Rutherford–Bohr atom

J. L. Heilbron
Office for History of Science and Technology, University of California, Berkeley, California 94720
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Bohr used to introduce his attempts to explain clearly the principles of the quantum theory of the atom with an historical sketch, beginning invariably with the nuclear model proposed by Rutherford. That was sound pedagogy but bad history. The Rutherford–Bohr atom stands in the middle of a line of work initiated by J.J. Thomson and concluded by the invention of quantum mechanics. Thompson's program derived its inspiration from the peculiar emphasis on models characteristic of British physics of the 19th century. Rutherford's atom was a late product of the goals and conceptions of Victorian science. Bohr's modifications, although ultimately fatal to Thomson's program, initially gave further impetus to it. In the early 1920s the most promising approach to an adequate theory of the atom appeared to be the literal and detailed elaboration of the classical mechanics of multiply periodic orbits. The approach succeeded, demonstrating in an unexpected way the force of an argument often advanced by Thomson: because a mechanical model is richer in implications than the considerations for which it was advanced, it can suggest new directions of research that may lead to important discoveries.

The development of the Bohr–Rutherford atom is one of a few episodes in the history of 20th-century physics to have received the sustained attention of historians. Since the method and purpose of this historiography will be as unfamiliar as its results, it may be useful to indicate how the objectives of the historian may differ from those of the retrospecting or historicizing physicist. Since the Physical Society has recently determined to establish a unit concerned with history, mention of these objectives may also be timely.

The most common problem for an historian is perhaps also his principal objective: establishing a periodization. Choosing the periods, or punctuating history, is a game with high stakes. The resulting divisions guide or constrain historical reconstructions and, ultimately, the educated person's general conception of self and society. If there is anything in historiography corresponding to a revolution in physics it is the introduction of a new periodization. Much depends, for example, on where we draw the line between the Middle Ages and the Renaissance. Wrangles over the demarcation are not disputes about dates, but about content and nature of periods; and each well-constructed position is informed by, and transmits, a particular view of society, culture, civilization, and human nature.

Because the notion of Scientific Revolution has been closely associated with the traditional concepts of Renaissance and Reformation, the historians' gross periodization has informed, and sometimes distorted, consideration of the origins of modern science. The same conclusion applies to constructs and divisions in modern history: the Enlightenment, the Second Industrial Revolution, the Victorian Era, the Interwar Years, etc. Periodization in history of science need not follow, but it should never ignore, periodization in general history.

With a few conspicuous exceptions, such as the early history of quantum theory and the development of the Rutherford–Bohr atom, the historiography of 20th-century physics has followed the periodization provided by physicists. The result has not been very satisfactory. Historians and retrospecting physicists do not ordinarily work on similar lines. The truth of this proposition is apparent whenever the two groups meet. In a recent conference, for example, contributors to nuclear physics during the 1930s lectured to those who planned to be their historians. Most of the physicists apparently understood the division of labor as follows: they were to supply prime raw material, namely, recollections of the spirit of the times, anecdotes, and benchmarks, while the historians, "who know history and dates and authorships wonderfully," were to tidy up the material, to clear the "underbrush . . . of dates, sources, priorities."[1] This understanding is mistaken on two counts: (1) the historian's business is not pettifoggery and (2) recollections are not the prime sources of history.

(1) The historian's usual product, and the basis of his periodizations, is a rational reconstruction of past events. In this respect his work has a formal analogy to the physicist's, who may be said to aim at a rational reconstruction of nature. There are other similarities as well. Both sorts of reconstructions are judged for their utility in discovering new facts and in finding unexpected connections among old ones; for their faithfulness to accepted rules of evidence; and for their plausibility in the light of prevailing theory and custom. Like physical theories, historical interpretations change in time, sometimes owing to new discoveries, sometimes to new points of view. It is no accident that modern academic physics and modern history entered the world together as alternatives to scholastic methods and curricula.

(2) The historian necessarily has a point of view different from that of the recollecting physicist. As Dirac discovered during a week spent with historians, we take an interest in precisely what the physicist wants (and manages) to forget, "the various intermediate steps and . . . false trails."[2] This preoccupation does not arise from perverse concern with failure. The false trails taken together lead more directly to the historian's goal of reconstructing the past state of science than the retrospectively clear highway of discovery. This state rested not only on ideas, instruments, and institutions formerly recognized and since largely forgotten, but also on interconnections in physics and between it and the

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wider society and culture that participants did not immediately experience and may never have perceived. The historian will not be able to recover the details of everyday life and work, the anecdotes and small adventures, the conversations and meetings, that make up the largest part of the recollections of the participants. But neither is that his business.

Discovering and marshalling evidence for interconnections, especially those not apparent to or hidden from the participants, constitute the particular task of the historian. The success of the enterprise may be judged by, among other things, the appropriateness of the periodization it implies. In the case before us, the early history of the problem of atomic structure, the first big break came not with Rutherford or even with Bohr, both of whom worked in or around the Victorian program established by J. J. Thomson, but in the early twenties, when a new generation, which preferred to redo rather than to patch up received physics, came into its own. The great divide was not Bohr's atom but the first world war.

From one defensible point of view Bohr's theory of 1913 appears not revolutionary but conservative, and even retrograde. This paradoxical notion will be made good after the origin and early development of the Rutherford–Bohr atom are traced.

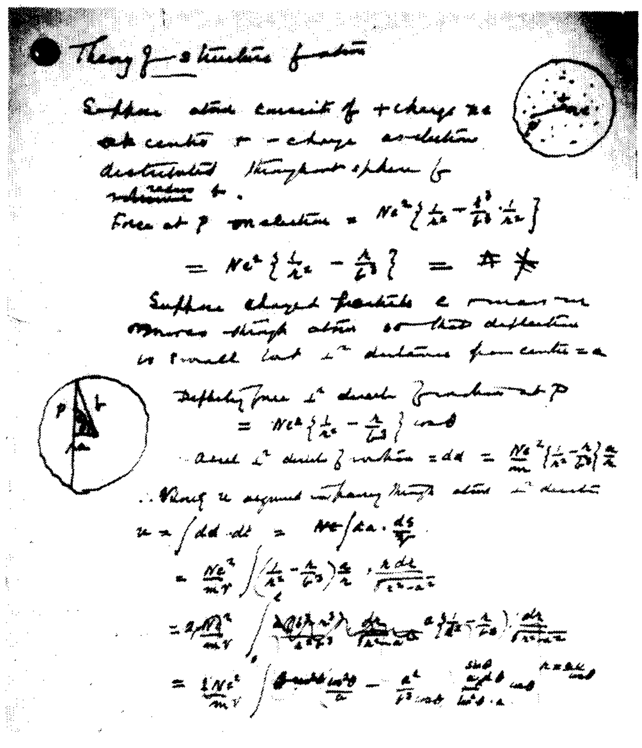


Fig. 1. First page of Rutherford's first manuscript on the nuclear model of the scattering of alpha particles, 1910.

I. THOMSON'S PROGRAM

The work of Bohr and Rutherford was a triumph of the program of Joseph John Thomson, who in 1910 had for 25 years occupied with distinction the most important post in physics in the English-speaking world, the Cavendish Professorship of Experimental Philosophy at Cambridge. Since 1903 he had been working out the properties of a model atom, which was to be dismissed by Rutherford in 1914 as fit only for a museum of scientific curiosities. It could then be dispensed with because by building on it Rutherford had found a more useful substitute. What distinguished Thomson's model from all other early ones was just its capacity for development. It was the first atomic model that lent itself to refinement by calculation and experiment.[3]

One knows that Thomson got around ignorance about the positive constituent of the atom by supposing that the negative electrons circulate in coplanar rings within a sphere that acts as if it were filled uniformly with a resistanceless positive charge. This arrangement has the great advantage over the Saturnian atom—in which the electron rings go outside a central positive nucleus—of mechanical stability. The Saturnian model had suggested itself to the first physicists to attempt to picture an atom containing electrons; but it was dropped after the discovery by one of Thomson's students, G. A. Schott, that it is not stable against small displacements of the electrons in the plane of their orbits.[4] Of course Thomson's atom, like the Saturnian, eventually will collapse from loss of energy by radiation. But, as Thomson showed, the loss in both cases can be made negligible, since electrons equally spaced around a ring absorb one another's radiation. The larger the number of electrons in a ring, the smaller their total radiation.

At first Thomson supposed that the electrons provided all or most of the weight of the atom. Hence their number n in an atom of weight A would be about $1000A$. To check this hypothesis he devised theories of the scattering of X and β rays by the electrons in his model atoms. Experiments done at the Cavendish showed that he had vastly overestimated the population of atoms; n came out about $2A$, not $1000A$. Here was the first great advance in the theory of atomic structure: it not only made the number of theoretical elements something like the number of chemical elements, it also showed that, on any theory, the positive constituent of the atom could not be symmetric with the negative. Obtaining a more accurate relation between n and A became a major goal of Thomson's program. In 1910 J. A. Crowther, using Thomson's latest theory of β scattering, deduced that $n = 3A$. A little later Rutherford found $n = A/2$ by analyzing α scattering. It is noteworthy that that relation, Rutherford's solution to the basic problem in Thomson's theory of atomic structure, stands as the principal result in the paper that we now recognize as announcing the discovery of the nuclear atom.

It is easy to see how closely Rutherford followed Thomson's analysis of the scattering of charged particles. He began with a model that looks identical to Thomson's (Fig. 1). Thinking away the atomic boundary, the limit of Thomson's spherical charge, presented an obstacle to him (Fig. 2); not until the very end of his analysis did he arrive at the elegant treatment of the hyperbolic orbit with which we are familiar. Meanwhile he had been adapting Thomson's mathematics. In his theory of β scattering Thomson had supposed that the deviation of a particle on passing through an atom is the result of many encounters with atomic electrons; and, consequently, that the observed scattering even in the case of a very thin target arises from the integration of many small deviations. This multiple-scattering theory would also have worked for Rutherford's data on α particles were there as many electrons in an atom as Thomson had originally thought. But with n of the same order of magnitude as A, then, as Rutherford learned on

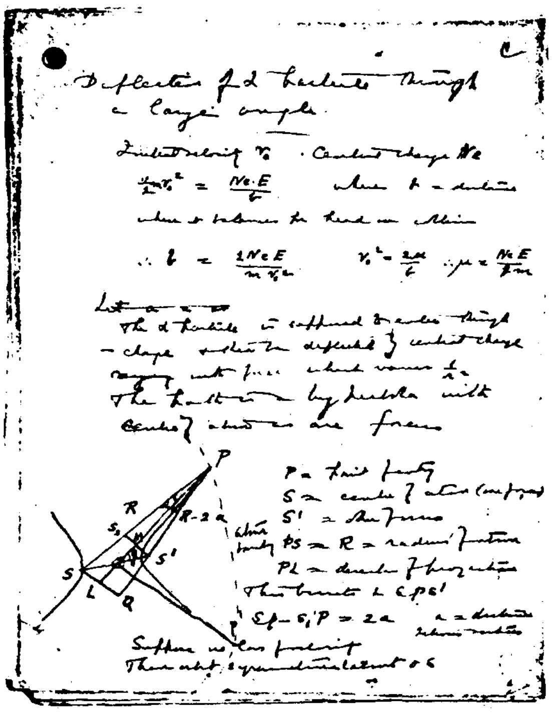


Fig. 2. Page from a later manuscript, also of 1910; note the dashed periphery of the Thomson atom.

repeating Thomson's calculation, diffuse atoms would be too flimsy to turn back fast, heavy α particles with the frequency observed. Were all the electrons assembled together at a point, or, what Rutherford came to prefer, were the positive sphere shrunk to a point, then the concentrated charge could give an α particle the needed kick in a single collision. The theoretical probability of an encounter close enough to give big deflections was in agreement with observation for thin gold and platinum targets if the n of a target atom were taken to be one-half its A.

A second leading line in Thomson's program was to explain the periodic properties of the elements. He made it plausible that periodicity could be a consequence of electromagnetic forces alone by examining the mechanical stability of a ring of electrons against small displacements around their equilibrium orbit. A single ring of 2–7 electrons within a neutralizing positive sphere is stable; the eighth electron brings trouble, and must go to the center to achieve stability. The ninth electron also goes inside, where it and its predecessor form an interior ring of 2. Thomson showed that in general the requirement of mechanical stability implies a unique distribution of electrons into rings for each total of n atomic electrons. And he pointed out strong analogies between the properties of certain model atoms and the chemical behavior of elements in the second and third periods of Mendeleev's table.

A third line concerned the building of molecules, the binding together of model atoms. In the vexed case of a diatomic molecule of an elementary gas like hydrogen or oxygen, Thomson argued that a transfer of charge between the initially identical constituents takes place. His illustration of the process is characteristic of his method. Imagine that at close approach each identical atom may be likened to a sealed flask partially filled with water and suspended by a spring (Fig. 3). The weak electrical interaction may be represented by a siphon connecting the flasks. Now the slightest displacement of one flask relative to the other will cause the siphon to flow, increasing the displacement; and the disparity will increase until the air pressure above the water in the lower flask equals the pressure driving the siphon. The flow of water may be taken as transfer of charge between identical model atoms, and the transfer as chemical binding.

Evidently we deal here with an analogy to an analogy. That was Thomson's practice: to multiply crude pictures in space and time of the physical or chemical process under study. In this way, he said, "we not only gain a very vivid conception of the process, but also often suggestions that the process ... must be connected with other processes, and thus further investigations are promoted."[5] Historians will recognize in this apology the standard rationale for the elaborate mechanical models designed by the Cambridge school from Kelvin and Maxwell through Larmor and Thomson. Unfriendly critics like Pierre Duhem thought it the method of the Victorian machine shop.[6] Its most successful exponent, Maxwell, contrasted its "robust color" with the "paleness and tenuity" of mere mathematical representation.

The Cambridge school did not believe their models to be literally true of nature nor did they endorse mechanical reduction naively. Their fertility of invention provided many representations of the same process; since none was unique, none could be literally true. Moreover, when pushed far enough, any particular model eventually failed. The goal of modelling was not truth but clear ideas suggestive of further connections and discoveries. It went without saying that models pregnant with connections should have as few evident negative analogies as possible, and that they should offer an easy intuition of molecular processes in space and time. Such models may be called "semiliteral" to indicate that, although understood to be less than exact simulacra, they were nonetheless intended as portraits, not as caricatures, of Nature.

II. BOHR'S INNOVATION

Niels Bohr was a great admirer of Thomson. In 1911, after obtaining his Ph.D. in Copenhagen with a thesis on the electron theory of metals, Bohr went to Cambridge to begin a year of postgraduate study. "I considered first of all Cambridge as the center of physics," Bohr later wrote, "and Thomson as a most wonderful man, a genius who had showed the way to everybody."[7] Bohr was then infatuated

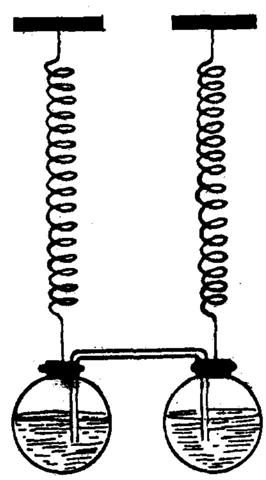


Fig. 3. Thomson's analogy to chemical bonding.

with things English, the novels of Dickens, the romance of the ancient university, and the semiliteral model making of the spiritual descendents of Maxwell. In acknowledgment of the strong British stamp on Bohr's physics, Larmor was so good as to consider him an honorary Cantabridgian.[8]

Bohr went to Cambridge to continue work on the electron theory of metals with the man who had started the subject. But Thomson had gone on to other things and Bohr decided to spend a few months learning the elements of experimentation with radioactivity under Rutherford at Manchester. Rutherford's atom model, then a year old, had not attracted much attention. Bohr became interested when he discovered an error in calculations on it made by C. G. Darwin, the Cambridge mathematical physicist in Rutherford's group. Bohr quickly perceived that Rutherford's main result, $n = A/2$, taken together with the nuclear model, gave a complete answer to the first of Thomson's problems: if the α particle, known then to be doubly ionized helium, had $n = 2$, it was natural to endow hydrogen with one electron, lithium with three, and so on. All problems regarding atomic weight could be swept into the nucleus along with the source of radioactive decay. These ideas—atomic number and isotopy—occurred to several other physicists and chemists about the same time. All but one of them had worked with Rutherford.

Encouraged by success, Bohr tried to adapt the nuclear model to the other principal concerns of Cambridge atom building. He soon discovered the mechanical instability of Saturnian atoms. But whereas earlier this instability had been considered fatal, Bohr now took it as an indication that the nuclear model was a good semiliteral representation of the atom. In his work on the electron theory he had come to believe that some condition not reducible to the principles of ordinary mechanics or electrodynamics was implicated in the stability and definiteness of atomic structure. He expressed the condition provisionally in a form similar to Planck's quantum hypothesis: in the steady or ground state, every atomic electron moves in an orbit determined by the force balance in ordinary mechanics and by the requirement that the ratio of its kinetic energy to its orbital frequency be a universal constant. Any such orbit Bohr declared to be stable against small displacements in its plane and against loss by radiation, which had become a serious problem in the reduced electronic populations of Rutherford's atoms.

Having secured the stability of his atoms via extramechanical fiat, Bohr tried to solve the remaining Thomson problems by exploiting ordinary mechanics. His eagerness to replace Thomson's account of periodicity may perhaps be gauged by an elementary error into which it led him (Fig. 4). In computing the total energy of an electron in a single-ring nuclear atom he counted the potential energy twice, and so reached the result that the total energy depends on the number of electrons in the ring. This result, which conflicts with a later theorem in the same manuscript that correctly makes the total energy the negative of the kinetic, made possible a spurious solution of periodic structure. A new ring must be started outside the first (not inside, as in Thomson's theory) when the total energy per electron becomes positive. Bohr's calculation fixed the maximum population of the innermost ring at seven, exactly the number that Thomson had found and close enough to the length of the second and third periods of the table of elements to be encouraging.

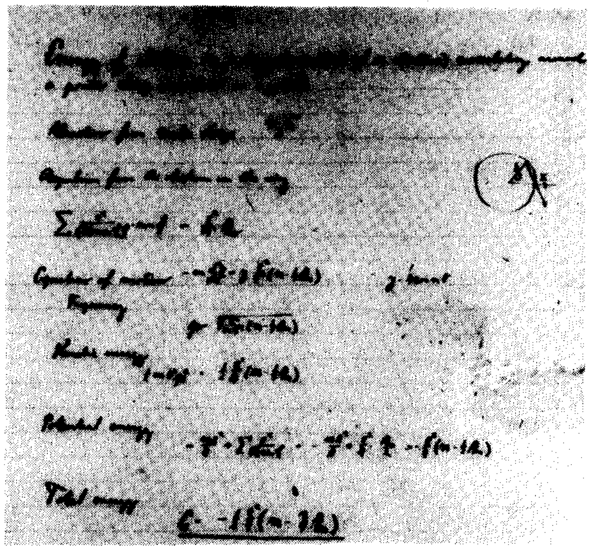


Fig. 4. Bohr's erroneous computation of the total energy of an electron in a single-ring nuclear model, 1912.

After discovering the computational error that gave rise to this spurious solution, Bohr made the ground state of an atom the configuration in which the electrons have the lowest total energy consistent with the fundamental conditions on their orbits. Unfortunately, this prescription did not yield structures reconcilable with known physical and chemical properties. Thomson had never confronted such a situation because he had never specified the electronic population of any particular atom. Bohr, proceeding from the doctrine of atomic number, knew the populations exactly for the lighter elements. His several attempts in 1912 and 1913 to guess at the distributions were based more on his interpretation of the physical and chemical evidence than on deduction from mechanical or any other principles (Fig. 5).

The construction of diatomic molecules of elementary gases also received Bohr's sustained attention. Contrary to Thomson, Bohr believed that the evidence favored nonpolar binding in these cases; instead of a transfer, he supposed a sharing of electrons donated by each constituent. The resultant structures are held together by a girdle of electrons orbiting in a plane perpendicular to the axis joining the two nuclei (Fig. 6). Here again Bohr could be more precise than Thomson. Computing the energies of the various configurations in terms of the undetermined universal constant, Bohr showed that hydrogen atoms should, and helium atoms should not, combine spontaneously into diatomic molecules. The demonstration required the principle from ordinary mechanics that systems left to themselves tend to the lowest available energy state.

These themes of Thomson's program occupy the last two of the three papers on the constitution of atoms and molecules that Bohr published in 1913. The connection of Bohr's atom with Thomson's has been obscured by the circumstance that only the first of the papers, that on the spectrum of hydrogen, is now remembered by physicists. This paper is indeed out of the Cambridge tradition. Thomson had not taken spectra as his guide. Neither had Bohr until after his return to Copenhagen, when the papers of J. W. Nicholson and the questions of a Danish colleague drew his attention to the Balmer formula. Bohr was able to expand his theory

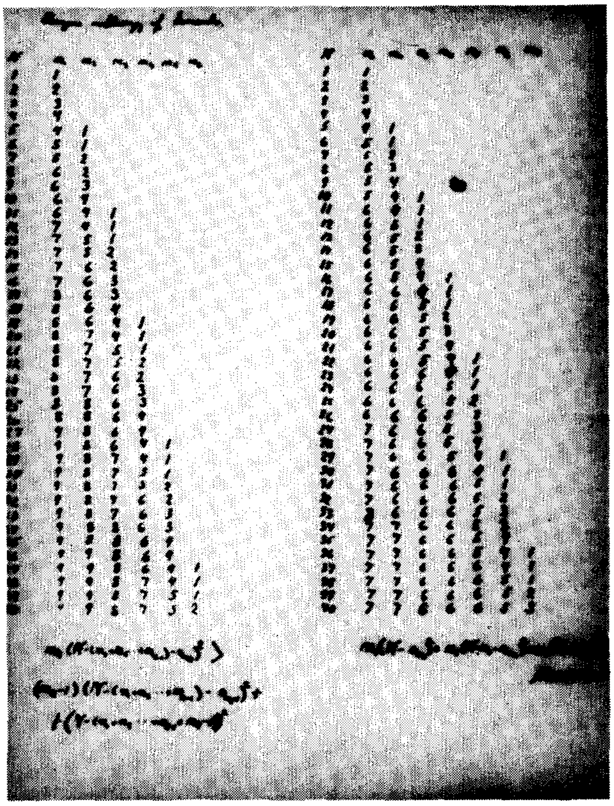


Fig. 5. Bohr's conjectures about the distribution of atomic electrons into concentric rings, 1912/3; N signifies nuclear charge and n_i the number of electrons in the ith ring from the nucleus.

to include higher stationary states, which he also characterized by a Planck-like condition, and to deduce the value of his universal constant from the spectroscopic formulae. The mysterious constant came out to be $h/2$. Bohr hurriedly redid his theory, beginning it with three different methods of introducing Planck's constant and producing the brilliant theory of the hydrogen spectrum that we all know. Then came the parts on atom and molecule building. The order of presentation is the inverse of the order of discovery, and a good example of the obstacles placed by physicists in the paths of their historians.

In the year between the publication of the papers and the outbreak of the first world war, which put an end to ordinary science, physicists did not have time to reach consensus about the Bohr theory. The British, who naturally took it

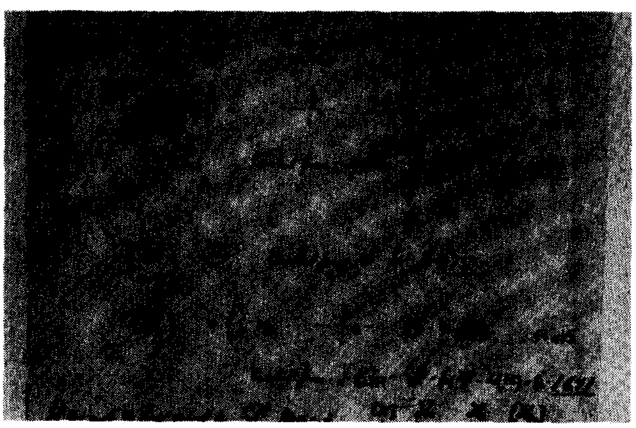


Fig. 6. Bohr's models of covalent bonds, 1912.

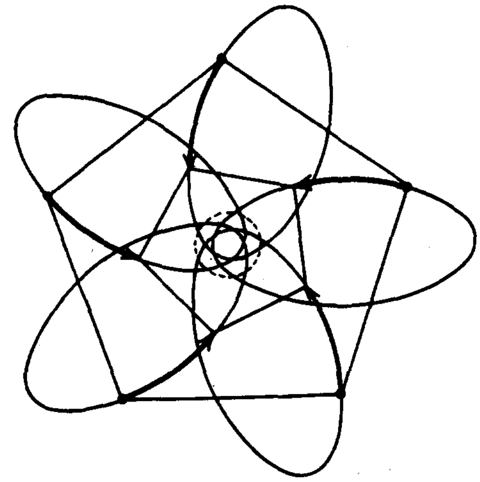


Fig. 7. Sommerfeld's *Ellipsenverein* of coupled electron orbits.

most seriously, in general esteemed it as an advance in the design of semiliteral atoms, although many rejected the *sacrificium intellectus* enjoined by the postulate of unanalyzable quantum jumps. Also the old guard deplored the nonmechanical stability condition, and tried to evade it. Several sought systems capable of vibrations satisfying the Balmer formula, and succeeded, in proportion to the artificiality of their mechanisms.[9] Thomson, whose ingenuity peaked when challenged, designed a model of photoemission that recovered Einstein's law and elucidated Planck's constant in terms of mechanical quantities. It was only necessary to suppose that within certain regions of the atom an electron can be trapped between an inverse-cube repulsion and an inverse-square attraction; and that, when displaced from equilibrium by light containing its resonance frequency, it falls into a region of uncompensated repulsion and is driven into the world as a photoelectron.

In Germany, where one affected skepticism about atomic models,[10] Bohr had the good fortune to arouse the interest of Arnold Sommerfeld, a mathematical mercenary, as it were, whose virtuosity had earlier served novel and conflicting theories of Maxwell, Boltzmann, Lorentz, and Einstein. Owing to his age Sommerfeld escaped military service. With the help of two students detained in Munich as enemy aliens, P. S. Epstein and A. Rubinowicz, he systematized Bohr's cumbersome ideas and extended them to a wide range of spectroscopic data. Taking on as usual the spirit of the theories he developed, Sommerfeld worked out the details of semiliteral models with crossed and elliptical orbits. His famous *Ellipsenverein* of precessing orbits—a system introduced to represent doublet spectra and fine structure—had its inspiration in a model once discussed by Schott (Fig. 7). For the Stark effect, Epstein designed orbits running around in annular tunnels with segments of parabolas as walls.[11]

At the war's end Sommerfeld's *Ellipsenverein* was made three-dimensional by his former student Alfred Landé in two bizarre structures known as *Polyedeverbände*. In one, appropriate for small values of the effective nuclear charge, electrons move in coordinated phases in small quantized circular orbits perpendicular to the diagonals of a cube (Fig. 8). In the other, electrons describe the octants of a cube in such a way that as each particle comes to the end of one of the 90° arcs defining its octant, it meets another, coming from the opposite direction, that bends its path into the next

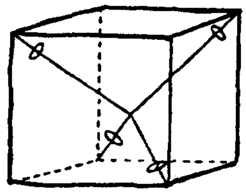


Fig. 8. *Würfelverband* for small values of the effective nuclear charge; the disks represent electron orbits.

90° segment (Fig. 9) The literalness with which Sommerfeld took these pictures appears from a card in which he lectured Landé on the details of the *Ellipsenverein*, the "true atomic music of the spheres" (Fig. 10).[12]

In 1921 Bohr returned to the problem of periodicity using the *Ellipsenverein*, the harmonic interplay stressed by Landé and Sommerfeld, and an appeal to ordinary mechanics that he called the Correspondence Principle. This time he came closer than before; on the basis of these principles, guesswork, and numerology, he predicted that a yet undiscovered element, which chemists sought among the rare earths, in fact was an analog of zirconium. The subsequent discovery of hafnium in Copenhagen in 1922 marked the high water of the tide raised by Thomson, Rutherford, Bohr, and Sommerfield. At about the same time Bohr commissioned a set of commemorative portraits. Figure 11 presents a few, drawn to scale, all paths in place; in the originals red orbits signified odd, black even values of the principal quantum number. To appreciate this atomic music of the spheres, imagine that electrons move along the orbits while the orbits themselves rotate and pulsate in exquisitely tuned harmony. As we know the music soon went sour. Within three years two or three quantum mechanics were invented that solved the problems of atomic physics in terms altogether different from those in which Thomson had posed them a quarter-century earlier.

III. THOMSON'S PROGRAM AND QUANTUM MECHANICS

The year 1922, the year of hafnium and the portraits, might be taken as an epoch in the history of the problem of atomic structure. So too might 1925–6, the birth date of quantum mechanics. Another possibility is that great divide in modern Western history, the first world war. It interrupted the sequence of generations of teacher and student; it shattered the stable world most academics had known; it brought new opportunities and objectives for science; and it broke faith in traditional approaches and solutions. The first generation of fully modern theoretical physicists was that of Heisenberg, Pauli, Dirac, and Fermi, all of whom entered the university just after World War I.

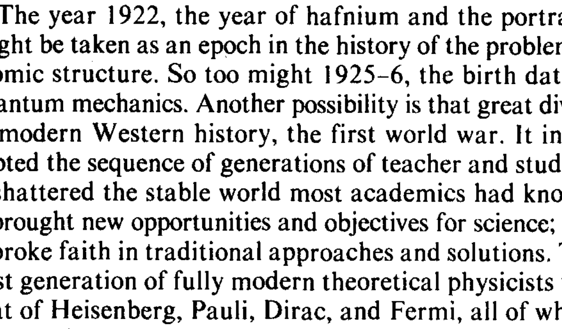


Fig. 9. *Wüfelverband* for large values of the effective nuclear charge; the small diagram shows the orbit of a single electron, the larger gives traces (*ABD, DGC, CFB, BEA*) of four orbits filling a hemisphere.

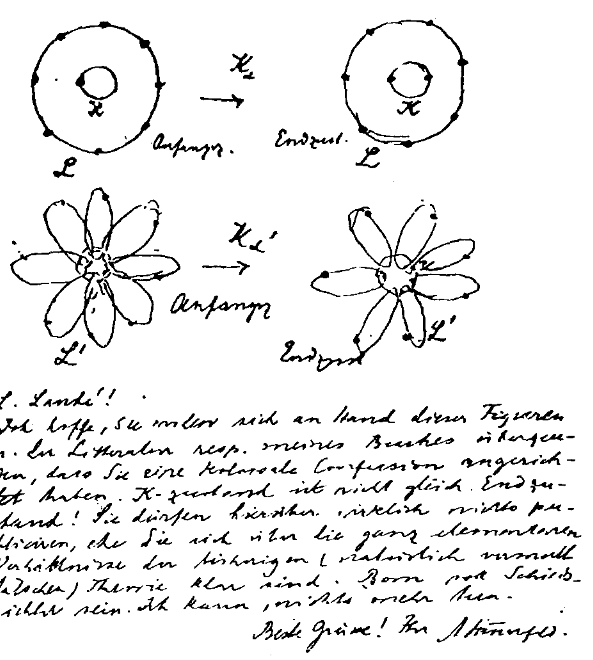
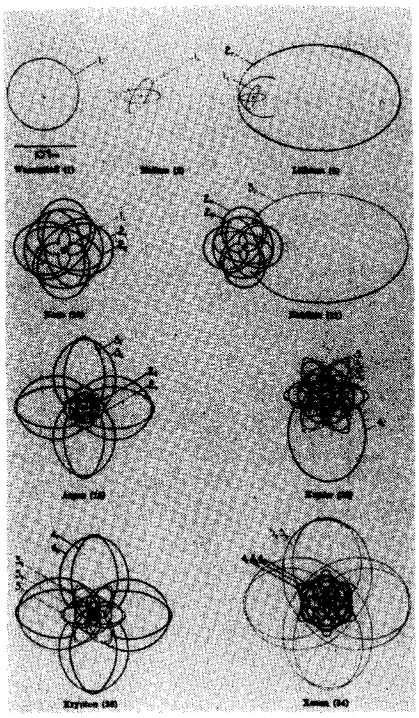


Fig. 10. Description of the transitions giving rise to the x-ray lines K_α and $K_{\alpha'}$.

This new generation had first to master its elders' wisdom about the nature of atoms. Here the war again played a decisive part by preferring the Bohr theory to all others. The conflict freed the theory from its natural competitors: likely designers of alternatives in France, Germany, and Great Britain were mobilized while Bohr and other neutrals, like Ehrenfest in Holland, elaborated the model in peace. The importance of the decision of the nonbelligerent Sommerfeld to cast Bohr's murky procedures into an easily worked algorithm has been mentioned. The generation of 1920 spent two or three years in mastering and trying to extend the Bohr–Sommerfeld theory. When they encountered their first big difficulties they had no scruples or ties to inhibit rejection of the semiclassical approach. That is why the remarkably rapid development of the Bohr theory immediately after the war could so quickly collapse into cries for something novel, for an unprecedented "quantum mechanics."

Only one of the two principal answers to this appeal, matrix mechanics, had roots in the tradition of atom modelling that we have been following. The other answer, wave mechanics, arose in connection with the theory of relativity and the problem of radiation. The initiator of this line of thought, Louis de Broglie, had had an unusual and spotty education in physics during and just after World War I. In the early twenties he decided to resolve the wave–particle engima, brought to the fore by the discovery of the Compton effect in 1922, by associating a light quantum (or material particle) with a travelling wave via an internal clock or vibration associated with the quantum (or particle). Certain relativistic relations yielded equations between the energy and momentum of the quantum (particle) and the frequency and velocity of the wave. Following an intervention by Einstein, Erwin Schrödinger sought a "wave equation" for de Broglie's vibrations in order to study the behavior of particles bound in atoms, where the associated waves would be highly diffracted.[13] In none of this did Bohr or his ideas figure prominently. Perhaps it was just their independence from those ideas, their place at or beyond the fringes of



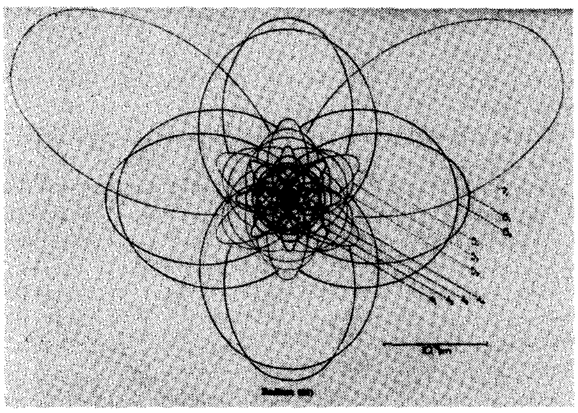


Fig. 11. Bohr's models of (a) various atoms up to xenon and (b) radium.

atomic physics, that enabled de Broglie and Schrödinger to succeed.[14]

Here the paradox mentioned earlier begins to take form. Bohr's ideas not only did not assist in the creation of wave mechanics, but in fact opposed it. For strong reasons Bohr had rejected the concept of light quanta and set aside the evidence in its favor: free radiation fell under the jurisdiction not of quantum physics but of Maxwell's equations.[15] Compton's discovery by no means overcame Bohr's opposition. Rather than admit light quanta, he preferred to give up the conservation of energy and momentum in individual microphysical events. The sketch of a theory of statistical conservation, published under the names of Bohr, H. A. Kramers, and John Slater in 1924, raised little support and much hostility outside and even inside Bohr's immediate circle.[16]

Furthermore (our paradoxer might insist) even the invention of matrix mechanics, although it owed much to Bohr, did not come directly from the Bohr–Sommerfeld theory. Heisenberg's successful response to the cry for a new quantum mechanics started when he gave up fiddling with semiliteral models, or "swindels," as he and Pauli came to call the increasingly artificial products of Bohr's program.[17] Instead, Heinsenberg turned to an old technique then recently revived to treat the phenomena of dispersion.

In the classical theory, anomalous dispersion occurs when light contains a frequency very close to that of the free oscillation of the material particles on which it falls. An elementary assumption about these oscillators almost sufficed: they were supposed to be elastically bound and resisted by a force proportional to their velocities. The qualification arises because on computing the strength of dispersion on the assumption that the oscillator is an electron, physicists found that the number of active dispersion centers is very much less than the number of molecules present. Hence the dispersion theory, which likened the molecule to a set of harmonic oscillators, had a strong negative analogy from the beginning.[18]

The case grew worse in Bohr's theory. Anomalous dispersion occurs at the frequencies radiated by atoms; but these frequencies, according to Bohr, do not relate in a way intelligible from ordinary mechanics to the frequencies present in an atom. On the classical theory it is just the frequencies of the electronic motions in the stationary states that determine the places of anomalous dispersion. Here the Bohr theory suffered from its triumph over the spectrum of hydrogen. Contrary to the classical theory, Bohr's gave values for the free periods of bound electrons, and these periods were not appropriate to the usual computation of dispersion. After this unpleasantness had been definitely recognized about 1916, the problem of dispersion temporarily slipped from the center of attention of physicists cultivating the Bohr atom.[19] It returned in the early twenties following renewed concern about the scattering of radiation stimulated by Compton's discovery. In 1924, frankly admitting the helplessness of Bohr's semiliteral approach, Kramers adapted the old scheme, associating with each stationary state of the atom a set of harmonic oscillators with free frequencies equal to those that could be radiated or absorbed by the atom in that state.[20] These sets of oscillators resemble a collection of differently tuned pianos, not a quantized Bohr atom.

Kramers went beyond mere transcription of the classical theory in two ways. He replaced the problematic number of oscillators by quantum-theoretical probabilities of transitions from the stationary states; and he invented oscillators with negative strengths to represent "negative dispersion," a phenomenon first recognized by Einstein and arising from an emission of energy, beyond what an excited atom would give spontaneously, when it is stimulated by a radiation field.[21] The Bohr–Kramers–Slater theory invoked a feckless field, without energy, which confers a probability of transition upon the atoms it bathes. The "radiation" from the virtual oscillators "conjugated" to atom A in a state a contributes to the probability that atom B in state b suffers a transition, provided that b can be reached immediately from a. Heisenberg worked with Kramers on developing the dispersion theory of virtual oscillators. He then undertook to remove from the theory all vestiges of the classical mechanical entities, such as vibration amplitudes and oscillator frequencies, in favor of relations among quantum-theoretical quantities alone. These quantities turned out to obey the algebra of matrices.

Since Bohr's disciples reached matrix mechanics only after they had abandoned semiliteral models for an updated form of an old approach to dispersion theory, our paradoxer might argue that Bohr's atomic model delayed progress insofar as it directed attention away from dispersion and from the technique of virtual oscillators. There is no doubt that the success of semiliteral models suppressed development of the technique, not only in the standard theory of dispersion but also in the treatment of magneto- and electro-optics proposed by H. A. Lorentz and refined by Woldemar Voigt. In Voigt's approach, the anomalous Zeeman effect can be described and related to other magneto-optical phenomena by assimilating atomic electrons to a set of *coupled* damped harmonic oscillators. Voigt conceded that the scheme was a *pis aller,* since the coupling implied by the mathematics "eluded an intuitive [anschaulich] explanation." Still, as Zeeman observed, want of "lucidity" is scarcely a decisive objection in physics, and there appeared to be no other way to advance. "It is most certainly not without analogy in other fields of physics that . . ., after striking beginnings with special problems, a molecular theory meets insurmountable difficulties with more complex ones while the use of general mechanical and thermodynamic principles can still bring progress."[22] It is notable that Heinsenberg began his career with an attempt to rework Voigt's approach in what he then thought was the spirit of quantum theory. This first effort won the puzzled admiration of Sommerfeld and continuing criticism from Bohr and Pauli.[23]

There were other promising early quantum-theoretical adaptations of the technique of virtual oscillators. Planck's initial theory might be so regarded: he did not take his oscillators to be a (semi-)literal model of an atom, but the simplest possible representation adequate for his single purpose, an analysis of black-body radiation.[24] A later, explicit example is Rubinowicz's theoretical justification of selection rules Sommerfeld had introduced to implement the quantum theory of spectra. Rubinowicz computed the ratio of energy to angular momentum radiated by a classical electron moving in an elliptical orbit—an *Ersatz-electron,* one "not physically real"—and compared it to a similar ratio for the transition of a quantized atom.[25] A final example, Rudolf Ladenburg's reinterpretation of classical dispersion theory, is perhaps the most interesting.[26] Ladenburg published his equations, which amount to Kramers' without the negative term, in 1921. The likening of an excited quantized atom to a monochromatic oscillator then went quite contrary to Bohr's program, and received little attention from Bohr or Sommerfeld until Kramers turned to the problem in 1924.

Whether or not we wish to go as far as our paradoxer, we should not fail to recognize that, for all its novelty, Bohr's semiliteral atomic model was a late product of Victorian physics. Its conservative features and initial triumph may have shunted aside other, more radical approaches to which, in the event, the postwar revolutionaries took recourse. This is not to say that Bohr's atomic model was not a contribution to physics of the first importance: it was the finest of the instruments of Thomson's school both for the exploration of atomic structure and for the discovery that semiliteral models of the classical type will not do in microphysics.

Bohr's doctrine of complementarity, suitably misinterpreted, may be regarded as the last salvage of Thomson's program. For although it should be considered a theory of measurement, it is often utilized as a rule for determining the extent of applicability of one or another set of classical concepts in the microphysical domain. In the latter form it has helped to keep alive such pseudo and pseudoprofound problems as how light (or matter) can be both wave and particle.

To sum up: Periodization is crucial to the historian, as it implies and registers the definition of an era. Recollecting physicists emphasize other matters than the interconnections on which the historian builds his periods. The historian should therefore beware of the physicist's periodization. In particular, Bohr's atomic theory belongs to the program of semiliteral model making initiated by J. J. Thomson and based on the methods of mid-Victorian Cambridge physics; contrary to ordinary physicists' history, the year of publication of Bohr's theory, 1913, does not mark a high point or terminus in a revolutionary era begun with Planck's introduction of the quantum in 1900. During the first world war the theory developed rapidly in the absence of natural competitors. After the war the Bohr atom became the vehicle of revolution in the hands of a few young men who advanced by repudiating the basis of the Thomson–Rutherford–Bohr–Sommerfeld approach. In the history of physics, as in general history, the first world war is the watershed between the 19th and the 20th centuries.[27]

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[2] P. Dirac, in *History of 20th century physics,* edited by C. Weiner (Academic, New York, 1977), p. 109.

[3] For references to the literature and further details see J. Heilbron in Ref. 2, p. 48–108; and Phys. Today **30,** 4 (1977), p. 23–30.

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[5] J. J. Thomson, *The corpuscular theory of matter* (Constable, London, 1907), p. 1–2; cf. Dict. Sci. Bio. **13,** 363–364 (1976).

[6] P. Duhem, *The aim and structure of physical theory,* translated by P. P. Wiener (Princeton University, Princeton, NJ, 1954), pp. 55–104.

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[8] J. Larmor, in *James Clerk Maxwell* (Cambridge University, Cambridge, 1931), p. 76.

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[10] Sommerfeld to Bohr, 4 September 1913, in N. Bohr, *On the constitution of atoms and molecules,* edited by L. Rosenfeld (Munksgaard, Copenhagen, 1963), p. lii; W. Voigt, Ann. Phys. **36,** 897 (1911).

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[13] M. Jammer, *The conceptual development of quantum mechanics* (McGraw-Hill, New York, 1966), pp. 236–270; R. H. Stuewer, *The Compton effect* (Science History Publications, New York, 1975), pp. 217–312.

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[15] Compare Ref. 12, pp. vii, 386; B. R. Wheaton, Ph.D. thesis, Princeton, 1978 (unpublished).

[16] M. Jammer, Ref. 13, pp. 181–188; Heisenberg to Pauli, 8 June 1924, in Wolfgang Pauli, *Wissenschaftlicher Briefwechsel, I: 1919–1929,* edited by A. Hermann *et al.* (Springer, New York, Heidelberg, 1979), p. 154; Pauli to Bohr, 2 October 1924, *ibid.*, pp. 163–166.

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[21] N. Bohr, H. Kramers, and J. Slater, Philos. Mag. **47,** 785–802 (1924);

Einstein, Phys. Z. **18**, 121-128 (1917).
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