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BOOK REVIEW

Theodore Arabatzis: *Representing Electrons: A Biographical Approach to Theoretical Entities*

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This book provides a good example of the significance of philosophy of science for a historical understanding of scientific practice, in the context of representing electrons, an unobservable entity. The book is organized in 9 chapters, references and an index. The List of the chapters provide a brief outline of how the book is organized: *Chapter 1* Methodological preliminaries; *Chapter 2* Why write biographies of theoretical entities? *Chapter 3* Rethinking 'the discovery of the electron'; *Chapter 4* The birth and infancy of the representation of the electron; *Chapter 5* The genesis of the quantum electron; *Chapter 6* Between relativity and correspondence; *Chapter 7* 'How the electrons spend their leisure time'*:* The chemists' perspective; *Chapter 8* Forced to spin by Uhlenbeck and Goudsmit; Chapter 9: Identifying the electron: Meaning variance and the historicity of scientific realism.

Starting from the 1890s up to around the introduction of quantum mechanics in 1925, the author pursues a central theme that revolves around the question: Who discovered the electron? It is generally believed that the electron was discovered in 1897 by J.J. Thomson. Arabatzis questions this claim and based on his biographical approach to representing electrons argues convincingly that no historical episode where controversy persists can be interpreted as constituting a discovery (Chap. 1). Besides Thomson's work on cathode rays, there were many other experimental situations that were interpreted as observable manifestations of the electron, such as: the Zeeman effect, the scattering of α-particles, the discovery of β-rays, the photoelectric effect, the thermionic emission, the cloud chamber tracks and Robert Millikan's oil drop experiments. This long history of experimental work delayed the formation of consensus within the scientific community. Consequently, Arabatzis follows a gradual process of construction of the electron, and at the same time does not endorse the social constructionist position: He believes that there is something in nature to be found that has properties to be identified, not merely agreements to be reached among scientists.

It is important to note that Arabatzis's approach is a biography of the representation of the electron and not of the electron itself (Chap. 2). Again, the role that the 'electron qua

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theoretical entity' played in theory construction, it was immaterial whether its counterpart in nature existed or not. If theoretical entities are constructions from experimental data, then the experimental determination of the charge-to-mass ratio of the electron played a fundamental role starting from Zeeman and Thomson. It determined the identity of the electron and functioned as its 'signature'. According to Arabatzis, several physicists (W. Pauli and W. Heisenberg) and chemists (e.g., G.N. Lewis) argued that a theoretical entity should not contain features that do not have observable counterparts, and consequently, "Their positivistic agenda was the main motivation behind the overthrow of the idea of electronic orbits" (p. 39). Arabatzis even suggests that Pauli was perhaps influenced by E. Mach. In this context it is interesting to recall that Heisenberg and Einstein had a conversation in Berlin in April 1926 on the subject of electronic orbits. At one stage Heisenberg told Einstein that based on his advice he had only included directly observable magnitudes as representatives of electron orbits. Einstein's response took Heisenberg by a surprise as he noted: "Only the theory decides what one can observe" (reproduced in Holton 2000, p. 40; for details see Niaz 2009, Chap. 9).

Arabatzis has argued cogently that belief in the existence of the electron qua subatomic particle presupposed a conviction in the existence of atoms (Chap. 3). Despite the opposition of leading physicists (Mach, Duhem, Ostwald) the atomic hypothesis was finally accepted by the scientific community around 1911. But the opposition also made the acceptance of the electron as a subatomic particle difficult, and consequently for Thomson to be identified as the discoverer. Some of the leading scientists were quite cautious in attributing the discovery of the electron to a particular scientist. E. Rutherford, for example, considered belief in the existence of the electron as the outcome of a gradual process of accumulating evidence, as opposed to an event. O. Richardson (a former student of Thomson) writing in 1914 considered that it was a 15 years process that established the existence of the electron, and not any isolated experiment or measurement. Again, according to some textbook accounts R. Millikan measured the charge on the electron in 1911(cf. Niaz 2000). On the contrary, Holton (1978) has argued that:

> Strictly speaking, his [Millikan's] experiments showed not that the elementary charge of electricity itself had to be atomic, but only (as he was aware) that the transfer of charges to and from small material bodies occurred in integral multiples of *e* [charge on the electron] (p. 184).

Against this background, I agree with Arabatzis that, "… the electron was not 'discovered' but was gradually incorporated in the ontology of physics and chemistry" (p. 63). It is important to recall that Lakatos (1974) was also quite skeptical of the role of crucial experiments and instant rationality in the history of science. For example, starting from 1905 it took almost 25 years for the Michelson-Morley experiment to be understood, considered as the greatest negative experiment in the history of science (Lakatos 1970).

J.J. Thomson's experiments to elucidate the nature of cathode rays (charged particles or waves in the ether?) provide a good illustration of Arabatzis's hypothesis that theoretical entities are, to some extent, constructions from experimental data (Chap. 4). For example in the 1890s there were conflicting pieces of experimental evidence about the nature of cathode rays. In 1883, H. Hertz could not obtain a deflection of the cathode rays electrostatically, which would have supported the particle nature of cathode rays. Thomson's (1897) innovation (in contrast to Hertz) consisted in having worked at "very high exhaustions" (p. 296). Arabatzis brings out this aspect very well (p. 97). For further details on this aspect and its educational implications see Niaz (1998). Despite his contributions, Arabatzis argues that Thomson cannot still be considered as the 'discoverer' of the electron, not because there were others (Larmor, Lorentz, Zeeman, Wiechert, Kaufmann) who made important contributions, but for the reason that the electron was not the product of a sudden discovery.

Notion of a problem situation helped Arabatzis to follow the transition involved in the construction of the electron, from classical to quantum identity (Chapter 5). The "Rutherford Memorandum" (written by Bohr in June–July, 1912) has played a crucial role in the reconstruction of the genesis of Bohr's model of the atom (Heilbron and Kuhn 1969). Actually, Bohr was quite skeptical about the relevance of spectra for his model of the atom. In his interview with Thomas Kuhn in 1962 (cited by Arabatzis), Bohr stated: "The spectra was a very difficult problem … Just as if you have the wing of a butterfly, then certainly it is very regular with the colors and so on, but nobody thought that one could get the basis of biology from the coloring of the wing of a butterfly" (Reproduced in Heilbron and Kuhn 1969, p. 257). Bohr's response that he was aware of the Balmer formula at the time he wrote the "Rutherford Memorandum" is an important facet of how scientists ignore contradictions in their early work. Interestingly, a letter written by Bohr to Rutherford on January 31, 1913, shows that even then he was not fully aware of the implications of spectroscopic research for his problem:

> I do not at all deal with the question of calculation of the frequencies corresponding to the lines in the visible spectrum. I have only tried, on the basis of the simple hypothesis, which I used from the beginning, to discuss the constitution of the atoms and molecules in their 'permanent state (Reproduced in Rosenfeld, 1963, pp. xxxvi–xxxvii).

Another example of such a contradiction was Millikan's claim in his autobiography written in 1950, that he had not rejected Einstein's hypothesis of light quanta to explain the photoelectric effect in 1916 (for details, see Niaz 2009, Chap. 8).

Bohr's theory of the atom in 1913 was based on the simplifying assumption that electrons within the atom move in circular orbits (Chap. 6). Bohr's model, however, could not explain fully even the hydrogen line spectrum (Balmer series). In consequence, Bohr attempted to construct a relativistic version of his 1913 theory, which was later developed by A. Sommerfeld. According to the relativistic variation mass of the electron would not be the same at every point of its orbit, leading to non-uniform velocity and elliptical orbits. It was expected that this 'auxiliary hypothesis' (Lakatos 1970) would double the lines in the hydrogen spectrum. Sommerfeld's relativistic calculations were in agreement with Paschen's measurements of spectral lines due to ionized helium. Despite some improvement this did not resolve all the problems faced by the Bohr-Sommerfeld theory (e.g., the anomalous Zeeman effect).

Arabatzis has developed an interesting thesis with respect to the double life of the electron, "… one in chemistry and one in physics, exhibiting symptoms of a split personality" (p. 175, Chap. 7). The problem situation of the chemists (especially G.N. Lewis) was based on an atom's outermost electrons, which were responsible for chemical combination. Lewis presented his ideas in the form of the cubic atom as early as 1902. However, according to Kohler (1971) when first proposed, Lewis's theory was completely out of tune with established belief, "From the standpoint of the polar theory the idea that two negative electrons could attract each other or that two atoms could share electrons was absurd" (p. 344). Even as late as 1928 the idea of the covalent bond was difficult to accept, "Since according to Coulomb's law two electrons should exert a repulsion for each other, the pairing of electrons seems at first glance to be a bizarre idea" (Rodebush 1928, p. 513). Actually, the polar bond based on Thomson's active support wielded considerable influence (for details, see Niaz 2009, Chap. 10). Arabatzis is correct in pointing out that one of the sources of resistance to Lewis's ideas (based on his problem situation) by the physicists was its classical picture of the electron. I found Arabatzis's suggestion that the origin of Lewis's conception of the shared bond, "… is a question that has not been definitely answered" (p. 183), somewhat intriguing.

In order to solve the difficulties faced by the Bohr-Sommerfeld theory, W. Heisenberg introduced the novel idea of half-integral quantum numbers (Chap. 8). The original idea of a spinning electron by R. Kronig and its later development by Goudsmit and Uhlenbeck were important contributions and make interesting reading. Unified treatment of the hydrogen and alkali spectra (went beyond Sommerfeld) was an important consequence of this innovation. The proposal of the electron spin as an extra degree of freedom facilitated half-integral quantum numbers and a further enrichment of the representation of the electron.

Did Stoney, Thomson, Larmor, Lorentz, Bohr and Pauli refer to the same entity when they used the term 'electron'? According to Arabatzis an answer to this question has important implications not only for his biographical approach but also B.C. van Fraassen's criticism of the realist position (Chap. 9). I. Hacking's realist position can be summarized as: If you spray electrons then they are real. Van Fraassen countered by saying: If they are real then you spray them. Hacking's emphasis on manipulability has been the subject of criticism as scientists use a variety of evaluative criteria. For example, Thomson could manipulate cathode rays in 1897 and still there was no consensus in the scientific community with respect to the nature of cathode rays. Arabatzis sustains that the realist position can be upheld if a core of meaning about the electron has proved relatively immune to theory change. One such property of the electron was the charge-to-mass ratio (electron's signature, cf. Chap. 2), which remained constant from about 1896–1925.

This position, however, has a caveat, stated cogently by Arabatzis:

> Even if a core of beliefs about electrons has not been affected by theory change, one cannot exclude the possibility of an alternative, empirically adequate theory that would not include electrons in its ontology. One could risk the prediction, however, extrapolating from the historical development of science, that several, if not all, of those properties would be ascribed to the new entities postulated by that, radically novel, theory (p. 257).

In my opinion, this is one of the most important issues discussed in this book. One of the examples provided by Arabatzis of such a change in entity was the ether in Lorentz's theory of electrons, when it was replaced by Einstein's special theory of relativity. Interestingly, some of ether's causal properties were taken over by the electromagnetic field. Another example, again related to the electron, was the Millikan-Ehrenhaft controversy in which the same properties found in oil/metal drop experiments were attributed by Ehrenhaft to a new entity … fractional charges or sub-electrons (cf. Holton 1978; Niaz 2005).

Finally, I would agree with Arabatzis that the electron was not discovered in 1897 by Thomson, but rather its representation was enriched through the collective efforts of various scientists over many years until a consensus was achieved in the scientific community. This book is an extraordinary source of historical and philosophical ideas that can be of interest to historians, philosophers of science, science educators and graduate students. In my opinion, some of the ideas espoused in this book would enmesh with Giere's (2006) *perspectivism*, Cobern and Loving's (2008) *epistemological realism*, as science is imperfect, incomplete, tentative and fallible. Tentative nature of scientific knowledge in the context of the electron's representation is well expressed by Putnam (1975) (cited by Arabatzis):

Bohr would have been referring to electrons when he used the word ‘electron,’ notwithstanding the fact that some of his beliefs about electrons were mistaken, and we are referring to those same particles notwithstanding the fact that some of our beliefs—even beliefs included in our scientific ‘definition’ of the term ‘electron’—may very likely turn out to be equally mistaken” (p. 197).

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