On the various historical accounts on statistical mechanics

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Abstract: The theory of statistical mechanics attracted the attention of the historians of Physics since it was innovative with respect to the Newtonian paradigm. A review of all past historical accounts is presented. Owing to the supervenience of a deeper insight on the foundations of such a theory, the previous historical appraisals result to be no more adequate. Hence, this case-study constitutes a stumbling block for interpreting the history of Physics of the times after Newton’s mechanics. A new account according to a new foundational viewpoint is here sketched. Contrarily to what the founders and the subsequent historians of physics thought, this theory results to be relied upon Lazare Carnot’s mechanics, which is incommensurable with Newton’s mechanics.

Keywords: Statistical mechanics, Historical accounts, Boltzmann, Foundations, Models of scientific theory, New historical account, Carnot’s mechanics

1. Introduction

Which physical theory as first marks a radical change with respect to Newton’s theory? According to the mainstream opinion in history of Physics, the new non-Newtonian theory which born as first, thermodynamics, was a so phenomenological theory – a ‘Baconian’ theory, as it was evaluated by Kuhn (1977) – to be at all inadequate for a comparison with the glorious Newtonian theoretical paradigm. The subsequent electromagnetic theory seemed to agree with the Newtonian paradigm, because Maxwell has founded it according to a model of mechanical vortices and subsequently he has derived it from the Lagrangian function, which, according to most scholars, is a merely technical prolongation of the Newtonian paradigm. It was thus the theory of statistical mechanics (StM) which through Boltzmann’s efforts for suggesting a new theoretical framework – including the phenomenological thermodynamics –, has represented a so decisive change with respect to the Newtonian paradigm to eventually prepare – according to an accredited opinion – essential parts of quantum mechanics, i.e. the subsequent theory of quanta and then the quantum statistics.

Past studies on the history of StM have exhausted the first phase of a historical investigation, i.e. to collect all relevant historical materials. Moreover, the subsequent several historical descriptions of the main events did not present important discrepancies. Instead, no common agreement exists among the accounts on the historical development of StM.

They are analysed by the present paper. I leave aside the externalists accounts as in influencing the present debate and also the accounts suggested before the recognition of the existence of the molecules by Perrin in the year 1908.

2. Internalist historical accounts whose focus is a conflict: Boltzmann as a scientific champion

According to the externalist historians the history of science is determined by social conflicts aimed to gain the political power. Also some internalist historians have conceived the history of science as determined by conflicts, in this case of scientific nature. Surely, in the history of Physics the birth of StM has ignited several disputes. First of all, about the atomistic view; furthermore about other divergences from the Newtonian paradigm: hard/elastic bodies, the conservation of energy or not, the introduction of probability in physics, the incredibly disordered motion of innumerable particles, the notion of reversibility/irreversibility, the notion of entropy, the direction of time.

One of the first historical accounts[1] of StM is Brunschvigc's one. (Brunschvigc 1922, pp. 365-376) He recalls negative Comte's appraisal on the atomic hypothesis of kinetic theory of gases (KTG), and even more on the use of probability. The positivistic philosopher had evaluated this theory "as one of those purely speculative exercises which resulted only in postponing and obstructing [my] reform of physics philosophy".

Brunschvigc considers this opposition as an 'anthropocentered dogmatism'. (Brunschvigc 1922, p. 371) But oddly enough, Brunschvigc seems to be reluctant to accept the subsequent victory of the atomic hypothesis; he considers that this theory was decisively proved not only owing to Perrin's evidence, but also owing to StM's introduction of the notion of probability, which subsequently produced useful results in science.

Since Boltzmann has intended his researches as a fight against contrary scientific opinions, some internalist historians of StM gave emphasis to his attitude; they have attributed to him a revolutionary role. Dugas' account (Dugas 1959) manifests this motivation through the title of his book, i.e. he wants to re-evaluate Boltzmann's works in order to encourage – through an historical example of a previous conflict in which a scientific minority eventually won – the contemporary minoritarian interpretation of quantum mechanics; just for this aim the preface is written by de Broglie, who supported an 'heterodox' interpretation of quantum mechanics.[2]

[1] Surely, the first relevant account was Ehrenfest (1911). However, it represents more an analysis on the foundational issues than an accurate historical account. (Klein 1978, p. 120)

[2] One more follower of this historiographic attitude is Hiebert (1971). It is curious that fifty years later, Renn (2008) has reiterated the same program, without mentioning the precedent studies and moreover without taking in account the new result of Bell's inequalities, which has radically changed that program which in

3. Kuhn's historical account of StM

Three historians – Koyré, Kuhn and Lakatos – have suggested new methods for studying the History of Physics. But few scholars attempted to apply these methods to specific case-studies.

Koyré's method is not directly applicable to the theories born after Newton's mechanics.[3] Kuhn (1969) was unable to apply his method – illustrated by his sketchy history of classical physics –, to the history of modern physics. Indeed, he had planned a book on the history of quantum mechanics. However, in the course of his preparatory work, he bounded its subject to the initial period of its development, i.e. the birth of quanta. (Kuhn 1978, Preface) Even in his particular case he renounced, with a great deception of the readers of this previous book, to make use of his interpretative notions (paradigm, anomaly, revolution, etc.).

However, Kuhn's new book had to deal with StM. His account is new and is one of the most relevant accounts. In particular, he attributes to Boltzmann a decisive role for the birth of the quanta. But he left open some questions. He recognises in Boltzmann's work some decisive failures. (Kuhn 1978, pp. 36-66)

Boltzmann several times has attempted to prove a continuous passage from mechanics to StM, in particular about the notion of entropy, which is the most divergent notion from the Newtonian paradigm. A crucial Kuhn's problem is to evaluate these attempts: Is the notion of entropy an essential innovation with respect to the Newtonian paradigm? Or, does this notion result – according to Boltzmann's plan – from a continuous theoretical passage? It is apparent that Kuhn remains in doubt because his account does not solve the following dilemmas: Has Boltzmann inconsistently made use of his theoretical tools, or was his work a scientific revolution, anticipating the further revolution of the birth of quantum mechanics, together with the quantum statistics? In the latter case, through which steps Boltzmann's revolution of StM was accomplished?

4. The debate about the applications of Lakatos' programs of research

Two scholars (Elkana 1974; Clark 1976) have applied to Boltzmann's program Lakatos' interpretative method of the programs of scientific research.[4]

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quantum mechanics is considered as a prosecution of Boltzmann's, i.e. the program of discovering the hidden variables of quantum mechanics.

[3] However, a relevant attempt to follow Koyré's method is declared by Scott. (1970, pp. V and XIV) This author analyses the contemporary environment of KTG's scientists. (Scott 1970, in particular ch. XII)

[4] A scholar remarked that "the gist of Lakatos's argument is the following. There are four major theories of rationality of scientific progress – each provides a theoretical framework for the rational reconstruction of the history of science […] . A. Inductivism – its internal history is alleged discoveries of hard facts and indicative generalizations. B. Conventionalism – its internal history is factual discoveries and the erection of pigeon-hole systems (theoretical networks). C. Falsificationism – its internal history depicts bold conjectures, improvements, and great negative crucial experiments. D. Methodology of Scientific Research Programmes (MSRP) – its internal history depicts the possibly never-ending rivalry…". (Elkana 1974, p. 245) These four theories parallel the four models of a scientific theory which will be presented in sect. 6; respectively:

Brush has summarised these results:

> Yehuda Elkana (1974) […] claimed that Boltzmann himself changed his views in the 1890s and no longer considered meaningful the question whether atoms really exist. […] Peter Clark (1976) argued that Boltzmann's original research program was "degenerating" after 1880 and therefore that it was reasonable (by Lakatosian criteria) for [the other] scientists to abandon it in favour of the rival programme based on macroscopic thermodynamics even though Boltzmann's programme had to be revived after the 1900 when the thermodynamic programme proved to be inadequate. (Brush 1983, p. 262)

But Nyhof (Nyhof 1988) has contested Clark's conclusions. He claimed that some philosophical objections were decisive for leading most scientists to disbelieve in and then abandon StM. Another historian, through a more accurate appraisal on the philosophical attitudes of the prominent founders of StM as well as more documentary novelties about the crucial role played by the specific heat anomaly, has suggested

> that in this episode science and philosophy were much more interconnected [than it is commonly suspected] and that [either Boltzmann's or Maxwell's] philosophy also influenced the internal development of [this] science. (de Regt 1996, p. 32) [Hence] both Clark and Nyhof fail to capture the complexity of actual theory (de Regt 1996, p. 60) [since] the scientific development of the kinetic theory of gas [read: StM] cannot be understood without taking in account the role of philosophy. (de Regt 1996, p. 31)

In conclusion, the scholars who applied Lakatos' method missed their target to adequately interpret the historical development of StM.

5. The fragile foundations of StM

After a long period in which historical accounts have all appraised positively Boltzmann's work, some scholars have contested this common view.

A recent review of the status of art is offered by Uffink's comprehensive paper. (Uffink 2004) He remarks that there is no agreement among the different accounts on Boltzmann's works, who in fact 'pursued many lines of thought', most of which generated treatments which are mostly incomplete. (Uffink 2004, sect. 1.3)

Even about the more general subject of the foundations of StM at present time there exists a 'dozen or so different schools' and moreover the present foundations are inaccurate. A scholar wrote:

> In statistical mechanics particularly, precision is an elusive goal. It is safe to say that a major portion of non-trivial results in statistical mechanics has been derived from inconsistent formulations. (Grad 1967, p. 49)

PO&PI, AO&PI, PO&AI, AO&AI. According to this parallelism Lakatos has closely approached the four models of a scientific theory, although in the particular case of the dynamical process of theory-research.

A more recent paper added:

> The first thing to say is that one comes away from this collection [of papers] with the impression that the conversation about the foundations of statistical mechanics is still very much in its infancy; there seems to be no general and stable consensus among the investigators represented here even about how some basic statistical-mechanics terminology is to be understood, or about what the central foundational problems of statistical mechanics are, or about what might or might not count as solving them. (Albert 2010)[5]

No surprise if past historians of StM followed divergent research directions. Uffink (2004) classed them in three groups (the scholars marked with * are added by myself):

1. Authors describing Boltzmann as a brilliant and conclusive researcher: Externalist ones*, Dugas*, Renn*.
2. Authors biased by prejudice, or confusing, or misguiding: von Plato, Lebowitz, Kac, Bricmont (1996), Goldstein.
3. Critics: the two Ehrenfest, Brush, Klein, Truesdell and Muncaster, Sklar*. (Uffink 2004, sect. 1.2)

I leave aside those authors who in some way did not centred the subject for rather taking in account the "critics" only: Ehrenfests, Truesdell and Muncaster, Sklar*. I quickly summarize their criticisms as follows.

After an investigation on the theory through sophisticated mathematical tools, Truesdell, Muncaster (1980) list four "open questions":

1. Do there exists and are unique the positive, classical solutions of the Maxwell-Boltzmann equation?
2. Which assumptions for the asymptotic trend to a grossly determined state?
3. Which interpretation of the H-Theorem and which bearing on the trend to equilibrium?;
4. Which asymptotic status of the Stokes-Kirchhoff theory? (Truesdell, Muncaster 1980, pp. 559-565)[6]

At the end of a detailed philosophical analysis on the problems arising from the introduction of probability in theoretical physics – i.e. the statistical explanation, the

[5] Lavis (1977) suggests an interesting, comprehensive graphic. He characterizes the inner structure of StM through four levels: 1° The mechanical system, considered either as a system with a large number of degrees of freedom or as an incompletely specified system. 2° The probability theory, considered under either a scientific viewpoint or a logical viewpoint. 3° The various methods: ergodic, ensemble, evolution, ignorance. 4° The microcanonical distribution, the canonical distribution, the thermodynamics system. (Lavis 1977, p. 256)

[6] One more radical criticism is the following one: "the molecular scheme employed… is not consistent with the principle of the Newtonian mechanics. […] *Maxwell assumptions regarding the molecular motions contradict the laws of analytical mechanics.* […] [This scheme] is a consequence neither of classical mechanics nor of the axioms of probability theory". (Truesdell, Mucaster 1980, pp. 102-103)

ergodic hypothesis, the inner asymmetry, the reduction of thermodynamics to statistical mechanics, the direction of time –, Sklar's authoritative book concludes:

> Anyone who has followed the debate from the days of Maxwell and Boltzmann to the present cannot but help be struck by the way in which the fundamental problems of the theory – the problems posed by its original discoveries and by the brilliant early critics – have remained as deep puzzles for over a century. Attempts at solving the profound quandaries at the foundations of statistical mechanics have led to some of the most innovative conceptual developments in physics. Furthermore, whole rich branches of mathematics, such as Ergodic Theory in all its present general glory, have been inspired by the need to find the right language and the right basic postulates to deal with the fundamental issues that arise when probabilistic reasoning is applied to the dynamics of systems. Yet despite the richness of the resources that have been developed and despite the immense clarification of the issues that has been obtained, the most basic questions of the explanatory accounts to be offered for the fundamental probabilistic posits of the theory and for the appearance of statistical temporal asymmetry in the world remain. (Sklar 1999, p. 420)
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> […] it is the author's view that many of the most important questions still remain unresolved in very fundamental and important ways. (Sklar, 1999, p. 413)

As a particular case, there exist four different limits of StM to thermodynamic. (No mention of this variety of limits by the handbooks, maybe in the aim to present a quick reduction of thermodynamics to the Newtonian mechanics).

Yet, Brush has contested this kind of presentation:

> […] one often encounters the claim "thermodynamics has been reduced to statistical mechanics". I consider it quite misleading. Thermodynamics is the science that deals with general relations between the thermal and mechanical energy of substances whose special constitutive properties are assumed to be known […] a theoretical calculation of the equation of state from statistical mechanics is not a derivation of reduction of thermodynamics itself. […] Yet this is what many philosophers present as the only concrete justification for the claim […] So far I have not found any philosophical discussion of the reduction of *thermodynamics* to statistical mechanics except for the problem of irreversibility, where the time asymmetry implied by the second law of thermodynamics is in question. Perhaps this is because the reduction is so far only a program, not a fact. But the philosophers of science do not seem to be familiar with the work that has actually be done in this area, in the 19^{th} century and recently. (Brush 1983, p. 260-261)

All these criticisms support de Regt's appraisal, already remembered in the above; without putting the right questions – the main task of a philosophy of StM – is not possible to have a satisfying account on the history of the StM. (de Regt 1996)

I conclude that no previous historical account adequately covers the novelty of StM with respect to the Newtonian paradigm.

6. A new appraisal through two basic dichotomies

In the historiography of Physics the interpretation of the history of StM, as a theory which is manifestly different from Newton's mechanics, represents a failure of not only the externalist historians of physics, but also the 'new historiographers'. Among the contributions of the latter ones, Kuhn's book on the black body theory represents the most relevant account on StM, although he did not apply his celebrated categories (Kuhn 1969) and moreover his conclusions are uncertain.

The new historiographers suggested a great interpretative power, but their various and spontaneous philosophical bases resulted to be capable to recognize neither an alternative theory to the Newtonian paradigm (not even the chemistry), nor a historical discontinuity before the year 1900, notwithstanding in 1962 Kuhn had announced scientific revolutions.

I suggest to study the history of StM through more complex but also more detailed categories. In previous writings I presented two dichotomies – respectively on the two kinds of infinity and the two kinds of organization of a theory – as the basic categories for a new interpretation of history of physics. (Drago 2001; Drago 2016)

Let us now recognize the basic choices of the Newton's mechanics. Its mathematics makes use of actual infinity (AI) and its organization deduces all laws from few principles-axioms (AO). Instead thermodynamics relies on an elementary mathematics, which makes use of potential infinity only (PI). By having a different basic choice (on the kind of mathematics), mechanics and thermodynamics are mutually 'incommensurable'; this phenomenon is manifested by the radical variations in meaning of the common basic notions. Hence, Boltzmann has compared two incommensurable theories. Lodschmidt's celebrated objection deals with rather than the notion of irreversibility, the essential incommensurability of the two theories, as it is manifested by the radical variation in the meaning of the basic notion of the time, either the continuous time or the after-then time.

Boltzmann's attention to the finite methods in mathematics led him to consider the choice PI as the most appropriate one. (Dugas 1959, pp. 25-29) This choice gives the same incommensurability as before; it gives reason for the great difficulties met by Boltzmann; he unnoticed to be dealing with radical variations in the meanings of several (mathematical) notions; for cause he tried several investigation paths for obtaining the wanted results.

StM relies on a mathematics which does not make essential use of AI. (Moreover it has an axiom-principle organization (AO), since is derived from a formulation of mechanics. Yet, StM's choice is for the alternative organization, i.e. the problem-based one (PO), when the theory is considered as the suggestion of a new method for reducing thermodynamics to a mechanical theory, or when the notion of probability is considered a basic methodological principle). Again its choice on the kind of mathematics differs from Newton's one. No surprise if its basic choices are very different from the Newtonian ones. (Drago, Saiello 1995, p. 117, Tables no.s 2 and 3) The ignorance of this incommensurability gives reason of the great difficulties of the philosophical investigations on StM, as remarked by de Regt.

Rather, already at the end of 18th Century Newton's theory had lost the monopoly of theoretical physics; the birth of Huygens-Leibniz-L. Carnot's (HLC) formulation of mechanics (together with Lagrange's one) has represented a crisis of the Newtonian paradigm. Later, in the middle of the 19th Century the birth of the KTG according to HLC's theory (Drago 2016) has deepened this unnoticed crisis. Hence, the reference formulation of mechanics for the notions and techniques of StM is L. Carnot's mechanics rather than Newton's.

Through considerations on chemistry's foundations, in a previous paper I has obtained Koyré's categories covering the classical theories which are alternative to Newton's mechanics, in particular L. Carnot's one: "Evanescence of the force-cause and discretization of the matter." (Drago 2001) It is manifest that these categories adequately interpret the basic notions of StM as well its theoretical attitude; hence StM is at variance with Newton's mechanics. Instead, historians of physics recognized in StM a mere prelude of the next crisis of quanta.

I conclude that since have ignored L. Carnot's formulation, past historical accounts are all defective, wrong or even misleading, exactly as previous critics of historiographies of StM independently concluded.

References

Albert D.Z. (2010). *Notre Dame Philosophical Reviews*, in Ernst G., Huetteman A. (eds.). *Time, Chance and Reduction. Philosophical Aspects of Statistical Mechanics*. Cambridge: Cambridge University Press [online]. URL: <https://ndpr.nd.edu/news/24477-time-chance-and-reduction-philosophical-aspects-of-statistical-mechanics/> [access date: 15/05/2016].

Bricmont J. (1996). "Science of Caos and Caos in Science", in Gross P.R., Levitt N., Lewis M.W (eds.), *The Flight from Science and Reason*. New York: New York Academy of Science, pp. 131-175.

Brunschvicg L. (1922). *L'expérience humaine et la causalité physique*. Paris: Alcan.

Brush S.G. (1976a). *The Kind of Motion we call Heat*. Amsterdam: North-Holland.

Brush S.G. (1976b). "Irreversibility and Indeterminism: Fourier to Heisenberg". *Journal of History of Ideas*, 37, pp. 603-630.

Brush S.G. (1980). *Statistical Mechanics and the Atomic Theory of Matter, from Boyle and Newton to Landau and Onsager*. Princeton: Princeton U.P.

Clark P. (1976). "Atomism vs. Thermodynamics", in Howson, C. (ed.), *Method and Appraisal in Physical Sciences*. Cambridge: Cambridge University Press, pp. 41-105.

de Regt H. (1996). "Philosophy of the Kinetic Theory of gases". *British Jornal for the Philosophy of Science*, 47, pp. 31-62.

Drago A. (1990). "I quattro modelli della realtà fisica". *Epistemologia,* 13, pp. 303-324.

Drago A. (2001). "The several categories suggested for the "new historiography of science": An interpretative analysis from a foundational viewpoint". *Epistemologia*, 24, pp. 48-82.

Drago A. (2012). *I quattro modelli della teoria meccanica*, in Toscano M., Giannini G., Giannetto E. (a cura di), *Intorno a Galileo: La storia della fisica e il punto di svolta Galileiano*. Rimini: Guaraldi, pp. 181-190.

Drago A. (2016). *The kinetic theory of gas was unwarily derived from Huygens-Leibniz-Carnot's formulation of mechanics*, in *Atti del Congresso Sisfa 2013* (to appear).

Drago A., Saiello, P. (1995). *Newtonian mechanics and the kinetic theory of gas*, in Kovacs L. (ed.), *History of Science in Teaching Physics*. Szombathély: Studia Physica Savariensia, pp. 113-118.

Dugas R. (1959). *La Thérmodynamique au sens de Boltzmann*. Neuchâtel: Griffon.

Elkana Y. (1974). *Boltzmann's scientific reaserach program and its alternatives*, in Elkana Y. (ed.), *The interaction Between Science and Philosophy*. Atlantic Highlands NJ: Humanities P., pp. 243-279.

Ehrenfest P., Ehrenfest T. (1911). *Begriffliche Grundlagen der Statistische Anfassung in der Mechanik* in *Encyklopädie der mathematischen Wissenschaften vol. 4, pt 32.* (Leipzig: Teubner). English translation (1959) *The Conceptual Foundations of the Statistical Approach in Mechanics.* Ithaca, NY: Cornell University.

Goldstein S. (2001). *Boltzmann's approach to Statistical Mechanics*, in Bricmont J., Durr D., Galavotti M., Petruccione F., Zanghi N. (eds.), *Chance in Physics. Foundations and Perspectives*. Berlin: Springer, pp. 39-54. Lect. Notes in Physics no. 574 [online]. URL: <http://arxiv.org/abs/cond-mat/0105242> [data di accesso: 15/05/2016].

Grad H (1967). *Levels of description in Statistical Mechanics and theory*, in Bunge, M. (ed.), *Delaware Seminar in Philosophy of Science*. Berlin: Springer, pp. 49-76.

Hiebert E.N. (1971). *The Energetic Controversy and the New Thermodynamics*, in Roller D.H.D. (ed.), *Perspective in the History of Science and Technology*. Norman: U. Oklahoma P., pp. 67-86.

Kac M. (1979). *Probability, Number theory, and Statistical physics: Selected papers*. Cambridge MA: MIT Press.

Klein M.J. (1973). *The Development of Boltzmann's Statistical Ideas*, in Cohen E.G.D., Thirring W. (eds.), *The Boltzmann's Equation*. Wien: Springer, pp. 53-106.

Kuhn T.S. (1969). *The Structure of the Scientific Revolutions*. Chicago: Chicago U.P..

Kuhn T.S. (1977). *Mathematical and experimental traditions in the development of the physical sciences,* in Kuhn T.S. (1977), *The Essential Tension*. Chicago: Chicago P., pp. 31-65.

Kuhn T.S. (1978). *Black-Body Theory and the Quantum Discontinuity*. Oxford: Clarendon P.

Lebowitz J.L. (1999). "Statistical Mechanics: A selective review of two central issues". *Reviews of Modern Physics*, 71 (2), pp. 346-357.

Mendoza E. (1961). "A Sketch for a History of the Kintetic Theory of Gases". *Physics Today*, 14, pp. 36-39.

Nyhof J. (1988). "Philosophical Objections to the Kinetic Theory". *British Journal for the Philosophy Science*, 39(1), pp. 81-109.

Plato J. von (1991). "Boltzmann ergodic Hypothesis". *Archive for the History of exact Sciences,* 42(2)*,* pp. 71-89.

Renn J. (2008). *Boltzmann and the end of the mechanicistic worldview*, in Gallavotti G., Reiter W.L., Yngvason J. (eds.). *Boltzmann's Legacy*. Zurich: European Mathematical Society Publishing House, pp. 7-23.

Sklar L. (1993). *Physics and Chance. Philosophical Issues in the Foundations of Statistical Mechanics*. Cambridge: Cambridge University Press.

Truesdell C. (1961). *Ergodic theory in classical statistical mechanics*, in Caldiorla, P. (ed.). *Ergodic Theories*. New York: Academic Press, pp. 21-56.

Truesdell C., Muncaster, R.G. (1980). *Foundations of Maxwell's Kinetic Theory of Gas*. New York: Academic Press.

Uffink J. (2004). *Boltzmann's Work in Statistical Physics*, in Zalta, E.N. (ed.). *Stanford Encyclopaedia of Philosophy* [online]. URL: <http://plato.stanford.edu/entries/statphys-Boltzmann/> [data di accesso: 15/05/2016].