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# When Historiography met Epistemology

## Duhem's early philosophy of science in context

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# *Introduction*

Physics as a definite body of knowledge, a definite academic training, and a definite profession, was the outcome of a historical process which took place in the second half of the nineteenth century. Until the first years of the twentieth century physics was practiced by scholars who belonged to various academic categories: mathematicians, physicists, engineers, and natural philosophers. If the emergence of physics as a definite academic discipline was a heritage of the late nineteenth century, the emergence of a new theoretical practice, and the settlements of chairs of theoretical physics were the most interesting outcome of that process. Late nineteenth century theoretical practice stemmed from the fruitful alliance between the tradition of mathematical physics and the most speculative side of the tradition of natural philosophy.[1]

In the debates on science which took place in France from the early 1870s to the early 1890s two main issues were at stake: determinism and reductionism. More specifically, the debate pivoted on two different although intertwined questions, namely how to combine the determinism of physical laws with human free will, and whether physiology and even psychology and sociology could be completely derived from the laws of physics. The most radical reductionism assumed that the emerging human and social sciences had to be based on natural sciences, and in their turn, natural sciences had to be reduced to Mechanics. In more general terms the debate focussed on what I might label "scientism", namely the claims that natural sciences represented the model for every reliable body of knowledge, and that social progress depended on and stemmed from scientific and technological progress.[2]

Theoretical physics, the history of physics, and meta-theoretical remarks on science were mutually interconnected in Duhem's actual praxis. In particular, he kept together what subsequently scholars split into two different subject matters, namely history and philosophy of science His theoretical design of unification between Mechanics, Thermodynamics and Chemistry, as well as his re-interpretation of the Aristotelian tradition on natural philosophy, could be pursued only by a scientist endowed with a deep mastery of physics, a wide knowledge of history, and a subtle philosophical sensitivity. The historical and epistemological remarks he had begun to publish systematically in the 1890s were subsequently collected in the book he published in 1906, *La théorie physique, son objet, et*

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[1] On the process of specialization and professionalization taking place at the end of the nineteenth century, see, for instance, Ross S. 1964, p. 66, and Morus I.R. 2005, pp. 3, 6-7, 20, and 53. In Italy and Great Britain physics was also practiced by scholars appointed to the chairs of mathematics. Until the end of the nineteenth century, at Cambridge and in Scottish universities, high mathematical physics was practised by scholars who held chairs of mathematics or natural philosophy. On the emergence of theoretical physics at the end of the nineteenth century, see McCormmach R. and Jungnickel C. 1986, vol. 2, pp. 33, 41-3, 48, and 55-6, and Bordoni S. 2008, pp. 35-45. On the concept of theoretical physics, see Boltzmann L. 1892, in Boltzmann L. 1974, pp. 5-11, and Boltzmann L. 1899, in Boltzmann L. 1974, p. 95. I am indebted to Massimiliano Badino for helpful discussions on the difference between actual theoretical practice and institutional theoretical physics.

[2] For the polysemy of the word "scientism", and its connection with the equally multivocal meaning of the word "positivism", see Paul H.W. 1968, p. 299, footnote 2.

*sa structure*. The most important papers he published on history and philosophy of science were hosted by the Belgian journal *Revue des questions scientifiques*.

Duhem was living in a period which was subsequently struck by the fall of the Second Empire, the war against Prussia, the defeat, the insurgency of the Commune, the ideological struggles on the laicism of the state, and the Dreyfuss case. He was a firm believer and, at the same time, an independent thinker: he disliked transforming scientific contents into apologetic arguments. He thought that the subtle connections among scientific practice, philosophical commitments, and religious faiths could be understood only starting from a clear separation between the three domains. He managed to catch the fruitfulness of some aspects of the Aristotelian tradition, both the theory of knowledge and natural philosophy, and at the same time, he refused to get uncritically involved in the revival of neo-Thomism.

# *1. Physics in the late XIX century*

The analysis of late XIX-century physics is a very demanding task, because both cultural and institutional transformations were involved. The emergence of a new theoretical practice and the settlement of chairs of theoretical physics should be distinguished, even though they overlapped to a certain extent. The academic recognition of theoretical physics was first achieved in German-speaking countries, although in a very contradictory way, but theoretical physics as an actual new practice in physics also appeared in France, Great Britain and then in Italy. We can mention Pierre Duhem and Henri Poincaré in France, Heinrich Hertz, Max Planck and Ludwig Boltzmann in German-speaking countries, Joseph J. Thomson, Francis G. FitzGerald, and Joseph Larmor in the British Isles, and Vito Volterra in Italy. Some of them had been trained as mathematicians, and some others were engineers. From the academic point of view, Poincaré and Volterra were mathematicians. J.J. Thomson and Larmor had passed the highly selective Cambridge Mathematical Tripos, even though the former had gained his first degree as an engineer. Among the first physicists who built up theoretical thermodynamics, William Macquorn Rankine and François Massieu had been trained as engineers, and held chairs of engineering in Scotland and France respectively. Josiah Willard Gibbs had also been trained as an engineer in the States, before undertaking his scientific specialisation in Europe. Duhem considered himself a physicist and a mathematician, although his physical theories were more appreciated by mathematicians than by physicists. Some of the above-mentioned characters can be labelled *theoretical physicists* because they were committed to both advanced mathematics and the most speculative side of natural philosophy.[3]

## *1.1. Theoretical practice and theoretical physics*

The hallmark of the new theoretical practice 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 could be labelled conceptual or theoretical: it dealt with principles, models, and patterns of explanation. That conceptual component, neither formal nor empirical, came to be 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 that conceptual

[3] With regard to the assessment of contemporary scientists, it is worth mentioning that, in 1898, the mathematical physicist Georg Helm classified Clauisus as "an outstanding representative of theoretical physics" ("ein hervorragender Vertreter theoretischer Physik"). See Helm G. 2000, p. 383 (Helm G. 1898, p. 343).

level. Conversely, a given set of phenomena could be consistently described by different theories.[4]

At this stage it would be useful to clarify the institutional aspect of the emergence of *theoretical physics*. Some years ago, Russell McCormmach and Christa Jungnickel wrote the history of the settlement of the first chairs of theoretical physics in the last decades of the nineteenth century. They explored German speaking countries and other neighbouring countries to a certain extent influenced by German cultural traditions. In Germany, where the institutionalisation of theoretical physics occurred first, the creation of extraordinary professorships for theoretical physics, mainly in Prussian universities, underwent a certain number of ambiguities. The authoritative study of McCormmach and Jungnickel deployed some of the ambiguities which accompanied that institutionalisation. The first professorships, they stated, "were created solely to support the ordinary professor of physics, not to acknowledge a new speciality". Moreover, those university positions "were planned as transitional positions for young physicists", as the first step towards a career in experimental physics. It seems that sometimes "theoretical physics" was looked upon as physics presented in a more sophisticated and complete way, including mathematical subtleties. In other words, theoretical physics as advanced physics or, simply, mathematical physics. Candidates were expected to show their skills in both the experimental and *theoretical* sides of physics, just as well as candidates to experimental positions were expected to. In some universities (Kiel for instance), on the contrary, "theoretical physics was recognized as a necessary speciality", endowed with a specific characteristic, "as a link between, and an enrichment of, mathematics on the one hand and the natural sciences on the other".[5]

In some way, this last feature actually supports the conception of theoretical physics as the integration of advanced mathematical physics and the tradition of natural philosophy. Nevertheless, in order to show how complex the emergence of *theoretical* physics in Germany was, the authors remarked that the appointment of Planck to theoretical physics at Kiel in 1885 implied that he "agreed to teach all of mathematical physics and, if necessary, to help out in experimental physics". Even more puzzling was the situation in some technical institutes, where the teaching "of 'applied' or 'technical' physics was left to the teachers of theoretical physics"; the authors specified that this happened "at several universities and technical institutes". [6]

The German institutional framework described by McCormach and Jungnickel shows how difficult a reliable historical reconstruction of theoretical physics as an actual scientific practice in the late nineteenth century is. The question is: are we able to single out one or more elements, in order to qualify European theoretical physics? Which original elements

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[4] A historical reconstruction of the new theoretical practice can be found in Giannetto E. 1995, pp. 165-6, Kragh H. 1996, p. 162, and Lacki J. 2007, p. 248. For a historical reconstruction from the point of view of an early twentieth-century scholar, see Merz J.T. 1912, p. 199.

[5] McCormmach R. and Jungnickel C. 1986, vol. 2, pp. 33 and 41-3. From the general historical point of view, it is worth mentioning that the institutionalisation of theoretical physics was contemporary with German political unification and the contribution of physics to the development of German industry. See McCormmach R. and Jungnickel C. 1986, vol. 2, p. 2.

[6] McCormmach R. and Jungnickel C. 1986, vol. 2, pp. 48 and 55-6.

emerged here and there in the last decades of the nineteenth century, even if in a puzzling way, and then became clearly identifiable?

Before the so-called Scientific Revolution, two intellectual traditions crossed the field of natural sciences: mathematics and natural philosophy. As Kuhn pointed out some decades ago, what nowadays we call "astronomy, statics, and optics" belonged to the tradition of mathematics: they required specific practices and languages, and "practitioners" could rely on "bodies of literature directed exclusively" at them. The body of knowledge dealing with other natural phenomena, "like heat and electricity", was within the scope of natural philosophy: in general, philosophical speculations on those subjects did not exclude some kind of practical observation or experience. If the motion of celestial bodies was studied in the context of mathematics, local motions, namely motion on the Earth's surface, were studied in the context of natural philosophy. Kuhn's historical picture did not exclude any kind of communication between the two traditions, as for instance the mathematical analysis of local motion, which was undertaken by some fourteenth-century scholars in Paris and Oxford.[7]

In some way, the distinction between the two traditions survived far into the nineteenth century, even though the processes that are sometimes qualified as Scientific Revolution led to a meaningful integration between the two fields. Indeed those processes involved a threefold alliance among the tradition of mathematics, the tradition of practical *arts*, and the tradition of natural philosophy. Both the speculative and empirical sides of natural philosophy underwent deep transformations: while Descartes put forward a new theoretical representation of the physical world, which merged with mathematics, skilful British experimenters set up the alliance between arts and natural philosophy, and marked the passage from the practice of making experiences to the practice of making experiments. During the nineteenth century, the mathematisation of what Kuhn called "Baconian sciences" or "Baconian fields" corresponded to a new implementation of the alliance between natural philosophy and mathematics. Starting from 1811, Fourier put forward a sophisticated mathematical theory of some thermal phenomena, and starting from 1821, Ampère put forward a detailed mathematical theory of electrodynamical effects. Shortly before, a new kind of abstract and highly mathematised physics had emerged: at the end of the eighteenth century, Lagrange had built up Analytical Mechanics, where the link between mathematical and empirical entities became quite complex. Even more complex became the relationship among mathematical space, physical space, and the abstract space defined by mathematical-physical parameters.[8]

In the last decades of the nineteenth century, in the context of an accomplished mathematisation of *Baconian* sciences, a further implementation of the alliance between mathematical physics and natural philosophy emerged. From here onwards I will simply

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[7] Kuhn T.S. 1976, pp. 5 and 8.

[8] Kuhn labelled "Baconian sciences" that field of natural philosophy which dealt with heat, electricity, magnetism, and other sets of phenomena where experimental investigations had actively been pursued in the decades which followed the so-called Scientific revolution, although no systematic mathematical theory had put forward. See Kuhn T.S. 1976, pp. 10-13. For some references to British experimenters, see Kuhn T.S. 1976, p. 12.

label it theoretical physics, without any further specifications on the distinction between actual practice and institutional aspects. Theoretical physics realized a more sophisticated integration between the recent tradition of Analytical Mechanics and the new theories of heat and electricity.

In this context the case of Italy is interesting: in the late nineteenth century, the existence of theoretical physics was acknowledged neither at the institutional level nor at the methodological level. There, mathematicians, following the tradition of applied mathematics, dealt with electromagnetic theories and developed sophisticated mathematical models for elastic or pseudo-elastic actions taking place in Euclidean or not-Euclidean spaces filled with aether. The theories outlined by Beltrami and Padova were the offspring of mathematicians deeply interested in physics; however, from the institutional point of view, they were definitely mathematicians rather than physicists.[9]

Poincaré, one of the chief protagonists of late XIX-century physics, made meaningful remarks on the new methodological attitude. In 1888, in his *Leçons sur la Théorie Mathématique de la lumière,* he pointed out that "many optical theories […] are available in order to explain optical phenomena and they all are plausible". He noted that "most of Fresnel's results are transferred without modifications to the electromagnetic theory of light", and that it was worthwhile taking into account such plurality of theories. In 1890, in his *Élecricité et Optique*, he remarked that two theories can be conceptually in contradiction and, at the same time, "useful instruments" for physical research.[10]

When he confined himself to mechanical theories, he stated that infinite mechanical explanations were consistent with a given set of phenomena. In particular, he found that the conceptual model making reference to two electric fluids was equivalent to the model claiming the existence of one electric fluid. Poincaré found that, in some way, meta-theoretical attitude and personal taste was at stake when empirical checks could not help us to distinguish between two different models.

> Entre toutes ces explications possibles, comment faire un choix pour lequel le secours de l'expérience nous fait défaut ? […] Notre choix ne peut donc plus être guidé que par des considérations où la part de l'appréciation personnelle est très grande.[11]

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[9] See Neri D. and Tazzioli R. 1994, pp. 21-31.

[10] See Poincaré H. 1889, pp. II, III and 2, in particular p. II: "Il serait dangereux de se borner à l'une d'elles; on risquerait ainsi d'éprouver à son endroit une confiance aveugle et par conséquent trompeuse. Il faut donc les étudier toutes et c'est la comparaison qui peut surtout être instructive." See also p. 2: "[La] théorie électro-magnétique conduit aux même résultats analytiques que la théorie des ondulations de Fresnel ; l'interprétation physique des formules seule diffère." See also p. XVII: "J'ai pris le parti d'exposer successivement deux théories complètes, mais entièrement différents." See Poincaré H. 1890, p. VIII: "Deux théories contradictoires peuvent en effet, pourvu qu'on ne le mêle pas, et qu'on n'y cherche pas le fond des choses, être toutes deux d'utiles instruments de recherches, …"

[11] Poincaré H. 1890, pp. XIV-XV. Maxwell, in his *Treatise*, had already pointed out that infinite mechanical models could account for "a given species of connexion between the motions of the parts of a system". See Maxwell J.C. 1881, vol. II, p. 428. Darrigol has recently framed Poincaré's pluralistic and evolutionary conception in the context of French science. See Darrigol O. 2007, p. 223 : "Cette conception plurielle, évolutive, et structurale de la théorie physique rompait avec la tradition française de mathématiques.".

In 1892, in a subsequent collection of lessons, *Thermodinamique*, he stressed the historical nature of scientific knowledge: the plurality of theoretical models had both synchronic and diachronic aspects. That plurality of "theoretical models and even metaphysical conceptions" made scientific enterprise definitely more interesting and meaningful. In the same year, Boltzmann published a paper, corresponding to the first essay of his *Populäre Schriften*. He did not say explicitly what theoretical physics was or should have been, but gave a historical account of the emergence of what he called a new "scientific method". Boltzmann's key word was "model": during the nineteenth century, in mathematics, there had been "the return from purely analytic to constructive methods and illustrations by means of models". Physics, as well as mathematics, saw the emergence of different kinds of models, and those models played an important role in theoretical physics.[12]

According to Boltzmann, the first stage in the establishment of "a sharply defined method of theoretical physics" corresponded to the models of matter and force put forward by "the great Parisian mathematicians", after the French revolution. The second stage corresponded to the application of microscopic models of matter in motion to explain the internal state of macroscopic bodies at rest; this stage was associated by Boltzmann to the names of Clausius and Maxwell. A further stage corresponded to the introduction of successful models in order to explain "biological forms and phenomena": Darwin's theory, according to Boltzmann, had realized just this kind of conceptual shift from *description* to *explanation*. At the same time, physics underwent a sort of internal *secession*, induced by the then widespread criticism of the concept of force. On the one hand, some physicists, like Kirchhoff and Hertz, "took a turn in the opposite direction", transforming physics "into a descriptive science properly speaking". On the other hand, others "were especially fond of the colourful wrappings of mechanical representation"; in other words, they, like W. Thomson, made use of detailed and expressive models, involving "steel, rubber, glue" and other machinery. Boltzmann saw also an intermediate methodology, wherein physicists made use of mechanical models, "seeing in their own excogitated mechanism not those of nature but merely pictures or analogies".[13]

I think that Boltzmann's historical reconstruction is reliable, even though I place the emergence of theoretical physics not at the beginning of the nineteenth century but just in that *secession* which took place in the second half of the century. I claim that, from the mainstream of mathematical physics, set up by *Parisian mathematicians*, theoretical physics emerged as the requirement of a new, more sophisticated relationship between conceptual models and mathematical structures. In other words, theoretical physics maintained a meaningful link with mathematical physics but, at the same time, at least to a certain extent,

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[12] See Poincaré H. 1892, p. XIV, and Boltzmann L. 1892, in Boltzmann L. 1974, pp. 5-7.

[13] Boltzmann L. 1892, in Boltzmann L. 1974, pp. 7-8. In 1897, he developed similar conceptions in the first volume of his *Vorlesungen über die Principe der Mechanik.* He though that some "unclarities in the principles of mechanics" were due to "not starting at once with hypothetical mental pictures". He realised that without those pictures or, more in general, without "any hypothetical features", a satisfactory scientific knowledge could not be attained. It was just "the use of pictures" that allowed the scientist to go "beyond an unsimplified memory mark for each separate phenomenon". He thought that mechanics, in particular, required "very special mental pictures from the outset", even though that method appeared as "the very opposite of the modern one". See Boltzmann L. 1897, in Boltzmann L. 1974, pp. 225-8.

involved a sort of independence between conceptual frameworks and mathematical structures.

This issue is highlightened in the second part of Boltzmann's *Schrift*. He pointed out three elements which contributed to establish the new trend: first, the introduction of "illustrative and tangible representations" of mathematical structures, second, the awareness that similar mathematical structures or "differential equations hold for the most various phenomena", and, eventually, the acknowledgement that equations could be "detached from the models" which had generated them. He found that the last element had been explicitly pointed out by Maxwell in his mature contributions to electromagnetism. At that stage, a sort of independence between mathematical structures and conceptual models was realized. The model could fall without dragging down the mathematical theory which he had triggered off.[14]

Consistently with the unifying power of mathematical structures, we see other instances of wide-scope designs of unification in physics. An early instance was offered by Rankine's design of abstract generalisation of Thermodynamics. In the last decades of the century, in Larmor's theories we find the unifying role played by an invisible entity like aether. We find Duhem's subtle interplay between mathematical, empirical, conceptual, historical and methodological aspects in the emergence of a physical theory. Moreover we find in Poincaré the legitimation of multiple theoretical approaches to a given set of phenomena, and therefore the possibility of multiple processes of unification. What all these physicists had in common was a sophisticated methodology of scientific practice: there was an original combination of confidence and disenchantment with regard to science.[15]

An instance of the actual practice of dissociating theoretical components from mathematical components in a physical theory is reported by Andrew Warwick in a recent book. Routh, one of the most successful among Cambridge's private tutors, used parts of Maxwell's *Treatise* as a textbook, but he taught the electromagnetism there contained "at least implicitly, in the form of an action-at-a-distance theory".[16] This fact shows that mathematical physics could actually be separated from theoretical physics, namely by the conceptual models giving meaning just to the mathematical component of the theory. As a consequence, mathematical and theoretical components could be independently accepted and taught. In some way, mathematical structures showed to be endowed with a meaning in themselves: that meaning could survive the rejection of the theoretical models they had stemmed from.

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[14] Boltzmann L. 1892, in Boltzmann L. 1974, pp. 9-11. See p. 11: "… the old hypotheses could be upheld only so long as everything went well; but now the occasional lack of agreement was no longer harmful, for one cannot reproach a mere analogy for being lame in some respects. […] In the end, philosophy generalised Maxwell's ideas to the point of maintaining that knowledge itself is nothing else than the finding of analogies."

[15] See, for instance, Boltzmann L. 1890, in Boltzmann L. 1974, pp. 33 and 35-6, Poincaré H. 1889, pp. II, III, and 2, Poincaré H. 1890, pp. VIII, and XIV-XV, Poincaré H. 1892, pp. XIV, and Larmor J. 1897, pp. 207 and 215.

[16] Striking enough, he "made no reference at all in his lecture notes to the field-theoretic approach adopted by Maxwell …, nor did he discuss the electromagnetic theory of light". (Warwick A. 2003, p. 307)

## *1.2. New technologies and social progress*

At the end of the nineteenth century, the emergence of theoretical physics was only one aspect of a wider transformation in the field of physical sciences. From the 1860s onwards, physics had experienced two important successes: in simplified terms, we could say that the first was internal and the second external to scientific practice. The former consisted in the systematisation of previous mathematical researches on heat and electricity. The latter consisted in the social success of science, which stemmed from recent technological achievements. Science had finally managed to realize, at least in part, Bacon's dream, and the myth of scientific progress emerged.[17]

We should analyse the two transformations separately. With regard to *internal* transformations, the second law of Thermodynamics and the concept of entropy let "the distinction between reversible and irreversible processes" emerge as "a basic feature in all natural events", as Cassirer remarked more than a half century ago. At the same time, "the Faraday-Maxwell field concept … stood in sharp contrast at the outset with the Newtonian idea of force". In other words, the new concepts of "electromagnetic field" and "entropy" challenged the explanatory power of the mechanical representations of the physical world.[18]

With regard to *external* transformations, the last decades of the nineteenth century saw the spread of electromagnetic technologies, which really managed to improve the everyday life of ordinary people, and changed the landscape of Western towns. In city streets, in private houses, and in factories, the sudden spread of electric light contributed to the emergence of new standards of life, and triggered off new conceptions of human progress. New electric technologies, and in particular electric light, led to the firm belief that social progress could only be brought about by scientific progress. The progress consisted in the spread of electric energy, electric lighting and telegraphy: by the end of the nineteenth century, a hundred thousand miles of telegraph cables connected the most important towns in the world, crossing mountains and oceans. Some contemporaries emphasized the new "century of electricity" emerging from the old "century of heat": electricity appeared as a more versatile source of energy, and more easily transferable. For the first time in the history of science, the emergence of new theories led to the emergence of new related technologies. Electromagnetic theories, which had emerged and developed since the 1830s, were immediately transformed into useful devices like electric engines and electric transformers. The latter, in particular, from the practical and symbolic point of view, represented a sort of electromagnetic implementation of mechanical lever: the possibility to equalize the effect of different weights by means of different arms transformed into the possibility to equalize the effect of different electric tensions by means of two different intensities of electric current.[19]

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[17] We could say that, in Kuhn's terms, there was some kind of revolution, even though no physicist was then claiming that he was making a revolution. Kuhn's historiographical theses are too well- known to be discussed here. See, for instance, Kuhn T.S. 1970, in Kuhn T.S. 1996, pp. 92-135.

[18] See Cassirer E. 1950, p. 85. The concept has been recently revived by Renn J. and Rauchhaupt U. 2005, pp. 31-2.

[19] The analogy was really put forward by the Italian mathematician and theoretical physicist Vito Volterra in 1907. See Volterra V. 1907, pp. 227-9.

In the same decades, the emergence of new theories of heat followed rather than predated the emergence of new thermal devices like the thermal engine. If we travel backwards along the history of science, we find that during the seventeenth century, when the so-called Scientific Revolution took place, the new science influenced and transformed the representations of the physical world and the relationship between theoretical and empirical practices. Nevertheless, the transformation did not manage to affect the material conditions of the life and habits of ordinary people. On the contrary, a widespread material transformation was the specific effect of scientific practice in the late nineteenth century. In some way, there was a revolution, namely the occurrence of meaningful events, which deeply transformed both the material and intellectual life. Nevertheless, physicists of the late nineteenth century never claimed that they were doing something revolutionary; only contemporary historians and observers acknowledged that a deep transformation was taking place, involving both science and social life. Even nowadays, historians rather than physicists look at that fin de siècle as a particularly meaningful stage.[20]

In Europe and in the United States of America, the actual and clearly perceived scientific progress gave rise to different social processes. A new ideology emerged, which might be qualified as *rhetoric of progress*. At the same time, nations and governments set up periodically great scientific exhibitions, where scientific progress conflated into the celebration of national strength and success. The first great scientific exhibition took place in London, at Hyde Park, in 1851 from 1 May to 11 October. The second was held in New York from 14 July 1853 to 14 November 1854. Paris in 1855, London again in 1862, and Paris again in 1867 followed. In 1889 Paris hosted another *Exposition Universelle* which was also intended as a celebration of the centenary of the French Revolution, in particular the storming of Bastille as the symbolic beginning of the uprising. The most impressive material representation of that world's fair was the Eiffel Tower, which had been completed in the same year, and was placed at the entrance of the fair itself.

Electromagnetic devices had their share of success in those international exhibitions. Electric energy was endowed with specific features, which made it a really new and better source of energy. First of all, it was easily transferable through long distances, and secondly it appeared as an actual clean energy when compared to smoke and offensive smells given out by steam engines and oil lamps. In 1891, the *Supplement* of a multi-volume *Encyclopaedia* of technology pointed out the essential feature of electric energy.

> En effet, l'électricité fournissant une lumière pure et fixe, ne chauffant pas et ne viciant pas l'air, constitue non pas un éclairage de luxe, mais un éclairage sain et salubre, et, par conséquent, véritablement de première nécessité. Détrônant le gaz pour cet usage, l'électricité ne le bannira pas de la maison : bien au contraire, elle lui ouvrira tout grand son débouché normal, qu'il n'a jusqu'ici envisagé que timidement et comme pis-aller, le chauffage.[21]

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[20] According to the four criteria for the existence of a Revolution in science, established by I. B. Cohen in 1985, we would not be allowed to speak of a revolution. See Cohen I.B. 1985, chapter II

21 Lami E.O. (ed.) 1891, p. 743. On the effects of the widespread telegraphic net, see Galison P. 2003, pp. 174-80.

In the late XIX century the history of technology became tightly linked to the history of theories, and from then onwards the links have never slackened, even though we see that the two histories proceeds at their own pace. It is worth stressing that, in those decades there was a dramatic increase in theoretical debates and, at the same time, a dramatic increase in technological applications. For the first time in the modern age, physics took the lead of the cultural debate, and at the same time induced meaningful transformations in everyday life.

### *1.3. Meta-theoretical debates*

It is worth remarking that the emergence of theoretical physics also corresponded to a new sensitivity to meta-theoretical issues: we find explicit designs of unification, explicit methodological remarks, explicit debates on the foundations of physics, accurate historical reconstructions, and original historiographical frameworks. 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: that meta-theoretical commitment emerged from the actual scientific practice, as a sort of new awareness.[22]

In the late XIX century, methodological debates did not resemble previous philosophical debates on the nature and boundaries of natural knowledge. They emerged in close connection with actual researches undertaken in the fields of physical sciences, and were looked upon as a sophisticated component of the actual scientific debate. As Cassirer remarked more than half a century ago, in the first decades of modern age, science had fought over its own existence: scientists had got involved in harsh quarrels with philosophers and thoelogians. In the late nineteenth century, after having won their struggle for existence, scientists brought conflict and competition within the boundaries of science, in particular inside the boundaries of physics.[23]

With regard to meta-theoretical debates, two different models of scientific knowledge were at stake. On the one hand, we find the attempt to go beyond the shield of visible phenomena, in order to catch their hidden or microscopic nature. On the other hand, we find mathematical representations, without any attempt to pursue subtler investigations on the intimate nature of matter, aether, and physical processes. We find the British Larmor, J.J. Thomson, FitzGerald and Oliver Lodge, but also the Continental Hendrik A. Lorentz and Boltzmann deployed on the first front. On the second front we find Gustav Kirchhoff, Ernst Mach, and the *energetists* Georg Helm and Wilhelm Ostwald. Among those who swung from one to the other meta-theoretical options we find Hertz and Planck: they followed

[22] See Cassirer E. 1950, pp. 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."

[23] Cassirer E. 1950, p. 84: "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."

Rankine, Maxwell, Clausius and Helmholtz's similar attitude. Some debates involved Helm against Planck, and FitzGerald against Ostwald.[24] Poincaré looked on the two complementary attitudes with Olympian detachment. Boltzmann, Poincaré and Duhem clearly described the two meta-theoretical attitudes: *explanations* by means of specific mechanical models on the one hand, or *descriptions* by means of a formal language on the other. Although Duhem spoke against the mechanical models intensely exploited by British physicists, the role of theory and meta-theory was so important in his actual scientific practice that we cannot put him beside Mach, Helm or Ostwald without some specifications on *Energetism* and the struggle against *Mechanism*.

In 1876, G. Kirchhoff, after he was appointed to the chairs of mathematical physics and then theoretical physics in Berlin, published the first volume of his four volume masterpiece, *Vorlesungen über mathematische Physik*. In the introduction, he claimed that physics, in particular mechanics, could not aspire to the *explanation* of the physical world, but had to confine itself to mere *description* of phenomena. Scientists had to confine themselves to "how phenomena take place, without inquiring into their causes". He claimed he was only interested in a pure description, based on the concepts of "space, time and matter" and carried out by means of "pure mathematics": the concept of force was considered as an auxiliary concept, devoid of any deep physical meaning: in no way could it be associated to the concept of cause.

> Aus diesem Grunde stelle ich es als die Aufgabe der Mechanik hin, die in der Natur vor sich gehenden Bewegungen zu beschreiben, und zwar vollständig und auf die einfachste Weise zu *beschreiben*. Ich will damit sagen, dass es sich nur darum handeln soll, anzugeben, *welches* die Erscheinungen sind, die stattfinden, nicht aber darum, ihre *Ursachen* zu ermitteln. Wenn man hiervon ausgeht und die Vorstellungen von Raum, Zeit und Materie voraussetzt, so gelangt man durch rein mathematische Betrachtungen zu den allgemeinen Gleichungen der Mechanik.[25]

Ernst Mach went much further, exposing physics to a deep analysis, which was at the same time logical, conceptual and historical. He claimed that every class of phenomena could undergo a plurality of explanations and, in addition, that explanations had changed over time (and will change over time), in the course of the history of science. In 1872, in his first important book, *Die Geschichte und die Wurzel der Satzes von der Erhaltung der Arbeit*, he stressed the importance of history in scientific knowledge: "even though we could learn from history nothing else than the variability of points of view", he wrote, "this would be really precious". The physical knowledge was historical in its nature and, at the same time, it involved a plurality of interpretations, as it happened in every kind of human intellectual activity. Mach thought that history helped us to look upon science as "something neither static nor complete".

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[24] In Germany the debate was quite sharp, mainly in 1895, at the annual conference of German scientists and physicians held in Lübeck.

[25] Kirchhoff G. 1877, "Vorrede", p. III. See also p. IV: "… die Einführung der Kraft hier nur ein Mittel bildet, um die Ausdrucksweise zu vereinfachen, um nämlich in kurzen Worten Gleichungen auszudrücken, …"

> In der That, wenn man aus der Geschichte nicht lernen würde, als die Verhänerlichkeit der Ansichten, so wäre sie schon unbezahlbar. Von der Wissenschaft gilt mehr als von irgend einem andern Ding das Heraklit'sche Wort: 'Mann kann nicht zweimal in denselben Fluss steigen.' Die Versuche den schönen Augenblick durch Lehrbücher festzuhalten, sind stets vergebliche gewesen. Man gewöhne sich also bei Zeiten daran, dass die Wissenschaft unfertig, veränderlich sei. Wer nur eine Ansicht oder eine Form einer Ansicht kennt, glaubt nicht, dass je eine andere dagewesen, glaubt nicht, dass je eine andere kommen wird, der zweifelnt nicht, der prüft nicht.[26]

According to Mach, physical researches had to follow a phenomenological approach: physics had to deal with phenomena and relationships among phenomena. We can only understand "phenomena by means of other phenomena": he claimed that even the description of physical events in terms of space and time was, in the end, a description in terms of optical devices and astronomical rotations. That concept was repeatedly stressed by Mach in different times and different books, papers and lectures. He looked upon science as a sophisticated tool, which could bring some kind of order in our mind: that order was nothing else but the process of knowledge. Therefore the most essential feature of science was its usefulness for the mind of researchers: by means of that order, or "economy of thought", they become able to spare time and intellectual efforts.[27]

Nevertheless, in the context of Mach's methodology, *economy* did not mean a sort of synthetical collection of phenomena and laws, in accordance to a trivial phenomenology; it required an intellectual performance, and in particular a search for connections. Scientists had to "find, then, what remains unaltered in the phenomena of nature, to discover the elements thereof and the mode of their interconnection and interdependence". *Economy* involved some kind of theoretical activity, devoted to "make the waiting for new experiences unnecessary". Mach considered science deeply committed to unification; one of its aims was "discovering methods of describing the greatest possible number of different objects at once and in the most concise manner".[28]

Mach took part in the debate on the foundations of physics in an original way: he thought that mechanics was neither the starting point of physics nor its general framework. What we call mechanics was nothing else but the last link in a chain of experiences put in some order by our laws; from this point of view he could consistently state "purely mechanical phenomena do not exist". Experiences and sensations concerned physiology; physiology, in its turn, dealt with chemical, thermal and electric phenomena, rather than mechanical. In brief, "purely mechanical phenomena" were mere "abstractions", which were put forward "either intentionally or from necessity, for facilitating our comprehension of things". We

[26] Mach E. 1872, in Mach E. 1909, p. 3.

[27] See Mach E. 1872, in Mach E. 1909, p. 35: "Das gegenwärtige Streben der Physik geht dahin, jede Erscheinung als Functionen anderer Erscheinungen und gewisser Raum – und Zeitlagen darzustellen. Denken wir uns nun die Raum – und Zeitlagen in den betreffenden Gleichungen in der oben gedachten Weise ersetzt, so erhalten wir einfach jede Erscheinung als Function anderer Erscheinungen." See also Mach E. 1883, in Mach E. 1960, p. xxiii and p. 7.

[28] Mach E. 1883, in Mach E. 1960, pp. 7-8. With regard to Mach's phenomenology, it is worth mentioning Cassirer's criticism. He pointed out the risk intrinsically connected to every phenomenology: a drift towards physiology and psychology. See Cassirer E. 1950, p. 101.

could say that physiology came before physics and, in physics, electromagnetic and thermal phenomena came before mechanical explanations.[29]

The relationship between scientific knowledge and the wide set of human experiences attracted the attention of other *fin de siècle* scientists. Planck, who did not share Mach's epistemology and subsequently had a sharp debate with him, wondered whether thermodynamics should be founded on mechanics or on experience.[30] He preferred a "more inductive approach" which he found consistent with "the present state of science". Nevertheless, that approach did not exclude a theoretical foundation: thermodynamics could also be based on mechanical or on electromagnetic foundation as well. He did not find the search for the most primitive entities and concepts so important: the most significant step was, in any case, the achievement of a real unification in the comprehension of nature.[31]

Generally speaking, the methodological tension between the phenomenological attitude and the attitude towards specific conceptual models was one of the main features of the debate which took place at the end of the nineteenth century. Those who practised a phenomenological approach opposed mechanical models, but mechanics, the target of the sharpest criticism of phenomenologists, was not abandoned. In reality, mechanics was one word endowed with at least two main meanings: Mechanics as specific mechanical models or machinery, on the one hand, and Mechanics as abstract and very general mathematical-physical structures, which could undergo a multiplicity of implementations, on the other. Some scientists did not relinquish the search for mechanical models in the context of electromagnetism and thermodynamics, whereas others stressed the usefulness of a very general approach on the track of Analytical Mechanics. Helmholtz, the *dean* of German physics, and W. Thomson, the *dean* of British physics, pursued different kinds of mechanism. In particular, W. Thomson claimed the necessity of specific mechanical models for an actual comprehension of physical processes. Electromagnetic phenomena, for instance, seemed to him too abstract and obscure without the help of the corresponding mechanical models. The electromagnetic explanation of light, in particular, seemed to him unsatisfactory, just because of the lack of a mechanical representation. The fact is that he found mechanical models as something more primitive and closer to the immediate understanding of the natural world than other conceptual models and languages. His *Baltimore Lectures* are the seat where that passionate commitment was more pointedly put forward: with regard to the electromagnetic theory of light, he complained that he could not

[29] Mach E. 1883, in Mach E. 1960, p. 596: "The production of mutual accelerations in masses is, to all appearances, a purely dynamical phenomenon. But with these dynamical results are always associated thermal, magnetic, electrical, and chemical phenomena, and the former are always modified in proportion as the latter are asserted. On the other hand, thermal, magnetic, electrical, and chemical conditions also can produce motions." See also p. 612: "Processes, thus, that in appearance are purely mechanical, are, in addition, to their evident mechanical features, always physiological, and, consequently, also electrical, chemical, and so forth."

[30] With regard to the debate between Mach and Planck, Planck's paper (the text of a 1908 public lecture held at Leiden University), which was published in 1909, Mach's answer, published in 1910, and Planck's subsequent paper, published in the same year, all appeared in the *Physikalische Zeitschrift*. See *Pys. Zeit*. 1910, XI, pp. 599-606, and 1186-90. See also "From the Preface to the first edition - April 1897", in Planck M. 1945, p. viii.

[31] See Planck M. 1897, in Planck M. 1945, p. ix.

"understand light as well as I can", and moreover he demanded that it should have been pursued "without introducing things that we understand even less of".[32]

### *1.4. Energy and Energetism*

In this context two words appear quite problematic: *energetism* and *mechanism*. With regard to the label *energetism*, Duhem gave it the meaning of generalized Thermodynamics, rather than the meaning of a world-view or general meta-theoretical commitment in favour of the concept of energy. We find a remarkable conceptual distance between Duhem and some upholders of *energetics* like Helm and Ostwald. If Duhem developed a sophisticated mathematical theory of thermodynamics, Ostwald developed a physical world-view wherein "the concept of matter, which has become indefinite and contradictory, had to be replaced by the concept of energy". In no way can the name of Duhem be associated to that kind of *energetism*.[33] With regard to the label *mechanism*, Duhem did not appreciate mechanical models, but relied on the structural analogy between Analytical Mechanics and Thermodynamics. He tried to build up a sophisticated abstract Mechanics, quite different from the mechanical models of British physicists. His theories could be qualified as a sort of *structural* mechanism: they were quite similar to Rankine's Energetics, where a generalised Mechanics merged with a generalised Thermodynamics.

In the last decades of the XIX century, the principle of Conservation of Energy appeared as a natural candidate for the process of unification of physics, and, a widespread debate focused on the concept of energy: it did not deal essentially with empirical or mathematical aspects, but the competition between two different theoretical streams. On the one hand, we find the traditional description of phenomena through space and time, by means of equations of motion and geometrical paths. On the other hand, some physicists claimed that only processes, transformations of energy and the corresponding numerical accounts made sense. The second theoretical option was then known with the name "energetism". Another query concerned the ontological status of energy itself: on the one hand, it could be imagined as a sort of substance, endowed with autonomous existence with regard to material bodies; on the other, it could be imagined as a sort of property or relation among material bodies. In Germany the debate was quite sharp, mainly around 1895, when the

[32] See the well-known passage of W. Thomson's 1884 *Baltimore Lectures*, in its original version, reprinted in Thomson W. 1987, p. 206: "I never satisfy myself until I can make a mechanical model of a thing. If I can make a mechanical model I understand it. As long as I cannot make a mechanical model all the way through I cannot understand; and that is why I cannot get the electromagnetic theory. Hence I cannot grasp the electromagnetic theory of light. I firmly believe in an electro-magnetic theory of light, and that when we understand electricity and magnetism and light, we shall see them all together as part of a whole." There is a slightly different quotation in Cassirer 1950, p. 115: the fact is that the version published in 1904 by the Cambridge University Press contains subsequent alterations.

[33] See Ostwald W. 1896, pp. 159-60. According to Anastasios Brenner, Ostwald's energetism represented a sort of "disproportional" answer to atomism (Brenner A. 1990, pp. 82 and 86). It is worth mentioning that, in the 1960s, the scientist Donald G. Miller wrote that Duhem "belonged to the community of energetists, together with Ernst Mach, Georg Helm, and Wilhelm Ostwald". See Miller D.G. 1967, p. 447. The warm relationship between Duhem and Ostwald cannot be interpreted as an agreement on the meaning and practice of Energetics. On their friendship, see Brouzeng P. 1981, vol. 2, pp. 226-8.

energetists set up the agenda at the annual conference of German scientists and physicians in Lübeck.[34]

In 1898 one of the main personalities of that conference, Georg Helm, pointed out the relevant features of a radical energetism. He considered energetics as the physical approach "*capable to a much greater degree than the old theories of adapting itself directly to our experiences*". His conception of "general theoretical physics" was so strict than he could accept "neither atoms nor energy nor any other such concept, but only those *experiences* which are immediately derived from groups of observations". Although energy was the key concept, he refused "to attribute substantial existence to energy", for he saw in it "a dubious departure from the original clarity of Robert Mayer's views". In the context of Energetism, another conceptual stream was represented by W. Ostwald. He spoke against the mechanical world-view and against the atomic models of matter: he claimed a strict phenomenological approach. At the beginning, even Boltzmann was interested in the new theoretical turn: he was among the organizers of the Lübeck conference, and contributed to the choice of the subject. Nevertheless, he could not share that sharp methodological commitment and opposed Ostwald and Helm's theses. The debate continued after the conference, through the pages of *Annalen der Physik*, between 1895 and 1896. Although Planck was interested in a phenomenological foundation of thermodynamics, in a paper published in 1896, he raised many objections to energetism: among them, the unclear distinction between reversible and irreversible phenomena. In general, he found that Energetism had no heuristic power, and it had reduced itself to an abstract speculation.[35]

In the same year, from the British Isles, FitzGerald, Professor at Trinity College, Dublin, qualified Ostwald's energetics as "unphilosophical as well as unscientific". FitzGerald emphasised the positive role of hypotheses and conceptual models in scientific enterprise; in other words, he emphasised the scientific value of his theoretical physics towards mathematical phenomenology, which despised those conceptual components. He thought that the scientist needed much more than a "dry catalogue" of facts: he needed, for instance, "a theory of gravitation" as well as "a hypothesis of natural selection". He feared that the commitment to a radical phenomenology stifled any meaningful intellectual performance. A specific bone of contention was represented by "the unexplained constitution of an ether" and, in general, the mechanical models for optics and electromagnetism. FitzGerald claimed the fruitfulness of conceptual models, in particular mechanical models, and addressed his sharp criticism to the core of Energetism. He depicted it as a regressive methodology, which attempted at explaining everything by means of a collection of different kinds of energy. That strategy seemed to him not so different from the strategy of those natural philosophers, who in the previous century had made use of sets of subtle fluids, which had to be

[34] On the importance of the Lübeck meeting as a "critical turning point in the fortunes" of Energetics, and on the different meta-theoretical attitudes of Helm and Ostwald, see Deltete R. 1999, p. 45.

[35] See Helm G. 1898, p. 362 (English edition, Helm G. 2000, p. 401). For the debate after the Lübeck conference, see Planck M. 1896, Helm G. 1895, Ostwald W. 1896, and Boltzmann L. 1896. See, in particular, Planck M. 1896, pp. 76-7: "Vor Allem hat die Energetik die Verschleierung des principiellen Gegensatzes zwischen reversibeln und irreversibeln Processen verschuldet, an dessen Heraus bereitung und weiterer Vertiefung nach meiner Ueberzeugung jeder Fortschritt der Thermodynamik und der Verwandtschaftslehre geknüpft ist." See also McCormmach R. and Jungnickel C. 1986, vol. 2, pp. 219-20. For a short account of the complex conceptual net involving Boltzmann, Ostwald and Planck's approaches to thermodynamics see Harman P.M. 1982, pp. 147-8. See also Cassirer E. 1950, pp. 96-7.

continuously updated.[36] FitzGerald did not go so far as to criticize his British colleagues, but it is worth remarking that, from the structural point of view, some British physicists pursued the same strategy: they represented new electromagnetic effects by adding new mathematical expressions for energy to the Lagrangian of the physical system under consideration.

However, energy had become a pivotal concept in all sections of physics. In Maxwell's theory, the interpretation of energy was closely linked to the interpretation on the nature of electromagnetic actions: the theoretical model of energy which Maxwell preferred was a consequence of the theoretical model of contiguous action which he supported. Afterwards, Poynting put electromagnetic energy in the foreground: invisible, transversal streams of energy were interpreted as the cause of visible electric currents. Other British physicists, like O. Lodge, emphasised the *substantialisation* of energy, namely a representation of energy akin to matter. In some way, energy, like matter, could spread through space and time: conservation of energy corresponded to the process of transfer from place to place in a finite time. The attention was turned to the propagating entity, namely energy, rather than to the medium through which the propagation took place, namely aether. Nevertheless, this did not cause the medium to be faded into the background.[37]

The substantialisation of energy was criticized by other British scientists, like Oliver Heaviside, as well as by German scientists like Helmholtz and Hertz. In a section on the conservation of energy, in a paper devoted to electromagnetic equations for bodies at rest, Hertz expressed his scepticism: he found questionable the concept of localisation of energy, and "the following of it from point to point". Nevertheless, that conception was warmly received in Germany by a young assistant of Helmholtz, Wilhelm Wien, and subsequently widely discussed by a *Privatdozent* of Karlsruhe university, Gustav Mie.[38]

In Planck's 1887 treatise on the Principle of the conservation of energy, localisation and *individualisation* of energy were as fundamental as its conservation. The theoretical model of streams of energy was not affected by the different hypotheses on the nature of the medium supporting that stream. According to Planck, a theory on the transfer of energy could dissociate its lot from the lot of whatever theory of aether. As he stated, "the fact that aether does not behave like solid, liquid or gaseous matter does not cause any difficulty to the infinitesimal theory".

> Und zwar ist es offenbar zunächst von gröβter Wichtigkeit, dass Wesen dieser Theorie vollkommen zu trennen von allen Hypothesen, mit denen man der Anschauung zu Hilfe

[36] See FitzGerald F.G. 1896, p. 441-2.

[37] See Poynting J.H. 1884 and Poynting J.H. 1885. I do not think that Poynting's substantialisation of energy opened the way to the desubstantialisation of aether. I think that the complex interplay among electromagnetic energy, aether and Faraday's tubes of force in Poynting and J.J. Thomson's theories led to a different kind of substantialisation of aether rather than an actual desubstantialisation.

[38] See Wien W. 1892 and Mie G. 1898. See also McCormmach R. and Jungnickel C. 1986, vol. 2, p. 224. On Hertz's critical account, see Hertz. H. 1890, in Hertz H. 1962, p. 220: "Considerations of this kind have not been yet been successfully applied to the simplest cases of transference of energy in ordinary mechanics; and hence it is still an open question whether, and to what extent, the conception of energy admits of being treated in this manner." Hertz displayed an interesting mechanical example in "Supplementary notes", at the end of the book (Hertz. H. 1890, in Hertz H. 1962, pp. 276-7). For a detailed analysis of this example, see Buchwald 1985a, pp. 41-3.

> kommt, die aber mit der Theorie an und für sich nichts zu thun haben. Die Schwierigkeiten, welche dabei unserem Vorstellungsvermögen erwachsen können, kommen durchaus nicht in Betracht; dass z. B. der Äther sich nicht so verhält wie einer der uns bekannten festen, flüssigen oder gasförmigen Körper, ist ein Umstand, welcher der Infinitesimaltheorie nicht die mindeste Verlegenheit bereitet.[39]

This indifference can be easily understood if we consider that the process of substantialisation, subdivision into elements and transfer of energy decreased the importance of the medium of propagation. The last passages of Planck's book focused once again on his "infinitesimal theory": the general theoretical model of actions propagating with continuity through both space and time seemed to Planck the new horizon of physics.[40]

It is worth noting that, in 1900, the German physicist Emil Cohn envisaged electromagnetic fields propagating through space, without any resort to aether: he explicitly stated that "we will avoid to speak of aether". As a consequence, he excluded "every molecular hypothesis, both mechanical and electrical, as well as every mechanical interpretation of electromagnetic processes" and decided to give up "all the consequences which can follow from such hypotheses".[41]

With regard to aether, its existence was not, in general, questioned, but its role in the representation of the physical world was twofold: as a primitive universal substratum, on the one hand, or as a medium among other media, on the other. The complex interplay between physical theories and general philosophical issues led some scientists to qualify the first representation as anti-materialistic. At the same time, after Ostwald's polemical address at Lübeck, devising mechanical models was censured as *materialistic* by the upholders of energetism, and their opponents censured energetism as *metaphysical*. I find that the debate involving Ostwald and Fitzgerald can hardly be translated into the opposition between *materialism* and *idealism*. FitzGerald could be qualified as an idealist or anti-materialist because of his *aethereal* world view, but his dynamical structures of aether (not so different from W. Thomson's vortex atom or Larmor's electron) might be qualified as materialistic machinery. If someone were fond of philosophical labels, I might say that FitzGerald and Ostwald arguably pursued two different, questionable kinds of anti-materialism.[42]

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[39] Planck M. 1887, pp. 245-6.

[40] See Planck M. 1887, p. 247: "Dann findet jede Erscheinung ihre vollständige Erklärung in den räumlich und zeitlich unmittelbar benachbarten Umständen, und alle endlichen Processe setzen sich aus Infinitesimalwirkungen zusammen."

[41] See Cohn E. 1900, p. 30: "Daneben noch von einem „Aether" zu sprechen, werden wir vermeiden. Wir schliessen nach dem Gesagten jede mechanische oder elektrische Molecularhypothese ebenso, wie jede mechanische Deutung elektromagnetischer Vorgänge aus, und verzichten damit auf alle Folgerungen, welche nur aus solchen Hypothesen fliessen können. Unsere Absicht bei diesem Vorgehen ist, zu untersuchen, wie weit man den Thatsachen der Erfahrung mit einem Mindestmaass theoretischer Annahmen gerecht werden kann." See also Darrigol O. 2000, pp. 260-1. In the 1890s, Ostwald sharply criticised the reality of aether. For some details on the role of aether in German scientific literature, see Kostro L. 2000, pp. 19-24.

[42] It seem to me that Kragh offered a good synthesis of the debate between Ostwald and Fitzgerald when he states that "FitzGerald agreed with Ostwald's anti-materialism, but, referring to vortex atom, denied that it implied anti-mechanism" (Kragh H. 1996, p. 85). On the historical reconstruction of some philosophical debates, see Merz J.T. 1912, p. 186, and Kragh H. 1996, pp. 64 and 67.

## *2. The philosophical debate on Science in the French context*

The intellectual landscape of France *fin de siècle* was crowded with different characters and different claims, and I can only confine myself to outlining a rich and branched debate. On the one hand we find some scientists, historians, and philosophers who relied on simplified epistemological and historiographical frameworks, and put forward an optimistic cult of human progress. On the other hand we find some philosophers who criticised determinism and reductionism. We also find other philosophers and scholars who pointed out how problematic some scientific concepts were, and how complex scientific practice was. A sophisticated point of view on science was put forward by scientists and philosophers who did not deny the effectiveness of scientific progress but were able to go beyond the simplified conception of scientific practice as an unproblematic alliance between mathematical and empirical procedures.

### *2.1. The emergence of anti-reductionism*

In 1874 the young philosopher Émile Boutroux published his dissertation, which was a remarkable piece of writing, even though it did not contain any specific reference to philosophical literature. The title of the book, *De la contingence des lois de la nature*, really expressed the author's main commitment: the epistemic status of scientific theories. From the outset he focused on two main issues: the relationship between scientific and philosophical knowledge, and the relationship between scientific statements and the human comprehension of the natural world. In a brief historical sketch, he claimed that in the "first stage" of natural philosophy ("science"), when human mind ("l'esprit") could rely only on its sensitivity ("se repose sur les sens"), the natural world appeared as a collection of "facts". In the following stage, when "a purely descriptive science appeared unsatisfactory", human mind aimed at "an explicative knowledge". Mere sensitivity could not help man to attain this target: the human mind entrusted the responsibility of "classifying, interpreting and explaining" empirical data to its more abstract skills. This improvement of natural philosophy had a price: an increasing "divergence" between natural philosophy and "real things".[43] The alliance between the order of mind and the plurality of experiences, between the multiplicity of facts and the unity of the natural "law" making reference to them, inevitably led to a paradox: the enlargement of the gulf which was intended to be bridged.

> En suivant cette méthode, l'esprit tend vers une conception du monde plus large que les précédentes. Le monde est une variété infinie de faits, et entre ces faits existent des liens nécessaires et immuables. La variété et l'unité, la contingence et la nécessité, le changement et

[43] Boutroux E. 1874, pp. 2-3.

> l'immutabilité sont les deux pôles des choses. La loi rend raison des phénomènes ; les phénomènes réalisent la loi. Cette conception du monde est à la foi synthétique et harmonieuse, puisqu'elle admet les contraires sans restriction, et néanmoins les concilie entre eux. [...]
>
> Et maintenant, cette conception elle-même est-elle définitive ? La science que peut créer l'entendement opérant sur les données des sens, est-elle susceptible de coïncider complètement avec l'objet à connaître ?[44]

According to Boutroux, a rationalist approach to the natural world relegated every specificity to a sort of "delusive world", and "free will" to the domain of an imperfect knowledge, where there was ignorance about "the causes of our actions". In some way, a rationalist approach led to "fatalism", both in the context of natural sciences and in the context of "psychology, history and social sciences". The Introduction of his doctoral dissertation ended with a question, or better hinted at a possibility: if "natural world exhibited a certain degree of actual contingency", then rationalism would not be "the final point of view" on the world. In the first chapter of his dissertation, Boutroux stressed the unbridgeable gap between the "purely formal" structures of logic, on the one hand, and "the experience", on the other. That gap did not allow us to transform "constant links" among real thing into "necessary links". He made use of a Kantian linguistic toolbox, where adjectives like "analytic", "synthetic" and "à priori" were at stake.[45] The ideal of the most radical rationalism would be the attainment of a single, very general law, which would encompass "all the laws of universe as specific instances", but any change in the least detail of the universe, would lead to a complete "disruption" of the universe itself, and for this reason Boutroux leaned towards a milder rationalism.

> On peut donc admettre la possibilité d'une nécessité de *fait* à coté de la nécessité de *droit*. Celle-ci existe lorsque la synthèse que développe l'analyse est posée *à priori* par l'esprit et unit un effet à une cause. Lorsque cette synthèse, sans être connue *à priori*, est impliquée dans un ensemble de faits connus, et qu'elle est constamment confirmée par l'expérience, elle manifeste, sinon la nécessité du tout, du moins la nécessité de chaque partie, à supposer que les autres soient réalisées.[46]

In the following chapter, Boutroux returned to the opposition between the stability of scientific laws and the variability of the actually experienced world. Not only did he find "impossible to attain an absolutely permanent law", not only was "the reality of change as evident as the reality of permanence, but an "abstract" law of causality could not account for "the universal intertwining", for the deep link between change and permanence, which was the basis of "life and real existence". Another issue at stake in Boutroux's dissertation deals with the explanatory power of classifications. Can we really claim that nature contains "a well-defined number of genera", and that "the presence or absence of specific features"

[44] Boutroux E. 1874, p. 4.
[45] Boutroux E. 1874, pp. 5-11.
[46] Boutroux E. 1874, pp. 15-16.

qualifies exactly these genera? Can we claim that "everything changes, apart from the law of change?" Boutroux suspected that the confidence in the laws of nature and a strict determinism led to a sort of "fatalism" or defeatism.[47] The complex relationships among empirical data prevent us from attaining a "natural classification", namely the perfect correspondence between actual experiences and abstract laws. Nature appeared to Boutroux as a battlefield, where "a radical contingency", a tendency towards "a fundamental state of dissemination and chaos", contrasted "the logical order", the tendency to converge towards "species and genera".[48]

The mathematisation of natural philosophy had introduced "a new, heterogeneous, and non-reducible element" in nature: continuity. A continuous space, a continuous time, and a continuous matter had led to the concept of continuous motion. The hypothesis of continuity had allowed natural philosophers to geometrise nature, but Boutroux turned upside down the Platonic myth of geometry, which had been a huge success at the dawn of modern science: geometry as the perfect model of an imperfect actual world. Boutroux looked upon geometrisation of nature as an impoverishment of reality, more specifically as an oversimplification of the luxuriant variety of nature, as "a negative feature" indeed.[49]

Mathematisation was nothing else but a simplified overview.

> Un tronc d'arbre qui, vu de près, est tortueux, paraît de plus en plus droit, à mesure qu'on le voit de plus loin. Quel besoin avons-nous de notions *à priori*, pour achever ce travail de simplification, et éliminer par la pensée tous les accidents, toutes les irrégularités, c'est-à-dire, d'une manière abstraite et vague, celles que nous voyons et celles que nous ne voyons pas ? Par là, sans doute, nous n'acquérons pas l'idée des choses supérieures à la réalité. C'est, au contraire, la réalité appauvrie, décharnée, réduite à l'état de squelette. Mais est-il donc si évident que les figures géométriques soient supérieures à la réalité ; et le monde en serait-il plus beau, s'il ne se composait que de cercles et de polygones parfaitement réguliers ?[50]

Boutroux acknowledged that mechanicism, namely the reduction of physics to matter ad motion, or "geometry and motion", had attained an apparent success, in the last decades. The mechanical theories of heat and light corresponded to a real scientific progress. Nevertheless, "the contingency of details" eluded the absolute determination of general laws, even in the domain of astronomy, where the revolutions of planets "do not reproduce exactly the same periods". Not to mention "the remarkable effects" produced by minor variations of position or weight around equilibrium, both in simple and complex natural

[47] Boutroux E. 1874, pp. 17-19, 30-31, 39, and 45.

[48] Boutroux E. 1874, pp. 46-7. See, in particular, p. 46: "Fort de l'idée des genres e des lois, l'esprit humain espérait remplacer les classifications artificielles par des classifications naturelles. Mais, avec le progrès de l'observation, telle classification, que l'on croyait naturelle, apparaît à son tour comme artificielle ; et l'on se demande s'il ne conviendrait pas de substituer à toute systématisation rationnelle le dessin pur et simple d'un arbre généalogique. Or, s'il est impossible de trouver dans la nature un rapport parfaitement constant ; si les propriétés et les lois les plus essentielles apparaissent comme indéterminées dans une certaine mesure : n'est-il pas vraisemblable que le principe même de la distribution des phénomènes en genres et espèces (lequel, dans son usage scientifique, n'est, en définitive, que la forme la plus générale et la plus abstraite des lois de la nature après le principe de la liaison causale) participe, lui aussi, de l'indétermination et de la contingence ?"

[49] Boutroux E. 1874, pp. 50-2 and 55.

[50] Boutroux E. 1874, p. 56.

systems: a disruptive avalanche could be generated by "the seed fallen from the beak of a bird on a mountain covered by snow".[51]

From a very general point of view, could matter and motion, and a handful of absolute laws, account for "the emergence of the universe"? More specifically, the reduction of thermal phenomena to motion could not account for the difference between macroscopic motion and "molecular motion": something else, "a new element" had to be added in order to catch that difference. Macroscopic motion could easily be associated with a visible effect, whereas the microscopic could not: that invisibility called into play "new and more sophisticated properties" of matter. Boutroux was familiar with the argument of mechanicists: if we knew "*all* the mechanical parameters" of a natural phenomenon, we would be able to predict it "with absolute certainty". He objected that the concept of the totality of mechanical conditions was an abstraction, and it was questionable whether something like "a finite number of totally determined mechanical conditions" really existed for a natural phenomenon.[52]

According to Boutroux, the reduction of nature to matter and motion was also undermined by discontinuities and qualitative transitions taking place in some physical and chemical processes.

> Le mouvement est susceptible de changer d'une manière continue : il n'en est pas de même de la transformation d'un état physique ou chimique en un autre. Quels sont les états physiques intermédiaires entre l'état électrique des pôles de la pile et l'état lumineux du charbon ? Les états physiques proprement dits peuvent-ils varier aussi peu que l'on veut, de même que leurs conditions mécaniques ? Enfin, n'y a-t-il pas des cas où le parallélisme semble effectivement violé, comme lorsque l'addition d'une faible quantité de mouvement transforme un phénomène chimique en phénomène lumineux et un phénomène lumineux en phénomène calorifique, ou fait passer un corps d'un état à un autre, c'est-à-dire produit brusquement un phénomène tout nouveau ?[53]

Even though scientists accepted a principle for conservation of "the physical action", it determined "the *intensity* rather than the *way*" of the physical transformation. It seems that Boutroux knew contemporary physics, and was aware of the debate raised by the emergence of Thermodynamics. From the cosmological point of view, he saw a dynamical universe, ruled by the competition between opposite trends. On the one hand, "an elementary cosmic matter" which condensed into stars "endowed with light and heat"; on the other, a process of "dissolution", which reduced stars to scattered "dust". Above all, he saw a complex universe, where "the contingency of general laws" generated "tiny variations" at the scale of "huge masses and long times", which were hard to be appreciated by human observers. According to Boutroux, the existence of living beings defied the mechanical world view: not only could the actions exerted by "physical and chemical forces" not account for the emergence of "complex living systems", but even for the existence of "elementary living

[51] Boutroux E. 1874, pp. 57 and 68-9.
[52] Boutroux E. 1874, pp. 70, 73, 76, and
[53] Boutroux E. 1874, p. 80.

matter". The "extreme instability of living systems" and their "hierarchical order", where "certain parts are subject to others" called into play "a new element, which could not be reduced to physical properties". That new element was characterised by "*individualisation*" and inner "*correlation*".[54]

In the hierarchy of the complexities of a human being, Boutroux found it legitimate to study "the physiological conditions of psychic life", as well as "the physical conditions of the organic life", and "the mechanical conditions of physical transformations", but without the possibility of a complete reduction of one level to another. Human consciousness could not be looked upon as "a phenomenon, or a property, or a function": it was "a sort of living mould", where "the whole universe can find place", in order to "undergo a metamorphosis". Even though the activity of the human mind could be quantified in terms of energy, the relative value of every thought did not depend on the amount of physical energy corresponding to it. There was a sort of incommensurability between the two levels. The differences in the physical activity of the brain could not account for the difference between "genius and madness". Moreover, in man, actions like "heroism and sacrifice" were able to "break the strongest resistances of his nature". Free will allowed "the human person" to act without being automatically "forced to submit his actions to a system which overtake him". Boutroux stressed that man's actions do not depend on the mere preservation of physical body: on the contrary, "the latter depends on the former".[55] It was just this specific behaviour of man which made so difficult a unified science for stones and men, and moreover the reduction of the latter to the former.

Making reference to a Peripatetic tradition, Boutroux stated that "superior forms cannot be deduced from the inferiors by way of analysis", because they "contain non-reducible elements": superior beings can receive their "*matter*" from the inferiors, but not their "*form*". He excluded any "exact correspondence" between "the superior world" and "the inferior worlds", which were subject to a mere "necessity (fatalité)". Modern science, mainly "deductive sciences", assumed the immobility of the laws of nature: it was an "abstract" science, which excluded "progress" and "decadence" which we observe in the real world. In some way, the opposition between stability and change corresponded to the opposition between quantity and quality. If "homogeneity and permanence" were assumed to be "*essential* and *absolute*" in the context of quantity, they became "*accidental* and *relative*" in the context of qualities.[56]

Boutroux imagined that the human mind, when put in front of the natural world, was under pressure from two sides: the formal structures of mathematical laws and logical procedures, on the one hand, and the wide set of natural events and experiences, on the other. The stability of mathematical laws was balanced by the contingent flux of becoming. What he termed "positive sciences" rested on "the conservation of being": they dealt with "change" as far as it was brought back to "permanence". A new principle, "a principle of

[54] Boutroux E. 1874, pp. 81, 84-5, 87, 89, 91, and 93.
[55] Boutroux E. 1874, pp. 113, 115, 132-3, 143 and 147-8.
[56] Boutroux E. 1874, pp. 151, 153, and 155.

creation" should balance the usual principles of conservation.[57] The natural world was in a state of dynamical equilibrium between preservation and transformation.

> On ne peut dire qu'une *partie* des êtres ou qu'une *face* des choses soient régis par des lois, tandis que les autres êtres ou l'autre face des choses seraient soustraites à la nécessité. Ce qui est vrai, c'est que, dans les mondes inférieures, la loi tient un si large place qu'elle se substitue presque à l'être ; dans les mondes supérieures, au contraire, l'être fait presque oublier la loi. Ainsi tout fait relève, non seulement du principe de conservation, mais aussi, et tout d'abord, d'un principe de création.
>
> L'être n'est donc, à aucun de ses degrés, connus jusque dans son fond, quand les sciences positives ont achevé leur œuvre. Il est connu dans sa nature et ses lois permanentes. Il reste à le connaître dans sa source créatrice.[58]

According to Boutroux, a theory of knowledge was in need of some kind of "*dynamical* sciences" to be put "*beside*" and "*above*" what he labelled as "static sciences", namely "positive" sciences. Not only had dynamical sciences to inquire into the "*nature*" of things, but also into their "*history*". This wider perspective allowed the philosopher to merge "liberty" with "necessity", and the "possibility of change" with the "possibility of permanence". In the end, with regard to "natural laws" emerging from the scientific tradition, Boutroux attempted to frame them into the "moral and aesthetical" structure of the world: scientific laws "would not possess an absolute existence". They represented a stage in the journey of knowledge: their "seeming" immutability corresponded to "the intrinsic stability" of the ideal model which had generated them.[59]

## *2.2. The emergence of naïve scientism*

Boutroux' sophisticated anti-reductionism was not the hegemonic attitude in the scientific community. In particular, we find a remarkable trust in a radical reductionism in scholars who contributed to the emergence of human sciences, a field of research where old intellectual practices like history and philosophy merged with new disciplines like psychology and sociology.

In 1878 Jules Soury, archivist and palaeographer with interests and serous studies in neurology and psychiatry, published a text which was something more than a booklet and something less than a book, *Jésus et les Évangiles*.[60] From the outset, in the first lines of the

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[57] Boutroux E. 1874, p. 158.

[58] Boutroux E. 1874, p. 159.

[59] Boutroux E. 1874, pp. 164-5, 167, 173, and 192-3.

[60] Soury received a degree in humanities in 1862, and in 1865 he began to attend the lesson of Jules-Bernard Luys and Auguste-Félix Voisin at "la Salpêtrière", a psychiatric hospital in Paris, which attracted many scholars from the whole Europe. In the meantime he attended the private lessons of the philosopher, historian, and scholar of Hebrew and Christian religion Renan, who had published his *Vie de Jésus* in 1863. In 1867 Soury became archivist-palaeographer, and began to publish on the journals *Le Temps*, *Revue des Deux Mondes*, and *Revue Scientifique*. For further information see Huard P. 1970, pp. 155-6.

*Foreword*, he claimed that he would have put forward a new, radical re-interpretation of Jesus Christ's practising and preaching: "after the god and the man" he was to take into account "the sick person" or "the patient" ("le malade"). If psychiatry had already emerged at the dawn of the new era, Jesus Christ would have been a psychiatric patient: he was so self-confident that he believed that "he was allowed to do everything" and "all belonged to him". Moreover "a perversion of his personal feelings" had emerged, in particular "towards his mother and brothers". The seat of these psychic perturbations was "the brain", and Jesus's brain was affected by "an inveterate engorgement". The physiological imbalance led to a series of corresponding psychical effects: "the strengthening of the imaginative faculty up to hallucination" and then "an extreme sensation of strength and power". At that stage, the patient Jesus was not connected to reality any more: thoughts had become "absurd and frantic". The author stressed that irritability was really exaggerated and could lead to explosions of violence.[61]

Soury did not question his thesis in any way: once it was put forward, it was pursued in a consistent way, without any detour or critical remark. The real Jesus was a clinical case, and it had to be looked upon as such. Moreover the psychiatric illness had its basis on physical transformations which the brain had undergone: the elusive behaviour of mind was nothing else but the automatic effect of material processes taking place inside the brain. According to this meta-theoretical assumption, the cause of "the partial or total disappearance of consciousness" could be found in "the consumption of elements of cortical brain". A proliferation of fat tissues progressively replaced ordinary cells in the brain cortex. Those parts of cortex "which managed to perform their functions suffered from blood congestion": once again, the psychical effect was "a more or less severe delirium". The illness could provisionally stop, and sometimes an apparent recovery could last for a while, but the final consequence could definitely be forecast. The progressive weakness of muscles and intellect, and the side-effects on liver and kidney, led inevitably to death. This would have been the end of Jesus, whether "Jewish people would have been badly advised, and Barabbas would have been crucified in place of him".[62]

Some historical remarks can be found here and there in Soury's book. That kind of illness, which he qualified as meningitis-encephalitis, was really "the illness of the present century" but its presence could be documented even in other centuries. The documentary evidence he put forward was based on a widespread logical misunderstanding. Cause and effect were inaccurately reversed. The cause was the anatomic and physiological alteration of the brain, and the effect was the emergence of peculiar ideas, which could be indifferently qualified as delirium or as political and religious commitment. Soury reversed the logical chain: the appearance of "political and religious passions were definitely as strong in Judah as in our country", and this fact showed that the physiology of the human brain could not have changed during the last two thousand years. The science of brain could rely on that persistence of essential features in both biological and psychological fields. Two main issues must be stressed: first, political and religious commitments could be identified with mental illness, and second, mental illness was the necessary result of a severe brain disorder.

[61] Soury J. 1878, pp. 7 and 9-11.
[62] Soury J. 1878, pp. 13-4.

The historical and logical short circuit was closed by the analogy between the turmoil which took place in Jerusalem some years after Jesus' death, and the *Commune* which had violently shaken Paris few years before.[63]

At this point genetics entered into play: Soury made reference in particular to family predisposition, and questionable chains of logical statements appeared once again. The apostle James, who was looked upon as Jesus' brother in a strict sense, shared the same religious commitment, and therefore was affected by the same brain disease. Soury stressed that this fact could "hardly be questioned". Among Jesus' relatives, ancestors and descendants, a parade of "fanatics, epileptics, suicides, and drunkards" could surely be found. Fortunately Jesus had kept himself "chaste as an ascetic", and he had not engendered children who might have been "idiot". Soury's language and concepts are quite sharps and disparaging, but they are based on a supposed objectivity, which set apart personal feelings. The ideological disdain stemmed from a radical reductionism, which was assumed as the hallmark of every serious scientific approach. He stressed "the importance of ecstasy and hallucination" in the life of "men who contributed to change deeply our concepts or the course of events". He seriously claimed that Islamism, after Buddhism and Christianity, emerged from the "visionary attitudes of an epileptic", but he acknowledged that the hallucinations of Jeanne d'Arc "had freed France".[64]

Soury's reductionist design was stressed in the following pages: "the origin of ideas and feelings" had to be found in "the physical and material structure of man". The nature of "the most advanced expressions of heart and mind" was neurological. Although his assumptions were quite radical, he claimed that he had not put forward "any hypothesis". He claimed he had confined himself to reading the Gospel without stretching the holy texts: he had only taken note of Jesus' portrait as emerging from the Gospel itself. His meta-theoretical naivety is really astonishing, but in some way he did not miss the point. If previous researches on Jesus' life and the emergence of Christianity had overlooked that supposed link between mental disease and religion, it was due to bias. As Soury himself stated, "we cannot find what we are not looking for". In other words, only the hypothesis that a religious practicing had a neurological basis allowed him to appreciate the supposed neurological basis of Jesus' practice and preaching. Obviously, only a specific theoretical commitment allows us to stress the importance of events which are in tune with our hypotheses. His meta-theoretical framework was consistent, apart from the statement that he had not put forward any hypothesis. At the end of the *Foreword*, the main hypothesis was stressed once again: visible "religious excitement" was the visible manifestation of "an injury of the nervous network".[65]

In the following *Introduction*, he insisted on the necessity for an objective appraisal, and he acknowledged that he was advancing on the same pathway which had been undertaken by Renan and other historians and philosophers. No disdain had urged him towards this kind of research: he was aware that Jesus was "one of the characters … who dominates the past and future of mankind". He acknowledged that the founder of Christianity had been one of

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[63] Soury J. 1878, p. 16.
[64] Soury J. 1878, pp. 18-21.
[65] Soury J. 1878, pp. 25-7.

the leaders "of our species", and the memory of the events which had studded his life had become the leading mythology of "the most remarkable part of mankind". The researches undertaken in the course of the XIX century had simply faded away "the immense shadow that the Son of God had cast on the world". The real, historical Jesus had slowly emerged from the mist of mythology, We could finally contemplate "the human features of a pale and thin *rabbi* coming from Galilee".[66]

It is worth remarking that, in Soury's historical-medical reconstruction, the old mythology gave way to a new mythology, and the new mythology rested on two meta-theoretical hypotheses. First, science was able to offer the true explanation for phenomena concerning body and mind, and second, physical processes could explain mental ones. The two pillars might be labelled *scientism* and *reductionism*. Even creativity, as well as other superior activities of human beings, stemmed from some kind of burnout. Moreover, creative and destructive activities, as well as health and illness, stemmed from the same physiological root.

> Quelles que soient, en effet, les idées du vulgaire sur la raison et la folie, la science démontre que toute faculté éminente de l'esprit, toute supériorité éclatante dans la science, dans l'art ou dans la vertu, résultent des influences croisées de l'hérédité et d'une suractivité de quelque fonction de l'organisme.
>
> Le bel et rare équilibre des fonctions physiologiques ne peut que nous donner une longue vie. Pour que le génie apparaisse, il faut que cet équilibre soit rompu. […]
>
> Outre qu'il n'est pas un entre nous qui n'eût des raisons de se réjouir d'être névropathique comme l'ont été Jésus, Socrate, Pascal, Newton ou Spinoza, - aux yeux du physiologiste, la santé et la maladie sont des vaines entités.[67]

Scientific *objectivity* required that some qualitative differences, which were extremely important in ordinary life, dissolved in front of scientific procedures and classifications. The difference between health and illness was definitely one of the most useful and convincing, but science had taught us that they were "merely two different conditions of life, which are ruled by the same laws". Even the difference between faith and scepticism was a difference in attitude, or better an anthropological difference, which stemmed from physiological processes taking place in the same brain. Nevertheless this did not prevent Soury from assessing the tradition of the Catholic Church: he saw "the inferiority of Catholic people of the new and old world when compared to Protestant nations". This specific mix between a supposed scientific objectivity and a sharply ideological commitment was one of the hallmarks of Soury's methodological approach. In reality, the intellectual mix was more complex, because he drifted to a sort of nostalgia for ancient times: his knowledge of ancient civilisation led him to claim the moral superiority of ancient people over modern ones. In the past he found "moral virtues which now we have lost", and in particular "those dignity, generosity, and naïf faith in the absolute from which saints and heroes emerged".[68]

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[66] Soury J. 1878, pp. 31-3.
[67] Soury J. 1878, pp. 34-5.
[68] Soury J. 1878, pp. 35-7 and 42.

In the concluding remarks of his Introduction, history and science merged into a wide-scope framework, which was at the same time a historiographical sketch and a scientist manifesto. Men had always found it hard to accept that their representations of divinity had much in common, and a historical genealogy could be put forward. If "the Gods of Mecca, Rome and Jerusalem stemmed from each other", the faith in Virgin Mary had its roots in the cult of the virgin mother Isis. At the same time, those holy characters were nothing more than a re-interpretation of more ancient religions in which the Sun and other planets and stars were worshipped. In any case, when Soury observed human history from the point of view of long-term physical processes taking place in the Universe, he realized that cosmic time was insensitive to "gods and hopes of mortal beings". Over time, nebulae had condensed, suns had flared, and then life had appeared: processes of generation and dissolution had continuously occurred as a manifestation of "the creative chaos of the eternal Universe". In the end, the only thing left was "the unity and the indifference of the whole".[69]

In the following chapters, the psychiatric analysis of Jesus' behaviour made way for the interpretation of historical events which had taken place in Palestine after Jesus' death. When he took into account "the wars of Judea", he praised the Romans because they searched for harmony and peace throughout the empire, and blamed Hebrew people for having resisted Roman attempts to restore the power of the empire on their lands. What strikes the reader is the series of adjectives which Soury used for describing the resistant Hebrews: the adjectives "deaf and unmoved" preceded the description of their faces as "wild, convulsive masks" and "monstrously misshaped by hate". That horde of "deranged people" had transformed their holy temple into "a shelter for outlaws, or better a lair for hyenas and jackals". Their crime consisted in their firm will "to remain Jewish", and the psychiatric side of that behaviour was qualified as "delirium" and "pestilence".[70]

We find here an explosive mixture of a simplified scientific reductionism, a conservative political commitment, and open anti-Semitic attitudes. The last two elements are further stressed in the following pages: not only was the resistance to Roman dominion qualified as "madness" but also as "a crime against civilisation". Hebrew people became "a set of semi-idiot rabbi" or "fanatical Pharisees". The skip from science to racism became even more audacious from the historical point of view: the Semitic Cartago and Jerusalem shared the same destiny because of the same anthropological impertinence. In the end, they were both crushed as a dangerous "hydra": emperor Titus' victory represented the "victory of civilisation on theocracy". The sack of the holy temple in Jerusalem was the triumph of "the ancient world", and at the same time the triumph of "our Aryan race". Not only were "Jewish people public enemies of empire", but also "a plague for the society".[71]

In the last pages of the book, when he came back to the emergence of Christianity, he focussed once again on the relationship between the cultural foundations of the Roman Empire and the body of beliefs and practices of the new believers. He remarked that "the

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[69] Soury J. 1878, pp. 46-7. It is worth remarking that he had started from a supposedly *objective*, scientific approach, and ended with loose cosmological remarks.
[70] Soury J. 1878, pp. 176 and 178.
[71] Soury J. 1878, pp. 178-81.

good and great emperors of the second century", namely Trajan, Adrian, Antoninus, and Marc Aurelius, had also been "the first and fierce persecutors of Jesus' religion". In reality, it was only during "the golden age of Roman liberal rulers" that "the age of martyrs" began. At the same time he could not deny that those emperors had been "the wisest and the most honest" in the history of Rome. The explanation was not so hard to find: Christian religion undermined the two pillars on which that society had rested for centuries. First, no overlap could be realised between the new religion and the already existing "religion of State", and second, the state could not accept the superimposition of "a different social structure". Christians lived inside the empire as "termites", which eroded the foundations of society from inside. Such a social practice stemmed directly from "the Semitic theocracy", which was transmitted to Christianity, which flourished throughout the Middle Ages until the end of the XIX century. In the times when Soury wrote, society was still doomed to fight against "the authority of Jewish tradition, and the claims of the Vicar of Christ".[72]

According to Soury, the transformations undergone by the Hebrew religion in the passage to Christianity, and the differences between them, were not as meaningful as the unbridgeable gulf between foolish traditional religions and the rational religion of State and Science.

After three years, in a book he devoted to the history of ancient natural philosophy, *Théories naturalistes du monde et de la vie dans l'antiquité*, he put forward a more sophisticated historiographical framework, where historicism merged with a sort of scientific determinism. He slackened his sharp scientism and his mythology of objective knowledge. He claimed that "all philosophical views have been necessary and legitimate at their times". They mirrored the different stages of the human mind, and they evolved together with the human mind. Even the most successful philosophical theory of the time would have followed the same fate: they excited and moved scholars, but our descendants would mercifully have smiled at their naivety.[73]

Soury looked upon atomism as the long-term conceptual stream which linked ancient natural philosophy to modern science, but atomism was the carrier of an intrinsic dichotomy. The materialists of the eighteenth century claimed that Democritus had based his natural philosophy on "the sole authority of physical perceptions and experience", but they had failed to catch the true nature of that philosophy. If we really assume that "there are only atoms and empty space", we rely on "two absolute and infinite entities": neither perceptions nor experience could help us discover absolutely elementary elements of matter or an infinite, immaterial extension. There was an unbridgeable gulf between the domain of abstract or mathematical representations, and the domain of empirical experiences. In some way modern science had deepened the dichotomy, because "the reduction of quality to

[72] Soury J. 1878, pp. 187-90. He also remarked that Renan himself, the great scholar of the emergence of Christianity, had remarked that no persecution had happened before Trajan.

[73] Soury 1881, p. 9. The book was nothing else but his doctoral dissertation in humanities, and was dedicated to Jules Renan. Between 1879 and 1881 Soury had been deeply involved in the settlement of a chair of History of religions at the *Collège de France*. In the end, although he had been backed by influential politicians, he failed to win the chair. For further details, see Huard P. 1970, p. 158.

quantity, and physics to mathematics" had realised "the most meaningful ideal of science", but that reduction could not be demonstrated to be an identity.[74]

More specifically, mechanicism, namely the reduction of natural phenomena to matter and motion, was in opposition to any empirical practice, which could only rely on the mediation of our senses. Soury saw experience as a sort of "jail", which contained and confined men "from birth to death"; on the other hand, mind could overstep those boundaries. Science was based on the alliance between experience, on the one hand, and abstract models stemming from mind, on the other: how could it cope with that dichotomy or intrinsic tension? Soury was aware that the tension could not be solved easily: he chose the concepts of "extension" and "solidity" as meaningful instances. It appeared to him that the two concepts were based on "our subjective experiences" of force and resistance, and in the end on "our muscular sensitivity". He reminded the reader that Boscovich and Faraday had assumed that the concept of matter was based on the concept of force. In other words, the concept of "atom" was reduced to the concept of "centre of forces". In its turn, it seemed that forces could be reduced to motions, but the question was: what is in motion? Invisible motions required invisible, "hypothetical atoms", and the process of reduction led to the starting point.[75]

In the end, Soury felt compelled to acknowledge that science was nothing else but an "ideal science": it could not elude "metaphysics". The assumption of invisible and questionable entities was a sort of necessity in scientific practice. At the same time, that necessity made science closer to the body of knowledge which was known as philosophy. Confining himself to early Greek natural philosophy, he acknowledged that Leucippus and Democritus' atoms, as well as atoms of the recent "materialist philosophy" were not structurally different from Elates' "being". They were "objects of faith" rather than consequences of experience. Both ancient natural philosophy and modern science had to make recourse to metaphysics: from this point of view, science had something in common with "idealism". This fact did not astonish Soury, because he knew that "man is the metaphysical animal par excellence". Neither "bodies nor minds in themselves" were directly approachable, and the mediation of human senses was necessary, as well as the mediation of mental abstractions. It was just starting from "experimental science" that men devised "ideal science".[76]

The following year, he published a book, *Philosophie naturelle*, which was in the track of an old tradition, natural history. It spanned over different subject matters, from the emergence of the first living cells to "the history of civilisation". In this book his philosophical options were expressed in a more cautious way. In the *Preface*, he came back to the foundations of science, and in particular the intrinsic tension between empirical and theoretical practices. He found that the landscape of human intellectual attitudes could be divided into three sets: the set of idle people, the set of followers of philosophical and

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[74] Soury J. 1881, pp. 9-11 and 14.

[75] Soury J. 1881, pp. 14-18. Three alternatives had always been at stake in the history of modern science: first, a continuous mediation between empirical and theoretical practices, second, the attempt to deny that intrinsic tension, as some founding fathers of modern science had put forward, or the predominance of one domain on the other.

[76] Soury J. 1881, pp. 19-20.

religious traditions, and the upholders of "scientific orthodoxy". Scientific dogmatism appeared to Soury as not so different from that of philosophy or religion. The conflict between conservative and innovative attitudes was particularly hard in the context of life sciences, where a dramatic clash between "old and new world views" was at stake. The debate involved "materialism", and specifically the new conception of life and mind as "specific features of matter" rather than "extra-physical forces". Once again, the series of questions was: what was matter? Could matter be reduced to force? And then could force be reduced to motion? And what was in motion? Once again, he saw an inescapable vicious circle, and the situation was worsened by the emergence of new kinds of matter and force like electricity. In the end, the intrinsic tension or "antithesis" between "world and consciousness" could not easily be overlooked. Nature and mind appeared as the two sides of the same coin: they were different from each other, and not reducible to each other, but complementary aspects of the same "reality".[77]

### *2.3. Sophisticated historical and epistemological frameworks*

The intellectual landscape of French speaking countries also included philosophers who were deeply interested in the latest developments of physics, and were interested in putting forward a sophisticated philosophical approach to science. In 1883 a Parisian publisher sent the book of a Swiss philosopher, Ernest Naville, to the printing press. The book dealt explicitly with physics and its history: the title *La physique moderne. Études historiques et philosophiques*, indicated that the analysis would have been both historical and critical. In the first lines of the short *Avant-propos*, the author expressed his main concern: although physics had fast developed in recent times, and had increased "the power of man onto nature", the foundation of those recent theories and their "philosophical consequences" had frequently been neglected. We must stress that, in that stage of the history of science, when the professionalization of scientific practice was accomplished, and the boundaries between the different sciences began to be firmly established, a reliable definition of "physics" could really be put forward. Naville separated physics "in its strict sense" from a broader field of physical sciences, which encompassed mechanics, celestial mechanics included, mineralogy, and some sections of geology. Making reference to recent debates on the boundaries between the different fields of science, he found questionable whether chemistry should be included into physics or had to enjoy an autonomous status. The study of "living beings was excluded": the field of physics started from its boundaries with "logic and mathematics" and ended at the dividing line with "biology", which had recently unified the fields of "botany and zoology". He stressed the suitability of the expression "modern physics", because every science had its own history, where different stages could be

[77] Soury J. 1882, pp. III-VIII.

identified. The adjective "modern" had obviously a relative meaning, because what appeared modern in a given time, would have become old in a subsequent stage of history.[78]

Naville found that the essential features of physics could be classified into three categories: "scientific, logic, and aesthetic". In the section he devoted to scientific features, he discussed five most meaningful "ways of considering the intrinsic nature of phenomena", and then he focussed on some specific differences between modern and ancient physics. He found that the first feature was "the mechanical nature of phenomena", namely their reduction to matter in motion. That reduction required a series of additional hypotheses: among them, the existence of "*molecular* or *atomic* motions" and "*aethereal* motions" besides the ordinary macroscopic "*mechanical* motions". In no way could the existence of aether stem from "direct perceptions": it was a hypothesis, and it was justified "just by its fruitful explicative power" in the context of optics and the theory of heat. The second feature was the unitary nature of all kinds of matter, which stemmed from chemistry, and implied that "the great variety of natural substances" could be looked upon as a combination of "a definite number of simple elements". This second feature was consistent with the first one: in some way it specified what kind of matter was in motion, and allowed physicists to overcome the traditional distinction among solid, liquid, and gaseous states.[79]

The third feature was labelled "the *transformation of motions*": Naville preferred his label to the most widespread expressions "*correlation of forces*" or "*transformation of forces*". He insisted on the concept of motion rather than force because the actual "experimental determination of force" was nothing else but "a real or virtual motion". The fourth feature specified that transformation required conservation, and in particular the conservation of both matter and motion: that principle of conservation implied a sort of symmetry between the two entities. When motion seemed to disappear, in reality it was transformed from the macroscopic into the molecular or aethereal kind. Naville interpreted energy as "the cause of real or virtual motions": this was the meaning of the two expressions "actual energy" or "living force", on the one hand, and "potential energy", on the other. It seemed questionable to Naville whether potential energy could be reduced to "an internal molecular motion". The last essential feature was in reality a meta-theoretical option, which had its roots in the Greek philosophical tradition: the possibility and necessity of a mathematical explanation of natural phenomena. Although mentioned as the last feature, we might say that it affected the other four, and offered them the suitable language.[80]

When he proceeded to explain the differences between modern and ancient physics, Naville stressed that the latter was "closer to direct experience" than the former. At the same time, in ancient physics he saw a lack of unity: for "every new fact … a new explicative principle" was required. On the contrary, the mechanical world-view corresponded to a really unifying trend in modern physics. Another powerful drive was represented by the

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[78] Naville E. 1883, pp. 1 and 3-4. He informed the readers that the first part of the book had already been published in 1872.

[79] Naville E. 1883, pp. 5-6 and 8-11. On the states of aggregation Naville wrote: "Pour la physique moderne, les états solides, liquides, gazeux sont considérés comme pouvant appartenir à tous les corps sans exception. La liquéfaction de l'oxygène récemment obtenue par MM. Raoul Pictet et Cailletet a confirmé cette manière de voir." (*Ibidem*, p. 11)

[80] Naville E. 1883, pp. 11-12, 14-16, 18-19, and 21.

sophisticated mathematisation of physical theories. The assessment of ancient science appears as commonplace rather than the result of a careful analysis, because the commitment to unifying all fields of physical sciences was a permanent process in the history of science, which survived the great transformations experienced by that body of knowledge. However Naville found that the fulfilment of the mechanical programme could not be completely accomplished: in reality it was "a delusion", because "the science of matter", differently from the science of motion, could not be reduced to mechanics. Here we find the consequence of the broad definition of physics he had put forward in the first pages of his book. If physics encompassed, at least in part, chemistry ("the science of matter"), the reduction of chemistry to mechanics was far from being fulfilled, if ever it could be. The same difficulty involved cosmology, which in a broad conception of physics could be included as "the most daring part of the complete programme of physics". In this case, "the distance … between the present state of science and the accomplishment of that programme" appeared to him really "immeasurable". In this disciplinary context, the success of the mechanical world-view depended on the choice of the dividing line between physics and other sciences.[81]

Of particular interest is the section on "the inertia of matter", where Naville discussed and criticised one of the most meaningful concepts of modern science, which also represented a dramatic watershed between ancient and new science. He found that the concept of inertia could offer a unifying conceptual framework for the whole body of knowledge of modern physics. Inertia encompassed both the concepts of matter and force: in the presence of two bodies, each of them represented "a force with regard to the other", in the sense of "the cause of a change in the motion". Matter could not be defined "independently of its connections" or forces which linked it to other parts of matter. Inertia was the "force … which opposed … the displacement" of matter itself, and as such it was "a real force". He quoted and endorsed Newton's passage "*Materiae vis insita est potentia resistendi*", that is "the inner power of matter consists of its possibility to resist". However he stressed that "inertia" was not a fact or piece of experience, but a concept, and the principle of inertia was a theoretical law. It was a peculiar theoretical law indeed, because it eluded any actual experimental check. The impossibility of an empirical detection depended on three specific, practical impossibilities. First, both in the astronomic and in microscopic fields, a real state of rest was impossible to be assumed and observed; second, the presence of gravitation implied the unquestionable existence of accelerations; third, an isolated body did not exist. Once again, something like the principle of inertia could not be justified by "direct observation" but by the fruitfulness of some "results corroborated by experience".[82]

Here we find an ambiguity which Naville overlooked: the principle of inertia could not be derived from experience; it was rather in contradiction with experience. At the same time, it could lead to consequences which were in accordance with experience. We have therefore two sets of experiences: those in accordance with the principle and those in contradiction with it. We should rely on experience, but the reply of experience was ambiguous:

[81] Naville E. 1883, pp. 22-4 and 28.

[82] Naville E. 1883, pp. 28, 32, and 35. The status of the principle of inertia as "a hypothesis" rather than "a simple induction" from experience, even though "a strongly confirmed hypothesis", was also stressed in a following section of the book. See *Ibidem*, p. 43.

moreover, a set of experiences was in direct contradiction with the principle, whereas the other set of experiences was in indirect accordance, namely at the end of a chain of deductions. Naville did not discuss the specific role of experience in scientific practice. A further, although minor, ambiguity stemmed from the impossibility of rest. In the macroscopic domain of celestial bodies, rest was really unobserved, but in the microscopic domain of the hypothetical molecules, rest was assumed to be impossible although it eluded any observation. Experimental and theoretical impossibilities obviously had a different status.

When he took into account "the logical features" of modern physics, he started from the dynamical condition of hypotheses: when a hypothesis received a certain "degree of … experimental confirmation" it could become a physical law endowed with a corresponding degree of certainty. The "degree of probability" of physical laws was a key-concept in Naville's historical-critical reconstruction. Concepts like *degrees* and *probability* were introduced in order to exclude that laws could be looked upon as "absolute laws revealing the eternal and necessary nature of things". The potential existence of specific facts in contradiction with a physical law required such a cautious attitude. In many pages he stressed "the experimental character of theories", and guarded against "the pretension to expressing an absolute and necessary order". The worst consequence of that pretension was a reductionist attitude, which had led "some scholars … to extended mechanicism to mankind", to "destroy the foundations of moral order", and to "disregard facts in contradiction with such theories". With regard to the necessity of avoiding that "dangerous delusion" he mentioned and quoted some passages from the physician and natural philosopher Robert Mayer, the mathematician Joseph Bertrand, and the authoritative physician Claude Bernard.[83]

It is worth remarking that, at that stage Naville did not point out specific differences between "laws" and "theories", and moreover he labelled "theory" the reductionist meta-theoretical attitude. It was only after having pointed out the necessity of an anti-dogmatic attitude towards science that he carefully specified the expressions "laws", "theories", and "principles". If laws were specifically identified with "experimental laws", which stemmed from "facts" and described them, theories aimed at their explanation. They dealt with "the nature of phenomena", whereas principles "led the thought towards the establishment of theories". Apparently a philosophical hierarchy led from the empirical to the theoretical and then to the meta-theoretical level, and ruled the three stages of the scientific enterprise. Nevertheless a loose interdependence rather that a strict relationship of dependence affected the three levels. A physical law did not necessarily required the formulation of a theory, and it was in some way independent of it: the existence and the validity of optical laws neither required a complete theory on the nature of light nor had led to the endorsement of "the emissive theory" rather than "the wave-theory". In the course of the deep transformations which had taken place in the history of science, those laws survived "the subsequent hegemony of the two theories".[84]

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[83] Naville E. 1883, pp. 41, 44-7, 50, and 52.
[84] Naville E. 1883, pp. 52-3

However theories represented the pivotal stage in scientific practice: "the elimination of theories would represent the elimination of science". There was a complex relationship between laws and theories: not only could laws trigger off theories but also cast doubt on them or even overturn them. On the other hand, not only could theory suggest new experiments but also "lead to the discovery of new facts and new laws". In its turn, the building up of theories was influenced by "guiding principles", even though their presence was not sometimes "perceived consciously". Principles ruled scientific practice, and corresponded to the meta-theoretical commitment to pursuing very general ideals like those "of order or harmony". Laws and principles could enjoy a relative stability: Newton's mathematical law of gravitation had survived the debates on the nature of gravitation, and different kinds of scientific practice had in common the search for order, unity and harmony. In contrast with the stability of mathematical laws and the persistence of regulative principles, theories represented the most problematic component of scientific enterprise: the history of science showed a continuous "fluctuation of theories", namely their emergence, success, fall, and potential re-emergence.[85]

Theories were placed between "the base" of "experimental laws" and "the top" of the ideal of "universal harmony": that "intermediate region" was the seat of those "changeable and provisional" entities. According to Naville, the history of science shows us the existence of "a continuous progress towards a deeper knowledge of the universal order", and theories were the most dynamical component in the pursuit of that progress. Physical laws corresponded to an empirical necessity, and transformed our knowledge into "an improvement of our material condition", whereas guiding principles corresponded to a rational necessity ("une confiance naturelle à la raison").[86] The dynamical process of emergence, development, and replacement of physical theories was the essence of scientific progress: it was just the caducity of theories which protected science from involution and decadence.

> Les théorie passent, la science demeure ; et comme la science n'est que la recherche de l'ordre et de l'harmonie, on peut dire que son principe est également confirmé par la naissance de des systèmes et par leur destruction. La naissance d'un système manifeste le besoin de l'esprit humain de trouver un ordre qui rende compte des faits. La destruction d'un système, provenant uniquement de son insuffisance, invite la pensée à la recherche d'un ordre plus élevé que celui qu'on avait conçu. Le spectacle de la succession rapide des théories qui se détruisent et se remplacent éveille naturellement le doute ; et croire nos théories actuelles à l'abri du souffle qui en a renversé tant d'autres serait une étrange fatuité. Mais l'histoire de la science, qui nous fait assister à la destruction des systèmes, nous montre qu'à un système détruit en succède un autre dont les conceptions sont plus solides et plus vaste.[87]

The section Naville devoted to what he labelled "the aesthetical features" of modern physics begins with an interesting remark on ancient science. He stressed that the latter had been "expressed poetically": in their search for a "universal harmony", ancient scholars

[85] Naville E. 1883, pp. 53-4.
[86] Naville E. 1883, pp. 55-4.
[87] Naville E. 1883, p. 55.

naturally merged physics with poetry. From the outset modern physics had been expressed "in prose" (the Naville adjective "prosaïque" is definitely ambiguous, as well as the correspondent English adjective *prosaic*), and apparently it had destroyed the ancient poetry of nature. In reality he found that, if an old poetry had been destroyed, "another had been created": the rational order of laws, from "specific laws" to "more general laws" disclosed a new kind of "harmony" and a new kind of "poetry". He dared to write that "the great advances in science" had been accompanied by "an evolution in the aesthetic sensitivity". At the same time he specified that in no way should we identify scientific aesthetics with the glorification of nature or the glorification of science in itself. In other words, scientific practice did not have to be transformed into mythology or in some kind of natural theology, which he qualified as "an aesthetic mistake".[88]

Naville devoted the fourth part of his book to "physics and ethics", and started from the fact that physiology had recently been influenced by physics, and psychology by physiology. In other words, a reductionist attitude had led to basing psychology on physiology and physiology on physics. Two potential risks had emerged and worried some scholars: the possibility that physics could put in danger the foundations of "spiritual order", and the existence of an intrinsic incompatibility between physics and ethics. His aim was to show that no serious opposition between them was actually at stake. His first step was to show the inconsistency of the meta-theoretical attitude which identified human thought with some kind of physical motion of matter. Some philosophers like Herbert Spencer had put forward a very general theory of mutual conversion among the different forces in nature: in accordance with that intellectual framework, human though stemmed from a specific transformation of mechanical energy, in the same way as sound, light and heat. Naville found that the passage from "perceived motions" to the act of "perception" was a sort of leap "from a world to another". The relationship between the domain of material transformations and the domain of thoughts appeared to him more complex than that which some scholars were inclined to assume.[89]

Naville's reconstruction of the recent debates cast light on the tight relationship between reductionism and determinism. Since "both the existence and the manifestation" of mental processes required "the motion of matter" as necessary support, reductionists inferred that ethical and spiritual attitudes stemmed necessarily from "their material conditions". The determinism which affected the material domain was automatically handed over to the moral domain: the deterministic laws of physics, which ruled the material domain had to rule the moral domain. Within that framework, freedom was looked upon as an illusion: an actual free will would have put in danger "the universal determinism of phenomena". Naville was able to understand that the hidden flaw in determinism was not represented by the actual practice of freedom, because we cannot demonstrate that an action is freely

[88] Naville E. 1883, pp. 57-63. I have translated Naville's adjective "prosaïque" with "in prose" because the correspondent English adjective *prosaic* might appear ambiguous. He found that, in recent times, the philosopher Auguste Comte, the physician Rudolf Virchow, the natural philosopher John Tyndall, and Helmholtz himself had leant towards various kinds of natural mythology. He quoted from their texts, and discussed some passages. See Naville E. 1883, pp. 63-5.
[89] Naville E. 1883, pp. 211-16.

performed rather than performed under constraints. The most sensitive issue was "the idea of freedom": how could the existence of the concept of freedom be explained in a deterministic way? Determinism could show that freedom was a delusive concept, but where did that concept stem from? Was it "an idea without cause", or a concept without a deterministic explanation? If ideas without cause had been accepted, then why not "motions without cause"? Determinism seemed to be tottering under its own weight.[90]

Naville's criticism to determinism focussed on a specific feature of the principle of conservation of energy: he found that the latter suffered from space- and time-indeterminism. Space-indeterminism depended on the fact that energy was a scalar quantity, which did not depend on "the direction of motion": if only the direction changed, kinetic energy did not change. Time-indeterminism depended on the fact that the conservation of energy did not depend on the duration of processes: transformations of energy in accordance with the principle could take place at "different times", and could require different time spans. Ethics and free will did not require the creation or destruction of energy but only the possibility of making use of energy where and when the subject found it useful or right. Unfortunately Naville did not seem aware that the change of direction in a given velocity required a force, and that such a force had to be provided by the subject or another physical system. All we can say is that the processes which preserve total energy are insensitive to space and time translation, as well as to time duration. However space- and time-indeterminism appeared to Naville consistent with free will and free ethics. The practice of freedom did not change the amount of energy but only allowed the subject to decide the occurrence of physical processes through space and time.[91]

Not only did human consciousness enjoy that intrinsic indeterminism, but also biological processes in general actually did. He mentioned Claude Bernard and his conception of a "living force" which corresponded to a specific "*legislative* power" in living beings. In other words, biological systems could *direct* but not *create* energy: they could "make different uses of physical motion although its amount does not change". Also in another passage he insisted on the concept that "the direction of motions can be changed, provided that the quantity be the same". The fact that Naville let this ambiguity propagate throughout his text does not facilitate the comprehension of his line of reasoning. The consequence is that his concept of time-indeterminism appears consistent but the concept of space-indeterminism remains more problematic. However, at the end of the section he devoted to this subject, only the role of time was in prominence. He concluded that "no opposition between physics and ethics" took place: human beings could not create force, but they could "make use of the amount at their disposal" at that time they could choose "for good or for the evil".[92]

He found that three kinds of motions could be found in the domain of life: there were mere "*mechanical* motions", which were "purely instinctive", then "*spontaneous* motions", which found their roots in "a specific power of living germs", and eventually "*free* or voluntary motions", which rested on "psychological base". Although the three kinds of motion were mutually linked to each other, no necessity and no reductionist process led

[90] Naville E. 1883, pp. 223 and 227.
[91] Naville E. 1883, pp. 228-9. He acknowledged that Cournot had put forward similar remarks.
[92] Naville E. 1883, pp. 235-6 and 239.

from mechanical motions to free motions. They were "different but not separate" because "the life of organisms" respected "the laws of inorganic matter", and "the life of mind" respected "the physiological tenets of living beings". At the end of the fourth part of the book, Naville stressed once again the delusive nature of the supposed opposition between physics and ethics. Moreover he mentioned the mathematician Boussinesq, who had published a booklet on the mutual consistency between the law of mathematical physics and ethical freedom. According to Naville, the fact that a philosopher and a mathematician had arrived at the same conclusions, reinforced the reliability of those conclusions.[93]

In the last part of his book, which he devoted to "philosophical consequences of modern physics", he noted that the influence of "specific sciences" on philosophy was a matter of fact. Both theoretical developments and technical applications of science had become "prominent intellectual hallmarks of the time". At the same time he warned against the recent trend of transforming the influence of science on philosophy into new philosophies based on physics or some other specific science. Philosophy dealt with something more general and more "universal", and no direct passage from the two domain was philosophically legitimate. First of all, a correct relationship between science and philosophy required a careful analysis of what the scientific method really was. Galileo had based his new science on a powerful alliance between reason and experience. If Bacon had stressed the role of experiments, Descartes and then Leibniz had emphasised the role of "deductive procedures", and the history of physics appeared as a series of actions and reactions of meta-theoretical nature. If the XVIII century had saw "a reaction in favour of empirism", rationalism had gained his hegemony at the beginning of the XIX century, and afterwards positivism could be looked upon as a reaction of empirism to the idealistic philosophy of nature. What did it remain after "the violent oscillations" between "absolute rationalism" and "strict empirism"? The key of the scientific method was nothing else but the continuous search for a dynamical balance between the two poles. Only after that specification could Naville face the consequence of the development of science on "the philosophical programme".[94]

He found that no field of intellectual activity really did aim at the unity of knowledge more than philosophy, but the search for unity clashed with "an intrinsic duality: matter and mind". Mind expressed itself in the knowledge of matter "as a subject" who could "not be reduced to its object". The logical and physical dualism was really deep, and it could not be easily solved. Nevertheless the gap could be spanned by a bridge, and the bridge was the existence of physical laws. Laws were rooted "into the intelligence" of the human subject rather than in physical phenomena, whereas "things in themselves" were nothing else but "the material conditions of all potential conceptions". Mathematics played an important role in the process because it highlighted "the *a priori* nature of reason". At the same time mathematics put in prominence the fruitful tension between reason and experience, because

[93] Naville E. 1883, pp. 241-2. It is well known that the polysemy of the French noun "esprit" makes it difficult to be suitably translated. I have chosen the English word "mind" because, in this context, Naville seems more interested to assure the relative autonomy of psychology and ethics rather than religion. In 1878 Boussinesq had published a book on the structural analogy between singular solutions of differential equations, and bifurcations in biological processes (see the *References* section).

[94] Naville E. 1883, pp. 243-5, 247-50.

"the ideas" which lay "at the basis of mathematics" could "not develop without the influence of experience", but their content did "not deal with experience". According to Naville, physics was deeply affected by the tension "between facts and thoughts", but at the same time it was a meaningful instance of the possible harmony between them: only that harmony could "make the world understandable". Physics showed that "the resemblance between phenomena and laws of thought" could really be pursued. Since that resemblance was "the condition for the potential existence of science", and a "real science" really did exist, that existence showed that "such a resemblance did really exist" as well.[95]

In the last pages of the book he explicitly expressed his philosophical commitment, and pointed out what he considered the "legitimate consequences" of modern physics. Two of them appeared to him of remarkable relevance: "the demolition of universal scepticism" and the demolition of "materialism" of ancient philosophers.[96] The presumption that modern science had managed to dismantle permanently both scepticism and materialism was an evident overstatement, even though his critical and historical analysis of science appears quite sophisticated and convincing in many points. In some way his firm belief in a non-dogmatic approach to knowledge contained slightly dogmatic nuances.

## *2.4. The translation of Stallo's book and other developments*

The following year the Parisian publisher Alcan sent the French translation of a book (which had been published in 1882 in the U.S.A.) by a German-born scholar, Johann Bernhard Stallo, to the printing press. The book was prefaced by the French chemist and mineralogist Charles Friedel, who drew the attention of readers only to one specific issue the book dealt with: the atomic constitution of matter and the author's criticism. I will confine myself to highlighting two specific issues which Stallo introduced into the debate on the foundations of physics in the French context. The first dealt with the above mentioned atomic constitution of matter and the kinetic theory, and the second dealt with inertia and gravitation.[97]

He noticed that, in recent times, a specific assumption on mass, namely "the molecular or atomic constitution of bodies", had prevailed, and had given rise to what he labelled "the atomo-mechanical theory". According to Stallo's reconstruction, that theory led to four statements, which had rarely been expressed in an explicit way. The first stated the existence of "*elementary units of mass*"; the second and third assumed that those elementary units were "*hard and inelastic*" but also "*absolutely inert and therefore purely passive*. The

[95] Naville E. 1883, pp. 258, 263-5, and 267. I have translated the French "conformité" into "resemblance", and "démontrer" into "to show", which is the weakest among the potential translations. I am obviously aware that, from the logical point of view, "to demonstrate" is more demanding than "to show". The fact is that the whole content of Naville's book seems in accordance with the mildest meaning of the verb: it seems to me that he made use of the expression "to demonstrate" in a very broad sense.

[96] Naville E. 1883, pp. 268-9.

[97] The title of Stallo's book, *The Concepts and Theories of Modern Physics*, was translated into French as *La matière et la physique moderne*. When compared with the original American edition, both the French title and Friedel's Introduction to French translation seems to focus specifically on the very sensitive issue of atomism.

fourth made reference to energy, and assumed that potential energy could be reduced to kinetic energy: in other words, any kind of force could be replaced by a system of masses and motions. It is worth mentioning that this scientific programme corresponded to the most radical mechanism, and it was not shared by the totality of scientists. Stallo's criticism was therefore addressed to that radical mechanism rather than other milder versions: to be more precise, he addressed the combination of a radical mechanism with atomism.[98]

Stallo noted that the first statement got rid of any qualitative difference between atoms, and this led to a divorce between physics and chemistry, as "the law of Avogadro or Ampère" easily showed. In fact, "under like conditions of pressure and temperature", equal volumes of gaseous substances "contain the same number of molecules", but they have not the same weight. Obviously upholders of the atomo-mechanical theory were able to surmise a mechanical model of atom which could explain the different densities of different kinds of matter. Atoms could be imagined as microscopic planetary systems, where the velocity of peripheral planets defined the radius and therefore the volume of the planetary atom itself. Different velocities would correspond to different densities for the same atom mass. Stallo was not satisfied with this explanation, because it accounted "for inequalities in the volumes of equal masses" rather than "inequalities of mass in the same volume". We might remark that even his objection is not satisfactory, because it overlaps the concept of macroscopic volume, namely gaseous volume, with the concept of microscopic volume, namely atomic volume.[99]

Even inelasticity of elementary atoms appeared questionable to Stallo from the point of view of recent developments in physics. On the one hand, he knew that elasticity involved "motion of parts", and the possibility of parts was inconsistent with the assumption of elementary units of mass. He traced back to Newton's Opticks the awareness that "the absolute hardness of the component particles of mass" was "an essential feature of the original order of nature". On the other hand, the recently developed "kinetic theory of gases" required some kind of elastic scattering between gaseous molecules and/or elastic scattering between the molecules and the walls of the container in order to account for gas pressure. If molecules "were wholly inelastic or imperfectly inelastic", inevitably their relentless motion "would soon come to an end". Moreover elasticity was also required by the principle of conservation of energy, because inelasticity led to the dissipation of mechanical energy. If macroscopic bodies could convert their inelasticity into the disordered motion of their microscopic components, elementary atoms could not rely on parts or components, and "no such conversion" was "possible". Even Helmholtz and William Thomson's model of atom as the result of "vortex motion in a perfectly homogeneous, incompressible and frictionless fluid" appeared unsatisfactory to Stallo.[100]

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[98] Stallo J.B. 1882, pp. 28-9. Stallo's book was re-published in the U.S.A. in 1884. It had something in common with the more famous book the German-speaking physicist Ernst Mach published in 1883. The latter was to be translated into French only in 1904.

[99] Stallo J.B. 1882, pp. 31-2 and 34-5. From the point of view of the foundation of mechanics, matter and motion had to be "disparate": in particular, mass had to be "indifferent to motion", and therefore it could not be the effect of motion. (See *Ibidem*, p. 39)

[100] Stallo J.B. 1882, pp. 40-2.

Once again, even the model of atom as "vortex-rings formed by rotational motion" within "an absolutely homogeneous, incompressible, perfect fluid" was unsatisfactory: it required elasticity because "their mutual approaches would result in rebounds similar to the resilience of perfectly elastic bodies". Moreover he wondered whether that specific kind of motion " in such an ideal fluid " was a "sensible motion", or in other words, an actually detectable motion. How could that "fluid destitute and incapable of difference" be "a vehicle of real motion as pure space"? Could the motion itself be the source of inertia, when mechanics was based on "the fundamental antithesis between mass and motion, inertia and energy"? He also quoted the Italian astronomer and physicist Angelo Secchi, who in his turn, had quoted the French mathematician Louis Poinsot: the latter had put forward a model of elastic collision undergone by an inelastic atom which was endowed with a rotational motion. In brief, rotation could transform an inelastic body into an elastic one. The mathematical-physical model was quite interesting, because part of the rotational energy transformed into translational energy, and the body was "often sent back with a velocity superior to its initial velocity" after having been thrown "against a fixed obstacle".[101]

However, to give up forces, in favour of purely mechanical effects, was tightly linked to giving up the concept of potential energy in favour of the kinetic nature of every kind of energy. Stallo found that such a reduction did not match with the complexity of natural processes, where some kind of "locking up" of kinetic energy "in the form of latent energy" was at stake. He mentioned "[t]he relatively permanent concretion of material forms" and "chemical action and reaction", but also "the evolution of vegetal and animal organisms". In brief, Stallo claimed that "the four cardinal propositions of the atomo-mechanical theory" were "severally denied by the sciences of chemistry, physics, and astronomy".[102]

In the central part of his book, Stallo faced once more "the theory of the atomic constitution of matter". In a footnote he specified that he should have distinguished between the concept of *molecule* and the concept of *atom*: if the former corresponded to "the ultimate products of the physical division of matter“, the latter corresponded to "the ultimate products of its chemical decomposition“. Nevertheless, in order to simplify his critical account, he was to use the word atom in the broader sense of "the least particles into which bodies are divisible by any means. Also in this case he tried to identify some basic statements which could synthesise the atomic theory or atomic "hypothesis". The first stated that atoms were "*simple, unchangeable, indestructible*"; the second, that the expansion of a body corresponded to "*an increase … of the spatial intervals between the atoms*"; the third, that "*different chemical elements*" had "*determinate specific weights*", which corresponded to "*their equivalent of combination*". His criticism was preceded by two main requirements which the set of theories which composed the theoretical structure of physics would have had to fulfil in order to be assessed as a reliable body of knowledge. If the first dealt with the empirical content of a theory, the second involved its logical consistency. In his words, atomic theory, like any other theory, would have had to be a good theory if "satisfactorily

[101] Stallo J.B. 1882, pp. 43-6.
[102] Stallo J.B. 1882, pp. 68 and 83.

and simply accounts for the phenomena", and if "it is in accord with itself and with the known laws of Reason and Nature".[103]

When he analysed the first statement on the atomic theory, namely the persistence and invariance of atoms under any transformation, he focussed on the question of whether essential features of atoms had to exactly correspond to the features of ordinary matter. In other words, did atoms have peculiar features, and in this case, how could those features account for the features of ordinary matter? In the end, he seemed to lend towards a meta-theoretical option, which he attributed to William Thomson: in no way could the hypothesis of atoms account for a macroscopic "property of a body which has not previously been attributed to the atoms themselves". From this point of view, the impenetrability of atoms and ordinary matter appeared as mutually inconsistent. If the impenetrability of atoms was assumed, the diffusion of gases and the existence of chemical compounds excluded that impenetrability could be a general property of matter. As a specific instance he offered the case of sulphuric acid and water, which "at ordinary temperatures do not sensibly yield to pressure", but when mixed, "the resulting volume is materially less than the aggregate volumes of the liquid mixed".[104]

With regard to the second statement, "the essential discontinuity of matter" seemed required by "dispersion and polarization of light". Nevertheless dispersion could not be explained from a mechanical point of view, because the velocity of waves could not depend on their wavelength but only on elasticity and inertia of the medium. Stallo remarked that from Poisson onwards, polarisation of light had been explained "by the hypothesis of the existence of finite intervals between the aethereal particles". The atomic theory could not account for that specific arrangement of molecules: the hypothesis of atoms and molecules did not imply an "alternation of unchangeable and indivisible atoms with absolute spatial voids". .[105]

The third statement involved chemistry, and assumed that atoms corresponded to "the different chemical elements", and that their weights corresponded "to their equivalent of combination. But those assumptions could not account for the variety of transformations of matter occurring in chemistry. He remarked that, when "heterogeneous bodies concur in definite proportions of weight or volume", and they interact, "they disappear". Although the final weight was "the aggregate of the weights of the interacting bodies", the new substance had "properties which are neither the sum nor the mean of the properties of the bodies concurring and interacting". Change of volume and exchanges of energy (heat) were frequently associated with those transformations. In brief, a "true chemical action" could not

103 Stallo J.B. 1882, p. 85. It is worth noticing the ambiguous reference to Reason and Nature in the requirement of empirical consistency (the second one). If the reference to nature seems suitably linked to empirical check, the reference to reason seems linked to logical consistency, which is the first (logical) requirement. In reality, a more analytical description of such requirements led him to list six sub-prescriptions. He mentioned "the consistence with themselves", "their agreement with the canons of logic", "their congruence with the facts", "their conformity with the ascertained order of Nature", the possibility to make "anticipations or prevision of facts verified by subsequent observation", and "their simplicity , or rather their reducing power". In some way, the six prescriptions appear redundant and less clear than the two main requirements. The first and second are not so different from each other, as well as the third and fourth. The fifth is not contained in the above two requirements.

104 Stallo J.B. 1882, pp. 89-90 and 101.

105 Stallo J.B. 1882, pp. 92-4.

be identified with "mere mechanical mixture or separation". According to Stallo, chemical transformations could be "distributed" in three classes: "the persistence of weight", "changes of volume and the evolution or involution of energy", and "the emergence of a wholly new complement of chemical properties". He stressed that in no way could "the atomic hypothesis" offer an explanation "of the phenomena of the second class", and it was "apparently incompatible … with the phenomena of the third class".[106]

In the chapter he devoted to the kinetic theory of gases, the key concept under investigation was "elasticity": that property was assumed "in the constituent solid" atom, but at the same time, it was "the very fact to be accounted for in the gas". In other words, the elasticity of gases was explained by means of the hypothesis that the atomic elements of gases were elastic. The effect had been transformed into a cause, and even worse, the cause of itself. What had to be explained had been transformed into a hypothesis: it was a circular procedure indeed. Stallo's criticism became very scratchy: that procedure was "utterly vain", or rather "worse than vain", namely "a complete inversion of the order of intelligence". It was "a resolution of identity into difference", "an unravelling of the Simplex into the Complex", an interpretation of the Known in terms of the Unknown", "an elucidation of the Evident by the Mysterious", …. The third assumption, namely the unperturbed paths of molecules, appeared to Stallo the "perfect realisation of the abstract concept of free and ceaseless rectilinear motion". Unfortunately, particles which "move independently without mutual attraction or repulsion or any sort of mutual action" were something "unheard-of … in the wide domain of sensible experience".[107]

Even gravitation seemed not so easy to explain by means of purely mechanical assumptions. After some pages of historical reconstruction, where Huygens, Newton, Johann Bernoulli, Euler and D'Alembert were mentioned and briefly discussed, Stallo took into account more recent theories and theoretical sketches where there was an attempt to reduce gravitation to a purely mechanical theory. The only theory which deserved to be presented in some detail was Georges-Louis LeSage's kinetic theory of gravitation, where the attraction between celestial bodies was the effect of the flux of unspecified tiny particles coming from unspecified regions of the universe, travelling in every direction. More specifically, for every couple of bodies, each of them acted as a screen for particles directed to the other on the side facing the former. Stallo remarked that those streams of particles required a huge amount of energy: although the "ultramundane corpuscles" were supposed to be of negligible mass, their velocities were supposed to be very great, and their number just as great. In the end, he found that the apparent action of gravitation at a distance seemed to be "an ultimate fact inexplicable on the principles of impact and pressure of bodies in immediate contact".[108]

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[106] Stallo J.B. 1882, pp. 99-100.

[107] Stallo J.B. 1882, pp. 119-22.

[108] Stallo J.B. 1882, pp. 54-65. In the last decades of the XIX century, LeSage's theory was also revived by British physicists. See, for instance, Maxwell J.C. 1875, pp. 473-5, and Thomson J.J. 1891, pp. 162-3. Both in the main text and in footnotes, we can find a long list of German scholars. Among non-German scholars he mentioned the French Pierre Simon de Laplace and François Arago, and the British Balfour Stewart and Peter Guthrie Tait.

He also found the mechanical dichotomy between matter and force unsatisfactory. Matter was "passive, dead" in itself, and "all motion or life" was "caused by force". He claimed that that "antagonism" was "utterly baseless", and that force was "nothing without mass", and mass "nothing without force". Unfortunately, in the history of physics he saw a dual reductionism: "the corpuscular theory" on the one hand, and "the dynamical theory" on the other. If the former considered "*inertia* … as real in itself", the latter considered atoms or molecules as "centres of force", where "force" was "an entity by itself". Stallo saw a deep correlation between "the conceptual terms *inertia* and *force*" rather than some kind of reduction of one to the other. Newton's expression *vis insita* seemed to him more suitable than other interpretations. The fact that inertia in itself was an empty concept could be linked to the oddness of the concept of "isolated existence of a body": it was "a pure fiction of the intellect". In brief, bodies existed "solely in virtue of their relations", and their "reality" lay "in their mutual actions". The mechanical concept of inertia was therefore "as unknown to experience as it is inconceivable in thought".[109]

In particular, neither gravitation nor chemical actions could be removed from any region of the universe.

> Every particle of matter of which we have any knowledge attracts every other particle in conformity to the laws of gravitation; and every material element exerts chemical, electrical and other forces upon other elements which, in respect of such force, are its correlates. A body can not, indeed, move itself; but this is true for the same reason that it can not exist in and by itself. The very presence of a body in space and time , as well as it motion, implies interaction with other bodies, and therefore *actio in distans*; consequently all attempts to reduce gravitation or chemical action to mere impact are aimless and absurd.[110]

He dared to imagine that the concept of mass "might be expanded", in order to extend the notion of inertia "to physical action generally" rather than "to mechanical motion alone". For instance, "thermally equivalent masses" of "chemical equivalent masses" could be imagined. Although "the determination of mass" had been established "on the basis of gravitation", the choice had to be looked upon as "a mere matter of convenience" rather than "founded on the nature of things".[111]

In the Introduction to the French translation of Stallo's book, the chemist Friedel focused on the status of the atomic theory of matter, but the specific subject matter gave him the opportunity to put forward wide-scope methodological remarks. He found that a fruitful and widely accepted scientific theory should be submitted to "a careful scrutiny" after a distinguished service "for the advancement of science". He considered worthwhile to

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[109] Stallo J.B. 1882, pp. 152 and 161-3. He listed Boscovich and Faraday among the followers of *dynamism*. (*Ibidem*, p. 162) Some remarks on the interdependence between matter and force had already been expressed by Helmholtz and Maxwell. See Helmholtz H. 1847, in Helmholtz H. 1889, p. 5, and Maxwell J.C. 1878, pp. 166-8. See also Kant I. 1787, in Kant 1881, pp. 271. On Kant's influence on Helmholtz, see Elkana Y. 1974, pp. 167. On the influence of Kant on William Whewell, on the influence of Whewell on the young Maxwell, and on Maxwell's *dynamical* conception of matter, see Harman P.M. 1998, pp. 28-36 and 190-4.

[110] Stallo J.B. 1882, p. 163.

[111] Stallo J.B. 1882, pp. 205.

criticize that theory "in its foundations", but he was aware that no serious objection, nor a set of objections, might "completely destroy a reliable scientific conception". It was far more probable that a theory had to be "more or less deeply modified". In any case, he found the criticism useful to make experimentalist ponder on an uncritically accepted body of knowledge.[112]

The starting point of every scientific enterprise could be found in "observation and experience", and a hierarchy of entities stemmed from that basis of perceptions. Immediately above he placed physical "laws", which allowed scientist "to collect an increasing number" of those observations and experiences by means of "a mathematical connection". At the top of the hierarchy he saw "simple and general principles". The intellectual path which led from the empirical basis to the first principles involved the passage from a higher to a lower degree of certainty. The process of continuous abstraction was intrinsically linked to the increasing "risk of being misled". Friedel thought that, in reality, "scientists committed to experimental researches" were naturally led to skip and underestimate those problems, and theoretical and meta-theoretical issues attracted only a minority of them. The actual scientific practice was the seat of a methodological tension between two different attitudes and communities: the necessity for debate on models and principles, on the one hand, and the automatic confidence in those principles and models, on the other. In the context of chemistry, that methodological tension was at stake in the case of the atomic theory of matter.[113]

The assumption of the existence of atoms allowed chemists "to represent the processes of combination in the simplest way", even though the confidence in "a single elementary kind of matter" was "a philosophical conception" rather than a scientific one. Nevertheless the criticism addressed to "Avogadro's theory in particular, and the atomic theory in general" appeared not so effective to him. However the problem had quite a different nature: even though the objections to the atomic theory had been more convincing than they actually were, which kind of theory would have replaced the atomic theory? Chemists should "be allowed to continue to make use of a theory which helps them to collect a great number of facts", and had led them "to discovery new facts every day". They were "aware of the flaws" of the theory, and they were waiting for "a mechanical explanation" of the processes which had been "labelled *atomicity* or valence of atoms". Once more two different communities of scholars were involved in the scientific progress: if "chemists" had clearly raised the issue, the solution was a task for "mathematicians".[114]

In 1885 the French philosopher Charles Renouvier published a critical analysis of the history of philosophy, and devoted some pages to the relationship between science and philosophy. That relationship deserved "a specific attention" because sciences "had always had the honour of suggesting new metaphysical pathways". Moreover he cast doubt on the possibility that something like "*the* science" and a single scientific method could really

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[112] Friedel C. 1884, pp. VII-VIII.

[113] Freidel C. 1884, p. VIII. His reference to scientists and theoretical and meta-theoretical debates involved two sharply defined communities: "les savants qui s'occupent d'investigations expérimentales" and "les philosophes".

[114] Freidel C. 1884, pp. VIII-IX and XI-XII.

exist: he saw a plurality of "specific sciences". He therefore focused on physics, and noted that, from Descartes onwards, what he labelled "the universal mechanism" entailed an "absolute determinism". It had been Laplace who had imagined the physical world as something that, "*a priori*, could be expressed by an equation of rational mechanics". If that possibility appeared questionable, even more questionable was the possibility that "science could encompass the whole" of human experience. In general, scientific theories were based on "hypotheses or hypothetical generalisations", and they could not be freed of "their uncertainties". He found that "absolute and unverifiable statements going beyond any possible experience" were forbidden to the "true scholar". [115]

In particular he questioned the empirical value of the principle of conservation of energy, which he looked upon as belonging to "the set of mathematical principles"; more in general he saw an unbridgeable gap between theory and experience. Another gap he saw between specific physical theories and the world-views which could be drawn from them: the most meaningful instance he took into account was the problematic link between "a mechanical physics" and "a universal mechanism" based on the reduction of "every kind of phenomenon in the universe" to "matter and force". In brief he saw science as a three level enterprise, which consisted of three different stages: empirical, theoretical, and metaphysical practices. The last stage brought science closer to philosophy, and even closer to ancient natural philosophies and "mythological" traditions.[116]

What was the borderline between science and philosophy? He imagined a very general physical law, which represented the most general content which could be expressed in scientific terms before entering the field of philosophy. Such law was of the kind $I = E - S$, where $E$ represented "forces" or better energies which entered a material system or "an organism", $S$ represented the energies going out, and $I$ those "held back inside the system". What "effort or will" was at stake inside the organism, and "why and how" those actions were linked to the fluxes of energy, was definitely outside the domain of science, and appertained to the domain "of metaphysics or psychology". Unfortunately, scientists who upheld "universal mechanism" trespassed on an inappropriate land, to which they could not lay claim. Renouvier found that a mechanical physics became mechanism in the sense of a metaphysical option when it claimed that not only did "perceptions correspond to motions", but also they were "motions in themselves". That metaphysical option was not so different from "a mythological body of knowledge", and it was looked upon by Renouvier as "the most recent implementation" of "absolute determinism".[117]

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[115] Renouvier C. 1885, pp. 286-8. A former Comte's disciple, he was an influential outsider, and founded the philosophical journal *La critique philosophique*.

[116] Renouvier C. 1885, pp. 289-90. That very general scientific law was not so different from the specific physical law which had already been labelled first Principle of Thermodynamics: $\Delta U = \Delta Q - \Delta L$, where $\Delta Q$ was the flux of entering heat, $\Delta L$ the mechanical work performed by the system, and $\Delta U$ the energy stored into the system itself.

[117] Renouvier C. 1885, pp. 290-1. In the mechanical world-view he found an inappropriate overlap between entities of different nature: mechanical motion seemed not so different from "the perception of heat" or even "the desire to sing" and "the desire to solve an equation".

## *2.5 The optimistic scientism*

In 1886 the authoritative chemist and politician Marcelin Berthelot published a book which was a collection of some papers he had already published in various journals in the 1860s and 1870s. He was an important character of the French Third Republic: as he himself explained to his readers, not only was he committed to science, but also to "specific applications to industry and national defence, public lecturing, and general politics". The scattered collection of texts on different subjects was however unified by the presence of specific "philosophical views", which could be looked upon "as a sort of intellectual and moral biography of the author". Among the four main subject matters, "scientific philosophy, history of science, public teaching, and politics and national defence", the first two deal with the context I am exploring, and the first essay of the book focuses exactly on what we might label history and philosophy of science. In reality the qualification might be too ambitious because the prestigious chemist confined himself to an apologetic and simplified history of science, and was committed to an equally simplified philosophical analysis of the science of his time. From the formal point of view, the essay was a public letter he had addressed to another intellectual father of the Third republic, the historian Ernest Renan.[118]

In the "Préface" to the book, which was arguably written in the same year of the publication, Berthelot outlined some basic theses of his "scientific philosophy", which he had already put forward in 1879, in his "grand work" on Chemical Mechanics. He mentioned the identity "in principle and in fact" between chemical processes in living beings and in inanimate bodies, the reduction of chemistry "to the most general laws of mechanics", and the perfect correspondence between the macroscopic domain of "stars surrounding us" and the microscopic domain of "atoms or slightest particles of bodies". He carefully stressed that his celebration of contemporary chemistry and physics did not lead him to the underestimation of the ancient body of knowledge, which had spawned some kind of "intermediate, half-mystic, and half-rational sciences". Some of them, for instance "alchemy and astrology", had greatly contributed to "the evolution of human mind".[119]

The specific subject matter of the first essay was "positive science", namely the scientific practice which started from "the establishment of facts", and connected them by means of "immediate relations". That "chain of relations", whose length "increased every day“, was nothing else but the body of positive science. Since science dealt with observable facts, it was not able to attain "the first causes or the end of the material world". Nevertheless it managed to lead mankind to "the explanation of a huge number of phenomena" merely on the basis of "the coarsest facts". We see that, on the one hand he stressed the limited scope of scientific practice, which could not aim at clarifying the destinies of men and universe, and on the other he endorsed an optimistic attitude to science. We find here both the faith in

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[118] Berthelot M. 1886, pp. II-IV. He was a member of the political and academic establishment in France: an influential chemist with serious interests in the history of science, professor at the *Collège de France*, moderate republican, he was also a member of Parliament and minister.

[119] Berthelot M. 1886, pp. V and VII. In this context he reminded the reader that he had recently published the book *Les origines de l'Alchimie*, and some excerpts were there reproduced.

a continuous and unbound scientific progress, and the confidence in a simplified scientific practice, which could smoothly and successfully lead from ordinary observation to very general scientific laws.[120]

He was willing to offer the reader a simple and interesting instance of that "positive" procedure: the explanation of the functioning of a torch or lamp, or in other words the answer to the question "why a torch lights up". The answer was really simple and correct: "while burning the torch sends out gases mixed with charcoal particles at high temperature". Equally sound appears Berthelot's remark that the answer was "not arbitrary". Less sound appears the following remark that the answer was not "founded on reasoning", and even less sound the claim that it stemmed from "a direct examination of the phenomenon". He insisted on the evidence of the chemical process: he stated that the torch contained "carbon and hydrogen, which are both fuel elements", and those were "observable facts". Moreover hydrogen was one of the components of water, and the production of water was one of the results of the combustion.[121]

We are facing here a sort of linguistic and conceptual superposition of meanings, which deals with the adjective "observable". It seems that Berthelot made use of "observable" in the sense of "scientifically explained" rather than in the sense of actually observable. He was an authoritative scholar, and any purely linguistic misunderstanding should be excluded. As a consequence we must admit a deliberate meta-theoretical commitment, where current scientific explanations had to be necessarily endowed with the hallmark of evidence. Scientific practice had to be associated with the disclosure of evidence rather than a more complex practice of interpretation and translation of facts into a series of rational entities, concepts, and statements. What observable evidence might lead us to state the identity between one of the invisible components of fire and one of the invisible components of water? How could we directly observe hydrogen?

In spite of these conceptual and linguistic mismatches, Berthelot continued with the chain of evident *facts*, such as the *fact* that the combination of oxygen with "the elements of the torch, namely carbon and hydrogen", generated heat. In the end he acknowledged that he had passed from "the observation of facts" to "more general notions", and specifically that those notions were "more general than the specific facts" from which he had started. The chain of generalisations could progress towards a supposed "more general law", which stated that "every chemical combination sends out heat". In a following passage, not only did he make use of the word "law" but also the word "cause": the relationship between chemical combination and heat flux was "one of the fundamental notions of chemistry", and at the same time "one of the causes" which produced "the most frequent and important effects in the universe".[122]

All these processes, laws, and causes, heat fluxes included, could be looked upon as "specific instances of mechanics". And here the empiricist rhetoric emerged once more: not only could "physics and chemistry be reduced to mechanics", but also the process of reduction was an empirical necessity rather than a rational option. According to Berthelot,

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[120] Berthelot M. 1886, pp. 4-5.
[121] Berthelot M. 1886, pp. 5-6.
[122] Berthelot M. 1886, p. 7.

the reduction did not stem from "confused and uncertain insights, or *a priori* reasoning" but from "unquestionable notions", which were "always based on observation and experience". Great advantages and great expectations emerged from that reduction: not only "great results" but also "general laws" in conformity with "the nature of things", and " a simple and invariable method". We are facing a circular practice, which once more started from "the perception of facts" by means of "observation and experience", then went on with the establishment of "relations", which stemmed from the comparison among simple facts, and led to "more general facts", and ended with the empirical check of those general facts by means of "observation and experience".[123]

The polysemy of the word "fact" is really striking, because Bertholet considered as facts what he immediately qualified as "progressive generalisations, deduced from previous facts". Scientific practice started from "specific and coarse phenomena", and led to "the most abstract and wide-scope natural laws", but the whole "pyramid of science", and in particular "all the foundations" of every layer of the structure rested "on observation and experience". In other words, scientific practice consisted in discovering an ever-wider domain of *facts*. The empirical nature of science was "one of the principles of positive science": in particular "no reality" could be "established by means of reasoning". Centuries of philosophy were swept away by that flood of "facts", and by a new, naïve philosophy. His new philosophy stated that "the conclusions we draw from …our conceptions" could only be "probable and never certain", whereas certainty was really attained by "a direct observation, which complies with reality".[124]

The history of science was reconstructed and presented to readers by means of some brush strokes, which roughly depicted the pathway that had led from the ancient natural philosophy to "the solid principle on which modern sciences rests". At the beginning, "Indian wise men" had relied on "meditation", and Greek philosophers, in particular "neo platonic", had relied on "the power of speculation": he found that "the advancement of mathematical sciences" had supported such a "delusion". Only modern scholars, and in particular "Galileo and Florence academicians", had managed to understand that axioms of mathematics had to be drawn "from observation", and conclusions had to be checked "by means of the same observation". That a circular process of this kind was not consistent with the actual development of mathematics seemed outside Berthelot's intellectual horizon. The emergence of abstract approaches to geometry in the first half of the XIX century was also beyond his horizon. According to his historical reconstruction, "the XVI [!] century" had seen the first achievements of "the forefathers of positive science", and the XVIII century had seen "the triumph of the new method". The "ultimate aim" of that century was to rule society "in accordance with the principle of science and reason".[125]

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[123] Berthelot M. 1886, pp. 9-10.

[124] Berthelot M. 1886, pp.10-11. I find that a short passage deserves to be quoted: "Une généralisation progressive, déduite des faits antérieures et vérifiée sans cesse par de nouvelles observations, conduit ainsi notre connaissance depuis les phénomènes vulgaires et particuliers jusqu'aux lois naturelles les plus abstraites et les plus étendues."(*Ibidem*, p. 10)

[125] Berthelot M. 1886, pp. 11-13. I imagine that "XVI century" was a simple misprint, and it should be read "XVII century". In spite of his simplified historiography and epistemology, Berthelot was a learned scientist.

The appearance of “reason” in this context seems quite strange, because Berthelot had continuously discouraged the use of reason in favour of ubiquitous observations and experiences. In reality this seems a rhetoric slip, because he immediately hastened to specify that “the moral domain”, as well as “the material domain”, required the usual methodological strategy. At first the establishment of “facts”, then their check by means of “observation”, and their subsequent mutual connection. Once more the whole procedure was based “on a ceaseless recourse to the same kind of observation”. The method which solved “every day the problems of the material and industrial world” was able to solve the “fundamental problems which emerged from the social organisation”. But what social facts could offer the point of departure of the *positive* procedure? Quite surprisingly “the primordial fact of human nature” was “the feeling of good and evil”, which was accompanied by other equally primitive entities: the “notion of duty” and the “fact” that “man feels to be free”. The semantic extension of the word “fact” was thus further widened in order to encompass human feelings in general.[126]

In the end, “positive science” had gained an unquestionable “authority”, which was based on “the necessary conformity between his results and the intrinsic nature of things”. Neither the conformity nor the corresponding necessity seemed to require a justification or at least an explanation. They were facts, even though facts of different level indeed. Every man endowed with a suitably basic education would have been able to appreciate “the results of positive science as the only gauge of certainty”. An optimistic trend marked the last stage of the history of science: “ancient attitudes, frequently stemmed from ignorance and imagination” was fading away in favour of “new conceptions based on the observation of nature”. Even the semantic extension of the word “nature” was accordingly widened in order to host “moral nature” besides “physical nature”. However the essential features of positive science were reliability and steadiness, differently from the ancient philosophies which “had ceaselessly changed” over time. The reliability of science was tightly linked to “the power it gives to man”: a power “on the world” as well as “on man himself”. Just for this reason, namely material success, Berthelot found that the new body of knowledge and practices would have never been “overturned”.[127]

In the second part of the essay Berthelot focused on what was outside the boundaries of positive science, for instance “the search for the origin and end of things”. Driven by “an invincible force”, the human mind was led “to conceive and imagine” entities and possibilities beyond “the chain” of scientific deductions. That force was in itself “a fact”, which could not be neglected or underestimated: its existence was “necessary”, and its “legitimacy” could not be questioned. In every season of history as well as “every people and every person”, man had searched for “a complete system encompassing his destiny and the destiny of the universe”. Although “every attempt to build absolute systems” had failed, the “general feelings” from which it stemmed were “intrinsic to human heart” and

[126] Berthelot M. 1886, pp. 13-5.

[127] Berthelot M. 1886, pp. 14-6. This optimistic and simplified history of science was not so different from a mythological tale pivoted around an almighty power of observation. The possibility that ancient people and ancient traditions, and in particular ancient scholars, had been involved in careful observations of nature, and the possibility that the role of observations was not so pivotal in the emergence of modern science, were not taken into account.

"legitimate as feelings". The whole of those trends and feelings had to do with what Berthelot labelled "ideal science". The fact is that, however potentially legitimate in itself, ideal science could find a full and actual legitimacy only if it subjected itself "to the method which makes reliable the foundations of positive science". In other words, philosophy and other intellectual practices had to follow the scientific method.[128]

That necessity, or better that meta-theoretical option, rested on the possibility of expanding scientific method outside its boundaries, which seems in contradiction with the limitations he had pointed out in the first pages of the essay. However the *positive* method had to be applied always and everywhere, and from the outset that option automatically excluded any "supernatural element". We are facing here a great reductionist design, where all human practices, feelings and desires pivoted around a powerful positive science. History offered some resistance to the design of reduction for two reasons. First, the knowledge of past is "intrinsically incomplete", and second, every kind of experimentation was equally intrinsically precluded. At the same time, history showed "the continuous advancement of science, material conditions of life, and morality". The existence of an actual scientific and social progress was "the *a posteriori* results of historical studies". Therefore history could not be submitted to the positive method, but could show the positive effects of that method. In some way its intrinsic link to the positive method was placed at a different level.[129]

Once more a simplified and optimistic representation of the contemporary trend emerged.

> Sciences physiques, sciences morales, c'est-à-dire sciences des réalités démontrables par l'observation ou par la témoignage, telles sont donc les sources uniques de la connaissance humaine. C'est avec leurs notions générales que nous devons ériger la pyramide progressive de la science idéale. Aucun problème n'est interdit à celle-ci : loin de là, elle seule a qualité pour les résoudre tous, car la méthode que je viens d'exposer est la seule qui conduise à la vérité.[130]

Unfortunately the access to truth was not as easy for "ideal science" as for "positive science". Only the latter could attain "truth" and "certainty", and only in that context the "dense net of facts" could be suitably connected by "certain and demonstrable relations". The steadiness of positive science was in contrast with the instability of ideal science, which would have been "always variable". We can see an unbridgeable gap between Boutroux and Naville's philosophical questioning, and Berthelot's enthusiastic confidence in the Galilean rhetoric of a science based on sound experiments and certain demonstrations. In reality Galileo's epistemology was more sophisticated than Berthelot's, because he had explicitly acknowledged the necessity of two elements in the scientific enterprise: mathematical

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[128] Berthelot M. 1886, pp. 17-9, 25, and 30-1.

[129] Berthelot M. 1886, pp. 32 and 34-5.

[130] Berthelot M. 1886, p. 36. Berthelot did not take into account the possibility that different bodies of knowledge required different methods, because the right method was only the *positive* one.

procedures and a suitable experimental practice. On the contrary, in Berthelot's epistemology we only find the monistic trust in observation.[131]

He relied on the soundness and patency of his pyramid of knowledge, and blamed philosophers for still not having understood "the new method". In the last passage of his essay, he collected once more the virtues of the new positive and ideal sciences. Differently from "the ancient method", which was "dogmatic", the new one rested on the acknowledgment of "individual opinions and freedom". How to combine "freedom" with "the intrinsic certainty" of positive science was a mystery as unfathomable as the unquestionable certainty which spontaneously emerged from the collection of individual observations.[132]

A similar intellectual optimism towards science can be found in the texts of the authoritative French historian Ernest Renan. In 1890 he published a book which contained remarks and reflexions he had written down in 1848 but he had left unpublished at that time. He thought that those pages, which had stemmed from the mind of "a frank young man", deserved to be published after more than forty years, because he still shared the same ideas, and because he found that those ideas were substantially up-to-date. In the "Foreword" he confessed he had only slightly changed his mind since he had begun "to think freely": he professed a well defined "religion", which consisted of "the advance of reason, that is science". He continued to believe that "only science could improve the bad condition of man", even though he did not see "the solution as close as he had seen at the time".[133]

He found that religious zealotry was the worst danger for human society: the "ignorance and blind faith of ancient times" had transformed human beings into "a fanatic crowd". Definitely, "immoral people" had to be preferred to "fanatic people". Luckily, science had really advanced, and "apart from some disillusion", proceeded "along the pathway" he had imagined. Science had helped mankind to attain true knowledge of "the origin of life": nothing existed outside "mankind" and "nature", and there was "no place for creation in the chain of causes and effects". He saw the universe as the seat of "a continuous, huge progress": if cosmology was the science which described the first stages of that progress, human history "show us the last accomplishments". Science allowed scholars to rely on some certainties: among them, he stressed the fact that "neither preternatural facts nor mysterious revelations had ever taken place", and the fact that "inequalities among races" had firmly been established.[134]

On the other hand, he did not expect that science could offer answers to the most enduring and demanding questions: "neither the will of nature nor the aim of the universe" could be grasped. Although "the relentless work performed during the XIX century" had allowed scientists to astonishingly increase "the knowledge of facts", he acknowledged that "the lot of mankind" had become "more mysterious than ever". Renan appreciated science more than any other human activity: it could satisfy "the noblest ambition of human nature",

[131] Berthelot M. 1886, pp. 36-7.
[132] Berthelot M. 1886, pp. 38 and 40.
[133] Renan E. 1890, pp. V, VII, and IX.
[134] Renan E. 1890, pp. X-XIV.

namely "curiosity", and at the same time it could "supply man with the only means for the improvement of his destiny". Nevertheless science could not "lead to truth": it could only "protect against mistake". That negative feature was of remarkable help indeed, because it prevented human beings "from being duped".[135]

The last passage of the "Foreword" contained "a tremendous question", which was in reality a rhetoric detour: provided that the extinction of mankind was as sure as its beginning, could "science be more eternal than humanity"? The answer was as rhetoric as the question, and in some ways it was more a piece of literature than a specific commitment to an idealistic philosophy:

> Nous ne craignons vraiment que la chute du ciel, et, même quand le ciel croulerait, nous nous endormirons tranquilles encore sur cette pensée : l'Etre, dont nous avons été l'efflorescence passagère, a toujours existé, existera toujours.[136]

In the same year Naville published the second edition of his 1883 book, and quite a different attitude towards science emerged. He found that some "spiritualist consequences of physics", which he had then put forward, could be restated exactly. He saw a "resurgence of materialism", which led to "psychological determinism", that was its immediate "consequence". As an instance of that intellectual trend he mentioned the great success and the many editions of the book *Kraft und Stoff* the physiologist Ludwig Büchner had first published in 1855. Even though he found that "the philosophical value" of the book was "nearly naught", its great influence on the cultural environment deserved to be contrasted.[137]

### *2.6. Beyond positivism*

In 1895 Boutroux published a book on the concept of natural law in the scientific context from the point of view of philosophy: it contained both historical and critical remarks. The book had the subtitle "Cours de M. Emile Boutroux professé à la Sorbonne en 1892-3", and reproduced the text of the fourteen lectures he had held in that academic year. The first lesson, "The problem of the meaning of natural laws" started from the role played by Bacon and Descartes in the emergence of modern science. Boutroux credited both of them with having given scientific laws "the distinctive feature of universality and reality". According to Boutroux, they had in common the specific "ambition" to "know real things in a definite way", even though Bacon had followed "the empiricist" and Descartes "the rationalist" pathway. They had gone far beyond "the ancient view" of ancient scholars, who had imagined "general and ideal laws", and had confined themselves to the realm of "possible and plausible". Nevertheless, rationalism had not managed to overcome the dichotomy

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[135] Renan E. 1890, pp. XVI-XIX.
[136] Renan E. 1890, p. XX.
[137] Naville E. 1890, pp. II-IV. He specified that thirteen German and the three French editions of Büchner's book had already been published in 1874.

between “physics, namely the field of efficient causes, and morale, the field of final causes”. Nor had empiricism solved the query, because had recourse to “the features of mind, which could not be reduced to mere experience” in order to explain the laws of nature. Empiricists had simply reduced “external laws” to “the internal laws” of the subject.[138]

If Descartes had found it hard “to link the actual to the universal”, conversely Bacon had found it hard “to link the universal to the actual”. From the philosophical point of view, it was not very easy to imagine “natural laws” which were “universal and actual” at the same time. Nevertheless, where philosophy seemed to have failed, modern science seemed to have succeeded.

> Quand nous nous expliquons l’universalité, la réalité nous échappe, et réciproquement. Faut-il donc rapprocher purement et simplement le rationalisme et l’empirisme ? Le rapprochement de ces deux points de vue opposés ne donnera qu’une juxtaposition et non une synthèse. Or, ce qui, pour la philosophie, n’était qu’un idéal et un problème, la science l’a réalisé. Elle a su allier les mathématiques et l’expérience, et fournir des lois à la fois concrètes et intelligibles.[139]

Science had become independent of philosophy, and every specific science, namely mechanics, physics, chemistry, and sciences of life had striven “to look like mathematics”. The existence of a series of specific sciences allowed Boutroux to raise some questions concerning “nature” and the “objectivity” of them. Did their specific features stem from a mere “difference in generality and complexity”, or did they require different foundations? Did the passage from a science to another require a new “principle”, more specifically “the introduction of a philosophically non-reducible principle”? Did they represent “elements” of reality or were they simple collections of “symbols”? Were they “absolutely true” or only true in a relative sense? Eventually Boutrox asked whether “determinism” was an essential feature of “nature”, or was it simply the way “by which we must link things, in order to make them objects of mind”. They were “ancient questions”, and Boutroux was attempting to frame them into “a current perspective”.[140]

Following a positivistic trend, he discussed scientific languages and contents in accordance with a predictable classification: at first logic, and then mathematics, mechanics, physics, chemistry, biology, psychology, and sociology. Strict determinism and necessity ruled the laws of logic: as Boutroux wrote, “logic is definitely the most perfect instance of absolute necessity”. At the same time, “it offers a minimum of objectivity”, because of the “unbridgeable gap” existing between logic and reality. Mathematics also enjoyed that status of “certainty”, even though logic and mathematics were quite different in their nature, and corresponded to “very different ways of reasoning”. If logic assumed the existence of entities to be linked together, mathematics built up its own entities. In mathematics, the principle of recursion tried to merge induction with deduction, and it was a procedure which went far beyond pure logic: it was a sort of “apodictic induction”. Mathematical laws

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[138] Boutroux E. 1895, pp. 5-7 and 9. After having taught at the *École Normale Supérieure*, in 1888 he was appointed to the chair of History of Modern Philosophy at the Sorbonne. See Craig E. (ed.) 1998, vol. 1, pp. 850-2.

[139] Boutroux E. 1895, p. 9.

[140] Boutroux E. 1895, pp. 10-11.

entailed "a very complex development", where both analysis and synthesis were at stake, as well as "a priori" and "a posteriori" lines of reasoning. However, he found that the existence of logic and mathematics showed that our mind was in need of a rational steadiness, and that some kind of structural similarity between the mind and the external world could be surmised.[141]

He devoted two lessons to "mechanical laws" and the next one to "physical laws", where Mechanics represented a body of knowledge which was included in the wider body of Physics. According to Boutroux, among "the laws of reality", "the laws of mechanics" were the most akin to the laws of mathematics. The most important among mechanical laws was "the principle of inertia", which marked the difference between ancient and modern natural philosophy, because it stated the equivalence between motion and rest. That equivalence had led Descartes to the abolition for the concept of force, but Newton had shown the necessity of a new concept of force, which was definitely "an extra-mathematical element". If Descartes had attempted to reduce physics to geometry, Newton had pointed out the impossibility of a too sharp reduction. Nevertheless, the stress on the mathematical character of natural laws corresponded to "the effort to fit things to our mind". That effort was in essence the keystone of modern science. The essential features of mathematical laws were "*continuity*" and "*immutability*", whereas our observations take place at "times which are separated from each other", and the natural world (living species, for instance) "has a history". Boutroux therefore stressed that continuity and immutability were not essential features of natural events in themselves.[142]

From Descartes onwards, mechanical laws suffered from "dogmatism", and even Leibniz and Newton were "dogmatic", even though "Newtonian science" should not be confused with "Newtonian metaphysics". Concepts like "homogeneous space devoid of any quality" and "extended and indivisible atom" stemmed from Newtonian metaphysics, and they were "contradictory" concepts. The representation of the natural world as a mechanical engine, whose behaviour was deterministic and completely defined by equations and initial conditions, was more a mythology than a matter of fact. Boutroux reminded readers that the mathematical physicist Boussinesq had pointed out that sometimes "initial conditions cannot define completely the way taken by the phenomenon". As Boussinesq himself had explained in 1878, "bifurcations" and "the action of a guiding power" were at stake.[143]

In the end, mechanical determinism appeared to Boutroux as the source of an unnatural separation between "laws" and "phenomena".

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[141] Boutroux E. 1895, pp. 19-24 and 30. See in particular, *Ibidem*, p. 30: "Les lois logiques et mathématiques témoignent du besoin qu'a l'esprit de concevoir les choses comme déterminées nécessairement ; mais l'on ne peut savoir *a priori* dans quelle mesure la réalité se conforme à ces symboles imaginés par l'esprit : c'est à l'observation et à l'analyse du réel qu'il appartient de nous apprendre si la mathématique règne effectivement dans le monde. Tout ce que l'on peut admettre, avant cette étude expérimentale, c'est qu'il y a vraisemblablement une certaine analogie entre notre nature intellectuelle et la nature des choses. Autrement l'homme serait isolé dans l'univers."

[142] Boutroux E. 1895, pp. 30-32, 34, and 37-8.

[143] Boutroux E. 1895, pp. 39-42 and 46. In 1895, Boussinesq held the chair of "Mécanique physique et expérimentale" at the Faculty of Science in Paris, which had been held by Henri Poincaré until 1886.

> La distinction des lois ou rapports et des phénomènes ou éléments, calquée sur celle des préceptes et de la volonté, est un artifice de l'esprit pour réduire en idée la plus grande part possible de la réalité donnée. Dans l'être même, cette distinction s'évanouit et, avec elle, le déterminisme qui la suppose.[144]

In the sixth lesson Boutroux faced the core of his philosophical remarks on science: he asked the reader whether the whole field of physics might be reduced to mechanics, in particular whether physical laws were "a specific case of mechanical determination", or whether they possessed "their own originality and meaning". This question concerned the whole hierarchy of positivistic sciences: logic, mathematics, mechanics, physics, chemistry, sciences of life, psychology, and sociology. Could scientists and philosophers rely on a comfortable reductionism, where mathematics stemmed from logic, mechanics from mechanics, …, and psychology from the sciences of life, or did every science require specific contents and methods? Boutroux knew that a reductionist pathway had been undertaken in physics, in particular in the context of thermodynamics. According to Boutroux, "the mechanical theory of heat" was a piece of evidence for a two-fold trend: "to rule out qualities", and "to reduce physics to mechanics".[145]

He was aware of the main issue at stake in the context of thermodynamics: the apparent incompatibility between the reversibility of purely mechanical processes and the irreversibility in physics or "actual mechanics", where "every work gives rise to heat". Moreover thermodynamics required a sort of qualitative hierarchy in the field of energy, namely "an element of differentiation and heterogeneity": the quality of ordered mechanical work was greater than the quality of disordered energy or heat.

> On peut établir comme règle universelle que toutes les fois qu'il y a travail, il ya, avec une production de chaleur, perte irréparable de la condition primitive. Cette loi introduit en physique un élément différent des éléments mécaniques. En mécanique, on considère une force qui conserve toujours la même nature et la même qualité ; en physique, au contraire, la qualité diffère ; le travail est d'une qualité supérieure à la chaleur ; la chaleur à 100° est d'une qualité supérieure à la chaleur à 99°. Jamais la chaleur ne reconstitue intégralement le travail dont elle est issue ; la qualité de l'énergie va toujours en diminuant, comme il résulte du principe de Clausius ; les phénomènes sont irréversibles, le résultat final est toujours une déchéance. Qu'est-ce à dire, sinon que la physique ne peut faire abstraction de la qualité, au moins de la qualité ainsi entendue ?[146]

Boutroux looked upon the principle of conservation of energy as a "pattern of laws" rather than "a single and well-defined law". Moreover, the principle made sense only if there was a change in the nature or status of energy, and that change consisted in the fact that "its quality was decreasing". In other words, a change was required in advance, in order to state "that energy is preserved throughout any change". In reality, "Clausius's principle" was nothing else but a principle "of change": the negative statement which corresponded to the

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[144] Boutroux E. 1895, p. 50.
[145] Boutroux E. 1895, pp. 51-2.
[146] Boutroux E. 1895, p. 54.

principle pointed out the impossibility to "determine phenomena in a definite way": it could not "bring about a complete determination".[147]

In reality, Boutroux saw a profound difference between the two principles of thermodynamics, and that difference corresponded to the difference between "necessity" and "determinism. He referred "necessity" to the ontology of a physical entity, and "determinism" to the set of connections and conditions of existence of that specific entity. If the first principle corresponded to a necessity but it was not deterministic, the second was deterministic but it did not correspond to a necessity.

> Il faut bien se garder, en effet, de confondre déterminisme et nécessité : la nécessité exprime l'impossibilité qu'une chose soit autrement qu'elle n'est ; le déterminisme exprime l'ensemble des conditions qui font que le phénomène doit être posé tel qu'il est, avec tous ses manières d'être. La loi de conservation est une loi de nécessité abstraite, mais non une loi de déterminisme ; d'autre part, tout loi qui, comme le principe de Clausius, règle la distribution de la force, est bien une loi de déterminisme, mais est et demeure exclusivement expérimentale. Une telle loi n'est plus, comme la loi de conservation, une condition d'intelligibilité.[148]

In order to allow readers to understand what a deterministic (but not necessary) law was, Boutroux mentioned the mathematical law of attraction: the dependence of force on the inverse square of distance rather than the inverse of distance was not "a condition of intelligibility" but a "merely experimental law". It dealt with determinism rather than necessity. If necessity involved logical constraints, specifically the law of identity "*A est A*", determinism involved observation and experience, namely empirical constraints. In the end, he found that necessity and determinism, namely "logic and experiment" could not merge easily with each other. Once again, the gap between logical and empirical processes was hard to bridge: in other words, he found either "necessity without determinism", or "determinism without necessity".[149]

Boutroux drew readers' attention to Stallo's criticism of atomism: "homogeneity, hardness, and inertia of atoms" were assumed by chemists despite the logical and physical difficulties which emerged from those assumptions. Boutroux acknowledged that "the atomic theory" had been useful and had given chemistry "a precious notation"; on the other hand, that theory "was not able to determine metaphysically the nature of matter". From a very general point of view, Boutroux agreed with Charles Friedel, who had written the Introduction to the French translation of Stallo's book, on the provisional role of a scientific theory. However we should continue to make use of a fruitful theory, its logical and physical faults notwithstanding. Scientists relied on "the wave theory of light", even though they were aware of "the contradictions in the conception of the luminiferous aether". In the

[147] Boutroux E. 1895, pp. 56-7.

[148] Boutroux E. 1895, p. 58. It seems that Boutroux looked upon *necessity* as the hypostasis of an entity, and *determinism* as the hypostasis of a relation.

[149] Boutroux E. 1895, pp. 58-9. He was showing how questionable was the traditional methodology of modern science from the philosophical point of view, in particular Galileo's alliance between *reasonable experiences* and *definite demonstrations*.

end, after a short historical report of atomism in the context of the eighteenth and nineteenth century, Boutroux concluded that “atomism can account for everything”, provided that the atom was endowed with “the features which it should explain”.[150]

On the track of the physiologist Claude Bernard, who had stated that the distinctive features of living beings were “organisation, generation, evolution, nutrition, frailty, illness, and death”, Boutroux wondered what “the relationship between vital power and physical-chemical features” was.[151] Once more the question at issue was reductionism: Boutroux' answer was in opposition to reductionism, and even the essential tension between determinism and necessity reappeared on the stage.

> En résumé, les lois de la physiologie apparaissent comme irréductibles. Le déterminisme physiologique, considéré en lui-même, diffère du déterminisme physico-chimique. Comme celui-ci différait du déterminisme purement mécanique. Il est plus étroit, puisqu'il règle des phénomènes que les lois physico-chimiques laissent indéterminée. Mais il repose sur une notion de loi plus complexe et plus obscure, à savoir la relation d'un fait, non seulement avec un autre fait, mais avec un fait posé comme fin du premier. Le déterminisme, en se resserrant, devient plus impénétrable et plus irréductible à la nécessité.[152]

In the conclusive chapter of his collection of lessons, Boutroux attempted to set the emergence of modern science, and XIX-century scientism and reductionism, against the background of the history of philosophy. In particular, he compared the alliance between mathematical language and experiments, which had been the hallmark of the scientific tradition between the XVII and XIX centuries, with “the ancient philosophy”, which on the contrary rested on the corresponding “dualism”. In other words, ancient philosophers halted in front of the unbridgeable gulf between logic and mathematics, on the one hand, and experiences, arts and crafts, on the other. In more philosophical terms, they stepped back before the invincible gap between “the realm of eternal and necessary”, where “truth” had its seat, and the iridescent and shaky realm of “phenomenon”. Modern natural philosophers had dared to overcome that dualism: in philosophical terms they trusted in the possibility of conflating “the science of being” and “the science of becoming”.[153]

What Boutrox wrote echoed what the early Aristotelian tradition had expressed in the texts collected under the title *Posterior Analytics*, where those ancient philosophers had inquired into the rift between logical structures and experiences.

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[150] Boutroux E. 1895, pp. 64-5. Stallo had made the same remarks, indeed, when he made reference to the “delusions that the elasticity of a solid atom is in less need of explanation than that of a bulky gaseous body. His criticism was definitely sharper than Boutroux'. See Stallo J.B. 1882, p. 128: “It may seem strange that so many of the leaders of scientific research, who have been trained in the severe schools of exact thought and rigorous analysis, should have wasted their efforts upon a theory so manifestly repugnant to all scientific sobriety – an hypothesis in which the very thing to be explained is but a small part of its explanatory assumptions.”

[151] Boutroux E. 1895, pp. 74, 77, and

[152] Boutroux E. 1895, p. 82. At the end of the second lesson devoted to “biological laws”, he re-stated that the stronger became the determinism, the weaker became its link with “necessity and mechanicism”. Once again, the overlap between the two concepts was looked upon by Boutroux as “the mistake of contemporary philosophy. See *Ibidem*, p. 102.

[153] Boutroux E. 1895, p. 135.

> Or la science moderne a pour caractère essentiel de tendre à abolir cette dualité. L'idée fondamentale en a été formulée par Descartes ; elle consiste à admettre qu'il y a un point de coïncidence entre le sensible et le mathématique, entre le devenir et l'être, que les choses sont, non pas des copies plus ou moins imparfaites des paradigmes intelligibles, mais des déterminations particulières des essences mathématiques elles-mêmes. […] Nulle connaissance empirique ne pouvait, comme telle, pour Aristote, prétendre à l'universalité et à la nécessité. […] Intimement unies, elles fondent une science absolue de la réalité sensible elle-même. Les mathématiques communiquent à la science la nécessité ; l'expérience, la valeur concrète. Telle est la racine du déterminisme moderne.[154]

Boutroux specified that the tight alliance between formal structures and experiences was simply a "belief", or something which was not *scientific* in itself: it was something like a faith or a mythology. The chain of subsequent reduction, which starting from logic should have led to sociology, was broken from the outset, because mechanics had "elements which were not reducible to purely mathematical determinations". In the end, Boutroux put forward two conclusions. First, the reductionist approach to nature had stemmed from the "ignorance" of the "incommensurability between reality and mathematics", but it had had "pleasant effects", namely the emergence of modern science itself. Second, the concept of one "science" which encompassed all sciences was misleading, because it was a mere "abstraction". There were many specific sciences, endowed with "their specific features [physionomie]". Alongside the series of different sciences, which went from astronomy to "the study of life and mind", he found a long series of "assumptions". They had a hierarchical structure: few and simple at the beginning, and "ever more plentiful and unfathomable" at the end.[155]

[154] Boutroux E. 1895, pp. 135-6.
[155] Boutroux E. 1895, pp. 136, 138-9, and 141.

## *3. Pierre Duhem from theoretical physics to meta-theoretical commitments*

In 1892 the young physicist Pierre Duhem published the first paper explicitly devoted to meta-theoretical issues or, to make use of a more recent expression, to philosophy of science.[156] The paper, whose title was "Quelques réflexions au sujet des théories physiques", was the first of a series of papers he published in the 1890s in the journal *Revue des questions scientifiques*. The journal was published by the *Société scientifique de Bruxelles*, which was an association of catholic scientists. Its aim was the presentation, discussion and critical account of scientific theories, without making recourse to mathematical details, but with particular attention to models, concepts, principles, and methodological issues. At that time Duhem had already published a book on thermodynamic potentials and their applications to different fields of physical sciences, and a demanding paper, "Equations générales de la thermodynamique", where he put forward an original mathematical approach to thermodynamics on the track of Analytical Mechanics.[157]

### *3.1 Duhem's first paper and the subsequent debate*

From the outset, he represented the scientific enterprise as a three-stages task: from the knowledge of "specific facts", the human mind was able to derive some "*experimental laws*" by induction, and then create a scientific theory. If the "scattered set" of facts dealt with the first level of pure "*empirism*", and the set of physical laws belonged to the level of the "*purely experimental science*", the set of physical theories had its seat in "*theoretical science*". If the objects of experimental laws were facts, the objects of physical theories were experimental laws. The nature of theoretical physics was just the subject Duhem was to investigate, and the investigation would have focussed on what he considered the most refined instance of theoretical physics, namely "*mathematical physics*". Physical theories were a sort of "relief for memory": they synthesised the body of knowledge stored in experimental laws, and at the same time they offered a general mathematical framework. The passage from laws to theories was a passage between different fields of knowledge, both of them endowed with a specific language. The mathematical engine of a theory performed a reinterpretation of the laws: it offered a sort of "picture" of them. The "*nature*" of the laws and the nature of their theoretical representations were definitely different.[158]

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[156] At that time, Duhem was 32, and was "maitre de conférences" at Lille University: in the same year, his wife died and he remained alone with a baby. For further details, see Jaki S.L. 1984, pp. 97-9, and Brouzeng P. 1987, p. 54.

[157] See in particular Duhem P. 1886, and Duhem P. 1891. The long list of Duhem's publications can be found in Duhem P. 1913, Jaki S. 1984, and Stoffel J.F. 2007.

[158] Duhem P. 1892b, in Duhem P. 1987, pp. 1-3.

The passage from one level to another required a sort of conceptual shift. As Duhem specified in the course of the paper, even the best known physical entities had no direct link with the corresponding experiences: if the human experience of heat could appear as "agreeable or disagreeable", the mathematical representation of temperature could be "added to another temperature, and multiplied or divided by a number". The correspondence between entities belonging to different levels entailed a sort of translation, and just as in every act of translation, a plurality of choices was available: there was no constraint, no necessity. The physical concept of temperature, for instance, had to satisfy two mathematical conditions: the same value had to be associated to equally warm bodies, and a greater value for a body A when A was warmer than B. According to Duhem, "every physical entity endowed with these properties could be chosen as *temperature*".[159]

The hypotheses of a theory enjoyed the same freedom which was enjoyed by basic entities or "definitions": the only constraint was the possibility to derive "*experimentally verifiable consequences*" from them. If the set of consequences was "wide-ranging" and in accordance with "experience", the theory had to be looked upon as good. Nevertheless, the choice of hypotheses should not be made "at random": there was to be an "ideal and perfect method". The last statement appears a bit surprising because of Duhem's stress on the conceptual gap between laws and theories, and the plurality of theories corresponding to a given set of laws. Such a method would consist in choosing hypotheses which were "the symbolic translation … of some experimental law belonging to the set to be represented". In reality Duhem found unsatisfactory such a procedure because a theory was something more sophisticated than a collection of laws, and offered a conceptual content richer than the mere superposition of experimental laws. The most meaningful instance of this theoretical surplus was Newton's theory of universal gravitation, which was not a mere translation of Kepler's laws in a formal language of higher level: it also contained propositions which could not be "derived by experience", but were "the result of" a further "elaboration".[160]

As a result of this conceptual surplus, there was a conceptual gap between the "consequences of the theories" and the experimental laws which they would represent. This was a very sensitive issue, and Duhem was aware that he had "to insist on it": there was an inescapable gap between the statements derived from the abstract structures of a theory, and the statements derived from the empirical practice and translated into laws.

> … une bonne théorie n'est pas une théorie dont aucune conséquence n'est en désaccord avec l'expérience; à prendre cette toise, il n'y aurait aucune bonne théorie; il est même vraisemblable que la création d'une bonne théorie surpasserait les forces de l'esprit humain. Une bonne théorie, c'est une théorie qui symbolise d'une manière suffisamment approchée un

[159] Duhem P. 1892b, in Duhem P. 1987, pp. 5-6.

[160] Duhem P. 1892b, in Duhem P. 1987, pp. 7-10. According to Duhem, the theoretical surplus of Newton's theory over Kepler's laws was two-fold: the reciprocity of the attraction, and the extension of the attraction to any couple of bodies.

ensemble étendu de lois physiques; qui ne rencontre de contradictions dans l'expérience que lorsqu'on cherche à l'appliquèrent dehors du domaine où l'on en veut faire usage.[161]

An important consequence followed: "the value of a theory" depended on "the set of laws to be summarised by this theory", and on "the degree of precision of the experimental methods by which the laws are set up or applied". In other words, a good theory might become a bad theory if "the boundaries of the field of application" were enlarged or shifted, or "the degree of experimental precision" was improved. Another consequence followed: scientists could decide to replace a good theory with a better one, but the choice in favour of the latter would not mean the wrecking of the former. The logic of scientific theories was not of the kind *true or false*: the better theory could be derived from the previous good one by some kind of conceptual enlargement or re-arrangement.[162]

Only at this point did Duhem go back to the "ideal form" of a physical theory, which was to be "the mere symbolic translation of an experimental law". Such a theory would not be satisfactory, because it would be "difficult to modify". On the contrary, when the hypotheses of the theory are far from the laws which they should explain, the theory is more general and pliable. Nevertheless, a heavy price was to be paid for these qualities: a more general theory was also more unsteady and subject to confutation or "destruction". The wider the scope of a theory was, the greater the risk of default. According to Duhem, a theory was a complex and pliable entity: a good theory was also wide-scope and pliable, but its strength was also its weakness.[163]

According to Duhem, the history of physics offered a bad example, or a "false ideal", of scientific theory: "the *mechanical theory*". In it, every physical entity had to be represented by means of "geometrical and mechanical elements of a given imaginary system". A specific instance of mechanical theory was the "*mechanical theory* of light", where the properties of light had to be derived from the mechanical properties of aether. Different implementation of that model had been put forward over time: a continuous versus a molecular aether, forces at a distance versus contiguous actions between elements of aether, and so on. Duhem refused mechanical theories mainly because of the constraint they imposed on basic entities and hypotheses: in particular, non-mechanical entities had to be represented as "the combination, sometimes very complicated, of mere mechanical concepts". He made reference to basic concepts like temperature, which had actually received a complex re-interpretation in terms of microscopic matter and motion. In any case, Duhem found that the historical trend was not in favour of mechanical theories: even the "the most complex" among them "had not managed to satisfactorily account for Carnot's principle", namely the second Principle of Thermodynamics. That failure had not prevented

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[161] Duhem P. 1892b, in Duhem P. 1987, pp. 11-12. These remarks would not have astonished the readers only if they had seriously taken into account the Aristotelic tradition, but modern science had grew up just in opposition to that tradition. This was the philosophical wound which subsequently Duhem tried to heal.

[162] Duhem P. 1892b, in Duhem P. 1987, pp. 12-13.

[163] Duhem P. 1892b, in Duhem P. 1987, p. 15.

the field of physics which had been labelled "*mechanical Theory of Heat*" from becoming "one of the most perfect *physical* theories under the name of Thermodynamics".[164]

Duhem's specific distrust in every mechanical theory stemmed from a more general distrust in every attempt to explain the nature of the material world, which he qualified as "a metaphysical explanation of the universe". He though that "the exact structure of the world" was unattainable, and that every attempt to grasp it would be doomed from the start: we would attain "a flimsy structure, which quickly collapses and needs to be replaced". Duhem was aware that the faith in scientific progress as the driving force of social and intellectual progress exerted a strong pressure on scientists and their scientific practice in the last decades of the nineteenth century. He found that "the crowd" demanded both "immediate applications directed to satisfy material needs", and "explications … directed to satisfy the ambition to all understand". The intellectual pressure was just as strong as the social pressure: the search for "the nature of things" concerned all kinds of people, "from the most superstitious savage to the most curious philosopher". Nevertheless, the scientist had to resist this pressure: a physical theory could offer nothing more than "a systematic coordination of laws", rather than "an explication of those laws".[165]

Thermodynamics offered a meaningful instance of the struggle between the two theoretical attitudes. On the one hand, there were both the interpretation of temperature as a symbolisation of "the notion of heat", and the principles of Thermodynamics as "generalisations of experimental laws". On the other, he found "a huge number of microscopic bodies in stationary motion", temperature interpreted as their "mean living force", a set of "convenient assumptions on their number, dimensions, and motions", and an attempt to derive "the principle of equivalence between heat and work, not to say Carnot's principle". If the latter attitude represented the specific kind of theoretical practice where the search for "the nature and causes of physical laws" was at stake, the former confined itself to a symbolisation of those laws, and called into play physical entities which could not be reduced to geometry and mechanics. He was strongly convinced that scientist should not be compelled to express "complex concepts" like temperature and quantity of heat in terms of "space, time and mass".[166]

Duhem acknowledged that Descartes, Newton, Huygens, Laplace, Poisson, Fresnel, and Cauchy's mechanical theories "had allowed science to greatly progress". How could "such a wholly mistaken idea on the role of physics" have led to that great success? Duhem's answer was both conceptual and historical, although not completely convincing: mechanical theories represented the "childhood" of science, when its progress was faster, but at the same time its role was "more roughly defined". The emergence of physics was associated with a sort of over-confidence in the power of mechanical models. However, for the time

[164] Duhem P. 1892b, in Duhem P. 1987, pp. 16-20. Duhem's definition of mechanical theory was quite abstract, and contained a linguistic vicious circle. The traditional definition of mechanical theory as a theory built up in terms of matter and motion would have been quite clearer.

[165] Duhem P. 1892b, in Duhem P. 1987, pp. 20-21.

[166] Duhem P. 1892b, in Duhem P. 1987, pp. 22-4.

being, Duhem saw the "decline of mechanical theories", and the emergence of "purely physical theories".[167]

In the context of that meta-theoretical debate, another issue was at stake: the existence of tight bonds between physics and philosophy. When physicists considered a physical theory as "an explanation of the laws of nature", they were committed to a definite philosophical attitude, and they could not but lend towards "a theory consistent with their philosophical belief". Simmetrically, when philosophers believed that "the nature of material phenomena" could be found in some physical theories, they were oriented to "draw their inspiration from those theories" in order to develop their metaphysical system. The close relationship between Descartes' physics and metaphysics was a meaningful example of that tight bond. He found that, in more recent times, Herbert Spencer's philosophy had been "heavily influenced by ideas borrowed from some thermodynamic theories". According to Duhem, only the awareness of the merely symbolic role of physical theories would have allowed scientists to become "independent of fashionable metaphysical systems", and at the same time "to give up their influence on metaphysics".[168]

Duhem quoted from and commented on two passages from the treatise which "the renowned analyst" Poincaré had published on optical theories in 1889. He agreed with Poincaré on the aim of physical theories: they could not "disclose the true nature of things", but only "coordinate the physical laws which experience allows us to understand". He disagreed with him on the meta-theoretical trend which led to "look upon the different theories, associated to a set of laws, as equivalent". He believed in the possibility of judging the relative value of different theories, and insisted on the importance of three main features: first of all the "scope of a theory", and "the number … and nature of hypotheses". The third feature was definitely the most sensitive, and Duhem attempted to qualify it in some way: the hypotheses of the best theory had to be "the simplest, the most natural, and in the closest connection with the data of experience".[169] That feature was really difficult to explain, and he confined himself to a very specific example: the comparison between Lamé and Cauchy' theories of double refraction. However, in the end, he put forward an optimistic synthesis between relativistic and dogmatic attitudes.

> Ainsi, en affirmant que la Physique Mathématique n'est pas l'explication du monde matériel, mais une simple représentation des lois découvertes par l'expérience, nous évitons l'obligation de déclarer vraie, pour chaque ordre de phénomènes, une théorie à l'exclusion de toute autre. Mais nous ne sommes pas condamnés pour cela à adopter toutes les théories, logiquement constituées, d'une même ensemble de lois: nous avons, pour choisir entre elles, des règles très sûres, qui, bien souvent, nous permettrons de préférer raisonnablement l'une d'entre elles à toutes les autres.[170]

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[167] Duhem P. 1892b, in Duhem P. 1987, pp. 24-6. In this context, "physical" means "non-mechanical". Physics and mechanics appear as complementary sections of science, rather than linked by the hierarchical relationship where mechanics is a part of physics.

[168] Duhem P. 1892b, in Duhem P. 1987, p. 26.

[169] Duhem P. 1892b, in Duhem P. 1987, pp. 27-8 and 31-2, and Poincaré H. 1889, p. II.

[170] Duhem P. 1892b, in Duhem P. 1987, p. 32.

In the last, brief section of the paper Duhem focused on the meaning and the "*usefulness*" of theoretical physics. Beyond the mere alliance between "experience and mathematical analysis", theoretical physics called into play the necessity of "a systematic linkage", and some kind of "speculation", in order to give sense and structure to "the knowledge received from the experimental method". How could scientists content themselves with "the confused and inextricable accumulation" of laws derived by experience?[171]

Duhem's paper raised some debate. In 1893, the first issue of the *Revue de questions scientifiques* hosted a long paper, sent by the engineer Eugène Vicaire: the author immediately claimed that "Duhem's fundamental thesis" was "false". The thesis under attack was what Duhem considered the aim of theoretical physics: the symbolic representation of physical laws, rather than their explanation in a metaphysical sense. Vicaire acknowledged that Duhem's thesis was widely shared by renowned scholars like the mathematician Henri Poincaré and the physicist Gustav Kirchhoff, and he found that it could be traced back to David Hume. Although he quoted with care from Poincaré's 1889 treatise on the mathematical theories of light, in no way did he try to point out the differences between Duhem and Poincaré. He was committed to fight a philosophical battle against some ideas which he looked upon as "destructive of every science", and he did not hide his disappointment regarding the "invasion of scepticism" in a journal which would have been "extraneous" to it. He would have tried a refutation: he had realised that "the danger was greater" than he had expected, and he found that "the necessity to contrast it" was "more pressing".[172]

Vicaire accused Duhem of not being able to "distinguish" between "applicative and explicative theories". Unfortunately, what he termed applicative theories was nothing else but what Duhem had termed "laws": the example he mentioned, the "laws of reflexion and refraction of light", is clear in this regard. What he termed explicative theories was just what Duhem had termed in the same way, apart from the adjective "explicative", which showed that he looked upon theories as actual explanations. In reality, he did not try to convince the reader that physical theories were explanations of the physical world: he contented himself with mentioning a "common and traditional point of view", which had been "always correct". When he concluded that only explicative theories were "real scientific theories", this was neither a confutation of Duhem's point of view, nor an effective line of reasoning, but a mere, legitimate philosophical claim.[173]

Vicaire's subsequent question is more interesting: was the coordination of physical laws the only aim of a physical theory? In reality he thought that there was something more meaningful: "the essential merit of theories", if not their actual "raison d'être", dealt with the beauty and "harmony" they introduced in the web of knowledge. Not only was the contemplation of that beauty "the highest satisfaction of mind", but also "the final aim" of science. Beside this main aim, there was the "usefulness" of theories, namely the possibility of "extending the scope" of laws, and even discovering new laws, or new fields of

[171] Duhem P. 1892b, in Duhem P. 1987, p. 37.
[172] Vicaire E. 1893, pp. 452-3.
[173] Vicaire E. 1893, pp. 453 and 456.

application. Furthermore, Vicaire also extended the "merit" of theories to laws: they owned an "intrinsic beauty", because of "the order they let emerge from nature". The three distinctive features of laws were "their practical usefulness, their intrinsic beauty, and the generation of theories". The third feature overturned in some way the relationship between laws and theories which Duhem had put forward. The progress towards the knowledge of the physical world required two steps: "from phenomena to their relations, and from relations to causes". That laws would generate theories, and that theories would be assimilated to causes, was definitely extraneous to Duhem's point of view on science.[174]

Vicaire faced many sensitive issues concerning scientific methodology, and put forward some bold and interesting theses. He stated that "crude facts" and "the subtlest hypotheses" had the same nature, and a series of intermediate entities from the former to the latter could be imagined. A final explanation of the physical world could legitimately be desired and imagined, even though never accomplished. The search for "an hypothesis on the structure and initial state of matter", and the hope to find it, also had a psychological root. Nobody could bear the effort to pursue "a scientific research for more than five minutes" unless he was attracted by "the unknown", and by the possibility of attaining "some mysteries of nature". This kind of natural "instinct" was stronger than "the prohibitions of a philosophy" which claimed to be "positive" but in reality was "negative".[175]

At this point Vicaire took into account similarities and differences between Poincaré and Duhem's meta-theoretical attitudes. Not only had Poincaré acknowledged the plurality of the theoretical interpretations of a given class of phenomena, but he had also written that the existence of mutually inconsistent components of a theory could be tolerated. Vicaire acknowledged that Duhem had not dared so much, but attributed his lack of radicalism to a sort of "fear". These are perhaps the most disagreeable passages of the paper, which are redeemed only in part by a footnote of appreciation for another paper Duhem had subsequently published in the same journal. Vicaire blamed Duhem for having "let himself be intimidated" by August Comte's "school", in order to "gain his certificate of civic spirit". He went on with his unpleasant and psychologically oriented remarks: the concern for "being compromised with metaphysics", had led Duhem to become quite similar to his "enemies". The accusation was false as to the content, and unfair as to the style, for Duhem was known for his independence of mind, and had never tried to please anyone.[176]

When Vicaire resumed his more rational line of reasoning, he stressed once more that "the search for natural truth and conformity to nature, and the closely related concept of cause" were at stake in physical theories. The "conformity, at least in part, to nature" was indeed the hallmark of a good theory, and it could not be attained by "pure chance". Unfortunately, on this specific issue, Vicaire's remarks became confused: he acknowledged that, from the logical point of view, "false assumptions" could lead to "true conclusions", but this fact did

[174] Vicaire E. 1893, pp. 453 and 459 and 461-3. I have translated the French "esprit" into the English "mind", even though the semantic scope of the two words are quite different. I have not found anything better.

[175] Vicaire E. 1893, pp. 468 and 472-4.

[176] Vicaire E. 1893, pp. 476 and 482. Vicaire's reference to Poincaré was correct only in part: Poincaré pluralism did not involve the presence of contradictions in the same theory. See Poincaré H. 1890, p. VIII: "Deux théories contradictoires peuvent en effet, pourvu qu'on ne le mêle pas, et qu'on n'y cherche pas le fond des choses, être toutes deux d'utiles instruments de recherches, …"

not imply the equivalence between “true and false assumptions”. He did not managed to grasp Duhem’s reflection on the problematic link between the abstract, rational nature of deductive procedures, and the empirical nature of observation and experience. Duhem had realised that no automatic connection between the two domains could be found, and theoretical physics occupied the wide space between them. Moreover, the effectiveness of the deductive *engine* was insensitive to the content of the hypotheses. The plurality of theoretical interpretations dealt with the inescapable gap between reason and experience: their different meta-theoretical attitudes notwithstanding, Duhem and Poincaré were aware of that gap. In the last passage of the paper, Vicaire appealed to “old, perpetually true principles”, and invited scientists to ”know nature, rather than handle symbols”. He claimed that it was a “noble ambition”, and the value of “truth and science” resided just in it.[177]

In the same year, another engineer, George Lechalas, took part in the debate triggered off by Duhem’s paper. He published a short answer to Vicaire in the june juillet 1893 issue of the neo-Thomist journal *Annales de philosophie chrétienne*, where Vicaire’s paper had been re-published in the april-may issue. After having stated that he agreed with Vicaire on almost the whole content of his paper, he confined himself to pointing out what he considered “an inaccurate notion of mechanical representation”. He stressed the difference between two different implementations of Mechanics: there was a mechanics based on the concept of force, and a mechanics which was not based on it but did not reject “some hypotheses on the mutual actions among bodies”. Lechalas phenomenological attitude was not far from Duhem’s: he stressed that, when we perform experiments, we get in touch only with “motions, together with their velocities and accelerations”. At the same time, differently from Duhem, Lechalas did not reject the concept of cause in general, but only “the anthropomorphic representation of causes”.[178]

### *3.2 Duhem’s second paper and other criticisms*

In 1893 Duhem published a paper in the *Revue des questions scientifiques*, where he reviewed a book which had been published by father Armand Leary, “philosopher, theologian, and scientist”, some years before. Duhem appreciated the fact that Leary had separated the “rights of divine revelation” from “the rights of science”: in particular, he had never based “a deduction leading to a scientific truth” on “a revealed truth”. Alongside the detailed description of Leray’s scientific conceptions, and historical remarks on the nature of matter and actions at a distance, he stressed two concepts: the different methods and aims of science and religion, and the different methods and aims of physics and metaphysics. If the first difference had clearly been stated by Leray, the second had already been stated by Newton. Duhem credited Newton with having considered physics as the reduction “of a great number of experimental laws to a few number of theoretical principles”, and

[177] Vicaire E. 1893, pp. 492-3 and 410.
[178] Lechalas G. 1893a, pp. 278-80.

metaphysics as the search for "the causes from which principles stem". Physics could really exist without metaphysics, although it intrinsically involved "an incomplete knowledge of the world".[179]

After some months, he published another paper in the *Revue des questions scientifiques*, which was specifically devoted to the problematic link between physics and metaphysics, and was intended as an answer to his critics, in particular Vicaire. Duhem's starting point was the distinction from physics and "*cosmology*". He reminded the reader that "physics" encompassed "perception of facts, discovery of laws, and building up of theories". The label cosmology corresponded to "the search for the nature of material things as causes of physical phenomena", living matter included. Cosmology was a subsystem of "*metaphysics*". Since he was aware that his definitions could generate some misunderstandings when compared to the philosophical tradition, he faced explicitly the comparison between the two sets of labels. What he had labelled cosmology corresponded to peripatetic "physics". What he had labelled metaphysics corresponded to the union of peripatetic "physics" and "metaphysics". Physics "in the modern sense" had no correspondence in peripatetic classification of knowledge: in ancient times, astronomy represented the subject and the actual practice closer to modern physics.[180]

According to Duhem, the distinction between physics and metaphysics did not depend on the subject matter under investigation, but "on the nature of our mind". Only "an angelic mind" could fill the gap between physics and metaphysics, and attain "a direct insight into the nature of things". Metaphysics was not a superior kind of knowledge in an absolute sense, because metaphysical knowledge required physical knowledge as a premise. He insisted on this specific issue: there was a "logical priority of physics over metaphysics". Furthermore, there was a deep asymmetry between them: the logical path leading from physics to metaphysics could not be univocally reversed. The knowledge of the physical world could legitimately lead to some hypotheses "on the nature of material things": then we might "get down the stair", and deduce new "phenomena … and laws" from those hypotheses. Unfortunately the procedure was unsafe because of the asymmetry between causes and effects: while "the knowledge of causes entails the knowledge of effects", a given effect "may stem from different causes". In other words, we can go from physics to metaphysics, but in reality we go towards a plurality of metaphysical options: there is no necessary and unambiguous relationship between the two bodies of knowledge.[181]

Physics was based on its own "method", which was independent of any metaphysics. It was the "*experimental method*", which was something more than a purely empirical practice: it required "a certain number of notions" and "principles". Those principles were

[179] Duhem P. 1893c, in Duhem P. 1987, pp. 41 and 76. In particular, with regard to the debate on the admissibility of actions at a distance, which was the subject of the second part of the paper, Duhem did not draw any conclusion: although "interesting from the point of view of metaphysics", any decision would "not affect the physical theories". (*Ibidem*, p. 82)

[180] Duhem P. 1893d, in Duhem P. 1987, pp. 84-5. It seems to me that the semantic field of Duhem's "cosmologie" overlap with that of "natural philosophy".

[181] Duhem P. 1893d, in Duhem P. 1987, pp. 87-9.

"*self-evident, independently of any metaphysics*", even though they could become "objects of investigation" for metaphysics.[182] In brief,

> *... il appartient à la métaphysique de rendre compte des fondements, évidents par eux-mêmes, sur lesquels repose la physique; mais cette étude n'ajoute rien à leur certitude et à leur evidence dans le domaine de la physique.*[183]

Duhem completed the complex hierarchy he had outlined in his first paper on theoretical physics: starting from phenomena, we can devise some physical laws, then from laws we can frame theories, and finally from theories we can put forward a plurality of metaphysical options. The link between every level and the subsequent was not ruled by necessity: there was a sort of independence between them. Looking at the hierarchy from the point of view of theories, Duhem found that the scope of physics did not change when it reached the theoretical level: it only became "better as to the form, better ordered, simpler, and therefore more attractive". A theory could not modify the content of the physical laws, which it linked together; at the same time, the theory could not say anything about "the essential purpose of these laws, or the nature of phenomena". Just for this reason, no theory could "ever be in contradiction with a metaphysical truth": to accept or reject a physical theory on the basis of a "metaphysical truth" made no sense.[184]

In any case, a theory had nothing to do with the truth: it could not be qualified as true or false, but "suitable or unsuitable, good or bad". The plurality of theoretical frameworks corresponding to a set of laws was consistent with this essential feature of theories. Duhem claimed that this thesis was "neither sceptic nor positivist" because he refused any philosophical commitment. The "destructive tendency" of scepticism, and the positivist trend to identify philosophical practice with scientific "method", could be contrasted only by the "precise delimitation" and the "radical separation" between physics and metaphysics.[185]

The second part of the paper was devoted to a historical reconstruction of the relationship between natural philosophy and metaphysics. Although his critics "insisted to be based on tradition", Duhem claimed that his thesis was essentially in accordance with "Aristotle and peripatetic tradition". In the following pages, he also mentioned Archimedes, Thomas Aquinas, and Copernicus, in order to show that the logical structure of deductive procedures was something different from the inquiry into the truth of hypotheses from which those procedures started. The necessity of this separation was acknowledged in the context of astronomy and applied mathematics, until the emergence of modern science. Unfortunately some founding fathers of modern science had overturned that meta-theoretical attitude, which had been accepted "by declining Scholastics with slavish conventionality".[186]

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[182] Duhem P. 1893d, in Duhem P. 1987, pp. 91-2.

[183] Duhem P. 1893d, in Duhem P. 1987, p. 93. With regard to the "evidence" and "certainty" of foundations of physics, Duhem did not offer any explanation or justification, although it was a very sensitive issue.

[184] Duhem P. 1893d, in Duhem P. 1987, pp. 94-5.

[185] Duhem P. 1893d, in Duhem P. 1987, pp. 97-100.

[186] Duhem P. 1893d, in Duhem P. 1987, pp. 100-104. At least from Aristotle's *Posterior Analytics* onwards, scholars were aware of the mutual independence between the correctness of deductive procedures starting from

This was a very sensitive issue, because it concerned the foundation of modern science, and Duhem took was concerned with a more detailed historical reconstruction. He found that Bacon had tried to "break the boundaries between the different fields of human knowledge"; in a similar way Kepler and Galileo had followed the delusive belief that "physical theories can attain the true causes" of things. Once the borderline between "the study of phenomena and their laws, on the one hand, and the search of causes, on the other" disappeared, scientists thought it was possible to "look upon physical theories as metaphysical explications". Duhem credited Descartes with having blurred the boundaries between science and metaphysics. Descartes had a great influence, on Huygens in particular, even though he did not manage to influence Pascal and Newton. In any case, in the XVIII and XIX century, "the definite awareness of the relationship between physics and metaphysics faded away progressively".[187]

Nevertheless Duhem found that, alongside that widespread and powerful trend, some traces of awareness remained alive. Some fragments of Laplace's *Exposition du système du monde*, and Ampere's *Théorie mathématique des phénomènes électrodynamiques* showed that physical theories could not merely be identified with the complete explanation of the natural world. In brief, "the sound and wise peripatetic tradition had not completely disappeared" even "in recent times, when people were so proud of the development of positive science".[188] At the same time, he acknowledged that a wrong point of view on science had not prevented science itself from experience a striking development. How could that scientific progress have taken place in spite of a mistaken meta-theoretical attitude towards science? Duhem did not expressed explicitly the question: he confined himself to remarking that sometime unexpected lands had been discovered while looking for other countries. What scientists had found was independent of what they had searched for: even this was a meaningful instance of the mutual independence between scientific (theoretical) practice and meta-theoretical commitment.

> Les exemples de Descartes et de Huygens nous montrent que l'on peut donner aux theories physiques une prodigieuse impulsion en se trompant sur leur nature et en les confondant avec les explications cosmologiques. [...] Souvent l'illusion enflamme l'activité humaine plus que la claire connaissance de l'objet à poursuivre; est-ce une raison pour confondre l'illusion avec la verité? D'admirables découvertes géographiques ont été faites par des aventuriers qui cherchaient le pays de l'or; faut-il, sur nos cartes, figurer l'Eldorado?[189]

Duhem's paper raised some debate in the philosophical environment. In a paper published in the *Annales de philosophie chrétienne*, the philosopher Domet de Vorges, remarked that Duhem had faced "a question of general methodology and high philosophy": therefore philosopher "should be allowed to have their say". In particular, he regretted that "metaphysics and the philosophy of Saint Thomas were involved in a dangerous trend". He

certain hypotheses, and the truth or reality of those hypotheses. Actually, some founding fathers of modern science believed in the possibility to naively bridge the gap.

[187] Duhem P. 1893d, in Duhem P. 1987, pp. 104 and 108.

[188] Duhem P. 1893d, in Duhem P. 1987, pp. 109-111.

[189] Duhem P. 1893d, in Duhem P. 1987, pp. 109-111.

did not disagree with Duhem on the distinction between physics and metaphysics, but on the use of the word “cause”, which was a term particularly “ambiguous”. According to Domet de Vorges, Duhem had not grasped the difference between “physical and metaphysical causes”. The concept of cause in physics had at least two different meanings: the meaning of an immediate connection, as in the case of heat which “causes” the dilatation of the thermometer, and the “more profound” meaning of hidden actions underlying apparently irregular phenomena, as in the case of the paths of planets in the sky”.[190]

Science could not “content itself with appearances”: all “the great physical theories” assumed that something exists, and ”can generate the appearances”. In this sense, science was in search of causes, and it was a legitimate search. The success of “Kant’s conceptions” had led to imagine “a sort of subjective science”, namely “an exercise of reasoning or computation, without any objective value, … without scientific value”.[191] The critical point, and the reason of the disagreement between Domet de Vorges and Duhem, was the gap between human reason and the material world. Domet pointed out rightly the philosophical issue at stake.

> Rejeter les hypothèses à titre d’explication de la nature des choses, c’est mettre entre la métaphysique et la physique un domaine inexploré, une région inconnaissable, précisément celle qui pourrait établir le lien entre les deux sciences.[192]

The gap between science and metaphysics was exactly the gap which could not be easily bridged according to the Aristotelian tradition, and according to the more recent Kantian tradition. In some way, Domet de Vorges was in tune with the prevailing meta-theoretical attitude in the scientific community, some confused specifications included: he stated that “scientific hypotheses are physical explanations”, provided that “scientists, according to Duhem, do not attribute a metaphysical nature to hypotheses”. The fact is that, according to Duhem, the identification of physical hypotheses with explanations transformed them into metaphysical statements: there was an objective philosophical mismatch between the two points of view. In the end, the philosopher Domet de Vorges agreed with the engineer Vicaire on having recourse to habits and common sense: it was “accepted by everybody” that hypotheses were “the pathway towards truth”.[193]

According to Domet, “an actual scientific attitude” would lead to the search for “the truth of things, first of all, and everywhere”. Unfortunately, the new trend in science, which was upheld by Duhem and “a large number of supporters”, and was taught at the Sorbonne, had propagated without any evident reason, in absence of any “new and serious difficulty”. In

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[190] Domet de Vorges E. 1893, pp. 137-8.

[191] Domet de Vorges E. 1893, pp. 139-41.

[192] Domet de Vorges E. 1893, p. 141. As I have already remarked in the case of Duhem 1893d, it seems to me that Domet’s identification of “objective value” with “scientific value” was not only in opposition to Duhem and Kant, as he himself acknowledged, but also to Aristotle, in particular Aristotle’s *Posterior Analytics*.

[193] Domet de Vorges E. 1893, pp. 141 and 144-5. He conceded that a hypothesis was not “directly verifiable”, and made reference to “facts which not only might be hidden at present, but also intrinsically unattainable”. At the same time, it could reach a “high degree of certainty”, and a whole science could be based on it. In reality, the main question at issue did not deal with the visibility of facts and the invisibility of hypotheses, but with their status.

fact, the reasons had to be looked for "outside the actual scientific practice". The first reason was the influence of Kantian philosophy, which rejected any entity which could not be "seen or seized": the "phenomenon" was attainable, but the "noumenous" was not. The second reason was "the overwhelming presence of mathematics in physics teaching". He expected that "the misuse of computation" would have led modern physics "to perdition" as well as "the misuse of logic" had brought discredit on "the Middle Ages philosophy".[194]

With regard to the second reason, Domet actually hit the mark, since the second half of the nineteenth century had seen the emergence of complex mathematical theories in the fields of mechanics, thermodynamics, and electromagnetism. On the contrary, the first reason made reference to a philosophical trend which had emerged a century before, and could not account for recent cultural processes. The fact is that Domet underestimated the emergence of theoretical physics, and the emergence of new sophisticated theories on electrodynamic and thermodynamic phenomena. The increasing mathematisation of theories, the systematisation of electrodynamics and thermodynamics, and the emergence of theoretical physics were mutually intertwined processes: theoretical physics was the result of a new fruitful alliance between the tradition of mathematical physics and the tradition of natural philosophy.

In the last passages of his paper, Domet questioned Duhem's interpretation of some quotations from Thomas Aquinas, but the difference between his and Duhem's interpretations appears so vague that it cannot be looked upon as the real issue at stake. On the other hand, both Aristotle and his followers were aware of the gap between the correctness of rational procedures and the truth of the hypotheses on which those procedures were based. When Domet attempted to frame a conclusion, he really focused on the keystone of the whole philosophical debate, and remarked that "the *esprit* of the ancient philosophy" was "essentially objectivist". This was the real issue at stake: the interpretation of a long-lasting tradition. The philosopher Domet could not accept that a physicist inquired into the philosophical tradition, in order to look for something which did not have to be found: the clear awareness of the intrinsic boundaries of every rational practice. The last passage of the paper could be looked upon as a ban on any further attempt to put in danger the honour of the philosophical tradition:

> Si M. Duhem veut à tout prix des antécédents à sa doctrine, il pourra les trouver chez les néocriticistes et les positivistes, mais nullement dans la philosophie traditionnelle.[195]

The following month, in a brief note, the engineer Lech alas avowed that he would have confined himself to demonstrating "a serious evidence of Duhem's intimate leaning towards positivism". Comparing Duhem's treatise on acoustics with that on optics, he had realised that the latter did not mention any elastic medium, whereas the former relied on "the usual conceptions on the cause of sound". How could Duhem "reject, *in principle*, the explicative power of aethereal oscillations" together with what he had labelled "*mechanical theory* of

[194] Domet de Vorges E. 1893, pp. 146-7.
[195] Domet de Vorges E. 1893, p. 151.

light"? At the same time, Lechalas acknowledged that, in the case of "sonorous perceptions", the elastic medium really interacts "with our organs", differently from the case of optical perceptions. In the end he acknowledged that the difference was actually "a real difference". The whole line of reasoning sounds quite strange: at first it seems that he did not manage to catch the difference between the status of ordinary matter and aether, but then he pointed out that the supposed contradiction was not so. What was really at stake in Lechalas' *Note*? The difference between his naïve realism and Duhem's more sophisticated philosophy, I suppose. In the end, he insisted that the rejection of "the theory which cannot be directly verified was essentially a positivist claim".[196]

In brief, according to Lechalas, Duhem was not in contradiction with himself: he was really a positivist. In any case, with regard to the specific case of optics and acoustic, he missed the point: according to Duhem, neither oscillation in the air nor aethereal oscillations could be looked upon as actual explications of the natural world.

The majority of his readers did not seem so intellectually equipped as to follow Duhem in his sophisticated historiographical and epistemological remarks: the intellectual gap between Lechalas and Duhem is a meaningful instance. Moreover Catholic scholars preferred to rely on a naïve realism, which appeared more in tune with their apologetic aims. That the catholic Duhem did not share the same attitude appeared absolutely unbearable to them.

### *3.3 New key-concepts : "natural classification" and "interpretation"*

In the same year, Duhem published another paper in the *Revue des questions scientifiques*, where he collected some remarks triggered off by the translation of William Thomson's *Popular Lectures and Addresses* into French. He started from "the astonishment" of French scholars in front of "English genius' specific way of interpreting and practising physics". That specificity consisted of "an imaginative faculty", which allowed English scholars "to envisage a complex set of concrete things". In the case of electromagnetic theories, where "French and German physicists conceived a bundle of lines of force", the English ones imagined "a bundle of elastic threads" which could "decrease their length and increase their section … at the same time". He found in Oliver Lodge the most radical implementation of that attitude. In general terms, that faculty represented "an extraordinary ability to grasp the concrete, and an extreme difficulty to appreciate the abstract".[197]

After having quoted a well-known passage from W. Thomson's *Baltimore Lectures*, where Thomson stated that he could not be satisfied with any physical explanation which did not consist of mechanical models, Duhem remarked that "the English school" relied on a specific kind of "mechanical explanation of physical phenomena".

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[196] Lechalas G. 1893b, pp. 312-14.

[197] Duhem P. 1893e, in Duhem 1987, pp. 113-117, footnotes at p. 117 included. He remarked that English geniality "had generated Shakespeare, but never a metaphysician".

> Ce n'est pas là, assurément, un caractère qui suffise à distinguer les doctrines anglaises des traditions scientifiques qui fleurissent en d'autres pays; les théories mécaniques sont issues d'un génie français, le génie de Descartes; elles ont longtemps régné sans contestation en France comme en Allemagne; ce qui distingue l'École anglaise, ce n'est pas d'avoir tenté la réduction de la matière à un mécanisme, c'est la forme particulière de ses tentatives dans ce but.[198]

This was a very important issue: English mechanicism had its roots in Descartes' mechanicism, but different kinds of mechanicism could be consistently conceived. It is worth remarking that an abstract kind of mechanicism was not far from Duhem's horizon. In the building up of his generalised thermodynamics, he had seriously taken into account the complexity of the natural world. At the same time, he relied on a very general and wide-scope mechanical approach, on the track of Lagrange's Analytical Mechanics. He had realised that specific mechanical models could not attain the complexity of the physical world, but a more abstract mechanical approach really could. When he praised "the unity" and the presence of "logical linkages" in a theory as specific features of "the French school", he suggested implicitly that he belonged to that tradition: he was "a geometer of Laplace and Cauchy's school", even though he did not agree with their specific geometrisation of physics. Duhem's mechanicism was a structural mechanicism: thermodynamic processes and chemical reactions could be represented by the generalisation of Analytical Mechanics rather than by microscopic models of interacting particles. We are facing here two different traditions of Mechanics: wide-scope mathematical structures, on the one hand, and specific mechanical models, on the other.[199]

When Duhem criticized the English school, and W.Thomson in particular, he pointed out a way of practising theoretical physics which he could not share. He could not accept the superposition of different mechanical models, a series of partial models which were "developed independently of each other". Browsing through Thomson's book, he had found a gas represented as "a set of tiny bullets, endowed with unthinkable velocities", which collided with each other "during their crazy race". But he had also found "material molecules" represented as a structure of "concentric shells connected by springs", or a "gyrostatic system" composed of "aethereal vortices".[200]

He acknowledged that his rejection of theories as mere collections of scattered models, which were consistent in themselves but mutually inconsistent, could appear in contradiction with his disbelief in physical theories as "metaphysical explanations". If he did not rely on the explanatory power of theories, but only on their symbolic, representative power, why should he have demanded a strictly logical foundation of such theories? This was a subtle objection indeed, and Duhem managed to answer only in a very general way: he was pursuing a third pathway between dogmatism and scepticism. He could not agree with those who "firmly attributed an ontological value to physical theories", and at the same

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[198] Duhem P. 1893e, in Duhem 1987, pp. 119.

[199] Duhem P. 1893e, in Duhem 1987, pp. 128.

[200] Duhem P. 1893e, p. 21, in Duhem 1987, p. 129. He noticed that the English way of practising theoretical physics was consistent with English law, which consisted of a "countless" collection and superposition of both "laws and habits". See *Ibidem*, p. 131.

time, he could not agree with those who looked upon “Laplace and Ampère’s methods, and W. Thomosn and Maxwell’s methods” as equivalent. He emphasised that, from the purely logical point of view, a physicist could not be prevented from representing “different sets of laws, and even a single set, by several incompatible theories”. It was just logic which granted the physicist this right. Nevertheless, physicists were not uniquely led by “purely logical reasons”: extra-logical reasons did not recommend the proliferation of incompatible theories. Extra-logical reasons could be synthesised in the claim of an ideal: the search for “perfection” in science.[201]

However the above-mentioned objection could be reiterated, as Duhem himself acknowledged. Why should a physicist pursue perfection, if he did not rely on an ontological foundation, and he had explicitly limited the explanatory power of the theory? Duhem answered that the physicist aimed at a “*natural classification* of laws”, where the demanding adjective “natural” required careful specifications. What he labelled “perfect and ideal theory” meant “the complete and appropriate metaphysical explanation of the mature of material things”. To be more precise, such a theory “would class the physical laws in a way which would reflect the metaphysical relationships among the essences from which the laws are emanated”. An ideal theory was not a real entity: it could not been attained, neither in practice nor in principle because of the unbridgeable gap between physics and metaphysics. It was “definitely outside the scope of human mind”, but could be contemplated or imagined. It was like a visible but unattainable horizon. The actual physical theories, put forward by real physicists, had to “strive for perfection“, even though perfection could not be realised.[202]

If we try to disentangle the subtle plot outlined by Duhem, we see that metaphysics was really at stake, even though he did not mix metaphysics with physics. Metaphysics was placed at a meta-theoretical level: it did not affect the meaning of a theory, but purposes and targets of scientific enterprise. This was a subtle difference indeed, which was not easy to grasp: Duhem’s third pathway between scepticism and dogmatism actually required intellectual boldness and philosophical skills. In this case, Duhem was really walking on thin ice. From the philosophical point of view, he considered himself more in tune with a revised Aristotelic tradition than with any other tradition, even though in no way in tune with his neo-Thomist colleagues. The extremely delicate balance that emerges from the following passage shows us how difficult it was to undertake that third pathway.

> Mais quelque imparfaites que soient nos théories physiques, elles peuvent et doivent tender au parfait; sans doute elles ne seront jamais qu’une classification, constatant des analogies entre les lois, mais ne saisissant pas de relations entre les essences; toutefois, nous pouvons et nous devons chercher à les établir de manière qu’il y ait quelque probabilité pour que les analogies mises par elles en lumière soient non pas des rapprochements accidentels, mais de véritables relations, manifestant les rapports qui existent réellement entre les essences; nous pouvons et

[201] Duhem P. 1893e, pp. 26-7, in Duhem 1987, pp. 132-5.
[202] Duhem P. 1893e, p. 29, in Duhem 1987, pp. 136-7.

> nous devons, en un mot, chercher à rendre ces classifications aussi peu *artificielles*, aussi *naturelles* que possible.[203]

In the last sections of the paper, Duhem tried to find an *entente cordiale* between abstract and imaginative attitudes in scientific practice. At first, he criticised the new generation of British physicists, who had crossed the boundaries which neither W. Thomson nor Maxwell had trespassed. They dared to inquire into "strange and disconcerting" subject matters, with the same self-confidence as in their researches on optics and electricity. In particular, he criticised William Crooks, Oliver Lodge, and Peter Guthrie Tait because of the researches they had undertaken on the "transmission of thought at a distance, spiritualism, and magic". At the same time, he acknowledged that their attitude "encouraged invention at the highest degree", even though it could become dangerous for science. In the end he acknowledged that "the inventions blossomed out on the Continent had not been as numerous and audacious as those blossomed in England and America".[204]

This partial re-evaluation of the imaginative side of scientific enterprise led Duhem to analyse the extra-logical elements which had concurred to the emergence of that complex scientific practice which he termed theoretical physics. Theoretical physics was not completely "subject to the inflexible laws of logic": the choice of hypotheses was an instance of extra-logic process, wherein "specific attitudes", the received view, and other "influences" were at stake. A physical theory was therefore the result of a historical process, and showed the hallmark of a specific place and time. I have already remarked that theoretical physics emerged from a sophisticated alliance between the formal structures of applied mathematics, and the most speculative side of natural philosophy. It emerged from the integration between two different traditions: the combination of highly mathematised structures and a net of consistent speculations brought about its specific character. According to Duhem, the above-mentioned extra-logical influences had a marked effect on the speculative component, on what Duhem termed "the hypothetical part of the theory". That component might reveal some faults, and it might be discarded after some time, whereas the logical or formal structure of the theory was apt to survive. Faults and mistakes were influenced by "the environment and race, and by physical barriers and political borderlines", whereas the structural features of a physical theory were, in some way, perpetual creations of mind.[205]

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[203] Duhem P. 1893e, p. 29, in Duhem 1987, p. 137.

[204] Duhem P. 1893e, pp. 31-2, in Duhem 1987, pp. 140-41. Duhem found that W. Thomson and Hermann von Helmholtz were the champions of the two different attitudes: imagination and specific models on the one hand, and abstraction and generality on the other. See Duhem P. 1893e, pp. 33-5, in Duhem 1987, pp. 141-3, in particular, p. 143: "… partant de la physique, Helmholtz remonte par l'analyse, de principe en principe, jusqu'à rencontrer la métaphysique; Thomson descend, de conséquence en conséquence, jusqu'aux applications industrielles; le premier est un des plus profonds philosophes de notre siècle; le second en est un des ingénieurs les plus inventifs." On Thomson's role in the realisation of a submarine cable for transatlantic telegraphic connection, see Smith C. and Norton Wise M. 1989, pp. 661-83.

[205] Duhem P. 1893e, pp. 37-8, in Duhem 1987, pp. 145-6. With regard to the net of speculations in a physical theory, Duhem's last sentence was borrowed by the Gospel: "the spirit blows where it wants".

After having faced aims and features of theoretical physics, in 1894 Duhem faced some questions emerging from experimental physics. The fifty-pages paper which resulted from his cogitations were ordered by means of a list of fourteen theses, divided into two parts: ten theses appeared under the question "what is an experience in physics?", and four under the subsequent question "what is a physical law?". If the second part resumed and completed some remarks he had already made in previous papers, the first part was substantially new. It pivoted on three fundamental theses: first, a physical experiment was not a purely empirical process; second, it could not be so powerful as to lead to the refutation of a single hypothesis; third, it was less reliable, even though more precise, than ordinary experience. He was aware that those theses would have astonished the readers of the *Revue des questions scientifiques*, and scandalised some mind "concerned about scientific rigour": he knew that, from Francis Bacon onwards, the rhetoric which accompanied scientific practice had led to more comfortable conclusions.[206]

With regard to the first fundamental thesis, Duhem invited the reader to enter a laboratory full of electromagnetic devices, and ask a physicist what he is doing: he could answer that "he is measuring an electric resistance" rather than "the oscillations of a bar which carries a little mirror". In other words, his answer does not deal with "facts", but "abstractions": electric resistance, temperature, and gas pressure are not facts, but rational representations of facts. Experimental practice consisted of "two parts", both "the observation of certain phenomena", and "the interpretation of the observed facts". In other words, the performance of a physical experiment requires the knowledge of "the accepted theories", and "the ability to apply them". From the structural point of view, experimental practice was not so different from theoretical practice, which required the interaction between two different components: formal structures and speculations.[207]

The new key-concept pointed out by Duhem was "interpretation".

> *Une expérience de physique est l'observation précise d'un groupe de phénomènes, accompagnée de l'*INTERPRETATION *de ces phénomènes; cette interprétation substitue aux données concrètes réellement recueillies par l'observation des représentations abstraites et symboliques qui leur correspondent en vertu des théories physiques admises par l'observateur.*

Duhem acknowledged that a purely empirical practice made sense in the early stages of the history of science, when theoretical frameworks were missing or only roughly outlined, but it made no sense in the case of "advanced sciences", when "mathematical structures play a fundamental role". When a science progresses, "the role played by theory in the interpretation of experimental facts" increases progressively. In the late nineteenth century, the pretension to having written "a *purely experimental* paper on physics" was not different

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[206] Duhem P. 1894c, pp. 3, 7, 11, and 35, in Duhem P. 1987, pp. 147, 151, 155, and 179. Apart from Bacon, Duhem mentioned Claude Bernard's *Introduction à l'étude la médicine expérimentale*.

[207] Duhem P. 1894c, pp. 4-5, in Duhem P. 1987, pp. 148-9.

from the pretension to having expressed "an idea without making use of any sign, neither spoken nor written".[208]

The second fundamental thesis stated that "*a physical experiment can never condemn an isolated hypothesis, but only a theoretical system*". When a physicist performs an experiment, in order to check the exactness or inexactness of a given proposition, he cannot "confine himself to making use of the statement under investigation": he must make use of a wider "set of theories, which he assumes without questioning them". If an expected prediction did not take place, we would be allowed to conclude that "there is something wrong" in the complex net of theories, but we would not be able to identify exactly the mistake.[209]

In brief, the complexity of science structure forbade scientists to perform logical procedures of decision on single statements: however paradoxical it might be, the *modus tollens* procedure could only be applied to the body of knowledge of science as a whole.

> En résumé, le physicien ne peut jamais soumettre au contrôle de l'expérience une hypothèse isolée, mais seulement tout un ensemble d'hypothèses; lorsque l'expérience est en désaccord avec se prévisions, elle lui apprend que l'une au moins des hypothèses qui constituent cet ensemble est erronée et doit être modifiée; mais elle ne lui désigne pas celle qui doit être changée.[210]

Physics could not be looked upon as a machine: we "cannot easily disassemble it, and check its components separately". Duhem compared physics to a living system: when "every part operates, even the most distant parts come into play". This is a sort of methodological or meta-theoretical holism. It is worth noting that the awareness of the complexity of the natural world, the rejection of a reductionist approach to it, and the design of a generalised thermodynamics preceded Duhem's meta-theoretical holism. This is not sufficient to claim that Duhem's holism directly stemmed from his physical theories, but we can take note of two subsequent steps in Duhem's intellectual pathway, the firs being theoretical, and the second meta-theoretical. At first Duhem tried to go beyond the purely mechanical model of the natural world, in favour of a more complex representation as a thermodynamic machine, which was the seat of irreversible processes. Then he attempted to go beyond simplified accounts of both theoretical and experimental physics. Eventually he arrived at a more complex representation of scientific practice as a living structure, where each part concurred to the realisation of the whole, and at the same time, was influenced by the whole. Skipping from the theoretical to the meta-theoretical level, not only has Duhem shown that we cannot act on the natural world as though it was clockwork, but also that we cannot look upon the body of knowledge of physics as it was mechanically assembled.

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[208] Duhem P. 1894c, pp. 8-10, in Duhem P. 1987, pp. 152-4.

[209] Duhem P. 1894c, pp. 11, and 13, in Duhem P. 1987, pp. 155 and 157. The second fundamental thesis has subsequently become known to philosophers as Duhem's holistic thesis. Under the label "Duhem-Quine thesis", it has been widely discussed and criticised. See the last section of the present Preprint.

[210] Duhem P. 1894c, p. 16, in Duhem P. 1987, p. 160.

> … l'horloger auquel on donne une montre qui ne marche pas en sépare tous les rouages et les examine un à un, jusqu'à ce qu'il ait trouvé celui qui est faussé ou brisé; le médecin auquel on présente un malade ne peut le disséquer pour établir son diagnostic; il doit deviner le siège du mal par la seule inspection des effets produits sur le corps entière; c'est à celui-ci, non à celui-là, que ressemble le physicien charge de redresser une théorie boiteuse.[211]

Differently from geometry, a physical theory is much more than its logical structure: if in geometry, "there is no place for a third alternative between two contradictory statements", in physics, the demonstration of a statement can never be attained. The reduction to absurd, which is typical of geometry, assures that a statement is true when its contradiction leads to a false consequences: since in physics such a reduction cannot have place, "*the experimentum crucis* is impossible". Just for this reason, the progress of physics cannot be a cumulative process as in geometry. According to Duhem, geometry grows by accumulation of new theorems, "which are demonstrated once forever"; physics is "a symbolic picture", which is continuously retouched, in order to widen its scope, and improve its unifying power.[212]

Duhem's third fundamental thesis dealt with the difference between ordinary experience and physical experiments. He remarked that, when honest people report a crude fact, that fact can be considered true and certain. On the contrary, a physical experiment was much more that the perception of a crude fact, and therefore its degree of certainty was much lesser. Moreover, the communication of a physical experiment was much more complex than the communication of a crude fact. An experiment performed by a physicist who shared our "interpretation of phenomena", and relied on the same set of "accepted theories", was not difficult to understand. On the contrary, an experiment performed by a physicist who did not share the same body of knowledge, or had practised in different periods of history, could appear nonsensical. In that case, things went as if the two physicists mutually spoke "foreign languages": the experiment of the one seems "meaningless" to the other.[213]

Duhem went on demonstrating that it was not easy to decide whether a given degree of approximation is reliable or not. Even in this case, purely logical procedures could not be of great help: a specific sensitivity was at stake, some sort of flair which dealt with "the esprit de finesse rather than with geometric sensitivity". Also for this reason, the degree of certainty of a physical experiment was lower than the degree of certainty of "the simple observation of a fact by non-scientific methods". At the same time, a physical experiment was overtaken by a common observation "as to precision". The uncertainty that affected experiments dealt with the "provisional" nature of physical laws: a law represented a set of phenomena with an approximation which "physicists could consider satisfactory at present, but might not be accepted afterwards". Duhem stressed that the provisional nature of a law was inescapable: it could not be looked upon as "true" today and "false" tomorrow, but "neither true nor false" at any time.[214]

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[211] Duhem P. 1894c, pp. 16-17, Duhem P. 1987, p. 160-61.

[212] Duhem P. 1894c, pp. 17, 19, and 21, in Duhem P. 1987, pp. 161, 163, and 165. See also p. 49, in p. 193.

[213] Duhem P. 1894c, pp. 31-3, in Duhem P. 1987, pp. 175-7.

[214] Duhem P. 1894c, pp. 35 and 44, in Duhem P. 1987, pp. 179 and 188.

In Duhem's meta-theoretical model, physical laws were the link between physical experiments and physical theories: their provisional condition stemmed from the intrinsic uncertainty of the outcome of experiments but also from a sort of *linguistic* uncertainty, due to the potential inadequacy of the set of concepts or "symbols" the physicist made use of. For an electrified gas, for instance, the set of symbols "density, temperature and pressure" was not sufficient, and the usual "law of compression and dilatation of gases" had to be modified. Physical laws were not eternal truth: they needed to be updated, revised, or replaced. According to Duhem, something like a final truth could not be attained: Pascal had managed to express this concept in an effective and imaginative way. He had compared truth to "a tip", which was "too subtle to be handled with care by our blunt tools". When we try to direct the tip towards a very precise point, the tools "crush the tip, and produce a coarse pressure, more onto the false than onto the true".[215]

In no way could scientists rely on automatic procedures for the attainment of perfect knowledge: if we look for more precision, we find more uncertainty. Duhem also offered a nice metaphor on the usefulness of precision, and the illusion of certainty. A botanist was in search of a rare tree, and asked two countrymen for information: the first simply said that the tree was actually in that wood, whereas the second specified what track he had to follow, and once there, how many steps in a given direction had to be performed. The botanist managed to find the tree, but after a slightly different number of steps. Duhem remarked that the content of the first piece of information "was true, and the second false", and then asked the reader: "what is the countryman who deserve more gratitude"?[216]

He noticed that, "in recent times", metaphysicians were "willing to borrow the laws of physics, in order to build up or destroy metaphysical systems". Philosophers had to realise that a physical law was not "an absolute truth". Moreover, they confused the content of the law with the mathematical structures which expressed that content: the provisional and revisable physical content did not have to be confused with "the certainty of mathematical propositions which gave the law its form". Once again he invited physicists and philosophers to disentangle the easily verifiable correctness of formal structures from the more questionable truth of the contents.[217]

### *3.4 Historiography met Epistemology*

In the same year Duhem published a paper on the history of optics. His history was something more than a mere collection of meaningful facts: from the outset he put forward an original historiographical framework. He looked upon the landscape of "physical

[215] Duhem P. 1894c, pp. 46 and 51, in Duhem P. 1987, pp. 190 and 195. See Pascal B. 1897, in Pascal B. 1976, p. 76: "La justice et la vérité sont deux points si subtiles, que nos instruments sont trop mousses pour y toucher exactement. S'ils y arrivent, ils en écachent la pointe, et appuient tout autour, plus sur le faux que sur le vrai." Pascal's *Pensées* were published for the first time in 1670.

[216] Duhem P. 1894c, p. 51, in Duhem P. 1987, p. 195.

[217] Duhem P. 1894c, p. 52, in Duhem P. 1987, p. 196. This was one of Aristotle's longest lasting heritages. Duhem invited physicists and philosophers not to forget it: modern science could not afford to forget it.

sciences" as a twofold entity. On the one side, there were systematic bodies of knowledge, which had been considered as such even in "ancient times and in the Middle Ages": among them he mentioned "astronomy, … hydrostatics, and the general principles of statics". On the other side, he saw only scattered experiences, often "mutually inconsistent and roughly performed": scholars interested in the history of natural philosophy could find neither "steady evolution" nor meaningful "logical links". According to Duhem, a reliable history of science required the existence of a body of knowledge whose essential features were a progressive and logical development. Those features were lacking in the researches on electricity, magnetism, heat, and other subjects, which philosophers and practitioners had undertaken over a long time-span, from the ancient Egyptian civilisation to the threshold of European Renaissance.[218]

He specified that he was writing "a history of physics to the benefit of physicists", and for this reason he was to confine himself to "the modern tradition", which emerged at the end of Renaissance. In other words, he decided not to inquire into previous traditions, because their languages and practices were too far from the languages and practices of contemporary scientists. The starting point of his history of physical sciences could not be but Descartes' mathematics and natural philosophy, and in particular his masterpieces "*Discours de la Méthode*, *Géometrie*, *Dioptrique*, and *Météores*".[219] After that he introduced the reader to Fermat, Huygens, and Newton's theoretical models, and described the subsequent models of optical and electromagnetic aethers. The last pages of the paper were devoted to Maxwell's electromagnetic theory of light, which Duhem criticized in some detail. He treated this theory as a suitable case study in order to discuss his meta-theoretical remarks on the features of a physical theory.

He focused on Maxwell's key-concept of "*displacement current*", namely the variations of polarization in dielectrics, which showed a formal analogy with ordinary electric currents in conductors. Two main issues were at stake: the polarization of aether as a specific instance of polarization in dielectrics, and the nature of the analogy between electric currents in conductors and dielectrics. Duhem remarked that "no experience", but only "an incomplete analogy", had led Maxwell to the new concept of electric current: in some way, "the electrodynamics of displacement currents" was a skillfully tailored, but purely theoretical, artifact. The theory could only rely on one experimental datum: the equality between "the velocity of propagation of displacement currents" and "the velocity of light in the same medium". There was "a logical chasm" between that identity, which "might be merely random", and "the hypothesis that light consisted of rapidly oscillating displacement currents". He found that Maxwell was not so different from Fresnel as to the ability "to design rather than justify his inventions". In particular, he highlighted the actual equivalence between the propagation of *displacement currents* and the propagation of "transverse oscillations through an elastic medium", on the one side, and "the indirect derivation of that velocity of propagation".[220]

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[218] Duhem P. 1894d, p. 94.
[219] Duhem P. 1894d, pp. 94-5.
[220] Duhem P. 1894d, pp. 118-20.

It is worth mentioning that in 1894 Maxwell's theory was looked upon as an authoritative but questionable theory, which had recently received a meaningful corroboration by Hertz's experiments. The sharpness of Duhem's criticism mirrored the gap between his meta-theoretical attitude and the looser attitudes of British physicists, which he had discussed in 1893. He wrote as if he were the watchman of French scientific tradition, and in particular the spokesman of the community of scholars who "loved clarity, and were concerned about exactness". Maxwell's theory induced "a painful surprise" in those minds which had been trained in the tradition of French mathematical physics. At the same time, Duhem was aware that, although at the beginning Maxwell's theory had been regarded as "a paradoxical and ingenious overview", subsequently it had gained much more consideration in most of the scientific community. In any case, Duhem found that "the murky and puzzling principles on which that theory was based" could not fit in with "the research style of French masters from Laplace to Cauchy".[221]

He considered himself as a follower of those masters and the corresponding tradition, where every physical theory had to satisfy at least three basic features. First, "not even the slightest presence of contradiction can be tolerated". Second, "the different parts of a theory must be logically connected". Third, "the number of independent hypotheses must be minimum". Unfortunately, recent and successful meta-theoretical views were driving scientific practice in the opposite direction: he lamented that the heuristic power and the fruitfulness in applications had become the main aims of physical theories.

> Mais, ces esprits-là se font rares aujourd'hui ; leurs exigences semblent exagérées à beaucoup de physiciens ; plusieurs même le trouvent un peu ridicules, et, avant la précision et la logique, qui ne satisfont que la raison, font passer la généralité des aperçus et l'imprévu des rapprochements, qui séduisent l'imagination ; aussi fait-on grâce à la théorie électromagnétique de l'obscurité de ses origines ; on lui demande seulement d'être féconde en applications.[222]

Duhem acknowledged that Maxwell's electromagnetic theory offered "some advantages over the previous elastic theory", but he found that a further improvement could be attained by Helmholtz's theory, which had "a wider scope", and did not rely on specific hypotheses on the structure of aether. However, he found that the replacement of Fresnel's elastic theory by the electromagnetic theory represented a progress with regard the tradition of mechanical models, because dielectric polarization could be looked upon as a new "primary quality", which could not be reduced to matter and motion. In reality, "the success of the electromagnetic theory" could not give a definite answer to the question whether a mechanical world view encompassed all physics. Moreover, he knew that Maxwell himself, and more persistently William Thomson, had attempted to reduce "the whole set of electric, magnetic and optical phenomena" to matter in motion. Nevertheless, in order to perform

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[221] Duhem P. 1894d, pp. 119-20.
[222] Duhem P. 1894d, p. 120.

that reduction, they had to pay a very high price: they had to devise “a strange and complex” structure of aether, which should be nothing else but the simplest dielectric.[223]

Duhem’s meta-theoretical commitment was both anti-mechanical and anti-reductionist. Maxwell’s and Thomson’s aethereal vortices appeared to him not so different from Descartes’ vortices, which had been superseded by Newton’s mechanics. His historical sensitivity and the knowledge of the actual scientific practice led him to an ironic remark: Thomson’s mechanical models might become “unquestionable truth of tomorrow” or “the unquestionable mistake of the day after tomorrow”. Even more ironically he stated that he had been “accidentally enticed” to quoted from Pascal. In reality, he had carefully read Pascal, and appreciated his approach to science. Pascal’s specific passage mocked any reductionist methodology, just making reference to the best known symbol of determinism and reductionism in the XVII century: the mechanical clock. As Pascal remarked, we can envisage rough mechanical models for every phenomenon. We can decompose a body into basic elements, in terms of “geometry and motion”, but how could we identify exactly those elements, and how could we “build up the whole structure“ starting from them?[224]

Duhem outlined a cyclical historiography: “the leading hypothesis” of a successful theory, which was considered as such by a given generation of scientists, had often been considered as “an evident mistake by the previous generation”. In its turn, a subsequent generation might consider that hypothesis as “an evidence of the ignorance of their forefathers”. According to Duhem, the history of optics was a meaningful instance of that oscillating trend: “XVII-century scholars contemptuously rejected the model of emission”, then “XVIII-century natural philosophers relied on this model and despised the wave model”, and now “XIX-century physicists retrieve the latter, and are surprised to find confidence in the former as a serious theory”. He depicted physical theories as dynamical entities, endowed with a history which could be easily outlined: they emerge, then they “multiply their successes accounting for disregarded or poorly-understood phenomena”, but at that time they make a common mistake. The hypotheses on which they rest become “absolute certainties”, and the “representation of the external world” offered by the theory is transformed into “an actual expression of the world structure”. But some difficulties soon

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[223] Duhem P. 1894d, pp. 120-1. In reality, as far as it is developed in his *Treatise* (1873), Maxwell’s theory is a mechanical theory, in accordance to the two meanings which could be associated with the adjective “mechanical”: specific mechanical models, on the one side, and the mathematical structures of abstract mechanics, on the other. In the first case, the theory was developed in explicit analogy with mechanical models of elastic solid bodies; in the second sense, Maxwell exploited the typical approach of Analytical Mechanics. In Maxwell’s *Treatise*, we find a sort of superposition between the two approaches. In the last decades of the XIX century, the debate on Maxwell’s theory was widespread in the scientific community. In 1893 Oliver Heaviside showed that Maxwell’s set of electromagnetic equations, which he had synthesised in the vector language, could not fit in with any mechanical model of aether. See Heaviside O. 1893, pp. 128-31, Buchwald J.Z. 1985b, pp. 288, 294, and 234, Buchwald J.Z. 1985c, p. 236, and Bordoni S. 2008, pp. 163-5. In particular, the comparison between Maxwell and Helmholtz’s theories was one of the main issues at stake. Hertz’s well-known experiments on the propagation of electromagnetic perturbations were also performed with the hope of detecting empirically the difference between the two theories. Hertz H. 1892, in Hertz H. 1962, p. 20, Doncel M.G. 1991, pp. 1 and 6, Darrigol O. 1993, p. 233, and Buchwald J.Z. 1994, p. xiii.

[224] Duhem P. 1894d, pp. 121-2. Pascal’s passage was drawn from one of his *Pensées*, which was explicitly addressed to Descartes: “Il faut dire en gros : cela se fait par figure et mouvement, car elle est vrai. Mais de dire quels, et composer la machine, cela est ridicule. Car cela est inutile et incertain et pénible.” See Pascal B. 1976, p. 72 [Brunschvicg’s edition, Pensée 79-84]; see also Pascal B. 1951, vol. 1, p. 66 [Lafuma’s edition, Pensée 84-174].

emerge, and the weight of the failures leads to the collapse of the theory: scientists "hasten to sweep away the debris in order to give room to another theory". In brief, Duhem outlined a historiographical framework where theories emerged, were successfully upheld, then suffered a dogmatic drift, were afterwards overwhelmed by subsequent defeats, and eventually were replaced by new theories.[225]

Duhem was aware that his representation of physical theory as provisional creations of human mind might lead to a sceptical attitude in general, and in particular an underestimation of science. He was aware that both scholars and common citizens wondered about "the fruitfulness of the efforts to built up and destroy in turns all those structures": in particular they wondered if "the scholarly elite" had manage to attain "an actual progress in the knowledge of the physical world". This state of uncertainty might lead to two dangerous attitudes: the idea that "the secrets of nature are unintelligible", and "the confidence in mere experiences" at the expense of whatever "theoretical practice". Duhem acknowledged that "the breeze of skepticism" was blowing through the French cultural environment, but he asked his readers "not to let that wind shake them". He hoped that an attentive reader was able to "disentangle the thread of a tradition", and single out traces of "a slow progress".[226]

This is the keystone of Duhem's historical histotiographical outline: the periodical series of successes and failures in scientific practice let a higher-level progress emerge. There was a positive heritage in the history of science, and it could be found even in outmoded or totally disappeared theories. First of all, a theory might disappear, but the specific "physical laws" which emerged together with it might survive. Descartes' optical theories were definitely outdated, but the law of light refraction was still valid, and it had continuously been re-interpreted by new theories. Huygens' law of refraction and "Newton's law on the series of colours in thin layers" had shared the same fate: they had become part of the contemporary body of knowledge. However, physical laws were not the only positive heritage of dead physical theories. Duhem considered theories as complex entities, definitely more complex and structured than a mere collection of physical laws: the mathematical language and the logical connections gave a consistent and unitary structure to a theory, and they could survive. The mathematical structures of physic were born together with Descartes' "mechanical hypotheses": mechanical hypotheses disappeared, but the mathematical structures [la physique mathématique] still survived.[227]

In brief, the emergence, development, dogmatization, crisis, and fall of every meaningful physical theory left on the ground a permanent and valuable heritage: empirical laws and formal structures. In this context Duhem stressed the extra-logical concept of the *fruitfulness* of a physical theory. Although philosophers and scientists have traditionally focused on the concepts of truth or falsity, he claimed that the value of scientific theories could be found beyond their supposed truth, because their truth was the outcome of a historical process, and

[225] Duhem P. 1894d, p. 122.
[226] Duhem P. 1894d, pp. 122-3.
[227] Duhem P. 1894d, pp. 123-4. What I have labelled "theoretical physics" or "theoretical practice" in the first sectin of the present Preprint was labelled "*mathematical physics*" by Duhem in the above mentioned passage. At the same time, Duhem labelled *mathematical physics* the formal structure of physical theories. As I have attempted to explain, late XIX-century theoretical practice was something more sophisticated than the tradition of French mathematical physics, and Duhem was one of the champions of that new practice.

therefore it was a provisional value. On the contrary, the fruitfulness of a theory was a permanent value. The "scaffolding" of a theory, namely the specific models and hypothesis on which it was based, was the provisional component of a theory. This flashy component could be separated from the hard core of the theory, which consisted of permanent mathematical entities. Although "Huygens' hydrodynamic ideas are outdated", they generated "the notion of wave surface"; although "Newton's luminiferous particles are disappeared", they generated the concept of "very short periods which correspond to colours"; although we cannot accept Young's analogy between "the aether of a light ray" and "a column of vibrating air", that analogy allowed scientists "to associate a direction with the quantity representing the phenomena of light"; although "Fresnel's aether and its motions are disappeared", we owe to them the formal analogy between the equation of light vibrations and "transverse vibration in elastic solids".[228]

At the end of the paper, Duhem synthesized his historiographical view, where the superposition of two historical processes was at stake: the short-term changes of specific hypotheses and models, and the long-term progress of mathematical structures and wide-scope key-concepts. On the one hand, we have the series of theories "which rise only to be overthrown", and the hypotheses "which a given century contemplates as the secret machinery of the Universe" but then "the following century shatters like the toy of a child". On the other hand, beneath that apparently idle process, "the slow but constant progress of mathematical physics goes on". A long-lasting and persistent stream of progress flowed underneath the transformations which affected the history of science.[229]

The last passage of the paper was extraordinary lyric: this feature is worth remarking, because an emphatic style was not common for Duhem. Probably he had found a suitable metaphor, which could poetically express his overview of the dynamical complexity of scientific practice and scientific progress.

> A l'heure où le flot monte à l'assaut d'une grève, une lame se forme, ondule, déferle et couvre le sol sec jusque-là ; mais, aussitôt, il lui faut abandonner sa conquête, laisser assécher le sable qu'elle avait couvert, et se perdre dans la lame qui se forme derrière elle ; ce fracas des lames, qui ne surgissent que pour s'écrouler, semble un vain effort de la mer, donnant un peu d'écume et un peu de bruit ; cependant, deux heures plus tard, la grève où vous avez marqué vos pas dort sous la profondeur des eaux ; par l'incessant va-et-vient des lames qui se dressent et qui se prisent, qui avancent et qui reculent, sans relâche, l'Océan a monté.[230]

In 1896 Duhem accomplished his scientific design of a wide-scope thermodynamics, which was, at the same time, a generalised mechanics. In particular he generalised the concept and Lagrangian structure of "equations of motion" in order to describe irreversible processes and chemical reactions. Then he attempted to look at his theory from the outside,

[228] Duhem P. 1894d, pp. 124-5.

[229] Duhem P. 1894d, p. 125. The original passage deserves to be quoted: "Ainsi, sous les théories qui ne s'élèvent que pour être abattues; sous les hypothèses qu'un siècle contemple comme le mécanisme secret et le ressort caché de l'Univers, et que le siècle suivant brise comme des jouets d'enfant, se poursuit le progrès lent, mais incessant, de la physique mathématique".

[230] Duhem P. 1894d, p. 125.

in the context of the history of physics. After having put forward new perspectives in physics, he put forward new perspectives in the history of physics. He outlined a history "of physical theories from the XVII century" to his days, and once more he sent the corresponding paper to the Belgian *Revue des questions scientifiques*, which had hosted the majority of his meta-theoretical papers. He started from a widespread historiographical thesis: modern physics had emerged as "a reaction to Scholastic philosophy". He claimed that "we cannot understand the origin" of modern science, and "disentangle its evolution" if we "disregard" that philosophical heritage. The Aristotelian tradition had produced countless commentaries, in which Aristotle's philosophy had been "explained, developed, and sometimes transformed".[231]

Duhem reminded the reader that, in that tradition, a sophisticated body of knowledge had been accumulated and re-interpreted, and the possibility of bridging the gulf between ancient natural philosophy and modern physics really did exist. He re-evaluated the fundamental distinction between "*substances*" and "*accidents*": the latter could belong to the categories of "*quantity*" or "*quality*". Relations of equality and inequality, and the operation of addition could be defined in the category of quantity, whereas no addition was possible between qualities: in modern words, if we put together two bodies at different temperature, in the composed body we cannot find the sum of the two temperatures. If geometry dealt with a specific class of transformations, namely "the change of shape and position in space or *local motion*", physics dealt with a more general class of transformations or "motions", which encompassed "every kind of transformations in the substance and qualities of bodies". Among this wide class of transformations Duhem listed "a body which becomes warmer or colder", "a source of light which becomes more or less bright", but also "a solid which becomes fluid, and a liquid which vaporises", any kind of "electrisation", and eventually chemical transformations, namely "the combination of simple elements which gives rise to mixtures", and conversely "the decomposition which resolves the mixture into its elements".[232]

According to Duhem, the declining Scholastics had contented itself with "tantalizing and distorting" Aristotle's natural philosophy, and had given up pursuing an autonomous study of nature. At the dawn of the modern ages, the situation had generated "a marked distaste" in scholars who had in high esteem "rigour" and "clarity": unfortunately they had identified "the great work of Aristotle and great masters like Thomas Aquinas" with "the idle and trivial exercises of their last followers". Francis Bacon had claimed that a new logic, which he had put forward in his *Novum Organum*, could replace Aristotle's *Organon*; Descartes had put forward a new natural philosophy, where "qualities had completely been banned by the study of material phenomena". This is the first historiographical thesis which emerges

[231] Duhem P. 1896b, in Duhem P. 1987, p. 198. For his general theory of physical and chemical transformations, and in particular his generalised *equations of motions*, see Duhem P. 1896a, pp. 70-4 and 89-107. For the transformations experienced by Aristotle natural philosophy in the first century of the Christian era, and in particular Johannes Philoponus' re-interpretation, See Ugaglia M, 2004, pp. 8-9, 90, 113, 130, 133, and135-6.
[232] Duhem P. 1896b, in Duhem P. 1987, pp. 198-202.

from Duhem historical reconstruction: the founding fathers of modern science had dismissed a vital and subtle philosophy together with their late, dull copies. [233]

Alongside a new scientific practice, a new philosophy emerged: the natural world could easily be "dismantled as … a mechanical clock", and "the mechanism of nature" could easily be "explained as the mechanism of a mill". Physics was reduced to geometry, and every thing could be explained by means of "extension and motion". The situation became not so different from the declining stage of Scholastics: Descartes' philosophy gave rise to "a crowd of ignorant followers who invented the most odd and complicated machinery to account for phenomena which they did not condescend to study". Vortices "of subtle matter, and ribbed microscopic particles were so pliable models that they could fit in any explanation". Once more he reminded the reader that Pascal had devoted one of his famous *Pensées* to some Cartesians, who were not better than the last decadent Scholastic philosophers as to "ridiculous devotion towards Master's words". Although everything might be analysed in terms of extension and motion, how could we reverse the process, and build up "the machine"? Pascal had found that this pretension was "ridiculous, useless, trying, and questionable"; moreover, even if it were possible, we would not believe that "all philosophy deserves an hour of pain".[234]

Duhem reminded the reader that even Leibniz had compared some of Descartes' followers to "Scholastic philosophers … who replaced the study of nature with … commentaries to [Aristotle's] *Physics*". Besides extension and motion, Leibniz had introduced the notion of force, which allowed matter to act and resist. He had appreciated the depth and soundness of Scholastic philosophers and theologians, and had expected "Descartes' physical novel" would have been forgotten. According with Duhem, Newton's physics was based on "matter, motion, and force", and stemmed from "experimental induction", but it was in accordance with "Leibniz's design", which had stemmed from a "metaphysical intuition". On the contrary, Christian Huygens, and Daniel and Jean II Bernoulli had confined themselves to "*matter and motion*", and had refused gravitation as "intrinsic quality of matter". Duhem claimed that Cartesians' "concern about occult causes" or qualities should have been directed at their own "vortices of subtle matter, elusive as they were". Those mechanical structures were always "endowed with the suitable features to account for the most puzzling phenomena".[235]

At the turn of the XIX century Newton's approach had been extended to the emerging new chemistry by Bethollet, and after Boscovich speculations, Poisson, Navier, and Cauchy had put forward "a complete theory of elasticity for solid bodies", which was based on "the hypothesis of molecular attraction". At first Tobie Mayer and Coulomb, and then Poisson, had applied a Newtonian approach to electric and magnetic elementary actions. Poisson had "announced the advent of *physical Mechanics*", in which all kinds of interactions were

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[233] Duhem P. 1896b, in Duhem P. 1987, pp. 203 and 205-6. Duhem credited Galileo with having been the actual founder of modern science: he had shown "how and experiment had to be performed, how the results had to be interpreted", and how a mathematical law had to be formulated. (*Ibidem*, p. 205)

[234] Duhem P. 1896b, in Duhem P. 1987, pp. 209-11. For Pascal's complete quotation see footnote 228.

[235] Duhem P. 1896b, in Duhem P. 1987, pp. 211, 213-15, 217, and 219.

reduced to molecular attraction, and Laplace had claimed that those microscopic force ruled all phenomena on Earth, in the same way as gravitation ruled phenomena in the skies.[236]

Nevertheless, in the first half of the nineteenth century, when Laplace's design of unification seemed to be successfully accomplished, Fourier's new science of heat shook that design. Afterwards the emerging kinetic theory, where the ultimate microscopic parts of a body were imagined in fast irregular motion, seemed to lead back to Cartesian theoretical models. According to Duhem, the new kinetic approach was encouraged by Helmholtz's discoveries "in the field of hydrodynamics", namely the establishment of "*vortex rings*" in a perfect fluid, which could "approach or move away from each other as if some forces at a distance acted on them". Since those vortex rings appeared independent of each other and "eternal", W. Thomson made use of his extraordinary ingenuity in order to transform them into a model of real "*atom*": he was followed by Tait, Maxwell and Lodge.[237]

Duhem pointed out the most attractive feature of "that new kind of Cartesianism", namely "the wide scope and the simplicity of the first hypotheses". Nevertheless, "the complication and eccentricity" of the vortex machinery began to "disgust" the physicists: the same had happened to "primitive Cartesianism" two centuries before. However the theory had not had much success on the Continent. In any case, neither "purely Cartesian mechanism" nor "dynamism" could fit in with the "new ideas" which had emerged from thermodynamics. This is the second historiographical thesis which emerges from Duhem's paper: the second Law of Thermodynamics had represented a watershed in the history of physics. In particular, it had shown that new physical concepts could not find room inside the boundaries of traditional Mechanics. Heat fluxes could be associated neither with "a specific fluid" nor with "little motions" of microscopic molecules: the physical system simply possessed "a certain degree of a given quality", and only a suitable mathematical theory could describe the transformation of that *quality*. He mentioned the mathematician Henri Poincaré and the engineer W. Macquorn Rankine as upholders of a similar approach to Thermodynamics. If the former had pointed out that "neither Leinbiz, Newton, and Boscovich's dynamism, nor Cartesians' pure mechanism were consistent with thermodynamics", the latter had previously claimed that a "purely Cartesian mechanism", which relied only on extension and motion "but refused any quality, was fruitless".[238]

Duhem translated his second fundamental thesis into a negative meta-theoretical statement: a physical theory that realised the mathematisation of qualities could not be looked upon as an *explanation* in traditional terms, because it could not be reduced to mechanical models.

> Les théorie mathématiques ainsi constituées n'ont plus, comme les théories cartésiennes, la prétention d'expliquer les lois découvertes par la méthode expérimentale, en remontrant de

[236] Duhem P. 1896b, in Duhem P. 1987, pp. 220-1. Poisson's *physical Mechanics* was in opposition to Lagrange's *Analytical Mechanics*, in which the specific form of interactions were not taken into account.

[237] Duhem P. 1896b, in Duhem P. 1987, pp. 222-4 and 226-7. Duhem specified that "the British physicists had developed the vortex theory". In that kind of theories, he saw the hallmark of British physics: this was consistent with what he had written in the paper on English physics three years before (Duhem P. 1893e).

[238] Duhem P. 1896b, in Duhem P. 1987, pp. 228-9. Duhem found worth remarking that Helmholtz had not been attracted by vortex theories. (See *Ibidem* p. 228)

> cause en cause jusqu'à éléments métaphysiquement simples et irréductibles des choses matérielles; la qualité, provisoirement regardée comme qualité première, à laquelle celle ramènent un groupe de propriétés, elles ne l'analysent pas; elles se contentent de la désigner par un nom, d'en noter l'intensité par un nombre qui croît ou décroit en même temps que cette intensité s'exalte ou s'atténue; elles laissent aux métaphysiciens le soin d'aller au delà; elles ne se piquent pas de savoir ce qu'est la lumière, ce qu'est la chaleur, ce qu'est l'électricité, mais seulement quels effets sont attribuables à la lumière, quels à la chaleur, quels à l'électricité; …[239]

Duhem found that his generalised thermodynamics had realised a new alliance among the tradition of Aristotle's logic and natural philosophy, the more recent tradition of Analytic Mechanics, and the even more recent developments of Thermodynamics. Generalised thermodynamics was as much a new perspective in physics as a new field of physics. It was Rankine who had put forward the label "*Energetics*" for that new perspective, because he had been the first to understand "the new role" of thermodynamics. The new mathematical approach was consistent with a modern re-interpretation of Aristotle's natural philosophy. At the same time it had been realised thanks to "the relentless efforts of experimenters and mathematicians during the last three centuries". The last passages of Duhem's paper are quite emphatic: he was aware of the originality of his generalised Mechanics or *Energetics*, and he was aware that he had managed to forge an alliance among different scientific traditions. He saw his theory as the accomplishment of the whole history of physics: it was the physics of Aristotle, but also the physics of Descartes, and a revised implementation of "the *universal mathematics*, which had been dreamed by the great philosopher of the XVII century". The convergence of different traditions allowed Duhem to emphatically claim that his generalised thermodynamics was even the physics of Kepler, Galilei, Pascal, Newton, Euler, Lagrange, Poisson, Green, Gauss, Robert Mayer, Sadi Carnot, Joule, W. Thomson, Clausius and Helmholtz.[240]

In the end, a patent exaggeration emerged. Duhem claimed that, after having impatiently abandoned the Scholastic tradition, the human mind "had spent three centuries … to pave the way to the authentic knowledge of the material world". The direction of research had frequently changed, but after a long detour, had arrived "at the starting point". Nevertheless, the work done had not been useless, even though, in some cases, it had not fulfilled the corresponding expectations. He dared to claim that science had arrived at a point which had been foreseen by the supreme Being who ruled "all those fluctuations".[241]

If the reference to the supreme Being neither added nor subtracted anything to/from his meta-theoretical remarks on science, the touch of excessive self-confidence on his theoretical physics, which was credited with being the accomplishment of the whole history of physics, was definitely in contradiction with his representation of science as an intrinsically provisional attainment. His enthusiastic trust in his physical insight led him to a serious philosophical inconsistency.

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[239] Duhem P. 1896b, in Duhem P. 1987, pp. 231-2.
[240] Duhem P. 1896b, in Duhem P. 1987, pp. 233-4.
[241] Duhem P. 1896b, in Duhem P. 1987, p. 234.

### *3.4 Further debates on physics, metaphysics, and religion*

In 1904 a French scholar trained in both science and philosophy published a long paper on Duhem's "scientific philosophy" in the philosophical journal *Revue de Métaphysique et de Morale*. He really managed to draw up a competent and insightful review of Duhem's philosophy of science: he was able to understand the wide range of his achievement in theoretical physics and philosophy, and in particular he managed to point out some problematic or paradoxical meta-theoretical theses. In the first passage of the paper, when he introduced the readers to Duhem's scientific and philosophical work, he mixed a respectful style with slightly ironic remarks: "no field of physics and chemistry" had been left untouched by the renowned physicist. Not only had he "overturned traditional mechanics", but he had also taken great care of accurately mentioning all the scholars who had made even the least contribution to his update of Mechanics. In other words, he had reconstructed and clarified the scientific tradition which he claimed to have accomplished. Duhem's meta-theoretical design consisted in framing "specific chemical and physical laws into a clear and logical structure". The goal was the fulfilment of a body of knowledge which was "as wide as exact", and at the same time "as sophisticated as rigorous". Unfortunately experimental scientists could not appreciate a theoretical effort which could not lead to "useful applications": chemists would have probably thought that it was "a good piece of physics", and physicists that it was "interesting mathematics".[242]

Rey was able to understand what the new theoretical physics really was: it had emerged as a specific practice, which could be placed "between experimental physics and more or less metaphysical speculations on the nature of matter". He noted that the cumulative and "linear conception of experimental science", which had enthusiastically been promoted by Berthelot, had been overtaken by "Rankine, Helmholtz, Dubois-Reymond, Ostwald, Poincaré, G. Milhaud, etc". Apart from specific and important differences, all of them shared a common conclusion: science could "explain nothing", nor could it attain "causes". Science could only offer "connections among phenomena", the outline of a formal "description", and some "previsions", at least "to a certain extent". No hypothesis could be looked upon as necessary, because "an infinity of equivalent ones" could be consistently devised. Moreover, even an empirical statement was not so different from a hypothesis, and other hypotheses dealt with the issue "to choose or disregard" elements of observation, "to connect them in a way or another", and "to aim for a specific practical purpose". In the end, theoretical and experimental practices could not be sharply separated, because what was qualified as "the results of experience" involved "mental processes" rather than "passive observation".[243]

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[242] Rey A. 1904, p. 699. For some details on Rey's philosophical and scientific training and interests see Gillispie C.C. (ed.) 1970-80, vol. 11, pp. 388-9. His presentation of Duhem shows an uncommon intellectual finesse indeed.

[243] Rey A. 1904, pp. 700 and 702-4.

In that context, Duhem's philosophy of science could be easily misunderstood, because his starting point had much in common with the new trend, even though the consequences were different. In no way could it be associated to "Poincaré' ideas" or even to the conceptions of "Bergson's radical followers". Duhem looked upon the existence of physical theories as "a necessity", and "theoretical physics" as a specific body of knowledge endowed with a specific tradition. The keystone of his original meta-theoretical commitment was the role of Mechanics: "traditional mechanics, or what might be labelled as the *mechanics of machinery*, was inadequate". A new mechanics was required, in order to describe electric, magnetic, and thermal processes, and even changes of physical state and chemical reactions, besides "local motion". What the ancient peripatetic tradition had labelled "*degradation and creation*" could not be excluded from the field of a wider and more powerful mechanics: the new mechanics had to be "a *physical mechanics*", and at the same time "a *chemical mechanics*". In Duhem's perspective, Mechanics became a body of knowledge much wider than the specific content of ordinary mechanics: it was a mathematical-physical language rather than a well defined subject matter. Just for this reason, it could not stem from specific experimental data: it had to be set up rationally, or "*more geometrico*", and had to be "compared to experience only afterwards".[244]

Rey found a sort of paradoxical feature in Duhem's design of a new mechanics. The old mechanics could not attain the complexity of "physical-chemical reality", but Duhem had not had recourse to experience in order to attain that complex reality. He managed to synthetically express Duhem's procedure by means of the statement "it seems that reality can be deduced from his procedure, rather than his procedure from reality". The rational *engine*, which led from the first hypotheses to empirical previsions, operated by means of logic and mathematics. Furthermore, from this methodological context another paradox emerged: Duhem's procedure was not so different from Descartes', the founding father of that mechanicism which Duhem so fiercely opposed. Rey specified that the analogy was striking, because "Descartes aimed to deduce the laws of the material world from a universal mathematics", but in no way his "rational dogmatism" could be associated with Duhem.[245]

At this point Rey discussed the most questionable among Duhem's theses: the existence of a convergence or progress in the sequence of physical theories, although no necessity and no empirical constraint affected the theoretical enterprise. How to explain that process, which Rey translated into the expressions "*continuous development*" and "*real evolution*"? Duhem thought that something like a scientific progress really existed, and his "extraordinary historical scholarship in physical and chemical sciences" had led him to highlight traces of that progress in the history of science. But this was a metaphysical commitment: Duhem had "a specific … metaphysics of science" even though he refused to acknowledge it as such. In this circumstance he was not so different from other scholars, who were "materialist in all their expressions and conceptions" but pretended to be "pure positivists", as if materialism were not "an evident metaphysics". In that metaphysical context "the reaction against

[244] Rey A. 1904, pp. 704, 707, and 710-11.
[245] Rey A. 1904, pp. 718-20.

atomistic and Cartesian ideas" found place, and the revival of "the fundamental principles of peripatetic body of knowledge".[246]

However Rey saw a remarkable difference between Scholastic and Duhem's philosophy: in the former, "the first principle were dictated by the intrinsic nature of things, and by a direct insight of the absolute", whereas in the latter they were "arbitrary and conventional". Once again the freedom to devise hypotheses was difficult to combine with the existence of "*one* theoretical physics, namely a system which was better than the others". In the end Rey found that "the original compromise … between the requirements of a theoretical science and nominalist scepticism" was really "subtle" in Duhem's philosophy. The differences between Duhem and nominalists were "most in the words than in the real foundations". According to Rey, Duhem's "qualitative conception of material universe", his distrust in "a complete explication of the universe" in mechanical terms, and his purely formal opposition to "a radical scientific scepticism" corresponded to "the scientific philosophy of a believer".[247]

The implicit reference to Duhem's Catholic faith just in the last lines of the paper triggered off Duhem's reaction. He published a paper in the *Annales de philosophie chrétienne*, where he restated the clear separation between scientific practice and religious commitment, and attempted to clarify his concept of *natural classification*. In 1906 Rey briefly replied that the term "believer" made reference to the distrust in the possibility of a material self-explanation of the world, and it did not have to be intended as a specific reference to a specific religious commitment. The debate was not important in itself, because Rey had simply made use of that adjective once, and at the end of a long paper, and because Duhem had already stated a clear separation between science and faith in the 1890s. The fact is that the ideological and political context of the French Third Republic at the turn of the XX century made the personal religious commitment a very sensitive issue. Only in that context further clarifications of questions which had already been clarified made sense. In reality, the most problematic issue at stake was the concept of natural classification, and on this specific issue I find that Rey's reference to the believer was slightly misleading, but his insistence on Duhem's metaphysical commitment did not miss the point. Duhem's concept of natural classification represented the most meaningful instance of a rapprochement between Duhem's physics and metaphysics.[248]

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[246] Rey A. 1904, pp. 731 and 733-4. He insisted on the actual metaphysical character of Duhem's meta-theoretical remarks in other passages of his paper. See *Ibidem*, p. 740: "Il prend donc parti, qu'il le veuille ou non pour une hypothèse métaphysique, tout comme le mécanisme".

[247] Rey A. 1904, pp. 742-4. Duhem found in history, in particular in the history of physical theories, the evidence of that existence. History of physics had proceeded in one direction, and unfruitful branches had continuously been discarded. But that solution led to a trivial historicism: what had happened corresponded to what had to happen. Rey did not insist on this specific issue, but we must stress that the identification between the actual history of science and the features of a good science led to a vicious circle: every theoretical or meta-theoretical choice stemmed from history but also brought a new contribution to the same history.

[248] On the timing of the debate between Duhem and Rey see Deltete R. 2008, pp. 627, 629, 634, and 636 fn 6. On the metaphysical meaning of Duhem's natural classification, see *Ibidem* p. 636: "Duhem sought to separate physics from metaphysics, but, I have argued, he also tried to bring them into contact. The key to this rapprochement was the concept of NC [natural classification], the idea that physical theory tends to a classification of physical and chemical phenomena that mirrors the ontological order of nature."

Nevertheless I find that the question is even more complex, because Duhem saw his natural classification as an ideal: it was not a real entity, and it was not even an entity. It was a process: a natural classification was unattainable, and on this specific feature he had always spoken clearly. Natural classification was the name of a practice, an effort, a trend: it was ideal in the sense that it was not a real outcome, it was not attainable, and it could not be exactly defined. Natural classification was an intrinsically puzzling concept, because it was linked to the pursuit of something that could not exist. At the same time it was an authentic philosophical concept: it was the name of Pascal's *esprit de finesse* in the context of natural sciences. I find that Pascal is the key, but the fact that Pascal was also a believer might be misleading once more. Neither at the time of Pascal nor at the time of Duhem did the majority of believers dared to walk along the dangerous path of thin ice between the shore of dogmatism and the shore of scepticism.

In 1906 Duhem published the book *La théorie physique, son objet – sa structure*, where he collected and sometimes updated the content of the papers he had published in the 1890s. He specified that his remarks were not "general ideas" *on* science, or abstract "cogitations in competition with concrete details", but specific remarks which had emerged from *inside* his "daily practice of science". Once more he claimed that physical theories could not be "*explications*", but simply mathematical deductions from few physical principles. At the same time, a physical theory was something more interesting than a mere combination of mathematical structures and empirical laws. There was also a conceptual structure where "common sense and mathematical logic … mix with each other in an inextricable way". The soundness of that conceptual network depended neither on empirical nor on formal procedures. It had to do with what Pascal had labelled "esprit de finesse": it was a meta-theoretical sensitivity which could help scientists overcome the essential tension between "dogmatism" and "scepticism". In this context the concept of "*natural classification*" made sense, on which he focused the attention of readers once more.[249]

In 1906 he frequently mentioned and explicitly quoted from Pascal, who represented his methodological landmark, because he had pursued a third way between scientific dogmatism and philosophical scepticism at the dawn of modern science. The commitment to pursuing that third way was a long-lasting task for Duhem, both in the physical and philosophical field.

Duhem was conscious of the complex relationship between experimental and theoretical practices. On the one hand, a single "*empirical fact*" could be translated into "*an infinite number of different theoretical facts*". On the other hand, no physical experiment could be performed and interpreted without any theoretical assumption. For this reason, an experiment could not lead to the confutation of a single hypothesis or theory: the confutation had a global effect on the whole "theoretical scaffolding" which allowed the experiment to be devised, performed, and interpreted. According to Duhem, a physical theory could only rely on a complex and indirect link between the domain of scattered facts

[249] Duhem P. 1906, pp. 1-2, 26, 36, 440-1, and 444.

and the domain of mathematical certainty. In this awareness we find one of the hallmarks of late nineteenth-century theoretical practice in physics.[250]

In 1911 he published the two-volumes *Traité d'énergétique ou de thermodynamique générale*, where he collected and updated most of his researches in theoretical physics. After having reminded the reader that the aim of theoretical physics was "connecting the existing laws with each other, in accordance with some *general principles*", he stressed the importance of "*Rational Mechanics*" as the best "code for the general principles of physics". In other words, Rational Mechanics offered the formal structure or the formal language for physics, even for the fields of physics outside mechanics. The language of rational mechanics had nothing to do with the specific mechanical models which had been used by some physicists in the context of Thermodynamics. That language had nothing to do with the "*mechanical explanation of the Universe*" which, according to Duhem, was completely unreliable. Two different kinds of Mechanics were at stake in the last decades of the nineteenth century, and if some crisis of mechanics had ever taken place, it did not affect "the rules of Rational Mechanics". The generalisation of those rules could be identified with his rational thermodynamics or "*Energetics*".[251]

Once again, on the track of his 1906 book, Duhem stressed the intrinsic tension between empirical and theoretical practice. When a theory was conceived, in the first stage it was not required to "take into account the facts of experience", but only to take care of its internal consistency. Only at the end of a complex process, the results of mathematical procedures had to be compared with "experimental laws". Nevertheless, a theory could not be designed "at random", but it required "a justification", and that justification was as "historical" as "logical". The history of physics was a melting pot of experiences, hypotheses, mathematical tools, specific models, wide-scope conceptual streams, and meta-theoretical options. It was the stage-set where the emergence, development, and fall of physical theories had found their representation. Both logic and history taught that those principles could never "fit exactly reality": we had to "reject or retouch" them continuously. He found that scientific practice was a dynamical process which converged towards the research of a natural classification.[252]

[250] Duhem P. 1906, pp. 217, 274, 303, and 328.
[251] Duhem P. 1911, tome I, pp. 1-3.
[252] Duhem P. 1911, tome I, pp. 4-5.

## *Brief outline of a slow disappearance and a questionable reappearance*

The publication of *La théorie physique, son objet – sa structure* made Duhem known as a philosopher of science. In the following years physics underwent meaningful transformation, and the domain of microscopic phenomena was explored by means of new daring hypotheses, models, and theories. In some way, it was a process of enlargement of Mechanics beyond its traditional boundaries, but microscopic hypotheses and models lay outside Duhem's theoretical and meta-theoretical horizon. In the meantime, the role of the history of science in Duhem's intellectual enterprise had become more prominent. In 1905-06 he had published the two volumes of *Les origines de la statique*, and the first part of *Études sur Léonard de Vinci: ceux qu'il a lu et ceux qui l'ont lu*. The second part was published in 1909, and the third in 1913, while in 1908 his ΣΟΖΕΙΝ ΤΑ ΦΑΙΝΟΜΕΝΑ, *Essai sur la notion de théorie physique de Platon à Galilée* had appeared. Starting from 1913 he published the first four volumes of his monumental *Le système du monde. Histoire des doctrines cosmologiques de Platon à Copernic*. After that he began to be considered an authoritative, even though controversial, historian of science, and his likewise controversial researches in theoretical physics were definitely overlooked. His philosophy of science was discussed for some years but afterwards overlooked as well.

In 1916, the year of Duhem's death, the Austrian philosopher and economist Otto Neurath published the paper "Zur Klassification von Hypothesensystemen", whose title suggested a philosophical analysis of the foundations of science, but the content of the paper showed a non-trivial interest in the history of physical theories, in particular Optics. The author mentioned Mach, but in the context of a wider tradition of research which made reference to the "achievements of a Goethe, a Whewell, a Mach, a Dühring, a Duhem in the field of the history of physics".[253]

He did not mention explicitly the paper Duhem had published in 1894 on the history of optics, nor did he mention Duhem's 1906 book, but Duhem was on the stage when he cautiously stated that "one is involuntary impelled to accord equal value to different systems of hypotheses". Likewise cautious was the statement that "[s]ome people like to dismiss this point of view as a new fashion that was introduced by Poincaré, Duhem and others". He explicitly discussed "Duhem's opinion" that "a modified emission theory" could account for "those facts of experience" which are supposed to be only explained by "a basic assumption that differs from the emission theory". He remarked that who criticised Duhem and Poincaré did "overlook entirely" history, because the competition between opposite conceptual models, and the alternating confidence in their actual explicative power, was continuously on stage in the history of science. It was a matter of fact that there and then "as well as a

[253] Neurath O. 1916, in Neurath O. 1983, p. 14.

hundred ago" there were "often several systems of hypotheses for the explanation of the same complex of facts".[254]

At the end of the paper, Neurath acknowledged once more that the "theory of systems of hypotheses had been greatly advanced by men like Mach, Duhem, Poincaré". He found that such a second-level theory was actually necessary, because "we need theories to classify things", but we also "need theories to classify theories". However Duhem would probably not have appreciated what Neurath ironically considered the possible "result of these considerations", namely "that the great physicist must necessarily be a bad philosopher".[255]

It is worth remarking that Duhem's 1906 *La théorie physique* had already been translated into German in 1908 by Friedrich Adler, and Duhem's 1903 *L'évolution de la mécanique* in 1912 by Philipp Frank. Ernst Mach sympathetically took care of the Introduction to the German edition of *La théorie physique*.

In Deccember 1921, at the annual session of the *Académie des sciences*, the mathematician and mathematical physicist Émile Picard reported on Duhem's "the life and work". In the paper which was published in 1922 in the *Mémoires de l'Académie des Sciences de l'Institut de France*, Picard stressed that Duhem was committed to exploiting the "the analogies between Lagrange's Analytical Mechanics and Thermodynamics. Just for this reason he was more appreciated by mathematicians than by physicists and chemists, although he "wanted to be" and really was "a theoretician" of "Mechanics, Physics, and Chemistry". He had been looked upon as "too physicist by the mathematicians, but also too mathematician by physicists and chemists".[256]

In Duhem's philosophical texts Picard found "remarkable pages" and traces of "a subtle thought". According to Duhem, not only physical theories had to be looked upon as synthetic and "economical representations of experimental laws", but also as "a classification" of those laws. Picard was aware that the concepts of "representation" and "classification" echoed "the pragmatic point of view", and therefore he attempted to explain the difference between the pragmatic and Duhem's "original position". Once more the key concept was "natural classification", and it seems that Picard did not find the concept problematic. He did not comment specifically on Duhem's reference to "an ontological order", and on the possibility of an actual correspondence between "connections among abstract notions" and "real connections among things". However he stated that the confidence in that correspondence represented "the extreme boundary of Duhem's scientific philosophy", where "physics … approached asymptotical metaphysics".[257]

With regard to Duhem's general commitment to philosophy, Picard remarked that he had sometimes been associated with "pragmatists", and had been "qualified as a Kantian" in "a conference of Catholic scholars held in Bruxelles", but in reality he had always been in tune with Pascal, "from whom he continuously quoted". He mentioned Rey's 1904 paper and

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254 Neurath O. 1916, in Neurath O. 1983, pp. 28-9.

255 Neurath O. 1916, in Neurath O. 1983, pp. 29 and 31. Both Adler and Frank were qualified by Don Howard as "ardent" or "enthusiastic" followers of Ernst Mach. See Howard D. 1990, pp. 364-5.

256 Picard É. 1922, pp. XCIX-C, CIV, CXVII, and CXXXV.

257 Picard É. 1922, pp. CXIX-XX.

Duhem's subsequent answer, and stressed that Duhem had considered physics "useless in metaphysical and theological debates". He was also convinced that the overconfidence in "some analogies between peripatetic physics and general Thermodynamics" was independent of any religious commitment. Duhem had managed to catch the deep conceptual links between some "physical insights" stemming from "ancient ages", and "certain views of contemporary science". He had dared to dig "below the superficial layer" of philosophic tradition, where ancient theories had been preserved as "dead and fossilised" heritage.[258]

The same year, in the Thomist *Revue de philosophie* founded in 1900, Father Mentré reminded the readers that Duhem had signed the first paper in that philosophical journal. In the following years, he had published many important researches on the history and philosophy of science, chapter after chapter of *La théorie physique* included. He praised Duhem for having always been insensitive to "personal ambitions and honours", and he appreciated the intrinsic links and the cross-fertilisation among science, philosophy, and history in Duhem's researches. At the same time, he qualified Duhem's philosophy of science as "disappointing" and "ambiguous". After some puzzling and inconclusive remarks, he clarified the reason for that disappointment: although quite sophisticated and essentially correct, "his religious philosophy" was essentially "defensive". In other words, Duhem's philosophy of science did not allow religion and philosophy to offer a suitable framework for scientific practice. He did not appreciate the intellectual harmony between Duhem and Pascal, and claimed that "Pascal position" was "dangerous", because of its sharp separation between science and faith, and its underestimation of the necessary interplay between "scientific and theological reasons".[259]

In 1932 the French mathematician Pierre Humbert published a scientific biography of Duhem: some biographical notes were accompanied by three chapters devoted to Duhem's researches in physics, philosophy and history. He pointed out that Duhem was more appreciated by mathematicians than by physicists: if he had found "esteem in the community of mathematicians", on the contrary he had lived in the community of physicists "under a cloud of suspicion because he made too much mathematics". Humbert also remarked that Duhem had lived in a context of "extreme scientism", which was one the hallmarks of late XIX-century French cultural environments. He looked upon Duhem as one of the leaders of a new, more sophisticated, approach to science: what Duhem, Poincaré, Boutroux, and Le Roy had in common, in spite of their specific conceptions, was the awareness of the complexity of scientific practice, the awareness of its intrinsic limitations, and the opposition to a "blind faith" in science. Although Humbert's methodological remarks were rougher than Duhem's, he stressed some essential features of Duhem's view:

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[258] Picard É. 1922, pp. CVI and CXXXVI-VIII.

[259] Mentré F. 1922, pp. 450, 454, 459-60, and 464. The Catholic journal had been founded in 1900.

the historical and provisional nature of science, and the plurality of theoretical interpretations of a given set of phenomena.[260]

In 1941, in the Prefatory Note to his book *The Methodology of Pierre Duhem*, the American scholar Armand Lowinger remarked that he knew of "extended comments on Duhem work only in French", apart from "the Preface by Mach to the German translation of *La Théorie physique*", and a paper published in *Isis* in 1936. Lowinger's book dealt mainly with *La Théorie physique*, and contains some oversimplified statements on Duhem's methodology. Duhem's point of view on the scientific practice was qualified as "methodological positivism", where "all entanglements with problems which do not lie strictly within the province of scientific methodology" are excluded. In reality the context and scope of Duhem's theoretical and meta-theoretical researches was wider than Lowinger claimed: the fact is that, in the 1940s, the echo of late XIX-century theoretical physics and philosophy of science had already faded away. The scientific practice in physics began to focus on huge projects on nuclear processes, and excluded meta-theoretical commitments intrinsically linked to that practice. Lowinger reported without regret that "(a)rguments pro and con" issues like "abstract methodological philosophy", or "the general history of science", or "the history of the particular theory in question" carried "no professional scientific weight". In the end, Duhem's conceptions were looked upon as too sophisticated: Lowinger found that, "(i)n the context of the actual scientific process", scientists were "enabled to rehabilitate induction and crucial experiment as integral parts of scientific methodology".[261]

After the Second World War some themes which had been put forward in the late XIX-century philosophy of science re-emerged in an unexpected way. In reality, Duhem's books and papers had almost been forgotten, but a new interest in some of his meta-theoretical theses emerged in the context of a philosophical tradition that was deeply linked to logic. In 1951 Willard van Orman Quine published a paper where he sharply criticised both the dichotomy analytic/synthetic and reductionism. He claimed that "[m]odern empiricism has been conditioned in large part by two dogmas". One of them was the belief "in some fundamental cleavage between truths which are *analytic*", namely statements independent of "matters of fact", and "truths which are *synthetic*, or grounded in facts". The other "dogma", reductionism, corresponded to the belief that "each meaningful statement" was equivalent to "some logical construct upon terms which refer to immediate experience". He found that both beliefs were "ill founded": he rather lent towards "a blurring of the supposed boundary between speculative metaphysics and natural science". Moreover, an inescapable effect followed: "a shift toward pragmatism".[262]

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[260] Humbert P. 1932, pp. 17, 62, 69, and 73. He was a (Catholic) mathematician who spent most of his career at Montpellier, and did some researches on the history of science. He dared to overturn the received view on Duhem's right-winger, and claimed that he had been a democrat (see *Ibidem*, p. 126, footnote 1: "ses préférences secretes le poussaient du côté des démocrates, chez qui il comptait beaucoup d'amis"). He also reported a speech wherein Duhem spoke in favour of the university training of girls (*Ibidem*, pp. 133-4).
[261] See Lowinger A. 1941, in Lowinger A. 1967, pp. 19, 165, and 170.
[262] Quine W.O. 1951, p. 20.

The distinction "between analytic and synthetic statements" was nothing else but "an unempirical dogma of empiricists", namely "a metaphysical article of faith". Although he acknowledged that reductionism "in its radical form" had "long since ceased to figure in Carnap's philosophy", he maintained that "the dogma of reductionism … in a subtler and more tenuous form" had continued "to influence the thought of empiricists". According to Quine, the new version of the dogma implied that "each statement, taken in isolation from its fellows, can admit of confirmation or information at all". On the contrary, he found that "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body". Moreover he found that the dogma of reductionism was tightly linked to "the other dogma", namely the existence of "a cleavage between the analytic and the synthetic".[263]

With regard to the connection with Duhem's meta-theoretical theses, it is worth remarking that Quine neither quoted from nor mentioned Duhem. Quine's paper represented an end and a beginning at the same time. It represented the end of a stage in the history of philosophy: he went after Duhem's intellectual framework had definitely disappeared, and the memory of Duhem's texts faded away. At the same time, it represented the beginning of a new interest in the dynamical structure of actual scientific practice, beyond a static logical analysis, which had been one of Duhem's specific commitments. In the Preface to the 1963 collection of essays *From the logical point of view*, Quine specified that his "critique of analyticity" had stemmed from "informal discussions, oral and written", which had taken place "from 1939 onwards with Professors Carnap, Alonzo Church, Nelson Goodman, Alfred Tarski, and Morton White". This means that neither before nor after the Second World War he was aware of Duhem's epistemological theses or was interested in discussing them explicitly.[264]

In 1960 the philosopher of science Adolf Grünbaum put forward a refutation of what he called "Duhemian argument". This expression and the other he made use of in the first pages of his paper, namely "Duhem's contention", "Duhemian fashion", and "Duhem's thesis", did not make detailed reference to Duhem's papers or books. However the English translation of the 1914 second edition of Duhem's 1906 book was mentioned in the first footnote, even though the book did not appear in the short bibliography. He also made use of the expression "Duhemian thesis" and the substantives "Duhemism" and "Duhemian". The adjective "Duhemian" appeared in the expressions "Duhemian schema", "Duhemian view", "Duhem's contention", and "Duhemian ambiguity". Although Quine is mentioned in the second line of the paper, he does not appear in the bibliography as well. On the contrary,

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[263] Quine W.V.O. 1951, pp. 34 and 38.

[264] Quine W.O. 1963, p. viii. In 1990 Don Howard reported that, in a private communication, Quine had acknowledged that Carl Hempel and Philipp Frank had pointed out "the kinship" of his views to Duhem's (See Howard D. 1990, p. 376, fn. 2). See also Howard D. 1990, p. 363: "Duhem's influence on Neurath was significant, direct, and generously acknowledged. His influence on Quine was equally significant, though indirect (Neurath was the principal intermediary), with Quine himself having been unaware of the parallel between his and Duhem's views until it was pointed out by others. And it was primarily through Quine's writings that Duhem's ideas have retained what currency they have in contemporary debates in the philosophy of science."

Einstein's *Autobiography* does appear, because he claimed that "Duhem's argument" had been "*articulated* and endorsed by Einstein" a decade before.[265]

From the outset we can notice that the language, the lines of reasoning, and the insistence on logical aspects, specifically logical calculus, are far from Duhem's actual language and conception of physical theories, where extra-empirical and extra-logical features were in prominence. In brief, the core of Duhem's meta-theoretical remarks got lost in a net of logical deductions which were "Duhemian" in appearance but were non-Duhemian with regard to the context. Historical dimension had disappeared as well: the emergence and hegemony of neo-positivism had made philosophy of science divorce from history of science. In the end, the re-emergence of what was called Duhem's philosophy of science (he had probably not appreciated such an expression) was a re-interpretation which entailed an over-simplification. In the brief abstract, Grünbaum claimed that he was to offer "a refutation of P. Duhem's thesis that the *falsifiability* of an isolated empirical hypothesis H as an *explanans* is *unavoidably inconclusive*". In reality he probably refuted an abstract *Duhemian* fellow who made sense only in the context of his philosophical framework.

In 1965 the philosopher of science Laurens Laudan published a short paper on Grünbaum's approach to Duhem's meta-theoretical remarks. He claimed that Grünbaum had "misconstrued Duhem's views on falsiafibility", and that "the logical blunder which he discussed should not be ascribed to Duhem". He found that there was a conceptual gap between Duhem and Grünbaum, and that over time some cultural processes had transformed "Duhem's conventionalism into the doctrine which Grünbaum attacks". More specifically, he stressed the necessity of undertaking "a careful analysis of the historical and textual context of Duhem's account of crucial experiments": that analysis would have shown "how far Grünbaum's argument misses the mark". He reminded the reader about "the naïve realism with which most scientists of the late nineteenth century discussed their theories": in that historical and cultural context, "Duhem was preoccupied with, and disturbed by" that kind of realism.[266]

In general, in the early 1960s some historiographical and epistemological theses and remarks, which had emerged in the late XIX century, raised new interest and new debates, but the memory of the historical context was rarely taken into account. Some characters were forgotten or misunderstood or separated from their contexts. From the 1970s onwards historians, historians of science, and philosophers of science began to be attracted by late XIX-century context in general, and Duhem's philosophy of science in particular. The debate among logicians continued as well.[267] Late XIX-century philosophy of science

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[265] Grünbaum A. 1960, in Harding S. (ed.) 1976, p. 119.

[266] Laudan L. 1965, p. 295. He made reference to the English translation of the second French edition (1914) of Duhem's *La théorie physique; son objet et sa structure*.

[267] With regard to historical researches, besides the already mentioned studies, see for instance Paul H.W. 1968, Paul H.W. 1972a, Paul H.W. 1972b, Redondi P. 1978, Paul H.W. 1979, Maiocchi R. 1985, Brouzeng P. 1987, Brenner A. 1990, Martin R.N.D. 1991, Deltete R. 1999, Stoffel J.F. 2002, Needham P. 2002, Stoffel J.F. 2007, and Brenner A., Needham P., Stump D.J., and Deltete R. 2011. With regard to the logical/philosophical debate see Ariew R. 1984, and van Fraassen B. 1989. Detailed references are given in the corresponding section below.

stemmed from a remarkable epistemological and historiographical awareness, and that awareness would deserve to be further explored.

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