physical reality.

What the analysis of this paper shows is that there was quite another side to Bohr's achievement of 1913. Once Bohr had conceived of the notion of accounting for atomic spectra through the transition of electrons between stationary states, the observed spectra and the requirement of concordance with then current physics drove him to a unique result, a particular energy spectrum for the electron. The passage to this result is laid out by Bohr himself in the demonstrative induction recounted in section 4.3. This shows that, in addition to any irrationality in Bohr's work, there is a core of sober theorising, firmly anchored in evidence. The reduced demonstrative induction of section 4.4 above shows us what this core is, how it is anchored in observation and, finally, that the success of Bohr's theory was quite independent of the much noticed inconsistency of his use of electrostatics in an electrodynamic system. While we admire Bohr for his brilliant, speculative leap, it is this sober core of his theory that survived the 1910s and was preserved in later theories of atoms and the electron. If we want to revel in the heroics of science, we should ask our history of science to report on these grand leaps. But if we want to understand how science succeeds in developing an ever more perfect picture of the physical world, we should ask our history of science about these stable cores of sound theorising that survive from one day to the next.

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NOTES

[1] In Ostwald's (1904, p. 508) words: 'Chemical dynamics has, therefore, made the atomic hypothesis unnecessary for this purpose [of deducing stoichiometric laws] and has put the theory of the stoichiometric laws on more secure ground than that furnished by a mere hypothesis.'

[2] If one plots the boiling point of a solution against the composition of the solution, the distinguishing point is a maximum or minimum of the curve. A solution boiling at this temperature does not change its composition.

[3] Translation from Brush (1976, Vol. 2, p. 699). Ostwald's emphasis. This demonstration of the atomic hypothesis represented a serious threat to the Second Law of Thermodynamics which is apparently violated by fluctuation phenomena such as Brownian motion. That the Law could be retained for at least macroscopic purposes required careful analysis and the resulting literature mutated and evolved into a quite surprising direction. See Earman and Norton 1998–1999.

[4] As quoted in Brush (1976, Vol. 1, p. 286) from Mach's *The History and Root of the Principle of Conservation of Energy* (Chicago: Open Court, 1911), p. 86.

[5] Although Bloor protests that such scepticism is not the goal of the strong programme (e.g. p. 166), it is hard to see how that goal can be disavowed when Bloor looks to sociology to 'explain the very content ... of scientific knowledge.' Certainly sufficient of his critics have supposedly misunderstood Bloor's intentions in this way for him to need an Afterword (pp. 165–170) devoted to correcting them. Among them is the eminent sociologist Ben-David.

[6] Such considerations do play a role in Collins' (1982, 1985, Ch. 4) analysis of the experimenter's regress. He describes the failure of scientists to achieve agreement with nature through experimental methods because of a fatal circularity in their use of experimental apparatus: correct results can only be obtained from good experiments; but good experiments are just those that produce correct results. Not even Collins holds that this regress supports a blanket scepticism about all experiment – some experiments are not defeated by it (see Collins 1985, p. 84). At best it suggests that some experimental claims are not well founded. In general the regress is broken by the independent calibration of the apparatus: we know which are the good experiments because we check that they give correct results in cases in which we know independently what the correct results are. Indeed it remains open as to whether there are any interesting cases of the regress. That Collins' example concerning the detection of gravitational radiation fails to illustrate the regress is shown by

Franklin (1994) and he also gives further general discussion of the role of calibration in experiment. (For Collins' rather odd reply, see Collins 1994.)

[7] See for example Dorfman (1978). The debate over Burt's work continues. See Mackintosh (1995).

[8] I will not consider Hacking's (1982) analysis in which he urges that the electron made the transition from a theoretical entity to one that was realistically construed when physicists began to manipulate electrons as part of further investigations. This is because Hacking's analysis does not reveal the basis for physicists belief in the reality of atoms; rather it displays evidence of that belief, their willingness to think of electrons as something that can be manipulated for other ends.

[9] The list quotes the row headings from Perrin's (1913) table, p. 215 from the translation volume.

[10] As an example of the latter, consider someone trying to mount a case for an aether in the nineteenth century. They may succeed in finding different methods of estimating the earth's (non-vanishing) velocity through the aether. But exactly because there is no such velocity, we would have no expectation that truly independent methods could yield concordant estimates.

[11] Thomson's remark contains a trivial, possibly even typographical error. The ratio e/m computed by Zeeman has the approximate value 10^{+7}; whereas its inverse m/e routinely computed by Thomson has the approximate value 10^{-7}. Millikan (1917, pp. 40, 41) was sufficiently impressed with Thomson's overall argument that he used it in his text to answer the question 'Do all atoms possess similar constituents? In other words, is there a primordial subatom out of which atoms are made?'. His answer came in recalling the magnitude and constancy of e/m recovered by Thomson and Wiechert for cathode rays and Zeeman's 1897 discovery of the same ratio for charges within atoms.

[12] See for example Bain (1998), DiSalle *et al.* (1994), Dorling (1973, 1990, 1995), Gunn (1997), Harper (1990, 1997), Harper and Smith (1995), and Norton (1993, 1994, 1995). For a critical response see Bonk (1997) and Hudson (1997).

[13] Bohr does not adhere to the modern practice of presenting the binding energy W of the electron as a negative number. His energy W is the positive energy released during formation of the atom. Thus lower energy electrons correspond to higher values of W. I will adhere to Bohr's sign convention.

[14] Bohr also alludes to a slightly more general analysis that would give the same result. He asserts that classical electrodynamic analysis of an emitting electron in an elliptical orbit will yield radiation at frequencies $n\omega$, where $n = 1, 2, 3, \ldots$ so that the emitted spectrum has frequencies $\nu = \omega n$. This is returned in his theory by taking the case of an electron with a large quantum number N and considering emissions associated with transitions from state N to state $(N-n)$. For large N and small n, the expressions (12) and (13) are now replaced by analogous expressions

$$\nu = \frac{\pi^2 me^2E^2}{2h^3c^2}\left(\frac{1}{(N-n)^2} - \frac{1}{N^2}\right) \approx \frac{\pi^2 me^2E^2}{2h^3N^3}\cdot\frac{2}{c^2}\cdot n$$

and

$$\nu = \frac{\pi^2 me^2E^2}{2h^3N^3}\cdot\frac{1}{c^3}\cdot n.$$

Comparison of the two expressions yields the same result, $c = 1/2$.

[15] Recall Bohr's sign convention for energy: W is the positive energy released on binding the electron to the nucleus, so the deeper the binding and the smaller the index, the greater the positive energy.

[16] While Bohr's 1913 theory is commonly presented in terms of the quantization of the orbital angular momentum of the electron, I have not cast the demonstrative induction in terms of angular momentum, because the theory's treatment of orbital angular momentum is not entirely satisfactory. Classical analysis shows that the angular momentum l of an electron with energy W is given by $l^2 = me^2E^2(1-\varepsilon^2)/2W$, where ε is the orbit's eccentricity. If we now substitute W with (4), the expression for quantized energy levels, we recover only a partial statement of the quantization of angular momentum: $l = \sqrt{1-\varepsilon^2}(h/2\pi)\tau$. This does not yet give us the quantization of orbital angular momentum into multiples of $h/2\pi$ since nothing yet precludes the eccentricity ε adopting a continuous range of values. The further condition needed to achieve this arose first in Sommerfeld's (1923, Chapter 2) elaboration of Bohr's theory in which he quantized both degrees of freedom of the two dimensional electron orbit, introducing a radial quantum number n_r and an azimuthal quantum number n_φ. Their sum $n = n_r + n_\varphi$ is the principal quantum number and corresponds to the τ of (4). In this scheme, ε is restricted to a discrete set of values by the condition $\sqrt{1-\varepsilon^2} = n_\varphi/n$. Substitution

into the above expression for angular momentum now returns the expected quantization of angular momentum $l=(h/2\pi)n_\varphi$. Since this quantization is expressed in terms of the azimuthal quantum number, Bohr was in no position to recover the result from his emission spectra. In a well known degeneracy in Sommerfeld's theory, the energy of a bound electron depends only on the principal quantum number and is $W=2\pi^2me^2E^2/(h^2(n_r+n_\varphi)^2)$, so that an examination of this energy spectrum (4) alone could not enable Bohr to discern the two quantum numbers that comprise the principal quantum number. Bohr was still able to report the quantization of angular momentum, but only by the artifice of momentarily restricting himself to circular orbits (Bohr 1913, p. 15). In that case the radial quantum number vanishes and the principal and azimuthal quantum numbers become equal.

[17] This is a notoriously difficult problem if we do not embed the inference in a richer framework, such as in a Bayesian analysis (and that introduces further problems). How many instances are needed to give a high degree of certainty in instance confirmation? One or two cyanide fatalities may convince us that large doses of cyanide are always fatal. But one or two dry summers may not convince us that all summers are dry. How much certainty accrues to an hypothesis when it makes a single successful prediction? How much with a second successful prediction?

[18] This condition couples naturally with E: Indexing of stationary states. But I need say little about E. since it adds essentially nothing to the suppositions of A. It functions more as a definition. Its equation (10) supplies the definition of an index τ of these stationary states, without restricting the character of these states.

[19] The qualification, of course, is that the range of τ varies in each case: The Lyman series only delivers the full range $\tau=1,2,3,\ldots$; the Balmer $\tau=2,3,4,\ldots$; the Paschen $\tau=3,4,\ldots$; etc. At the time of writing Bohr (1913), he knew only of the Balmer and Paschen series but anticipated the existence of the others as series (p. 9) 'which are not observed, but the existence of which may be expected.' These two then known series are already sufficient to give the overdetermination under discussion.

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