Widening the boundaries of classical physics:
from Einstein's 1909 paper back to late nineteenth-century theoretical physics

Stefano Bordoni
University of Urbino
stefano.bordoni@gmail.com

1. From the early XX century back to the late XIX century

In the paper Einstein published in the *Physiklische Zeitschrift* in 1909, he re-shaped the hypotheses which he had put forward in 1905 and re-stated in 1906. He focussed on the problematic link between electromagnetism and thermodynamics, in particular on the status of Maxwell's equations, the irreversibility of electromagnetic processes, and the reconstruction of «the law deduced by Jeans». He pointed out the theoretical mismatch between «current theoretical views», which inevitably led «to the law propounded by Mr. Jeans», and known «facts». Then he analysed Planck's theory, and he found that it was founded on classical assumptions stemming from Electromagnetism and Thermodynamics: it led to new results by means of ingenious contrivances. He claimed he would have clarified the assumptions from which Planck's result could be consistently derived, because «Planck's presentation of his own theory suffers from a certain logical imperfection»[1].

The search for a *physical* «definition of the probability *W* of a state» led Einstein to reverse the mathematical procedure which had led Boltzmann and Planck to the computation of *S* starting from the computation of *W*. Einstein suggested starting from the empirical expression for the entropy to derive the expression for *W*: the latter would correspond to a probability

[1] Einstein (1909, engl. trans. 357-61).

Stefano Bordoni, "Widening the boundaries of classical physics: from Einstein's 1909 paper back to late nineteenth-century theoretical physics", in Isabella Tassani (a cura di), *Oltre la fisica normale. Interpretazioni alternative e teorie non standard nella fisica moderna*, pp. 25-52.

endowed with physical meaning. Einstein reminded the readers that, in 1905 he had followed the same logical path. If in 1905 he had started from Wien's law of radiation, in 1909 he decided to start from Planck's law. He concentrated on the relationship between the fluctuation of energy and the fluctuation of probability, and then computed «the fluctuations of radiation pressure, due to fluctuations of the momentum». In the end, two different terms of different nature emerged from the computation of the fluctuations of electromagnetic energy and momentum. If the second term was consistent with the wave-like behaviour of electromagnetic radiation, the first term had a corpuscular nature, and microscopic amounts of energy $h\nu$ were at stake. He claimed that «the constitution of radiation must be different from what we currently believe», and that the new representation led to a more natural interpretation of Planck's law[2].

In the last part of his paper, Einstein faced some dimensional relationships, and discussed the numerical relationship which linked «the light quantum constant h to the elementary quantum ε of electricity». He reminded the reader that even «the elementary quantum ε» was «an outsider in Maxwell-Lorentz's electrodynamics». In particular, he remarked that «[o]utside forces must be enlisted in order to construct the electron in the theory». Einstein hinted at a unified theory wherein the physical constants h and ε could spontaneously emerge in the same way that electrodynamics and electromagnetic radiation had spontaneously emerged from Maxwell's theory[3].

In some way Einstein pointed out a structural analogy between the emergence of the discrete nature of radiation from the background of a classical continuous representation, on the one hand, and the emergence of the discrete nature of electricity from the background of Maxwell's continuous representation, on the other.

In a paper published in 1910 in the *Annalen der Physik*, Planck associated Einstein and Stark to J.J. Thomson and Larmor. He noted that the four physicists had put forward an extremely radical interpretation of electromagnetic radiation: even in the case of «electromagnetic processes in pure vacuum», they had imagined «*diskreten Quanten*» or «*Lichtquanten*». Although Planck did not explicitly quote from it, in 1909 Larmor had published a paper (in the *Proceedings of the Royal Society*) devoted to the

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[2] Einstein (1909, engl. trans. 363-9).

[3] *Ibid*., 372-4. See, in particular, p. 374: «The relation $h = \varepsilon^2 / c$ seems to me to indicate that the same modification of the theory that will contain the elementary quantum ε as a consequence will also contain the quantum structure of radiation as a consequence».

statistical interpretation of electromagnetic radiation. According to Larmor, a «ray», or «filament of light», could be looked upon as «a statistical aggregate»: the statistical «constitution of the ray» mirrored the statistical distribution of energy «in the radiant element of mass». The «general thesis» he developed was a «molecular statistics of distribution of energy», which gave birth to a re-derivation of «Planck's formula for natural radiation»[4].

In his 1910 review, Planck faced the general query concerning continuity and discontinuity, both for matter and energy, but in the end, he found that every «*Korpuskulartheorie*» appeared weak and unreliable to people «relying on the electromagnetic nature of light». He thought that a radical assumption of discontinuity in the structure of light would have led physics back to the old debates taking place in the eighteenth century. Could a physicist put in danger the fruitful alliance between the wave theory of light and Maxwell's electromagnetic theory, for the sake of a questionable hypothesis? Although he acknowledged the existence of some connection between his own view and J.J. Thomson, Larmor and Einstein's views, for the time being, Planck restated his trust in «Maxwell-Hertz's equations for empty space, which excluded the existence of energy quanta in vacuum»[5].

Planck had already made similar remarks the year before, in the lectures he had held at Columbia University. In particular, in the sixth lecture, «Heat radiation. Statistical theory», he had claimed that «the most radical view» had put forward by J.J. Thomson, Larmor, Einstein, and Stark, who thought that «the propagation of electromagnetic waves in a pure vacuum does not occur precisely in accordance with the Maxwellian field equations», but by

[4] See Larmor (1909, 91). He reminded the reader that in 1902 he had already published a very brief *Report* (eleven lines), «in which it was essayed to replace Planck's statistics of bipolar vibrators by statistics of elements of radiant disturbance» (Larmor (1909, 86-8, 91)). See Larmor (1902, 546): « […] various difficulties attending this [namely Planck's] procedure are evaded, and the same result attained, by discarding the vibrators and considering the random distribution of the permanent element of the radiation itself, among the differential elements of volume of the enclosure, somewhat on the analogy of the Newtonian corpuscular theory of optics». See Planck (1910a, 761): «Am radikalsten verfährt hier von den englischen Physikern J.J. Thomson, auch Larmor, von den deutschen Physikern A. Einstein und mit ihm J. Stark. Dieselben neigen zu der Ansicht, daß sogar die elektrodynamische Vorgänge im reinen Vakuum, also auch Lichtwellen, nicht stetig verlaufen, sondern nach diskreten Quanten von der Größe *hn*, den ‚Lichtquanten', wobei *n* die Schwingungszahl bedeutet». For the diffusion of Larmor's papers, see Kuhn (1987, 136-7, 314).

[5] See Planck (1910a, 763-4, 767-8). Planck's review was really oversimplified: neither the differences between J.J. Thomson and Einstein, nor the differences between J.J. Thomson and Larmor were taken into account.

means of definite energy quanta. He found it «not necessary to proceed in so revolutionary a manner»; he would have confined himself to «seeking the significance of the energy quantum $h\nu$ solely in mutual actions with which the resonators influence one another». In any case, a «definite decision with regard to these important questions» could stem only from «further experience»[6].

I have decided to take into account the conceptual and historical reconstruction Planck outlined in 1909 and 1910, and the melting pot of complementary theoretical models and meta-theoretical options which was turned on in the last decades of the nineteenth century.

In reality, in the early 1890s, before his well-known experiments on cathode rays, J.J. Thomson had outlined a discrete model of electromagnetic radiation. In the same years, Larmor was trying to match continuous with discrete models for matter and electricity.

J.J. Thomson and Larmor were strongly involved in the emergence of late nineteenth century theoretical physics. Although the emergence of chairs of «theoretical physics» in German speaking countries in the last decades of the nineteenth century must be distinguished from «theoretical physics» as a new practice in physics, the latter emerged as a really new approach[7]. The hallmark of theoretical physics was the awareness that the alliance between the mathematical language and the experimental practice celebrated by Galileo had to be updated. Besides *definite demonstrations* and *sound experiments* there was a third component, which we could label conceptual or theoretical: it dealt with principles, models, and patterns of explanation. That conceptual component, neither formal nor empirical, was looked upon as a fundamental component of scientific practice. Different theories could share the same mathematical framework and make reference to the same kind of experiments: the difference among them could be found just at the conceptual level. Conversely, a given set of phenomena could be consistently described by different theories[8].

The emergence of theoretical physics also corresponded to a new sensitivity to meta-theoretical issues: we find explicit designs of unification, explicit methodological remarks, and explicit debates on the foundations of

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[6] Planck (1910b, engl. trans., 1998, 95-6).

[7] For the institutional aspects, see McCormmach & Jungnickel (1986, II vol., 33, 41-3, 48, 55-6).

[8] It seems to me that a similar point of view has been put forward in Giannetto (1995, 165-6), Kragh (1996, 162), D'Agostino (2000, ix), and Lacki (2007, 248). For a historical reconstruction from the point of view of an early twentieth-century scholar, see Merz (1912, 199).

physics. In that season, all these cogitations were looked upon as intrinsic aspects of scientific practice. Scientists did not entrust philosophers with reflections on aims and methods of science: meta-theoretical remarks emerged from the actual scientific practice, as a sort of new awareness[9].

It seems to me that L. Boltzmann managed to frame theoretical physics from the historical and conceptual points of view. In a lecture held in 1904, in St. Louis (USA), at the *Congress of Arts and Science*, he qualified «the development of experimental physics» as «continuously progressive». He saw some permanent achievements: among them, «the various applications of Röntgen rays» or «the utilisation of the Hertz waves in wireless telegraphy». On the contrary, he acknowledged that the «battle which the theories have to fight is, however, an infinitely wearisome one». Theoretical physics dealt with «certain disputed questions which existed from the beginning» and which «will live as long as the science». In other words, theoretical physics deals with conceptions which continuously emerge, then are neglected and subsequently re-emerge. One of the «problems» which he found «as old as the science and still unsolved» concerned the choice between *discrete* and *continuous* in the representation of matter. Moreover, the historical consciousness, which had already emerged in scientists of the last decades of the nineteenth century, found in Boltzmann an advanced interpretation. Physical theories could not be looked upon as «incontrovertibly established truths», for they were based on hypotheses which «require and are capable of continuous development»[10].

2. Continuity versus discontinuity, and mechanics versus probability

Swinging between discrete and continuous theoretical models was indeed one of the man features of Boltzmann's pathway to thermodynamics. In the 1870s, Ludwig Boltzmann tried to go far beyond Maxwell's microscopic interpretation of equilibrium in rarefied gases: he aimed at inquiring into the processes leading to equilibrium. In the first lines of his 1872 paper

[9] See Cassirer (1950, 83-4): «Now not only does the picture of nature show new features, but the view of what a natural science can and should be and the problems and aims it must set itself undergoes more and more radical transformation. In no earlier period do we meet such extensive argument over the very conception of physics, and in none is the debate so acrimonious. […] When Mach or Planck, Boltzmann or Ostwald, Poincaré or Duhem are asked what a physical theory is and what it can accomplish we receive not only different but contradictory answers, and it is clear that we are witnessing more than a change in the purpose and intent of investigation».

[10] Boltzmann (1905, 592-5).

"Weiteren Studien über das Wärmegleichgewicht unter Gasmolekülen", he reminded the reader about the foundations of the mechanical theory of heat. Molecules were always in motion, but the motion was invisible and undetectable: only the "average values» could be detected by human senses[11].

A thermodynamic theory required therefore two different levels: a microscopic invisible, and a macroscopic visible one. Statistics and probability could bridge the gap between the two levels. Boltzmann claimed that probability did not mean uncertainty: the presence of the laws of probability in the mechanical theory of heat did not represent a flaw in the foundations of the theory. Probabilistic laws were ordinary mathematical laws as certain as the other mathematical laws: we should not confuse an "incomplete demonstration» with a "completely demonstrated law of the theory of probability»[12].

The pivotal mathematical entity was «the number of molecules whose living force lies between *x* and $x + dx$, at a given time *t*, in a given space *r*»: Boltzmann labelled $f(x,t)\,dx$ this differential function. From the mathematical point of view, he had to face a «two-steps task»: the «determination of a differential equation for $f(x,t)$», and the subsequent «integration». He assumed that "the variation of the function stemmed only from the collisions» between couples of molecules. The keystone of the whole procedure was therefore the computation of the collisions[13]. That a differential equation, namely a mathematical structure based on a continuous variation over time, depended on intrinsically discontinuous processes like collisions, sounds quite astonishing: much more than the specific mathematical difficulties, this was the crucial challenge Boltzmann had to cope with. The function $f(x,t)$ did not belong to the tradition of mathematical physics: a re-interpretation of the concepts of dynamic equation and time-evolution of a physical system was at stake. That function had to bridge the gap between two different traditions in Mechanics: the laws of scattering between solid bodies, which were confined at the invisible microscopic level of interacting molecules, and the equations of motions, which ruled the macroscopic observable behaviour of the whole gas[14].

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[11] Boltzmann (1872), in Boltzmann (1909, I Band, 316).
[12] *Ibid.*, 317-8.
[13] *Ibid.*, 322.
[14] *Ibid.*, 322-3.

In a next section of the essay, the problematic link between mathematical algorithms and physical concepts was newly at stake, for Boltzmann transformed his integro-defferential equation into an infinite sum of discrete terms. That a late-nineteenth century physicist trained in the tradition of mathematical physics replaced integrals with infinite sums, seems quite puzzling, even though a discrete mathematical model was in accordance with the physical foundations of the kinetic theory of gases. Boltzmann himself tried to justify his theoreticl choice[15].

The new discrete procedure Boltzmann was undertaking required that the variable *x*, representing the living force of a molecule, could assume only a series of multiple values of a given quantity ε. This is perhaps the most astonishing feature of Boltzmann new theoretical model: energy, just like matter, could rely on a basic unit. In other words, Boltzmann put forward an atomic or molecular representation of energy alongside an atomic or molecular representation of matter. Here we can appreciate one of the main features of late-nineteenth-century theoretical physics: the explicit awareness that a plurality of theoretical models could account for a given class of phenomena. The continuous function $f(x,t)$ had to be replaced by a series of statistical weights: the number w_1 of molecules with energy ε, the number w_2 of molecules with energy 2ε, and so on. The label $N_{\chi\lambda}^{kl}$ represented «the number of collisions» which transformed the energies $k\varepsilon$ and $l\varepsilon$ of two molecules into the energies $\chi\varepsilon$ and $\lambda\varepsilon$[16].

Two important features of Boltzmann's theoretical pathway deserve to be emphasised. First, Boltzmann forced Mechanics and Statistics to stay beside each other. Second, he gave up the demand that the behaviour of a physical system as a whole be reduced to, and explained by, the behaviour of its components. Every molecular component followed the laws of ordinary mechanics, but the whole followed statistical laws: the whole could not be looked upon as a mere sum of its microscopic parts[17].

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[15] See *Ibid.,* 347: «Die Integrale sind bekanntlich nichts anderes als symbolische Bezeichnungen für Summen unendlich vieler, unendlich kleiner Glieder. Die symbolische Bezeichnung der Integralrechnung zeichnet sich nur durch eine solche Kürze aus, dass es in den meisten Fällen nur zu unnützen Weitschweifigkeiten führen würde, wenn man die Integrale erst als Summen von *p* Gliedern hinschriebe und dann *p* immer größer werden ließe. Trotzdem aber gibt es Fälle, in denen die letztere Methode wegen der Allgemeinheit, die sie erzielt, namentlich aber wegen der größeren Anschaulichkeit, in der sie die verschiedenen Lösungen eines Problems erscheinen lässt, nicht ganz zu verschmähen ist».

[16] *Ibid.* 348-9.

[17] In this conceptual gap, Cassirer saw a deep transformation of "the ideal of knowledge". See Cassirer (1936, 97): «Denn eben der Umstand, dass so weitreichende Aussagen über

In 1877 Boltzmann published an even longer paper, where he reminded the reader that the function *E* he had introduced in 1872 could never increase, and that it reached its minimum value at thermal equilibrium. He also reminded the reader about a recently published paper, "Bemerkungen über einige Probleme der mechanischen Wärmetheorie": there he had shown that «there are more uniform than non-uniform distributions» of living force among the molecules of a gas, and that a great probability «that the distribution become uniform over time» followed[18].

The molecules could assume only discrete values of velocity: the model was qualified by Boltzmann himself as «fictitious» and «not corresponding to an actual mechanical problem», although «much easier to handle mathematically». The series of available «living forces» corresponded to an «arithmetic progression» $0,\ \varepsilon,\ 2\varepsilon,\ 3\varepsilon,\ .....,\ p\varepsilon$ with an upper bound $P = p\varepsilon$. These values of the energy could be «distributed over the *n* molecule in all possible ways», provided that the sum of all energies was preserved over time, and assumed a given value $\lambda \cdot \varepsilon = L$ [19].

Boltzmann called «complexions» the different distribution of energy among the *n* molecules, which corresponded to the same number of molecules endowed with a given value of energy. In other words, a complexion was a simple permutation in a fixed state or distribution of energy. If a given state corresponds to « w_0 molecules with null living force, w_1 molecules with living force ε, w_2 with living force 2ε, and so on», there is a given umber of complexions corresponding to the state, which Boltzmann labelled «the number of complexions» B or «number of permutations» or «permutability of a given distribution». In his 1877 paper, the *discrete function* B took on the crucial role played by the *discrete function* $N_{\chi\lambda}^{kl}$ in his 1872 paper[20].

The computation of the «permutability» $\mathrm{B} = n!\,/\,w_0! \cdot w_1! \cdot w_2! \cdot \ldots\ldots \cdot w_p!$ was submitted to the conservation of matter and energy. For every state, the number of complexions corresponded to the number of permutations among

ein physikalisches Ganze unter Verzicht auf die Kenntnis der einzelnen Teile möglich sind, stellt vom Standpunkt der reinen Punktmechanik eine Paradoxie dar und enthält eine Umbildung des Erkenntnisideals, das sie bisher durchgeführt hatte».

18 Boltzmann (1877b), in Boltzmann (1909, II Band, 164).

19 *Ibid*., 167-9.

20 *Ibid*., 169-70. At this stage of Boltzmann's theorisation, every specific mechanical model was dismissed. See Campogalliani (1992, 455): «[…] in questo ambito ogni modello ancorato alla meccanica delle collisioni molecolari risulta sostanzialmente accantonato […]».

all the molecules divided by the number of internal permutations among the members of every set of molecules owning the same energy. Boltzmann identified the minimum of the denominator with the minimum of its logarithm, because the denominator «is a product» of factorials. At this point, he suddenly changed his model, «in order to apply the differential calculus» to a computation based on the discrete structure of integer numbers. He transformed the *factorial function* into the *Gamma function*, which was a generalisation of the factorial function to continuous numerical sets[21].

Another mathematical switch was activated at this point; he re-translated the expression *to be minimised* into a discrete form. Subsequently the quantity ε was interpreted as «a very small quantity», and the frequencies $w_0, w_1, w_2, \ldots\ldots w_p$ were expressed by means of a continuous function *f(x)*[22].

After having devoted some pages to multi-atomic molecules, and many more pages to analysing different distributions of probability, in the last section Boltzmann faced «the relationship between entropy and distribution of probability». He stressed the structural similarity between the function Ω, representing the probability of a given state, and the entropy dQ/T in any «reversible change of state»[23].

However, in the 1880s, some German-speaking scientists cast doubts on atomism and microscopic interpretations of the second principle of

[21] *Ibid*., 175-6. In the subsequent years, Boltzmann tried to clarify the conceptual tension between continuous and discontinuous theoretical models. In two papers, first published in the *Annalen der Physik und Chemie* in 1897, and then in his *Populäre Schriften*, he claimed that «[a]tomism seems inseparable from the concept of the continuum». He noticed that in the theory of heat conduction and in the theory of elasticity, «one first imagine a finite number of elementary particles that act on each other according to certain simple laws and then once again looks for the limit as this number increases». In any case, we have to start from «a finite number of elements» even in integral calculus. According to Boltzmann, mathematical procedure required the passage from discontinuous to continuous representations, just in this order. See Boltzmann (1897a, 44), and Boltzmann (1897b, 55). On the Kantian *flavour* of Boltzmann approach to that conceptual tension, see Dugas (1959, 73).

[22] *Ibid*., 177 and 187-8.

[23] *Ibid*., 216-7. For a comparison with his 1872 line of reasoning, see Boltzmann (1872), in Boltzmann (1909, I Band, 399-400). Cassirer found that Boltzmann had managed to remove the “paradoxical and extraneous nature (*Fremdheit*)» of the second Principle of Thermodynamics in the context of Mechanics. Just for this reason, he qualified Boltzmann as «one of the most rigorous representatives of classic Mechanics». See Cassirer (1936, 95-6). The fact is that, in Boltzmann’s theory, the second Principle did not stem from Mechanics, but from statistical and probabilistic hypotheses unrelated to Mechanics.

Thermodynamics: among them we find the young Planck, who had an extraordinary tenure at the University of Kiel. In 1882, in the last paragraph of a paper devoted to vaporisation, melting and sublimation, he made some sharp remarks on the second Principle. He found that the consequences of that principle and «the assumption of finite atoms» were mutually «incompatible», and imagined that «a battle (*Kampf*) between the two hypotheses» would have taken place in the near future. Making use of an emphatic metaphor, which did not fit in with the plain style of the paper, Planck foresaw that the battle would lead to «the loss of life» for one of the opponents. Although he considered «however premature» any definite prediction, he saw some evidence in favour of the hypothesis of «continuous matter» and against the atomic theory, «its great results notwithstanding»[24].

In 1880, when he had published the dissertation *Gleichgewichtzustände isotroper Körper in verschiedenen Temperaturen*, in order to be given the *venia legendi*, he had outlined a mathematical theory where the mechanics of continuous media merged with thermal processes. He relied on the two principles of «the mechanical theory of heat», and «specific assumptions on the molecular structure (*Beschaffenheit*) of bodies» were «not necessary». In accordance with this theoretical option, he assumed that isotropic bodies consisted of «*continuous* matter»[25].

A widespread debate on the foundations of Thermodynamics involved the scientific community for many years, and some British physicists criticised the mechanical and probabilistic interpretations of the second Principle. Edward P. Culverwell was one of the British scientists who were dissatisfied with Boltzmann's explanation of the drift of a physical system towards equilibrium. In 1890, he had remarked that «no one» had managed to show that «a set of particles having any given initial conditions» would

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[24] Planck (1882, 474-5). In a footnote he made reference to two recent German editions of Maxwell's *Theory of Heat*, in particular to a passage where the author played with an omnipotent being who was able to separate fast from slow molecules. See Maxwell (1872, 308-9), and Maxwell (1885, 328-9). See Kuhn (1987, 23-4) for the identification of Planck's reference to German editions with the above mentioned passage by Maxwell.

[25] Planck (1880, 1). Planck became *Privatdocent* at the University of Munich in 1880, and was appointed as extraordinary professor of physics at the University of Kiel in 1885. In 1889, two years after Kirchhoff's death, he became assistant professor at the University of Berlin, and director of the Institute for Theoretical Physics: in 1892 he was appointed ordinary professor. See McCormmach & Jungnickel (1986, vol. 2, 51-2, 152, 254), and Gillispie (ed.) (1970-80, Volume XI, 8).

have approach the «permanent configuration» of equilibrium, «as time goes on»[26].

In 1894, Boltzmann took part to the annual meeting of the British Association for the Advancement of Science, and his communications raised some debate, which continued in the pages of the scientific journal *Nature* in 1895. The British journal also hosted a paper where Boltzmann tried to clarify his probabilistic approach to Thermodynamics. He clearly stated that the second Law could «never be proved mathematically by means of the equations of dynamics alone». This was a very important statement, because he explicitly acknowledged that something else was at stake besides the mechanical model of the kinetic theory. In reality, that *something else* was the statistical independence of the dynamical parameters of the different molecules, and it was a hypothesis in contrast with the laws of mechanics. In some sense Boltzmann's answer to Culverwell's objection was in accordance with Culwerwell's objection itself: the demonstration of Boltzmann's theorem required "some assumption" of not-mechanical nature[27].

3. Continuous and discrete structures for the electromagnetic field

In a paper published in 1891, Poynting's model of tubes of force allowed J.J. Thomson, then Cavendish Professor of Experimental Physics (the chair previously held by Maxwell), to undertake a relevant conceptual shift. The electric field as a continuous entity transformed into a new «molecular» theory, where electric fields were imagined as a collection or discrete, individual entities, endowed with their own identity. He introduced two levels of investigations, macroscopic and microscopic. In thermodynamics, the macroscopic level of the theory of gases corresponded to the microscopic level of the kinetic molecular theory: in some way, the latter was an *explanation* of the former. The microscopic level corresponded to a higher level of comprehension or to a finer interpretation. In the electro-magnetic theory, to a macroscopic level, described in terms of continuous fields, corresponded a microscopic level, described in terms of an invisible,

[26] Culverwell (1890, 95). Among the problems still unsolved, Culverwell mentioned the determination of the mathematical law for intermolecular force, and the role played by the luminiferous aether.

[27] Boltzmann (1895), in Boltzmann (1909, III. Band, pp. 539-40). See Culverwell (1895, 246, above quoted). There was another issue, indeed, which Boltzmann did not face explicitly, a fundamental question which emerges whenever the argument of velocities reversal comes into play: where would the required energy come from?

discrete structure: the tubes of electric induction. J.J. Thomson put forward a conceptual shift towards a *kinetic molecular* theory of energy, the same conceptual shift already realized in the case of matter[28].

Another conceptual shift occurred in the representation of matter, from the model of solid dielectrics to the model of electrolytes. Electrolytes were exactly the kind of matter which was not easy to explain in the context of Maxwell's theoretical framework. At the same time, gases seemed to exhibit the same behaviour of electrolytes when electricity passed through them. Liquid electrolytes and ionised gases became the new model of matter «undergoing chemical changes when the electricity passes through them». The theory Maxwell had put forward was essentially a theory based on solid dielectrics and conductors; now liquids and gases were on the stage and Thomson attempted to explain the properties of metals by means of the properties of liquids and gases[29].

In 1893, in the treatise *Recent Researches in Electricity and Magnetism*, J.J. Thomson put forward a discrete structure for matter, electricity and energy, provided that the tubes of force represented a sort of substantialisation of the electromagnetic energy stored in the field. Inside a molecule, Thomson saw short tubes of force keeping atoms close to each other, in order to assure molecular stability: in this case, the length of the tubes were of the same order of molecular dimensions. On the contrary, if the length of the tubes was far greater than molecular dimensions, we would have in front of us atoms «chemically free»[30]. Not only was matter embedded in a net of tubes of force but even aether was. Indeed, tubes of force were not a mere materialisation of electric forces: Thomson imagined a sea of tubes of force spread throughout aether even without any electric force. There was a distribution of tubes corresponding to an unperturbed state. The effect of electric forces was an overbalance in the sea of tubes: electric forces made tubes move towards a specific direction. The drift of the tubes, driven by the electric forces, gave rise to electrodynamic effects, for instance the establishment of a magnetic field[31].

[28] See Thomson (1891, 150): «We may regard the method from one point of view as being a kind of molecular theory of electricity, the properties of the electric field being explained as the effects produced by the motion of multitudes of tubes of electrostatic induction; just as in the molecular theory of gases the properties of the gas are explained as the result of the motion of its molecules».
[29] See Thomson (1891, 151).
[30] See Thomson (1893, 3).
[31] *Ibid*., 4.

Tubes of force were the *hardware* associated to energetic processes. They underwent a sort of Law of Conservation: they could be neither created nor destroyed. A symmetry between matter and energy was explicitly assumed: in Thomson's theoretical model, the *sea* of tubes of force behaved as a *cloud* of molecules in a gas[32].

A statistical aspect of Thomson's theory emerged, an aspect which connected electromagnetism to thermodynamics: in both cases, the macroscopic picture was the statistic effect of a great number of microscopic events. Thomson was strongly committed to a meta-theoretical issue, which flowed through the specific features of his theory like an enduring conceptual stream. This issue was the pursuit of the unity of physics. The theoretical model of «molecular» electric tubes of force allowed him to realize at least a certain degree of unification[33].

In a subsequent section, *Electromagnetic Theory of Light*, Thomson tried to give a more detailed account of propagation of light in terms of tubes of force. He thought that Faraday's tubes of force could help to «form a mental picture of the processes which on the Electromagnetic theory accompany the propagation of light». The propagation of a plane wave could be interpreted as «a bundle of Faraday tubes» moving at right angles to themselves and producing a magnetic force oriented at right angles with regard to both the direction of the tubes and the direction of motion[34].

Starting from Maxwell's electromagnetic fields, represented as stresses propagating through a continuous solid medium, Thomson arrived at a representation of fields as a sea of discrete units carrying energy and

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[32] See Thomson (1893, 4): «Thus, from our point of view, this method of looking at electrical phenomena may be regarded as forming a kind of molecular theory of Electricity, the Faraday tubes taking the place of the molecules in the Kinetic Theory of Gases: the object of the method being to explain the phenomena of the electric field as due to the motion of these tubes, just as it is the object of the Kinetic Theory of Gases to explain the properties of a gas as due to the motion of its molecules. The tubes also resemble the molecules of a gas in another respect, as we regard them as incapable of destruction or creation.»

[33] I agree with J. Navarro when he stresses J.J. Thomson effort to attain a unified representation of physical and chemical phenomena, but I do not find that the «metaphysics of the continuum» was the unifying element. See Navarro (2005, 272-3).

[34] Thomson (1893, 11, 42): If there is no reflection the electromotive intensity and the magnetic force travel with uniform velocity v outwards from the plane of disturbances and always bear a constant ratio to each other. By supposing the number of tubes issuing from the plane source per unit time to vary harmonically we arrive at the conception of a divergent wave as a series of Faraday tubes traveling outwards with the velocity of light. In this case the places of maximum, zero and minimum electromotive intensity will correspond respectively to places of maximum, zero and minimum magnetic force.

momentum. The wave theory of light, then a well-established theory, seemed violently shaken by a conception which echoed ancient, outmoded theories[35].

The conceptual tension between the *discrete* and the *continuous* affected aether, matter, energy and electric charge. This tension led to a unified view, where a new symmetry emerged between matter and energy: both were represented as discrete structure emerging from the background of a continuous medium. Invisible, discrete, microscopic structures explained the properties of apparently continuous, macroscopic phenomena. J.J. Thomson tried to transform Maxwell's theory into a unified picture where atomic models of matter stood beside *atomic* models of fields. One unit of matter corresponded to one unit of electricity, and one unit of tube of force connected units of matter-charge to each other[36].

From 1893 to 1897, Larmor, then fellow of the *Royal Society*, published in the *Philosophical Transactions* three thick papers under the title «A Dynamical Theory of the Electric and Luminiferous Medium». The title drew readers' attention to aether, which represented the keystone of the whole project: it was the seat of electrical and optical phenomena, and it was involved in the constitution of matter.

In 1894, Larmor tried to clarify the relationship between electricity and structure of matter. The lines of twist starting from an atom and ending on another atom of the same molecule resembled the short tubes of force connecting the atoms in a molecule as suggested by J.J. Thomson some years before. In that theoretical model, Maxwell's transfer of electricity as pure propagation of breakdowns of elasticity across the aether appeared not completely satisfactory, because the seat of electricity could also be inside matter. Therefore Larmor took a step forward: the transfer of electricity also consisted of the «convection of atomic charges». Electric charge underwent a conceptual shift from a phenomenon connected to the distribution and transfer of energy to a phenomenon connected to the distribution and transfer of matter. Conversely, matter became a peculiar entity, stemming from dynamical actions taking place in the aether. However, a sort of

[35] *Ibid*., 43: «This view of the Electromagnetic Theory of Light has some of the characteristics of Newtonian Emission theory; it is not, however, open to the objections to which that theory was liable, as the things emitted are Faraday tubes, having definite positions at right angles to the direction of propagation of the light. With such a structure the light can be polarised, while this could not happen if the things emitted were small symmetrical particles as on the Newtonian Theory».

[36] For further remarks on J.J. Thomson's theoretical researches between 1891 and 1893, see Bordoni (2008, chapters 12 and 13).

conceptual continuity was assured, for the transfer of particles, represented as dynamical structures of the aether, was not so different from the transfer of *pure* energy. In other words, in Larmor's general framework, matter and energy, in their intimate nature, were not radically different from each other[37].

The motion of a charged particle through aether produced an «elastic effect of convection through the medium», consisting of «a twist round its line of movement». The effect was not so different from the propagation of elastic actions in *displacement* currents: such a twist was just the common feature of every kind of electric current. At the same time Larmor acknowledged that he had not managed to enlighten what he considered the core of every electromagnetic theory: «the detailed relations of aether to matter». He assumed that the basic dynamic entity was placed at the sub-atomic level, and he labelled «electron» that entity. The new solution, the «electron», confirmed the integration between the continuous *substratum* and the discrete unit, in some way a *particle*, of electric charge. The specific unifying element of the new theory was the convective nature of all kind of electric currents, both macroscopic and microscopic[38].

Independently from their peculiar nature of dynamical singularities in the aether, electrons were electric charges in motion along closed paths, therefore undergoing an accelerate motion. Consistently with Maxwell's electromagnetic theory of radiation, accelerated electric charges would have sent forth electromagnetic waves. That effect was in contrast with Larmor's atomic model, for a swift damping of electronic motion would have followed. To save the model, Larmor introduced (*ad hoc*, indeed) the concept of «steady motion», and the concept of perturbation of a steady motion. Electric waves could stem only from those perturbations[39].

This new condition of «steady motion» broke the symmetry between macroscopic and microscopic level, for the condition of *steadiness* appeared

[37] See Larmor (1894, 771).
[38] Larmor (1894, 807).
[39] See Larmor (1894, 808): «It may be objected that a rapidly revolving system of electrons is effectively a vibrator, and would be subject to intense radiation of its energy. That however does not seem to be the case. We may on the contrary propound the general principle that whenever the motion of any dynamical system is determined by imposed conditions at its boundaries or elsewhere, which are of a steady character, a steady motion of the system will usually correspond, after the preliminary oscillations, if any, have disappeared by radiation or viscosity. A system of electrons moving steadily across the medium, or rotating steadily round a centre, would thus carry a steady configuration of strain along with it; and no radiation will be propagated away except when this steady state of motion is disturbed».

suitable only for the latter. Unfortunately, the tension between *macroscopic* and *microscopic*, which seemed to have been overcome by the attribution of a convective nature even to microscopic currents, re-appeared once again. There was a difference between the intimate nature of matter, concerning microphysics, and its visible features, concerning ordinary physics[40].

The double nature of electrons, as individual building blocks of matter, on the one hand, and as dynamical structures of aether, on the other, affected their behaviour with regard to velocity. As long as their velocity remained far less than the velocity of radiation, their dynamical properties could be expressed "in terms of the position of the electrons at the instant". When their velocities approached that of radiation, they had to be «treated by the methods appropriate to a continuum». In other words, low velocity electrons behaved like particles, whilst high velocity electrons behaved like radiation[41].

Larmor's electron as a rotational stress in the aether led to a model of electric current not so different from Thomson's, because an electronic flow could be looked upon as a motion of some kind of aethereal perturbation. I find that, beyond some specific, important features, which differentiated Larmor's electrons from Thomson's tubes of force, both entities consisted of dynamical and aethereal structures propagating through aether itself. Moreover, in both cases, we are dealing with the propagation of a series of discrete units, either tubes of force or electrons[42].

After 1894, Larmor went on inquiring into the aethereal concentration of energy which was peculiar to his electron. In 1895, in the first lines of the second paper of the trilogy «A Dynamical Theory of the Electric and Luminiferous Medium», he re-introduced «electrons or permanent strain-

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[40] It is worth mentioning that, since the dawn of natural philosophy, two general conceptions on the link between *macroscopic* and *microscopic* world had been on the stage. On the one hand, the conception of an invisible small-scale structure as a tiny copy of the large-scale world; on the other hand, the conception of an invisible small-scale structure endowed with specific features, following different laws. The main hallmark of ancient atomism was the physical gap between the ordinary, visible world, and the invisible world of atoms: the latter was an *explanation* of the former.

[41] Larmor (1894, 811). For further remarks on Larmor's theoretical researches between 1894 and 1895, see Bordoni (2011, 36-54).

[42] A different appraisal can be found in Darrigol (2000, 168, 174). Darrigol claimed that Poynting and J.J. Thomson's theoretical model of electric current as an effect of the convergence and dissolution of tubes of force «preserved a Maxwellian intuition of the electric current». On the contrary, the *electron* Larmor introduced in 1894, represented an alternative to Maxwell's leading theoretical model, as well as *particles* (1892) and *ions* (1895) which Lorentz introduced in the same years.

centres in the aether, which form a part of, or possibly the whole of, the constitution of the atoms of matter»[43].

In his 1900 *Aether and Matter*, Larmor put forward a unified view for both electromagnetic fields and matter. On the one hand, electromagnetic actions consisted of «elastic actions across the aether», so that «an electric field must be a field of strain». On the other hand, *protions*, endowed with intrinsic electric charge, «must be surrounded by a field of permanent or intrinsic aethereal strain» and therefore they must be «in whole or in part a nucleus of intrinsic strain in the aether». Propagations of pure fields and propagation of elementary matter yielded the same effects; in other words, Maxwell's *displacement* currents and convective electric currents shared the same intimate nature. He portrayed *protions* or *electrons* as something which «can move or slip freely about through that medium much in the way that a knot slips along a rope»[44].

In 1904 J.J. Thomson published a booklet, *Electricity and Matter*, wherein he collected together some lectures he had held in Yale in 1903; within a few months, Thomson's booklet was translated into German and other languages. In the third chapter, «Effects due to acceleration of the Faraday's tubes», Thomson focussed on the interaction between Röntgen rays and matter. He remarked that «Röntgen rays are able to pass very long distances through gases, and as they pass through the gas they ionise it». What he found difficult to explain was that «the number of molecules so split up is, however, an exceedingly small fraction, less than one billionth, even for strong rays, of the number of molecules in the gas». The question was: why were not all the molecules crossed by that kind of radiation affected in the same way? In other words, «if the conditions in the front of the wave are uniform, all the molecules of the gas are exposed to the same conditions»: how could the fact «that so small a proportion of them are split up» be explained? Perhaps the concentration of energy which modified the microscopic structure of matter did not have its seat in Röntgen rays but in matter itself. Perhaps only high-energy molecules could experience the ionisation when interacting with the rays. Nevertheless, in this case, the probability of the ionisation would have shown some kind of dependence on gas temperature, namely on its internal energy: «the ionisation produced by the Röntgen rays ought to increase very rapidly as the temperature increases»[45]. This was not the case and therefore J.J. Thomson resorted to

[43] Larmor (1895, 695, 697 and 706).
[44] Larmor (1900, 26, 86).
[45] Thomson (1904, 63-4).

his 1893 theoretical model of electromagnetic radiation as a bundle of discrete tubes of force. He thought that the selective ionisation could be explained only if, «instead of supposing the front of the Röntgen ray to be uniform, we suppose that it consists of specks of great intensity separated by considerable intervals where the intensity is very small». According to that hypothesis, the microscopic properties of electromagnetic radiation were similar to the properties of microscopic particles: in J.J. Thomson's words, «the case becomes analogous to a swarm of cathode rays passing through the gas». Indeed, that flux of elementary corpuscles showed the same behaviour of X-rays: «The number of molecules which get into collision with the rays may be a very small fraction of the whole number of molecules». In 1904, J.J. Thomson imagined tubes of force «as discrete threads embedded in a continuous ether, giving to the latter a fibrous structure». He assumed that both aether and electromagnetic waves were endowed with a discrete structure: it was a solution, he remarked, «which I have not seen noticed»[46].

4. Concluding remarks on *classical* physics

Now the question is: why, in more recent secondary literature has not the conceptual link between J.J. Thomson and Einstein (however problematic it may be) been taken into account? I must stress that what appears as a sort of *missing link* in recent literature, was acknowledged as an important link by some physicists in the first half of the twentieth century[47].

[46] Thomson J.J. 1904, 63, 65.

[47] In reality, between the 1950s and the 1980s, historians payed attention to the conceptual link between J.J. Thomson and Einstein, but more recently, the issue has been skipped by historians. In 1953, E.T. Whittaker acknowledged that the «apparent contradiction between the wave-properties of radiation and some of its other properties had been considered by J.J. Thomson in his Silliman lectures of 1903». See Whittaker (1953, 93). In 1963, M. Klein, confined himself to note that Einstein's 1905 paper on light *quanta* did not show any evidence «that he was aware of or influenced by Thomson's ideas». See Klein (1963, 62 and 80). In 1967, R. McCormmach stated that Einstein's «views have certain close similarities with Thomson's, and they should be examined». When he drew his conclusion he claimed that «Thomson's theory of light was inconclusive» and «the predicted structure remained largely qualitative in theory and undetectable in the laboratory». But he acknowledged that «Thomson contributed to the twentieth-century revolution in the theory of light». See McCormmach (1967, 387). In 1978, C. Tarsitani remarked that the query about the nature of radiation «had already been raised by J.J. Thomson before 1905, without any reference to photoelectric effect». See Tarsitani (1978, 255-6). In 1983, B.R. Wheaton remarked that «Thomson had speculated that lines or 'tubes' of force might be

Millikan, both in *The Electron*, the book he published in 1917, and in his 1924 *Nobel Lecture* took explicitly into account the link between J.J. Thomson and Einstein. The photo-electric effect and X-rays scattering could be accounted for «in terms of a corpuscular theory», wherein «the energy of an escaping electron comes from the absorption of a light-corpuscle». Einstein's 1905 hypothesis seemed to Millikan a daring implementation of Thomson's theoretical model. The former appeared to Millikan definitely unreliable: «I shall not attempt to present the basis for such an assumption, for, as a matter of fact, it had almost none at the time»[48]. In any case, and independently from the unsatisfactory theoretical foundations, he acknowledged that the process of «emission of energy by an atom is a discontinuous or explosive process». That «explosive» feature suggested to Millikan the hypothesis that the cause of the photoelectric effect or X-rays scattering was placed in matter rather than in radiation. This model was called by Millikan the «loading theory», because the process of accumulation of energy inside the atom was its main feature. According to Millikan, an unknown mechanism concerning the structure of the atom, and some unknown structure of aether were involved. He completely overturned the meaning of Einstein's *quantum* theory: not only, in his words, the «Thomson-Einstein theory throws the whole burden of accounting for the new facts upon the unknown nature of the ether», but Thomson and Einstein were associated in their supposed attempt to make «radical assumptions about its structure»[49]. That Einstein's theoretical model did not require any aether was perhaps beyond Millikan's conceptual horizon.

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more than just mathematical abstractions». See Wheaton (1983, 78); see also Wheaton (1983, 16, 109, 138). On the contrary, Cassidy's survey of Einstein's 1905 paper on light *quanta* begins with the sharp sentence: «Einstein was the first to propose that light behaves in some circumstances as if it consists of localized units, or quanta». See Cassidy (2005, 15, 17). In a detailed and authoritative paper, J. Norton claimed that, differently from «special relativity and the inertia of energy», which he looked upon as «a fulfillment of the 19th century tradition in electrodynamics», Einstein's hypothesis of «spatially localized quanta of energy – stands in direct contradiction with that most perfect product of 19th century science». See Norton (2006, 72).

[48] Millikan (1917, 221-3). Einstein's «lokalisierten Energiequanten» appeared to Millikan nothing more than a specific feature of J.J. Thomson's *fibrous aether*. In eight pages (from 231 to 238), there are eight occurrences of expressions like «Thomson-Einstein theory», «Thomson-Einstein hypothesis of localized energy», «Thomson-Einstein theory of localized energy», «Thomson-Einstein assumption of bundles of localized energy travelling through the ether», or eventually «Thomson-Einstein semi-corpuscular theory».

[49] Millikan (1917, 234-7). B.R. Wheaton claimed that an «integral part of Einstein's rejection of the medium for light waves was his suggestion of the lightquantum hypothesis». See Wheaton (1983, 106).

In his 1924 Nobel lecture, he recollected his efforts to find «some crucial test for the Thomson-Planck-Einstein conception of localized radiant energy.» According to Millikan, Einstein's theory combined Thomson's conception with «the facts of quanta discovered by Planck through his analysis of black-body radiation», in order to obtain «an equation which should govern, from his viewpoint, the interchange of energy between ether waves and electrons». Although «the reality of Einstein's light quanta may be considered as experimentally established», he thought that «the conception of the localised light quanta out of which Einstein got his equation must still be regarded as far from being established»[50].

Two elements are worth mentioning: first, Millikan failed to acknowledge Thomson's 1893 theoretical contribution, and, second, he misunderstood the nature of the conceptual link between J.J. Thomson and Einstein[51].

Obviously, Planck's 1900 search for a new law for the distribution of electromagnetic radiation, Einstein's 1905 attempt to overcome the asymmetry between matter and radiation, and J.J. Thomson's outline of a unified picture represented sharply different pathways to the integration between discrete and continuous models for energy. At the same time, Planck, Einstein, J.J. Thomson, and Larmor's different theoretical approaches could be looked upon as different implementations of the same attempt to integrate complementary conceptions. The connections among them are meaningful but quite problematic, and the different specific features of their correspondent theories should not be overshadowed[52].

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[50] Millikan (1924, 61-65). Once again he only saw two alternatives: either «the mechanism of interaction between ether waves and electrons has its seat in the unknown conditions and laws existing within the atom», or such a *mechanism* «is to be looked for primarily in the essentially corpuscular Thomson-Planck-Einstein conception as to the nature of the radiant energy ».

[51] R. Stuewer pointed out two elements. First, «Millikan, in common with almost all physicists at the time, rejected Einstein's light quantum hypothesis as an interpretation of his photoelectric-effect experiments of 1915». Second, Millikan himself, in his Autobiography, published in 1950, revised his appraisal and stated that the phenomenon «scarcely permits of any other interpretation than that which Einstein had originally suggested». Stuewer qualified that sharp change as an instance of «revisionist history». On this issue, and on the attitudes of the scientific community towards Einstein's hypothesis in the 1910s, see Stuewer (2006, 543-8).

[52] I find worth mentioning Renn's general interpretation of Einstein's 1905 papers. The hypothesis of light quanta was interpreted as an attempt to solve the problems at the borderline between electromagnetism and thermodynamics. The hypothesis of the equivalence between electromagnetic radiation and inertial mass was interpreted as an

Planck's 1910 review in the *Annalen der Physik* was really oversimplified: neither the relevant differences between J.J. Thomson and Einstein, nor the less relevant differences between J.J. Thomson and Larmor were taken into account. Planck continued to swing between continuous and discrete theoretical models: in his talk at the French Society of Physics in 1911, the persistence and co-existence of complementary theoretical models both for matter and energy, and the co-existence of determinism and indeterminism vividly emerge. If discrete electromagnetic processes were involved in «the emission of energy», in accordance with «the laws of chance», absorption took place «in a perfect continuous way». The two-fold behaviour of radiation allowed Planck to recover a sort of symmetry between matter and radiation: discontinuous processes were involved both «in pure energy of radiation, like heat radiation, Röntgen rays, and γ rays, and in material rays, like cathode rays and α and β rays.»[53]

In 1913, in the second edition of his *Vorlesungen über die Theorie der Wärmestrahlung*, Planck insisted on this two-fold theoretical approach. He assumed that absorption was a continuous process, while emission was a discrete one, and it had the feature of a random process[54]. He did not think that swinging between opposite theoretical model was a disparaging meta-theoretical option.

Both in Boltzmann's pathway to Thermodynamics in the 1870s, and in Larmor and J.J. Thomson's pathways to Electromagnetism in the 1890s, we find an attempt at integration between discrete and continuous models, and

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attempt to solve the problems at the borderline between mechanics and electromagnetism. See Renn & von Rauchhaupt (2005, 32). See also Renn (2006b, 43).

[53] See Planck (1911, 358-9): «Il me semble donc nécessaire de modifier l'hypothèse des éléments d'énergie de la façon suivante. Seule *l'émission* de l'énergie se fait par à-coups, par quantités d'énergie ε entières et d'après les lois du hasard; l'absorption, au contraire, se poursuit d'une manière parfaitement continue. […] On suppose ici, en effet, qu'une molécule ne peut émettre de l'énergie de vibration que suivant certaines quantités déterminées ε, qu'il s'agisse de pure énergie de rayonnement comme dans le rayonnement calorifique, les rayons Röntgen et les rayons γ, ou d'un rayonnement corpusculaire, comme dans le cas des rayons cathodiques et des rayons α et β.» See also p. 359: «Il semble aussi que, dans l'émission des rayons cathodiques, dans l'effet photo-électrique, de même que dans les phénomènes de la radioactivité, […] elle doive jouer un rôle fondamental».

[54] See Planck (1913), in Planck (1915, 153): «[…] we shall assume that the emission does not take place continuously, as does the absorption, but that it occurs only at certain definite times, suddenly, in pulses, and in particular we assume that an oscillator can emit energy only at the moment when its energy of vibration, *U*, is an integral multiple *n* of the energy $\varepsilon = h\nu$. Whether it then really emits or whether its energy of vibration increases further by absorption will be regarded as a matter of chance».

between macroscopic and microscopic levels, both for matter and radiation. Can we say that we are dealing here with *classical* physics? In reality, the question is: what is really classical physics?

I see two historiographical alternatives: either the path of *classical* physics ended around the 1880s, or it ended around the 1920s. In that time span, the torch of theoretical physics flourished and quickly faded away. According to the second historiographical framework, Boltzmann's 1877 paper on the probabilistic interpretation of Thermodynamics, and J.J. Thomson's 1893 treatise on Electromagnetism belong to classical physics, and therefore Planck's theoretical pathway from 1900 to 1911 belongs to classical physics, and Einstein's 1909 paper can be looked upon as a sort synthesis of classical physics. According to the second framework, Boltzmann's paper and J.J. Thomson's treatise do not belong to classical physics, and therefore they represent a new kind of physics; Einstein's new theories can suitably be associated to Larmor and J.J. Thomson's XIX-century physics. However, in both cases, we find a substantial continuity in the history of physics until, at least, 1911.

The relationship between Planck and Einstein's new theories of electromagnetic radiation, on the one hand, and the body of knowledge which had emerged in the last decades of the nineteenth century, on the other, represents a very sensitive historiographical issue, and it has not been widely analyzed yet. I have confined myself to casting some light on the field, which is still waiting for being further explored. From a more general historiographical perspective, I find that we must stress changes and innovation introduced by the early twentieth-century theoretical physics and, at the same time, we must acknowledge the importance of theoretical researches which took place at the end of the nineteenth century. There was continuity in the attempt to integrate complementary conceptions for matter and energy; there was discontinuity in the specific features of Planck and Einstein's theories. I find that «continuity and innovation» should not be «disjunctive, mutually exclusive predicates». Sometimes the concept of scientific revolution «describes only the gross structures of scientific change». When we take into account the fine structure, we have the opportunity to appreciate elements of both continuity and discontinuity[55].

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[55] See Funkenstein (1986, 14). He claimed that what we look upon as «new», often «consists not in the invention of new categories or new figures of thought, but rather in a surprising employment of existing ones». E. Giannetto has recently remarked that «nature and origins of quantum physics» had meaningful roots in Larmor's theoretical researches. See Giannetto (2007, 178, 181). See Miller (1984, 312). I think that my sketch does justice to the old-fashioned concepts of *forerunner* and *anticipation*. At the level of specific

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theoretical features of a theory, these concepts make no sense, because specific features are untranslatable. At the level of general conceptual models, we find persistence or recurrent re-emergence of themes or models: therefore nobody can claim to have *anticipated* a long-term tradition.

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