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Atomism and Energetics at the End of the 19th Century: The Luebeck Meeting of 1895

Antonio A. P. Videira

Centro Brasileiro de Pesquisas Físicas - CBPF/CNPq
Rua Dr. Xavier Sigaud, 150
22290-180 - Rio de Janeiro, RJ - Brasil

Departamento de Astrofisica
Observatório Nacional - CNPq
Rua General José Cristino, nº 77, São Cristovão,
20921-400 - Rio de Janeiro-RJ-Brazil
e-mail: Guto@Kepler.ON.BR

Abstract : In the last quarter of the 19th century, atomism and energetics were the main epistemological lines of thought that disputed the preference of the German scientific community. Our paper presents and discuss the most important aspects of these lines of thought as they were understood by their principal thinkers : Boltzmann and Ostwald. We also compare these two epistemological trends. It becomes clear that, in the historical period under consideration, philosophy has contributed to explain the deep questions which affected the scientific domain of those days.

Resumo : Durante o último quartel do século 19, o atomismo e o energetismo eram as mais importantes linhas de pensamento epistemológico que disputavam a preferência da comunidade científica alemã. O presente artigo apresenta e discute os aspectos mais relevantes de ambas as linhas de pensamento, tal como elas eram compreendidas pelos seus mais significativos formuladores : Boltzmann e Ostwald. Nós também as comparamos entre si, tornando-se claro que, no período que está sendo por ora examinado, a filosofia contribuiu para explicar as questões fundamentais que afetavam o domínio científico daquele tempo.

I-Introduction

The final years of the 19th century saw the birth of new discoveries in science - specially in mathematics and in physics - that radically altered the hitherto dominating paradigms of scientific thinking. In mathematics, the deep queries of Cantor and of Dedekind addressing the central question of the continuum, of infinite sets and of transfinite numbers led to a reformulation of the axiomatic foundations, as well as of the logical structure of the whole body of mathematics by Fregge, by Hilbert, and by Russell. In physics, the entirely new problems brought for by the findings of Hertz and of Roentgen (in the eighties) concerning the electromagnetic radiation, the related questions of black body radiation and of the spectra of chemical elements, together with the first evidence of the atomistic constitution of matter (with the discovery of the electron by Thomson in 1897), constituted inequivocal indications of the unavoidability of the introduction of radical changes and revolutionary innovations in the description of natural phenomena.

The unfolding of new realms, not directly accessible to sensorial experience, such as transfinite numbers, infinite sets, parallel logics, X-rays, electrons and the like, introduced a deep and severe strain in to the increasingly interlocked worlds of mathematics and physics. What directly concerns us here, however, is the rebirth of several critical movements - issued from and prompted by the new needs posed by all these disturbing and exciting scientific findings, and as relevant as they were - which, surpassing the strict objectivies and goals of science, reached into the domains of fundamental philosophical inquiry. It was understood then that questions like :

"what are the roles played in science by the set of admitted hypothesis (postulates or axioms)?"

or "what is the role of the accepted axiomatic basis in the determination of the relevant experiments to perform?", it was then perceived, transcended the strict domains of science, pertaining instead to the field of philosophical/epistemological inquiry.By itself, this explains and justifies why some of the most creative minds of the time resolutely committed themselves to the analysis of the interrelationship between the different new scientific issues which had come forth

in the last quarter of the nineteeth century; which analyses inevitably led to the unchartered areas of philosophical thought and epistemological questioning. Maxwell in England, Helmholtz, Hertz, Mach and Boltzmann in Germany, and Poincaré in France (1), just to mention some of the most representative members of the scientific community, proved to be remarkably adept to the kind of reasoning best characterized as epistemological rather than as purely scientific. All of them unquestionably placed their most profound thinking at the very frontier between the requirements, the purposes and the results of physics, and the general questions and the basic tenets of philosophy.

Actually, the most eminent European physicists of the second half of the last century should not be understood as scientists in the modern sense - in their majority quite estranged from philosophical enquires

- but rather as scientists-philosophers, whose work can only be properly grasped as jointly encompassing the physics and the epistemology involved. Notwithstanding this, even if they were convinced of the need of discussing the epistemological implications and constraints of [their] science, since, primarily, they were professional scientists, their philosophical interventions were explicitly addressed to an audience of other scientists like themselves : the epistemological issues raised by them were meant to be exclusively discussed inside the safe and reasonable walls of a restricted forum, wherein only *bona fide* accepted and well established members were admitted. Scientists are rarely at ease in their interactions with professional philosophers, on one hand, feeling, more often than not, unconfortable with both the specialized reasoning and the specialized language employed by the latter, on the other, besides never being entirely capable of putting aside their ever present doubts about the real grasp of scientific issues by outsiders (i.e., all those, including scientists, who do not realy belong to the restricted circle accessible to just a few chosen ones.). Accordingly, philosophers and historians of science - including even those who had acquired professional scientific training - were, and still are, left to discuss, often nearly the same questions, among themselves. This, in spite of the well established influence of philosophers on the gestation of scientific theories, and of the reciprocal well recognized influence of scientists over the unfolding of philosophical ideas (2).

II- Scientific theories as representations of the world

The final years of the last century brought to science an unsetting lack of certainty of what should be the ultimate true content [and objectives] of a scientific theory. It was understood then
- and forefully expressed by the main core of the physicists' community of the time, in personal gatherings at scientific meetings and through exchanges in professional journals, - that the fast changing needs, roles and language of the theoretical explanations conjoined the need to revise what ought to be the essential criterions in deciding whether a given scientific theory should be considered as good or not. There were, *grosso modo*, two major epistemological conceptions competing for approval inside the scientific community : one asking whether a physical theory ought to describe only what is observed to occur in nature; the other, contrarily to this, demanding whether a physical theory should try to formulate true explanations of natural phenomena, e.g., as affirmed by the correspondence theory of truth.

One of the most important presumptions of the first line of thought concerned the (scientific and epistemological) impossibility of attaining the ontological level responsible for the
world as it effectively is. In other words : is phenomenology - as defined by Mach(3), Kirchhoff(4) and Duhem(5) - the true ideal of all scientific theories?

Concerning the second point of view, Maxwell first and then Hertz and Boltzmann, answered by the affirmative, since they did not conceive to be possible that any physical (or, for that matter, any scientific) theory could attain the ontological level of nature. No physical theory, no matter how well formulated, could decisively and definetely decide the true essence or the ultimate constituents of physical reality, being utterly impossible, therefore, to make any statement as to why physical reality is as we perceive it to be. For the three of them (as they often repeated), a scientific theory is no more than a representation of a given set of natural phenomena : **analogy** for Maxwell, **Bild**, **Vorstellung** or **Darstellung** for Boltzmann and Hertz. We encounter here an idea already forward by Kant in his epistemological writings, but, which, apparently, as Boltzmann himself repeatedly emphasized in several occasions(6), was either unknown to, or had been forgotten by, the scientific community. According to him, it was due to Maxwell's efforts - during the time he was developing his electromagnetic theory - that this representational aspect of all scientific theories was brought to the attention of the physicists community.

The epistemological differences between the phenomenological and representational modes of doing science - neither of which can attain the ontological level of the world - will become apparent latter on through Maxwell's and Boltzmann's positions on these issues. Actually, Helmholtz had already stressed(7) that, for him, a model - a term designating a conceptual structure which has not yet attained the level of a full-bodied theory - should be considered as merely a temporary and not fully adequate ersatz to a complete and permanent theory. And this, the establishment of well-formulated, complete and true, that is, permanent theories, ought, ideally, to be the ultimate goal of physics and of science in general.

For Maxwell, a true theory of electromagnetism should be framed over a mechanical, or, as he preferred to say, over a dynamical foundation. In his work, Maxwell determinedly employed the Lagrangian formalism, based on a state function defined over the formal n-dimensional space of configurations (the number n of dimensions varying from system to system). This formulation did not allow, however, the establishment of a direct correspondance between the electromagnetic theory built upon this "abstract" framework and the "real" mechanical constituents of the world. Of course, neither Maxwell nor anyone else since him has been able to formulate a description of the electromagnetic phenomena on a strictly mechanical basis.

The same belief on the fruitefulness of an analytical mechanics description of physics was held by Boltzmann, who, following Maxwell's own work on the subject, based his epochal work on the kinetic theory of gases on a statistical mechanics basis (known since then as the Maxwell-Boltzmann statistics). Like Maxwell, the Viennese master also never abandoned his deep conviction that the analytical formulation of mechanics - either lagragian or on the hamiltonian formulation (based on the so-called phase space, with twice the number of dimensions of the configuration space) - should be the basis of all good physical theories. That this is so is abundantly demonstrated, according to Boltzmann, by analytical mechanics own history as a fertile contibution to other areas of physics (Boltzmann gave the examples of acoustics and optics). That is, analytical mechanics was, indeed, a fruitful theory. How fruitful it would prove to be for the development of physics during our century he never found out, however.

Boltzmann died a little over a year after a young bureaucrat in Berne had produced single-handed an eventful revolution with the closely spaced publication, in the spring of 1905, of a series of epochal papers, which, besides introducing the special theory of relativity (based on Maxwell's electromagnetic theory) and presenting the quantum nature of light (related with the photoelectric effect), brought forth the atomistic basis of nature (related with the Brownian movement). Had he lived a few years more and Boltzmann would have seen his atomistic convictions entirely vindicated ; and, twenty years after his death, Schroedinger, a fellow Viennese (who had seen his expectations to begin his initiation in theoretical physics with the great thinker be thoroughly frustrated with his tragic disappearence) came forward with his Wellenmechanik (Wave Mechanics), based on a (entirely novel) differential equation that inequivocally took the Hamiltonian formalism fully to the forefront of the descrition of the quantum world. Still more impressive and far reaching was the development, during the thirties and forties, of a relativistic formulation of a quantum field theory that came as a fittingly crowning of Boltzmann's belief on the adequacy of the Lagrangian and Hamiltonian languages to picture the most intimate aspects of the world of physical phenomena.

III- The bitter meeting of 1895 at Luebeck

The meeting of the German Scientific Association for the year 1895, was held at the northern town of Luebeck. Two conferences were given there, one by Wilhelm Ostwald of Leipzig, the acknowledged leader of physical chemistry, and the other by Georg Helm of the Technische Hochschule of Dresden, on energetics, a seemingly promising new theory, influenced by the positivist ideas championed by Mach and which foresaw the possibility of opening up new ways of thought referring to the physical description of the world.

Ostwald presented a version of energetics most in vogue at the time, and mainly of his own making, that considered energy and not matter (the latter made up or not of atoms in movement) as the most fundamental concept in the whole of physical science. Every physical theory, in order to be regarded as scientifically consistent and true, ought to be founded on the concept of energy, the only real (in a ontological sense) entity of all natural phenomena (which would involve, therefore, nothing more than mere energy transformations). This configured a grievous mistake for phenomenologists, as Ostwald liked to label himself.

Ostwald chose for his conference the provocative title 'Die Ueberwindung des wissenschaftlichen Materialismus' ('The Overcome of Scientific Materialism', this translation is mine)(8), which held as its core idea and main thesis that the mechanistic description of nature ought to be completely abandoned, there being altogether no more place or use for it altogether, either in science or in epistemology. Indeed, as the science of (pure) thermodynamics had made clear once and for all, mechanistic ideas, based on atomistic principles, had entirely ceased to be of any use ; in fact, had ceased to have any meaning whatsoever.

On the other hand, Helm, who was then professor of physics at the Polytechnic Institute of Dresden, and who also defended in his own conference that the mechanicist *Weltanschauung* ought to be replaced by the energitist picture of natural phenomena, had reasons enough to come out of the Luebeck meeting feeling quite unsatisfied. So much so that on the same evening of his talk, feeling somewhat dejected, he wrote to his wife, bitterly expressing his unacceptance of the very negative reception accorded to Ostwald's and his own proposals concerning the ideas of energetics(9).

The immediate reason for this was the heated debate which immediately ensured his ill-fated presentation. The main opponent present there, the "disturber" who saw to it that the obtention of a final consensus on the matter of energetics was made impossible was, of course, Boltzmann. As soon as Helm had finished his lecture, Boltzmann intervened arguing to the effect that even if he could agree that it was indeed relevant to uphold interpretations of physical phenomena differing from the one founded on atomistic principles, the energetist programme (or approach) was, in fact, irrealizable and indeed epistemologically false. According to Boltzmann, seconded by the mathematician Felix Klein, both Ostwald and Helm had not understood that physical theories do not contain any ontological meaning whatever ; or, equivalently, that physical theories, in order to be epistemologically consistent and scientifically feasible could not make statements concerning the meaning of physical reality. As reported by Arnold Sommerfeld, the confrotation between Boltzmann and Ostwald (a personal friend of his) "equaled outwardly and inwardly the struggle of the bull with the supple matador. But this time the bull conquered the matador despite all his finesse. The arguments of Boltzmann drove trough. All the young mathematicians stood on his side."(10).

Throughout his argumentation in favour of atomism, and repeated later on the pages of the Annalen der Physik und Chemie (11), Boltzmann's intention was to demonstrate that, in physics, atomism was to have more credit than energetics, because it explicitly showed that some concepts (as, in particular, the concept of atom) have their origin required by the mathematical methods employed in physics, in particular differential equations(12). Moreover, the atomistic ideal reinforced Boltzmann's belief of a scientific theory as a Darstellung (representation) of Nature, reflecting our own way of dealing with the world. Atomic ideas, successfully used by chemistry and physics alike for so long, and so, of inequivocal historical importance on their own right, were ideally suited, according to him, to prove that all scientific entities were free creations of the human mind. His commited and almost passionate defense went on to stress that science should wary of abandoning such a fruitful idea, of well established historical significance lest dogmatism be installed in science. This, Boltzmann severely admonished, could, for always, undermine scientific progress.

IV-Conclusion

The Luebeck meeting of 1895 explicitly reflected the state of physics, of its unanswered deep queries, not only in its choice of paramount scientific issues, but also of how and by whom were these issues defended and attacked. The main debate was focused on the historical question of the still elusive physical reality of discrete atoms against a continuum description of nature, based on the energy concept. Right at the moment when the unmistakable signs of the most sweeping and radical revolution in physical science were beginning to take form - with the discovery by Wilhelm Roentgen, that same year of 1895, of X-rays ; with the first evidence of the fundamental discretness of nature with the discovery, two years later, by J.J. Thomson, of the electron, with the still unsolved riddle of black-body radiation which, at the very dawn of the new century, finally pushed reluctant Max Planck over the brink - some of the most representative physicists of the German speaking world gathered to discuss what they agreed to consider as one of the still unsolved open questions of their science.

But in 1895, at Luebeck, it remained frustratingly clear that nothing could be definitely cleared : as tantalizingly close the solutions could seem to be, time was not quite ripe out. Forceful, compelling and even high-strung debates were held over central physical matters with quite a good portion of the stress being laid on epistemological arguments, rather than on the still unavailable empirical evidence. Accordingly, the physical theories championed at Luebeck owed as much to philosophical preferences as to scientific requirements. This was certainly the case of both Boltzmann and Ostwald.

V- Notes

(1) a) Ludwig Boltzmann 1905 *Populaere Schriften* . J. A. Barth, Leipzig

L. Boltzmann 1990 *Vorlesungen ueber Naturfilosofi*. Springer Verlag, Heidelberg/ New York

b) Pierre Duhem 1892 Quelques Réflexions au Sujet des Théories Physiques *Révue Générale des Sciences* **XXXI** : 139-177

P. Duhem 1988 *La Théorie Physique: Son Objet et Sa Structure* .Vrin, Paris

c) Georg Helm 1898 *Die Energetik nach ihrer Geschichtlichen Entwicklung* . Veit and Co., Leipzig

G, Helm 1896 Zur Energetik *Annalen der Physik* **57**: 646-659 .

d) Hermann von Helmholtz 1984 Einleitung zu den Vorlesungen ueber theoretische Physik, *In Zur Grundlegung der theoretischen Physik*, R. Rompe, H.-J. Treder, eds. Akademie Verlag, Berlin

e) Heinrich Hertz 1984 Einleitung zur Mechanik, *In Zur Grundlegung der theoretischen Physik*, R. Rompe, H.-J. Treder eds. Akademie Verlag, Berlin

f) Ernst Mach 1900 *Die Principien der Waemelehre historisch-naturwissenschaftlichen entwickelt* . J. A. Barth, Leipzig
E. Mach 1910 Die Leitgedanken meiner naturwissenschaftlichen Erkentnislehre und ihre Aufnahme durch die Zeitgenossen *Physialische Zeitschrift* **XI** : 599-606

g) Wilhelm Ostwald 1904 Die Ueberwindung des wissenschaftlichen Materialismus, pp.220-240 *In Abhandlungen und Vortraege, Allgemeines Inhaltes (1887-1903)* . Veit and Co., Leipzig.
W. Ostwald 1896 Zur Energetik *Annalen der Physik* **58** : 154-167

h) Henri Poincaré 1968 *La Science et l'Hypothèse* . Flammarion, Paris
H. Poincaré 1909 *La Valeur de la Science* . Flammarion, Paris

(2) Paradigmatic, in our century, are the well documented cases of the influence of Spinoza, Hume and Mach on Einstein, and of Kirkegaard and Høffdingon on Bohr.

(3) See note 1.

(4) Gustav Kirchhoff 1876 *Vorlesungen ueber mathematische Physik I: Mechanik* Teubner, Leipzig

(5) See note 1.

(6) See Ludwig Boltzmann 1905 Ueber die Entwicklung der Methoden der theoretischen Physik in neuerer Zeit, p198-227 *In Populaere Schriften* J.A. Barth, Leipzig

(7) See note 1.

(8) See note 1.

(9) See Martin Curd 1978 *Ludwig Boltzmann's Philosophy of Science:Theories, Pictures and Analogies*. PhD thesis University of Pittsburgh, p.175.

(10) See Walter Moore 1989 *Schroedinger,Life and Thought* . Cambridge University Press, Cambridge, p.38

(11) See Ludwig Boltzmann 1905 Ueber die Unentbehrlichkeit des Atomismus in den Naturwissenschaften, p.141-157 *In Populaere Schriften* . J.A. Barth, Leipzig and *Annalen der Physik und Chemie* **58** (1896), p.595

(12) See Antonio A. P. Videira 1992 *Atomisme Epistémologique et Pluralisme Théorique dans la Pensée de Boltzmann.* PhD thesis Equipe Rehseis-Université Paris VII, p.55-94