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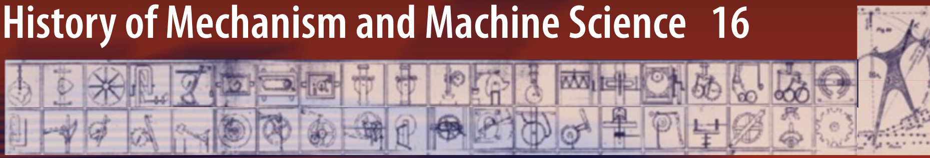

Evelyne Barbin
Raffaele Pisano *Editors*

The Dialectic Relation Between Physics and Mathematics in the XIXth Century

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Evelyne Barbin • Raffaele Pisano
Editors

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Springer

Editors
Evelyne Barbin
Laboratoire Jean Leray
Université de Nantes
Nantes
France

Raffaele Pisano
CFV-Université de Nantes
France
and
RCTHS-University
of West Bohemia in Pilsen
Czech Republic
France

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Foreword

Mathematical Physics and Theoretical Physics as Dialectics Between Mathematics and Physics

From the beginning of the nineteenth century, the scientists themselves are explicitly concerned by the problems arising from the relations between Mathematics and Physics. Their different answers constituted the different kinds of what they called "Mathematical Physics" and "Theoretical Physics". Indeed, many of their reflections focused on certain antinomies or dualities like oppositions between continuum and atoms, dualities between facts and laws, empiricism and rationalism, reasoning and theoretical foundation, and also turned into confrontations, like between atomism and energetism, mechanism and functionalism. In a more precise sense, the constructions of Mathematical Physics and Theoretical Physics were the answers that they gave to dissolve (analysis) or to thematize (synthesis) the antinomies and the oppositions, in any case to pass over them by the invention of mathematical or physical concepts, mathematically or structurally linked. It is under this last condition that the dualities became dialectics.

The historical approach permits to analyze the conditions of the births of the answers, concepts and theories, and to examine their explicit confrontations. Moreover, this approach shows the manners that are used by scientists to adapt the concurrent positions and so to create new conceptions. The different answers are constructed by scientists, heirs of scientific or philosophical schools and gathered in universities or nations. This book proposes to take a European scale to analyze many examples of dualities, confrontations and dialectics and to try to have a more global view. Indeed, there was a plurality of new conceptions, but there was also a circulation between them. This circulation did not only depend of the possibilities of communication at this period, but also of the same willingness showed by the scientists to obtain a unity of the different physical sciences.

Let us examine some of these aspects in the light of the different chapters of this book. From the beginning of the nineteenth century, many difficulties occurred in the mathematical treatment of the phenomena of Nature, despite the success of the methods of the previous century. One of them was the lack of simplicity of the mathematics involved in physical sciences, particularly because it seemed not possible to realize them in an immediate perception.

For René-Just Haüy, the research of simple results corresponded to his conviction on the simplicity of Nature. It served explicitly to deduce the "true ratio" between measurements of angle, in his work on crystallography of 1801 (Chap. 1). Samuel Christian Weiss, an heir of the German philosophy, criticized Haüy's conception because it presents two defaults, the atomism and the empiricism, on which it depends. Weiss overcame the opposition between molecule and continuum and the conflict between empiricism and rationalism, by introducing a simple mathematical concept, which is the axis of symmetry (1814).

While for James Clerk Maxwell, the simplicity was not to seek in the things of Nature but in the process of the reasoning. Moreover, this simplification may take the form of "a purely mathematical formula" or of "a physical hypothesis" (Chap. 2). Here, a duality opposes the process of reasoning to the theory of knowledge. His criticism of Ampère's works on electro-magnetism shows the kind of rupture he introduced. Maxwell considered that the reasoning of Ampère on infinitesimal elements is not sufficient to show the electro-magnetic actions. For him, the connection between two sciences has to depend on a "physical analogy" (1856), that means a similarity established by the reasoning between the laws of one science and those of another. This programme will have an achievement with the concept of electromagnetic field (1865).

The confrontation between English and continental conceptions gives other examples of the resolution of dualities. The researches on the theory of elasticity in Italy are interesting from this point of view, because they concern the opposition between atomism and energetism (Chap. 3). In his first work on elasticity (1866), Enrico Betti assumed bodies as formed by molecules, and he introduced a potential function with consideration of forces. But, the next year, on the basis of William Thomson's works, he gave an energetic meaning to the potential. In his book, entitled *Theory of elasticity*, the exposition begins with the principles of potential energy and virtual work.

The meeting between Joseph Liouville and William Thomson in Paris in 1845 had important consequences on the kind of mathematics elaborated by the former in connexion with Physics, and so on his conception about relations between Physics and Mathematics (Chap. 4). Indeed, Liouville considered in 1832 that the solution of most physico-mathematical problems only consists in solving differential equations. But, in Paris, Thomson showed to his French colleague a transformation that maps an electrostatic problem into another problem easier to prove. One consequence will be the geometrical theorem of Liouville on conformal mappings of space given in 1850.

The historical approach indicates many fields where the willingness of unity expressed itself, from the active reasoning of the scientist to his abstract speculations. In the different physics, is it possible to always use the same reasoning for the English Maxwell, the same mathematics for the French scientist Gabriel Lamé or the same principles for the German scientist Carl Neumann. For this purpose, the context of the writing of treatises is important to analyze the implemention of the unity of physical sciences, like we can see in two chapters devoted to French and German universities.

With Gabriel Lamé, the work on a unified Mathematical Physics is linked with his lessons given to students in the university of La Sorbonne in Paris in the year 1850. He proposed a mathematical unity with the invention of curvilinear coordinates, and for him the truly universal principle of Nature is aether. His pupil Emile Mathieu conceived the project of a complete treatise when he gave lessons in the university of La Sorbonne in Paris (Chap. 5). For him, the unity could not be found in the aether, but in the unique procedure, which operates in each of the physical sciences. He intended to work with a minimum of physical principles, and a strong mathematical theory, which is developed in the first volume of the treatise in 1873. The Mathieu's treatise, edited from this date until 1890, took into account the different works done before and research to adapt them to his own conception. For instance, he quoted Maxwell as well as Wilhelm Weber and Carl Neumann.

Neumann expressed his point of view on Physical Theory in his inaugural lectures given in Tübingen and then in the university of Leipzig in 1869. For him, the task of the physicist is to explain all phenomena that occur in Nature by a few basic principles as possible and a few inexplicable facts as possible (Chap. 6). The proper aim of mathematics consists in finding out the principles from which the laws of empirical facts can be derived in a mathematically correct way, and so to find out principles "equivalent" to the empirical facts.

One result of the conceptions of Neumann and Mathieu is that the theories appeared to be temporary, because they depend of the mathematics involved by the scientist and by the facts he could or he decided to gather. In 1870, Neumann conceived that a physical theory is always incomplete and changeable. Some years later, Mathieu presented the theories in their historical process and gave historical developments, which lead to them, in his treatises. The idea that the theories depend of the historical process is present in the epistemologies of the beginning of the twentieth century, like those of Pierre Duhem in 1906 or Ernst Cassirer in 1910. For Duhem, there is a continuum of progress in history, while for Cassirer there exists a fundamental rupture in the historical passage from a science thought as a description of perceptions to a science constructed as a set of mathematical relations.

The Chap. 7 of this book propose two epistemological reflections on theoretical Physics. The first one considers that "physical world" force, pressure and entropy always existed and that their properties are independent from the development of our physical knowledge (Chap. 7). But "the world of physics" has undergone a number of fundamental changes and the most important of them concerns the language of physics. On this basis a reconstruction of the historical development of the language of physical theories is proposed. It is the case for the field theory from 1831 to the end of the century.

The Chap. 8 emphasizes that the pluralism in the relationship between Physics and Mathematics in the nineteenth century was disregarded (Chap. 8). For instance, it is noted that the date of "birth" of each new theory of the century was often contested. After a theory was considered as completed, some foundational parts were added some decades later, like for the thermodynamics in 1850, which was seriously modified in 1893 by Planck and in 1896 by Mach. This could be interpreted by the fact that the "true differences" among the various relations between mathematics and physics were ignored. This can also explain the crisis in the first years of the twentieth century. The reason would be that the community of physicists was unaware of the profound changes occurring at the time. This interesting historical and epistemological problem is surely one of the questions, which can serve as a vital lead to read this book.

One possible conclusion of this book can be that the important differences between conceptions have not to mask to us a major agreement between scientists on the necessity to work on the relations between mathematics and physics themselves. For many French scientists, the Mathematical Physics consisted in associating a differential equation to a phenomena and to solve it. So, the physical sense of a differential equation supposed an interpretation of the symbols in terms of physical notions (potential, time, etc.), and a physical science consisted in linking symbols and notions. While the physical analogy of Maxwell supposed the establishment of a relation between notions of two physical sciences, it created a link between physical notions. In the first case, the mathematical connections between physical notions were those given by the calculus on magnitudes (continuum), while in the second case, the mathematical connections could be purely relational.

In any case, the correspondence between laws and facts became a dialectic expressed in the terms in which the task of mathematics is given. Now the purpose was not to link physical notions to things but to link physical concepts together by mathematical relations. The function of the concept is to gather and to order the empirical material. There were two mathematically ordered series in correspondence: series of facts and series of physical concepts. In 1905, Henri Poincaré explained that if the experience made known a relation between bodies, A and B, which was complicated, then we could introduce three intermediary relations: a relation between A and a figure A', a relation between B and a figure B' and finally a mathematical relation between A' and B', which had the property to be simple. So, mathematical relations were the instruments to construct simplicity. He explained that science was a classification and precisely a system of relations. For him, the objectivity had to be found in these relations and not in the isolated things.

This book takes into account most of the contributions of the Symposium organized by Evelyne Barbin and Raffaele Pisano in the 4th International Conference of the European Society for the History of Science (ESHS), which was held in Barcelona from 18th to 25th of November 2010. It was specially prepared with the purpose to make known some recent developments in the history of science and to share the interesting exchanges of the symposium.

Laboratoire Jean Leray
Université de Nantes
France
Nantes, February 2012

Évelyne Barbin

Contents

The Construction of Group Theory in Crystallography

Bernard Maitte

Abstract This article sets out to retrace the manner in which Group Theory evolved in crystallography. To engage in this study it is necessary to select, amongst all the approaches to crystals, those which, from the point of view of modern science, mark a step towards the establishment of our current understanding. In this way it favours our current perspective. To compensate this distortion, we recall the context in which each explanation that marks history appears. It so becomes clear that notions of triperiodic assemblages and the crystallographic laws of crystal systems and symmetrical classes do not derive from "natural" observations but were compiled and belong to precise theories.

1 Preamble: Contexts

I am going to recount the history of the construction of group theory in crystallography. This construction will prove to be extremely rich in that it allows the connection between physical and symmetrical properties, leads Pierre Curie to make symmetry a tool of reasoning, is used in the twentieth century as a cross-border theory between different disciplines and marks the origins of radio crystallography and spectroscopic studies. As we shall see, this history extends over two centuries, crossing diverse branches of science (natural philosophy, physics, mineralogy, crystallography, chemistry and mathematics), and developing varying characteristics depending on the geographical location concerned. Different scientific traditions are set in motion and give rise to the use of different methodologies. In the

B. Maitte (✉)
Centre d'Histoire des Sciences et Epistémologie (CHSE/UMR STL-8163),
Université de Lille 1, Lille, France
e-mail: bernard.maitte@univ-lille1.fr

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seventeenth century it is Kepler and Hooke who, amidst the fever of observations and interpretations which are typical of the birth of a "new science", study both with the naked eye and under microscope the regular shape of crystals and attempt to explain them through a subjacent structure, inaccessible to the best instruments at their disposal. Their efforts are taken up by Christiaan Huygens who contributes to the creation of a geometric and "hypothetical-deductive" science, based on mechanisms which apply in the world of Descartes: a world filled with infinitely liquid ether, composed of elements which resemble hard spheres, in contact with one another. He observes calcite, its patterns, its cleavages, its birefringence, and shows that all these properties possess a common symmetry. He represents them as a repetition in the three directions of space (a triperiodic assemblage underlying ether-immersed molecules), a mass which slows down the speed of passing light waves. He deducts from this hypothesis various physical properties, not yet, but soon to be observed, and accurately predicts the numerical consequences which he verifies though experiment. The development of this rich approach is however brought to a halt by Isaac Newton, partisan of a vacant world of discontinuous matter, of a corpuscular theory of light, a Newton who makes full use of his authority, and false measurements to discredit it.

It is in fact much later on and independently of the work of its precursors that the history moves forward. It is linked to the prodigious effervescence of the century of the Enlightenment, inspired by Locke and Malebranche. For them, the "great systems" of mechanics are incomplete: gravity can only be explained, according to Newton, through theology, and life cannot be accounted for merely by a machine. For Locke (1690), human understanding emanates from two sources: on the one hand external objects which provide qualities, and on the other perceptions and the mind which supply him with ideas. To increase knowledge, it is necessary to discover sensory properties. If the definition of a world system remains the objective for which to aim, it is an ideal whose achievement cannot leave aside patient analysis. From that point, the focus must be on empiricism and finding in our non-mechanical reasoning new channels for science. The 'truth' is only the temporary matching of all our discoveries. To give our knowledge this limit guarantees tolerance. Savants and philosophers, deists and atheists study with passion all that had been neglected by the great systems: polyhedra, electricity, magnetism, nature, life. The history of natural science becomes a moment in the history of all sciences, a history that is written in parlours, in the "cabinets de curiosité" where naturalists expose their findings or purchases from their different excursions, and attempt to classify them, turning their attention to crystals. Romé de l'Isle wants to determine their sensory properties. To this end he measures the dihedral angles of the crystal faces and notes the equality between the homologous faces. He lays down a first empirical law which links together these properties: the faces of all crystals of the same species form constant angles (1780). He attempts to elucidate this concept of "constant angles" by an underlying structure, and for this establishes what he justly considers as a new science: crystallography.

Shortly afterwards, another French naturalist, René–Just Haüy carries out the same studies. He silences Romé de l'Isle's precedence, coming closer to all the scholars of the end of the eighteenth century, who, drawing on the success achieved by Newton's predictions of terrestrial and celestial phenomena, pursue these developments in physics and inscribe them within a renewed framework. Laplace, Monge, Berthollet and others define a world mathematically wherein all facts can be explained by forces exerted on masses which from the infinitely great (the universe) to the infinitely small (chemistry), constitute trajectories identified in absolute space and time. Haüy shows them that geometrization can affect natural sciences. He relies on a molecular hypothesis to develop a first law of symmetry and goes on to precisely determine a considerable number of physical properties. At the end of a lifetime's intense work, within which his ideas evolve gradually, he illustrates to a fairly satisfying degree the inclination of crystal faces and the external forms and physical properties of crystals, classified in seven crystal systems. Or, almost as there still remain some "marvellous exceptions".

It is just these exceptions that German crystallographers set out to explain. Building on Haüy's work, and above all following the tradition of "Naturphilosophie", principally developed by Kant and by Schelling, they refuse at the beginning of the nineteenth century the molecular concept of matter. They prefer the theory of continuous matter, divisible to infinity, a space in which reside antagonistic forces, attractive and repulsive, which can reach equilibrium. The place at which the points of equilibrium are reached is a crystal face. Homologous faces are produced by analogous forces, organised along convergent lines. Those with equivalent symmetry correspond to "axes of symmetry", a new concept introduced by Weiss. He defines several convergent axes compatible to crystals, combines them and defines the symmetrical classes, grouping them in crystal systems. Haüy's exceptions are explained by these classes. Mohs and Hessel complete Weiss's inventory. The German crystallographers identify 32 classes of symmetry which are organised in the 7 systems. During the same period, still within the Naturphilosophie movement, and through his search to identify the antagonistic forces which can only fill space, Ørsted takes note of the reciprocal action wielded by a magnetic bar and a current: it is perpendicular to both. This force cannot be Newtonian. Its direction had not been noticed by the scientists who, like Coulomb, measured the intensity of forces in function of distance and had concluded their variation according to Newton's laws. Through his experiences, Ørsted founded electro-magnetism, which would be developed in France by Ampère, and in England by Faraday and Maxwell: all contributed to bring into question Laplace and Newton's physics and substitute weightless fluids by the spread of continuous waves across bodies and an ether-filled space.

As far as crystals are concerned, the results obtained in Germany are deliberately ignored by French mineralogists. As far as crystals are concerned, the results obtained in Germany are deliberately ignored by French mineralogists. The reason is that they cannot accept the introduction of axes of symmetry which they could perhaps consider as lines of molecules, but which remain in their minds as vague and incomprehensible philosophical notions. Delafosse and Durozier write Delafosse and Durozier write:

> (German) Mineralogists (express) [. . .] hostility [. . .] to [. . .] the [. . .] molecular theory (the cause of which needs to be examined) . . . in the idealist theory: this type of metaphysical consideration which reoccupies all German intellectuals. A few ambiguities taken from the Greeks, a few fallacies based on Kant's contradictions, have led the German physicists to prefer in the study and interpretation of natural phenomena the sort of vague and obscure explanations that they call "dynamic" rather than the simple clear and positive notions that we draw from atomistic hypotheses.[1]

In 1840 Delafosse takes up Haüy's research, regretting that he did not "give to his work the character of a theory of physics", explains the "marvellous exceptions" in a Newtonian framework, imagining that molecules of which crystals are constituted are polyhedral in shape and repeat themselves in networks. This lattice concept is assured a bright future: in 1848 Bravais lists 14 possible modes, classified into the seven systems. He limits his speculation to "purely geometrical speculation", arising from his wish to protect his work from relying on a Newtonian vision of matter at a time when this concept is more and more brought into question. As soon as the following year, however, he applies his networks to crystallography and is the first in France to introduce axis of symmetry. But it is a German, Sohncke, who extends the interest of the use of lattices through the combination of system, classes and modes, and he inventories what we call the "Sohncke groups". The identification of groups in crystallography is not yet over. Fedorov introduces elements of symmetry with translation (screw-axes and gliding planes) which leave the lattice types invariable. Setting out to achieve a formal mathematic study, he can show that (independently and simultaneously as does Schoënflies) there are 232 sub-groups in the groups, the classes the modes and the systems. Reflecting on the approach of these crystallographers leads Pierre Curie to take it one decisive step further. No longer limiting the approach to shapes and the study of shapes, he considers symmetry as a tool of reasoning, in which shape is mentally integrated and generates ideas. The development of Group Theory in crystallography is thus almost complete. It has come about thanks to fruitful crossing of different presuppositions and scientific traditions, but accepting results obtained by others, on the condition that they have been verified. Science then forgets it origins and the way in which knowledge has been founded.

[1]Delafosse and Durozier, 579. "Les minéralogistes [allemands manifestent] [. . .] de l'antipathie [. . .] pour la théorie moléculaire [il faut en rechercher la cause] [. . .] dans la philosophie idéaliste : ce type de considération métaphysique qui préoccupe tous les intellectuels allemands. Quelques équivoques reprises des grecs, quelques sophismes basés sur les fameuses antinomies de Kant, ont conduit les physiciens allemands à préférer dans l'étude et l'interprétation des phénomènes naturels les sortes de vagues et obscures explications qu'ils appellent "dynamiques" plutôt que des simples, claires et positives notions que nous tirons des hypothèses atomistes."

2 The Origins of the Model of Triperiodic Assemblage

2.1 The First Approaches

In his work the Strena, Kepler (1609) examines snowflakes and certain crystals, deciphers their patterns following on the one hand the tradition of Aristotle, asserting that all gems have a liquid origin, and on the other hand interpreting them as geometric examples of the Creator's perfection, and lastly using mechanical arguments by which they are the macroscopic results of the assemblage of spherical particles invisible to both the naked eye and magnifying glass (Kepler). It is also the "prodigiously regular pattern" of snow that Descartes (1637) studies in his *Dioptrique*. He explains this in an exclusively mechanical manner, through the assemblage of small spherical particles of iced water, in contact with one another. In his *Micrographia*, Hooke (1665) describes the crystallisations of salts, the external forms of these crystals, by imagining their construction as an assemblage of small identical spheres coming into contact with one another (Hooke, 85–86).

In his *Traité de la Lumière*, Huygens (1690) justifies the external patterns of "Icelandic spath" (calcite), its cleavages and its birefringence with the supposition that calcite is built by the repetition (invisible by the most powerful microscopes), in the three directions defining the rhombohedron, of small molecules that possess a triperiodic structure within which are arranged small masses comparable to spheroids in contact with one another and arranged symmetrically aligned according to an axis. The sides of the rhombohedron repeat themselves with respect to this axis, as do the cleavages of the mineral, and its optical properties (the axis is itself an isotropic direction). He deduces from this triperiodic assemblage and from his wavelike theory of light (based on the hypothesis of an ether-filled world) the exact values of birefringence for particular sections. He then verifies these by experiment and extends his deductions to quartz and mica, discovering their birefringence (Huygens, chapter V). But Newton (1702) disqualifies Huygens's work after his death. Newton prefers to imagine, without going any further, that the physical properties result from the assemblage of ultramicroscopic corpuscles possessing determined geometric forms (Newton, Question XXV).

Cartesians and Newtonians are thus in agreement in that they describe macroscopic properties by microscopic structure, put forward that this microscopic structure of matter is discontinued and want to explain the world in terms of mechanics, but they are opposed in that the former suppose the world to be full, and that gravity is the result of a mechanical action, whilst the latter consider the world as void where gravity is the constant action of God over the world. The attention of the best physicists concentrates around this debate and obliterates all questions considered to be non-essential to the controversy. The premises established by Hooke, Huygens and Newton with reference to crystalline structure will not be the subject of the developments they merit for a long time.

2.2 The Sensualists

As a reaction against the Cartesian and Newtonian mindset of systems and to counter their assertions, explaining all things via mechanics – even life! – an entire stream of thought establishes itself in the first half of the eighteenth century, with the aim of finding new perspectives for science based on non-mechanical reasoning. The work of Wallerius is translated into French by D'Holbach (1753), which looks into the medicinal or moral qualities of stones as well as their natural form, their size. This work which is accessible and translated into the vernacular captivates curious minds, excites collectors, is debated in parlours and stimulates research amongst a whole class of idle society who are passionate about natural curiosities (Wallerius). Amateurs probe just for pleasure and initiate an infinite amount of original ideas. Dictionaries inform these curious souls on every kind of subject, and become fashionable. Classifications are developed: chemists favour compositions; descriptors prefer to dispense with chemistry – too complex – and claim to know all the features of a mineral type thanks to the careful examination of directly observable properties, their sensory qualities : the taste, the smell, the sensation on the tongue, the external shape, the solidity, hardness, consistency, colour, shine. The two groups ignore a large number of mineral qualities and focus on a sole characteristic which they deem to be essential. The sensualist stream of thought no longer wants to explain the crystallisation or the cohesion of minerals but to connect diverse samples described via fixed laws. Jean–Baptiste Romé de l'Isle, who belongs to this stream, observes the minute details of the collections assembled in the "cabinets" and parlours, publishes catalogues of reasoned identification of the minerals, attaching particular importance to external forms and increases the number of inventoried forms from 40 to 400. He measures the angles of plane faces, compares the dihedral angles of sides with the aid of gauges and writes in 1772 an *Essai de Cristallographie* which enjoys a great success (Romé de l'Isle 1772). With the aim of measuring with precision the dihedral angles between the sides of crystals, he seeks out the mechanician, Carangeot, who perfects a "mapping goniometer". Armed with this instrument, Romé de l'Isle is able to measure systematically the angles of all the crystals in his possession and to establish that minerals of the same type always present constant interfacial angles. He is then able to write and publish, in the introduction to his Cristallographie (1783) what is known as the first law of crystallography: the constancy of dihedral angles:

> Crystal faces can vary in their shape and in their relative dimensions, but the respective inclination of these same faces is constant and invariable in each species.[2]

[2] Romé de l'Isle (1783, I, 93). "Les faces d'un cristal peuvent varier dans leur figure et dans leurs dimensions relatives; mais l'inclinaison respective de ces mêmes faces est constante et invariable dans chaque espèce."

Romé de l'Isle can now idealise crystalline forms: since the respective development of the sides of a mineral is not important, he can therefore represent crystals through models in which all the homologous faces develop in the same way (Fig. 1).

Fig. 1 Romé de l'Isle: idealise crystalline forms

In this way, crystalline types can be characterised by means of three parameters: their density, their hardness and their polyhedral form. Romé de l'Isle is in agreement with Salomon's assertions in the book of Wisdom: "You can prepare all things with measurements, number and weights", and notes that this supplies a guide to direct the over-complex chemistry. Having been able to quantify certain significant properties, Romé aspires to the foundation of a "New Science", crystallography. To this end he reads his precursors, criticises them, carries out his own experiments and infers from this a coherent reasoning: crystals are composed of tiny corpuscles which draw near during crystallisation but which remain at a distance from one another. These corpuscles are inaccessible, and will perhaps remain so forever, but we shall perhaps be able to reach the smallest part that the crystal can present: it is composed of corpuscles of different nature, but possessing all the properties of a crystal – the "integrant molecules". The "integrant molecules" stack together to form crystals, spread through geometry into six categories: the six convex polyhedrons which fill space (tetrahedron, cube, octahedron, rhombohedra, parallelepiped, rhombohedral octahedrons and dodecahedrons with triangular faces). A purely geometrical operation gives mathematic proof of the validity of the deduction: one can trace the idealised forms from polyhedrons by intersecting their vertices or edges with the faces. These are the "truncations," and Romé de l'Isle concludes his introduction, before going on to describe the minerals classified by their chemical composition (salts, acids), with the precision:

> We are still far from being able to account for the internal and hidden mechanism of crystallisation: it one of Nature's mysteries [. . .]. The reason for this is simple: elementary molecules and even the first "integrant molecules. [. . .] are [. . .] inaccessible to our senses, and the smallest speck of matter that our eyes can perceive through a microscope, far from being regarded a simple being, is already very compound. Let us thus rest with what observation offers us, if we do not want to substitute the majestic silence of Nature with regards to great principals with our dreams of our imagination.[3]

3 Haüy's Great Geometrical Studies

3.1 The Reduction of Real Properties

René–Just Haüy, originally a botanist, comes to mineralogy thanks to one of Daubenton's classes. He wants to extend to crystals Linné's classification which distinguishes the number of petals of flowers. He wants to ignore Romé de l'Isle's work, and for this reason forges the tale of a chance discovery of the cleavages of calcite. From 1881 to 1884, he multiplies his crystal research, especially that of rhombohedral cleavages (which can only be done within the limits of what is visible to the eye, magnifying glass and microscope), the cubic cleavages of sea salt and the octahedral cleavages of fluorine. He expounds his first theory on their growth

[3]Romé de l'Isle (1783, I, 83). "Quant au mécanisme interne et caché de la cristallisation, nous sommes encore bien éloignés de pouvoir en rendre compte : c'est un mystère de la Nature [. . .] La raison en est simple, c'est que les molécules élémentaires et même les molécules premières intégrantes [. . .] sont [. . .] inaccessibles à nos sens; et que la plus petite parcelle de matière que nos yeux puissent apercevoir à l'aide du microscope, loin de pouvoir être regardée comme un être simple, est déjà très composée. Tenons-nous en donc à ce que l'observation nous présente, si nous ne voulons pas substituer les rêves de notre imagination au silence majestueux de la Nature sur les grands principes." (Author's parentheses and italic style).

(by successive strips). In 1884, he publishes his *Essai d'une théorie de la structure des cristaux* (Haüy 1784). In this text, he shows that he has multiplied geometric measures carried out with the Carangeot's goniometer, indicates that it is not for him to be concerned with what is crystallisation, but only to make measurements and so infer their structure. Thanks to faces, cleavages, their measurement, geometrical studies of the inclusions and ridges when the crystal does not cleave, Haüy comes to consider this structure as being made up of three-dimensionally stacked constituent molecules. The variety of the faces and their inclination is explained by the steps, the decrements, in this stacking. The constituting molecule which stacks possesses what Haüy calls a simple form; and the solids formed by the same steps secondary forms. Notwithstanding this precision, there is not a great deal of new matter in this text compared to the deductions of Romé, who is still not cited by Haüy. There is a commonality between the integrating molecules of one and the constituting molecules of the other. Haüy does however notice that these measurements are slightly different from those he was expecting from the decrements.

> Given a crystal, to determine the precise form of its constituting molecules, their respective organisation, and the laws that follow the variations of the lamina of which it is composed.[4]

In the extremely vast field of study of crystals, too difficult to treat, Haüy operates a reduction of tangible properties: he selects a property, the constancy of angles, which allows him to deduct the constituting molecule. This is not inevitably a reality in itself, but renders possible the implementation of a geometrization which accounts for numerical data relative to the chosen property. The constituent molecule is thus not only figurative, and bordering on trivial, but it plays a selecting role in that it separates the "pertinent" from the "non-pertinent." Haüy adds an important sentence concerning the classification of crystals, which shows to what extent he considers his approach to be reductionist:

> We will certainly never be able to use Crystallography as the basis of a methodical division of minerals [. . .]. Forms can [. . .] only be used in a subsidiary manner, and as secondary traits [. . .].[5]

In order to ensure the influence of his research, Haüy astutely approaches both the Académie des Sciences, of whom he is an elected member as botanist, and Laplace and his followers. The latter see with surprise and joy geometry enter into the realm of natural sciences.

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[4] Haüy (1784, 25). "Etant donné un cristal, déterminer la forme précise de ses molécules constituantes, leur arrangement respectif, et les lois que suivent les variations des lames dont il est compose".

[5] Haüy (1784, 5).

3.2 Haüy's Theory of Structure of 1792: A Step Towards Generalization

In the 1792 work *l'Exposition abrégée de la théorie sur la structure des cristaux*, Haüy describes the mechanical divisions and observations which allow the distinction of primitive forms and the multiple secondary forms of each crystal studied (Haüy 1792). It is this diversity which Haüy wants to scale down by the unity of the theory. As crystals cannot be divided to infinity, the physics of Laplace postulates that we inevitably reach a limit beyond which we would reach such small particles that they can be no further divided without analysis, that is, without destroying the nature of their substance. This limit is that of integrant molecules – Haüy takes up the term used by Romé de l'Isle, deceased in 1790, still without citing him: their division would give their constituting chemical molecules.

The diverse secondary forms result from the three-dimensional organisation of identical integrant molecules within a same structure. Haüy puts forward an idea showing that the rhombohedral dodecahedron can be considered as the stacking of cubes whose faces are determined by the same law of decrement. His reasoning now applies to geometry in space, but taken "layer by layer". These layers always form invisible steps. For each mineral, a law of decrement and an integrant molecule must be extracted, necessitating observations, measurements, calculations, forecasts and verifications. Through trigonometric calculations and measurements made with the goniometer, Haüy manages to illustrate that his elegant concept explains diverse secondary forms, idealised in the same way as Romé de l'Isle – still not cited. The stacking of successive layers is an awkward operation. Once it is proven that it is thus possible to account for the multiplicity of secondary forms, one might as well proceed in the opposite way as did Romé de l'Isle: the inclinations of faces are known, as is the integrant molecule, Haüy shows that the faces can be geometrically considered as planes intersecting the angles or edges of the integrant molecules (they are produced by decrements on the angles, decrements on the edges or mixed decrements) a further step towards a desirable unity. It remains to define for all known crystals the possible primitive forms. It is not a simple problem. Haüy wanted to achieve a great simplicity. He manages to reduce to six the number of categories : the parallelepiped generally is made up of the cube, the rhombohedron and all solids complete with six parallel faces two by two; the regular tetrahedron, the triangular faced octahedron, the hexagonal prism, the rhombohedral dodecahedron and the dodecahedron with isosceles triangular faces. Romé de l'Isle had already distinguished in his crystallography six types of integrant molecule structures. Haüy includes some more (in general the parallelepiped), and also distinguishes the rhombohedral dodecahedron and the hexagonal prism (Fig. 2).

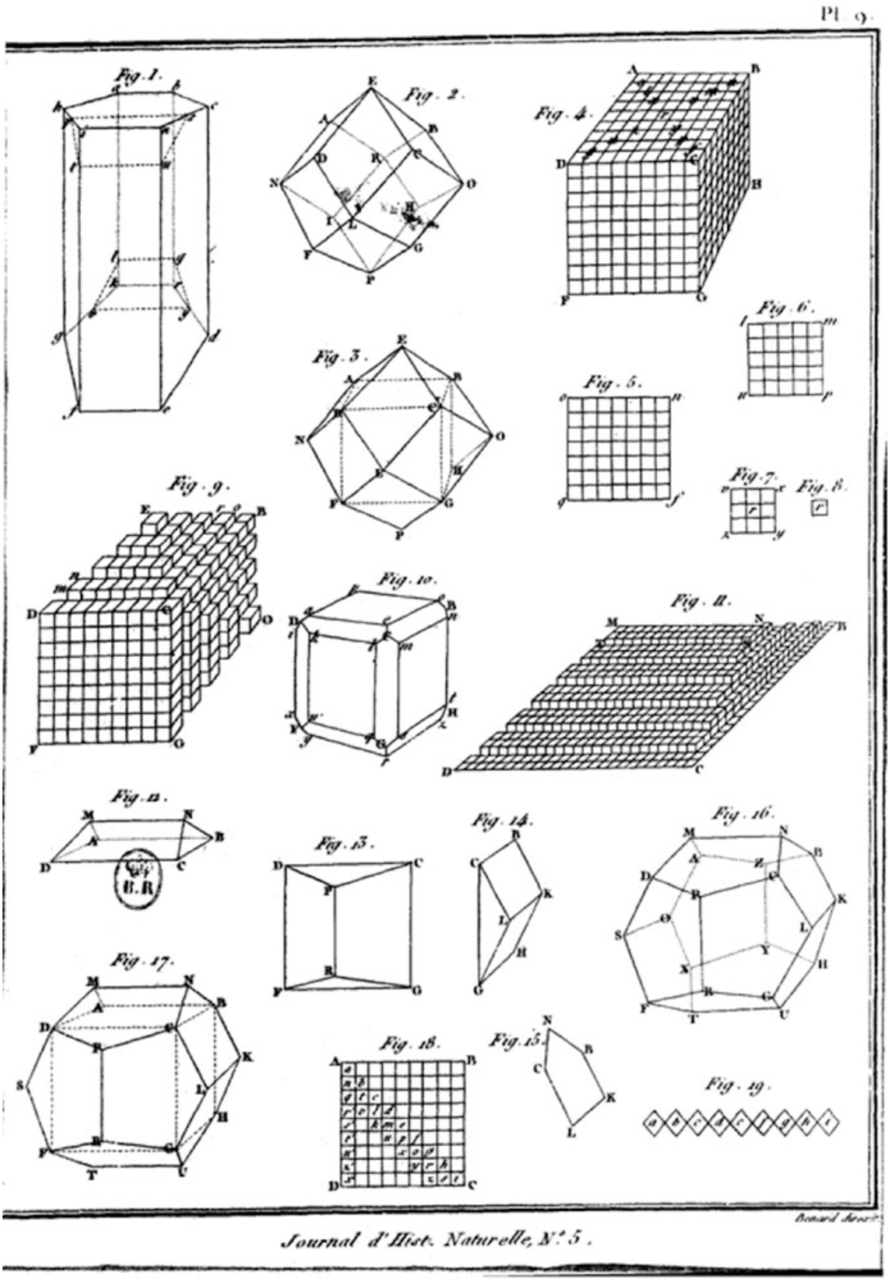

Fig. 2 A plate from Haüy's theory of structure[6]

[6](Haüy 1792, pl 9).

The most delicate point of the theory is elsewhere: by limiting the theory to the division (by thought) of each crystal in six parallel sections two by two, we always obtain a parallelepiped nucleus, which is often the integrant molecule. But certain minerals have a parallelepiped that is divisible, as is the rest of the crystal, by further (real) sections made in different directions to its faces. In this way, fluorine cubes can thus be divided diagonally: in this way we extract an octahedron and 12 tetrahedra without ever being able to reduce the result of the division to unity. The geometric ideal is still marked by the weight of material facts. And Haüy continues his search for beauty:

> By so adopting the tetrahedra in dubious cases, [...] we would generally reduce all integrant molecular forms to three remarkably simple forms, that is the parallelepiped which is the simplest of solids with two by two parallel faces, the triangular prism which is the simplest of prisms, and the tetrahedra which is the simplest of pyramids [...]. As for the rest, I would refrain from furthering any pronouncements on this subject [...] new research remains to be made, to advance further towards the primitive laws under which the Creator has governed crystallization, and which are themselves none other than the immediate effects of his supreme will.[7]

3.3 *The Traité de Mineralogy of 1801*

The precise measurements which Haüy carried out on all the physical properties of crystals and his relations with Laplace lead to his selection, despite him being both a naturalist and quite weak in mathematics, to give the physics lectures at the *École Normale* of year III (Guyon et al. 2006, 56–91). He devotes several lectures to crystallography and demonstrates how he has enriched his theory thanks to numerous studies carried out on particular minerals. He focussed on the double refraction of calcite (1788 and 1792), quartz (1792), other minerals (1793), read Christiaan Huygens and Isaac Newton on this subject, showed that in the conflict

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[7] Haüy (1792, 48). "En adoptant donc le tétraèdre, dans les cas douteux [...] on réduirait en général toutes les formes de molécules intégrantes à trois formes remarquables par leur simplicité, savoir le parallélépipède qui est le plus simple des solides dont les faces sont parallèles deux à deux, le prisme triangulaire qui est le plus simple des prismes, et le tétraèdre qui est la plus simple des pyramides [...]. Au reste, je m'abstiendrai de prononcer sur ce sujet... il resterait de nouvelles recherches à faire, pour remonter encore de quelques pas vers les lois primitives auxquelles le Créateur a soumis la cristallisation, et qui ne sont elles-mêmes autre chose que les effets immédiats de sa volonté *suprême*".

on the measurement of birefringence and calcite it is Huygens who is right. In this lecture, Haüy pays a great homage to Romé de l'Isle:

> [. . .] in a word, his crystallography is the fruit of an immense work in terms of its breadth, almost entirely new for this subject, and extremely precious in terms of its use [. . .].[8]

This is the first recognition of the Romé de l'Isle's merit, which is to be repeated textually in the *Traité de minéralogie* of 1801, in which he further defines his theory of structure.

The *Traité de minéralogie* is Haüy's great work, and even if on many points he is to later correct and develop certain proposals, we can feel the final achievements of the efforts of a man whose theory has reached maturity (Haüy 1801, I). He plans to describe the properties of all minerals and to classify them. Physics, chemistry and geometry contribute together towards mineralogical analysis. The division and borderlines between these three sciences is habitually unclear, they must

> [. . .] advance hand in hand . . . and walk [. . .] along a same line [. . .] the determination of species belongs to chemistry but geometry should be brought to the fore to approach all known minerals along the same viewpoint in order to compare them, study their traits, and question one by one the experiment and the theory on the different phenomena to which they are subject.[9]

We are far from the assertion contained in the Essay:

> [. . .] never shall we be able to use crystallography as the basis of a methodical distribution method of minerals.[10]

The thesis mixes and decrees different types of classifications, in which all known minerals are organised.

Haüy clearly also uses two different approaches to his crystallographic studies. The first with the help of marvellous tables using the projection methods of Monge: this approach illustrates the physical theory of the layers of decrement that Haüy renders perfectly clear, notably using rhombohedral dodecahedron and calcite scalenohedrons (Fig. 3).

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[8]Haüy (1795), an III in Guyon et al., 58. "[. . .] en un mot, sa cristallographie est le fruit d'un travail immense par son étendue, presque entièrement neuf par son objet, et très précieuse par son utilité".

[9]Haüy (1801, I, 22). "[. . .] se prêter la main [. . .] et marcher [. . .] sur une même ligne [. . .] c'est à la chimie qu'il appartient la détermination des espèces mais que la géométrie [. . .] soit associé à la balance pour rapprocher tous les minéraux connus sous un même point de vue, pour les comparer entre eux, étudier leurs caractères, et interroger tour à tour l'expérience et la théorie sur les différents phénomènes dont ils sont susceptible."

[10]Haüy (1784, 5). "[. . .] jamais on ne pourra faire de la cristallographie la base d'une distribution méthodique des minéraux."

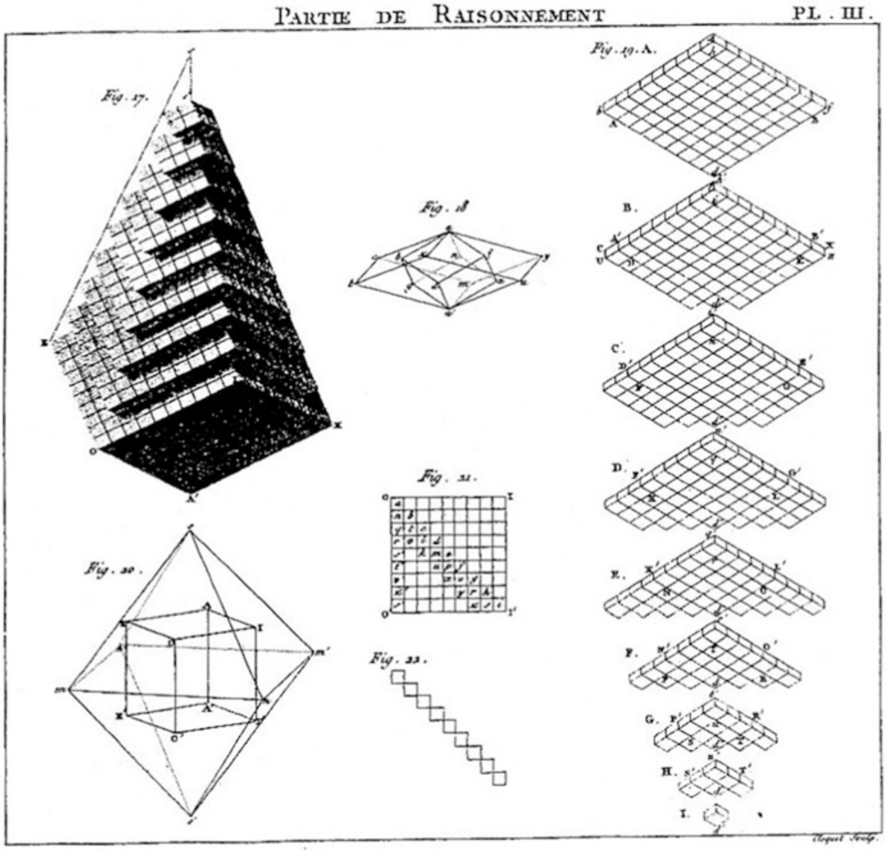

Fig. 3 A plate from development of a scalenohedron by decretement of two rows of integrant molecules on the edges (1801) (Haüy 1801, V, Atlas, pl 3, Fig 17)

With the second approach he notably shows how geometry allows the prediction and naming of secondary forms once the integrant molecule and the base parallelepiped are determined. From here he goes on, via successive truncations on the edges and the vertices and demonstrates that we can recognize form. This geometrical part sumptuously develops a mathematical analysis of crystals: a crystal is always composed of one of six possible primitive forms, which are reduced to three types of integrant molecules. These are parallelepipeds or can be grouped as such. Haüy calls these parallelepipeds substrate molecules. It is

> [...] a kind of unity to which one can bring the structure of all crystals in general, in such a way as to allow us to uphold the data provided, in the application of calculations to all possible crystalline forms.[11]

[11] Haüy (1801, I, 52). "[...] une espèce d'unité à laquelle on peut ramener la structure de tous les cristaux en général, en sorte que l'on est libre de s'en tenir aux données qu'elle fournit, dans l'application du calcul, à toutes les formes cristallines possibles."

From the ideals of parallelepipeds, Haüy can surmise them as intersected on the edges, the vertices or in an intermediary manner and so represent the decrements leading to the formation of faces, giving the resulting secondary forms and predict and name the angles. Haüy can only carry out this work after having himself mentally visualized all the consequences of the laws of decrement, so bringing the reader step by step to geometric reasoning and its analytical translation. Haüy develops his ideas over 240 pages of dense study, calling on numerous demonstrations of plane trigonometry. He is to become, as Cuvier is to say, the legislator of mineralogy, a legislator of whose reasoning the naturalists of his day will never be able to follow, but will enthral physicians and geometers. I am unable, within the framework of this study, to detail even merely the crystallographic section of the *Traité de minéralogie*: I prefer to centre on this work and those who followed it to show the beauty of its doctrine, but also that it was not at all finished and that Haüy will go on to improve it.

3.4 The Final Studies: A New Classification, the Law of Symmetry

Berthollet had demonstrated that the chemical compositions of crystals could not be identical to that of their integrant molecules. Haüy accepts this criticism and changes his position with regards to the classification of crystals. In his final work, the *Traité de Cristallographie* (1822), he writes that "[. . .] it is only in Geometry that all crystals are pure."[12] He adds:

> I can prove [. . .] that we owe the distinction of types to Crystallography rather than to chemistry, and I base this principally on the fact that the essential condition, which demands that the type be represented, can only be fulfilled by the science which depicts minerals as nature has produced them, and not the science which has revealed them with the aid of an operation which removes their characteristic traits.[13]

On the other hand Wollaston demonstrated with the help of a precise goniometer, which Haüy refuses to use as it is the invention of an Englishman that the measurements of the angles of crystals differ from the calculations obtained from

[12] Haüy (1822, I, xlix). "[. . .] il n'y a que la Géométrie pour laquelle tous les minéraux soient purs."

[13] Haüy (1815, I, 1). "Je prouve [. . .] que c'est à la Cristallographie plutôt qu'à la chimie qu'appartient la distinction des espèces, et je me fonde principalement sur ce que la condition essentielle, qui exige que l'espèce soit représentée, ne peut être remplie que par celle des deux sciences qui nous dépeint les minéraux tels que les a produits la nature, et non celle qui ne nous les fait connaître qu'à l'aide d'une opération dont les résultats ont effacé leurs traits caractéristiques."

Haüy's theory of structure, sometimes by as much as 20°. Haüy justifies these differences by means of an interesting epistemological theory:

> The mechanical measurements of the angles between the faces of crystals [. . .] cannot be rigorous [. . .] the resulting reports [. . .] are represented by large numbers, the use of which would harm the elegance of the theory and make it less easy to handle. I demonstrate how I come to deduct from these approximate and complicated reports a limit recognisable through its simplicity and in which resides very probably the only real report, that of Nature.[14]

Haüy bends the experience to the needs of the theory, a position which he expresses in this way:

> We recognize here what is generally characteristic of the laws generated by the power and wisdom of God who created and governs. Economy and simplicity in the means, richness and limitless generative capacity of the results.[15]

Such sentences constantly appear in Haüy's writing and represent his profound conviction. But this subjugation of experiment is scarcely appreciated – as we shall see – by his contemporaries, above all by foreigners.

Haüy will find an opportunity to further approach the desired perfection via another means. In the *Traité* of 1801, he had classified minerals according to their substrate parallelepipeds (which he does not enumerate), and had proposed a notation of faces according to the mental truncations made on the vertices and edges. Generalising this method of notation of faces to all the minerals of which he has knowledge, in 1815 he proposes his law of symmetry, which allows the deduction of all the faces of all forms of crystals.

> If the edge of a primitive parallelepiped undergoes a certain decrement or remains equal, the other analogous edges will undergo the same decrement or will remain identical. The same applies to angles [Fig. 4].[16]

[14]Haüy (1822, I, vj). "[. . .] les mesures mécaniques des angles que font entre elles les faces [. . .] ne peuvent être rigoureuses [. . .] les rapports qui en dérivent [. . .] seront représentés par de grands nombres, dont l'usage nuira à l'élégance de la théorie et la rendra moins maniable. Je fais voir comment je suis arrivé à déduire de ces rapports approximatifs et compliqués, une limite qui se reconnaît à sa simplicité et dans laquelle réside très probablement le véritable rapport, qui est celui de la nature."

[15]Haüy (1822, I, ix). "On reconnaît ici ce qui caractérise en général les lois émanées de la puissance et de la sagesse du Dieu qui l'a créée et qui la dirige. Economie et simplicité dans les moyens, richesse et fécondité inépuisables dans les resultants".

[16]Haüy (1801, I, ij). "Si le bord d'un parallélépipède primitif subit un certain décroissement ou reste identique à lui même, les autres bords analogues subiront le même décroissement ou resteront identiques. Il en est de même pour les angles."

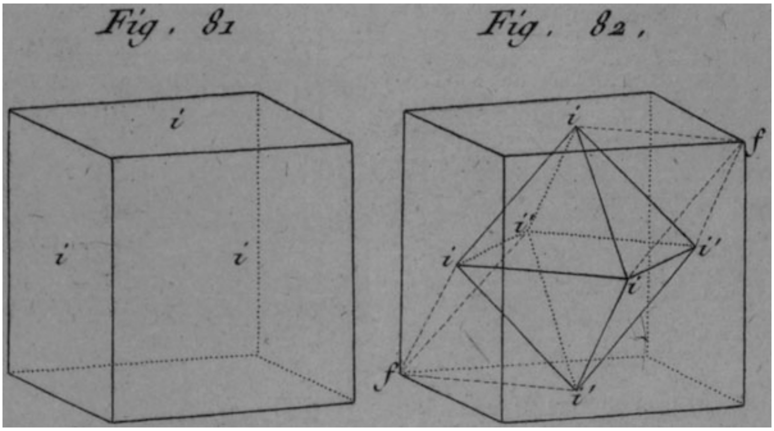

Fig. 4 The law of symmetry: If an angle of a primitive cube undergoes a certain décrément, the other analogous angles will undergo the same decrement (1801) (Haüy 1801, Atlas, pl 3, Figs 81, 82)

But, as early as 1815, Haüy accepts that there are a few exceptions to his law: certain forms presented by cobaltite – tetrahedrons – can be deducted from a cube by the decrement of one out of two vertices; certain dodecahedral crystals from pyrite can be found from the reduction of one out of two edges of a cube. We use the term "mériédriques" (hemihedral or tetrahedral) to describe these forms (Fig. 5).

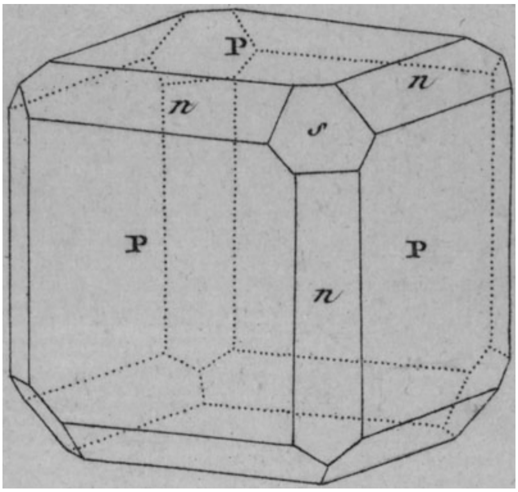

Fig. 5 The marvellous exceptions (1801) (Haüy 1801, Atlas, pl 7. Haüy accepts that there are a few exceptions to his law: certain forms presented – tetrahedrons – can be deducted from a cube by the decrement of one out of two vertices)

Haüy recognizes this weakness. Basing his argument on the fact that the cause of crystallization is unknown, he justifies his rare and marvellous exceptions, by particular circumstances which would have deviated crystallization from the route it would have otherwise taken if left to its own devices. It is these same circumstances that produce, so he thinks, pyro-electricity of tourmaline: the distortion of the configuration of its different constituting parts. But is it not a defect in Haüy's theory which reveals all these exceptions? These are relative to the law of symmetry, and to the law of rational reduction, whose measures do not account for the theory's forecasts? It is precisely this that the German crystallographer Weiss believes, translating in 1804 the *Traité de Minéralogie*, and including a foreword with a large introduction advancing another conception of crystals, conforms to Naturphilosophie and solving these problems.

4 Crystals and the Naturphilosophie Movement

In parallel, and against the "molecular" conception of matter, formed of small individual masses, another stream of thought had always been asserted, more or less strongly affiliated to stoical or Pythagorean ideas, describing matter divisible to infinity. This current of thought had been depicted in the seventeenth and eighteenth centuries by Leibniz, Swedenborg, Boscovich and Kant. Without entering into detail, let us say that, for these intellectuals, matter is continuous and that they attach a great importance to the description of attractive or repulsive forces, exercised at purely mathematical points. For them, the cohesion of matter is the result of the dynamic equilibrium of the actions of these forces. For Boscovich, for example, Leibniz's monad becomes the centre of attractive and repulsive forces which fill a finite space, not because of the plurality of its parts, but due to its links with other monads: two points are destined to either move towards or apart from each other, it is this same determination that we call force, understanding by this not its mode of action, but the determination itself, its origin, the extent of which changes with the change of distances. When the distance between the points goes beyond a certain limit, the force is attractive; it becomes repulsive within this limit. For Kant ([1785] 1883), matter does not only occupy space, but it fills it, exercising a resistance to all other matter that would tend to occupy it (Kant). It must in this case exercise a repulsive force. If it existed alone, matter would disappear into space. An attractive force must thus – beyond a certain distance – balance the repulsive force, in order to put an end to the dissipation. The attractive force cannot exist alone either: matter would then be reduced to a point. To consider that the forces of attraction and repulsion are exercised between corpuscles is tantamount to shifting the problem without solving it: indeed, we can so ask ourselves how the corpuscle itself fills space. Kant infers from this reasoning that matter is continuous. We cannot say that it is granted with two forces, but that we construct it with two forces, that it is none other than the mutual limitation of attraction and repulsion. For Boscovich, attractive and repulsive forces are not isotropically distributed during crystallization. Planes thus appear in crystals. These concepts of polar matter are to influence the German stream of thought of "Naturphilosophie" and lead to the crystallographic studies of Schelling, Weiss, Mohs and Hessel, who, as we shall see, will bring out the concept of crystalline symmetry, give new shape to Haüy's laws and allow for new developments of mathematic crystallography.

For Schelling (1798), there a universal power of formation in nature which produces individual things with determined roles (von Shelling). Following Aristotelian philosophy, he judges that the idea of individuality arises from the fact that an object (or a living thing) is composed of matter and form. In a fluid, no one part can be distinguished from another by its form, it is not individualised, but if the fluid is rectified in a calm manner, and not disturbed, it crystallises, and the crystal takes on its own shape, becomes differentiated, composed of matter and form. The positive principal of all crystallisations is the same: to attain form is a first condition. It is important to study why we can observe differences in crystallisations. These differences can only be provoked by a negative principal, and the cause of this is what we should pursue. Schelling notes:

> For Haüy all the crystallisations are regarded as secondary formations arising from the different aggregations of invariant primitive forms [. . .]. This is merely a clever trick, since it can in no case be proven that such a simple form is not itself secondary [. . .].[17]

Even if Schelling cannot himself define the negative principle, he sees a first victory in the "polar" conception of matter in Davy's electro-chemical theories and Ørsted and Faraday's electromagnetic theories.

Returning to Kant and Schellings theories, Weiss (1814) takes them considerably further within the field of crystallography. In his translation of Haüy's *Traité de minéralogie*, he develops dynamic applications of crystallisation, in which he defends the polar conception of matter. In a fluid, attractive and repulsive forces are in equilibrium. Crystallisation happens when the repulsive forces become dominant; as they are not isotropic, the resulting forms are variable, depending on the directionality of the repulsive forces, possessing characteristic angles. Identical faces appear perpendicular to the directions in which the repulsive forces are identical. These are really level and not in layers as Haüy thinks. They are, as we can see, directions that are characterized by the crystalline type. It is the distribution of identical and convergent directions that must be studied. Weiss develops to this end the concept of axes of symmetry, he counts the axes compatible with crystals (let us say in the order of 1, 2, 3, 4, 6), and gives the crystallisation what we can call a "vectorial" description, concretely applying this method to calcite, and then to other crystals (Fig. 6).

[17] von Shelling (1798, 209).

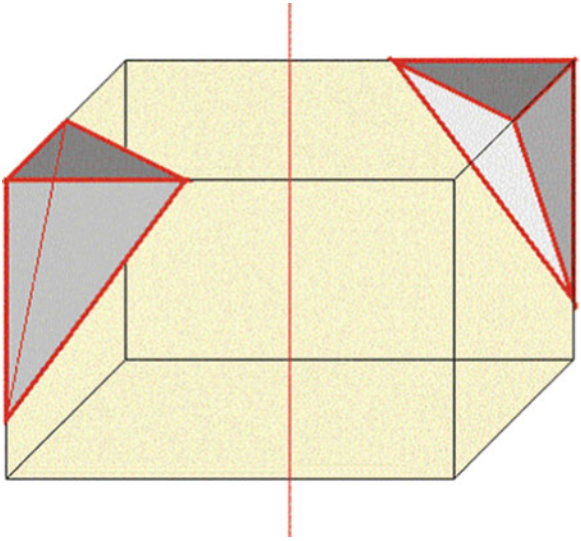

Fig. 6 Weiss: axes of symmetry (1814–1815) ([BM])

He then demonstrates that the application of axes of symmetry is not only a geometrical point of view, but is also significant in terms of physics (the faces and cleavages repeat themselves of course in relation to these axes, as does birefringence and other physical properties of each crystal). With Weiss, the notion of crystalline symmetry, implicit and confused with Haüy, becomes clearly apparent for the first time. In December 1815, Weiss writes a dissertation in which he classifies crystals in different categories, distinguished by their axes of symmetry (he defines the cubic (sphenohedral), tetragonal, hexagonal and orthorhombic categories), subdivides them in hemihedral classes and calculates the number of faces belonging to the crystals of these symmetries (we would say the degree of symmetry). He uses spherical trigonometry (and no longer plane trigonometry as does Haüy) to collate the measurements of angles between faces, and Haüy's law of decrement which he corrects (calling it the law of rational intersections). He gives a new rating of external forms – no longer descriptive like Haüy's – but based on the determined relation between the intersections of each face of a crystal with the reference axes (Weiss). Lastly, and above all, he is no longer obliged to consider "mériédries" as exceptions: they are the product of the action of axes of symmetry of inferior order to holohedral axes. Weiss commits several errors – in the classification of minerals in the different categories of symmetry and is not able to – and he makes note of this – enumerate them all. Despite all the difficulties, this research is well received in Germany as it is in symbiosis with Naturphilosophie and because it brings the strict mathematical rationalism that was lacking from Haüy's theory. In 1822, Mohs publishes a crystallographic paper in which he wants – even more than Weiss – to transform crystallography into a geometrical science. His work allows the enrichment of the number of elements of symmetry (he introduces the opposite elements of centre and mirror). He also completes the categories of symmetry distinguished by Weiss, adding those that we currently call monoclinic and triclinic. The six "crystalline systems" (seven if we distinguish hexagonal and rhombohedral) are definitively determined.

In 1830, J. F. C. Hessel returns to the studies of Weiss and Mohs. (Hessel[18]). In order to find all the subdivisions ("mériédriques") of the categories of symmetry, he has the idea of finding all the elements of symmetry compatible with crystalline polyhedra, and defines the direct and opposing axes of the orders 1, 2, 3, 4, 6. By so combining in all possible manners these convergent elements, he manages to enumerate the 32 different classes of symmetry, which can be found in the six (seven) categories (systems). All known crystals can be sorted in these classes, but certain geometrically defined classes correspond to no known crystal found in Nature. The work of mineralogists will be to "fill" the still "empty" classes: the geometrical systemisation of the study of crystalline forms is completed.

At the same time in England, W. H. Miller (1839) engages in another theoretical study: basing himself on the "law of rational truncation", he uses the rating of his compatriot Whewell to define the intersections of planes (which can be face planes or cleavages) with a system of crystalline axes. He enumerates seven possible systems, portrays these planes using the "method that the German professors, Neumann and Grassmann, invented independently one from the other (stereographic projection)",[19] introduces analysis (coordination of points and faces) to replace spherical trigonometry, uses the study in stereographic projection of the position of the pole in relation to the elements of symmetry of a crystal to explain the number of faces of each form and show the same polyhedral forma can belong to several different symmetries. Unaware of Hessel's work, he only uses those from empirically known "mériédries", but his work allows the integration of all observed forms in a same mathematical logic, and elucidates how faces repeat themselves, and gives new tools to crystallographic analysis (the rating which will allow the calculation in vectorial space, the projection which allows the portrayal and deductions of the laws of symmetry).

Hessel's deductions seem moreover to have been totally unrecognized – even in Germany – for more than half a century. It is true that in France, crystallographers had deliberately chosen, as I mentioned earlier, to ignore the work of the Germans which are based on "idealist philosophy":

> German physicists "prefer" in the study and interpretation of natural phenomena the sort of vague and obscure explanations that they call "dynamic" rather than the simple clear and positive notions that we draw from atomistic hypotheses. They reject each theory, limiting themselves to experiment and concentrate on nuances instead of accounting for phenomena, the construction of material bodies recognized by Newtonian philosophy, which seem to them to be too mechanistic and coarse as they simultaneously touch both sense and reason [. . .]."[20]

[18]The derivation of the 32 crystal classes appears in n° 89, pp. 91–124.

[19]The stereographic projection was invented by Arab mathematicians of the tenth century.

[20]Delafosse and Durozier, 579. "Les physiciens allemands "préfèrent" dans l'étude et l'interprétation des phénomènes actuels les sortes de vagues et obscures explications qu'ils

such as Haüy's students, Durozier and Delafosse (1857) (Delafosse and Durozier) explain it in their biography of him. The French reject the concept of axes of symmetry, this notion so unclear, used by German crystallographers, which could, almost, according to Delafosse (1845), take the meaning of line of molecules (Delafosse 1845). They thus prefer to move on from molecular conception of matter to resolve the difficulties encountered by Haüy.

5 Lattices and Space Groups of Symmetry

5.1 Lattices

Haüy had so strongly defended the concept of integrant molecules that his contemporaries could believe that his conception of the structure of crystals was their formation by triperiodic assemblage of these elementary solids filling space. Such a conception could not suffice to account for the physical properties of crystals. Wanting to explain them, his student Delafosse regrets, after the death of his teacher, that Haüy

> [. . .] had neglected all properties other than cleavage [and so had not] applied physical theory to his research.[21]

He notes that crystals with equal geometry can have different physical properties, returns to the notion of hemihedrism and explains facts presuming that crystals that are geometrically similar can correspond to different molecular structures. The crystalline structure would be formed by atomic polyhedrons which do not fill space, but placed on the nodes of a triperiodic lattice, defining three series of parallel planes. These planes obviously determine parallelepipeds, abstractions which can be viewed alongside Haüy's primitive parallelepipeds. Like him, Delafosse distinguishes six crystalline systems. But atomic polyhedra are not necessarily the same in shape as unit cells and can only be half or quarterly physically identical to polyhedrons characteristic of the holohedra: the principal lines of the lattice remain the same. Here we have a breakthrough, for the French, the explanation of the remarkable exceptions – hemihedrons and tetrahedrons. For Delafosse (1840),

appellent "dynamiques" plutôt que des simples, claires et positives notions que nous tirons des hypothèses atomistes. Ils rejettent chaque théorie pour se limiter eux-mêmes à l'expérience et posent des subtilités triviales à la place des représentations de phénomènes, des constructions de corps matériels qui sont admis par la philosophie newtonienne, qui leur semble trop mécaniste et trop grossière parce qu'ils parlent simultanément aux sens et à la raison [. . .]."

[21] Delafosse (1840, 394).

each crystalline system "subdivides when we consider the physical modifications which can bring about a change in form or structure of crystals".[22] Too much a physicist, he does not take up the study of the possible subdivisions: he only describes and categorises, a great task alone, those which he learns about through experiment and observation. His idea of a lattice made up of atomic polyhedra which no not fill space allow him to account for the evolution of numerous physical properties (form, cleavage, optical properties, pyroelectricity, density) with the considered direction. Judging that he does not possess enough information to be able to completely determine the material structure of crystals, Delafosse deliberately chooses to schematise it with the help of a mechanical model which enables one to account for those in which he is interested (see in the following Fig. 7).

The notions of atomic patterns in lattice networks, of unit cells introduced by Delafosse, were to be generalized in the exclusively geometric research carried out by Bravais (1848): reducing polyhedral molecules not filling space to simple points (their centres of gravity), he is able to engage in a formal mathematical study which marks history. He studies the translation lattices at one, two and three dimensions, takes up the notion of nodes, introduces those of lattice lines, of simple unit cells and demonstrates their equality of surfaces (or volumes). He enumerates four different axial systems allowing one to build plane unit cells (parallelograms), seven axes of symmetry in a three-dimensional space (parallelepiped unit cells – he distinguishes the rhombohedron from the hexagonal prism), demonstrates the incompatibility of pentagonal symmetries or symmetries greater than six with the existence of a lattice and studies the laws of equidistance of the lines or the planes. From here he deducts that in each system of possible axes we can sometimes distinguish different lattice parameters, introducing the concept of multiple unit cells, enumerating 5 modes of bi-dimensional lattices and 14 modes of three-dimensional lattices, the Bravais lattices, which he organises in the seven crystal systems (Bravais [1848] 1850) (see in the following Fig. 8).

This purely geometric study concludes with the following sentences:

> Despite the fact that the precedent Dissertation could be considered as merely a purely geometrical speculation, and that the relations that are demonstrated therein are independent of the physical properties of bodies, this work has been conducted by its author with the idea of using it in the future to explain fundamental facts of crystallography, and it is especially with this aim in mind that it has been written. It has always been accepted, since Haüy, either implicitly or explicitly, that the centres of the molecules of crystals are arranged at equal intervals, following straight lines, parallel to the intersections of cleavage. The geometrical system formed by these centres is then none other than what we have called a "Point Assemblage", and all considerations developed by this Dissertation can be applied to this. If we now accept that any kind of operation intervenes to arrange the Assemblage which forms at the point of crystallisation, a geometrical structure that

[22] Delafosse (1840, 395). "[. . .] se subdivise quand on considère les modifications physiques que peut entraîner un changement dans la forme ou la structure des cristaux."

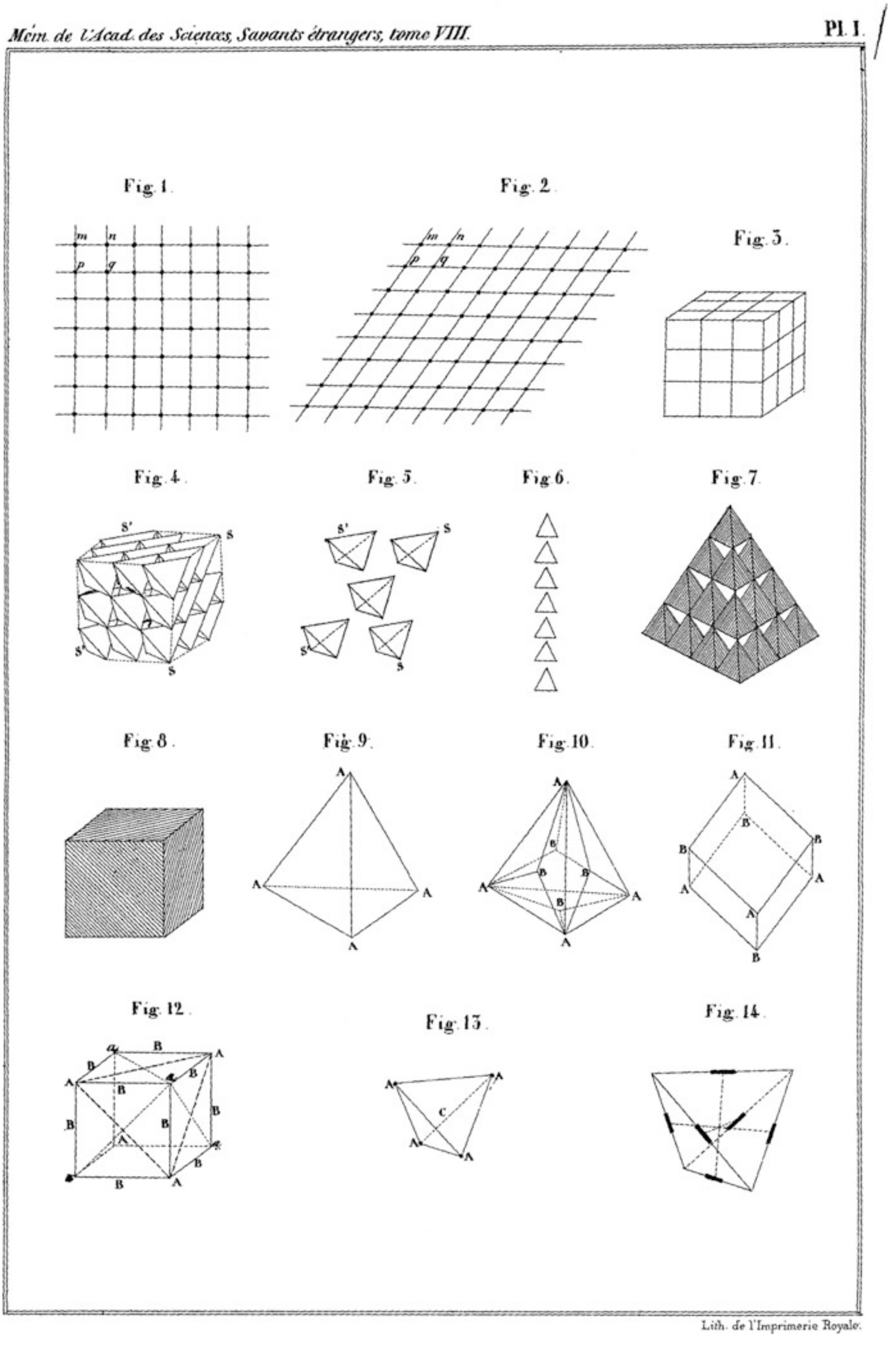

Fig. 7 The notions of lattice and unit cells introduced by Delafosse (1840) (Delafosse 1840, Pl, I)

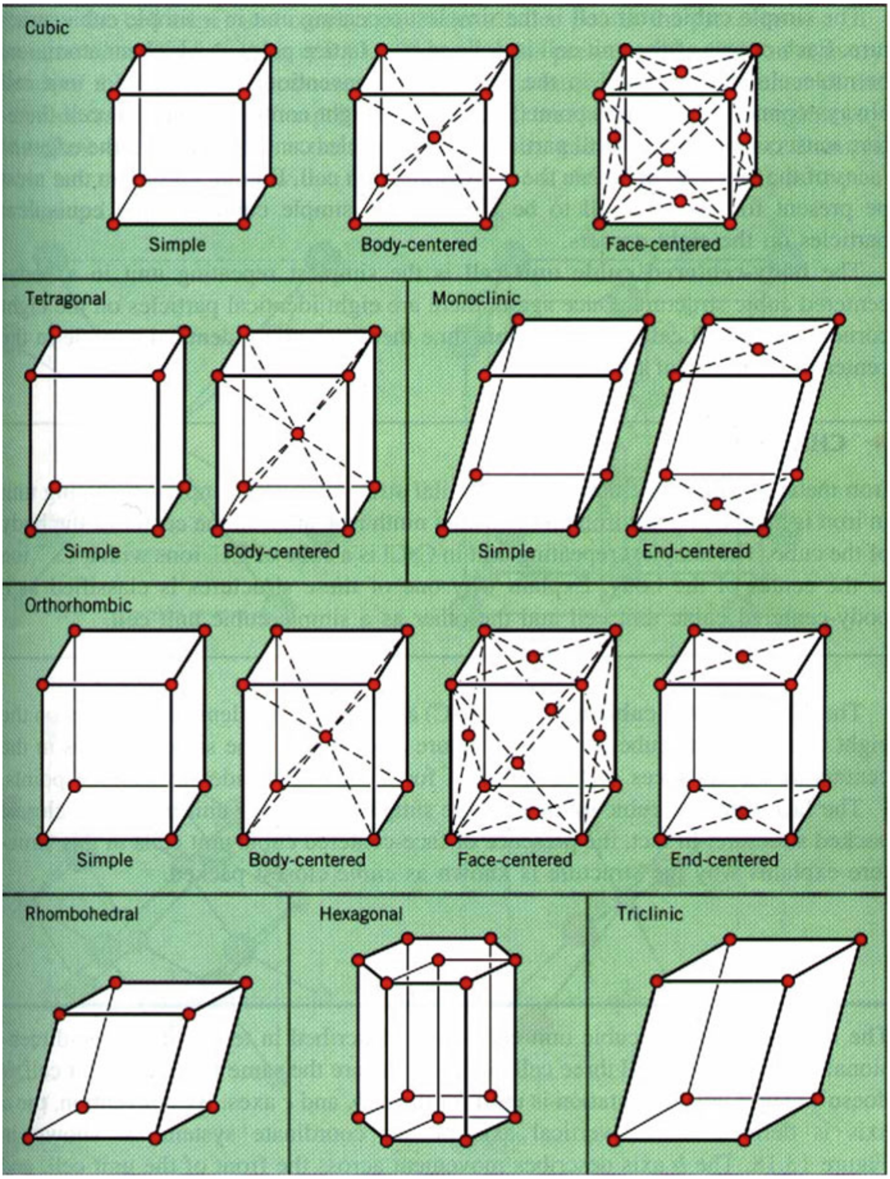

Fig. 8 Bravais' lattices (1848) (Bravais [1848] 1850, modern interpretation)

> has a non symmetrical structure, it is clear that the definitively formed Assemblage will belong to one of our seven classes [. . .]. The observation of crystallised bodies, whether natural or artificial, subsequently proves this point, such that the geometrical division of Assemblages corresponds accurately to that obtained through a patient and close study of the different crystal systems [. . .] But what is the cause of this tendency for the formation of Assemblages, which shapes the centres of molecules of crystals towards symmetrical regularity? This is what I shall attempt to explain in another Dissertation [. . .].[23]

We can see that Bravais wants to separate pure geometrical speculation from the cause which leads to the Assemblage taking on a symmetrical arrangement during crystallisation. He only estimates, prudently, that the observations of crystal bodies subsequently prove what theoretical studies reveal. Why take such precautions? Why not start from the observation to deduce theory? The phrases Bravais uses prove that he considers that crystals are really a material assemblage, that they obey the laws of mechanics (he speaks briefly in his conclusion of) are

> [. . .] polyhedral, or if you like, polyatomic form of the molecule of crystallised body as determining the type of symmetry of the corresponding crystalline Assemblage (and which) explains simply the example of phenomena of *hemihedry*.[24]

Nevertheless Bravais does not want to base his research on this fundament, he prefers to give it a purely mathematical character as protection from any risk of linking his work to mechanics, or indeed to atomism. In 1848, the recent resounding failures of Newtonian mechanics indeed incite to prudence and reinforces the stream of thought to which most scientists adhere. For them, to explain is no longer the deduction of a material reality allowing us to account for observable facts, nor is

[23]Bravais ([1848] 1850, 127, 128). "Quoique le Mémoire qui précède puisse être considéré comme l'étant d'une pure spéculation géométrique, et que les relations qui y sont démontrées soient indépendantes des propriétés physiques des corps, cependant ce travail a été exécuté, par l'auteur, avec la pensée de s'en servir ultérieurement pour l'explication des faits fondamentaux de la cristallographie, et c'est vers ce but que sa rédaction a été spécialement dirigée. Il a toujours été admis, depuis Haüy, soit implicitement, soit explicitement, que les centres de molécules des corps cristallisés sont distribués, à des intervalles égaux, suivant des séries rectilignes, parallèles aux intersections des plans de clivage. Le système géométrique formé par ces centres n'est donc rien autre chose que ce que nous avons nommé un "Assemblage de points", et toutes les considérations développées dans ce Mémoire lui sont applicables. Si maintenant l'on admet qu'une cause quelconque intervienne pour disposer l'Assemblage qui se constitue, au moment de la cristallisation, une structure géométrique qu'à une structure non symétrique, il est clair que l'Assemblage définitivement formé appartiendra à l'une des nos sept classes [. . .]. L'observation des corps cristallisés, naturels ou artificiels, prouve a posteriori qu'il en est bien ainsi; aussi la division géométrique des Assemblages correspond-elle fidèlement à celle qu'une étude patiente et attentive a porté à établir entre les différentes systèmes cristallins [. . .]. Mais quelle est la cause de cette tendance des Assemblages, que forment les centres des molécules des cristaux, vers la régularité symétrique ? C'est ce que j'essayerai d'expliquer dans un autre Mémoire."

[24]Bravais ([1848] 1850, 128). "[. . .] forme polyédrique, ou, si l'on veut, polyatomique, de la molécule au corps cristallisé comme déterminant le genre de symétrie de l'Assemblage cristallin correspondant [. . .] [et qui] explique d'une manière simple l'exemple des phénomènes de *l'hémiédrie*)".

it the use of a model to schematise a subjacent reality remaining inaccessible in its totality, but it is to state mathematically laws that are as general as possible which allow one "to predict to be able". It is only in a second work, quite separate from the theoretical study of 1848 and entitled Etudes cristallographiques (1849), that Bravais will attempt to apply concretely his lattice theory to crystals. He will then use, with more than a 30 year delay compared to German crystallographers, axes of symmetry. He combines all the axes, planes and centres of symmetry that polyhedral molecules can posses according to him (and he forgets a few), in order to obtain all the types of symmetry to which they can belong (Bravais 1850). He counts 23 of them, which he organises into seven crystal classes (systems) and can thereby explain the classes of "mériédres" as engendered by the triperiodic repetition of a polyhedral molecule whose symmetry is equal to the half or the quarter of that of the parallelepiped unit cell of the lattice and account for the existence of observed crystalline forms as well as certain physical properties (cleavages).

5.2 Space Groups

The systemization and the beauty introduced by geometrical studies in crystallography leads mineralogists to definitively adopt the classification of crystals based on symmetry, whilst crystallographers continue, for mere pleasure, to conduct theoretical mathematical studies induced by lattice theory, which appears to lead to no practical application. Bravais did not think of combining the lattice modes he had defined and the different possible symmetries of polyhedral molecules, perhaps because he had reasoned too much on points. Yet, in a same network, the symmetry generated is different if one or other point groups, having its own symmetry, is repeated by translation. It is Leonard Sohncke (1879) who was the first to express this problem and perform the combinations (Sohncke). In 1879, he so reaches the definition of 61 different space groups (Sohncke's groups) spread out amongst the 32 classes of symmetry (at last known) and in six (seven) systems.

Six years later, Nicolaï Fedorov (1885) seeks all the polyhedra which, through the juxtaposition of identical or symmetrically inverse models, fills space without any gaps and mentions another problem linked to the first: to find all the different clusters of small congruent spheres (or points) such that each sphere is surrounded by all the others in an identical or inversely symmetrical manner (Fedorov). The problem of regular divisions of space or regular point systems (or spheres) is thus posed and studied for the first time in a very general manner, but is only solved 6 years later by Fedorov (1891) and, independently from him, in the same year by Schoënflies (Schoënflies 1891). Both of them treat the problem from a general point of view, ignore shapes and only consider the symmetry of the configuration. They introduce non-convergent elements of symmetry into the lattice (gliding planes and screw-axes), which allows the discovery of translations (Fig. 9).

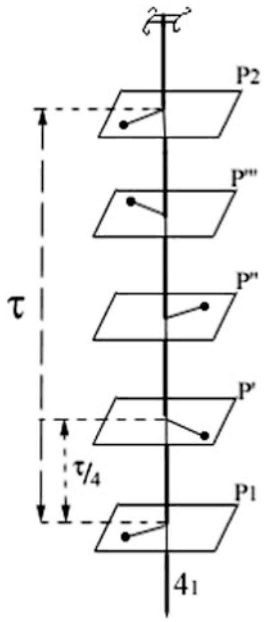

Fig. 9 Screw-axe 4-1 (A modern representation)

They so enumerate 230 different space groups (of which there are 17 planes, which it will later be discovered are all present in the Islamic decorations of the Alhambra in Grenada, dating back to the fourteenth century), integrating Sohncke's groups and organised in the 32 classes of symmetry, then into the 7 crystal systems. Their work, leading to no practical application is not noticed, no more than is the work of the Russian crystallographers Choubnikov and Belov (in the first half of the twentieth century) which, by inversing the + or − signs, count from the 230 groups, 1,651 sub-groups. The rational construction of group theory then reached the stage at which we now recognise it. It had only occasionally relied on practical considerations and had always pushed beyond the logic imagined by Huygens.

In 1894, Pierre Curie had the idea of systematically studying not only crystal symmetry, and its physical properties, but also the symmetry of physical phenomena itself, in his *Dissertation sur la symétrie dans les phénomènes physiques* (Curie). He thus opens a new field of study: the interrogations he proposes to consider will later constitute a particularly rich range of applications for group theory in physics. But beyond this technical aspect, P. Curie transforms the manner of posing problems by moving from the study of the symmetry of a phenomenon or a physical state, to that of systematic determination of the symmetry of the law. The symmetry then integrates itself in mental structure and becomes a tool of reasoning, a tool that will be used by Einstein (1905) and Poincaré (1908) and has made symmetry today a cross-border concept which ignores disciplinary divisions. In 1912, Von Laüe diffracts X rays through crystals, proving both that they are made up of atomic networks and that the latter have an electromagnetic nature. Radio-crystallography will use from 1924 this property to lead to the determination of structures. For this, it will be necessary to use Fedorov's 230 groups, the first practical application of the theoretical deductions made 20 years earlier.

6 Conclusion

With the systematic uses of groups of symmetry in several fields of science, a unique framework describes a set of various phenomena which first seemed to be totally disconnected from one another. Symmetry appears as a transborder notion and reveals a fruitful harmony between very different objects, levels, scales. This approach can be seen as a quest of perfection, a pursuit of Beauty.

Then, what a surprise it was for experimental physicists when they found, in 1984, that rules of symmetry, which we thought well established, were affected with exceptions (Schechtman). Those became a subject for researches and led to the systematic studies of "quasi-crystals". From this moment it is fruitful for science to overtake the perfection obtained while looking with appetence for "splinters of symmetry", breaks, transgressions . . . already existing in tilers of Islamic countries and built by Albrecht Dürer and Simon Stevin in their time (Maitte 1999). These open new prospects and show how the most contemporary research could benefit from the confrontation with everything connected to culture.

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Historical Reflections on the Physics Mathematics Relationship in Electromagnetic Theory

Raffaele Pisano

Abstract In this paper I present a historical inquiry on the relationship between physics and mathematics in electromagnetic theory around the nineteenth century. The investigation is within the domain of the history of physics. By essentially following Maxwell's fundamental aspects of physics mathematics in his *A Treatise on Electricity and Magnetism*, some epistemological reflections will be put forth, as well as observations regarding the different scientific approaches between Faraday's *Experimental Researches in Electricity* and Maxwell's science.

1 Physics Mathematics from a Physical Standpoint

1.1 Maxwell's Debate with Faraday

Generally, complete biographical and scientific sketches of Faraday and Maxwell are well documented.[1] Thus, for the sake of brevity, here I avoid discourse on their biographical accounts. I will rather comment on some chapters of *A Treatise on*

[1]Mainly: Everitt (Everitt), Pearce (Pearce), Williams (Williams), Agassi (1971, 2008), Arianrhod (Arianrhod), Mahon (Mahon), Russel (Russel), Harman (1990, 1998, 2004), Hamilton (2002, 2004), Gooding (Gooding), Gladstone (Gladstone), Meurig (Meurig), Bence (Bence), Tyndall (Tyndall), Baggott (Baggott), Cantor (Cantor), Glazebrook (Glazebrook), Heaviside (Heaviside), Hirshfeld (Hirshfeld), Thompson (Thompson), Tolstoy (Tolstoy), Heilbron (Heilbron), Darrigol (2000). Particulary James' studies (James) on Faraday's *correspondence* are indispensable.

R. Pisano (✉)
Centre François Viète Épistémologie, Historie des Sciences et des Technique,
University of Nantes, France [at that time. Currently: Centre Sciences, Sociétés, Cultures dans leurs évolutions (Scité), University of Lille 1, France]

Research Centre for the Theory and History of Science, Department of Philosophy,
University of West Bohemia in Pilsen, Czech Republic
e-mail: pisanoraffaele@iol.it

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Electricity and Magnetism that are relevant to the aim of this paper, as well as on Faraday's different scientific approach in *Experimental Researches in Electricity.* I will also include some historical–epistemological reflections within physics mathematics.

> I also found that several of the most fertile methods of research discovered by mathematicians could be much better in terms of ideas derived from Faraday than in their original form.[2]

1.2 On Modelling and Processes of Reasoning

In two volumes[3] of *A Treatise on Electricity and Magnetism* (Maxwell 1873) Maxwell's main cultural focus was to propose a new view of electromagnetism and the natural world. The book was also intended to be used as a Cambridge text for students taking the *Tripos examinations*. Maxwell's new thought is most evident in the final section of *A Treatise on Electricity and Magnetism* (Maxwell 1873, II, Pt IV). The physics mathematics aspect starts with his use of mathematics, e.g., the role played by *identity,* such as an equation valid for all values of the variables. We know that a similar aspect in physics is not possible; results in physics depend on measurements and their correspondence to phenomena. According to Simpson (2005, 8–10) the two sides of this identity have been used to propose alternate views of physical reality, that is, an attempt to move the physics to physics mathematics is also represented by vector and scalar concepts. In mathematics, *identities* express the same value in very different ways; in prose, we have *figures of speech* to say the same thing in different ways (*Ivi*). Thus, rhetorical issues in mathematics are essentially *mathematical figures of speech*. When these figures inspire major turns of thought, we can refer to them as *figures of thought* (Simpson 2005, 1–32). Maxwell's diagrams[4] express a cross-section of the electrostatic field of two unequally charged spheres. The electric field is static in the diagram and could be the image of an elastic medium under strain. The passage to a kinetic field (Maxwell 1873, II, Pt IV) is an evident expression of development of mathematical inquiry in physics. Thus the relationship between physics and mathematics in terms of dialectic would be expressed by figures, as well. In this sense, geometry (one of Maxwell's greatest interests) plays an important role in this dialectic. In fact, geometric figures are crucial counterparts to the analytical argument in *A Treatise on Electricity and Magnetism.* For example, part III (Maxwell 1873, II, Pt III) is mostly centered on the magnetic shell as a mathematical physical figure. The same is true for the diagrams.

[2] Maxwell (1873, I, Preface, xi, line 6).

[3] *A Treatise on Electricity and Magnetism* had three editions in 1873, 1881 and 1891. Only the first and part of the second was edited by Maxwell. Since Maxwell's theory is a pure physics mathematics field (D'Agostino) all editions lack a dichotomy between electric charge and field. These topics will be part of post-Maxwell theories (Larmor 1891, 1892).

[4] E.g., see: Maxwell (1873, I, plate I, plate V, II, 143). See also *Ivi*, 145, 403.

The figure of a shell begins as purely mathematical and in due course becomes a physical quantity. When we envision it, it is painted with a polar medium whose density at any point represents a magnetic strength and we think in terms of a modelling action-at-a-distance theory, with the assumption that this magnetic shell will exert a force on an imagined unit magnetic pole placed at any distance. A potential can therefore be associated with any point by measuring the work required to bring the unit pole to that point, e.g., Gauss's theorem, Green's theorem, Stokes' theorem. Gauss's theorem transforms the physical idea of a shell from a mere summation of parts (as, e.g., indicated by the surface integral in Green's theorem) into an intact whole, represented in its integrity by the solid angle subtended by its boundary. Stokes's theorem works on shell and boundary (Simpson 2005, 15). We also may refer to Maxwell's interest in this subject as a holistic aspect of the potential instead of the complex, detailed action of individual forces.

For both volumes of *A Treatise on Electricity and Magnetism* Maxwell draws mathematical images, diagrams and visual "Electromagnetic instruments" (Maxwell 1873, II, Pt IV, chaps XV) to better explain physical situations that mathematics, solely, sometimes cannot explain. They also refer to illustrations of a more general and mathematical character (e.g., Maxwell 1873, II, Pt IV, chap XVI) compared to, e.g., the previous physical system Maxwell presented (e.g., Maxwell 1873, II, Pt IV, chap XIV). Thus, they help the discourse (especially physical mental models, though experiment) within the physics mathematics domain to clarify the basic physical[5] and complex structure of a natural phenomenon, e.g., the pattern of a field. For example, Ørsted's effect shows (Maxwell 1873, II, 143) a complete electric circuit and the magnetic directionality of the entire surrounding space. The direction of an electric current is generally thought of as passing from a positive electrode to a negative. In order to provide a better physical idea of the phenomenon, Maxwell included a voltaic source. Solid arrows show the direction of current flow in his diagram. The figure represents "[. . .] the same relation between a circular current and the magnetic effect it surrounds" (Simpson, 19, line 3). It should be noted that most of the physical devices and ideas also represented as figures, were born in Faraday's laboratory (Faraday 1839–1855, III, plate II). Generally, the figures contained in *A Treatise on Electricity and Magnetism* are not mere pictures but precise mathematical figures, which are most certainly addressed to the mind to simplify a physical–electrodynamical model (Buchwald). It is interesting to understand the range of Maxwell's figures by comparing some of them in *A Treatise on Electricity and Magnetism*, which, for the sake of brevity, I do not discuss here. Nevertheless, this kind of investigation may refer to

> [. . .] a mathematical construction, but may equally refer to the image of a physical object produced in the laboratory. [Therefore in some figures] [. . .] mathematical construction and physical image coincide [. . .]. They are examples of what Maxwell calls *eye-knowledge*.[6]

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[5] Of course, in order to have a larger view of the behavior of the relationship from other standpoints, one should also study (with respect to previous scientific theories) *L'espace physique entre mathématiques et philosophie* (Szczeciniarz 2006; see also *Id*., 2008).

[6] Simpson (2005, 21–22, line 17).

Finally, the relationship between physics and mathematics (and geometry) is created by figures, as well. Another important aspect related to the physics mathematics in Maxwell's work is the role played by *analogies*, which abound in the *Treatise*. Maxwell presented early ideas on the use of physical analogies in science in his *On Physical Lines of Forces* (Maxwell 1855–1856; 1861–1862) where he specified that:

> The present state of electrical seems peculiarly unfavourable to speculation. [. . .]; some part of the mathematical theory of magnetism are established, while in other parts the experimental data are wanting; [. . .]. Such a theory must accurately satisfy those laws [on electrical theory], the mathematical form of which is known, and must afford the means of calculating the effects in the limiting cases where the known formulae are inapplicable. [. . .]. The first process therefore is the effectual of study of the science, must be one of simplification and reduction of the results [. . .]. The results of this simplification may take the form of a purely mathematical formula or of a physical hypothesis.[7]

> In order to obtain physical ideas without adopting a physical theory we must make ourselves familiar with the existence of physical analogies. By a physical analogy I mean that partial similarity between the laws of one science and those of another which makes each of them illustrate the other. Thus all mathematical sciences are founded on relations between physical laws and laws of numbers, so that the aim of exact science is to reduce the problems of nature to the determination of quantities by operations with numbers.[8] [. . .].

> It is by the use of analogies [. . .] that I have attempted to bring before the mind, in a convenient and manageable form, those mathematical ideas which are necessary to the study of the phenomena of electricity. The methods are generally those suggested by the *process of reasoning* [my emphasis] which are found in the researches of Faraday*, [. . .] are very generally supposed to be of an indefinite and unmathematical character, when compared with those employed by the professed mathematicians.[9]

The concept of *analogy,* which he called "processes of reasoning" (Maxwell 1856, 157, line 29), is at the foundation of many of much of Maxwell's reasonings and figures.[10] In this case, the link between mathematics and physics is amplified. In fact, in physics mathematics, e.g., we may refer to physical analogies in which processes of measurements or foundations (Beth; Lindsday) or formulations in one physical domain, such as that of mechanics, optics or fluids, may be compared to corresponding processes of measurements in another domain, such as electricity or magnetism, and establish relationships within each domain. This is one of the main aims of the history of foundations and historical epistemology of science for shared knowledge. For instance, one can also consider the relationships between charged bodies and elastic bodies under strain; or, electrostatic phenomena (Coulomb) in terms of an elastic medium. From a physics mathematics point of view, Maxwell used these processes or reasoning to discuss the use of the infinite in his mathematics. He tried to incorporate an elastic continuum as the seat of potential energy of the electric charge in the completed account of the electromagnetic field.

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[7]Maxwell ([read 1855 and 1856] 1855) 155, line 1.

[8]*Ivi*, 157, line 7.

[9]*Ivi*, 157, line 29. (Author's symbol).

[10]See also Maxwell (1860).

> Maxwell's methodology had more original components. He developed the classification of quantities as short-cut through the method of formal analogies.[11]

Particularly, if we delve into the depths of the historical foundations of science, we can see an interesting analogy between electric theory and mechanics as the following table concisely demonstrates:

Table 1 Mechanics and electricity . Some general analogies among fundamental magnitudes of the theories[12]

(Lazare Carnot's) mechanics[13]	Electric theory
Quantity of motion Ip_i	Electric charge Q
Flux of quantity of motion Ip_i	Flux of electriccharge of current I_Q
Velocity v_i	Electric potential V
Viscosity η	Electric conductance σ
Mechanical resistance R_p	Electric resistance R_{Qr}
Mass of a body, m	Capacity of a charged body, C
Mechanical inductance $1/k$ (k of a spring)	Electric inductance I

E.g., *how to explain analogies with attraction and repulsion* (see also Fufay)?

> In a philosophical point of view, moreover, it is exceedingly important that two methods should be compared [. . .] while at the same time the fundamental conceptions of what actually takes place, as well as most of the secondary conceptions of the quantities concerned, are radically different.[14]

In general one can claim:

Mechanical actions decrease the potential (electrodynamics)	Pressing by fluids on walls (hydrodynamics)

Particularly among theories:

Refraction of light	Reflection of trajectories of particles in a force field
Light propagation	Propagation of vibrations in an elastic body
Heat propagation in a uniform body	Action at-a-distance

An analogy between fluid flow and a magnetic field seems appropriate since flow is somehow involved in the electric current that produces the magnetic field. We can also think of the *current* in the Voltaic pile and the *heat* in a heat machine (Pisano 2004; Gillispie and Pisano 2012). Maxwell followed this line of thinking to include

[11] Darrigol (2000, 175, line 31).

[12] Gillispie and Pisano (2012) and related references on Drago (1988). See also Pisano (2011a, b).

[13] Carnot (1786, 1803a, b).

[14] Maxwell (1873, I, xii, line 20).

a kinetic account of magnetism in his approach to the electromagnetic theory. For the aim of this paper, we can see *On Faraday's Lines of Force* (Maxwell [read 1855 and 1856] 1855, 155–229) where Maxwell showed his intention of bypassing formal theory with analogy in order to show mathematical relations in ways that non-mathematicians could understand, an idea put forth by experimentalists like Faraday.

> [...] to have been in reality a mathematician of very high order–one from whom the mathematicians of the future may derive valuable and fertile methods.[15]

> [Maxwell to Faraday] Now as far as I know you are the first person in whom the idea of bodies acting at a distance by throwing the surrounding medium into a state of constraint has arisen, as a principle to be actually believed.[16]

This can also help us to appreciate Maxwell's character when he produced his set of physics mathematics equations, which were reasonably described as a mathematical version of Faraday's experiments and main ideas in physics. Maxwell was interested in natural phenomena and attempted a mathematical interpretation, raising the following question: "Are there Real Analogies in Nature?" (Campbell and Garnett [1882] 1969, 235–244). An answer to this question would be given through the idea of a connected mechanical system (expressed in Lagrange's equations).

1.3 On Physical Ideas and Mathematical Quantities

It is scientifically significant for the development of *authentic science* and for the history of science that in *A Treatise on Electricity and Magnetism* Maxwell showed his admiration for Faraday by dedicating a chapter of 26 pages (Maxwell 1873, II, Pt IV, chap I, 128–145) to Michael Faraday's physical ideas. Moreover, he proposed theoretical arguments to describe the basic phenomena of electricity and magnetism (Maxwell 1873, II, Pt IV, chap I). He freely demonstrated his scientific devotion to Faraday, wishing to induce readers to share his views. From a physics mathematics point of view and its dialectic, this chapter is emblematic.

> As I proceeded with the study of Faraday, I perceived that his method of conceiving the phenomena was also a mathematical one, though not exhibited in the conventional form of mathematical symbols. I also found that these methods were capable of being expressed in the ordinary mathematical forms, and thus compared with those of the professed mathematicians.[17]

Here, Maxwell's main intention was to show that the widespread scientific concerns with regard to this concept could be reduced.

[15] Maxwell ([1890] 2003, II, 360, line 11).

[16] Part of a letter written by Maxwell to Faraday on 9 November 1857 (Faraday 2008, Letter 3354, 301, line 24).

[17] Maxwell (1873, I, *Preface*, x, line 7).

At the beginning of this first chapter, Maxwell proposed a new interpretation of Ørsted's effect (Ørsted) from physics mathematics operators. He examined two circuits carrying two currents and sought to characterize the electric force concept of their interaction in terms of energy. Thus, he avoided any foundational discussion on the nature of the force, focusing on the concept of energy, that is, an integration. A crucial concept in physics mathematics is that, together with its inverse, differentiation is one of the two main operations in calculus. Particularly, his new point of view (including energy) will allow us to understand the action of one current upon another. Maxwell was careful to formulate two interacting currents in terms of potential[18] and mutual energy.

> [. . .] the magnetic force in a field can be deduced from a [mathematical] potential function, as in several former instances, but the potential is in this case a function having an infinite series of values whose common difference is $4\pi i$. The differential coefficients of the potential with respect to the coordinates have, however, definite and single values at every point. The existence of a potential function in the field near an electric current is not a self-evident result of the principle of the [total] conservation of energy [. . .]. We must therefore for the present consider the law of force and the existence of a potential as resting on the evidence of the experiment already described.[19]

In this sense, when focusing on scalar functions to describe a physical system, an eventual discussion of the electric force concept should take place *a posteriori*. Of course, this physics mathematics approach can leave a physicist with some doubt. In fact, Maxwell's mathematical reasoning produced local results where a point, in its physical system, can become one of the infinite results of a differential equation,[20] as opposed to the role played by *an electric pole at point Z, the charge at point Z,* etc. Therefore, an infinitesimal point (in infinitesimal analysis) cannot describe the entire physical system and its interactions. In this sense, the field concept and its mathematical framework would help the discussion. When considering this, Simpson noted that Faraday has already used an *exploring coil* (Faraday 1855, II, Plate II) in order to sample magnetic action at multiple locations near a magnet. Simpson suggested that Maxwell also followed this practice in his *A Treatise on Electricity and Magnetism* with the two circuits. While one circuit becomes "[. . .] an infinitesimal exploring loop, a new and richer "point P" to replace the old: a physical loop, not a mathematical monopole" (Simpson 2005, 43, line 20, author's marks). Thus the loop becomes a dynamic entity in its infinitesimal form and not just an exploratory body. As previously mentioned, in this part of his *A Treatise on Electricity and Magnetism,* Maxwell established the foundations and relationship between electricity and magnetism, that is, a new scientific theory within the physics

[18]Of course, as always occurs in science, the electromagnetic theory was developed thanks to many other related and correlated studies (Fox; Giannetto) with Faraday and Maxwell. For instance, one may consider Neumann's contribution (Schlote 2005, 123–140).

[19]Maxwell (1873, II, Pt IV, 130, line 19).

[20]A similar situation happened to the Newtonian second law of motion.

mathematics domain. Therefore, he required a new dynamical theory, for this new phenomenon. New physics mathematics theory also means new magnitudes, which should be both mathematical and physical. The same situation occurred in mechanics (e.g.: velocity, acceleration, space time). Therefore, in order to build a new dynamical theory, Maxwell needed to build something unlike Newtonian mathematics. It is not by chance that Maxwell established this new theory by using Lagrange's equations. He freely referenced them in Chapter V, *On the equations of motion of a connected system* (Maxwell 1873, II, 183–194).

1.4 On Electric Induction

Maxwell also dedicated most of Chapter III to Ampère's (1826; Darrigol 2000) differing theory of electrodynamics (Maxwell 1873, II, 146–161). Moreover, a logical comparison contrasting Ampère (e.g., *Ivi*, 146–156) and Faraday (e.g., *Ivi*, chap III)'s methods emerges, together with his evident preference for Faraday's ideas. This intellectual contrast is important for the aim of this paper both for understanding the role of mathematics in his physics mathematics and for understanding the kind of mathematics and scientific approach he preferred, with respect to distinguished mathematicians and physics scholars such as André-Marie Ampère[21] (1775–1836). For example, Maxwell's opinion on the complex mathematical way of arriving at Ampère's fundamental equation is noted. The equation can be found in two ways[22]:

$$d^2 f = ii' \frac{ds ds'}{r^2} \left(\sin\alpha \sin\beta \cos\gamma - \frac{1}{2} \cos\alpha \cos\beta \right)$$

$$d^2 f = ii' \frac{ds ds'}{r^2} \left(\cos\omega - \frac{3}{2} \cos\alpha \cos\beta \right)$$

By considering the advanced mathematics that one should employ to arrive at Ampère's equations and by considering his convincing argument on the crucial role played by Newtonian mechanical force to explain these new electric phenomena as well, it is clear that Maxwell could not accept the Newtonian paradigm[23] to explain

[21] Ampère developed a mathematical theory to describe previously observed electromagnetic phenomena and he proposed many new foundations to study.

[22] The argument and its formula were read by Ampère at *Académie des Sciences* (Ampère 1826, 151, ft 1, 204; see also: Ampère 1822a, 293–318; see 317 and his comment ft 1; Ampère 1822b, XX, 187; 188; Ampère 1822c, XX, 405–419).

[23] It is known that Ampère clearly tried to mathematically describe the physical world, and new electric-magnetic phenomena as well, by using Newtonian laws. He conducted many studies to achieve this result in a new physical domain. Let us think, e.g., of his physics mathematics where a distance r appears in the denominator as a square. In this case, the law of the inverse square describes the force (like Newton for gravity) between current elements. Thackray proposes an interesting essay on Newton and his ideas in the history of science (Thackray 1970; Pisano 2007).

and mathematically describe the new kind of interactions between two elementary bodies. Moreover, the Newtonian law of universal gravitation could not be used since no interaction (attractive–repulsive) existed: one action was conceived, solely. In chapters II and III (Maxwell 1873, II, Pt IV) these intellectual and foundational aspects of Maxwell's physics mathematics theory clearly emerge; most notably the discussion on the impossibility of accepting Ampère's result (Ampère 1822a, b, 1827) since his reasoning was scientifically too weak to show true electromagnetic actions between one infinitesimal element and another. Here, the physics mathematics relationship changes since the mathematical interpretation of the physical phenomenon changes. Nevertheless, history teaches us that Maxwell's framework reconsidered Ampère's theory of forces between current elements. Ampère's law with Maxwell's correction establishes that a magnetic field can be generated both by electrical current (*Ampère's law*) and by changing the electric field (also known as *Ampère's law with Maxwell's correction*). These corrections are particularly important since they show that not only does a changing magnetic field induce an electric field, but a changing electric field also induces a magnetic field: the definitive physics mathematics interaction between electric and magnetic domains without, e. g., measuring and discussing the physical nature of magnitudes. One of the reasons may be related to (a) a consolidated mechanical Newtonian theory, (b) the authorship of Ampère in the international scientific panorama of the nineteenth century, (c) the assumption of new theoretical elements in his *A Treatise on Electricity and Magnetism* would allow Maxwell to review some reasoning and try to produce a new style of thinking in this newly debated domain of science, (d) gravitation law, weaker than electric law, is applied to all bodies, while electromagnetic laws for charged bodies only, etc. Therefore, new studies could be done.

A new discussion immediately follows the aforementioned discussion and it is connected to one of the main elements of Maxwell's physics mathematics (Maxwell 1873, II, Pt IV, III). This idea is indebted to Faraday's experimental physics: electric induction where one circuit, carrying a current, induces a current in another nearby circuit. The difference (with respect to Ampère's physics mathematics) in methods of reasoning and mathematical procedure concerning *electric induction* phenomena emerged. For instance, (ca. Faraday-) Maxwell's physics mathematics considered the action between two bodies A, and B, letting an intervening medium act on body B. The equations are in terms of mutual energy.[24] Ampère considered a direct action between two bodies A, and B, leaving the mathematical equations in terms of forces of any intervening physical medium.

Particularly, *Induction of Electric Currents* (read November 24, 1831) was one of Faraday's first discoveries in electricity[25] (Sweetman) looking back to similar electrostatic phenomena, where one can observe the production of an unbalanced electric charge on an uncharged conductive body as a result of a charged body (opposite sign) being brought close to it without touching it. Finally, only a

[24] Maxwell also discussed electric forces in terms of *electromotive force* (Maxwell 1873, II, Pt IV, IV–V).

[25] For a *historiography reassessment* see Agassi (2008, 466–468).

concise reaction in the second wire when the current in the first circuit was being turned on or off was produced. Faraday named this state the "*electro-tonic* state" (Faraday 1839–1855, I, Is, §3, p 16, line 28) but, even though he found a way around this problem by reasoning in terms of lines of force, he never mathematically demonstrates it. Therefore, from a physics mathematics standpoint, Maxwell first pointed out this concept, which was so important in Faraday's physics, by attempting a geometrical and mathematical solution in *On induction of electric current* (Maxwell 1873, II, Pt IV, III, 172–173). He began by stating his unsatisfied equation and reasoning on the matter (Maxwell 1873, II, Pt IV, III, 172–173). Then, he continued his studies in a short chapter, *Induction of a current on itself* (Maxwell 1873, II, Pt IV, IV, 180–183) specifically on *Induction of a current on itself* (Faraday 1839–1855, I, IXs). From a physical standpoint, Faraday noted (*Ivi*) a new form of inductive effect should (in fact) exist for a battery. Sparks take place when the current is unexpectedly interrupted. Nevertheless, the effect is lacking when this same wire is uncoiled and extended out in a straight line. According to Faraday, this effect is, at first glance, similar to mechanical *inertia*:

> 1077. Returning to the phenomenon, the first thought that arises in the mind is, that the electricity circulates with something like *momentum* or *inertia* in the wire, and that thus a long wire produces effects at the instant the current is stopped, which a short wire cannot produce.[26]

The analogy is interesting from a Newtonian mathematical point of view. If we go on to think from a physical viewpoint, Faraday's ideas contain an important assumption: the foundation of the theory is shifting from static to kinetics and moving systems. From a physics mathematics point of view, Maxwell produced analogies with images of the force of (matter) water in a pipe etc. These analogies are not adequate since, in effect, the water flow is unaffected by changes in the pipe and effects of self-induction, (and related phenomena) depend entirely on the configuration of the conductor. Maxwell stated that when motion is not occurring within the wire, then it should belong to the surrounding space. With regard to the correlated matter concept in his physics mathematics electromagnetic theory, Maxwell stated his fundamental dynamical idea of matter; it cannot be seen or touched, and since it is connected to energy and momentum[27] (Maxwell 1873, II, Pt IV, §550, 181–182) concepts, calculations should be advanced:

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[26]Faraday (1839–1855, I, IXs, 330, line 9).

[27]He referred to Torricelli's relation on the idea of matter: "Torricelli [. . .] has expressed the relation between the idea of matter on the one hand and those of force and momentum on the other, neither of which can exist without the other." (Maxwell [1890] 2003, II, 812). "[. . .] as Torricelli remarked 'is a quintessence of so subtle a nature that it cannot be contained in any vessel except the inmost substance of material things ["La forza poi, e gl'impeti, sono astratti tanto sottili, son quintessenze tanto spiritose, che in altre ampolle non si posson racchiudere, fuor che nell'intima corpulenza de' solidi naturali" (Torricelli 1715), *Lezioni Accademiche*, p 25; author's quotation marks]. Hence all these theories lead to the conception of a medium in which the propagation takes place, and if we admit this medium as an hypothesis, I think it ought to occupy a prominent place in our investigations, and that we ought to endeavour to construct a mental representation of

> It is difficult, however, for the mind which has once recognized the analogy between the phenomena of self-induction and those of the motion of material bodies, to abandon altogether the help of this analogy, or to admit that it is entirely superficial and misleading. The fundamental idea of matter, as capable by its motion of becoming the recipient of momentum and energy, is so interwoven with our forms of thought that, whenever we catch a glimpse of it in any part of nature, we feel that a path is before us leading, sooner or later, to the complete understanding of the subject.[28]

For Maxwell, a dynamical theory is strictly founded on the concept of energy. The matter appears to be very different from the Newtonian mass concept since, here, it moves toward a new dynamical style of thinking. Here, concerning energy, Maxwell is convinced that it is present in the space surrounding the current-carrying conductor.

> Again, when the current is left to itself, it may be made to do mechanical work by moving magnets, and the inductive effect of these motions will, by Lenz's law, stop the current sooner than the resistance of the circuit alone would have stopped it. In this way part of the energy of the current may be transformed into mechanical work instead of heat. It appears, therefore, that a system containing an electric current is a seat of energy of some kind; and since we can form no conception of an electric current except as a kinetic phenomenon[* footnote to "Faraday, *Exp. Res.* (283)"], its energy must be kinetic energy, that is to say, the energy which a moving body has in virtue of its motion.[29]

Nevertheless, since we cannot have evidence for the existence of moving bodies, rather than Newtonian matter, in the open space near a conductor, Maxwell's seems to have a conception of physical matter (Maxwell 1920) very closely related to a kind of material–mathematical modelling for interpreting a physical system. Thus, his electromagnetic theory is something more than his final physics mathematics equations. It is related to an existent state of matter: "a physical system bearing energy associated with every [mathematical] point in [geometrical] space" (Simpson 2005, 58, line 8). Faraday called it *electrostatic induction*.

Finally, Maxwell provided a mathematical form for Faraday's physical reasoning, but we should specify that he did not obtain the satisfaction of resolving it. On the other hand, in this case, Faraday's aim was more than a physical explanation. He was looking for an *electronic state* that truly exists in nature.

2 Physics Mathematics from a Mathematical Standpoint

In the previous sections, we dealt with physics mathematics evidence, from a physical point of view in relation to Faraday's physics. Here, we will focus on physics mathematics evidence but from a strictly mathematical point of view (McAuly).

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all the details of its action, and this has been my constant aim in this treatise." (Maxwell 1873, II, §866, p 438, line 9). On Torricelli see: (Capecchi and Pisano 2007).

[28]Maxwell (1873, II Pt IV, IV, 181, line 36).

[29]Maxwell (1873, II Pt IV, IV, 182, line 30).

2.1 On Physics Mathematics in Electromagnetic Theory

In this section, we will mainly be dealing with *On the equation of motion of a connected system* (Maxwell 1873, II, Pt IV, V). Maxwell's effort passed through Newtonian science to reach a new physics mathematics based on the concept of energy instead of that of force.[30] However, a question may be: *what kind of force concept, energy are we dealing with?* In analytical mechanics, *equations of motion* are equations that describe the behaviour of a system in terms of its motion as a function of space and time (e.g., the motion of a particle under the influence of a force). Sometimes the term refers to the differential equations that the system satisfies (e.g., Newton's second law or Euler–Lagrange equations[31]), and sometimes to the solutions to those equations. With regard to Maxwell, a question may be: *what kind of mathematics did he prefer to use for his physics mathematics of the electromagnetic theory?* Maxwell needed an adequate mathematics vastly different from that used in the Newtonian paradigm. The alternative mechanical science (until Laplace's[32] mechanics and just before Lazare Carnot's mechanics) was Lagrange's analytical mechanics (Capecchi and Drago 2005; Panza 2003b; Blay).

Maxwell used Lagrange's mechanics as a new approach to physical theory, thinking that it might be the closest to Faraday's physical problems expressed in a non-mathematical way. Thus, Maxwell reasoned in broader terms about the connected mechanical system. Since Lagrange's equations of motion are expressed in terms of rates of change, a kind of differential equation is obtained. Nevertheless, Maxwell first accepted Lagrange's method and then his equations to describe electromagnetic problems (Maxwell 1873, II, Pt IV, V, 193–194). His preliminary ideas on the role played by physical magnitudes in this Lagrangian approach are evident in some parts of *Treatise on Electricity and Magnetism* (Maxwell 1873, II, Pt IV, V, 184–186, 191).

In all of chapter V (Maxwell 1873, II, Pt IV, V, 184–194) differing from Newtonian mechanics, Maxwell expressed a motion and energy relations within the system as a whole, rather than in terms of laws of motion governing the actions of forces. Here, after proposing several methods, he announced a third method related to Lagrange's[33] equations:

> 563.] There is a third method of expressing the kinetic energy, which is generally, indeed, regarded as a fundamental one. By solving the equations (3) [$\dot{q} = \frac{dT_p}{dp}$. (*Ivi,* 189)] we may express the momenta in terms of the velocities, and then, introducing these values in (13)

[30]Thus, mass is another Newtonian concept Maxwell does not use.

[31]Interesting new approaches to the use of mathematics for physical variables have recently been established within complexity theory; they are also very interesting from an historical/philosophical standpoint (Longo).

[32]Gillispie (1997).

[33]It is interesting to remark that in chapter V (Maxwell 1873, II) Maxwell used Lagrange's equations (Lagrange) to formulate his own general equation for electromagnetic theory, but only in chapter VI would it describe a particular physical system.

> [$T_{p\dot{q}} = \frac{1}{2}(p_1\dot{q}_1 + p_2\dot{q}_2 + \&c.)$. (*Ivi,* 191)], we shall have an expression of T involving only the velocities and the variables. When T is expressed in this form we shall indicate it by the symbol $T_{\dot{q}}$. This is the form in which the kinetic energy is expressed in the equations of Lagrange.[34]

With regard to equations of motion in his electromagnetic theory, I note that in the Lagrange system,[35] the field is a *continuum*; it remains whole and in substance undivided. Since Maxwell aimed at formulating a dynamical justification to field equations, in this part, he focused on the fact that the magnetic field appeared as a completely kinetic system. Thus, he assumed that the energy of the system should be totally kinetic (Maxwell 1873, 189–192). Particularly,

> In the case of a system with several variables an expression such as *dT/dqi,* whose purpose is in effect to test the dependence of T on the variable *qi* exclusively, does not denote a simple derivative. Since it is the variation of *qi alone* that is wanted, the configuration of the rest of the system must be held unchanged while *qi* is varied. This evidently does not represent an actual, or even possible, motion of the system but rather, in effect, a sort of thought-experiment. [In effect] The derivative taken in this purely conceptual manner is today termed the *partial* derivative of T with respect to *qi*.[36]

Regarding the use of the partial differential in his physics mathematics, Maxwell mainly referred to partial differentiation of the energy function, which, in his calculations, represented his mathematical–dynamical idea to solve physical operations related to his *dynamical relations of thought* between physics and mathematics and his aim to propose a dynamical justification for field equations. Maxwell formulated his mathematical field concept through several phases, e.g.: (a) a geometrical study of Faraday's hydrodynamic analogical[37] model, based on lines of systems of forces – imagined by Faraday – and "[. . .] the collection of imaginary properties [. . .]" (Maxwell 1856, 160, line 4) of the *theory of motion of an incompressible fluid* (*Ivi*), (b) a concrete mechanical model (Maxwell 1861–1862, part I), based on the production of magnetic forces from electric current, called molecular vortexes (Kragh), (c) a dynamical justification for field equations (Maxwell 1865, 1873).

2.2 On the Field Concept and Mathematical Operators

The field concept is a part of Maxwell's theory, which constitutes a defining moment in *A Treatise on Electricity and Magnetism*. In order to include Faraday's insight,

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[34] Maxwell (1873, II, Pt IV, V, 191, line 12).

[35] Of course, a strict relationship between Lagrange's equations and the second law becomes evident for a single moving body.

[36] Simpson (2005, 65, line 10). Author's italics.

[37] By means of an analogical model he could establish mathematical relationships between some physical quantities defined (yet not very well, e.g., intensity of electricity) by Faraday and hydrodynamic quantities (e.g., force).

Maxwell projected and realized one of the basic structures of his new physics mathematics: Faraday's early physical concept of the field:

> Faraday had no mathematical or mechanical preconceptions, and his theory mostly reflected patient experimental explorations.[38]

> Faraday and Thomson invented field theory: they introduced theoretical entities in space between electric and magnetic sources, and they elaborated powerful techniques for investigating the properties of these entities.[39]

> Proceeding from Faraday's and Thomson's writings, Maxwell reached the essentials of his electromagnetic field theory stepwise, in three great memoirs. In *On Faraday's line of force* his aim was to obtain a mathematical expression of Faraday's field conception.[40]

In Maxwell's opinion, the field is part of a new theory, physics mathematics within the domain of a physical theory.

In 1831, Michael Faraday demonstrated (without using mathematics) the reciprocal effect, in which a moving magnet in the vicinity of a coil of wire produced an electric current. Ørsted's experiment on a magnetic needle led Faraday, around the 1830s, to conceive the law of electromagnetic induction (Faraday 1839–1855; see also Williams) and a notion of the magnetic field.[41] It should be noted that Faraday, even though he previously used the terms *magnetic curves*, and *lines of magnetic forces* (e.g., Faraday 1839–1855, I, §114, 32; see footnote), officially used the word "field" in his *Diary* for the first time on November 7, 1845, (Nersessian 1989, note 7, 6). Other crucial definitions were proposed in 1845 (Faraday 1839–1855, III, §2149, 2) and late 1852. The later definition is the most important and decisive field concept (Faraday 1839–1855, III, §3071, 328). Faraday introduced the *magnetic field* concept in 1845 but published it in 1846 (Faraday 1839–1855, III, §2247, 29; see also: *Ivi*, §2252). On other occasions, he used the term *magnetic field* (e.g.: Faraday 1839–1855, III, §2463–§2475, §2806–§2810, §2831, §3171). Faraday presented a very clear definition of the *magnetic field* in 1850 at *The Royal Society of London,* which was published *posthumously* in 1851 (Faraday 1839–1855, III, §2806, 203). It is a crucial concept that Maxwell consequently studied from a physics mathematics point of view[42] (mainly: Maxwell 1865, 460, 1873, I, §44, 44; II, §476, 128). This idea would be followed without a priority of physical measurements, that is, the idea was to remain within the physics mathematics domain.

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[38]Darrigol (2000, 136, line 4).

[39]Darrigol (2000, 134, line 10).

[40]Darrigol (2000, 172, line 29). Author's quotations marks.

[41]For the sake of brevity, here I avoid fully discussing the *history of field concept*. On details concerning the history of *The field conceptsof Faraday and Maxwell,* see a recent work by Assis, Ribeiro and Vannucci (Assis, Ribeiro and Vannucci; see also Nersessian).

[42]Later, its theory essentially became a field theory making use of Lagrange's equations of motion. I refer the reader to secondary literature (Darrigol 2000, 2005; Siegel 1981).

By following differentiation of the energy function and by using Lagrange's equations of motion, Maxwell was able to formulate his momentum concept (Maxwell 1873, II, Pt IV, V, 190–191), one of the basic concepts for building his *Dynamical theory of electromagnetism,* which he presented in chapter VI (Maxwell 1873, II, Pt IV, VI, 195–205). In order to proceed toward his dynamical project, he needed to establish the mathematical conditions to write a form of the equations capable of describing a particular[43] physical system within electromagnetic theory. In his words:

> 567.] In this outline of the fundamental principles of the principles of the dynamics of a connected system, we have kept out of view the mechanics by which the parts of the system are connected. We have not even written down a set of equations to indicate how the motion of any part of the system depends on the variation of the variable [. . .].[44]
>
> Our only assumptions are, that the connexions of the system are such that time is not explicitly contained in the equations of conditions, and that the principle of the conservation of energy is applicable to the system. Such a description of the methods of pure dynamics is not unnecessary, because Lagrange and most of his followers, to whom we are in debt for these methods [. . .].[45]
>
> As the development of the ideas and methods of pure mathematics has rendered it possible, by forming a mathematical theory of dynamics, to bring to light many truths which could not have been discovered without mathematical training, so, if we are to form dynamical theories of other sciences, we must have our minds imbued with these dynamical truths as well as with mathematical methods.[46]

In chapter VI (Maxwell 1873, II, Pt IV, VI) Maxwell focused on this point, seeking adequate coefficients to convert previous general equations (Maxwell 1873, II, Pt IV, VI, 198–199). From a strictly mathematical point of view, he pointed out three categories among coordinates q_i (Maxwell 1873, II, Pt IV, VI, 197–198) obtaining the total energy T divided into three parts, generating terms of three forms: $x_i x_j, y_i y_j$, and $x_i y_j$. Non-homogeneous coordinates are combined in terms of the form $x_i y_j$. From a physical point of view, they are mechanical (x) and electrical (y) variables. These products give rise to three corresponding sources of kinetic energy when they appear in the equation for T.

> 571.] [. . .]. We may therefore divide T into three portions, in the first of which T_m, the velocities of the coordinates x only occour, while in the second, T_e, the velocities of the coordinates y only occour, and in the third, T_{me}, each term contains the product of the velocities of two coordinates of which one is x and the other y.

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[43] Previously, in chapter V, Maxwell pointed out (from Lagrange) a general form of the equations of motion which are not able to describe a particular physical system.

[44] Maxwell (1873, II, Pt IV, V, 193, line 34).

[45] Maxwell 1873, II, Pt IV, V, 194, line 5).

[46] Maxwell 1873, II, Pt IV, V, 194, line 22).

We have therefore $T = T_m + T_e + T_{me}$

$$T_m = \frac{1}{2}(x_1 x_1)\,\dot{x}_1^2 + \&c. + (x_1 x_2)\,\dot{x}_1 \dot{x}_2 + \&c.,$$

Where $$T_e = \frac{1}{2}(y_1 y_1)\,\dot{y}_1^2 + \&c. + (y_1 y_2)\,\dot{y}_1 \dot{y}_2 + \&c.,$$

$$T_{me} = \frac{1}{2}(x_1 y_1)\,\dot{x}_1 \dot{y}_1 + \&c.$$ [47]

These physics mathematics arguments dealt with three methods "[. . .] of detecting the existence of the terms of the form T_{me}, none of which have hitherto led to any positive results" (Maxwell 1873, II, Pt IV, VI, 205, line 27). Thus, the equations from chapters VI were made to bear on a simple electromagnetic system consisting of only two circuits in *Theory of electric circuits* (Maxwell 1873, II, Pt IV, VII). His reasoning would be extended to more complex systems, which involve various conductors and a variety of mechanical motions and corresponding equations, interpreted in electrical terms in accordance with Faraday's experimental discoveries in electromagnetic induction. Finally, electromagnetic phenomena are very well connected with equations of motion and their forms generated the foundation for Maxwell's theory of the electromagnetic field.

2.3 On Magnetic Vector Potential and Electric Displacement

As previously mentioned, when establishing the *electronic state* as a state of momentum, Maxwell was guided by Faraday's studies. Particularly, in Faraday's scientific panorama, Lagrange's method does not entail electromagnetic theory. Therefore, in *Exploring of the field by means of the secondary circuit* (Maxwell 1873, II, Pt IV, VIII, 211–226), Maxwell returned to Faraday's experimental methods of the moving wire to explore the magnetic field in its new form. A consequence of this choice, of course, was not making the relationship with field's momentum a priority. His return to Faraday's physical style of thinking certainly concerned his main scientific ideas and methods, which he should have converted into physics mathematics reasoning. In fact to accomplish this, Maxwell introduced a vector, which in some way referred to a quantity of momentum at every point in space. The vector is the very intricate *magnetic vector potential* (Maxwell 1873, II, II, §405, 27; Bork) which assumed an important role in Maxwell's reasoning on his field concept. His reasoning is significant for physics mathematics and for his related translation of Faraday's thought. In chapter[48] VIII (Maxwell 1873, II, VIII) Maxwell argued on the very mathematical structure of the electromagnetic

[47] Maxwell (1873, II, Pt IV, V, 197–198, line 36).

[48] And in (II and) IX (*Ivi*).

field, often using Faraday's studies and experimental data to accomplish this goal. In order to do succeed in his endeavour, Maxwell needs

> 594.] [...] to deduce from dynamical principles the expressions for the electromagnetic force acting on the conductor carrying an electric current through the magnetic field, and for the electromotive force acting on the electricity within a body moving in the magnetic field.[49]

For the sake of brevity, Faraday's physics and Maxwell's physics mathematics dialectic (Maxwell 1873, II, Pt IV, VIII, 215–218) regarding the field concept may be listed as the following: (a) initially no current (b) by means of the field a current inducted is (measured-) calculated, (c) a constant current passes, (d) a mechanical force is then calculated. We can observe that (a) calculating the electromotive force acts on the generalized electrical variable and (b) this determines the electromagnetic (mechanical) force (Maxwell 1873, II, Pt IV, VIII, 218–221) on the wire. This is Maxwell's *affectionate* tentative to develop a "[...] mathematical method which we shall adopt [that] may be compared with the experimental method used by Faraday"[50] (Maxwell 1873, II, Pt IV, VIII, 217, line 5). At the end of this reasoning (*Ivi*), a different and evident scientific approach emerges with respect to early reasoning proposed by Maxwell in the first chapters (*Ivi*), in which, e.g., he used Gauss's theory of scalar potentials to discuss general elements of the magnetic field. In chapter VIII (*Ivi*), by using Faraday's wire, Maxwell once again selected Lagrangian mechanics for his momentum. (Simpson 2005, 108–109). Maxwell's physics mathematics style thinking emerges for both methods and concepts related to the old scalar theory of magnetism (Maxwell 1873, II, Pt IV, VIII, 224–225).

Maxwell provides a dynamical explanation of the field theory (Maxwell 1873, II, Pt IV, IX, 227) in *General equations of the electromagnetic field* (*Ivi*) where the terms *General equations* in the title of the chapter would be (possibly) a *definitive form of physics mathematics equations*. He listed them from "A" to "L" (Maxwell 1873, II, Pt IV, IX, 227–233) which, for the sake of brevity and since there are various works related to this matter, I do not reproduce them here. In writing these equations, Maxwell established the same differential cell established in the previous chapter, however, they are now connected by an elastic medium and potential energy.

A physics mathematics interaction between the magnetic and electric domain is once again presented, but now this interaction is thanks to a new theoretical element which Maxwell called *Electric displacement* (Maxwell 1873, II, Pt IV, IX, §608, 232). It is a quantity that is defined in terms of the rate of change of electric displacement as a field. *Electric displacement* has the units of electric current density, and it has an associated magnetic field just as actual currents do. However, it is not an electric current of moving charges, but a time-varying

[49] Maxwell (1873, II, Pt IV, VIII, 217, line 1).

[50] I suggest examining "Fig. 38" (Maxwell 1873, II, Pt IV, VIII, 217) presented by Maxwell during his reasoning.

electric field. It is the "Equation of True Currents (H)" (Maxwell 1873, II, Pt IV, IX, §610, [232–]233), where the *true electric current* (*Ivi*, 232), *density of the current of conduction* with *time-variation of electric displacement* and therefore total movement of electricity are taken into account. Maxwell also wrote that equation in terms of its components where the relations between physical quantities and mathematical ones are more evident. The dynamical relation of thought between mathematical operators and physical quantities concerning the *electric displacement* was first conceived by Maxwell in his *On Physical Lines of Force*[51] in connection with the displacement of electric particles in a dielectric medium where Maxwell added the *displacement current* to the electric current term in *Ampère's Circuital Law* (Maxwell [1890] 2003, I, pp. 471–474). Another account was discussed in *A Dynamical Theory of the Electromagnetic Field* (Maxwell 1865). In this case, he used this amended version of *Ampère's Circuital Law* to obtain[52] the electromagnetic wave equation. The displacement current term is now seen as a crucial addition that completed Maxwell's equations and is necessary for explaining many phenomena, in particular the existence of electromagnetic waves. Maxwell, as announced in his previous works, has now, by means of the *electromagnetic field*, completed his research program and life project by establishing a new style of thinking and conceiving a new science through the interaction of electric and magnetic phenomena.

2.4 A Road to Physics Mathematics?

In general, a unit of measurement is effectively a standardised quantity of a physical (and chemical) property, used as a factor to express occurring quantities of that property. Therefore, any value of a physical quantity is expressed as a comparison to a unit of that quantity. In the physics mathematics domain one generally precedes by means of calculations, therefore the units of measurement are not a priority in terms of a solution to an analytical problem (Lindsay, Margenau and Margenau). In this sense, the physical (and chemical) nature of the quantities is not a priority.[53]

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[51]Maxwell firstly wrote *On Faraday's Lines of Force* (Maxwell [1890] 2003, I, 155–229) which was completed in1856. In 1861(−2) he wrote another ambitious paper, *On Physical Lines of Force* (Maxwell [1890] 2003, I, 451–513) where he elaborated his theory on mechanical vortex (Thomson 1883; see also *Id.,*1881, 1885, 1891).

[52]Generally, this derivation now seems commonly reasonable in physics mathematics, however, it combines the general idea of uniting electricity, magnetism and optics into one single unified theory. I do not comment on this derivation.

[53]For instance, one can see an analogous situation concerning heat and temperature concepts in the analytical theory of heat (Fourier 1807, 1822, Lamé 1836, 1861; see also Pisano and Capecchi) with respect to Sadi Carnot's thermodynamic theory (Pisano 2010, 2011a, b; Gillispie and Pisano 2012). I briefly note that physics considers the indispensable agreement between theoretical data

One may discuss the role played by a certain science in history (e.g., physics), focusing solely on the historical period and the kind of mathematics adopted. For my aim, the most important aspect is the role played by the relationship between physics and mathematics adopted in a scientific theory in order to describe mathematical laws – e.g., the second Newtonian mathematical law of motion[54] (Panza 2002). Time is a crucial physical magnitude in mechanics, but in the aforementioned law, it (with space) is also a mathematical magnitude since it is involved in derivative operations. Most importantly, if we lose their mathematical sense, we would lose the entire mechanical paradigm. Nevertheless, the approaches to conceive and define foundational *mechanical–physical quantities* and their *mathematical quantities* and interpretations change both within a physics mathematics domain and a physical one. One could think of mathematical solutions to Lagrange's energy equations, rather than the crucial role played by collisions and geometric motion in Lazare Carnot's algebraic mechanics or Faraday's experimental science with respect to Ampère's mechanical approach in the electric current domain and finally the physic mathematics choices in Maxwell's electromagnetic theory.

Physical science makes use of experimental[55] apparatuses to observe and measure physical magnitudes. During and after an experiment, this apparatus may be illustrated and/or designed. Generally, this procedure is not employed in pure mathematical studies. Thus, one can claim that experiments and their illustrations can be strictly characterized by physical principles and magnitudes to be measured. A modelling of results of the experimental apparatus allows for the broadening of the hypotheses and the establishment of certain theses. If one avoids study-modelling experimental results, one may generate an analytical scientific theory since there is no interest in the nature of physical magnitudes and their measurements. For example, a *quasistatic process* is a thermodynamic process that happens infinitely slowly. However, it is very important to note that *no* current–real process is quasistatic. Therefore, such processes can only be approximated by performing them infinitesimally slowly. *But what does it mean from an empirical physical standpoint?* A process cannot be static (equilibrium situation?) and process (non-equilibrium situation or dynamical one?) at the same time . . . and vice-versa. However this reasoning assumes a certain quantification and *scientificity* if one considers the dynamical-thought standpoint in the use of an infinitesimal concept in mathematics. In this case an infinitesimal point may express a (dynamical-thought) idea of quantities (e.g., in a differential equation) so small that there is no way

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and observations/experimental data (including the properties of magnitudes) to establish a physical theory. Generally, such arguments are not considered rigorous by physics mathematics.

[54]In this reasoning I consider the Newtonian science and its development in the history.

[55]From a physical standpoint and for the aim of this paper, I remark that the *Physicist and Natural Philosopher* (Everitt) was the founder (1874) of *Cavendish Laboratory,* thanks to William Cavendish – 7th Duke of Devonshire who was also Chancellor of Cambridge University and donated money for the construction of the physics laboratory. Maxwell became the first *Cavendish Professor of Physics* (1871–1879) for a tenure in *experimental physics* (Gmellin).

to see them or to measure them or, in common speech, an infinitesimal point is a quantity which is smaller than any feasible measurement, but not zero in size; and at the same time, so small that it cannot be distinguished from zero by any available means.[56] *Again, what does it mean in an empirical physics?* And, *if we do not use strict empirical procedures and instruments*, *what kind of physics are we talking about?* For example, *is distance s and time t the same magnitudes (numerator and denominator) in* $v = ds/dt$? And, *if we delete the friction from theory of heat (or thermodynamics) should we restore and come back to the mechanics of the seventeenth century?* And above all, *what about perpetual motion and the law of inertia?*

This mentioned mathematical formalism radically changed Faraday's basic concepts and explained them in abstract concepts in a different order: first the local ones around a point and then the global ones.[57] In these types of theories there are various mathematical aspects so the theory appeared entirely mathematical, e.g. the analytic theories. In fact, the content was a sophisticated mathematics (differential calculus by partial derivatives, integral calculus, series, etc.) in order to interpret each field of phenomena. Therefore, a physical theory in origin was absorbed by mathematics, producing two crucial consequences: the birth of a new analytical theory and, at the same time, the scientific-cultural demise of the previous classical physical theory of heat. This analytical approach is one of the reasons why in physics mathematics (in terms of a discipline), e.g., a result of an integration (e.g., related with an energy concept) rather than a differential equation (e.g., related with a dynamical force concept) can be correctly accepted without discussing physical–chemical properties, physical significance, and units of measurements of the interested quantities. Instead, it refers to the use of advanced mathematical methods to solve mathematical aspects of problems in physics (i.e., one can see McAulay (McAulay)). This should be accomplished by studying and solving problems inspired by physics within a mathematically rigorous framework.

[56]Generally speaking in constructive mathematics a point is a range (not an infinitesimal point) typical of empirical measurements in physics. Of course this mathematical approach did not properly obtain the entire *power of calculus* such as is possible to have by means of infinitesimal analyses.

[57]There is not enough space in this paper, but it would be interesting to discuss the alternative theories that came about before Maxwell's physics mathematics and those that continued after. For example, Weber and Riemann (Pisano and Casolaro) based their theory on those entities that charges and magnetic dipoles interact with and act on, that is to say, the material supports of electromagnetism. They succeeded in repeating a great part of the theory but not all of it; for example, the displacement current, since it is without material supports, remains mostly outside. Other highly developed theories of electromagnetism were that of Helmholtz and that of Poincaré (1890, 1897, 1900). Only the birth of relativity put an end to their rivalry because only Maxwell was in agreement with this new innovation.

3 Conclusion

Among physicists, mathematicians, historians and philosophers[58] who are credited width the study of *mathematical physical quantities*[59] by means of experiments, modelling, properties, existences, structures etc. one can strictly focus on how physics and mathematics work in a unique discipline *physics mathematics* (or, if one prefers, *mathematics physics*). Of course, I do not mean a mathematical application in physics and vice-versa but rather a new (at that time) way to consider this science. Here it is a new discipline *physics mathematics* and not mathematical physics. A new methodological approach to solve physical problems (origin) where the quantities can be physical and mathematical at the same time (first novelty) and measurements are not a priority or a prerogative (second novelty) that makes, however, a coherent and valid physical science. Thus, the emergency in physics mathematics (discipline) belonged to physics, not in an advanced use of mathematics to solve physical problems, since physics has changed its *face* since, within this new physical mathematical discipline, it changed its foundations. With regards to Maxwell:

> Maxwell's challenge was to expound a new doctrine and at the same time to establish a new standard in the treatment of current problems. In order to meet these two conflicting requirements, he carefully separated the basic mathematical and empirical foundations of the subject from more speculative theory. [...] Maxwell defined the basic physical quantities in a neutral manner that could be accepted both by fluid and field theorists. For example, he introduced the quantity of electric charge of a body by means of Faraday's hollow conductors [...] with these neutral definitions, Maxwell could conduct much of mathematical analysis without deciding the nature of electricity and magnetism.[60]

Thus, I refer to a structured discipline, having its own hypothesis, methods of demonstration, an internal coherent logic etc., where the change in the kind of infinity in mathematics produces a change in both significant physical processes (Pisano 2010) and interpretations of *physical quantities*. One of the possible scientific approaches to understanding the history of the foundations of science may be to combine historical and epistemological aspects (primary sources, historical hypothesis, shared knowledge and epistemological interpretations) by means of a logical and mathematical inquiry called the *historical epistemology of science* (Pisano 2011b).

Below, I present a possible key of investigation,[61] *a mò* of conclusion related to Faraday and Maxwell's accounts:

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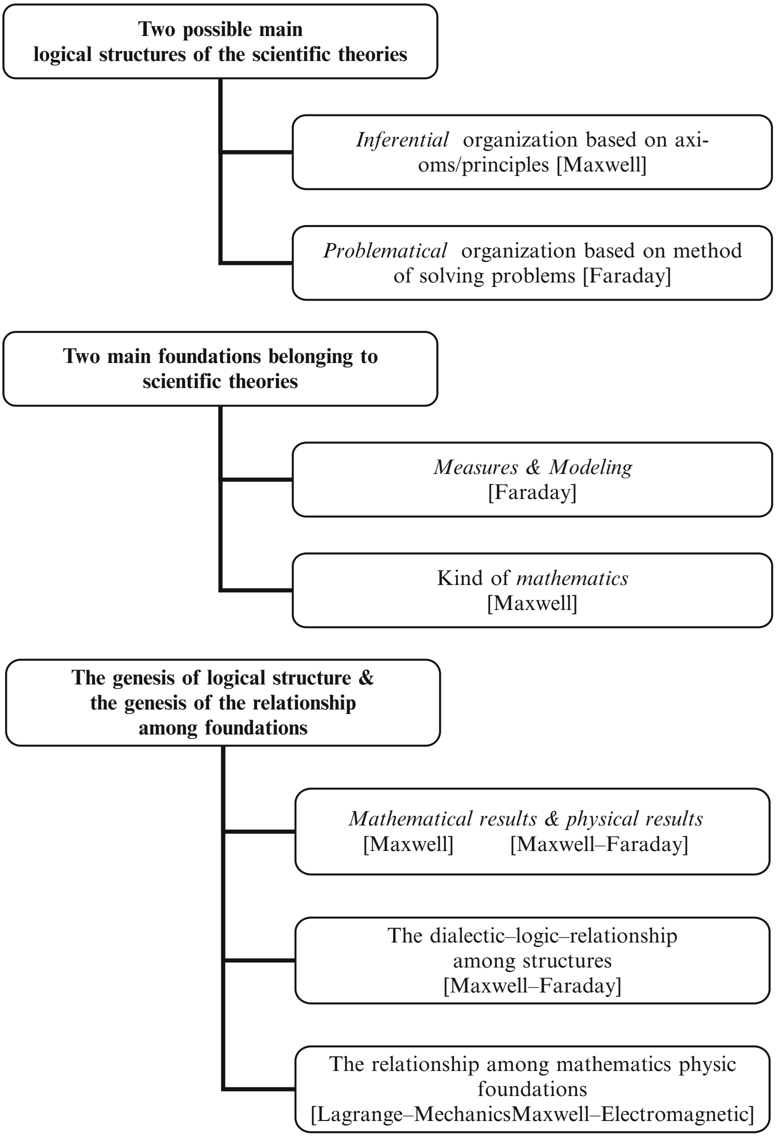
[58]Whewell conducted an interesting study on inductive processes in science and the philosophy of science during Maxwell's time (Whewell 1837, 1840).

[59]A recent study on the role played by *mathematical objects* in philosophy of mathematics is presented in (Panza 2003a).

[60]Darrigol (2000, 167, line 1).

[61]A previous interesting account on the "options in the science and history of science" was presented in (Drago 1988, 2007; Pisano and Gaudiello 2009a, b).



A new theory may essentially mean new magnitudes, new instruments to measure, new results recognized from a physical and mathematical standpoint. The new physics mathematics theory was not simply an advanced mathematical procedure applied to a previous experimental theory; it was more. It was an improvement of the same contents from a new physics standpoint, using new mathematical magnitudes and new quantities. Thus, it was necessary to establish a new theory having new magnitudes to justify both the *new nature of ideas* and to avoid hiding physical reasoning with a "[. . .] terrific array of symbols [. . .]".[62] For my aim, the novelty is the new conception of physical quantities; they are not solely physical, nor solely mathematical, but physics mathematics quantities since they, altogether, represent *relationships of thought* among *mathematical quantities* and *physical structures* (including logic and language) in order to foster "[. . .] reducing these [experimental electric and magnetic] phenomena into scientific form [. . .]" (Maxwell 1865, 459, line 6). Mathematical techniques of derivatives and integration are the instruments to link new mathematical results to the new physical *process of reasoning*.

Finally, Maxwell mainly based his work on Faraday's experimental genius regarding how science works and characteristics of scientific methods, together with Lagrange's mechanics. Maxwell's formidable approach, physically and mathematically integrated, constructed a new theory for a new discipline, physics mathematics, crucial for both science and the history (epistemology–philosophy) of science.

> I have confined myself almost entirely to the mathematical treatment of the subject, but I would recommend the student, after he has learned, experimentally if possible, what are the phenomena to be observed, to read carefully Faraday's *Experimental Research in Electricity*.[63]

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[62]It concerned the famous debate (1886) between Peter Guthrie Tait (1831–1901) and Ludwig Eduard Boltzmann (1844–1906). On this, see the editorial *On some question in the kinetic theory of gases. Reply to Prof. Boltzmann* (1888) in Philosophical Magazine, 5s, 25/154. The polemic on the advanced use of mathematics with respect to previous mechanical theories involved many other scholars; leading physicists considered the findings with scepticism.

[63]Maxwell (1873, I, *Preface*, xiii, line 14) (Author's italic).

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Mathematical Physics in Italy in the XIX Century: The Theory of Elasticity

Danilo Capecchi

Abstract In the second half of the nineteenth century there was in Italy an important group of mathematicians who focused their attention on mathematical physics. The most prominent of them were Enrico Betti, Eugenio Beltrami, Gregorio Ricci–Curbastro and some others (Vito Volterra, Carlo Somigliana and Tullio Levi Civita) whose activity persevered for many years in the twentieth century. In this article, I will write about the contribution of this group to the theory of elasticity. The best representative writing on continuum mechanics and elasticity as theories of mathematical physics is presented in the book *Teoria della elasticità* by Enrico Betti. The book is interesting not only for the particular results found but also for its structure which became paradigmatic for the development of subsequent texts on elasticity, not only those in Italian. Betti's interest was concentrated on the mathematical aspects of a physical theory. Physical principles are not discussed; they are only exposed in the most formal way possible. The objective is to arrive, without discussing epistemological or empirical problems, at the formulation and solution of differential equations that rule elasticity, as had become classic in the emerging mathematical physics. Beltrami wrote no complete books on elasticity; however, his contribution to this field was perhaps more original than that of Betti. A similar consideration holds true for Volterra and Somigliana.

1 Introduction

During the nineteenth century in Europe, the gap between mathematics and physics was becoming large. Mathematicians were concerned with the 'new' mathematics in an attempt to make it more rigorous and to solve some theoretical problems;

D. Capecchi (✉)
Department of Ingegneria Strutturale e Geotecnica, University of Rome "La Sapienza", Rome, Italy
e-mail: danilo.capecchi@uniroma1.it

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physicists were instead mainly concerned with experimental activities. In this context a new branch of learning emerged, now called *mathematical physics*. It originated from some physical problems, formulated by means of complex mathematical relations, whose solutions required the mathematical skills possessed only by professional mathematicians. Most of them were not interested in the physical aspects of the problems but only in developing daring mathematical theories which had physical repercussions at most as a secondary objective. This attitude became more pronounced towards the end of the century.

Italy followed other European countries, even though with some delay. The general scientific level in Italy, at the end of the eighteenth century was quite low; this was true for mathematics too. During the Napoleonic period things changed. In particular, in the *Istituto nazionale* of Bologna, by means of Vincenzo Brunacci, the new mathematics reached very enthusiastic people who in the following years were able to spread mathematics and mathematical physics in Italy.

Vincenzo Brunacci (1768–1818) was a follower of Lagrange. He became professor of mathematics in 1790 at the *Scuola marina* in Livorno and in 1798 published his first relevant work in mathematics, *Calcolo integrale delle equazioni lineari* (Brunacci 1798) which was soon followed by other works (Brunacci 1802, 1804). Since 1801 he was professor of *Matematica sublime* in Pavia and transferred his ideas to his students, among which were Antonio Bordoni, Fabrizio Ottaviano Mossotti and Gabrio Piola, the most brilliant mathematicians of the first half of the nineteenth century. Bordoni was an academic and succeeded Brunacci in the chair of mathematics in Pavia; Mossotti after some vicissitudes set up as professor of physics in Pisa; Piola followed a more private career. Brunacci is then at the top of a genealogy collecting all the greatest Italian mathematicians, in a more or less direct way. In Milan: Piola, Brioschi, Tardy; in Pisa: Mossotti, Betti, Dini, Arzelà, Volterra, Ricci Curbastro, Enriques, etc.; in Pavia: Bordoni, Codazzi, Cremona, Beltrami, etc. (Bottazzini 1982, 1989; Reves 1989; Pepe 2007).

However, if it is true that Brunacci was the architect of the redemption of Italian mathematics it should be said that inspiration in mathematical physics came directly from Joseph Louis Lagrange (1736–1813), who even after his departure for Berlin and Paris had maintained contact with Italy, where he was seen as the great compatriot. Suggestions of Lagrange on continuous mechanics, in particular the treatment of internal efforts, gave rise to a line of study completely Italian and in contrast with molecular French mechanics, a line of study that still has great vitality.

2 The Milanese School

Though Piola refused the role of professor at university he however had some brilliant private students among which were Francesco Brioschi (1824–1890) and Placido Tardy (1816–1914). Brioschi was also Bordoni's student and taught analytical mechanics in the University of Pavia. In 1863 he founded the *Politecnico di Milano*, where he worked until his death; here, he taught mainly hydraulics, analytical mechanics and construction engineering. In 1865 he served the Senate of the Kingdom, and in 1884 succeeded Quintino Sella as president of the *Accademia Nazionale dei Lincei*. As a mathematician, Brioschi publicized in Italy various algebraic theories and studied how to solve fifth and sixth degree equations with elliptic functions. Placido Tardy studied in Milan and Paris. In 1841 he was appointed professor at Messina University, but in 1847, escaped to Florence for political reasons. In 1859 he was a private professor of mathematics in his home near Genoa University, of which he eventually became rector. His scientific production concerns mathematical analysis and mainly the fractional order derivation.

2.1 Gabrio Piola

Count Gabrio Piola Daverio (1794–1850) was born in Milan in 1794 in a rich and aristocratic family. He studied mathematics at Pavia University as pupil of Vincenzo Brunacci. In 1818 he edited *Elementi di geometria e algebra* by Brunacci. In 1824 he won a competition organized by the *Regio istituto lombardo* with a long paper concerning applications about Lagrangian mechanics. In 1824 he was offered a chair in applied mathematics at Pavia University, which he refused for family reasons.

In spite of his decision to not follow an academic career, Piola dedicated a large amount of his time to teaching mathematics and with Paolo Frisiani gave regular lessons at his house. A man of high culture, Piola also devoted himself to the history of literature and philosophy. His epistemological view about science in general and mathematics in particular is referred to in (Piola 1825a).

Though Piola was one of the most brilliant mathematical physicists of the nineteenth century he was for a long time neglected (Finzi–Somigliana 1939–1940). His revival is due mainly to Clifford Truesdell, a great supporter of Italian scholars, and Walter Noll, who was well aware of the interaction during the late 1800s of the Italian school with the German school. Piola's influence is still important. It is customary in contemporary mechanics to attribute to Piola: (a) two tensors of stress considered in the not deformed configuration; (b) an important theorem about the field equations based on the virtual work principle (Capecchi and Ruta 2007).

Piola had an empiric and positivist strategy which he applies in a convincing and interesting way to the mechanics of extended bodies. He questions the need to introduce uncertain hypotheses on the constitution of matter by adopting a model of corpuscles and forces. He states that it is sufficient to refer to evident and certain phenomena: for instance, in rigid bodies, the shape of the body remains unaltered. Then, one may use the undisputed equation of balance of virtual work; only after one has found a model and equations based exclusively on phenomena, Piola says, is it reasonable to look for deeper analyses:

> This is the great advantage of Analytical Mechanics. It allows us to put the facts about which we have clear ideas into equation, without forcing us to consider unclear ideas [...]. The action of active or passive forces (according to a well known distinction by Lagrange) is such that we can sometimes have some ideas about them; but more often there remain [...]

> all doubts that the course of nature is different [. . .]. But in the Analytical Mechanics the effects of internal forces are contemplated, not the forces themselves; namely, the constraint equations which must be satisfied [. . .] and in this way, bypassed all difficulties about the action of forces, we have the same certain and exact equations as if those would result from the thorough knowledge of these actions.[1]

Piola's work on continuum mechanics concerned fluids and solids. This work was published in various years (Piola 1825b, 1833, 1836, 1844, 1848, 1856), with the 1833 paper probably being the most relevant one. For this reason it will be commented on in depth. Though it is shorter than the rest it contains nearly all of his most relevant elements and achievements on mechanics.

2.1.1 1833. Meccanica de' corpi naturalmente estesi

The title of the 1833 paper is quite ambiguous because the term 'estesi' (extended) implies both the adjectives 'rigid' and 'deformable' while Piola in the paper studied only rigid bodies. Piola will remain ambiguous throughout the entire paper, and will use a notation that is suitable for deformable bodies as well. To analyze the internal forces, Piola generalizes the approach used by Lagrange for wires or fluids, considering internal forces as reactions of appropriate constraints. In the case of the rigid body these conditions of constraint are those expressed by a mutual invariance between the distance of its points. Piola starts by characterizing rigid motions both globally and locally. Points of bodies are labeled according to two different systems of coordinates. The former refers to axes called (a, b, c) (after Lagrange 1811), rigidly connected with the bodies and mobile in space – reference configuration. The latter system of coordinates refers to axes fixed in space and are denoted by (x, y, z) – present configuration. The equations of rigid kinematics as given below:

$$\begin{aligned} x &= f + \alpha_1 a + \beta_1 b + \gamma_1 c \\ y &= g + \alpha_2 a + \beta_2 b + \gamma_2 c \\ z &= h + \alpha_3 a + \beta_3 b + \gamma_3 c \end{aligned} \tag{1}$$

where, (f, g, h) are the coordinates of a reference point and $(\alpha_i, \beta_i, \gamma_i)$ are the director cosines of (x, y, z), are derived with respect to (a, b, c). Piola so obtains the following relations:

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[1]Ecco il maggiore vantaggio del sistema della Meccanica Analitica. Esso ci fa mettere in equazione i fatti di cui abbiamo le idee chiare senza obbligarci a considerare le cagioni di cui abbiamo idee oscure [. . .]. L'azione delle forze attive o passive (secondo una nota distinzione di Lagrange) è qualche volta tale che possiamo farcene un concetto, ma il più sovente rimane [. . .] tutto il dubbio che il magistero della natura sia ben diverso [. . .]. Ma nella M.A. si contemplano gli effetti delle forze interne e non le forze stesse, vale a dire le equazioni di condizione che devono essere soddisfatte [. . .] e in tal modo, saltate tutte le difficoltàintorno alle azioni delle forze, si hanno le stesse equazioni sicure ed esatte che si avrebbero da una perspicua cognizione di dette azioni (Piola 1833, pp. 203–204).

$$\left(\frac{dx}{da}\right)^2+\left(\frac{dy}{da}\right)^2+\left(\frac{dz}{da}\right)^2=\left(\frac{dx}{db}\right)^2+\left(\frac{dy}{db}\right)^2+\left(\frac{dz}{db}\right)^2$$
$$=\left(\frac{dx}{da}\right)^2+\left(\frac{dy}{da}\right)^2+\left(\frac{dz}{da}\right)^2=1$$
$$\left(\frac{dx}{da}\right)\left(\frac{dy}{da}\right)+\left(\frac{dx}{db}\right)\left(\frac{dy}{db}\right)+\left(\frac{dx}{dc}\right)\left(\frac{dy}{dc}\right)$$
$$=\left(\frac{dx}{da}\right)\left(\frac{dz}{da}\right)+\left(\frac{dx}{db}\right)\left(\frac{dx}{db}\right)+\left(\frac{dx}{dc}\right)\left(\frac{dz}{dc}\right)$$
$$=\left(\frac{dy}{da}\right)\left(\frac{dz}{da}\right)+\left(\frac{dy}{db}\right)\left(\frac{dz}{db}\right)+\left(\frac{dy}{dc}\right)\left(\frac{dz}{dc}\right)=0 \tag{2}$$

To write down the balance equation Piola uses the technique developed by Lagrange in the *Mécanique analytique*, by equating to zero the virtual work of volume (density) forces including inertia forces integrated over the body's volume:

$$\int da\int db\int dc\ \Gamma\mathrm{H}\left[\left(\frac{d^2x}{dt^2}-X\right)\delta x+\left(\frac{d^2y}{dt^2}-Y\right)+\delta y\left(\frac{d^2z}{dt^2}-Z\right)\delta z\right]=0 \tag{3}$$

where, Γ is the mass density and H is the Jacobian of the transformation $(x, y, z)=\mathbf{x}(a, b, c)$ from (a, b, c) to (x, y, z) and $(\delta x, \delta y, \delta z)$, the virtual displacement of the material point at (a, b, c). At this point Piola reminds the reader that the virtual displacement $(\delta x, \delta y, \delta z)$ is not free but it is constrained according to relations (2). To free $(\delta x, \delta y, \delta z)$ from any constraints the Lagrange multiplier method must be used by adding to the integral (3) the integral of constraint relations (2) which, reproducing Piola's original text, assumes the following expression:

$$Sda\ Sdb\ Sdc\cdot A\left\{\left(\frac{dx}{da}\right)\left(\frac{d\delta x}{da}\right)+\left(\frac{dy}{da}\right)\left(\frac{d\delta y}{da}\right)+\left(\frac{dz}{da}\right)\left(\frac{d\delta z}{da}\right)\right\}$$
$$Sda\ Sdb\ Sdc\cdot B\left\{\left(\frac{dx}{da}\right)\left(\frac{d\delta x}{da}\right)+\left(\frac{dy}{da}\right)\left(\frac{d\delta y}{da}\right)+\left(\frac{dz}{da}\right)\left(\frac{d\delta z}{da}\right)\right\}$$
$$Sda\ Sdb\ Sdc\cdot C\left\{\left(\frac{dx}{da}\right)\left(\frac{d\delta x}{da}\right)+\left(\frac{dy}{da}\right)\left(\frac{d\delta y}{da}\right)+\left(\frac{dz}{da}\right)\left(\frac{d\delta z}{da}\right)\right\}$$
$$Sda\ Sdb\ Sdc\cdot F\left\{\left(\frac{dx}{da}\right)\left(\frac{d\delta x}{da}\right)+\left(\frac{dy}{da}\right)\left(\frac{d\delta y}{da}\right)+\left(\frac{dz}{da}\right)\left(\frac{d\delta z}{da}\right)\right\}$$
$$+\left\{\left(\frac{dx}{da}\right)\left(\frac{d\delta x}{da}\right)+\left(\frac{dy}{da}\right)\left(\frac{d\delta y}{da}\right)+\left(\frac{dz}{da}\right)\left(\frac{d\delta z}{da}\right)\right\}$$

$$Sda\, Sdb\, Sdc \cdot E\left\{\left(\frac{dx}{da}\right)\left(\frac{d\delta x}{da}\right)+\left(\frac{dy}{da}\right)\left(\frac{d\delta y}{da}\right)+\left(\frac{dz}{da}\right)\left(\frac{d\delta z}{da}\right)\right\}$$
$$+\left\{\left(\frac{dx}{da}\right)\left(\frac{d\delta x}{da}\right)+\left(\frac{dy}{da}\right)\left(\frac{d\delta y}{da}\right)+\left(\frac{dz}{da}\right)\left(\frac{d\delta z}{da}\right)\right\}$$
$$Sda\, Sdb\, Sdc \cdot D\left\{\left(\frac{dx}{da}\right)\left(\frac{d\delta x}{da}\right)+\left(\frac{dy}{da}\right)\left(\frac{d\delta y}{da}\right)+\left(\frac{dz}{da}\right)\left(\frac{d\delta z}{da}\right)\right\}$$
$$+\left\{\left(\frac{dx}{da}\right)\left(\frac{d\delta x}{da}\right)+\left(\frac{dy}{da}\right)\left(\frac{d\delta y}{da}\right)+\left(\frac{dz}{da}\right)\left(\frac{d\delta z}{da}\right)\right\}. \quad (4)$$

where, (*A, B, C, D, E, F*) are Lagrangian multipliers which are functions of (*a*, *b*, *c*), and S is the integral sign.

The variational problem (3) leads to two systems of definite integrals: one in volume and the other on its boundary. Piola limits himself to the first integral, which with some mathematics, leads to the following equation defined in the reference configuration (variables *a*, *b*, *c*):

$$\Gamma\left[X-\left(\frac{d^2x}{dt^2}\right)\right]+P=0$$
$$\Gamma\left[Y-\left(\frac{d^2y}{dt^2}\right)\right]+Q=0$$
$$\Gamma\left[Z-\left(\frac{d^2Z}{dt^2}\right)\right]+R=0 \quad (5)$$

where *P, Q, R* are functions of the derivative of (*A, B, C, D, E, F*) with respect to (a, b, c).

Piola is however able to project the balance equation (5) into the present configuration (*x, y, z*) by a theorem proved by him which transforms the differential operator in (*a, b, c*) to a differential operator in (*x, y, z*). Eventually he obtains the following equations:

$$\Gamma\left[X-\left(\frac{d^2x}{dt^2}\right)\right]+\left(\frac{dA}{dX}\right)+\left(\frac{dF}{dY}\right)+\left(\frac{dE}{dZ}\right)=0$$
$$\Gamma\left[Y-\left(\frac{d^2y}{dt^2}\right)\right]+\left(\frac{dF}{dX}\right)+\left(\frac{dB}{dY}\right)+\left(\frac{dD}{dZ}\right)=0$$
$$\Gamma\left[Z-\left(\frac{d^2Z}{dt^2}\right)\right]+\left(\frac{dE}{dX}\right)+\left(\frac{dD}{dY}\right)+\left(\frac{dC}{dZ}\right)=0 \quad (6)$$

These equations when compared with the results obtained by Cauchy (1827) and Poisson (1829), allow one to give a mechanical meaning to Lagrangian multipliers

(*A, B, C, D, E, F*): they are the stresses in the present configuration (*x, y, z*). Piola's originality with respect to the French school is clear from the above explanation. He was able to avoid any assumptions about internal forces. His mechanics of continuum is completely identified with mathematical analysis. Only at the end of his memoir does Piola make it clear that his objective was to find the equilibrium equations of the internal forces that coincide with the corresponding equations already found by French scientists.

2.1.2 1848. Intorno alle equazioni fondamentali

The 1848 memoir *Intorno alle equazioni fondamentali* presents an indepth revision of the *Meccanica de' corpi naturalmente estesi*. Piola eliminated inaccuracies and mistakes acknowledged by himself. Indeed during the 12 years after the edition of *Meccanica de' corpi naturalmente estesi* there were great improvements in mathematics (Cauchy and Lacroix created a new theory of integration) and the theory of elasticity (Cauchy and Saint Venant introduced a precise concept of deformation).

Besides the improvements of exposition, *Intorno alle equazioni fondamentali* shows some important novelties: the extension of the equilibrium equations to deformable bodies, the analysis of boundary integrals in the variational equations (3), the particularization of the results to bi- and mono-dimensional continua. Another new concept of importance is that of 'ideal disposition' with uniform and unitary mass density. The introduction of this regular disposition allows Piola to easily develop an interesting and original finite difference analysis and use the modern theory of integration to transform in a rigorous way summations into integrals. In the study of deformable bodies Piola introduces an intermediate configuration χ_p between the ideal and the actual so that the path from '**a**' to '**x**' is decomposed into a path from '**a**' to '**p**' and a path from '**p**' to '**x**'. This allows Piola to overcome the difficulties elegantly and originally to provide internal constraint equations for deformable bodies.

2.1.3 1856. Di un principio controverso

Piola died in 1850; however, in 1856 Francesco Brioschi edited a posthumous writing titled *Di un principio controverso* (on a controversial principle). The controversial principle was the principle of virtual work as formulated by Lagrange. Piola in his paper considered virtual displacements as a change of coordinates. In doing so he is obliged to make use of infinitesimal displacements, a concept that in a previous work he strongly stated should be avoided. Moreover he offers a molecular representation of internal forces and provides a clear and modern interpretation of the measures of deformability.

3 The Pisan School

Ottaviano Mossotti (1791–1863) was the most influential for the Pisa mathematical school. At the Pisa University, the professors with him were Giovanni Battista Amici, Pietro Obici, Luigi Pacinotti, Guglielmo Libri and Gaetano Giorgini.

In 1813 Mossotti started to work at the Brera astronomic observatory. In 1840, during a relaunch of the scientific activity at Pisa University sponsored by the Grand Duchy, Mossotti was called to Pisa to teach mathematical physics and celestial mechanics. In 1848 he made war in Curtatone and Montanara as the chief of the battalion of Pisa University. In 1861 he was appointed senator of the Italian Kingdom.

After some works on hydraulics, Mossotti got his first important results in the field of astronomy. He then considered the problem of intermolecular interactions and arrived at proposing a model that could explain various phenomena and in particular the property of dielectrics. The model according to which the ether surrounding the molecules, by condensation in a asymmetrical way, polarized them, was recovered by Rudolf Clausius, leading to Clausius–Mossotti's equation.

3.1 Enrico Betti

Enrico Betti (1823–1892) studied at Pisa University as student of Ottaviano Mossotti. In 1849 Betti left Pisa to teach in a high school in Pistoia. The relative cultural insulation determined the original characters of his researches on the solution by radicals of algebraic equations. Though Galois' works originated in the 1820s, still in the second half of the nineteenth century they were found hard to be understood even in France.

In 1859 Piola was appointed professor in Pisa. In this period he, along with Brioschi and Casorati, started a long trip to the universities of Göttingen, Berlin and Paris, originating the internationalization of Italian mathematics. They met Dirichlet, Dedekind and Riemann in Göttingen; Weierstrass, Kronecker and Kummer in Berlin; and Hermite and Bertrand in Paris. The most influential for Betti was Riemann.

His admiration for Riemann compelled Betti to ask him to become professor in Pisa in the chair vacated by Mossotti's death. Riemann refused for health reasons; he however spent a couple of years (1863–1865) in Pisa, to improve his health. The presence of Riemann had a great influence over Betti in addressing his interest towards mathematical physics.

In 1863 Betti became editor in chief with Riccardo Felici of the *Nuovo Cimento*, a journal founded by Matteucci, in which he started to publish his papers on potential theory. In 1865 Betti was appointed director of the *Scuola Normale* in Pisa. In 1862 Betti was made a deputy and later became a senator of the Italian parliament.

3.1.1 Betti's Principles of the Theory of Elasticity

Betti explored several aspects of mathematical physics; one of the most important was that regarding classical mechanics. His former works had assumed a mechanistic approach, where force and not energy is the funding concept and virtual work the regulating law. In a previous work on capillarity (Betti 1866), Betti assumed bodies as formed by molecules which attract each other at short distance and repel at very short distance, and which do not practically interact at larger, but still very short distances. In his memoirs on Newtonian forces Betti declared his Newtonian ideology (Betti 1863–1864). Indeed he introduced a potential function, but only on mathematical grounds, as a function from which forces can be obtained by derivation.

Betti changed attitude in a second memoir on capillarity (Betti 1867), by giving the potential an energetic meaning and a founding role, on the basis of Thomson's studies. This change was definitive in the *Teoria della elasticità* (Betti 1874), where no reference is made to internal forces, even avoiding the explicit mention of stress. When Betti wrote the *Teoria della elasticità*, the theory of elasticity was already mature with known principles, though not completely shared. The exposition developed then like the modern handbooks, following the axiomatic approach. Betti's principles are on one hand the concepts of potential energy and strains, and on the other hand the principle of virtual work. Though the book is not particularly original for its theoretical aspects, it is important for the exposition of a general procedure to evaluate the displacements on a three-dimensional elastic continuum and for the solution of specific problems. Moreover, the manner in which the single arguments are presented will become paradigmatic for most handbooks on the theory of elasticity.

Strain. While French scholars defined strain on the basis of geometric intuition, Betti works only on analytical ground and makes use of infinitesimals (generally contrasted by Piola and Bordoni). Strain is defined as the variation of the linear element ds:

$$ds = dx^2 + dy^2 + dz^2 \tag{7}$$

representing the coordinates of the generic point P of the continuum in the actual configuration considered as a function of the coordinate (ξ, η, ζ) of P in the reference configuration. For small strain variation δds is close to Δds and the ratio $\delta ds/ds$ is close to the percentage length variation of ds, which is a natural definition for strain. To identify the single components of strains it is enough to assume suitable values for (dx, dy, dz). For instance, by assuming $(dx = dz = 0)$ the strain component along x is obtained. With some other consideration Betti is able to find the expression of angular distortion as well.

Potential of elastic forces. Betti refers to Thomson's work to provide a thermodynamical interpretation of the potential of elastic forces, which in fact becomes a form of internal energy. Thermodynamics however, at the time, concerned only homogeneous thermal processes, while in continuous mechanics, heterogeneous processes are prevalent. To overcome this difficulty Betti, following Thomson, divided the continuum S into infinitesimal elements dS, each of them considered as homogeneous. Thus, the whole potential energy can be expressed as a summation of all the infinitesimal ones. Therefore if P is the density of elastic potential energy, the whole potential energy for S is given by:

$$\Phi = \int P dS \tag{8}$$

Subsequently Betti, like Thomson and Green, assumes that P depends on strain components of which he neglects any power higher than the second, thereby obtaining the following quadratic expression:

$$P = \sum_{i=1}^{3} \sum_{j=1}^{3} A_{ij} x^i x^j \tag{9}$$

where, $A_{\alpha\beta}$ are constitutive constants and x^α is the generic component of the strain.

The principle of virtual work. Betti writes down the equilibrium equation by means of the virtual work principle which assumes the form:

$$\delta\Phi + \int_S \rho(X\delta u + Y\delta v + Z\delta w)dS + \int_S (L\delta u + M\delta v + N\delta w)d\sigma = 0 \tag{10}$$

where, (X, Y, Z) and (L, M, N) are respectively the external density volume and surface forces acting in S and on its boundary σ. From the variational problem (10), Betti easily obtains the field and surface equations which are expressed without the explicit use of stress components.

Mutual work theorem. The statement of the mutual work theorem is probably the most known contribution made by Betti in the theory of elasticity. It was formulated by him in the following form:

> If in an elastic homogeneous body, two systems of displacements are respectively equilibrated with two systems of forces, the summation of the force components of the former system times the corresponding displacement components of the same points in the latter systems equals the summation of the force components of the latter system times the displacement components of the same points in the former system.[2]

[2]Se in un corpo solido elastico omogeneo due sistemi di spostamenti fanno equilibrio a due sistemi di forze applicate alle superficie, la somma dei prodotti delle componenti delle forze del primo sistema per le componenti degli spostamenti negli stessi punti del secondo è uguale alla somma dei prodotti delle componenti delle forze del secondo sistema per le componenti degli spostamenti nei medesimi punti del primo Betti (1872, p. 40).

which assumes the mathematical form:

$$\int_{\sigma}(L''u' + M''v' + N''w')d\sigma + \int_{S}(X''u' + Y''v' + Z''w')dS$$
$$= \int_{\sigma}(L'u'' + M'v'' + N'w'')d\sigma + \int_{S}(X''u' + Y''v' + Z''w')dS \qquad (11)$$

where, (u, v, w), (L, M, N) (X, Y, Z) are respectively the displacement, surface force density, volume force densities, and the apices distinguish system one $(')$ from system two $('')$.

In fact Eq. (11) is an extension of that referred to in the *Teoria della elasticità*, which was valid only for surface forces. Its derivation is quite simple but very original and interesting; Betti considers this derivation very similar to that carried out by Green, now known as Green's second identity.

The mutual work theorem was used by Betti to find the displacement field, u, v, w of a continuous three-dimensional body subjected to external forces. To present the general theory, Betti dedicates three chapters of his book while some of the chapters are devoted to specific applications, such as the interesting Saint-Venant problem, i.e. the solution of the elastic problem of a homogeneous elastic prismatic solid.

3.2 *Betti's Students*

As already stated, in 1865 Betti became the director of the *Scuola Normale* in Pisa and had numerous students. The concurrent presence, with Betti, of Riemann and Beltrami contributed to making the Pisa mathematical school perhaps the most important in Italy. Among the Pisan students the following must be cited: E. Padova, E. Bertini, C. Arzelà, G. Ascoli, U. Dini, G. Ricci-Curbastro, G. Volterra, V. Cerruti, G. Lauricella, C. Somigliana, S. Pincherle, M. Pieri, F. Enriques, L. Bianchi (Capecchi 2006).

In the following I will refer only to Volterra and Cerruti who followed Betti's footsteps and carried out very interesting works on the theory of elasticity.

3.2.1 Vito Volterra

Vito Volterra (1869–1940) was the most distinguished of Betti's students and one of the most prominent Italian scientists of all time. In 1879 he entered the *Scuola Normale* of Pisa. In 1892 he became dean of the Science faculty. In 1893 he moved from Pisa to Turin for the chair of higher mechanics. He was a member of the *Accademia dei XL* and of the *Accademia delle science* of Turin, and was a corresponding member of various Italian academies. In 1889 he was appointed a member of the *Accademia dei Lincei*. In 1909 he became dean of the faculty of science at Roma University.

In 1922, he joined the opposition to the fascist regime and in 1931 he was one of only 12 out of 1,250 professors who refused to take a mandatory oath of loyalty. As a result of his refusal to sign the oath he was compelled to resign his university post as well as his membership in scientific academies.

It is truly hard to present an idea of Vito Volterra's work, because it spreads over many subjects. His contribution to the theory of elasticity concerned dislocations and elastic hereditary phenomena theories. Physicists had already pointed out the existence of hysteretic behavior, according to which the strains of a body depend not only on the actual stresses but also on all loading–unloading cycles to which the body had been subjected; that is bodies are endowed with memory. Volterra studied in depth the case where the hereditariness was represented by an integral of time, linear in the strain components. The usual partial derivative equations of mathematical physics are replaced by integral equations for which he proposed a general theory of integration.

Contemporaneously to his studies on hereditariness Volterra carried out a theory of dislocations, which now bears his name (Volterra's dislocations), which is a theory of the equilibrium of elastic bodies where coercion states are possible. These are states of tension not due to external forces but due to deformations, caused by addition or subtraction of matter along some surfaces. This occurs for instance when a ring is cut at a normal section and then welded after having a small slice of it removed (Volterra 1905a, b, c, d, 1906, 1907). Weingarten (1901) too considered the possibility of constrictions, but Volterra was the first to elaborate a consistent theory.

3.2.2 Valentino Cerruti

Valentino Cerruti (1850–1909) in 1873 obtained a degree as civil engineer in Turin. Still student he published interesting papers on analytical geometry in Battaglini's journal, *Giornale di matematiche*. In 1873 he became assistant professor of hydraulics in the *Scuola di applicazione per ingegneri* (application school for engineers) in Rome. In 1874 he was appointed lecturer of physics, and in 1881 became full professor of rational mechanics. Subsequently, he was rector of the Rome University, dean of the science faculty, and again rector. Since 1889 he was consultant of the national edition works of Galileo directed by Antonio Favaro. He succeeded to Cremona at the S*cuola di applicazione per ingegneri* and was a member of the *Società italiana dei XL* of other Italian academies.

Cerruti studied dynamics of small amplitude motions of systems impeded by the resistance of the medium. In a seminal work in 1880 he generalized Betti's theorem of reciprocity and its consequences from static to dynamic fields, finding the characteristic integrals endowed with characteristic singularity in space and time (Cerruti 1880). An oversight resulted in a calculation mistake without which he would have obtained, 2 years before Kirchhoff, the mathematical expression of Huygens' principle; Somigliana took over this priority later. Cerruti offered a simpler form to Betti's results on the evaluation of fields of displacement for three-dimensional elastic continua and reduced the number of auxiliary functions to be assigned in advance. He systematically applied these results to isotropic soils, spheres and spherical shells, so this method is known by his name associated with that of Betti, i.e. the *Cerruti–Betti* method (Cerruti 1883). Cerruti also dealt with the calculation of the deformation of an undefined body limited by a plane in the two principal cases in which the displacement of the limit points of the points of the plane or the forces applied to the individual elements of the plane were given.

4 The Pavia School

The Pavia school holds a particular position because it stemmed directly from Vincenzo Brunacci. The immediate Brunacci successor in the chair of *Matematica sublime* was Antonio Bordoni.

Antonio Maria Bordoni (1789–1860) studied mathematics at the University of Pavia with Brunacci. He graduated in 1807 and then was appointed lecturer in mathematics and physics at the military school in Pavia, during Napoleon's occupation of North Italy. In 1817, after the defeat of Napoleon he was appointed professor of elementary pure mathematics at the University of Pavia. In the following year he was appointed to the chair of higher calculus, geodesy and hydraulics.

Bordoni began to study differential geometry as early as the 1820s. When he became acquainted with the work of Liouville and the ideas of Gauss, he encouraged his colleagues and students at the University of Pavia to develop them. In 1860, Bordoni was appointed as a senator of the Kingdom but died 1 month later. Among the publications in the latter part of his career we mention the texts *Lezioni di calcolo sublime* (Bordoni 1831) and *Trattato di geodesia elementare* (Bordoni 1833). He was honoured with election to various academies, including the *Accademia dei XL*.

Among Bordoni's students at Pavia were Felice Casorati, who became his assistant after graduating in 1856. Previously, Francesco Brioschi had been Bordoni's student graduating in 1845 having written a thesis under Bordoni's supervision. Brioschi was appointed professor of applied mathematics at the University of Pavia in 1852, thus becoming Bordoni's colleague. Luigi Cremona was also taught by Bordoni (and by Brioschi), while Eugenio Beltrami, a Brioschi student and the most representative scholar of Pavia School was not able to complete his studies.

4.1 Eugenio Beltrami

Eugenio Beltrami (1835–1900) studied at Pavia University as a student of Brioschi, but he had to discontinue his studies because of financial hardship and spent the next several years as a secretary working for the Lombardy–Venice railroad company. Notwithstanding a low basic qualification in mathematics, Beltrami recovered quickly and was appointed to the University of Bologna as a professor during 1862, the year he published his first research paper, with Brioschi's help. In 1864 he was offered the chair of geodesy at Pisa University. In Pisa he became close with Betti and knew Riemann. From 1891 until the end of his life Beltrami lived in Rome. He became president of the *Accademia dei Lincei* during 1898 and a senator of the Kingdom of Italy during 1899.

Beltrami was essentially a self-taught man. In Pisa he addressed his studies towards the geometry of surfaces, on the footsteps of Gauss, Lobacevskij and Cremona, and made an important contribution to non-Euclidian geometry. He was a great mathematical physicist as well, in particular he found new results in non-Euclidian spaces. Beltrami's differential techniques influenced the birth of tensor calculus creating the basis for the ideas developed subsequently by Ricci–Curbastro and Levi–Civita.

The first organic paper by Beltrami on the theory of elasticity was in 1882 and concerned the equations of elastic equilibrium in a space with constant curvature where a body with volume S and surface σ is present (Beltrami 1880–1882). The problem was studied, as in Piola's, without any assumption about the internal forces.

Beltrami's theory stemmed from the results obtained by Lamé using curvilinear coordinates and from some subsequent works by Carl Neumann and Borchardt. The latter simplified Lamé's calculations with the use of a potential function in curvilinear coordinates (Lamé 1859). According to Beltrami their approach, although they led to correct results, could be improved. Lamé, Neumann and Borchardt formulated the problem in Cartesian coordinates, implicitly assuming a Euclidian space. Beltrami instead proved directly the equilibrium elastic equations without any assumption on the nature of space.

His central idea lies in a suitable metrics and from it of suitable infinitesimal strain measures. He assumes the following expression for the infinitesimal element:

$$ds^2 = Q_1^2 dq_1^2 + Q_2^2 dq_2^2 + Q_3^2 dq_3^2 \tag{12}$$

where, q_1, q_2, q_3 are curvilinear coordinates and Q_1, Q_2, Q_3 are functions of q_1, q_2, q_3 (notice, the metrics will be Euclidian for $Q_1 = Q_2 = Q_3 = 1$).

Beltrami considers six auxiliary quantities θ_1, θ_2, θ_3, ω_1, ω_2, ω_3, depending on $q_1, q_2, q_3, Q_1, Q_2, Q_3$ which allow him to write the equation:

$$\frac{\delta ds}{ds} = \lambda_1^2 d\theta_1 + \lambda_2^2 d\theta_2 + \lambda_3^2 d\theta_3 + \lambda_2\lambda_3 d\omega_1^2 + \lambda_1\lambda_3 d\omega_2^2 + \lambda_1\lambda_2 d\omega_3^2 \tag{13}$$

where, λ_1, λ_2, λ_3 are the cosines of the angles that the linear element *ds* makes with the coordinate axes. Subsequently, he introduces the following expression for the virtual work:

$$\int (\Theta_1 d\theta_1 + \Theta_2 d\theta_2 + \Theta_3 d\theta_3 + \Omega_1 d\omega_1 + \Omega_2 d\omega_2 + \Omega_3 d\omega_3)\, dS \tag{14}$$

where, Θ_1, Θ_2, Θ_3 , Ω_1, Ω_2, Ω_3 are not a priori specified expression, function of q_1, q_2, q_3.

The previous expressions of virtual work allow one to give a mechanical meaning to the terms $\theta_1, \theta_2, \theta_3, \omega_1, \omega_2, \omega_3, \Theta_1, \Theta_2, \Theta_3, \Omega_1, \Omega_2, \Omega_3$: they are respectively strain and stress components. Notice in Beltrami's approach some analogies with Piola's (and Lagrange's). The stress components are defined a posteriori as conjugate quantities of strains. They are simply the coefficients of strains in the virtual work expression. The main difference when compared with Piola is that Beltrami has no problem in speaking about forces from the beginning, thus maintaining his reasoning closer to physics.

The equations obtained by Beltrami solving the variational problem associated with the previous virtual work expression are coincident with those given by Lamé; Beltrami's results are however independent of the V Euclid's postulate.

4.1.1 Congruence Equations

Between 1884 and 1886 Beltrami published three relevant papers on Maxwell's electromagnetic theory (Beltrami 1884a, b, 1885, 1886) in an attempt to furnish a mechanical interpretation to it. For sake of space I will not discuss them, though they are pertinent to the theory of elasticity. I will however refer to the content of a note to an 1886 paper where Beltrami proves that the six equations:

$$\begin{aligned}
&\frac{\partial^2\beta}{\partial z^2}+\frac{\partial^2\gamma}{\partial y^2}=\frac{\partial^2\nu}{\partial x^2} && \frac{\partial^2\alpha}{\partial y\partial z}=\frac{1}{2}\frac{\partial}{\partial x}\left(\frac{\partial\mu}{\partial y}+\frac{\partial\lambda}{\partial z}-\frac{\partial\nu}{\partial x}\right)\\
&\frac{\partial^2\alpha}{\partial z^2}+\frac{\partial^2\gamma}{\partial x^2}=\frac{\partial^2\mu}{\partial y^2} && \frac{\partial^2\beta}{\partial x\partial z}=\frac{1}{2}\frac{\partial}{\partial \mathrm{x}}\left(\frac{\partial\nu}{\partial z}+\frac{\partial\lambda}{\partial x}-\frac{\partial\mu}{\partial y}\right)\\
&\frac{\partial^2\alpha}{\partial y^2}+\frac{\partial^2\beta}{\partial x^2}=\frac{\partial^2\lambda}{\partial z^2} && \frac{\partial^2\gamma}{\partial x\partial y}=\frac{1}{2}\frac{\partial}{\partial x}\left(\frac{\partial\nu}{\partial y}+\frac{\partial\mu}{\partial x}-\frac{\partial\lambda}{\partial z}\right)
\end{aligned} \tag{15}$$

which were known as necessary conditions because the six functions α, β, γ, ν, μ, λ of x, y, z represented the components of congruent strains, are also sufficient conditions. Beltrami considered these equations again in two subsequent papers where he presented different proofs (Beltrami 1889a, b).

4.1.2 Beltrami–Michell Equations

Congruency equations were the occasion for Beltrami to write down equilibrium equations for stresses corresponding to congruent strains for an isotropic elastic three-dimensional continuum (Beltrami 1892):

$$\begin{aligned}
&\frac{\partial^2 P}{\partial x^2}+C\Delta p_{xx}=0; && \frac{\partial^2 P}{\partial y^2}+C\Delta p_{yy}=0; && \frac{\partial^2 P}{\partial x^2}+C\Delta p_{zz}=0\\
&\frac{\partial^2 P}{\partial y\partial z}+C\Delta p_{yz}=0; && \frac{\partial^2 P}{\partial z\partial x}+C\Delta p_{zx}=0; && \frac{\partial^2 P}{\partial x\partial y}+C\Delta p_{xy}=0
\end{aligned} \tag{16}$$

where, Δ is the Laplace operator, p_{ij} are the stress components, $P = p_{xx} + p_{yy} + p_{zz}$, and C is a constitutive constant. Equation (16) is valid in the absence of body force; its generalization to the case of body forces is due to Michell, and the so-called Beltrami–Michell equations were obtained.

4.1.3 Works on Structural Mechanics

Beltrami studied the theory of elasticity mainly to solve problems in the electromagnetic field. There are however at least two works dedicated specifically to structural mechanics; one concerning the strength of materials and the other concerning the equilibrium of membranes.

Strength of materials. In Beltrami's times there were two criteria for verifying the resistance of a body under a three-dimensional state of stress, recalled by Beltrami at the very beginning of his paper on the subject (Beltrami 1885). They limit either the maximum stress or the maximum strain. Beltrami suggests a method which accounts for both strain and stress by posing a limit to strain energy density. Beltrami's criterion had good success at the beginning but was soon replaced by a criterion which limited only the distortion part of the strain density energy, thereby providing more consistent results for structural materials such as steel.

Equilibrium of thin shells. In an 1882 work, Beltrami studied non- deformable in the plane thin shells (Beltrami 1882), in Leocorn's footsteps, with the aim of clarifying that Mossotti's assumption of normal stress equal in all directions is inconsistent. According to the same variational approach of the work of 1882, Beltrami presented the virtual work expression of the external forces for a thin shell of surface σ and boundary s:

$$\int (X\delta x + Y\delta y + Z\delta z)\, d\sigma + \int (X_s\delta x + Y_s\delta y + Z_s\delta z)\, d\sigma \tag{17}$$

where the force components are represented in a Cartesian system of coordinates. Internal forces are not given a physical characterization but are considered simply as Lagrange multipliers for the rigidity in plane constraints, which are stated by the metric of the linear element:

$$ds^2 = E du^2 + 2F du dv + G dv^2 \tag{18}$$

where u and v are the curvilinear coordinates on the membrane; and E, F, G are known functions of u and v, depending on the local curvature. On the basis of the rigidity condition they can be expressed as $E = \text{const.}$, $F = \text{const.}$, $G = \text{const.}$, so that the variational equilibrium equation has the form:

$$\begin{aligned} &\int (X\delta x + Y\delta y + Z\delta z)\, d\sigma + \int (X_s\delta x + Y_s\delta y + Z_s\delta z)\, ds \\ &\frac{1}{2}\int (\lambda\delta E + 2\mu\delta F + \nu\delta G)\, \frac{d\sigma}{H} = 0 \end{aligned} \tag{19}$$

where, λ, μ, ν are three Lagrangian multipliers depending on u, v; and H is a normalizing function. By solving this variational problem, Beltrami obtained the field and equilibrium equations in the intrinsic coordinate system u, v from which it is clear that the Lagrange multipliers, up to a scale factor, are the stress components and that λ and ν (the normal stresses) are in general different from each other, contrary to Mossotti's assumption.

4.2 *Beltrami's Students*

4.2.1 Carlo Somigliana

Carlo Somigliana (1860–1855) was one of the greatest Italian scientists at the turn of the nineteenth and twentieth centuries. He was a student of Beltrami and Casorati in Pavia and of Betti and Dini in Pisa where he got his degree in 1881. He was given a chair in mathematical physics in Turin where he remained for all of his working time. His name is connected to important results on the theory of elasticity and on the potential theory. In the years 1906 and 1907 Somigliana published the integral formulae fundamental for elastic dynamics. Just a short time later he published important results on elastic dislocations and found under general assumptions that dislocations other than Volterra's exist in simply connected bodies.

In 1891 Somigliana discussed the integration of the equations of mathematical physics by means of 'simple solutions', which play the role of Green's functions, obtained with a generalization of Betti's mutual work theorem (Somigliana 1891b). In a paper of 1888 Somigliana had improved Betti's formulae and had been able to avoid the introduction of the cubic dilatation Θ. Somigliana's formulae were subsequently improved by Lauricella who eliminated all 'superfluous' terms (Lauricella 1895, 1909).

Still in 1891 he made a further attempt, following Beltrami's, to consider ether as an elastic and homogeneous fluid without 'strange' physical properties (Somigliana 1891a). He deduced a stress state in ether corresponding to the field displacements given by Maxwell's equations, and proved that an elastic medium exists, the deformation of which gives this system of stresses. This is an interesting result though not conclusive, because it is not possible to deduce from it the electric interaction between two conductors. Somigliana also reconnected to Volterra's dislocations in very interesting papers (Somigliana 1908, 1914–1915; Di Pasquale 1996).

4.2.2 Ernesto Cesaro

Ernesto Cesaro (1859–1906) studied at the *École de mines* in Liege with his brother. He had difficulty in being enrolled in an Italian University as he lacked a secondary-school diploma. He was successful in 1883, however, without being able to obtain a degree. In 1887 he was given an 'ad honorem' degree at the Science faculty of

Rome University. He taught *Algebra complementare* and mathematical physics in Palermo. In 1891 he was appointed to a chair of infinitesimal calculus at Naples University, where he also taught higher analysis.

He wrote some textbooks and several notes and papers. His production is limited in continuous mechanics. His most relevant work is definitely his *Introduzione alla teoria matematica della elasticità* (Cesaro 1894a), which contains the Palermo lectures on the theory of elasticity. The textbook has prevalently a didactic scope and is influenced by Betti's *Teoria della elasticità*; differently from Betti, however, Cesaro introduces the concept of stress. The last part of the book is very interesting from a theoretical point of view as well, being connected to the study of non-Euclidean space, already considered in a previous memoir (Cesaro 1894b).

In 1906 Cesaro presented an original paper on the evaluation of displacements for an elastic half space. The novelty was the use of a perturbation approach (Cesaro 1906).

5 Conclusion

In the second half of the nineteenth century Italian mathematical physics reached results of excellence in Europe, especially in the field of continuum mechanics and elasticity theory. The main protagonists were Gabrio Piola, Eugenio Beltrami and Enrico Betti. They brought out a highly innovative and purely Italian line of research connected to Lagrange's work on continuous systems in the *Mécanique analytique*. The main tool of analysis was the principle of virtual work that allowed definition of stresses, the internal forces, as dual magnitudes of deformations and to address without difficulties, with the exclusion of the analytical ones, complex problems like those of a continuum embedded in three-dimensional non-Euclidean spaces, curved plates and large displacements of bodies.

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The Interaction of Physics, Mechanics and Mathematics in Joseph Liouville's Research

Jesper Lützen

Abstract As many of his contemporaries did, Joseph Liouville often emphasized the importance of physics for mathematical research. His own works provide a host of examples of interactions between mathematics and physics. This paper analyses some of them. It is shown how Laplacian physics gave rise to Liouville's theory of differentiation of arbitrary order, how Kelvin's research on electrostatics gave rise to Liouville's theorem about conformal mappings and how the theory of heat conduction gave rise to Sturm-Liouville theory. It will be shown how the problem of the shape of the planets was an important inspiration for Liouville's far reaching studies of Lamé functions and spectral theory of a particular type of integral operators. Finally the interactions between Liouville's work on mechanics and differential geometry will be discussed.

1 Introduction

When the École Polytechnique celebrated its centenary in 1894 it published a volume containing short biographies of the most prominent of its professors. In the biography of Joseph Liouville (1809–1882) Herman Laurent (1841–1908) recalled:

> I have often heard Liouville say that it is to the study of natural phenomena and in particular to mechanics that mathematics owes its most important developments, and this truth certainly manifests itself in the memoires of this illustrious mathematician.[1]

[1] J'ai souvent entendu Liouville dire que c'est à l'étude des phénomènes naturels et, en particulier à la Mécanique, que les Mathématiques doivent leurs développements les plus importants, et cette vérité se manifeste certainement à la lecture des mémoires de cet illustre géomètre (Laurent 1894, 132).

J. Lützen (✉)
Department of Mathematical Sciences, University of Copenhagen, Copenhagen, Denmark
e-mail: lutzen@math.ku.dk

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The applied view of mathematics that Laurent attributed to Liouville was quite typical of French mathematicians of the early nineteenth century. It was reflected in the educational system: The highest level of mathematics teaching was offered at an engineering school, the École Polytechnique. Most French research mathematicians were trained there and many later became professors at the school or at one of the more applied engineering schools. Liouville was no exception. He studied at the École Polytechnique from 1825 till 1827 and then continued his engineering training for three more years at the École des Ponts et Chaussées. Before he entered the École Polytechnique, Liouville's mathematical tastes seem to have been rather pure. At least the mathematical notes he confided to his first notebook (Liouville MS 3615(1)) deal with projective geometry. But during his student years he acquired a taste for more applied areas of mathematics and for mathematical physics.

Yet his interest in applications was purely scientific rather than practical. "My taste and my work make industrial applications repugnant to me" he wrote to his friend Jean Daniel Colladon (1802–1893) when the latter suggested that Liouville replace him as professor of rational mechanics at the École Centrale des Arts et Manufactures (Pothier 1887, 120). With these words he wanted to stress that, while he was interested in the rational mechanics post, he did not want to replace Colladon as a teacher in the course on steam engines. Liouville was in fact entrusted with the mechanics course at this more practical engineering school, but his teaching was criticized for being too theoretical and so he left the job after a few years (Lützen 1990, 29–30). His interests and teaching style were more suited for the École polytechnique where he taught analysis and mechanics from 1831 (from1838 as a professor) till 1851.

It is well known that the nineteenth century witnessed a gradual shift from an applied view of mathematics to a purer view that emphasized the autonomy of mathematics. This change was partly a stylistic change. Indeed, similar problems were treated in a paper by Michel Chasles (1793–1880) with the applied title: "Énoncé de deux theorems généraux sur l'attraction des corps et la théorie de la chaleur" (Chasles) and in a paper by Hermann Amandus Schwarz (1843–1923) with the pure sounding title: *Ueber die Integration der partiellen Differentialgleichung* $\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} = 0$ *unter vorgeschriebene Grenz-und-Unstetigkeitsbedingungen* (Schwarz 1870). But the change also reflected a neo-humanistic and particularly German conviction that "mathematics exists solely for the honor of the human mind". This quote is often attributed to Liouville's friend Carl Gustav Jacob Jacobi (1804–1851).

As far as his publications are concerned, Liouville took part in this movement from applied toward purer mathematics. Indeed after 1856 he published exclusively in the purest branch of mathematics: number theory. His interest in this area was reinforced by his other close German friend Peter Gustav Lejeune Dirichlet (1805–1859). In his post-1850 research on quadratic forms Liouville was even a purist compared to Dirichlet, in the sense that he tried as far as possible to avoid analytic arguments.

However Liouville's late publications on quadratic forms do not seem to reflect a change of view on his part concerning the nature of mathematics. Indeed, Laurent's previously quoted recollections of Liouville's emphasis on physics and mechanics as the major sources of inspiration for mathematics stem from the end of Liouville's life when Laurent was his student. If we disregard the years after 1856, where Liouville's mathematical creativity seems to have diminished, his output was throughout his life a mix of pure and applied mathematics, reflecting the title of the journal he created in 1836 and edited for 39 years: *Journal de mathématiques pures et appliqués.*

As far as the interaction with physics is concerned, one can divide Liouville's mathematical works in three classes:

1. Mathematics of natural science
   (a) Electrodynamics
   (b) Heat
   (c) Various subjects of mechanics:
      - Rotating masses of fluid
      - Hamilton–Jacobi mechanics
      - Potential theory (gravitation and electrostatics)
2. Pure mathematics inspired by natural science
   (a) Differentiation of arbitrary order (fractional calculus)
   (b) Sturm–Liouville theory
   (c) Geometry
3. Purely pure mathematics
   (a) Integration in finite form
   (b) Transcendental numbers
   (c) Doubly periodic functions
   (d) Galois theory
   (e) Number theory

In this paper I shall discuss the first two classes of works where there is a strong interaction between physics and Liouville's mathematics. That does not mean that there is no relation to physics in the works labeled purely pure. For example Liouville showed how elliptic integrals were of use in celestial mechanics (Liouville 1836) but his theory of doubly periodic functions (Liouville 1880) does not seem to have grown out of this application. Similarly Liouville's theory of integration in finite form occasionally deals with integrals and differential equations that turn up in physical applications, but the problem of integration in finite form itself seems to be of a purely mathematical nature.

2 Laplacian Physics and Differentiation of Arbitrary Order

Liouville's first major contribution to mathematics was a comprehensive theory of differentiation of arbitrary order, i.e. a theory of the meaning and properties of differential operators of the form $(d/dx)^{\mu}$ where μ is any arbitrary complex number. Since this theory is particularly useful when μ is a fraction, the theory is nowadays known under the name fractional calculus.

From the time of the invention of the differential calculus several mathematicians had contemplated the meaning of the symbol $(d/dx)^{\mu} f(x)$. Liouville referred to Leibniz, Euler, Laplace, Lacroix and Fourier but correctly pointed out that "the geometers whom I have quoted only deal with this matter in passing and have not gone deeper into the theory" (Liouville 1832, 71). Niels Henrik Abel (1802–1829) had also used fractional derivatives in a study of a generalization of the brachistochrone problem. However, his use of fractional calculus was only presented in a paper in Norwegian (Abel 1823) and did not make it into the German version (Abel 1826). Thus it is almost certain that Liouville did not know of Abel's earlier work in this area.

Liouville introduced his theory about differentiation of arbitrary order in a paper entitled *Sur quelques questions de géométrie et de mécanique, et sur un nouveau genre pour résoudre ces questions* (Liouville 1832). The mechanical problems mentioned in the title were all of the following kind: It is supposed to be known how the interaction between two macroscopic bodies varies with changing distance between the two bodies and the problem is to determine a law of interaction between their microscopic parts that will give rise to this macroscopic interaction.

This type of problem was of great importance in Laplace's research program in physics (see Fox). Inspired by the success of the Newtonian theory of universal gravitation Pierre Simon Laplace (1749–1827) had insisted that all physical phenomena should be explained as the result of interactions at a distance between the molecules of matter and imponderable fluids. When the fundamental law of attraction between infinitesimal molecules became known, the interaction between two macroscopic bodies could be found by integrating the interaction over the bodies. However, before this "synthetic" determination of macroscopic interactions could take place, one needed to determine the microscopic force laws. Since one cannot make experiments with microscopic entities, one needed to determine the microscopic force laws analytically from their integrated macroscopic consequences that could be determined experimentally. This is precisely the kind of question that Liouville addressed in his first paper on differentiation of arbitrary order. For example, assuming that the force between the molecules or mass elements dm_1 and dm_2 of matter is a central force depending only on the distance r between the molecules, Liouville showed how this force $f(r)dm_1dm_2$ could be determined if one knows one of the following interactions as a function of distance:

- The attraction between two parallel lines, one finite and the other infinite,
- The attraction between a parallelepiped and a mass element,

- The attraction between two parallelepipeds,
- The attraction between a circle and a mass element on its circumference for varying radii.

Liouville had encountered such problems as a student when he followed the course of 1826–1827 at the Collège de France on electrodynamics taught by his teacher of analysis and mechanics at the École Polytechnique André-Marie Ampère (1775–1836). Ampère's theory of electrodynamics has often been described as a reaction against Laplacian physics. However, it shared an important characteristic in common with Laplace's program, namely the idea that the macroscopic interactions were due to microscopic interactions at a distance. In Ampère's theory it was not molecules or microscopic elements of an imponderable fluid that interacted but rather infinitesimal elements of conducting wires. The elementary force between two conducting elements was supposed to depend not only on the distance between them but also on the mutual orientation in space of the two elements. This is where Ampère broke with Laplace's ideas.

Ampère designed several experimental setups from which he could determine the elementary force between two conducting elements. Of course all the experiments had to deal with the interaction between two macroscopic closed circuits. Thus Ampère claimed that it was an experimental fact that a finite linear conductor of length l and a parallel infinite linear conductor at a distance y interact with a force that is proportional to l/y. Ampère also argued that the force between two infinitesimal conducting elements of length ds and ds' is of the form

$$ii'dsds'(\cos\varepsilon + (k-1)\cos\theta\cos\theta')\varphi(r) \tag{1}$$

where i and i' denote the currents in the two conductors, k is a constant, r is the distance and ε the angle between the two elements, θ and θ' are the angles that the elements make with the line connecting them and φ is a function that must be determined experimentally.

In his lectures at the Collège de France, Ampère had assumed that $\varphi(r) = 1/r^n$ and had shown that $n = 2$ and $k = 1/2$. This information about Ampère's course stems from notes taken by Liouville and corrected by Ampère himself (Lützen 1990, 263–272). Having completed the lecture notes, Liouville continued to make additions to Ampère's arguments. For example he derived Ampère's elementary force law under the weaker assumption that $\varphi(r)$ is a series of powers of r (Lützen 1990, p. 274).

Finally in his first paper on differentiation of arbitrary order (Liouville 1832) Liouville tried to derive the same result without making any assumption about the function $\varphi(r)$. The problem is to determine $\varphi(r)$ from the experimental "fact" mentioned above concerning the attraction between an infinite linear conductor and a parallel finite conductor. Integrating the elementary force (1) along the two conductors and equating the result to $\alpha\, l/y$ Liouville found that

$$\int_0^\infty \left(1+(k-1)\,\frac{s^2}{s^2+y^2}\right)\frac{\varphi\left(\sqrt{s^2+y^2}\right)}{\sqrt{s^2+y^2}}ds=\frac{\alpha}{2y^2} \tag{2}$$

which is an integral equation for the unknown function φ. Using the fundamental identity in his new calculus of arbitrary order

$$\left(\frac{d}{dx}\right)^{-\mu}\varphi(x)=\int^\mu \varphi(x)dx^\mu=\frac{1}{(-1)^\mu\Gamma(\mu)}\int_0^\infty \varphi(x+\alpha)\alpha^{\mu-1}d\alpha \tag{3}$$

which holds for functions tending to zero at infinity, he transformed the integral equation (2) into the fractional differential equation

$$\int^{\frac{1}{2}} F(z)d\,(z)^{\frac{1}{2}}+\frac{1-k}{2}\int^{\frac{3}{2}}\frac{F(z)}{z}d\,(z)^{\frac{3}{2}}=\frac{\alpha}{\sqrt{-1}\Gamma\left({}^1\!/_2\right)z}$$

where $y^2=z$ and $\frac{\varphi(y)}{y}=F(z)$. Taking the derivative of order ½ on both sides of this equation he obtained the ordinary differential equation

$$z\frac{dP(z)}{dz}+\frac{1-k}{2}P(z)=\frac{\alpha}{2z\sqrt{z}} \tag{4}$$

for the function

$$P(z)=\int\frac{F(z)dz}{z}.$$

This differential equation has the complete integral

$$P(z)=\frac{C}{(1-k)\,/z^2}-\frac{\alpha}{k+2}\frac{1}{z\sqrt{z}}$$

where C is an arbitrary constant. Reintroducing the original variables Liouville finally found

$$\varphi(r)=-\frac{(1-k)\,C}{2r^{-k}}+\frac{3\alpha}{2\,(k+2)}\frac{1}{r^2}. \tag{5}$$

Thus he had determined the general form of the fundamental force between two infinitesimal conducting elements, and he had shown that in addition to the $1/r^2$ term in (5) that Ampère had found, the mentioned experimental fact also allowed a second r^k term. As explained by Liouville this term occurs because, when integrated along the two wires in the position they are supposed to have in the experiment, this term will contribute nothing to the force.

All the physical problems Liouville discussed in his first paper on differentiation of arbitrary order were solved along the same lines: Some postulated experimental fact about the interaction between two macroscopic bodies was expressed as an integral equation in a function that expressed the fundamental force between the microscopic elements (of matter, of current, of magnetism). In all cases the integral in the equation was some kind of convolution integral that could be translated into a fractional derivative by way of the fundamental identity (3). By solving the resulting fractional differential equation, Liouville could finally determine the fundamental microscopic force law. He explicitly emphasized the role played by integral equations and fractional calculus in this method of solution:

> The solution of most physico-mathematical problems basically depends on a question similar to those we have dealt with, namely the determination of an arbitrary function placed under the integral sign [. . .] thus the properties of differentials of arbitrary order are linked with the most useful mathematical theories.[2]

So it is rather clear that it was physical applications that led Liouville to develop his theory of differentiation of arbitrary order. More specifically, Ampère's electrodynamic theory had introduced him to the fundamental problem of Laplacian physics, namely the problem of determining the fundamental microscopic force laws from their macroscopic consequences. He discovered that this question led to integral equations and he discovered that these equations could be transformed into differential equations of fractional order. This seems to have led him to develop a theory of differentiation of arbitrary order.

So we can conclude that it was a physical research program, Laplacian physics, and its extension to Ampère's electrodynamics, that led Liouville to develop his first major contribution to mathematics. The physical origin even had a clear impact on the details of the theory. For example many of Liouville's theorems were based on the fundamental identity (3) and thus required that the functions involved tend to zero at infinity. This requirement was natural for Liouville who used the theory to investigate potentials and force functions that naturally have that property. However, it is not a necessary requirement in a theory of differentiation of arbitrary order and it had not been made by Liouville's precursors (see Ross 1977). Liouville also proved results that are valid under other conditions (Lützen 1990, p. 208). Still, his preference for formulas valid for functions tending to zero at infinity was clearly dictated by the applications.

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[2]La résolution de la plupart des problèmes physico-mathématiques dépend au fond d'une question semblable à celle que nous venons de traiter, et de la détermination d'une fonction arbitraire placée sous le signe $\int$ come la fonction $\varphi(x)$. Ainsi les propriétés des différentielles à indices quelconques se lient aux théories mathématiques les plus épineuses et les plus utiles (Liouville 1832, 15).

3 Electrostatics and Inversions in Spheres

In 1845 the young William Thomson (1824–1907) (later ennobled as Lord Kelvin) visited Paris where he had many conversations with Liouville concerning many different subjects in mathematics and mathematical physics. In particular, Liouville encouraged Thomson to write a paper comparing Faraday's new ideas on electromagnetism with traditional French electrodynamics. Also electrostatics was a subject of conversation. Thomson had brought a copy of an *Essay on the application of mathematical analysis to the theories of electricity and magnetism* (Green 1828) by George Green (1793–1841) along to Paris. When he presented it to Liouville, the latter immediately sensed its importance and helped Thomson to spread the news of this hitherto obscure essay. It led Liouville to take up some of his previous potential theoretic ideas that I shall return to below. Here I shall explain how Liouville's and Thomson's discussions and correspondence about electrostatics led the former to his important theorem about conformal mappings of space.

While in Paris, Thomson told Liouville about a method he called the method of electrical images. It is a transformation that maps an electrostatic problem into another problem that might be easier to solve. The transformation maps a point P in space into another point P' on the line OP, where O is a fixed point, in such a way that $OP \cdot OP' = R^2$.

This means that it maps the interior of the sphere with center O and radius R to the space outside the sphere and vice versa. For that reason the transformation is normally called an inversion in the sphere.

After Thomson had returned to Cambridge he sent a letter on the new method to Liouville who immediately published it in his journal (Thomson). The following year Liouville published a new paper by Thomson on the subject, and this time he added a long *Note sur deux lettres de M. Thomson relative à l'emploi d'un système nouveau de coordonnées orthogonales dans quelques problèmes des théorie de la chaleur et de l'électricité, et au problème de la distribution d'électricité sur le segment d'une couche sphérique infiniment mince* (Liouville 1847a).

Among the many analytic and geometric theorems Liouville proved in this note was the following: Any transformation T from space into space which satisfies the condition

$$\left|T(x) - T(x')\right| = p^2(x)p^2(x')\left|x - x'\right|$$

for a sufficiently nice function p, can be composed of similitudes and inversions in spheres. He also pointed out that such transformations are angle preserving or conformal. This, he noted, is the three-dimensional analogue of the property of drawing of geographical maps. In a footnote he then showed that the conformal maps of the plane (considered as the complex plane) are precisely the holomorphic functions. This theorem had been implied but not explicitly stated in a paper by Carl Friedrich Gauss (1777–1855) (Gauss 1825). The following year Liouville formulated the problem of finding a similar characterization of the conformal maps of space, but as he admitted in (Liouville 1850b, p. 616) he did not know the answer at that time. However in 1850 he announced in his journal (Liouville 1850a) that any conformal mapping can be composed of a similitude and an inversion in a sphere. This surprising theorem now bears Liouville's name. The proof was published later the same year as one of several notes that Liouville added to his new edition of Monge's *Application de l'analyse à la géométrie* (Liouville 1850b, 614). It was based on the results of Gabriel Lamé (1795–1870) concerning orthogonal coordinates. In this way Thomson's research on electrostatics led Liouville to his famous geometrical theorem on conformal mappings of space.

4 Theory of Heat and Sturm–Liouville Theory

As most French mathematicians of his generation, Liouville had a lively interest in the mathematical theory of heat conduction in solids. His earliest research dealing with heat exchange at a distance between molecules of matter bears the stamp of the Laplacian approach promoted by Siméon Denis Poisson (1781–1840), but after his memoire on the subject was sharply criticized by Poisson himself he adopted a more agnostic approach to the nature of heat, similar to that of Joseph Fourier (1768–1830). His first published paper on the subject (Liouville 1830) dealt with conduction of heat in an unequally polished bar. Fourier and Poisson had dealt only with homogeneous and equally polished materials, so Liouville was in virgin territory. He set up the differential equation describing the problem and used the method of successive approximations to establish that there is a stationary solution of the problem. This was the first use of the method of successive approximations, predating Picard's use of it by 60 years. He then tried to deduce various properties of the solution from the series arising from the successive approximations, but his arguments were far from rigorous, even with the standards of the day.

However, he did much better when he returned to the subject in 1836. At that time he had become aware of the work of his friend Charles François Sturm (1803–1855). In the period 1829–1835 Sturm had approached the more general question of heat conduction in an inhomogeneous bar. He set up the partial differential equation for the temperature function and after separating variables[3] arrived at the following ordinary differential equation:

$$\big(k(x)V'(x)\big)' + (g(x)r - l(x))\,V(x) = 0 \quad for \quad x \in (\alpha, \beta)$$

and the boundary conditions

$$\begin{aligned} k(x)V'(x) - hV(x) &= 0 \quad for \quad x = \alpha \\ k(x)V'(x) + HV(x) &= 0 \quad for \quad x = \beta. \end{aligned}$$

[3]The method of separation of variables in a partial differential equation had been introduced in its totality by Fourier (see Lützen 1887).

Here *k, g* and *l* are "arbitrary" positive functions representing the physical properties of the material, *h* and *H* are positive constants and *r* is a parameter that must be determined in such a way that there exist non-trivial solutions to the boundary problem.

Problems of this kind had been solved earlier by Fourier, but only in special cases, where the functions *k, g* and *l* were simple and specified functions. Sturm wanted to study the solutions in the general case where these functions were supposed to be known but entirely general in nature. In that generality he was faced with the same problem that met Liouville in 1830: One cannot find a manageable expression of the solution. Instead Sturm had the entirely novel idea of deducing the properties of the solutions directly from the differential equation itself and without knowing an expression of the solutions. The properties he could deduce were qualitative in nature, such as: the behavior of the zeroes and the oscillations.

In particular he deduced that there exists a denumerable increasing sequence of values of $r(r_1, r_2, r_3, \ldots)$ (later named eigenvalues) for which there exist non-trivial solutions $(V_1, V_2, V_3, \ldots)$ (later named eigenfunctions). Moreover the *n*th eigenfunction has $\mathrm{n}-1$ zeroes and the zeroes of an eigenfunction separates the zeroes of the following one. Sturm's results were published in two long papers in the first volume of Liouville's Journal (Sturm 1836a, b).

Armed with Sturm's qualitative results, Liouville could now prove that any "arbitrary" function on the interval (α, β) could be expanded in a generalized Fourier series i.e. as an infinite sum of eigenfunctions. In fact he gave two proofs of increasing generality. In the first proof (Liouville 1837a) he assumed the function to be twice differentiable (and to satisfy the boundary condition), which gave him a uniform bound for the terms of the series. In the second proof (Liouville 1837b) he assumed only that *f* be piecewise continuous and piecewise monotonic. Using an estimate of the asymptotical behavior of the eigenfunctions he reduced the problem to the problem of convergence of ordinary trigonometric Fourier series where the assumptions allowed him to appeal to Dirichlet's rigorous convergence proof of 1829 (Dirichlet).

Liouville also tried to generalize his and Sturm's results to differential equations of higher order, but only got partial results. For example he tried without success to prove that the Fourier series of an arbitrary function converges. There are good reasons for his lack of success. Indeed, the Fourier series converges only for very special functions (see Haagerup's appendix in Lützen 1984). One can speculate why Liouville's otherwise strong intuition failed him here. Perhaps it was the purely mathematical nature of the higher-order equations. Liouville had made a generalization just for the sake of generality, and so the equations did not possess the physical content that the second-order equations did. Thus one may argue that Liouville's intuition about mathematics, at least in this case, seems to have had a physical origin.

Sturm–Liouville theory marks the beginning of a new and qualitative approach to differential equations. Where earlier mathematicians had been interested in finding formulas expressing solutions of differential equations, Sturm and Liouville focused on qualitative and conceptual problems such as existence of solutions,[4] the number and behavior of their zeroes and their monotonicity and the possibility of expanding an arbitrary function on the set of eigenfunctions (completeness). As we have seen, it was the generality of the physical problem that Sturm and Liouville asked themselves that forced them to give up the old formula-based approach and initiate the new qualitative approach. And when the qualitative theory of differential equations was continued at the end of the century by Henri Poincaré (1854–1912) it was also a physical problem that prompted it: the three-body problem of celestial mechanics (Kline, 730–738).

5 Celestial Mechanics

In some biographies of Liouville it is stated that he did research in statistical mechanics. Considering that this branch of physics did not emerge until the end of Liouville's life, this claim is clearly unfounded. The root of the misunderstanding is the important role played by the so-called Liouville's theorem concerning the constancy in phase space of the volume of a domain under a Hamiltonian flow. This theorem does go back to a paper by Liouville (1838), but in this paper it is presented as a special case of a theorem about the behavior of a particular functional determinant formed from a solution of a specific type of differential equations.[5] But despite its pure formulation, the theorem had an applied origin. However, the origin was not in the theory of heat but in celestial mechanics. Liouville had noticed that, when one tries to solve perturbation problems by the method of variation of the arbitrary constants, one was often led to a particular functional determinant. He therefore studied its behavior as a function of time and showed that it was a constant for a large class of differential equations. In his publications Liouville did not mention that Hamilton's equations are among the differential equations for which Liouville's theorem holds, but soon Jacobi (Jacobi, §7) made use of the theorem in this connection with due credit to his friend Liouville. Finally, Ludwig Boltzmann (1844–1906) in 1871 showed its applicability in statistical mechanics (Boltzmann 1871). In this paper he derived Liouville's theorem anew, but after Kirchhoff in his lectures on heat had called attention to Liouville's priority, Boltzmann named the theorem Liouville's theorem in his influential Vorlesungen über Gastheorie (Boltzmann 1896–1898).

Where Liouville's theorem "on the volume in phase space" was just a small byproduct of Liouville's engagement in celestial mechanics, the problem of the figure of the planets led Liouville to many groundbreaking investigations of equilibrium figures of rotating masses of fluid and further to results concerning Lamé functions and spectral theory of integral operators. Much of this research was

[4]Cauchy had already emphasized this problem in his lectures at the École polytechnique (Cauchy).
[5]See Lützen (1990, 657–665) for more details.

never published in Liouville's lifetime but can be reconstructed partially from the many pages he wrote in his notebooks (Liouville MS) about the subjects.

In 1740 Colin Maclaurin (1698–1746) had shown that an ellipsoid of revolution (i.e. having a circular equator) could be an equilibrium figure of a rotating mass of fluid. The general opinion was that such Maclaurin ellipsoids were the only possible figures of equilibrium, and Joseph Louis Lagrange (1736–1813) even gave a "proof" of this opinion in his *Mécanique Analytique* (Lagrange 1811–1815, I, §199–204). Thus it came as a great surprise when Jacobi in 1834 showed that there also exist rotating three-axial ellipsoids (i.e. having an elliptic equator) that are in equilibrium.

Liouville immediately jumped on this new result and gave a proof of it from the formulas in Laplace's *Mécanique Céleste* (Liouville 1834). Eight years later C.O. Meyer, a student of Jacobi's, published a paper on the behavior of the Maclaurin- and the Jacobi-ellipsoid as a function of their angular velocity, and Liouville followed suit with an investigation of their behavior as a function of the physically more important angular momentum (Liouville 1843). He found that for angular momenta below a certain value there exists only a Maclaurin ellipsoid in equilibrium and no Jacobi ellipsoids. However for momenta above this value there exist both a Maclaurin and a Jacobi ellipsoid in equilibrium. What was even more surprising was that, for those values of the angular momentum where there exist both a Maclaurin ellipsoid and a Jacobi ellipsoid in equilibrium, it is the latter that is in a stable equilibrium. The Maclaurin ellipsoid is stable only for smaller values of the angular momentum. This was in contradiction with a general belief that had been expressed earlier by James Ivory (Ivory, 65–66).

Liouville published this main result of his research in the Comptes Rendus (Liouville 1842) but never found time to publish his proof. However his notebooks allow a reconstruction of his path to the theorem (Lützen 1990, 483–512). Liouville's idea was to develop a perturbation of the shape of the ellipsoid as a series of ellipsoidal harmonics or Lamé functions. In this connection he developed a great number of new identities for such functions and the so-called Lamé functions of the second kind that he introduced for this purpose. These identities and their proofs were published later in two lengthy papers of 1846 (Liouville 1846a, b). It is somewhat ironic that when Poincaré later used these results in his discussion of the shape of rotating masses of fluid (Poincaré 1885), he had no idea that Liouville had derived them with the purpose of studying the exact same problem. The only one of Liouville's successors who mentioned Liouville's results was Aleksei Mikhailovich Liapounoff (1857–1918) but he regretted that except for the few short mentions of the main results "I have not been able to find any other indications of Liouville's research in this area" (Liapounoff, 7).

In 1842 Liouville also discovered that his formulas for Lamé functions allowed him to solve a special case of a problem posed by Gauss. In (Gauss 1840) the prince of mathematics had analyzed the following variant of the later Dirichlet problem: Given a function U on a closed surface S, determine a charge distribution on the surface giving rise to the potential U on the surface. Liouville realized that if the surface is an ellipsoid he could solve the problem by developing the given function U on the system of Lamé functions. Moreover he saw that the reason for this is that the Lamé functions satisfy a particular integral identity and in 1845 he discovered that he could find similar functions on an arbitrary closed surface S. They just have to be eigenfunctions of the integral operator

$$A(\varsigma) = \iint_S \frac{\varsigma(x')}{|x - x'|} l(x') d\omega'$$

where l is a function on the surface representing the equilibrium distribution of charge and x and x' are points on the surface and one integrates with respect to x' (Liouville 1845a).

In a series of notes from 1845 to 1847 Liouville undertook an extended study of these eigenfunctions (for details see Lützen 1990, p. 601–635). Except for one short note (Liouville 1845b) in which he showed that a more general type of symmetric operator has only real eigenvalues, he did not publish the results of his investigations. However his notes give a clear impression of a very visionary research program that anticipated many results by about 40 years. For example he showed how one can determine the eigenfunctions by a variational method that is now called after Lord Rayleigh (1842–1919) and Walter Ritz (1878–1909) who used it 30 and 60 years later respectively (Rayleigh; Ritz). This variational method, which is a generalization of the Dirichlet principle, suggested the existence of eigenfunctions. Liouville further proved the orthogonality of eigenfunctions (although he did not use such geometric language) and proved that if the Fourier series of a function defined on the surface converges, then it converges towards the function one expands. However, much to his regret he could not prove convergence of the Fourier series for a general class of functions. This and perhaps an uneasiness concerning Dirichlet's principle (that he questioned in his notes long before Weierstrass' famous counterexample of 1870) might have convinced Liouville that he could not publish his results.

In this respect his successor Poincaré showed less restraint when in 1896 he published results similar to those of Liouville without having a rigorous proof of the existence of his so-called fundamental functions that are similar to Liouvilles eigenfunctions (Poincaré 1896, p. 118 ff). Thus the problem of equilibrium shapes of rotating masses of fluid led Liouville to a vast area of investigation that resulted in new insights into the problem itself and to new results concerning Lamé functions. Combined with Gauss's study of electrostatics it led Liouville further to a rich study of spectral theory of a particular type of integral operator, where his results were far ahead of his time.

6 Rational Mechanics and Geometry

As a last example of how physics and mathematics were connected in Liouville's work, we shall have a look at his closely interwoven works on rational mechanics and differential geometry.

His main inspiration in the domain of rational mechanics came from Jacobi whose ideas he helped introduce to a French audience. He wrote many papers on Hamilton–Jacobi mechanics. In one of his more celebrated papers (Liouville 1855), he proved that if one knows half of the integrals to Hamilton's equations and they are in involution (have mutually vanishing Poisson brackets) then the rest of the integrals can be found by quadrature. This theorem is now named after Liouville. However, instead of explaining the circumstances surrounding Liouville's discovery of this theorem (Lützen 1990, 670–679), I shall discuss the process leading to the so-called Liouvillian integrable mechanical systems.

In 1839 Jacobi discovered that he could determine the equation of a geodesic on an ellipsoid when the ellipsoid is equipped with ellipsoidal coordinates. His discovery was published immediately but the proof was only given in 1844 by Liouville (1844). In the proof Liouville considered the geodesic as the trajectory of a particle moving on the surface of the ellipsoid without being influenced by external forces. From Lagrange's equations he derived the differential equation for the geodesics, and due to the special orthogonal nature of the chosen coordinates this equation could be separated, yielding a solution by quadrature.

Liouville immediately generalized this idea to the motion of a point mass moving on a surface under the influence of forces of a particular kind. He equipped the surface with his favorite type of orthogonal coordinates, so-called isothermal coordinates i.e. coordinates α, β for which the line element has the form

$$ds^2 = \lambda(\alpha, \beta)(d\alpha^2 + d\beta^2) \tag{6}$$

If C describes the total energy of the point mass and the applied force is described by a potential energy U such that $(C - U)\lambda$ can be separated as follows,

$$(C - U)\lambda = f(\alpha) - F(\beta),$$

then Liouville could easily solve the equations of motion in the same way as in the case of geodesic motion on an ellipsoid. Thus in such cases the motion of the point could be determined by separation of variables and quadrature. He published this result in (Liouville 1846c) and then generalized it first to a point mass moving in space (Liouville 1846d, 1847b) and then to a system of point masses (Liouville 1847c). Around 1900 it was shown that, in a sense, such Liouvillian systems exhaust the systems whose motion can be found by separation of variables.

As exemplified by this sequence of events, much of Liouville's work of mechanics was closely related to his differential geometric research on geodesics. I shall conclude by pointing out a particularly farsighted connection that arose in connection with Liouville's work on the principle of least action. In a course of 1850–1851 at the Collège de France devoted to differential forms, Liouville discussed the use of such forms in mechanics (Lützen 1990, 751–755, 684–686). He pointed out that if a surface is equipped with isothermal coordinates (6) then, according to Jacobi's version of the principle of least action, the trajectory of a point mass of total energy C moving in a conservative force field with potential U minimizes the action integral

$$A = \int \sqrt{(C - U)\,\lambda\,(d\alpha^2 + d\beta^2)}.$$

In his lectures Liouville remarked that the minimization of this integral would likewise lead to the trajectory in a plane of a point influenced by a force with potential $(C - U)\lambda$. Thus one can change one mechanical problem into another one by simultaneously changing the force and the geometry. In the same way one can get rid of all forces by interpreting the minimization of the action integral as a way to obtain geodesics on a surface with the line element

$$ds^2 = (C - U)\lambda(d\alpha^2 + d\beta^2).$$

Liouville did not point out this ultimate geometrization of forces, but in 1856 the analogy between trajectories of a mechanical system and geodesics on a surface suggested to him that a method used by Ludwig Schläfli (1814–1895) to transform the line element of a geodesic into a sum of squares of which the first is an exact differential (Schläfli 1847–1852) could be generalized to the motion of a general system of point masses. This led him to a new derivation of the Hamilton–Jacobi formalism, and to the introduction of what has later been called action–angle coordinates (Liouville 1856).

Liouville's ideas concerning the analogies between trajectories and geodesics were carried further by Rudolf Lipschitz (1832–1903) (Lipschitz) and Gaston Darboux (1842–1917) (Darboux; see in particular the preface). The latter pointed out that these analogies showed the connection between Gauss's ideas in differential geometry and Jacobi's ideas in mechanics. In a certain way, Albert Einstein's differential geometric explanation of gravity in the general theory of relativity is a later development of such ideas. However there is no evidence that Einstein knew of the works of his mathematical predecessors.

7 Conclusion

Liouville's research had many different sources of inspiration. Until 1840 most of the inspiration came from his French peers. After that time he was increasingly inspired by his students, the authors in his journal and his German colleagues, in particular Gauss and his friends Dirichlet and Jacobi.

Until 1856 both his published papers and his unpublished research were to a large degree devoted to applied subjects such as electrodynamics, electrostatics, theory of heat and in particular celestial and rational mechanics. Moreover, many of his pure mathematical works were inspired by physics. This holds true of his theory of differentiation of arbitrary order, Sturm–Liouville theory and differential geometry.

In the latter discipline the interaction went both ways: Mechanical problems and theorems gave rise to new geometric results and geometric results were applied to mechanics.

Thus Liouville's mathematics is a very fine illustration of the close relation that existed between mathematics and physics in France in the middle of the nineteenth century.

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Mathematical Physics in the Style of Gabriel Lamé and the Treatise of Emile Mathieu

Évelyne Barbin and René Guitart

Abstract The *Treatise of Mathematical Physics* of Emile Mathieu, published from 1873 to 1890, provided an exposition of the specific French "Mathematical Physics" inherited from Lamé, himself an heir of Poisson, Fourier, and Laplace. The works of all these authors had significant differences, but they were pursuing the same goal, described here with its relation to Theoretical Physics.

1 The "Mathematical Physics" of Laplace, Fourier, and Poisson

At the end of the eighteenth century, due especially to the work of Lagrange and Laplace, the undisputed centre of European Mathematical Physics was Paris and this situation prevailed until around 1830 (Greenberg, 77). In the following century that predominance declined and even some historians, such as Herivel, considered that no great French theoretical physicist existed in the period 1850–1870 (Herivel, 130).

Nevertheless, Grattan-Guinness observed that, if we view the history of physics as not only the history of conceptual innovation and experimentation, but also as a history of engineering applications, then French physics was very active after 1830: e.g. Navier, Poncelet, Clapeyron, Coriolis (Grattan-Guinness 1990). The question is to decide if inventions in pure mathematics or in engineering are or are not creative physics (Grattan-Guinness 1993). Moreover, Grattan-Guinness contended that, following the very mathematical scheme of the mechanico-molecular method

É. Barbin (✉)
Laboratoire Jean Leray, Université de Nantes, Nantes, France
e-mail: evelyne.barbin@wanadoo.fr

R. Guitart
Institut de Mathématiques de Jussieu, Université Denis Diderot Paris 7, Paris, France
e-mail: rene.guitart@orange.fr

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of Laplace, as was the case in the French school (see hereinafter), caused an increase in the complexity of mathematical topics, after which scientists began to devote more and more energy to mathematical issues, at the expense of experimental investigation.

In Physics at the end of the nineteenth century, in the hands of Poincaré, Theoretical physics was neither a simple application of mathematics, nor, after some mathematical modelling, a final reduction of the understanding of nature to mathematical challenges; its aim was the representation and explanation of observed physical phenomena in nature. In theoretical physics the basic point was to distinguish principles, in a very narrow relation with experimental observations. For Poincaré, in 1904, there were five or six such principles: conservation of energy, energy degradation, equality of action and reaction, relativity, conservation of mass and the principle of least action (Poincaré, 126–127). Mathematical Physics was an analytic framework for the description of physical theories, and also an ideal of understanding. In its broad sense, it was mainly characterized by the use of representations by partial differential equations.

Strict Mathematical Physics is not the same as theoretical physics; in Mathematical Physics, pure analysis and physics are intertwined, in tension with each other—an idea that was clear in the mind of Poincaré, who worked in both areas (Paty). So, with such a distinction in mind and accepting the tension between theoretical physics and Mathematical Physics, we could understand the position of Herivel as a valorisation of "creative" theoretical physics, strongly related to experiences (forgetting this tension), and the one of Grattan-Guinness as a reminder of this tension.

Concerning French theoretical physics and Mathematical Physics, we have to distinguish between two tendencies: one is the mechanico-molecular (Laplace, Ampère, Poisson); the other is the physico-analytic (Fourier). Of course both tendencies use basic mathematics, as well as modelling by partial differential equations, in such a way that physical problems can be solved by integrating these equations.

For the physico-analytic tendency, physical laws derived from observation are of primary importance, and are not to be explained in simpler terms; the primary causes will stay unknown. For instance, for Fourier the theory of heat had to be based on the law of the action of heat, and the subject of heat by itself was a separate branch of physics, with no necessary connection with dynamics and inter-molecular forces for instance. So the phenomenon of heat was a category which cannot be *sui generis* reduced to any other category.

> Whatever is the extension of mechanical theories, they don't apply to phenomena of heat, which are of a specific nature, out of explanation by movements and equilibrium.[1]

The same ideas applied to other categories of physical subjects (light, electricity, and so on):

[1] Fourier, ij–iij.

> [. . .] physics by the variety and complication of its phenomena will always evidently be very inferior to astronomy whatever its future progress may be.[2]

Thus the analytic theory of heat starts with the following argument:

> The actions of heat are related to some constant laws, which cannot be discovered without the help of mathematical analysis. The goal of theory exposed here is to demonstrate these laws; it reduces any physical inquiry, on the subject of heat, to questions of integral calculus on elements given by experience.[3]

For the mechanico-molecular tendency in Lagrange's view, mechanics is at first only reduced to a matter of mathematics, to pure mathematical relations and calculations, "replacing the physical linkages of bodies by equations between the coordinates of their various points", in such a way that a physical problem is reduced to a "point of analysis". Then, against this approach, Laplace returned to a dynamical perspective, based on the use of force. Robert Fox showed that in the period 1805–1807 (Fox, 100), Laplace gave his energies almost completely to investigation in molecular physics. As expressed by Poisson in his *Dissertation on elastic bodies*, read before the Académie on 24th November 1828:

> Beside this admirable conception [of Lagrange] one can now place physical mechanics, whose unique principle is to connect everything by molecular attractions.[4]

Siméon-Denis Poisson was a student of Fourier, a very good friend of Biot, and Arago wrote that Lagrange "assigned to Poisson a place among Huyghens, Newton, d'Alembert, and Laplace" (Arago, 674), and so his evaluation of the issues is of great importance. As quoted by J. R. Hofmann:

> [. . .] the goal for Laplace, Biot and Poisson was to account for physical phenomena in terms of central forces acting between material particles, particles of light, and the other imponderable fluids of heat, electricity, and magnetism.[5]

So, for this laplacian school, the various physical disciplines, not yet explained in the fundamental framework of forces and particles, have to be considered as "sciences of waiting" or "partial theories" (in Lamé's words). So, on the subjects of heat, electricity and light, Lamé wrote:

> It will be against evidence to not admit that these three partial theories progress toward a common unique source, a general theory of which they will appear as corollary or particular chapters.[6]

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[2]Comte, 429.

[3]Les effets de la chaleur sont assujétis à des lois constants que l'on ne peut découvrir sans le secours de l'analyse mathématique. La Théorie que nous allons exposer a pour objet de démontrer ces lois; elle réduit toutes les recherches physiques, sur la propagation de la chaleur, à des questions de calcul intégral dont les elements sont donnés par l'expérience (Fourier, 1).

[4]Quoted in Herivel, 123.

[5]Hofmann, 295.

[6]Lamé (1840, 2–8).

However, between the two attitudes (physico-analytic or mechanico-molecular) Poisson did not choose definitely, and he used both. So he wrote:

> The application of mathematical analysis to questions in physics is based, in each case, on a certain number of laws obtained from observation or, in their absence, on hypotheses that one wishes to verify. The mathematical calculation plays no part in these suppositions. It serves only to develop their consequences to the greatest possible extent, and consequently it offers the best method for comparing theories with experimental results, from all points of view.[7]

Here Poisson was examining the theory of heat, and he adopted a kind of axiomatic point of view (far from the mechanico-molecular approach, and near to Fourier's philosophy), based on three observations on which only the calculations had to be founded (Arnold). In fact, Poisson had the vast project of a complete *Treatise of Mathematical Physics* (not achieved), in which "Pure analysis is not the goal, but the tool; application to phenomena is the essential purpose" (Hermite). In the foreword of the second edition of his *Treatise of Mechanics* of 1833, Poisson wrote:

> Its main goal is to be an introduction to a Treatise of Mathematical Physics, of which my 'new theory of capillary action', published one year ago, is already a part; the other parts will be my various memoirs on equilibrium and movement of elastic bodies and fluids, or on imponderable fluids, that I project to collect and to complete as well as possible.[8]

Later he included his book *Mathematical theory of heat* of 1835 (with *Treatise of Mathematical Physics* as its primary title page) as a chapter of this project (Poisson 1835). In this book he accessed historical references by referring to the work of Lamé on the law of temperatures inside a homogenous ellipsoid with the help of elliptic functions. At the end of the introduction he stated that this book is the second part of the *Treatise of Mathematical Physics*, in which, with no specific order, he planned to consider the various questions in physics to which he could apply analysis. In the previous page he specified that:

> It will be a matter of deducing, by rigorous analysis, all the consequences of a general hypothesis on the communication of heat, founded on experiment and analogy. These consequences will come from a transformation of the hypothesis, to which calculations add or subtract nothing; and then their perfect conformity with observed phenomenon prove without any doubt the veracity of the theory.[9]

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[7]L'application de l'analyse mathématique à des questions de physique est fondée, dans chaque cas, sur un certain nombre de lois données par l'observation, ou, à leur défaut, sur des hypothèses que l'on veut vérifier ; le calcul n'ajoute rien à ces suppositions ; il sert seulement à les développer jusque dans leurs moindres conséquences, et par conséquent il offre le moyen le plus propre à comparer, sous tous les points de vue, les théories à l'expérience (Poisson 1815, 435).

[8]Sa destination principale est de servir d'introduction à un Traité de Physique mathématique, dont la Nouvelle théorie de l'Action capillaire, que j'ai publiée il y a un an, est déjà une partie ; les autres parties se composeront des différents Mémoires que j'ai écrits, soit sur l'équilibre et le mouvement des corps élastiques et fluids, soit sur les fluides impondérables, et que je me propose de réunir et de render aussi complets qu'il me sera donné de le faire (Poisson 1833, *Foreword* (not paginated)).

[9]Il s'agira de déduire, par un calcul rigoureux, toutes les consequences d'une hypothèse générale sur la communication de la chaleur, fondée sur l'expérience et l'analogie. Ces consequences seront

Let us observe that the first sentences of the *Treatise of Mechanics*, the introductory volume of the project, are:

> Matter is whatever affects our senses in any way. Bodies are parts of matter limited in any direction, and so they have a shape and a volume. The mass of a body is the quantity of matter in it.[10]

This basic datum of a body, with the consideration of a potential in it, was the starting point in Lamé's approach.

2 The "Mathematical Physics" of Gabriel Lamé

For Lamé, the thinking of a physical theory is closely linked with the necessity of mathematics, as he explained in his *Courses of physics of the École Polytechnique*, firstly edited in 1836 (Locqueneux). For him,

> [. . .] the laws of a physical theory have to be the corollaries of a single law; but the discovery of this law cannot be other than the result of the reasoning, and it is here that the mathematical analysis becomes essential.[11]

The process consists of starting with a hypothesis, translating it into an algebraic language and obtaining mathematical formulas. As these formulas can indicate new facts that the physicist can verify, it results in incontestable proofs of the reality of the initial hypothesis.

This conception of Mathematical Physics is expressed throughout the *Lessons* written by Lamé from 1852 to 1861 in four books: *Lessons on mathematical theory of elasticity* (1852), *Lessons on the inverse functions of transcendents and isotherm surfaces* (1857), *Lessons on curvilinear coordinates and their applications* (1859) and *Lessons on the analytical theory of heat* (1861). In the beginning of the preface of his *Lessons* on elasticity, he proposed a kind of programme:

> Mathematical Physics, in itself, is a very modern creation, which exclusively belongs to the Geometers of our century. Today this science contains only three chapters, more or less diverse, which are treated in a full rational way; which means they depend only on unquestionable principles and laws. These chapters are: the theory of static electricity on the surface of conducting bodies; the analytical theory of heat; the mathematical theory of elasticity of solid bodies.[12]

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alors une transformation de l'hypothèse meme, à laquelle le calcul n'ôte et n'ajoute rien; et leur parfaite conformité avec les phénomènes observes ne pourra laisser aucun doute sur la vérité de la théorie (Poisson 1835, 5).

[10]Poisson (1833, 1).

[11]Lamé (1840, 8).

[12]La Physique mathématique, proprement dite, est une création toute moderne, qui appartient exclusivement aux Géomètres de notre siècle. Aujourd'hui cette science ne comprend en réalité que trois chapitres, diversement étendus, qui soient traités rationnellement ; c'est-à-dire qui ne s'appuient que sur des principes ou des lois incontestables. Ces chapitres sont: la théorie de

He noticed that the last chapter was the most difficult but also the most useful for industrial practice. He also explained that it will not be long until the Analysis will embrace other parts of general Physics, like the theory of light or the electro-dynamical phenomena.

According to Lamé, "true Mathematical Physics is a science as rigorous as rational Mechanics". So, it is important to distinguish this rational Physics from its applications, which lean on uncertain principles or on empirical formulas. The empirical and partial theories are useful by their applications; they are not yet sciences but they will be soon. So, the purpose of his *Lessons* was to teach the "true Mathematical Physics" to students who will become the future engineers.

Unity, which was regarded as the future of rational Physics, was obtained thanks to Mathematical Physics. In this sense, one of the most important results of Lamé was his theory of curvilinear coordinates, presented in the *Lessons on curvilinear coordinates and their applications* of 1859. In the "preliminary discourse" of these *Lessons*, he explained the necessity of "a Geometry considered from the point of view of Mathematical Physics" (Lamé 1859, v), studying a family of curves linked by a common property. Indeed, the purpose of the science of hydrostatics is to determinate the surfaces of the same level of pressure; in celestial mechanics it is surfaces of equal potential and in the analytical theory of heat it is surfaces of equal temperature. The theory of light introduced the surfaces of waves and the mathematical theory of elasticity introduced three families of conjugate and orthogonal surfaces. From this came the idea of curvilinear coordinates, which is essential in all fields of Mathematical Physics:

> In all these fields, the question is always to integrate or to determine functions, which has to verify one or several partial differential equations of the second order, expressing the physical laws which govern the functions. Furthermore, these functions, or their general integrals, have to satisfy other partial differential equations of the first order, at each point of the boundary of the treated body. But this problem of double integration would be completely inaccessible without the help of a convenient system of coordinates, such that the surface is expressed by the fact that one of these coordinates is a constant.[13]

In some sense, this was close to the conception of Leibniz and his friend Bernoulli of how to solve physical problems such as the catenary, the isochron curve or the bratystochron curve. About the catenary, Leibniz wrote to John Bernoulli in 1695:

l'électricité statique à la surface des conducteurs; la théorie analytique de la chaleur ; enfin la théorie mathématique de l'élasticité des corps solides (Lamé 1852, v).

[13]Dans toutes ces branches, il s'agit toujours d'intégrer, ou de déterminer, une ou plusieurs functions qui doivent verifier une ou plusieurs équations aux différences partielles du second ordre, exprimant les lois physiques qui régissent les fonctions dont il s'agit. Et en outre, ces fonctions, ou leurs integrales générales, doivent verifier d'autres équations aux différences partielles du premier ordre, pour tous les points appartenant à la surface qui limite le corps que l'on veut traiter. Or ce problème de double integration serait complètement inabordable, si l'on ne parvenait pas à rapporter les points du corps à un système de coordonnées tel que la surface, ou les diverses parties qui la composent, soient exprimées par une de ses coordonnées égalée à une constane (Lamé 1859, viii).

> It is not always easy to reduce the [physical] problems to an inverse problem of tangents. For instance, for the research of the catenary, if we did not know a property of its tangents by mechanical theorems in relation to its center of gravity, it would be difficult to find the construction.[14]

So, for Lamé as for Poisson, a physical phenomenon is always the data of a given body equipped with a function. From this point of view, the mathematical theory of curvilinear coordinates plays a major role. It is also one of the most important of Lamé's contributions to the mathematics of surfaces (Chasles 146–149). The mathematics of Lamé constituted an interesting dialectics between algebra and geometry (Barbin).

This introduction of curvilinear coordinates allows us to unify the different physical parts. The function is the potential in the theory of attraction, and the function is the temperature in the theory of heat, but if the temperature is independent of time, then the general partial differential equation of the second order is the same for these two theories. As well, "the theory of potential sways between two analogies: the first one is the hydrostatics and the second is the theory of heat" (Lamé 1859, ix). Finally, the isostatic systems correspond to the orthogonality of three families of surfaces governed by a partial differential equation of fourth order. Lamé wrote the wanted unity with enthusiasm:

> New connection which foresees the future advent of an unique rational science, containing by its formulas, the three fields of applied mathematics, that I defined, and, moreover, the theory of sound waves and light waves, which are not something else than the elasticity in the dynamical state.[15]

In his *Dissertation on the isotherm surfaces in solids* of 1837, Lamé defined a family of "isotherm surfaces" (Lamé 1837) as a family of surfaces

$$\lambda\,(x, y, z) = \lambda_0,$$

such that there exists a function V(x, y, z) for which the values depend only on λ(x, y, z), so that

$$V\,(x, y, z) = V_0$$

is the initial family and V(x, y, z) has a laplacian equal to zero ($\Delta V = 0$). Therefore, V can be considered as a temperature or a thermic parameter. Lamé showed that V can be expressed as a function of λ:

$$V(\lambda) = A \int_a^{\lambda} d\lambda/\varphi(\lambda) + B$$

[14]Leibniz (1989, 190).

[15]Nouveau rapprochement qui fait entrevoir l'événement futur d'une science rationnelle unique, embrassant, par les mêmes formules, les trois branches des mathématiques appliqués, que je viens de définir, et en outre, la théorie des ondes sonores et celle des ondes lumineuses, qui ne sont autres que la théorie générale de l'élasticité dans l'état dynamique (Lamé 1859, x).

with

$$\varphi(\lambda) = \exp\left(\int g(\lambda)d\lambda\right)$$

and

$$g(\lambda) = (\Delta\lambda)/(\lambda' x + \lambda' y + \lambda' \nu + \lambda' z)^2.$$

He emphasized the interest of the curvilinear coordinates in space and the notion of a triple orthogonal system. Three families of surfaces

$$f_1\,(x, y, z) = h_1, \quad f_2\,(x, y, z) = h_2, \quad f_3\,(x, y, z) = h_3,$$

constitute a “triple orthogonal system” if by each point of space, it passes one surface of each family and if at this point these surfaces are orthogonal. Following Dupin’s theorem (Dupin), these surfaces cut each other along the curvature lines. The fundamental theorem is given in the *Dissertation on orthogonal and isotherm surfaces* of 1843: the only isotherm and triple orthogonal systems are the systems of confocal quadrics (Lamé 1843).

In his *Lessons* of 1859, Lamé especially studied the case of ellipsoidal coordinates (Lamé 1859). He introduced three families of surfaces: ellipsoids, hyperboloids with one sheet and hyperboloids with two sheets, which have the respective equations:

$$x^2/\rho^2 + y^2/(\rho^2 - b^2) + z^2/(\rho^2 - c^2) = 1$$
$$x^2/\mu^2 + y^2/(\mu^2 - b^2) - z^2/(c^2 - \mu^2) = 1$$
$$x^2/\nu^2 - y^2/(b^2 - \nu^2) - z^2/(c^2 - \nu^2) = 1$$

with $c > b > 0$ and $\rho > c > \mu > b > \nu > 0$. A point (x, y, z) is located by the three geometrical parameters ρ, μ, ν, which are called “elliptical” or “ellipsoidal coordinates”. He showed that these three families form a triple orthogonal system, then that the three functions written with the help of the elliptical functions:

$$\xi = \int_c^\rho d\rho/\sqrt{(\rho^2 - b^2)}\,\sqrt{(\rho^2 - c^2)},$$

$$\eta = \int_b^\mu d\mu/\sqrt{(\mu^2 - b^2)}\,\sqrt{(c^2 - \mu^2)},$$

$$\zeta = \int_0^\nu d\nu/\sqrt{(b^2 - \nu^2)}\,\sqrt{(c^2 - \nu^2)},$$

are three thermic parameters and this proves that the system is isotherm (Guitart).

3 The Unity of Physics: The Aether and Its Equation

The purpose of Lamé in his *Dissertation on the laws of equilibrium of the aetheral fluid* of 1834 was to find the differential equations of light (Lamé 1834). He noticed that the works of Fresnel confirmed the existence of a universal fluid where the light waves are propagated. But some difficulty remains because the laws which govern the "aetheral fluid" are not known. He explained that, because of the nature of aether, it is not possible to know these laws by experiences. But we can hope to find them by applications of mathematics to the complex phenomena of which these laws are the causes. He proposed to start with two general facts, to take one of them as a "fundamental principle" and to deduce the other one by the calculus.

Accordingly with experiments on interferences of Fresnel on light, he took as a fundamental principle, that "light is caused by vibrations of aether without change of density" (Lamé 1833, 194). As a general phenomenon, he chose the fact that there exist translucent bodies, weighted mediums in which light waves are propagated. He had to calculate the law of distribution of molecules in the neighborhood of a vibrating molecule. Finally, he obtained the differential equations that represent the vibrations of light in the aether and the general equation of the equilibrium of the aether, on the surface or inside translucent bodies. This equation is (Lamé 1834, 213):

$$\mathrm{d}^2\log\rho/\mathrm{dx}^2 + \mathrm{d}^2\log\rho/\mathrm{dx}^2 + \mathrm{d}^2\log\rho/\mathrm{dx}^2 = 0.$$

He integrated the equation with a special case of particular coordinates. For Lamé, the aether is the "truly universal principle of nature", as he wrote in his *Note on the working to follow to discover the only true universal principle of Nature* (Lamé 1863). He explains that

> [. . .] constant works led him to a kind of a new definition of the Mathematical Physics, to the prediction of the true aim towards this general science converges.[16]

He began the paper with a historical outline of six successive fields of work: capillarity, electricity, magnetic, propagation of heat, of light, and elasticity of bodies. From this, he announced three predictions. Firstly, the principles of the first three fields will be reached when we know those of the last three fields. Secondly, the theories of elasticity and light have to merge. Thirdly, there will remain only two theories and Lamé concluded that the true principle of physical nature will come from their fusion.

But the propagation of light in a vacuum and in planetary spaces, and the phenomenon of interferences, unquestionably show the existence of an aetheral fluid. For Lamé, this kind of matter is more universal and active than the weighted matter. So, "[. . .] the future sciences will recognize in the aether the true king of

[16]Lamé (1863, 983).

the physical nature [. . .]"[17] and "[. . .] the cristallography, where Fresnel created the theory of light, will be always the laboratory that we have to choose to further the general science.[18]

Lamé explained that he recently proved new results on propagation of light in translucent bodies by using the theory of elasticity "created by Clapeyron". For him, this "theoretical extension" gave the only rigorous proof of the existence of free aether in translucent bodies.

The "Note" of 1863 shows that Lamé's works on Mathematical Physics and the existence of aether are two faces of the same thought on the unity of physics. This claimed unity, associated with the name of his friend Clapeyron, seems to be an echo of saint-simonian philosophy, which impregnated the thought of the two scientists in their youth (Régnier). Lamé wrote that he thought that he was the only geometer who worked on this kind of questions but many communications of the Académie des sciences showed to him that there exist possible colleagues in Switzerland, in Germany, in Austria. So, he concluded the "Note" by a kind of last will and testament:

> The true tendency of the physical–mathematical work of our century being admitted, it was important to well define the present state and to prepare the future. Most of the workers of the already made work have disappeared and I am the oldest of those who stayed. Before leaving the place, I thought that I have a duty to fulfill, the duty to collect, to purify, to simplify the obtained results, with the aim to facilitate to our successors the completion of the total work. It was the purpose of the four Courses that I published successively. The following Course had to summarize the others, in a more concise but also in a more complete form; but I am conscious of the strength and of the time, which will fail to finish this last Course, which the present note had to serve as an introduction.[19]

4 Emile Mathieu and His Treatise Project

Emile Mathieu was born in 1835 in Metz. He entered the École Polytechnique in 1854 and 5 years later defended before the university La Sorbonne (Paris) a thesis in Algebra on the number of values of a function and on transitive functions (Floquet 2). Throughout his life he worked on many fields of mathematics: algebra, theory

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[17]Lamé (1863, 986).

[18]Lamé (1863, 987).

[19]La véritable tendance de l'oeuvre physico-mathématique de notre siècle étant reconnue, il importait de bien définir son état présent et de préparer son avenir. La plupart des ouvriers du travail déjà exécuté n'existent plus, et je suis le doyen de ceux qui restent. Avant de quitter cette place, j'ai pensé que j'avais un devoir à remplir celui de recueillir, de purifier, de simplifier les résultats obtenus, afin de faciliter à nos successeurs l'achèvement de l'oeuvre totale. Tel a été le but des quatre Cours que j'ai successivement publiés. Le suivant devait les résumer tous, sous la forme la plus concise et en même temps la plus complète; mais je sens que les forces et le temps me feront défaut pour terminer ce dernier Cours, auquel la Note actuelle devait servir d'introduction (Lamé 1863, 989).

of numbers, integral calculus, elliptic functions, celestial mechanics and analytical mechanics. In 1866, Gabriel Lamé was too ill to give the Course on Mathematical Physics at La Sorbonne, and he proposed Mathieu to be his substitute. This proposal was not accepted by the Minister Victor Duruy, but, in 1867, Mathieu became a "free teacher" at La Sorbonne. His Course "Mathematics" was announced in the *Revue des cours scientifiques* as a course on *Methods of integration of Mathematical Physics*.[20] It became the first volume of his *Treatise on Mathematical Physics*. Then he obtained a chair of Professor in the Faculty of Besançon and finally in the Faculty of Nancy in 1873 where he stayed until his death in 1890.

In 1863, Mathieu published a first note on Mathematical Physics, "on the law of liquids through tubes of very small diameter" in the *Comptes rendus de l'Académie des Sciences* (Duhem, 158). During a 20 year period, he published almost 20 papers on Mathematical Physics, where he showed himself to be the continuator of Laplace, Fresnel, Poisson and Lamé, at the same time constructing his own tools and programme and solving difficult problems.

His first paper appeared in 1866 in the *Journal de mathématiques pures et appliquées*, on the dispersion of light. After having mentioned the works of Poisson and Cauchy, Mathieu proceeded in the way shown by Fresnel and Lamé. Fresnel, in his *Dissertation on double refraction* of 1821, obtained the equation of a surface of a wave of light by composition of vibrations in an aetheral fluid. Subsequently, in his book of 1852, Lamé explained that the theory of elasticity has to be applied to light (Lamé 1852). Mathieu wrote: "this work of M. Lamé is the basis of our dissertation" (Mathieu 1866, 51). Indeed, he began to give a new form for the equations of elasticity; later he showed the advantage of this form and then he gave a complete answer for the dispersion of light in uniaxial crystals.

Mathieu came back to the theory of elasticity in a paper of 1868 of the same *Journal*, with the difficult subject of the vibrations of an elliptic membrane. The cases of a rectangular and of an equilateral triangular membrane had already been studied by Lamé, and the case of a circular membrane also by Bourget. Mathieu's purpose was to determine "by the analysis" all the circumstances of the oscillatory motion of a membrane subjected to an equal tension in all directions. He began to describe the results of the experiences obtained by Félix Savart in 1840 for an elliptic membrane, that is two systems of nodal lines, which are ellipses or hyperbolas having the same focus as the ellipse of the membrane. As he wrote about these experiences: "Mathematical Physics has to give an account of the facts of experience" (Mathieu 1869b, 257). To solve "by the analysis" the case of an elliptic membrane, he introduced the differential equation nowadays called "Mathieu's equation" (Mathieu 1868, 146):

$$\mathrm{d}^2\mathrm{P}/\mathrm{d}\alpha^2 + (\mathrm{N} - 4\,\lambda^2\,\mathrm{c}^2\cos^2\alpha)\,\mathrm{P} = 0.$$

[20]The Course was every Monday and Wednesday afternoon and began in November, *Revue des cours scientifiques de France et de l'étranger*, (Yung E, Alglave E 1867–1968, 5, 832).

He arrived at this equation by separating the variables in the equation of a vibrating membrane, written with elliptic coordinates.

After a paper on the motion of the temperature, where he used the curvilinear coordinates of Lamé to solve the difficult problem of solids limited by circular cylinders and lemniscatic cylinders, Mathieu came back to the theory of elasticity, but to give a parallel study on two theories: the theory of potential and the theory of elasticity. It is the start of his important paper of 1869: *Dissertation on the partial differential equation of the fourth order $\Delta\Delta u = 0$ on the equilibrium of elasticity in a solid*. He explained that

$$\Delta \mathrm{v} = 0$$

is the equation of the second order of the potential and that his purpose is to study the properties of the equation of the fourth order

$$\Delta\Delta \mathrm{u} = 0.$$

He wrote

> This equation can be found in Mathematical Physics [. . .] when an homogeneous solid, which has the same elasticity in all directions, is submitted on its surface to tensions which keep it in an equilibrium of elasticity.[. . .] So we understand the interest to study the function which satisfies this equation and we will see that this study will permit one to integrate it.[21]

In the first part of the paper, he gave Poisson's formula for the equation of potential

$$\Delta \mathrm{v} = 0$$

and stated that there always exists one and only one finite and continuous function v with continuous derivatives inside of the surface s, which satisfies $\Delta \mathrm{v} = 0$, and which has a given value on the surface s. He pointed out that this theorem on potential is not easy to prove, but it is almost obvious for the equilibrium of temperature, which is expressed as the same equation $\Delta \mathrm{v} = 0$. He mentioned that Green also proved the theorem by using the theory of electricity. He concluded that "many theorems on the potential became intuitive by substituting for them those of the equilibrium of temperature" (Mathieu 1869c, 384).

[21]Cette équation se rencontre en physique mathématique ; [. . .] quand un corps solide, homogène, et dont l'élasticité est la même dans tous les sens, est soumis à la surface à des pressions qui le maintiennent en équilibre d'élasticité. [. . .] On comprend donc l'intérêt qu'il y a à s'occuper de la fonction qui satisfait à l'équation, et nous ferons voir d'ailleurs que cette étude permettra d'intégrer cette equation (Mathieu 1869c, 378).

In the second part of the paper he came to the equation $\Delta\Delta u = 0$ and gave the concepts of first potential and second potential, already introduced by Lamé in his *Theory of elasticity* (Lamé 1852, 70–71). He considered the potential given by the triple integral

$$v = \int\int\int \phi\ (a,\ b,\ c)\,/\,r\,da\,db\,dc$$

where r is the distance of the point (x, y, z) to the variable point (a, b, c). We have

$$\Delta v = 0 \text{ or } \Delta v = -4\,\pi\,\phi\,(x,\ y,\ z)$$

depending on whether the point (x, y, z) is inside or outside of the volume. Then he considered the function:

$$w = \int\int\int r\,\phi\,(a,\ b,\ c)\ da\,db\,dc$$

and as Lamé he obtained:

$$\Delta w = 2v$$

So, we have:

$$\Delta\Delta w = 0 \text{ or } \Delta\Delta w = -8\,\pi\,\phi\,(x,\ y,\ z)$$

depending on whether the point (x, y, z) is inside or outside of the volume. He called *w* the “second potential” and *v* the “first potential”. He proved that it is always possible to find a function *u* such that

$$\Delta\Delta u = 0$$

inside a surface σ, which is continuous, as well as its three derivatives up to the third order, and such that its value and the value of its Δ are given on the surface. The paper goes on with the boundary conditions which perfectly determine the solution of $\Delta\Delta u = 0$.

The paper *Dissertation on the integration of the partial differential equations of Mathematical Physics* of 1872 can be considered as the first one where Mathieu gave a general conception of Mathematical Physics. He wrote:

> The principal partial differential equations that we meet in Mathematical Physics are
>
> $$\Delta u = 0,\ \Delta\Delta u = 0,\ \Delta u = -a^2 u,\ du/dt = a^2\,\Delta u,\ d^2u/dt^2 = a^2\,\Delta u,$$
>
> where t is time. The function u, which represents a temperature, a potential or a molecular motion, satisfies one of these equations inside a solid limited by a surface σ or inside a plane

> limited by a line σ. Moreover, u and its derivatives of the first order are to be continuous in this space.[22]

The five equations are well known as corresponding to various physical phenomena, but the paper does not recall them systematically. Mathieu began to come back to the parallelism between the two cases

$$\Delta u = 0,\ \Delta\Delta u = 0,$$

with two theorems. The first theorem stated that every function u of x, y, z which satisfies the equation u = 0 inside a volume limited by a surface σ, and which is continuous, as well as its derivatives of the first order, can be considered as the potential of an infinitely thin layer distributed on the surface s. The second theorem stated that every function u of x, y, z which satisfies the equation $\Delta\Delta u = 0$ inside a surface σ, and which is continuous, as well as its derivatives of the first order, is the sum of the first potential of an infinitely thin layer distributed on s and of the second potential of a similar layer distributed on the same surface σ. Mathieu linked the first theorem to the work of Green on electricity, and the second theorem to his own work on elasticity.

The purpose of this paper was to solve all these partial differential equations of Mathematical Physics in solids of any forms. To solve them completely, it is necessary to give one or two boundary conditions. This conception is completely in accordance with the spirit of the Course given at La Sorbonne in 1867–1868. Indeed, in the same year 1872 and in the same *Journal de mathématiques pures et appliquées*, Mathieu wrote a short paper titled *On the publication of a course on Mathematical Physics given in Paris in 1867 and 1868* where he explained:

> The solutions of the partial differential equations in Physics present a particular character, which distinguishes them from the solutions that we meet in the other fields of Mathematics. Generally, the intended functions not only satisfy these equations inside a surface, but, moreover, on this surface they satisfy certain equations that we called boundary conditions [...]. After the analytical form of a problem of Mathematical Physics was well specified, it is obvious that we could substitute for this problem a pure analytical question; but we have to note that in this way we would take off this problem something of its interest and its clarity almost always.[23]

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[22] Les principales équations aux différences partielles que l'on rencontre dans la Physique mathématique sont les suivantes $\Delta u = 0$, $\Delta\Delta u = 0$, $\Delta u = -a^2 u$, $du/dt = a^2\,\Delta u$, $d^2u/dt^2 = a^2\,\Delta u$ dans lesquelles *t* désigne le temps. La fonction *u*, qui représente une température, un potentiel ou un déplacement moléculaire, satisfait à une de ces équations dans l'intérieur d'une corps déterminé par une surface σ ou dans l'intérieur d'une surface plane limitée par une ligne *s*. De plus, *u* et ses dérivées du premier ordre doivent varier d'une manière continue dans cet espace (Mathieu 1872a, 249).

[23] Les intégrations des équations aux différences partielles de la Physique présentent un caractère particulier qui les distingue des integrations que l'on rencontre dans les autres branches des Mathématiques. En general, les fonctions que l'on y cherche satisfont non seulement à ces equations dans l'intérieur d'une surface, mais elles satisfont, de plus, sur cette surface, à de certaines equations que l'on appelle les conditions aux limites. [...] Après avoir bien precise

Mathieu gave historical examples to show that the series given by pure mathematicians are not sufficient because in Mathematical Physics one needs to know when these series are convergent. For instance, for the vibrating-string problem, he compared the solution of d'Alembert without fixing the extremities of the string and the best solution of Daniel Bernoulli. He paid tribute to the "remarkable books" published by Lamé, but he pronounced two criticisms: Lamé dismissed the historical part in his books and he did not clarify the results of his predecessors.

The Course was published in 1873 under the title *Course of Mathematical Physics*. The first sentences of the Preface are:

> The Book that we publish could be used as a first volume of a Treatise of Mathematical Physics, which would involve all that you know as the most rigorous in this field of Mathematics. So we would give to the present volume the title: Methods of integration in Mathematical Physics. The treatises related to this subject are: the Analytical theory of heat of Fourier, the mathematical theory of heat of Poisson and the books of Lamé.[24]

We can conclude from this that, in 1873, Mathieu had the project of a complete treatise in mind and that this first volume is related to the most simple equations of Mathematical Physics. He explained that he will mention the works of all his predecessors:

> We came back on all their works, and we tried to present the state of the Science today in this field of Analysis. We took care to treat successive questions with the greatest uniformity possible, to emphasize the methods and to avoid any calculations, which have no mathematical interest. Indeed, as the field of Science grows, it is necessary to state principles with more clarity and concision and to delete clever calculations and to substitute for them some transformations of those for which we have to give an account.[25]

So, the job of Mathematical Physics was not only to make uniform the different fields of Physics but also to reconcile differences in other works on Physics elaborated during this period. Indeed, Mathieu gave the example of the three successors of Fresnel, namely Mac Cullagh, Neumann and Lamé, who worked without knowing and reading each other.

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la forme analytique d'un problème de Physique mathématique, il est evident que l'on pourrait à ce problème substituer une question d'analyse pure ; mais il convient de remarquer que l'on ôterait par là presque toujours de son intérêt et de sa claret (Mathieu 1872b, 418).

[24]L'ouvrage que nous publions pourrait servir de premier volume à un Traité de Physique mathématique qui renfermerait tout ce que l'on sait de plus rigoureux dans cette branche des Mathématiques. Alors on donnerait au volume actuel ce titre : *Méthodes d'intégrations en Physique mathématique*. Les Traités qui se rapportent à ce sujet sont : la *Théorie analytique de la chaleur*, par Fourier ; la *Théorie mathématique de la chaleur*, par Poisson, et les ouvrages de Lamé (Mathieu 1873, v).

[25]Nous sommes revenus sur tout ces travaux, et nous avons cherché à exposer l'état actuel de la Science sur cette branche d'Analyse. Nous avons eu soin de traiter les questions qui se présentent successivement avec le plus d'uniformité possible, de mettre en relief les methodes et d'éviter tout calcul qui soit sans intérêt mathématique. En effet, à mesure que le domaine de la Science s'agrandit, il faut en exposer les principes avec plus de clarté et de concision et supprimer les calculs habiles pour leur substituer des transformations dont on doit rendre compte (*Ivi*, vii).

The Book *Course of Mathematical Physics* is composed of nine chapters. Chapter 1 follows a historical order with "the use of trigonometric series" for a vibrating-string and for the heat in a solid. Chapter 2 discusses Lamé's works "on isotherm surfaces and on curvilinear coordinates" and Chapter 3 contains new results on "equilibrium of temperatures in indefinite cylinders". Chapter 4 "on linear differential equations of second order" leans on Sturm's ideas but with simpler theorems. The next chapters return to these subjects: "motion of a vibrating membrane and temperatures of cylinders" in Chapter 5; "distribution of a temperature in a sphere" in Chapter 6 with Laplace and Poisson's theorems and "distribution of the heat in an indefinite medium and temperatures of the globe" in Chapter 7 with simpler results than those of Poisson; "on equilibrium of the temperature of the ellipsoid" in Chapter 8. Chapter 9 "on the cooling of the planetary ellipsoid" contains new results of Mathieu. Many of these chapters contain new proofs or new results provided by Mathieu.

5 Mathieu's *Treatise of Mathematical Physics*

In 1883, Mathieu published a book on the theory of capillarity. He presented it as the second volume of a series of volumes constituting a *Treatise of Mathematical Physics*, with a first volume which is the *Course of Mathematical Physics* of 1873. From Floquet, we know that ten volumes had been planned, but at the death of Mathieu in 1890, three volumes had not yet been written: on the theories of light (some handwritten notes exist), on the motion of a gas, on acoustics, etc. (Floquet, 23–24). No book on thermodynamics was planned. So the existing books are: I. *Course of Mathematical Physics* (1873); II. *The theory of capillarity* (1883); III–IV. *Theory of potential and its applications to electrostatics and to magnetism* (1885–1886); V. *Theory of electrodynamics* (1888); VI–VII. *Theory of elasticity in solid bodies* (1890).

We have to notice that Mathieu published another book in 1878, his very interesting *Analytical dynamics* (Mathieu 1878). But this book was not included as a volume in the series of 1883. Perhaps because it is a part of Celestial mechanics and Astronomy, which had not been exactly considered as included in Physics since Lamé. In this book, Mathieu intended to write with a maximum of mathematical analysis and a minimum of principles from geometrical or mechanical reasoning. From the opposite standpoint, he recommended the treatise of Mechanics edited by Résal. He recalled that a nice updating of Lagrange's *Analytical mechanics* is provided in the "excellent Notes" of Joseph Bertrand, but he claimed that now all the old results have to be completely re-evaluated in the light of the discoveries of Poisson, Hamilton, and Jacobi. It is the purpose of this book. According to Mathieu, it did not include statics and hydrodynamics, and it presents a way to emphasize the uniformity of the analytic treatment.

Section 1 on general theorems of dynamics began with Bernoulli's principle, because "all the science of equilibrium is based on the principle of virtual speeds", as Mathieu wrote. Then he continued with d'Alembert's principle, and provided a new simple proof of hamiltonian equations. The main discoveries of Lagrange, Poisson, Hamilton and Jacobi are introduced in the next section. The book includes the theories of movement of material points, of rotations of a solid body and of relative movements. A theory of perturbations is also given, which contains the general formula for the expression of perturbations obtained by Mathieu himself in 1874 in the *Journal de Liouville.* It finished with a study on projectiles in air, in which he integrated the trajectories "without any special hypothesis on the law of air resistance, which is left to the choice of the calculator" (Mathieu 1874, 267).

The volume on capillarity began with an historical introduction on the subject, where Mathieu referred to Borelli (1670) by quoting Poggendorff in his *Histoire de la Physique*, translated into French in 1883 (Poggendorf, 249). But for him, the real mathematical theory began with Young and with Laplace in 1805–1806, and then with Poisson and Gauss. In the five chapters, these theories are presented, criticized, compared with experiments, and often completed and generalized.

Mathieu started with an application of the principle of virtual speeds (as it is expressed in his *Analytical dynamics*) to the description of Laplace's conception of the force of capillarity. Laplace agreed with Hawksbee's conception: in a vertical capillary tube, the water rises with a meniscus which has a higher level than the water outside, and this capillarity can be explained by a special attractive force between two molecules m and M. This force is different from gravity, but it also acts on the line mM as a function F(r), where r is the distance between m and M, depending on the nature of the matter of m and the matter of M. It is noteworthy that in fact F(r) is not exactly known, just as it is assumed that its value is significant and not equivalent to zero for only very small values of r (less than the radius of an activity sphere). More precisely, for Laplace, the hypothesis on F(r) is that

$$\Phi\,(\mathrm{r}) = \int_{\mathrm{r}}^{\infty} \mathrm{F}\,(\mathrm{r})\ \mathrm{dr}$$

is almost null if r exceeds the radius of activity.

Mathieu emphasized that these capillarity forces derived from a capillarity potential. He specified that he "extends the word 'potential' to the case of an attraction which is not as the inverse of the square of the distance" (Mathieu 1883, 9), this potential V being of the form

$$\mathrm{V} = \int\int_{\Sigma} \Phi\,(\mathrm{r})\ \rho\,(\mathrm{M})\ \mathrm{d}\sigma$$

So, by integration, Mathieu obtained the corresponding partial differential equation for the surface of a liquid partially free and partially in contact with a solid:

$$\mathrm{Z} - \mathrm{h} = \mathrm{M}\ (1/\mathrm{R}_1 + 1/\mathrm{R}_2)\,,$$

where R_1 and R_2 are the principal curvatures, expressed by partial differentials of z. Then, he obtained the value of the angle at the junction line, and then the value of the superficial tension. These calculations by Mathieu are always available, even if the density of the liquid is variable.

It results that all the capillarity phenomena could be explained and that these problems are physical problems in Lamé's style: a partial differential equation and some bounding conditions. All the examples observed by Laplace are again examined here and unified and extended as far as possible. There are problems on the rise and depression of a liquid close to the inner wall, on the superposed liquids, on the suspension of a liquid in the air by a capillary tube, on the rise of a liquid by means of a horizontal disc and on the shapes of drops of liquid put on a horizontal plane or suspended. Mathieu gave a new proof of Bertrand's result on the volume of a drop. Perhaps the more important new result of Mathieu is an improvement of the Archimedes' principle, which is that any floating object is buoyed up by a force equal to the weight of the fluid displaced by the object, taking into account the pressure by capillary forces or superficial tension. Mathieu solved this problem for an object of any shape. Before him, this problem was solved by Poisson only in a very special case, as Mathieu wrote:

> In his book [on capillarity], Poisson took the theory with a very difficult point of view, studying the modifications of pressure due to the capillary action on a body embedded in a liquid. Despite his great skill, he solved the problem only in the case of a body of revolution with a vertical axis.[26]

The book is not at all an abstract mathematical book, but, starting with physical principles borrowed from its predecessors, Mathieu constructed a very deep calculus in order to model problems with potentials by partial differential equations. Several difficult mathematical challenges are surmounted, but also physical results are obtained, with real qualitative meanings, and are submitted for checking by experiment. Clearly here, Mathieu is a true physicist and proud to be so. For instance, Mathieu pointed out in 1863 that he was the first to prove that, when a liquid is poured into a capillary tube, there is a very thin layer of motionless liquid near the side surface (Mathieu 1883, 50).

Let us remark that the next year Henri Résal, a student of Poncelet and Lamé, also published a book titled *Mathematical Physics* (Résal 1884), where he claimed that Mathematical Physics originates from the project of Poisson, and that "its starting point has to be considered as the theory of capillarity as formulated by Laplace". In 1888, Résal published a *Treatise of Mathematical Physics* in two volumes (Résal et al. 1888–1889).

[26]Dans son livre [sur la capillarité], Poisson a pris cette théorie par un côté très difficilement accessible. Il étudie en effet dans cet endroit les modifications de la pression, sur un corps plongé en partie dans un liquide, par l'action capillaire. Mais, bien qu'il déploie dans cette recherché la plus grande habileté, il ne parvient à résoudre la question, que pour un corps de revolution dont l'axe est vertical (Mathieu 1883, 4).

In the two following volumes (1885–1886) on the *Theory of potential and its first applications*, Mathieu preferred to separate the exposition of the principles from their applications, to obtain a more rigorous account, as he wrote:

> A lot of problem theorems on potential are interesting for the physical properties that they exhibit; but the separation of these theorems from their applications offers the advantage to exhibit them with a complete rigor. These mathematical results, which were born in Mathematical Physics, are now carried into pure mathematics.[27]

Mathieu began Chapter 1 with the potential of gravitation in 1/r, the calculus of the corresponding laplacian and the Poisson formula. He gave Green's formula for the integration of

$$U\Delta V - V\Delta U,$$

and he pointed out that this formula was employed by Fourier and Poisson for the cooling of a body, a long time before Green wrote his paper on electricity. Then he presented Dirichlet's principle on the minimization of energy. The next chapter is devoted to the potential of a thin layer of matter on a surface and to the problem of determination of the functions that could be expressed as such a potential. He introduced Green's function as the key process for such "inverse" potential problems. Nowadays, these two chapters is still considered as an excellent clear introduction to classical potential theory.

In the third chapter, Mathieu reproduced some of his own results published in the *Journal de Liouville*, on logarithmical potential (in log r), on calorific potential (in cos(ar)/r), on second potential (in r). As Mathieu wrote, the properties of these potentials are analogous to the properties of the potential of gravitation, and they are present in Mathematical Physics. Then he explained how the theory of potential is equivalent to the theory of heat in the case of stable situations of equilibrium of temperature. He also included Duhamel's equation of heat in a crystallized body and the corresponding potential (Duhamel). The final chapter is devoted to the attraction of ellipsoids, Legendre's theorem, and the calculus of the potential of an ellipse, according to Betti.

In the volume on electrostatics, Mathieu referred first to Poisson's works. We also know that Poisson was one of the primary references for Green. Basically, he began as in the case of gravitation, with a potential in 1/r, but with a density of charge, which could be positive or negative. Furthermore, the charges could move in conductors and remain in equilibrium at their surfaces. Mathieu studied the distribution of electricity on a conical conductor and the mutual influences of two

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[27] Une grande partie des théorèmes relatifs au potentiel prennent leur principal intérêt dans les propriétés physiquesq u'ils démontrent ; mais la séparation des premiers théorèmes de leurs applications a cet avantage de permettre de les exposer plus facilement avec une complète rigueur. [. . .] Les resultants mathématiques, qui sont exposés ici, ont pris leur origine dans la Physique mathématique, masi beaucoup peuvent être transportés, ou l'ont déjà été, dans des recherches de Mathématiques pures (Mathieu 1885–1886, *Preface*).

spherical charges. For the theory of dielectrics, Mathieu adopted Maxwell's starting point, but contrary to Maxwell he claimed that the deformation of the dielectric could not be assimilated with the deformation of an isotropic solid (Mathieu 1885–1886, 110). For the theory of magnetism, Mathieu's was Poisson's theory with two magnetic fluids. He also used Coulomb's conceptions: the difference between magnetism and electricity is that, in the case of electricity, there are no free magnetic charges but only magnets (dipoles) and double layer distributions (*Ivi*, 149). With respect to that, Mathieu modified the theory of Poisson, with different physical ideas but with the same equations for magnetic induction (*Ivi*, 155). For instance, he did not admit the division of a magnetic body into separate magnetic particles.

The next volume, published in 1888, was on the *Theory of electrodynamics*. In Mathieu's view, the central idea was a continuation of the volume on potential, namely assuming that if a permanent current goes through a conductor, then its edging surfaces are covered with a double layer of electricity. In Chapter 1, he gave the general principle on the movement of electricity inside a conductor, and, in the other chapters he studied particular results obtained by many physicists. In Chapter 2, he exposed Kirchhoff's and Ampère's results on permanent linear currents, and, in the next chapter, introduced induction according to Weber, Helmholtz, Neumann and Maxwell. In Chapter 4, Mathieu exposed his theory of the double layer, in agreement with Kirchhoff's views. The other chapters are devoted to permanent currents in plates, electric units, movements of electricity in conductors of arbitrary shapes, and finally telegraphic wires.

The final two volumes dealt with the subject of elasticity in solid bodies. On the one hand, the initial purpose was to contribute to the art of engineering, with the approximated calculus of strength of materials. But on the other hand, elasticity is considered as a fundamental question in physics, for instance the elasticity of aether explains the theory of light, the actions of electrical particles and of celestial bodies (Mathieu 1890, 1–2).

Chapter 1 begins with Lamé's determination of the ellipsoid of elasticity at each point of a body (nowadays it is identified with the elasticity tensor), and the differential equations of elasticity are presented in several ways. Chapter 2 ends with a proof that the system of elasticity forces inside a body, even isotropic, cannot be considered as a system of attractions and repulsions among molecules following a function of distances. Saint-Venant's theory of torsion and flexion of cylinders is the purpose of Chapter 3. In Chapter 4, following Lamé, the equations of elasticity in curvilinear coordinates are introduced, but with simpler calculations. Precisely, Mathieu worked with a family of surfaces and its orthogonal trajectories. The following chapters used this point of view. Chapter 5 studied the deformations of thin rods, with a theory more rigorous than the one of Kirchhoff, and the next chapter studies the vibrations of plane membranes. Mathieu recalled that, in his first volume, he studied the vibrations of circular and elliptical plane membranes (*Ivi*, 200). Chapter 7 is devoted to acoustics, with the study of propagation of sound. Chapter 8 explains the vibrations of a curved strip, according to an article of Mathieu in 1882 in the *Journal de l'Ecole Polytechnique*, and the next one, according to the same paper, the vibrations of bells. And finally, Chapter 10 expresses the equilibrium of elasticity of a rectangular prism.

6 Conclusion

In Book III of his *Optiks*, Newton suggested that, as is the case for gravity, magnetism, and electricity, all phenomena in Nature could be explained by various attractions of particles of bodies (Newton 1722, 453). Following him, in the *Exposition du system du monde* of 1796, Laplace began a general study of the main results of the application of analysis to phenomena provoked by molecular actions, which are different from gravity. In the beginning of the nineteenth century in Arcueil's circle, organized by Laplace and Berthollet, this mechanico-molecular view was the first step towards mathematisation of all of Physics. The first important study in this style was the study of capillarity by Laplace in 1806.

In the hands of Poisson, attractions are replaced by their potentials, but for him the potentials are only mathematical artefacts. Later, potentials became more concrete and were considered as direct representations of physical phenomena. For Lamé, and also for Mathieu, a physical phenomenon is precisely a potential function in a given body, with some bounding conditions and satisfying a specific partial differential equation. Then, the initial analysis by forces between particles lost some of their importance, and played a more heuristic part in order to get the potential. Mathieu showed that even a phenomenon such as elasticity could not be explained by mutual attraction of particles alone depending on distances. With Lamé, the field of Mathematical Physics changed on two points. The physical one was the conception of a universal part played by the aether in any phenomena; the mathematical one was the calculus of curvilinear coordinates. But these two points were linked when the differential equation for the density of aether at equilibrium was obtained.

The idea of an inverse potential problem, as introduced and studied by Green, was not truly used in Lamé's works, but it became central to Mathieu's contributions. Mathieu took notice of the works of his predecessors, by using their phenomenological analysis to get a potential and a differential equation. Then, in a very appropriate mathematical manner, he succeeded in solving the mathematical problem, and he finished by coming back to experiments and applications. Several among his contributions are precisely integral representations by calculus of a Green's function.

Whatever these differences, the Courses of Poisson, Lamé and Mathieu were three significant steps in the large project of finding a universal method to solve physical problems, where each theory is compounded of well-structured general principles and an open list of particular problems.

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The Emergence of Mathematical Physics at the University of Leipzig

Karl-Heinz Schlote

Abstract Except for the well-known blossoming of theoretical physics with the group around Werner Heisenberg at the University of Leipzig at the end of the 1920s, the tradition of mathematical physics had been analyzed in only a few aspects, in particular the work of Carl Neumann and his contributions to the shaping of mathematical physics in general and the theory of electrodynamics in particular. However, the establishment of mathematical physics and its strong position at the University of Leipzig, with Neumann as its leading figure in the last third of the nineteenth century, formed important preconditions for the later upswing. That process is analyzed in this article, focusing on the work of Neumann. It includes a discussion of his ideas on the structure of a physical theory and the role of mathematics in physics as well as his impact on the interaction of mathematics and physics.

1 Introduction

Looking back upon the history of mathematics at the University of Leipzig we can state a long lasting tradition of mathematical physics. Mathematicians like August Ferdinand Möbius (1790–1868), Carl Neumann (1832–1925), Leon Lichtenstein (1878–1933), Ernst Hölder (1901–1990), Herbert Beckert (1920–2004) and Paul Günther (1926–1996) were representatives of this tradition.

Gustav Theodor Fechner (1801–1887) and Wilhelm Weber (1804–1891) could also be mentioned. However, the last two were known mostly for experimental results, Fechner for the first experimental confirmation of Ohm's law by exact

K.-H. Schlote (✉)
Universität Hildesheim, Institut für Mathematik und angewandte Informatik, Germany
e-mail: schlote49@yahoo.de

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measurements (published 1831)[1] and Weber for geomagnetic measurements, as well as for constructing and using the first practical long-range galvanic telegraph, in collaboration with Carl Friedrich Gauss (1777–1855). Would it not be more correct to characterize both physicists as experimentalists? Certainly not, since both achieved substantial theoretical work too. This raises such questions as: What does the concept of mathematical physics mean? Can mathematical physics be distinguished from theoretical physics, and what are the differences between the two? It is very hard to answer these questions since the characterization of the concepts varied very much depending on the different times and the persons who gave the characterization. Therefore I will restrict myself to a rough description of mathematical physics at Leipzig that is orientated by the discussion in the second half of the nineteenth century, especially focused on the opinion of C. Neumann: Mathematical physics is understood in general as the mathematical treatment of physical problems and the deductive construction of a physical theory on the basis of existing fundamental principles and models of physical explanation. There is no direct inference concerning experimental practice or the physical explanation of phenomena. In the following we analyse how that mode of mathematical physics emerged at Leipzig in the last third of the nineteenth century. The exposition is divided into four sections. Firstly, the appointment of C. Neumann and some changes in the representation of mathematics at the University of Leipzig are described. Secondly, Neumann's view of mathematical physics will be characterised and then thirdly his investigations into mathematical physics are analysed, especially his treatment of electrodynamics, or how he realized his point of view. The activities of Neumann's colleagues are considered in the fourth section to complete the picture of mathematical physics at the University of Leipzig. It also includes a sketch of the further development there up to the turn of the century, including some consequences of Neumann's activities.

2 Neumann's Appointment at the University of Leipzig

The gradual improvement of the mathematical standard at Leipzig increased after the middle of the century especially in the 1860s.[2] The most important step took place in 1868 with an alternation in both mathematical chairs. At the beginning of the 1860s, mathematics at Leipzig was represented by the full professors Moritz Wilhelm Drobisch (1802–1896), and August Ferdinand Möbius, as well as by the extraordinary professor Wilhelm Scheibner (1826–1908). Lecturers (Privatdozenten) like Hermann Hankel (1839–1873) and Adolph Mayer (1839–1908) supplemented the staff in 1863 and 1865 respectively. In 1866 Drobisch,

[1] Fechner (1831).

[2] A survey of the development of mathematics at Leipzig's university is given in (Girlich and Schlote). For a detailed description of the changes in 1868 see Schlote (2001, 230–234).

who rendered outstanding services in the organization of sciences but did no notable mathematical research, gave up his chair of mathematics and restricted his activities to his professorship of philosophy. His successor to the mathematical professorship was Scheibner. Since Möbius died only some months later the second mathematical chair—to be precise, the professorship for higher mechanics and astronomy—became vacant, too. It is remarkable that in its report for appointment (Denominationsbericht) to the Saxon Ministry the philosophical faculty pointed out only the new synthetic geometry as Möbius' main field of research that had to be covered by Möbius' successor. The faculty did not mention his investigations into the application of mathematics to mechanics and astronomy. Of course, the latter were traditional fields of mathematical physics and did not present a new trend of research as did synthetic geometry, however mathematical physics formed a noteworthy and developing field. The philosophical faculty proposed the following mathematicians to the ministry: (1) Alfred Clebsch (1833–1872) from the University of Gießen, (2) Hermann Hankel (1839–1873) who had left Leipzig and followed an appointment at the University of Erlangen, (3) Carl Neumann (1832–1925) from the University of Tübingen and Richard Baltzer (1818–1887) from the polytechnical school at Dresden. The faculty did not fail to underscore in the proposal for appointment that only Hankel and Baltzer met the faculty's demands in regard to geometry.

The appointment of Clebsch failed, because he had recently accepted a call to Goettingen and did not want to leave that position. After that the ministry ignored the sequence of proposals and appointed the 36-year-old Neumann as full professor of mathematics at the University of Leipzig. Neumann was a well-known representative of higher mathematics[3] and mathematical physics. The emergence of mathematical physics was strengthened by Karl von der Mühll who had become a lecturer of that field in December 1867. Finally the work of Mayer, the second lecturer of mathematics named above, had to be taken into account. Several topics of his investigations into partial differential equations as well as into the calculus of variations had close relations to the treatment of physical problems too. Thus, mathematical physics reached a prominent position in mathematical research. Within a few months it appeared in lectures at Leipzig and held this position from the winter term 1868 onwards for the following decades.

3 Neumann's View of Mathematical Physics

What were the problems that Neumann treated in mathematical physics and what was the point of view upon which he based his investigations? Problems of mathematical physics formed a starting point and a leading thread of Neumann's

[3]Neumann had received high appreciation in this respect for his book about Riemann's theory of Abelian integrals, which opened up for many contemporary mathematicians a path to Riemann's new ideas about multi-valued functions of complex variables (Neumann 1865b).

scientific career. He was a member of the famous mathematical–physical school at the University of Koenigsberg founded by his father Franz Neumann (1798–1895) and Carl Gustav Jakob Jacobi (1804–1851). After solving a problem of mechanics by applying the theory of hyperelliptic integrals in his thesis, Neumann tackled the analysis of the so-called Faraday effect, the turn of the plane of polarization of light by magnetism in his postdoctoral thesis (Habilitationsschrift) in 1858.[4] Five years later he published an extended version characterized as an attempt at a mathematical theory.[5] Neumann considered the theory of light and according to his opinion some basic principles that allowed an explanation of various phenomena had already been formulated. These principles had to be checked and corrected if necessary, and then a mathematical theory had to be established on this basis. Some of the principles that Neumann took as a basis of his investigations about the Faraday effect were the following: Firstly, he assumed that the model used by André Marie Ampère (1775–1836) and Wilhelm Weber (1804–1891) for explaining some kinds of magnetism could be applied. Secondly, the aether or the luminiferous aether was envisaged as a system of particles that could be treated like an incompressible liquid and therefore followed the rules of hydrodynamics. Thirdly, and this was the crucial point, Neumann adopted Weber's law about the force acting between two electrical particles and formulated an analogous formula describing the force that an electrical particle μ exerts on a particle m of the aether:

$$\mu m \left\{ \frac{d\Phi}{dr} + G\frac{d\Phi}{dr}\left(\frac{dr}{dt}\right)^2 + 2G\Phi\frac{d^2r}{dt^2} \right\}$$

(r the distance between μ and m, G a constant).

This argument was based on the picture that there exists an "impact of electrical processes within the body on the movement of the light (particles)"[6] We will not go into further details here apart from the following fact: Neumann's formula included the special function Φ as a main part, the so-called potential or statical potential. In the case of electrical particles the function Φ represents the potential that both particles at rest act on each other (statical potential). However, Neumann did not want to restrict Φ any more than necessary and therefore assumed that Φ was an arbitrary (differentiable) function of the only variable r. This refers to the mathematical means applied by Neumann, the potential theory. Following a common trend of that time he used potential theory very often in his investigations of physical problems. Furthermore, he gave much work to the improvement of the methods of potential theory.

[4] Neumann (1858).

[5] Neumann (1863).

[6] "[. . .] jene elektrischen Vorgänge im Innern des Körpers auf die Lichtbewegung influiren" (Neumann 1863, 5). All translations from the German are mine. They are not word for word but describe only the content in the main.

As a part of his dealing with physical problems Neumann developed and shaped his methodological view on mathematical physics, on the proper structure of a physical theory, and on the tasks mathematicians and physicists had to fulfil in the process of forming such theories. Besides his articles and books there are some documents from the early period of his scientific career in which he expressed his point of view, above all these are his inaugural lectures held at Tübingen in 1865 and at Leipzig in 1869.[7] In the first one, the Tübingen lecture, Neumann tried to draw a picture of the state of physics at that time and considered the theory of light and of heat as well as the theory of electricity and magnetism. Whereas he could state that there exists a theoretical explanation of the phenomena of light and heat, the theoretical description of the electrical and magnetic phenomena was very confusing. Even the fundamental principles themselves were questionable in that domain.[8] Therefore the main task still consisted in detecting those fundamental principles and concepts and this had to be done by the physicists. Neumann clearly determined in general the task of the physicists in this context. They had

> [. . .] to explain all phenomena that occur in nature by as few basic principles as possible, this means by as few inexplicable facts as possible. The larger the number of phenomena that are included in a physical theory and at the same time the smaller the number of inexplicable facts as the basis of the theory the more perfect (complete) is the theory.[9]

Using the concept of two electrical fluids of opposite charge, Neumann sketched the difficulties that occurred in and impeded a theoretical description of this domain of electricity and magnetism. He tried hard to make a substantial contribution to the solution of that problem. In 1868 he published the first results of his research as a mathematical investigation regarding the principles of electrodynamics.[10]

However, Neumann's methodological views on mathematical physics had not come to an end and underwent some changes in the following year after his appointment at Leipzig. He drew the decisive stimulus from Jacobi's lecture on analytical mechanics held in Berlin during the winter term 1847–1848. Neumann became acquainted with the lecture through the notes of his Leipzig colleague Wilhelm Scheibner.[11] The notes caused Neumann to deepen his considerations about mathematical physics in general as well as analytical mechanics in particular.

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[7] Neumann (1865a, 1870a).

[8] Neumann (1865a, 31).

[9] [. . .] alle Erscheinungen, die in der Natur vor sich gehen, auf möglichst wenige Grundvorstellungen, d. i. auf möglichst wenige *unbegreiflich* bleibende Dinge zurückzuführen. Je *grösser* die Anzahl von Erscheinungen ist, welche von einer physikalischen Theorie umfasst werden, und je *kleiner* gleichzeitig die Anzahl der unerklärbaren Dinge ist, auf welche jene Erscheinungen zurückgeführt werden, um so vollkommener wird die Theorie zu nennen sein (Neumann 1865a, 17; author's italic).

[10] Neumann (1868).

[11] Neumann (1869, 257). (As regards the content of that article, Neumann still followed the line of his former publications and inspected the conditions under which the theorem of virtual displacement could be derived in an exact deductive manner.)

Some results could be found in his inaugural lecture in 1869. Publishing the latter one in a leaflet he summarized his point of view in regard to mathematical physics in the preface as follows:

> It should be commonly accepted, that the proper aim of the mathematical science consists in the task to find out as few principles as possible (principles that by the way don't allow a further explanation) from which the general laws of the empirical facts can be derived in a mathematically correct way; in other words to find out principles that are *equivalent* to the empirical facts. If so, then it has to be an absolutely necessary task to carefully think through those principles that occurred already with some confirmation in one of the fields of natural science. Moreover, the meaning of those principles should be, if possible, presented in such a manner that the equivalence with the corresponding empirical facts could be seen.[12]

Neumann continued his comment that he had thought through the principles of analytical mechanics in such a manner and subjected especially Galileo's principle of inertia to a thorough analysis in his lecture. The consequences and the impacts that Neumann's exposition of his review of the principles had in regard to analytical mechanics will not be discussed here in detail. It should only be mentioned that he criticized the inexact formulation of Galileo's principle and cast doubt upon the status of that principle as an axiom. The concept of a "movement along a straight line" was not defined precisely, since the determination of an unchanging frame of reference was missing. He introduced such an "absolute rigid" frame in the form of the body "Alpha" and emphasized that its existence was a necessary assumption, an indispensable concept for the establishment of the theory. A second point of criticism was formed by the concept of uniformity in regard to the movement. Neumann deduced the hypothetical character of Galileo's principle and demonstrated that it cannot be classified as a fundamental principle, being easy and not further explainable. Following Pulte, that lecture marked the beginning of the end of classical mechanics and the classical mathematical philosophy of nature. He put the lecture in a line with other well-known publications that represented instances of the transition from the classical concept of science to the modern one.[13]

However, the quotation above showed that Neumann used analytical mechanics as an example only to demonstrate his ideas about the structure of a theory in mathematical physics and the duties of mathematicians and physicists in constructing

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[12]Wenn das eigentliche Ziel der mathematischen Naturwissenschaft, wie allgemein anerkannt werden dürfte, darin besteht, möglichst wenige (übrigens nicht weiter erklärbare) Principien zu entdecken, aus denen die allgemeinen Gesetze der empirisch gegebenen Thatsachen mit mathematischer Nothwendigkeit emporsteigen, also Principien zu entdecken, welche den empirischen Thatsachen *aequivalent* sind, – so muss es als eine Aufgabe von unabweisbarer Wichtigkeit erscheinen, diejenigen Principien, welche in irgend einem Gebiet der Naturwissenschaft bereits mit einiger Sicherheit zu Tage getreten sind, in sorgfältiger Weise zu durchdenken, und den Inhalt dieser Principien womöglich in solcher Form darzulegen, dass jener Anforderung der Aequivalenz mit den betreffenden empirischen Thatsachen wirklich entsprochen werde (Neumann 1870a, 3; author's italic).

[13]Pulte, 400. For a detailed discussion see Chap. VI, sect 3. In regard to the terms of the "classical" and "modern concept of science" he referred to Diemer (Diemer).

such theories. As stated above, at first the physicists had to find and to formulate fundamental principles and concepts. This formed a necessary prerequisite for further progress.

Being a mathematician Neumann started then from the unexplainable basic assumptions or principles derived by physicists and pursued construction of a physical theory by giving these principles a mathematical formulation and by using an axiomatic approach analogous to the model of Euclidean geometry. He believed that known phenomena and empirical facts as well as general theorems should be deduced in that way. This meant in particular that experiments could be used to test a proposed theory or to decide which of two theories is the more appropriate one. However, according to Neumann's opinion it is not the mathematician's concern to work out such experiments for testing a theory or to carry them out. As a mathematician he focused his attention on the mathematical description of the principles and above all on improvement of the applied mathematical means. He was firmly convinced, as he wrote some decades later in a letter to Otto Wiener (1862–1927), that substantial progress in theoretical physics will be reached only by going through all the known facts and explanations thoroughly and precisely from a mathematical point of view.[14]

Going back to the inaugural lectures, it should still be mentioned that Neumann not only explained his view of the structure of a physical theory but supplemented it in respect to the character of the chosen principles. He pointed out a fundamental difference between the mathematical axioms and the unexplainable principles of a physical theory in regard to his use of an axiomatic approach. The physical principles can never be described as true or probably true, they always embody some uncertainty or arbitrariness and incomprehensibility. This fact could be seen as a deficiency but it cannot be avoided. If we did not found a physical theory on some incomprehensible and hypothetical basic principles but started with propositions that are irrefutably certain and unassailably true, then we are forced to resort to mathematical or logical propositions. However, it was absolutely impossible to deduce a physical theory from such strict formal propositions.[15]

Therefore a theory in mathematical physics differed fundamentally from a mathematical or logical theory. The nature of mathematical physics theories in general was determined by the fact that these theories had to be seen as subjective creations

[14]"Ich [Neumann, K.-H. S.] dagegen glaube, daß wesentliche Fortschritte [in der theoretischen Physik, K.-H. S.] nur in *sehr langer Zeit* zu erwarten sind, und daß dazu in erster Linie eine genaue Durcharbeitung des schon Vorhandenen erforderlich ist" (Library, University of Leipzig, Dep. of Handwritings, N 96 (Legacy O. Wiener), letter from Neumann to O. Wiener, November 29, 1902; author's italic).

[15]Denn wollten wir eine physikalische Theorie nicht von irgend welchen unbegreiflichen und hypothetischen Grundvorstellungen, sondern von Sätzen ausgehen lassen, die den Stempel *unumstösslicher Sicherheit* an sich tragen, die durch sich selber die Bürgschaft *unangreifbarer Wahrheit* bieten, so würden wir gezwungen sein, zu den Sätzen der Logik oder Mathematik unsere Zuflucht zu nehmen. Aus derartigen rein formalen Sätzen eine physikalische Theorie deduciren zu wollen, würde aber ein Ding der Unmöglichkeit sein (Neumann 1870a, 12; author's italic).

of the human mind that (based on arbitrarily chosen principles and deduced in a strong mathematical manner) should present a picture of the phenomena as true as possible.[16]

Finally, arbitrariness in respect to the choice of fundamental principles leads to the consequence that a physical theory will always be incomplete and changeable. It was only possible to appreciate a choice of principles as the best one at a certain time, but not forever.[17]

Neumann's approach to physics agreed with the point of view of some of his mathematician-colleagues, like Clebsch or von der Mühll. However, there appeared several different opinions especially from the side of the physicists. Neumann's debate with Hermann von Helmholtz (1821–1894) about the foundation of electrodynamics that took place in the 1870s is well known. In the following we will sketch how Neumann realized his point of view in his investigations about electrodynamics.

4 Neumann's Investigations in Mathematical Physics – Electrodynamics

Neumann's intensive researches on electrodynamics can be traced back to the 1860s, the time before he came to Leipzig. As he stated in his lecture in 1865 the theory of electricity and magnetism was not well developed. Its fundamental principles were formulated in a very complicated manner, changed very often, and the process of finding appropriate principles had not come to an end. Neumann contributed to this search for the fundamental principles in his first extensive publication in this field titled *Principles of electrodynamics*.[18] He followed the dualistic approach of Wilhelm Weber who assumed the existence of two electrical fluids of opposite charge. Furthermore Weber had proposed a fundamental law describing the electrodynamic force acting between two current elements. On that base he established a theory that could explain many of the electrodynamic phenomena known at that time. Nevertheless Neumann was also familiar with the ideas and concepts of other representatives of continental electrodynamics like Franz Neumann, Hermann Günther Grassmann (1809–1877), André-Marie Ampère, Jean Baptiste Biot (1774–1862) and Félix Savart (1791–1841). I suppose

[16] [. . .] dass diese Theorien angesehen werden müssen als subjective, aus uns selber entsprungene Gestaltungen, welche (von willkührlich zu wählenden Principien aus, in streng mathematischer Weise entwickelt) ein möglichst treues Bild der Erscheinungen zu liefern bestimmt sind (Neumann 1870a, b, 22).

[17] Neumann (1870a, 13).

[18] Neumann (1870a).

that he knew the field-theoretical concept of Michael Faraday (1791–1867) and its "mathematical" description by James Clerk Maxwell (1831–1879), too, but he probably did not appreciate it as a serious theory at that time.

Neumann determined the principle of energy conservation and the introduction of elementary forces of second kind as the clue for establishing a theory of electrodynamics. Elementary forces of second kind depended not only from position in space but also from velocity, acceleration and other circumstances. He successfully deduced the potential of these forces and this opened to him a way to establish the theory of electrodynamics on the base of potential theory. Five years later, in 1873, he gave a full presentation of his ideas in a book entitled *The electrical forces*.[19] He carefully listed the basic assumptions or principles on which a theory of electrodynamics could be based and divided them into four groups according to the degree of certainty ascribed to each group. In this context he differentiated among axiom, principle and hypothesis to stress their different character. This shows a similarity to Euclid's axiomatic method too. He thoroughly analyzed various fundamental assumptions (axioms, principles, or hypotheses), their implications and the logical relationships among them. From his principles he managed to deduce an elementary electrodynamic law describing the two components of the force acting from a current element in one conductor to a point in a second conductor. Neumann believed that this law completed his system of principles. He was able to establish the theory with a clear structure possessing an internal logical consistency and gave an explanation of the electrodynamic phenomena. In the same year he improved his theory once more by proving that he could construct it without using two hypotheses that he had classified as belonging to the most controversial and uncertain ones.[20]

One year later Neumann, with a further publication about the electrical forces in particular Weber's law, marked a temporary end of studies about electrodynamics as well as his disagreement with Helmholtz.[21] He reflected on the previous development and pointed out as a main result of his deductive treatment of electrodynamics that the prevailing ideas had to be separated into two almost independent groups. Each group had to be treated specifically since each of them contained some seeds of the truth. In this context Neumann emphasized again some features of his view on mathematical physics.

> The mathematician has to fulfil an important, invaluable task in the field of physics, which should not be underestimated. This task consists in the thorough investigation of *existing* physical ideas, and the demonstration of all its possible consequences as precisely as

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[19] Neumann (1873).

[20] Neumann (1874). The whole volume of the "Abhandlungen [. . .]" is dated 1874. However, the numbers of the volume were published separately. Neumann's treatise was published as number 6 in 1873.

[21] Neumann (1878). The whole volume of the "Abhandlungen [. . .]" is dated 1878. The numbers of the volume were published separately. Neumann's treatise was published as number 2 in 1874. In regard to Neumann's debate with Helmholtz, Helmholtz's contributions to electrodynamics as well as the development of electrodynamics see: Buchwald; Kaiser 1993; Darrigol 2000, sect. 6.3, 2.2, Appendix 7.

> possible. In fact, it consists in a *deductive* development of these ideas. Such deductive developments will serve, if executed in strict and almost linear direction, to improve the clarity of the respective field; they will contribute to an enhancement of our mental view regarding width and preciseness, and add the confidence and security that will be required for a successful *induction*, i.e. the emergence of *new* and even *better* ideas.[22]

The alternative choice between the two groups of principles underscored impressively the malleability of the theory. Referring to the role of phenomena or experiments as criteria for verifying physical laws, Neumann demanded: "Only such cases whose existence or practicability has been approved can be used to examine a hypothetical physical law."[23]

All in all, to sum up Neumann's voluminous publications of 1873 and 1874, he presented a careful deductive establishment of the theory. However, he did not attain new important results and the discrepancies with other theories could not be resolved within the then current state of knowledge. Besides this, his stubborn adherence to selected ideas on the nature of electrical phenomena had already become evident at this stage, a characteristic that burdened his further research in mathematical physics.

Following his own point of view, Neumann did not search for new experiments that made a decision among theories. This was the duty of the physicists. He looked for an improvement of mathematical methods and shifted to other topics, returning to electrodynamics more than a decade later.[24] In his later works Neumann did not offer any new general aspects or ideas in respect to the structure of mathematical physics. However, it should not be denied that he made further contributions to elucidation of the characterization of mechanical principles (or axioms) as hypotheses or conventions, in particular to the body "Alpha".[25]

With respect to mathematical methods, Neumann's most important contribution to potential theory will only be mentioned briefly. He had solved the two- as well as the three-dimensional boundary value problem, also called the Dirichlet problem, in the 1860s.[26] The correctness of the Dirichlet principle did not seem to be in doubt at

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[22]Vielmehr hat der Mathematiker im Gebiete der Physik eine wichtige und nicht zu unterschätzende Aufgabe. Sie besteht darin, die einstweilen *vorhandenen* physikalischen Vorstellungen näher zu erforschen, ihre Consequenzen nach allen Seiten mit möglichster Strenge zu verfolgen; mit einem Wort, sie besteht darin, diese Vorstellungen *deductiv* zu entwickeln. Solche *deductive* Entwickelungen werden, namentlich wenn sie in festen und möglichst geradlinigen Zügen ausgeführt sind, dazu dienen, die Uebersichtlichkeit des betreffenden Gebietes zu vergrössern; sie werden beitragen, um gewissermassen unserm geistigen Blick allmählig diejenige Weite und Schärfe, namentlich aber diejenige Ruhe und Sicherheit zu geben, welche zu einer glücklichen *Induction* d. i. zum Emportauchen *neuer und besserer* Vorstellungen erforderlich sind (Neumann 1878, 196f; author's italic).

[23]Neumann (1878, 98). "Als Controle eines noch hypothetischen physikalischen Gesetzes können nur solche Fälle benutzt werden, deren Wirklichkeit oder Realisirbarkeit nachgewiesen ist."

[24]Neumann (1893, 1898, 1902).

[25]Neumann (1908).

[26]Neumann (1861, 1864).

that time. This changed over the course of a few years. In 1870 he presented a sketch of his method of arithmetical means, thereby replacing the Dirichlet principle.[27] He described the method in detail 7 years later in his book *Untersuchungen über das Logarithmische und Newtonsche Potential* (Investigations about the logarithmic and Newtonian potential).[28] A cornerstone of his method was formed by a theorem of Gauss first published in 1813 and the so-called theory of a double layer. Gauss had proven in his theorem that the integral

$$\int \frac{\cos\vartheta}{r^2} d\sigma$$

takes the values 0, 2π or 4π depending on the point x lies outside the surface, on it or inside.

(The integral was taken for an arbitrary point x and a closed surface σ in space. $d\sigma$ denotes an (infinitesimal) element of the surface, r the distance from x to $d\sigma$ and ϑ the angle between r and the inner normal of $d\sigma$.)[29] Neumann extended the theorem to the case that the integrand was multiplied with a function μ which was continuous on the whole surface.

Denoting that extended integral by

$$\mathrm{W(x)}\ (W(x) = \int \frac{\mu \cdot \cos\vartheta}{r^2} d\sigma)$$

(W(x) is used instead of Neumann's notation W, to avoid confusion with the following distinction). He distinguished for an arbitrary point s on the surface three values of W: First, W(s) the value of the integral for x = s, second, $W_i(s)$ the limit of W(x) where x approaches s from the inside of the surface, third, $W_a(s)$ the limit of W(x) where x approaches s from the outside of the surface. After showing that the integral W(x) can be represented as the potential of a double layer on σ, Neumann constructed a series of functions $W^{(n)}(x)$ by iteration. The iteration starts by choosing $\mu = f$, f being the given boundary value except for scaling, hence

$$W^{(0)}(x) = \int \frac{f \cdot \cos\vartheta}{r^2} d\sigma, \quad W^{(n+1)}(x) = \int \frac{W^{(n)} \cdot \cos\vartheta}{r^2} d\sigma^{(0)}(\mathrm{x}).$$

Then Neumann derived different series from the $W^{(n)}(x)$ and proved their convergence. This was the main part of his method and he could complete the proof only by restricting the class of domains (surfaces) to special convex ones. The inner as well as the outer boundary value problem was solved by the limit of one of the

[27] Neumann (1870b).

[28] Neumann (1877).

[29] Gauss (1813, 9).

series mentioned above.[30] Neumann also gave an exact description and solution of the second boundary value problem in this publication, probably for the first time. He returned to potential theory and to his method of arithmetical means several times in his life. He improved the method repeatedly, in particular he diminished the restrictions put to the boundary values as well as to the domain. In addition he applied his methods of potential theory to several problems of electrostatic, electrodynamics, and other fields like hydromechanics.[31]

Summarizing Neumann's work on mathematical physics, the following features should be pointed out: (1) He stuck to his point of view mentioned above and took a mathematically dominated approach. (2) He looked for basic physical principles to improve the construction of the theory. However, he did not deduce these principles from experimental data but derived them by theoretical considerations. (3) At an early stage he emphasized that electrodynamics and the theory of heat could not be based on the principles of mechanics only. (4) At the same time he did not fully appreciate the work of Maxwell and Hertz in electrodynamics.

Thus, he developed a high mathematical standard in constructing a physical theory and, stimulated by physical phenomena, he analysed several mathematical gaps in the formulation of theoretical concepts. But he failed to accept new ideas that did not meet his strong mathematical criteria.

5 Some Consequences of Neumann's Work – The Blossom of Mathematical Physics in Leipzig

Mathematical physics began to flourish at the University of Leipzig due to the work of Neumann, von der Mühll and Mayer from the late 1860s onwards. Neumann became the leading personality in this process. Von der Mühll contributed to this upswing mainly by his lectures. He covered a broad range of physical topics and treated the main fields of physics in his lectures including analytical mechanics, hydrodynamics, the theory of elasticity, optics, electrodynamics, and the theory of heat. Analytical mechanics, calculus of variations, and the theory of ordinary and partial differential equations formed the main fields of research of Adolph Mayer. He focused his work not so much on the treatment of physical problems but more on the investigation of general mathematical methods and principles that were applied in mathematical physics. For instance, he improved and supplemented several methods for solving differential equations, especially the theory of Jacobi. But he also treated various general problems of analytical mechanics regarding the movement of systems of mass points under additional restrictive conditions. Furthermore, he analysed various problems in the calculus of variations; a class

[30]The 'inner problem' asks for the potential function of points inside the surface, the 'outer problem' for points on the outside of the surface.

[31]For an analysis of Neumann's work on potential theory see Schlote (2004).

of them is now called Mayer's problems (Mayersche Probleme). The proof of the method of Lagrangian multipliers and the contributions to the development of field theory were two further topics of Mayer's investigations in the calculus of variations. The latter included among other features the construction of the so-called Mayer field. Mayer's activities in the calculus of variations formed the starting point for a new tradition in the research of mathematical physics at the Leipzig Mathematical Institute. All in all, the three mathematicians ensured a good representation of theoretical and mathematical physics and this characterized the peculiarity at the University of Leipzig. For instance, they gave 145 lecture courses that treated topics of mathematical physics during the three decades after Neumann's appointment, 53 in the first, 65 in the second and 27 in the third decade. These numbers include the "Exercises in mathematical physics" held by von der Mühll every term from the winter term 1880/1881 until the winter term 1888/1889 and Neumann's "Mathematical–physical seminar" that took place six times during the period from the summer term 1885 till the winter term 1891/1892. However, courses like "Potential theory" or "Calculus of variations" were not included in the count. At the same time there was no lecture course titled "Theoretical Physics" or anything like that before 1892. Moreover, physicists announced, and held only a few courses in, topics that could be characterized as mainly theoretical at that time.

Whereas a professorship in theoretical physics was created at many German universities, Leipzig's physicists did not apply to the Saxon ministry for such a professorship. As a consequence the emergence of mathematical and theoretical physics developed a one-sided mathematical character. The physical point of view that seems to be indispensable in the process of deducing and formulating the fundamental assumptions and principles of a theory was missed nearly completely at the physical institute in Leipzig. This shortcoming did not count so much at the beginning of that process but it grew in time. In addition, there were two further facts that finally caused an application for an extraordinary professorship of theoretical physics by the philosophical faculty in February 1894. Firstly, the differences between physicists and mathematicians regarding the treatment of physical problems became more prominent. Neumann and von der Mühll presented their lectures from a prevailing mathematical point of view. In addition the tendency towards a rigorization of mathematics had to be taken into account. The explanation of physical phenomena was pushed into the background by these facts. Most of the physicists could not and did not want to follow that way since it was complicated, sometimes incomprehensible and gave no hints to further theoretical and experimental research. Secondly, von der Mühll left his Leipzig position and returned to Basel in 1889. Neumann restricted the number of lectures on mathematical physics[32] and above all he ignored the arrangements that he had reached with his colleagues

[32]This refers to lecture courses with a stronger orientation towards physics like "Electrodynamics", "Theory of heat", "Theory of light", or "Hydrodynamics" and much less to courses with a larger amount of mathematical aspects like "Analytical mechanics" or "Introduction to mathematical physics".

about the lecture courses. Therefore it was not surprising that, besides the head of the physical institute Gustav Wiedemann (1826–1899) and the dean Wilhelm Ostwald (1853–1932) also two of Neumann's closer colleagues, the mathematician Sophus Lie (1842–1899) and the astronomer Heinrich Bruns (1848–1919), worked out together the draft of an application that the philosophical faculty agreed to nearly without any changes.[33]

The ministry accepted the application of the faculty and in spring 1894 it appointed Hermann Ebert (1861–1913) to an extraordinary professorship on theoretical physics. Ebert lectured only one term in Leipzig and accepted a call to Kiel in autumn 1894. His successor was Paul Drude (1853–1906), an outstanding theoretical physicist, well known for his contribution to the theory of electrons. He installed a course in theoretical physics completing Ebert's efforts. The physicists at Leipzig caught up on the backlog in the development of theoretical physics within a few years. Drude continued his extensive optical investigations during his time at Leipzig. He was a follower of Maxwell's theory of electromagnetism and used it to explain optical phenomena.[34] After discovering and analysing an anomaly of the dispersion of electromagnetic waves, Drude tackled several new problems. The spreading of electromagnetic waves in conductors and the dependency of optical and electrical constants from the inner constitution of the bodies or substances formed two of his main topics of research and led him in particular to deal with the theory of electrons. Drude demonstrated a thoroughly balanced relation between theory and experiment, between mathematical–physical knowledge and physical imaginations in his work. Since his point of view did not agree with Neumann's opinion in regard to the theory of electrodynamics, it could be supposed that both scientists discussed this topic. However, there was no indication that such a discussion took place during Drude's stay in Leipzig. Also Neumann's renewed turn to the Maxwell–Hertz theory about the year 1900 showed no influence of Drude and appeared to be more a continuation of his long lasting treatment with the "electrical forces" and the problems of electrodynamics.

The significance of Neumann's investigations in mathematical physics rests on their methodical aspects. Despite his difficult personality and his slight success in the development of electrodynamics, he revealed the basic difference between a theory in mathematical physics and in mathematics and in that way he initiated, besides others, a discussion of the fundamentals of mechanics that finally ended

[33]It could be supposed that Lie, who followed F. Klein as the chair of geometry in 1886, could fill the gap in mathematical physics. However, he focused his research on the theory of continuous groups and the application of group theory to differential equations and sketched only roughly the importance of his results to physics. His results were mostly seen as a special topic in pure mathematics and did not get a high appreciation in his time. Lie repeatedly pointed out in several of his lectures the importance of his results to physics, but he worked out in detail only a few instances like the application of the transformation of contact in geometrical optics. The significance of Lie groups and Lie algebras with all its implications in mathematical physics was realised only some decades later.

[34]Drude (1894).

in the emergence of a new concept of science. At the same time he demonstrated the value of high mathematical standards in the process of theory-building. But he overestimated it as a criterion for the quality and development of a physical theory and he and his colleagues established a mathematical physics dominated by mathematics. However, this fostered the distinction between mathematical and theoretical physics and had undoubtedly a positive influence on Mathematical Physics.

6 Conclusion

Mathematical Physics remained a prominent field of mathematical research at Leipzig. Some decades later mathematical methods became more and more an indispensable tool in theoretical physics and we see new intensive interrelations between mathematics and physics during the time of Heisenberg at Leipzig. Nevertheless, the development during the last third of the nineteenth century paved the way and created basic preconditions for the blossoming that occurred in the late 1920s and early 1930s. The importance and the impact should not be underestimated. Not very successful in concrete results, especially in the foundation of electrodynamics, Neumann's work offered interesting stimulations to further research from a methodical point of view. The emergence of theoretical physics and its interrelations with mathematical physics showed some peculiarities and deviations from the general trend of development. The detailed look on the interaction of mathematics and physics at the local level witnessed a wealth of dynamic and contributes to a colourful picture of the process in more details.

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On Boundaries of the Language of Physics

Ladislav Kvasz

Abstract The aim of the present paper is to outline a method of reconstruction of the historical development of the language of physical theories. We will apply the theory presented in *Patterns of Change, Linguistic Innovations in the Development of Classical Mathematics* to the analysis of linguistic innovations in physics. Our method is based on a reconstruction of the following potentialities of language: *analytical power*, *expressive power*, *integrative power*, and *explanatory power*, as well as *analytical boundaries* and *expressive boundaries*. One of the results of our reconstruction is a new interpretation of Kant's antinomies of pure reason. If we relate Kant's antinomies to the language, they retain validity.

1 Introduction

The *world of physics* extends far beyond our sensory world. It is inhabited by huge objects, whose dimensions exceed everything we can imagine; it contains tiny particles that defy our imagination because of their extreme smallness. These objects are subjected to various forces and invisible radiation. Objects of our sensory world occupy only a tiny slice of the vast scales of physical quantities. Dimensions, temperatures, pressures, and densities which we know from our daily experience form only a small interval on the scale of dimensions, temperatures, pressures, and densities that appear in physics. Unlike the physical world, the world of physics changes along with how scientists proceed in uncovering ever more remote regions of the universe. Before Newton there were no forces in the world of physics, before

L. Kvasz (✉)
Institute of Philosophy of the Academy of Sciences of Czech Republic, Jilská 1,
Prague 110 00, Czech Republic
e-mail: ladislavkvasz@gmail.com

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Torricelli there was no pressure, before Clausius there was no entropy. Of course, in the *physical world* force, pressure, and entropy always existed and their properties are independent from the development of our physical knowledge. In contrast to the physical world, the *world of physics* has undergone a number of fundamental changes. When Kuhn said that two physicists who support different paradigms live in different worlds, he had in mind different *worlds of physics* and not different *physical worlds*. The aim of the present paper is a reflection of the most important changes in the world of physics. I will proceed indirectly, using the reconstruction of changes in the language of physics, which I call *re-codings*. The present article is a development of the ideas from my *Patterns of Change, Linguistic Innovations in the Development of Classical Mathematics* (Kvasz 2008).

By re-coding in physics I mean a change in the world of physics; a change of the way physical phenomena, objects and processes are detected, described, and explained. An example of re-coding would thus be the transition from the Newtonian to the relativistic representation. In the Newtonian representation the world is described as a system of particles which move under the influence of forces in three-dimensional empty space. I include into the *Newtonian representation* besides Newton's own theory also the theories of Euler (1736), Lagrange (1788), and Hamilton (1835), because these differ from Newton's original theory more in mathematical detail (which has to be analyzed by different means, see Kvasz 2011) than in physical content. The most obvious re-codings are the transitions to the mechanistic, the relativistic and the quantum representations. These representations differ in the kind of space they use: three-dimensional Euclidean space, four dimensional space–time, and an infinite-dimensional Hilbert space.

Our analysis of re-codings in mathematics has benefited from the work of Gottlob Frege. In his *Funktion und Begriff* (Frege 1891) Frege summarized the development of mathematics by showing that it consisted in a gradual *increase of the generality of language*: going from constants (arithmetic) to variables (algebra) and to functional variables (calculus). In physics we do not have at our disposal an analysis of the development of its language comparable to the analysis made by Frege. Therefore our analysis of re-codings in physics will be much more tentative.

2 Linguistic Approach to the Development of Physics

In *Patterns of Change* I identified aspects of the language of mathematics, the changes of which accompany re-codings (Kvasz 2008, 16):

1. *Logical power* – how complex formulas can be proven in the language,
2. *Expressive power* – what new things the language can express, which were inexpressible at the previous stages,
3. *Explanatory power* – how the language can explain the failures which occurred at the previous stages,
4. *Integrative power* – shows the sort of unity and order the language enables us to see in places where we perceived just unrelated particular cases at the previous stages,

5. *Logical boundaries* – are marked by occurrence of unexpected paradoxical expressions,
6. *Expressive boundaries* – are marked by failures of the language to describe complex situations.

The evolution of the language of mathematics consists in the growth of its *logical* and *expressive power* – the later stages of development make it possible to prove more theorems and to describe a wider range of phenomena. The *explanatory* and the *integrative power* of the language also gradually increase – the later stages of development of the language provide a deeper understanding of its methods and offer a more unified view of its subject. To overcome the *logical* and *expressive boundaries*, more and more sophisticated and subtle techniques are developed. Our aim is to introduce analogous aspects into the analysis of the language of physics.

Transferring the notion of *logical power* from mathematics to physics is not difficult. In physics it is more appropriate to call it *analytical power* of language, and to understand it as related not to proving of theorems, as in mathematics, but to derivation of formulas. We will characterize the analytical power of the language of a particular physical theory by the kind of formulas which it is possible to derive in the given language using the accepted postulates of the theory (without the further use of empirical data). As an illustration we can take Newton's derivation of Kepler's laws. For Kepler the elliptical form of the planetary orbits was an empirical fact. In the language of Newtonian mechanics this proposition can be derived analytically from the law of gravity. The ability of the language to derive a particular law illustrates its analytical power.

Equally clear is the case of the *expressive power*, which represents the ability of the language to represent some aspect of nature. In the history of physics there are many cases when a phenomenon that defied description by means of the language of the "old" theory and was therefore seen as an anomaly could be clearly and unambiguously described by means of the language of the "new" theory. Such cases illustrate the increase of the expressive power of the language of physics.

We can find in physics also an analogy of the *explanatory power* of language. As an example we can take the explanation of stability of matter by quantum mechanics. In classical physics it is not clear why the electrons that orbit in the atoms forming for instance a chair do not disintegrate. From the principles of classical physics follows that it is not possible to form a stable configuration of charged particles that would be maintained by electromagnetic forces. Therefore the perturbation of the atoms of the chair that would be caused by your sitting down on it would lead to large changes in the trajectories of the electrons, causing a disintegration of the whole chair. Heisenberg's principle of uncertainty makes it possible to explain why matter is stable, i.e. why despite perturbations the electrons remain near their original locations. According to this principle, an electron can get closer to a proton (i.e. make its location in space more precise) only at the price of increasing its energy (due to the increase in the uncertainty of its momentum). This mechanism ensures the stability of the ground state of the atoms. Thus the language of quantum mechanics makes it possible to explain the stability that for classical physics was a mystery.

Illustrations of the *integrative power* of language are the great unifications, such as Newton's unification of the terrestrial and celestial mechanics, or Maxwell's unification of electrodynamics and optics. The analyses presented in this paper ascribe these unifications to the integrative power of language.

In addition to these "positive" aspects of the language we will try to transfer from mathematics to physics also the notions of *analytical* and *expressive boundaries* of language.[1] In our opinion, these boundaries are one of the most interesting aspects of language of science.

3 Re-presentations in the History of Physics

After we described the aspects of the language of physics we can proceed to the analysis of the historical material. We will try to interpret the evolution of classical physics from a linguistic point of view as a sequence of innovations; as a process of growth of the analytical, expressive, explanatory and integrative power. We will start with Newtonian physics because it is generally accepted as the paradigm of classical physics.

3.1 Newtonian Physics

Newton provides a description of interaction by means of forces acting at a distance. Thus for a body to be able to act onto another body it is not necessarily that they are in immediate contact. Bodies can act on each other even when they are apart. An example of such an action at a distance is the Newtonian force of gravitation. For Descartes interaction consisted in collision accompanied by a passive transference of momentum from one body to another. In contrast to this, Newtonian

[1] The reader may be surprised that in the epistemological reconstruction of physics a mention of something like the *predictive power of language* is missing. In the history of science we witnessed a number of spectacular predictions, like the prediction of the return of a comet by Halley, or the prediction of the bending of light rays near the Sun by Einstein. In our view, however, in the majority of such cases we have to do with a connection of an *epistemological fact* that the given language makes it possible to derive the existence of the particular phenomenon with a contingent *historical circumstance* that the analytical derivation was made before the empirical discovery. From the epistemological point of view it is not so important whether the theory predicts the phenomenon or explains it after the discovery. What matters is the existence of the analytical connection between the principles of the theory and the phenomenon. From the psychological point of view there is of course a big difference. Predicting a phenomenon has a touch of divination and the public views it as a sign of power. From the epistemological point of view, however, the temporal order is unimportant. Epistemologically significant is only the derivability of the phenomenon. Thus when epistemology studies the development of knowledge, the time in which it reconstructs this development is not the historical time. Epistemology abstracts from the contingent details of the historical development and analyzes only the relations of logical derivability between theories belonging to different historical stages.

interaction is an active process. In the Newtonian system *forces can generate motion*. Therefore, the total quantity of motion (if we understand it like Descartes did, as a *scalar* quantity *mv*) is not preserved. According to Newton, the total amount of motion (the Cartesian quantity *mv*) increases in the course of free fall.

3.1.1 Analytical Power of Newtonian Physics

An illustration of the analytical power of the language of Newtonian physics is the well known *derivation of Kepler's laws from the law of universal gravitation*. The proposition about the elliptical form of the planetary orbits was for Kepler an empirical law. He discovered it while analyzing the data of the motion of Mars. In Newtonian mechanics this law can be derived from the law of universal gravitation. Of course, historically it went the other way round – Newton used Kepler's laws in his discovery of the law of universal gravitation. But the direction in which we pass from one law to the other is irrelevant. What is important is the existence of analytical ties between these two propositions, and these ties illustrate the analytical power of the language of Newtonian physics.

3.1.2 Expressive Power of Newtonian Physics

Cartesian physics was unable to represent friction. Descartes described interaction by means of his law of conservation of the quantity of motion. Friction violates this law, it causes a decrease of the total quantity of (mechanical) motion, and therefore it cannot be incorporated into the Cartesian description of interaction. For Newtonian physics the description of friction presents no problem. Newton describes interactions of bodies by his second law. If there is friction in the system, it just means that in the particular equation we have to add a new term. This clearly illustrates the expressive power of the language of Newtonian physics.

3.1.3 Explanatory Power of Newtonian Physics

An illustration of the explanatory power of the language of Newtonian physics is the successful explanation of the tides. Newtonian mechanics explained the tides as a result of the action of the gravitational force of the moon on the water in seas and oceans. This explanation represented a great progress in comparison with Galileo, who tried to explain the tides as a result of the combination of Earth's rotation with its orbiting around the Sun.

3.1.4 Integrative Power of Newtonian Physics

An illustration of the integrative power of the language of Newtonian physics is the *unification of terrestrial and celestial mechanics*. We can see a forerunner of this unification in Galileo, who by his telescope discovered the mountains on the Moon and came to the conclusion that the Moon consists of the same substance as the Earth (see Galileo 1610; see also Drake 1957). However, for Galileo the unification of heavens and Earth was based on observation. He did not have at his disposal a formal language that would allow expression of this intuited unity analytically. That was done by Newton.

3.1.5 Analytical Boundaries of Newtonian Physics

Newtonian physics can answer many questions about the dynamics of the solar system. Nevertheless, this theory is far from being complete. One of the first who realized this was Immanuel Kant, who formulated a series of antinomies revealing the boundaries of the Newtonian description of the world (see Kant 1781). Kant considered the antinomies as properties of pure reason. He did not attach them to a particular physical theory, but he saw in them boundaries that limit our ability to create any theory. If this were true, the antinomies would be insurmountable. However, as the history of physics showed, Kant's antinomies can be overcome. General theory of relativity eliminated the antinomy of the finiteness versus the infiniteness of space by replacing Euclidean space with a Riemannian one. In spite of this, Kant's antinomies do not lose their importance. The only thing we have to do is to tie them to the particular language in which they were formulated. Thus I propose to interpret the antinomy of the finiteness versus the infiniteness of space as a reflection of the *external nature of space in Newtonian physics*. In my view, this antinomy is not a feature of human reason but rather of language. And it is a feature not of language as such, but of the language of Newtonian mechanics. The antinomy of the finiteness versus the infiniteness of space can be interpreted as a reference to the analytical boundaries of the language of Newtonian physics.[2]

3.1.6 Expressive Boundaries of Newtonian Physics

The expressive boundaries of the language of Newtonian physics can be seen in the problem of the regular structure of the solar system. The structure of the solar

[2]Interpreting the antinomy of finiteness or infiniteness of space as a manifestation of the expressive boundaries of the language of Newtonian physics, which were overcome by the theory of relativity, I don't claim that relativistic physics can give here some definitive answer. I claim only that for the theory of relativity the question about the finiteness of space is no longer a speculative question. When we take not human reason, but the language of a particular physical theory as the foundation of Kant's antinomies, we can interpret Mach's criticism of the notions of absolute space and absolute time as a deepening of the Kantian position. Mach discovered the external character of space in Newtonian physics (see Mach 1893). He thus diverted the sting of Kant's criticism from the limits of pure reason to the limits of a particular physical theory. Our approach can be seen as a further development of Mach's criticism, claiming that these problems are not restricted to Newton's system, but are a systematic feature of all theories. This feature is thus universal, just as Kant believed. The only difference is that it is not related to reason but to language.

system shows a high degree of regularity: the planets orbit around the Sun in the same direction and their orbits lie roughly in the same plane. Descartes explained this regularity by the existence of the vortex of fine matter. When Newton proved the impossibility of the existence of such a vortex, the regular structure of the solar system became incomprehensible. Newton interpreted it as a sign of God’s plan. He used a theological argument where the physical arguments were lacking. In my view, this theological explanation can be seen as a manifestation of the expressive boundaries of the language of Newtonian physics.

A more spectacular manifestation of the expressive boundaries of the language of Newtonian physics can be seen in problems where Newton reached a conclusion that contradicted his experimental data. Let us consider the calculation of the speed of sound (Prop. L, Prob. XII of the second book of the *Principia*), where he derived 968 ft per second, i.e. 295 ms^{-1}, which is 17% less than the actual value of the speed of sound. Newton’s own experiments at Trinity College gave a value well above this theoretical prediction, and so he embarked on a series of emendations, which Richard Westfall called an “egregious fraud” (Westfall 1971, 497). Similarly problematic is Newton’s calculation of the time in which a fluid escapes through a hole in the bottom of a container (Prop. XXXVI, Prob. VIII of the second book, of the second edition of *Principia*). Here he obtained twice the value that he got in the measurement. Considering this problem Westfall notes that here “the dynamics completely failed” (Westfall 1971, 501). In these examples apart from the purely mechanical side of the problem, which Newton correctly understood, was also an additional aspect (a thermodynamic or a hydrodynamic one, respectively), which Newton did not take into account, and which was responsible for the considerable deviation of his calculations from the experimental data. A full understanding of these additional aspects was not possible until the transition from Newtonian physics to the theory of continua and fluids (discussed below). Therefore, I think that these failures do not constitute an “egregious fraud” or a “complete failure” of Newton’s dynamics. They are manifestations of the expressive boundaries of its language. It is not surprising that these derivations failed. In the language of Newtonian physics the speed of sound cannot be derived.[3] It is remarkable that, despite the inappropriateness of the language, Newton still got a reasonable result.

[3] I use the term “Newtonian physics” in a narrower sense than it is used by the physical community. In my view it is important that Newton did not use partial differential equations. Physicists consider their introduction a “technical detail” and so downplay the conceptual changes which are involved. Here we can quote the very words of Newton from his letter to Halley: “*Mathematicians emerging and defining all be satisfied with the calculator are only dry and weak staff and someone else who did nothing but making the claim for everything and everything seizes, will occupy all the discoveries for themselves*.” (see Vavilov 1945). Here Newton thinks of Hook, who made a claim to the discovery of the law of universal gravitation. His words, however, have a deeper meaning, and show that Newton did not consider the mathematical aspect of physics a subordinate one.

3.2 The Theory of Continua and Fluids

In Newton's universe interaction is mediated by forces acting at a distance. This finds its expression in the fact that the equations of motion have the form of ordinary differential equations. In 1713, when trying to describe the vibrating string, Brook Taylor hypothetically distinguished an element of the string and examined the forces acting on it from the side of the contiguous elements. In principle he only applied Newton's second law to the element of the string. A little later, in 1736, Euler formulated a research program that systematically studied forces acting inside of a substance. This program led to the birth of a new representation. The first trace of awareness of the fact that this program abandoned the framework of Newtonian physics is the title of Euler's work: *Discovery of a new principle of mechanics* from 1750, where Euler formulated the principle, according to which the differential equations describing the motion of a free body remain valid also if they are used to describe not the body as a whole (as Newton did), but for the description of an element of a body or a fluid. During the eighteenth century the theory of continua and fluids was gradually developed and it represents an extended body as composed of parts – the elements of the continuum – which are *hypothetically distinguished*. These elements have the same characteristics as the whole continuum (the same density, elasticity, hardness) but are so small as to allow the transition to differentials. In mathematical terms this meant that besides the ordinary differential equations of Newtonian mechanics, which describe the motion of the body as a whole, a new kind of equations emerged – the partial differential equations. These are equations such as the equation of the *vibrating string*, the equations of the *fluid dynamics* and the equation of *heat conduction*. They describe the motion of a continuum or the spread of activity (stress, compression, etc.) in a continuous medium.[4] For the description of various phenomena, several new fluids were postulated (the electric fluid, magnetic fluid, the caloric, the phlogiston, the ether). These theories resemble Cartesian physics, and many historians see in Euler a successor of the Cartesian program, or even a hidden Cartesian. In my view this is a mistake. Euler described the action of forces by means of differential equations, just like Newton. Thus he does not make a step backwards to Descartes, but goes forward to create a new representation.

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[4]The forces acting between the elements of a continuum are a mathematical rendering of the Cartesian idea of action as a contact. They are local forces and their action is given by direct contact of one element of the medium on adjacent elements. This idea of action is fundamentally different from the action of Newtonian forces. Their introduction cannot be interpreted as a return to the Cartesian approach, because according to the theory of continua and fluids forces enter into differential equations. That means that the overall scheme of description of action is not a Cartesian one (based on conservation laws) but a further development of Newton's idea of describing interaction by means of a differential equation.

3.2.1 Analytical Power of the Theory of Continua and Fluids

The language of the theory of continua and fluids made it possible to derive differential equations describing different physical processes such as vibrations of a string, flow of water, heat conduction or electric currents. Perhaps the most spectacular achievements were Fourier's derivation of the equation of heat conduction published in his book *Théorie analytique de la chaleur* (Fourier 1822), and Carnot's derivation of the formula describing the effectiveness of thermal machines published in the book *Réflexions sur la puissance motrice de feu* (Carnot S). Fourier derived his equation and Carnot proved his formula under the supposition that there is a fluid that they called *caloric*. Less than 20 years after the publication of Carnot's book, Joule proved that there is no *caloric*. However, despite the fact that *caloric* has gone, the formulas remained. This illustrates the analytical power of language. This power is independent of whether some quantity used in the derivation exists or not. Regardless whether *caloric* exists or not, Fourier's equation describes heat conduction with high accuracy. The derivation of this equation illustrates the analytical power of the language of the theory of continua and fluids.

3.2.2 Expressive Power of the Theory of Continua and Fluids

One of the few mistakes in Newton's *Principia* was the derivation of the speed of sound. Newton calculated it by means of a trick, when he likened the vibrations of air in the sound wave to a mechanical pendulum (such tricks indicate that the derivation transcends the possibilities of the language of Newtonian mechanics). The correct value of the speed of sound was obtained by Laplace in 1816, when he realized that the compression of air in the sound wave is not isothermal, as Newton implicitly assumed. The compression in the sound wave heats the air and the increase of temperature raises the speed of the sound. A theoretical justification of Laplace's derivation was given in 1823 by Poisson, who assumed that the amount of heat contained in a given volume of air remains constant during its compression. Such processes are called *adiabatic*. The notion of an adiabatic process belongs to the theory of continua and fluids. If we imagine the air as a sponge imbued with caloric; an *isothermal compression* of the air corresponds to pressing of the sponge by which the caloric is squeezed from the sponge. The compressed sponge occupies a smaller volume and this smaller volume can take only a smaller amount of caloric. The superfluous caloric must flow away from the sponge. When the pressing of the sponge is too fast, and in the sound wave compression and expansion alternates 1,000 times per second, the caloric does not manage to leave the sponge and thus is pressed together with it. The pressing of the caloric, i.e. the increasing of the amount of caloric in a unit of volume, is nothing else but a rise of temperature. Poisson realized that in sound waves the vibrations run so fast that the caloric is compressed together with the air, and so the condition of an isothermal process is violated. Here we see how the representation of heat as a fluid makes it possible to analyze the thermal conditions in the sound wave and to arrive at the correct value for the speed of sound. The ability to *express the difference between an isothermal and an adiabatic process* is an illustration of the expressive power of the language of the theory of continua and fluids.

3.2.3 Explanatory Power of the Theory of Continua and Fluids

The expressive boundaries of Newtonian physics were illustrated above by its inability to explain the regular structure of the solar system. For the theory of continua and fluids it is easy to explain this regularity. It is sufficient to assume that the solar system evolved from a cloud of dust by condensation (as Kant explained it in 1755 in his *Universal Natural History and Theory of the Heavens*) or it was created by separation of some matter from the Sun (as Laplace explained it in 1796, in his *Exposition du system du monde*). One might object that to assume in the explanation of the structure of the solar system God's intervention is just as good as to assume a rotating ball of gas. We have no idea how this ball came into rotation. This is true; any explanation that does not want to be circular must be based on assumptions that are further not explained. However, the difference between Newton's explanation of the regularity of the solar system by God's intervention and the explanations of Kant or Laplace is that the assumptions made by Kant and Laplace are expressed in the language of physics, and so the whole explanation takes place inside this language. Newton's explanation on the other hand is an explanation of a fact expressed in the language of physics by a cause that is not physical. Therefore, the transition from Newton to Kant and Laplace represents an increase of the explanatory power of the language of physics.

3.2.4 Integrative Power of the Theory of Continua and Fluids

The integrative power of the language of the theory of continua and fluids found its expression in the mechanistic worldview. The *mechanistic worldview* is often attributed to Newton, but Newtonian physics did not have at its disposal the wealth of theoretical tools that are necessary for the creation of the mechanistic worldview. Moreover, Newton himself, as his works on alchemy indicate, did not believe in a purely mechanistic worldview. Therefore, the attribution of this worldview to Newton is misleading. The mechanistic worldview is not the worldview of Newtonian physics, which contained a number of non-mechanical affinities between chemical elements. It is a worldview that contains besides the mechanical phenomena also heat and electricity, as well as acoustic and optical phenomena. This worldview is mechanistic because it offers for all these diverse phenomena explanation in terms of mechanical motion of some elastic continuum (vibrating string, vibrating aether) or fluid (caloric, electric fluid, magnetic fluid). The mechanistic worldview is thus the manifestation of the integrative power of the language of the theory of continua and fluids.

3.2.5 Analytical Boundaries of the Theory of Continua and Fluids

Just as Kant's antinomy of finite versus infinite space illustrated the logical boundaries of Newtonian physics, the antinomy of finite versus infinite divisibility of matter can be taken as an illustration of the logical boundaries of the language of the theory of continua and fluids. Kant's antinomy shows that the notion of a volume element in the theory of continua and fluids is external. The divisibility of matter is postulated (just like in Newtonian physics the infinity of space was postulated) but it is not experimentally fixed. When we discriminate in a continuum a volume element, it is a mathematical operation which is not backed by an experimental procedure that would determine the properties of matter at this level of magnitude. We simply assume that matter is homogeneous and so an element of volume has the same properties as the body as a whole. Kant pointed here to a fundamental problem. However, just like in the previous case, we do not ascribe this antinomy to reason as such. Quantum mechanics removed this antinomy of Kant when it showed that due to the uncertainty principle any further division is accompanied by an increase in energy. The *speculative* question of divisibility of matter became a practical question of availability of ever higher and higher energies, which ultimately lead to a technical problem of building larger and larger accelerators. This shows that Kant's antinomy belongs not to reason but it indicates the analytical boundaries of the language of the theory of continua and fluids.

3.2.6 Expressive Boundaries of the Theory of Continua and Fluids

The theory of continua and fluids describes the thermal phenomena by postulating the existence of a new substance, the *caloric*. Then the flow of caloric through the pores of matter corresponds to heat conduction, while the increase of temperature corresponds to the accumulation of caloric in a given volume. In 1843 Joule determined the mechanical equivalent of heat, thus showing that mechanical work can be converted into heat. The emergence of heat from mechanical work contradicts the idea that heat is a substance (a fluid). Joule's experiments thus show the expressive boundaries of the language of the theory of continua and fluids – this language cannot describe the generation of heat by mechanical work just like the language of the Cartesian physics could not describe friction.

3.3 The Theory of Atoms and Energies

The theory of atoms and energies emerged from a crisis of the mechanistic world-view, which was the consequence of progress in several disciplines. In chemistry it was discovered that air is not a simple elastic continuum, as it was represented by the *theory of continua and fluids*, but it is a mixture of several different substances.

In 1755 Black discovered the carbon dioxide (*fixed air*), in 1766 Cavendish discovered the hydrogen (*inflammable air*). These discoveries culminated in 1789 in Lavoisier's oxidation theory of combustion, which replaced the phlogiston theory and led to the creation of the notion of a chemical element. Developments in calorimetry led to Joule's measurement of the mechanical equivalent of heat, which discredited the notion of caloric. Thus Lavoisier discredited the phlogiston, Joule the caloric and finally the theory of relativity discredited the last weightless fluid – the aether. Advances in the theory of materials such as optical glass or steel during the first half of the nineteenth century brought physics beyond the limits of the theory of continua and fluids. These advances fundamentally changed our understanding of the structure of matter.

Around the middle of the nineteenth century a transition occurred from the hypothetical postulation of mathematical continua or weightless fluids to an empirical study of the structure of materials and of the processes taking place in them. The emergence of a new representation is closely linked to the abandonment of fluids. This new representation got rid of the mathematically postulated substances on which the previous description was based, and went one level deeper in the description of the structure of matter. The theory of continua and fluids simply postulated continuous substances representing macroscopic phenomena like fire, heat, and electricity and created a mathematical language which made it possible to calculate their aspects (like Fourier's equation of heat conduction derived on the basis of the postulation of caloric, or Maxwell's equations of electrodynamics derived on the basis of the postulation of aether). With the increase in the accuracy of experimental methods physics was able to take a step beyond the macroscopic level. The macroscopic properties that the theory of continua and fluids simply turned into hypothetical substances become now statistical averages of properties of the real particles of the microscopic levels of description.

3.3.1 Analytical Power of the Theory of Atoms and Energies

As in the cases of the previous languages, also in the case of the language *the theory of atoms and energies* the first hints of what will later become a new representation emerged as technical tricks designed to solve problems for which the standard methods of the old language were not suitable. Henri Navier, who contributed to the development of the theory of continua and fluids by a definition of the modulus of elasticity and by the experimental determination of its value for iron, submitted in 1822 his *Mémoire sur les lois du mouvements des fluides*, where he derived the *equation of motion of an incompressible viscous fluid*. Although the equation itself contains only variables characterizing the fluid as a continuum, Navier derived it on the assumption that the liquid consists of molecules, the forces of interaction between which are proportional to their mutual velocities. Thus the notion of a molecule occurred in the theory of continua and fluids as a trick, allowing the derivation of the equation of motion of the fluid (just like Planck's quanta will occur some eighty years later, and like Taylor introduced material forces a century earlier). Navier's idea does not fit into the "orthodox" theory of continua and fluids: a molecule is something fundamentally different from an element into which the continuum is cut in this theory. The macroscopic properties of the continuum are not simply transferred onto the molecules as the theory of continua used to transfer the macroscopic properties onto the elements of the continuum. On the contrary, the molecules have properties that are different from the macroscopic properties of matter, and the properties of the continuum are derived statistically (and not simply transferred) from the properties of the molecules. Atoms or molecules, from which a liquid is composed, are *not hypothetical entities* postulated mathematically, but *physically real objects*, although their size, number and characteristics were little known. Physically real means that the discrimination of atoms or molecules is not a hypothetical act of definition (as was the case of the volume element of a continuum) but that atoms are taken to be real material particles.

3.3.2 Expressive Power of the Theory of Atoms and Energies

Perhaps the best illustration of the expressive power of the language of the theory of atoms and energies is chemistry. In the early nineteenth century chemistry had a long history of successful empirical research, but only the theory of atoms and energies was able to put the results of this research on solid foundations. Newton intensively worked on alchemy. This aspect of his activities is often ignored or it is regarded as an irrational thread of his character. In my opinion, Newton's alchemical research was rational, but it must be interpreted on the background of the contemporary understanding of matter. Matter, as Descartes, Newton or Euler understood it, was something almost mathematical. Although matter had various properties such as elasticity, density, color and so on, they could be changed by means of external influences. As Descartes showed on the example of wax, by heating the wax it loses its form, color and solidity and receives new features such as transparency and fluidity. Attributes of substances are therefore not stable, they do not determine the nature of things, but are just accidents that can be avoided. Against the background of this understanding of matter the belief in the possibility of transmutation of elements must be seen as a rational option. The systematic failure of all attempts of transmutation was incomprehensible from the point of view of Cartesian and Newtonian physics, but also from the point of view of the theory of continua and fluids. Only when in the framework of the theory of atoms and energies the concept of a chemical element was born, did these failures became explicable.

3.3.3 Explanatory Power of the Theory of Atoms and Energies

The phenomenon of irreversibility was discovered already in the framework of the theory of continua and fluids when Fourier derived his equation of heat conduction. Heat always flows from the warmer body to the colder one and not in the opposite direction. This phenomenon is in conflict with Newtonian physics because its laws are invariant to the change of the direction of time and thus to each process its inverse is also possible. But heat cannot flow in the opposite direction. As long as physicists believed in the existence of caloric, the phenomenon of irreversibility of thermal processes could be interpreted as one of the peculiarities of this weightless substance. However, when in 1843 Joule showed that caloric does not exists and heat is the kinetic energy of atomic motion, the irreversibility of thermal processes got into conflict with the reversibility of physical laws describing motion at the microscopic level. The Austrian physicist Joseph Loschmidt formulated this objection in 1876. Rudolf Clausius expressed the irreversibility in a mathematical form in 1865 using the concept of entropy (Clausius). Conceptual clarification of irreversibility was achieved by Ludwig Boltzmann in 1877 (Boltzmann 1877, 1909). This clarification can be seen as an illustration of the explanatory power of the language of the theory of atoms and energies.

3.3.4 Integrative Power of the Theory of Atoms and Energies

The integrative power of the language of the theory of atoms and energies is manifested in the law of the conservation of energy. Phenomena that were at the previous stages of the development of physics described as dissimilar and their dissimilarity was even fixed by the postulation of different fluids, such as phlogiston or caloric, were now incorporated into a single framework. The unity of the previous (mechanistic) worldview was a formal unity. It was based on the fact that different areas of physics have been described in the same way – by postulating a fluid and describing its motion by means of a differential equation. But every region has retained its independence due to its special fluid. So thermodynamics described the motion of caloric, electrodynamics the motion of the electric fluid and the theory of combustion described the motion of phlogiston. The theory of atoms and energies either totally discarded the particular fluid (as in the case of caloric or phlogiston) or atomized it (as in the case of the electric fluid), and in this way incorporated all the above-discussed phenomena into a single framework. Mechanical, acoustic, thermal, optical, electrical, and chemical phenomena are processes of transmission or transformation of energy. The integrative power of the language of the theory of atoms and energies contributed to the technological revolution of the nineteenth century. As long as we viewed technology through the language of the theory of continua and fluids, it disintegrated into partial areas corresponding to the particular fluids. Only the concept of energy enables us to unite these partial areas into a single industrial technology.

3.3.5 Analytical Boundaries of the Theory of Atoms and Energies

In 1820 André Marie Ampère discovered the force acting between two electric currents. He formulated a quantitative law describing how two elements of current act on each other. When in 1897 it was discovered that electric current is a collective motion of charged particles, it followed from Ampère's law that an electric current must act not only on another current but also on moving charged particles. In the case of a single particle moving with constant velocity we can calculate from Ampere's law the force that acts on it from the side of the current. But there is a hidden problem here. If we look at the same situation from the viewpoint of the coordinate system joined with the charged particle, in this system the particle is motionless and the current moves. Nevertheless, it is well known that an electric current does not act by force on a motionless particle. Thus, whether a current acts by a force on a charged particle or not depends on the coordinate system in which we describe the situation. But forces exist objectively, independently of the choice of the coordinate system in which we describe them. This shows that in the theory of atoms and energies *the change of the coordinate system has an external character*. It is not tied to the experimental procedures. This paradox may thus be added to the list of Kant's antinomies. It indicates the analytic boundaries of the language of the theory of atoms and energies.

3.3.6 Expressive Boundaries of the Theory of Atoms and Energies

If we take matter as composed of atoms and atoms as tiny balls, there is no reason why these tiny balls could not rotate and vibrate. Assuming that the laws of mechanics apply on the atomic level, each atom can be seen as a rigid body with an infinite number of degrees of freedom, one for every mode of internal oscillations. In the state of thermal equilibrium at temperature T each degree of freedom has energy equal to kT. Since in classical mechanics the energy of oscillations is proportional to their amplitude, internal oscillations can be brought about with arbitrarily small energy. From the fact that there are an infinite number of internal degrees of freedom which can absorb energy, it follows that *eventually all energy will be absorbed by internal degrees of freedom of atoms*, which of course contradicts our experience. It might be objected that every macroscopic body has also an infinite number of internal degrees of freedom and so this paradox should have appeared already in the theory of continua and fluids. But this is not true. The theory of continua and fluids understood heat as a fluid, so the thermal equilibrium and the oscillations of the body were ontologically separated. Only when the theory of atoms and energies interpreted heat as the energy of atomic motion, could a connection between the internal degrees of freedom and the distribution of heat appear.

3.4 Field Theory

Field theory grew out of the work of Michael Faraday, who introduced the concept of field lines to visualize the action of electric and magnetic forces on charges, currents and magnets. Faraday used this notion in the description of the electromagnetic induction, which he discovered in 1831. Most physicists did not take Faraday's lines of force seriously, seeing in them only a heuristic device that may be helpful in discovering new facts, but adds nothing to the facts themselves. James Clark Maxwell came to the conclusion that Faraday's field lines are not merely a heuristic device for visualization of electromagnetic phenomena, but have an independent physical content. Thus he rewrote Faraday's qualitative considerations into a mathematical form and in the 1860s gradually turned Faraday's concept of field lines into that of a field. Maxwell's ideas were further developed by Hendrik Lorentz who incorporated into Maxwell's theory a description of the interaction between the field and matter. Lorentz attempted to reconcile field theory with the theory of atoms and energies. To this end he introduced laws describing the contraction of atoms in motion, now called *Lorentz transformations*. Gradually it became clear that no such reconciliation is possible and in 1905 independently Albert Einstein and Henri Poincaré concluded that field theory is a new representation, requiring new interpretation of the fundamental categories of time and space. The speed of light, which previously characterized the spread of electromagnetic waves, i.e. a relatively limited range of phenomena, suddenly appeared in the definition of mass or in equations describing the transformation of coordinates between different coordinate systems. Field theory was no longer a theory describing a limited class of phenomena. It became a language used in the description of almost all phenomena. Field theory turned into a representation of the entire world of physics.

3.4.1 Analytical Power of Field Theory

The analytical power of the language of field theory can be illustrated by Maxwell's discovery of the displacement current. When he rewrote into mathematical form all the known facts about electric and magnetic fields, he found that the equations thus obtained are asymmetrical. A changing magnetic field generates an electric field (Faraday's law of electromagnetic induction), but a changing electric field had no analogous effect. Led by the idea of symmetry, Maxwell postulated the existence of a magnetic field generated by a changing electric field. This effect was not yet discovered because its detection requires very special conditions which cannot be discovered by chance. When he introduced into the equations an additional term in order to make the equations symmetric, he found that they have a solution in the form of electromagnetic waves. Maxwell published his discovery in 1873 and 1886 Heinrich Hertz proved experimentally the existence of the electromagnetic waves predicted by Maxwell. Shortly thereafter a technical development started, leading from the telegraph and radio through television and radar to telecommunications satellites and mobile phones. Nevertheless, it is important to remember that all these developments started on a sheet of paper when the idea of symmetry led Maxwell to introduce a new term into his equations. Maxwell's discovery can be seen as an illustration of the analytical power of the language of physics. The transcription of experimental data into a mathematical form of differential equations revealed a gap in the data and the introduction of a new term into these equations led to the prediction of electromagnetic waves. When Maxwell determined the velocity of these waves, he obtained the speed of light, known from optics. This led him to the bold idea to interpret light as electromagnetic waves and to *derive the laws of optics from the laws of electrodynamics*. This derivation can be seen as an illustration of the analytical power of the language of field theory.

3.4.2 Expressive Power of Field Theory

To illustrate the expressive power of the language of field theory we can take Einstein's theory of the rotation of Mercury's perihelion. Astronomers knew for some time about the rotation of Mercury's perihelion, which is 43 arcseconds per century. Newtonian theory could not explain that value. This discrepancy between the experimental data and the theoretically predicted value resembles Newton's calculation of the speed of sound. As in that case, also here we are dealing with the boundaries of the language of Newtonian physics. Einstein's derivation of the correct value of the rotation velocity illustrates the increase of the expressive power of the language of physics.

3.4.3 Explanatory Power of Field Theory

We illustrated the analytical boundaries of the language of the theory of atoms and energies by the following paradox: When we turn in our description of a moving charged particle to the coordinate system coupled to that particle, the force by which an electric current acts on this particle, disappears. This paradox can be explained only in the framework of field theory. The trick is that when we turn to the coordinate system coupled to the flying particle, the conductor in which the electric current flows starts to move with respect to us. As a result of this motion a relativistic length contraction in the moving conductor will appear. This contraction will shorten the intervals between the positive charges of the metal grid as well as those between the negative charges forming an electric current. Nevertheless, as the grid and the negative charges move with different velocities, the corresponding contractions will be different, and this will lead to the formation of a non-compensated charge on the conductor. Thus, the conductor ceases to be electrically neutral, and so the charged particle will be subject to a force also in the coordinate system coupled to it. This force will produce the same effects as Ampere's law predicts for the original system. Length contraction is an aspect of the language of field theory. Therefore our example illustrates the explanatory power of this language.

3.4.4 Integrative Power of Field Theory

An example of the integrative power of the language of field theory is the discovery of the *connection between space and time*. From the viewpoint of classical physics time and space are fundamentally different. Despite the deep difference of how time and space are experienced by us, field theory discovered their connection. Time and space form a four-dimensional continuum, called space–time. Thus the language of field theory introduced into the physical image of the world a deeper unity.

3.4.5 Analytical Boundaries of Field Theory

Field theory cannot explain how it is possible that the bodies around us are stable and do not alter their shape. Electric and magnetic forces have an interesting feature – they cannot sustain a *stable configuration of charged particles*. The reason is as follows: Suppose that we would like to create a configuration of several charged particles so that it would be stable with respect to small perturbations. This would mean that if we choose one of these particles, the other particles should create in its vicinity such a field that after a small change of the position of our chosen particle from its position, the forces of the field would return it (this is the meaning of the notion of stability). Thus if we imagine a small sphere around our particle, so small that there are no other charges in it, the lines of the field generated by the remaining particles must intersect this sphere pointing inwards (so that if the chosen particle would try to leave the sphere the field would return it back). But according to Maxwell's equations this is not possible.

Trying to create a stable position as a dynamic configuration cannot save the situation. Everyone knows the toy that cannot stand on its top, but when you spin it, it does so easily. Therefore, one could think that in a similar fashion even if a static configuration of charged particles cannot be stable, a stable configuration can be obtained as some dynamic structure. Unfortunately, this hope is quickly shattered by Maxwell's equations, because in such a case the motion of the charged particles must be curvilinear. But moving along a curve, the charged particle would emit electromagnetic radiation and thus lose energy. In an atom, its electrons would after a very short time fall on the nucleus. The material substance must therefore be held together by something that is inexpressible in the language of field theory. For field theory the stability of matter is a mystery.

3.4.6 Expressive Boundaries of Field Theory

During the nineteenth century the experimental research of black body radiation achieved considerable results. Physicists measured the curves that indicate the intensity of radiation at different parts of the spectrum for many different temperatures. Wilhelm Wien formulated in 1894 a law that approximated these curves rather well at high frequencies, but at low frequencies it led to divergence (the so-called infrared divergence). In 1900 Rayleigh and Jeans derived another law, which accurately described the curves at low frequencies, but led to a divergence at high frequencies (the so-called ultraviolet divergence). Thus there were laws working at both ends of the spectrum, but it was not possible to connect these asymptotic laws. The *black body radiation* is thus a phenomenon that characterizes the expressive boundaries of the language of field theory.

4 Conclusions

The aim of the paper was to show the usefulness of the notions of analytical, expressive, explanatory, and integrative power of the language of physics for the interpretation of the development of physics. In a short paper it was not possible to supplement our analysis with all the necessary historical detail. Nevertheless, I hope that the reader was able to form an idea of how this reconstruction works.

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The Relationship Between Physics and Mathematics in the XIXth Century: The Disregarded Birth of a Foundational Pluralism

Antonino Drago

Abstract In my previous historical works I suggested that four scientific choices constitute the foundations of Physics. By means of these choices I will interpret the history of the relationship between Mathematics and Theoretical Physics in the nineteenth century. A particular pair of choices shaped the Newtonian relationship between Mathematics and Physics, which was so efficient in producing new theoretical results that it became a paradigm. In the nineteenth century new formulations of mechanics were made according to different basic choices, so that in theoretical Physics three other pairs of choices began to co-exist with the Newtonian relationship. Very few scientists of that time recognised this pluralism; instead, the community of physicists interpreted the new theories as either mere variations of the dominant one, or loose scientific attitudes to be put aside in order to follow the theoretical progress of the dominant paradigm. But just after the mid-century the pluralism of the relationships between Physics and Mathematics came to the fore again, this time the previous alternative choices shaped new physical theories – thermodynamics, electromagnetism – concerning entirely new fields of phenomena. But this novelty was interpreted as simply a conflict – possibly, a contradiction – between the new basic notions and the old ones; in particular, at the end of the nineteenth century there was a great debate about the theoretical role played by the new notion of energy in contrast with the old notion of force. The persistent lack of awareness of the pluralism of relationships is the reason for both the inconclusiveness of this debate and the dramatic crisis occurring in theoretical Physics from the year 1900. This time the crisis was caused above all by two experimental data (both the quantum $h\nu$ and the light velocity of light c as the highest possible velocity) which are incompatible with the Newtonian relationship between Mathematics and Physics. Correspondingly, two “revolutionary” theories

A. Drago (✉)
University of Pisa, Italy
e-mail: drago@unina.it

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emerged, again according to the alternative choices to those of this paradigm; both theories required a new relationship with Mathematics, including respectively discrete mathematics and groups.

1 Introduction

The encyclopaedic study by Grattan-Guinness on both Mathematics and Physics at the time of the French revolution, pointed out a serious problem in the historiography of Physics of the nineteenth century. The origin of the problem can be traced back to the two distinct attitudes of historians of science in writing their accounts

> [. . .] the principal difficulty I have found [is due to the following fact:] Unfortunately, no major synthesising study of French Mathematics of this period has ever been attempted [. . .] why our ignorance of these developments is so profound [?].[1]

It is the purpose of this paper to solve this problem by introducing a novelty, i.e. an essential pluralism in both Mathematics and theoretical Physics and hence in the relationship between Physics and Mathematics (RPM).

2 The Two Basic Options in the Foundations of Physics

To say that the foundations of Physics include both Mathematics and Logic is a trivial statement, which, however, becomes very significant when we recognise a dichotomous option in the foundations of both Mathematics and Logic.

Studies in the foundations of Mathematics in the last century translated the two philosophical choices of either actual infinity (AI) or potential infinity (PI) into the choices of either *classical mathematics* or *constructive mathematics* (Markov; Bishop). Let us recall that classical mathematics relies on AI since its first notions; e.g., the classical method for defining the $\varepsilon - \delta$ limit implies AI because – as Du Bois Raymond first stated – it adds to the constructive approximation process the ability to choose an accurately determined point among infinite points (Kogbetlianz, Appendix 2). One may add as further notions implying AI, Zermelo's axiom, Zorn's lemma etc. Instead, both Markov's and Bishop's mathematics rely on PI. In actual fact, Markov applied the principle suggested by him (the Markov principle) which is more powerful than potential infinity; Bishop's mathematics also applies some questionable principles. But these local additions to PI do not substantially change the great distance between their resulting mathematics and classical mathematics.

[1]Grattan-Guinness (1990), p. 61.

In fact, the notions of constructive mathematics are more adequate to physical notions (e.g., entropy) and principles (e.g., the thermodynamic third), as well as of discarding some formulations of a theory as undecidable either in their principles or in their mathematical techniques (Drago 1986, 1991, 1996; Da Costa and Doria 1991). Hence, it offers a new view of the history of theoretical Physics, branching according to several lines of historical development.

In the history of classical physical theories to this option on the kind of Mathematics corresponds a clear-cut distinction between the theories making use of respectively the infinitesimal analysis or the mathematics of at most the elementary functions which are all proved to be constructive functions.

Incidentally, in the past the two choices for either AI or PI have been recognised by Koyré's analysis (Koyré 1959) of the shorter historical period of the birth of modern science. Galileo devoted the long journey of his last book (Galileo 1638, III Day) to debate the kind of infinity a theoretical physicist had to choose; but he confessed he was unable to decide. This conclusion indirectly stated the non-mathematical nature of this issue, which indeed represents an option (between the above two kinds of infinity). Subsequently, Lazare Carnot (1803, 3), Duhem (1906, pt. II, Sect. 4), and Weyl (Weyl 1929, vii) suggested the alternative choice to the dominant one.

Moreover, several scholars have already maintained that in the foundations of classical physics two choices are possible: either *a deductive organisation of a theory* from few principles, as Aristotle first suggested (AO), or *an organisation of a theory aimed at solving a basic problem* (PO). These choices were recognised by D'Alembert (1754), Lazare Carnot (1786, 101–103, 1803, xvii), Poincaré (Poincaré 1903, chap. *Optique et Electricité*, 1905, chap. VII) and Einstein (Einstein 1934).

Each such choice is formalised as the choice of *one of two basic kinds of logic*. Indeed, a deductive theory requires a deductive process that is absolutely reliable; such a requirement is satisfied by *classical logic* only, because it makes a clear-cut distinction between True and False. On the other hand, an organisation aimed at solving a basic problem has to look for a new scientific method, which is discovered only through inductive reasoning, which is based on a different logic from the classical one, i.e. by *a non-classical logic*.

In the original writings founding such theories, a kind of logic is manifested in the occurrences of doubly negated statements, whose corresponding affirmative statements lack scientific evidence (DNSs) (Drago 2005); i.e. in such cases the law of double negation fails; this law constitutes the very borderline between classical logic and (most kinds of) non-classical logic (van Dalen and Troelstra 1988, I, 56ss). The DNSs usually are chained up together by the author with the aim of proving a thesis through an *ad absurdum* proof. (See the instances of Avogadro's way of arguing in chemistry (Drago and Oliva 1999); Sadi Carnot's way in thermodynamics (Drago and Pisano 2000); Lagrange's way in mechanics (Drago 2009); Einstein's way in special relativity (Drago 2010), Lobachevsky's way in non-Euclidean geometry (Bazhanov and Drago 2010)).

The following table illustrates the above choices both from a philosophical and a formal mathematical point of view. It changes the commonly vague idea of the foundations of Physics into a formal structure of mathematised components.[2]

Table 1 The basic choices in the foundations of science

	AI	PI	AO	PO
Intuitive philosophical notion	Actual infinity	Potential infinity	Aristotelian organisation	Problem-based organisation
Formal mathematical notion	Classical mathematics	Constructive mathematics	Classical logic	Non-classical logic

Legenda: *AI* actual infinity, *AO* Aristotelian organisation, *PI* potential infinity, *PO* problem-based organisation

Since the two choices on each option are dichotomous in nature, it is impossible for the same scientific theory to rely on both choices of an option, e.g. on either both AI and PI, or both PO and AO. Owing to the dichotomous character of each option, there exist four pairs of choices. Each pair of choices of the two options shapes a *model of scientific theory* (= MST), which is the ideal model for all theories relying upon the same two basic choices.

The fact that each pair of theories differs in its basic choices, and hence in its MSTs, gives rise to a phenomenon of mutual *incommensurability*. This definition formalises the intuitive notion introduced by both Feyerabend (Feyerabend 1969, chap. 17) and Kuhn (Kuhn 1969, chaps. IX–XI). The incommensurability is manifested by *radical variations in the meaning* (RVMs) of some basic notions. In the above we have already considered the RVMs in the notions of both infinity (either AI or PI) and organisation of a theory (either PO or AO). But several more specific notions undergo RVMs. As an instance, let us take as incommensurable theories Newton's mechanics and phenomenological thermodynamics – whose mathematics makes use of the most elementary functions, that are proved to be all constructive (PI); they present the following RVMs in their basic notions: time – either a continuous variable, or the two-valued variable after/then[3] –; dynamics – either an infinite trajectory or a finite process –; etc.

The incommensurability is also theoretical; when a theory plays such a dominant role in theoretical Physics as to obscure any different MST it is called *a paradigm*. This definition formalises Kuhn's celebrated, albeit intuitive notion (Mastermann 1970). Notoriously Newtonian mechanics was a paradigm for two centuries; that means that its basic choices – i.e. a deductive organisation from the celebrated

[2]It is remarkable that Physics curricula – both at the high schools and at the Universities – share the same quadripartite division of theoretical physics (Drago 2004b). This fact shows, on one hand, ingenuity on the part of physics teachers and, on the other hand, the relevance of the foundational framework suggested by the two options.

[3]The correspondence Clarke–Leibniz (Alexander 1956) illustrates a polemic about just this RVM.

three principles (AO) and the use of the infinitesimals which directly represented actual infinity in mathematics (AI) – obscured the respective alternative choices as representing a backward attitude.

Unfortunately, all physicists (apart from a few exceptions) ignored the two independent choices in the foundations of Physics. Rather, they naively made use of a basic notion (e.g., force) in order to imagine the foundations of the entire corpus of theoretical physics; with deeper insight they reconsidered and chose a single theory as constituting the foundations.

In summary, from the above illustration we have obtained several categories (two options, four choices, four MSTs, incommensurability, RVM, notions and theories) which were seen as constituting the foundations of theoretical physics as a whole; They all allow us to interpret the history of theoretical Physics in a detailed way. In the following we shall apply them to the history of nineteenth century Physics.

3 The Births of Four Kinds of RPMs: Incommensurability Phenomena

Let us now consider the RPM. It undergoes RVMs because it depends, of course, on the two choices of Mathematics, but it also depends on the two choices of the organisations of a physical theory; an AO theory – e.g. Newton's – attributes to mathematics the role of an abstract mathematics which is pre-conceived with respect to the contents of the physical theory; a PO theory – e.g. phenomenological thermodynamics – attributes to mathematics an instrumental role for merely formalising the experimental phenomena at issue.

Hence, we have to consider four kinds of RPMs corresponding to the four couples of basic choices characterising the four MSTs. We will call them Newtonian, Lagrangian, Descartesian, and Carnotian according to the scientists who suggested the respective representative theories.

The Descartesian RPM was born through the first complete physical theory, i.e. geometrical optics. It applied to optical phenomena the pre-conceived geometry of the ruler and compass whose operative use assured that the resulting mathematics avoids actual infinity (PI). The organisation of the theory is a deductive one (AO) from two well-known principles.

With the birth of his mechanics, Newton's AO and AI choices concretized a particular RPM which we call *the Newtonian RPM*; the organisation of his main theory begins from three principles (AO) which apply the pre-conceived mathematics of metaphysical infinitesimals (AI).[4] Since Newton's choice of the

[4]Some historians (e.g., Guicciardini 1999) considered that Newton's mathematics made use of a "geometrical method"; yet, he solved problems which are manifestly impossible in Euclidean geometry. Rather, Newton's mathematical method relied upon geometrical *intuition*, which Cavalieri and Torricelli had already promoted to an AI technique for obtaining a complete theory of calculus, although a less powerful one than Leibniz' calculus (Drago 2003a).

kind of mathematics was different from the PI of the previous theory, these theories gave rise to an incommensurability phenomenon. He unsuccessfully tried to introduce infinitesimals into optics (Shapiro 1984). He did however succeed in including the previous theory as a particular case of his mechanics by conceiving of light as being composed of massive quanta (at the unrecognised theoretical cost of conceiving them through extraordinary properties, i.e. as moving at an infinite speed and mutually destroying each other in the interference phenomena). As a result of this misleading "resolution" of the first incommensurability phenomenon, Descartesian optics – and hence its RPM – was relegated to a subordinate role and later was ignored. Thus began the tradition of considering the Newtonian RPM as the only RPM in theoretical physics; indeed, the fact that for two millennia AO was considered the only organisation of a scientific theory, together with the discovery of the marvellous mathematical power of infinitesimals (AI), convinced physicists that theoretical physics had accomplished an epochal transition. In fact, for a century the Newtonian RPM dominated theoretical physics through both its metaphysical mathematics (AI) and its Faustian notion of force–cause, whose specification for a field of phenomena was capable of including it in an AO theory.

The Carnotian RPM. A century later the well-known mathematician, physicist and strategist L. Carnot suggested a formulation of mechanics which dismissed the entire Newtonian theoretical framework (Carnot 1786). He included Leibniz's laws regarding the elastic impact of bodies in a complete theory of all mechanical phenomena through the introduction of two novelties. First, he suggested an index of elasticity, making it possible to take into account both elastic and anelastic bodies; in such a way his theory offered a complete solution to the problem (PO), which, according to him, was the basic one for the entire theory of mechanics: What are the invariants during an impact of whatsoever bodies. Moreover, he derived the impact laws from a new mathematical principle, i.e. the principle of virtual velocities, although formalised through a notion of virtual displacement which is different from the usual one.[5] That amounted to an instrumental use of the trigonometric functions and the simpler functions, which are proved to be constructive functions (PI).

Remarkably, he intelligently illustrated, in a pluralist spirit, the option concerning the two kinds of organisation of a theory:

> Among the philosophers who are occupied with research into the laws of motion, some make of mechanics an experimental science, the others, a purely rational science; that is to say, the former in comparing the phenomena of nature break them up [. . .] in order to know what they have in common, and thereby reduce them to a small number of principal facts [. . .]; the others begin with hypotheses, then reasoning in consequence upon their suppositions, come to discover the laws which bodies follow in their motions, if these hypotheses conform to nature, then comparing the results with the phenomena, and finding them to be in accord, conclude from this that their hypothesis is exact.[6]

[5]Its foundations generalised also D'Alembert's attempt to give a new foundation to mechanics (Hankins 1970, pp 174–176).

[6]Carnot (1786, p. 102).

Moreover, he stated that "all ideas come from the senses", mathematical ideas, too (Carnot 1803, 2–3). Hence, being operative in nature, they do not imply AI. At almost the same time as the birth of L. Carnot's theory, Lavoisier suggested a new theory, Chemistry (1789), which shocked contemporary scientists. Manifestly its mathematics did not overcome PI; in particular, a chemical reaction was described by merely rational numbers. Moreover, this theory is based upon the problem (PO) of identifying all the constitutive elements of matter. It reiterated the Carnot RPM, which is mutually incommensurable with the Newtonian RPM in both choices. About the great distance between the two theories see (Thackray 1970).

One more interesting case is represented by the birth in 1784 of crystallography by Haüy. Its Mathematics, trigonometry, was elementary (PI) and the theory is aimed to solve the problem of how many crystals are possible in nature (PO). Hence, this theory too was incommensurable with the Newtonian RPM. Until the first decades of the nineteenth century this theory was included by textbooks as one of the subjects of Physics; later, it was relegated to an auxiliary role in an applied science, i.e. Mineralogy.

The Lagrangian RPM. At the same time, Lagrange suggested one more formulation of mechanics (Lagrange 1788). This theory made an instrumental use of metaphysical infinitesimals (AI), because they "shorten and simplify" its development (Lagrange 1788. See *Preface*, II edition 1811).[7] Lagrange claimed to offer a new technique capable of solving any physical problem (PO).[8]

A further challenge to the dominant role of Newtonian RPM was the discovery of phenomena which at present belong to the subject of physical optics – e.g. polarization (1801) –; they could not be explained by any hypothesis derived from Newton's mechanics. A remedy was suggested by appealing to new differential equations relating to the Newtonian RPM, but at the cost of dismissing Newton's hypotheses and admitting Christiaan Huygens' rival hypothesis – i.e. light as a wave – and moreover at the cost of a long work of some decades to formalise wave phenomena by means of this new mathematical technique. However, geometrical optics was *no longer being* disregarded; hence, the return of the Descartesian RPM.

As a consequence, from the end of the eighteenth century new theories and above all the new *formulations* of mechanics gave rise to *different RPMs* so that *the different representative theories brought about incommensurabilities.*

[7]Let us remark that in the common language of that time the word "algebraic method" often means "by means of calculus", since Lagrange, in his *Theorie des Fonctions* (Lagrange 1797), wanted to reduce the latter to something like an algebra.

[8]I leave aside Maupertuis' mechanics since at that time it received dubious appraisals.

4 The Manifest Conflicts Generated by Mutually Incommensurable RPMs

At present we know that conflicts between two theories, belonging to *two different RPMs*, are unavoidable owing to mutual incommensurability, together with the radical variations in meanings of their basic notions. In the history of theoretical Physics what conflicts among scientists were caused by incommensurabilities?

I have insufficient space to fully illustrate three first cases of incommensurability but will identify them here. Firstly, the case of *the theory of the impact of bodies* in the two versions, Newton's and Leibniz' (Dugas 1950). Secondly, *the three different mechanical interpretations of the acoustic phenomena* by respectively Euler, D'Alembert and Bernoulli (Drago and Romano 1995). Third, *the first theories of gases*; through a pre-conception of a metaphysical kind, Newton hindered further advances in gas theory for a century (Cardwell 1971, chap. 1). At that time it was precisely this accusation made against physicists by the chemist F. Venel, i.e. that of being unable to understand real phenomena, while chemists "[. . .] are curious of neither infinity, nor physical novels." They are free of "[. . .] the mistakes which disfigured Physics" (Venel 1754, p. 388 II).

Let us rather consider the time around the French revolution, when the creation of new theories, and hence the creation of new RPMs, caused conflicts since some scientists abandoned the dominant Newtonian RPM.

Carnot's mechanics, owing to the mutual incommensurability of its RPM with the Newtonian RPM in both basic choices, was undervalued in that time and even more in subsequent times; the common view of the followers of the dominant Newtonian MST was that it belonged to a specifically engineering approach.

The birth of chemistry, belonging to the new Carnot RPM, was called the "chemical revolution" and generated long, heated debates among scientists. Lavoisier's foundation of a new theory was suspected to be a strange deviation from the scientific realm.

Lagrange's theory was also incommensurable with the Newtonian RPM owing to its different organisation (PO). Although the former theory proved to be more powerful than the latter, the great scientific influence of the follower of Newton's mechanics, Laplace, confined this mechanics to a lateral role.

As a conclusion, the incommensurabilities prevented an adequate appraisal of the new theories. Moreover, at the end of the eighteenth century a great debate started on *the role played by the principle of virtual velocities in the theory of mechanics*. Lagrange stated that the basic notions of Newtonian mechanics are insufficient to study constrained motions, because we do not know constraints' reactions *a priori* (Lagrange 1788, pt II, I, 4). Hence he explicitly founded his mechanics on this new principle; which however in Lagrange's opinion (Lagrange 1788, pt I, I, 1.18) was not abstract enough from phenomena to play the role of the first axiom in a deductive theory (AO); hence, he proposed to "demonstrate" it. There followed a concerted theoretical effort on the part of the major physicists of his time to include the new principle in the old deductive organisation of mechanics; but unsuccessfully. After some decades, owing to a declining interest in a seemingly unsolvable question the entire debate extinguished itself (Poinsot 1975).

At present we recognise that, according to Lagrange's former remark about our a priori ignorance of constraints' reactions, the principle of virtual velocities constitutes the methodological principle for solving the basic problem of how to overcome ignorance of reactions to constraints; hence, Lagrange misinterpreted the role of the principle of virtual velocities because it initiates not an AO, but a PO theory; as a consequence, it is incompatible with the Newtonian RPM. Unfortunately, the debate among physicists did not recognise that this principle starts development of the alternative organisation to PO, already described by Lazare Carnot.

Incidentally, let us add that at the same time in the foundations of Mathematics the main problem was how to demonstrate Euclid's fifth postulate. In the 1830s two scholars, both living in peripheral areas with respect to European science (the Russian Lobachevsky and the Hungarian Bolyai), bravely suggested a new answer; i.e. no longer the search for a proof, rather the assumption of the *independence* of this postulate from the others; by looking for new versions of this postulate they developed new geometrical theories. In fact, both scholars founded their theories according to the PO (Bazhanov and Drago 2010; Drago 2002). Why was it that no physicist, in the same way as these mathematicians, recognised that *the principle of virtual velocities is independent* of Newtonian principles? (In this story the most unfortunate fact is the loss of a paper by Lobachevsky on the principle of virtual velocities (Bazhanov and Drago 1998, 132) that may be conceived in a PO theory).

There also was a vigorous, and also inconclusive, debate about the option concerning the kind of mathematics. The elementary mathematics in L. Carnot's mechanics, Chemistry and Thermodynamics was commonly disqualified as incomparably weaker than the calculus founded upon infinitesimals. Instead the previous principle does not necessarily use infinitesimal analysis – a fact which is clear if the several versions of it are examined (Drago 1993); but no one stressed this point. However, Lagrange's *Théorie des Fonctions* (Lagrange 1797) suggested for calculus a new foundation exploiting the Taylor expansion of a function. Since at present we know that all analytical functions belong to constructive mathematics (Bishop 1967, 120ff), we recognise that this foundation of calculus closely approached the calculus of constructive mathematics; hence, Lagrange's theory, lacking infinitesimals, relying on PI.

But Lagrange himself undervalued the novelty by dismissing it when, introducing his celebrated mechanics, he made use of infinitesimal analysis. Moreover, Cauchy contrasted with this foundation certain functions that cannot be developed in Taylor series, as if this fact irremediably contradicted the whole of Lagrange's foundation. Afterwards, Lagrange's alternative was dismissed, whereas Cauchy started a "rigorous reform" of calculus, according to which classical mathematics should be free of infinitesimals, but again it actually relied on AI, even if covertly.[9] Hence this debate also was unsuccessful in recognising the corresponding option, between AI and PI and the dominant theoretical physics saw the use of classical mathematics as the only one possible.

Following the *birth* of incommensurabilities, physicists lost their awareness of the historical evolution of theoretical physics. During the Restoration, each time two theories presented radical differences in their structures the dominant group of physicists misinterpreted the theoretical differences as mere innovations to be derived from the old Newtonian RPM. For example, when Ampère succeeded in formalising through calculus the laws of some phenomena belonging to the – previously seemingly irreducible – field of electricity, it was considered that this new field of phenomena could finally be fitted into the old RPM and he was celebrated as "the new Newton of electricity". Instead, just a few years later, the surprising Oersted's experiment, linking together electricity with magnetism, started a new theoretical perspective.

Finally, theoretical physicists obscured the new theories belonging to different RPMs. Although Lagrange's mechanics was exalted by Comte as a very advanced theory because it was almost entirely included in calculus (of course, AI) it was undervalued for several decades[10] (as was Hamilton's mechanics later). L. Carnot's mechanics was also obscured as a specific mechanics and eventually ignored. It has been re-discovered in recent times, 200 years after its birth (Gillispie 1971; Drago 2004a).

Lavoisier's Chemistry was the object of even worse censure, owing to both its apparently insignificant mathematics and the large number of RVMs in its basic notions with respect to those of mechanics. It was impossible to include Chemistry in the paradigmatic RPM; however, it was also impossible to obscure it; since at that time scientists were becoming increasingly professionalised, it obtained a strong support from a specific social milieu; it was indispensable to both pharmacists and the emerging chemical engineers (Ben-David 1964, chap. VI). E.g. in 1804 in reactionary Russia, Kazan University was founded essentially to support the chemical engineers in local mines (Vuchinich 1963; Zagoskin 1906). Eventually, since in the history of Science, Chemistry had marked a radical divergence from Newton's theoretical legacy, conversely, it was detached – in agreement also with the revolutionary Comte (Comte 1830–1842, Book II, 304, Book III, 4, 28–29) from the body of the physical theories; it was considered a different science, if at all a science, maybe a metaphysics.

[9] Recall Du Bois Raymond's criticism of Cauchy's inclusion of AI in the definition of a mathematical limit. However, also Cauchy's mere restriction of AI met with strong resistance; for instance, in England, Newtonian calculus was not dismissed before the end of the century.

[10] Poisson's rivalry with Lagrange is well known (Duhem 1903, 72). In fact, one main difference between Poisson's mechanics and Lagrange's concerned the limit operation from a sum to an integral (*Ivi*, pp. 81ff), precisely the crucial notion marking the difference between constructive mathematics and classical mathematics.

5 Restoration of the Newtonian Paradigm and the Almost Ignored Inventions of Radical Novelties

In the first decades of the nineteenth century a successful attempt to restore the Newtonian RPM followed. After the 1820s the dominant role of Newtonian RPM in theoretical Physics appeared to *most relevant* physicists (Laplace, Poisson, Cauchy, etc.) to have been restored, as if the previous birth of a pluralism in the RPMs had never occurred. However, this operation implied also a high cost in physicists' awareness of the foundations of theoretical physics. First of all, the physicists represented them using inadequate intellectual tools, i.e. not the two basic choices of the Newtonian RPM, AO and AI, but either the basic notion of the Newtonian RPM, i.e. force, or Newton's mechanics itself. Secondly, the program of these Newtonian physicists, which involved either translating theories into the old RPM or obscuring them, met crucial defeats. Laplace's extended the notion of gravitational force to all central forces; but this way of explaining the relevant gas laws was very cumbersome. Fourier also improved this RPM through a new differential equation explaining heat transport; but he failed to provide a comprehensive theory of heat phenomena. After 15 years of partial advances, he concluded in the preface of his book that heat phenomena are irreducible to the Newtonian mechanics (Fourier 1822, *Discours préliminaire*). Several theorists enlarged the notion of a force acting on a point-mass to that of a tensor at a point in a body; but they failed to achieve a new general framework for theoretical physics and were only capable of explaining phenomena within matter conceived as an Euler continuum.

Third, the cultural monopoly enjoyed by the Newtonian RPM obstructed theoretical research aimed at building suitable theories regarding new fields of phenomena (heat, electricity, magnetism); these theories were developed thanks to geniuses (e.g. S. Carnot, Faraday) who overcame great theoretical difficulties.[11]

After the discovery of the analogous Coulomb's law in magnetism, physicists aimed at constructing theories of these two fields of phenomena independently and in parallel, of course within the Newtonian RPM. Instead, Faraday was successful in solving the problem (PO) of combining magnetism and electricity into a unitary theory by introducing new notions with respect to the Newtonian RPM. Furthermore, he – as several other physicists of that time (e.g. Melloni) –, deliberately worked "without" mathematics, since it was suspected to be unavoidably the Newtonian one.

[11]It is not by chance that some scientists following different RPMs from the Newtonian suffered persecution. For example, L. Carnot was excluded from the re-born French *Académie des Science* and then expelled from the country. Galois was rejected twice at the admission examination for the *Ecole Polytéchnique* and his revolutionary work was ignored by the academicians; he died very young in mysterious circumstances. At the *Ecole des Arts et Métiers* the classes of Désormes were regularly attended by police spies (Birembaut 1975). Faraday, Comte, Meyer and almost surely Sadi Carnot suffered mental illness. Poncelet's projective geometry, non-Euclidean geometries by Lobachevsky, Gauss, Bolyai, Riemann and Bellavitis' vector calculus suffered academic misfortune.

In 1824 Sadi Carnot produced a very surprising physical theory. Although dealing with the phenomena pertaining to the most advanced technological advances (heat engines), his theory did not make use of the more advanced mathematics of that time, i.e. differential equations (except for a footnote, p 73). Rather, he reiterated the choice of a mathematics of the more simple functions, in this case the logarithmic function, which is a constructive function (PI).[12] Moreover, he did not appeal to (metaphysical) principles – e.g. about the nature of heat – for deducing physical laws; rather, his theory relied upon a problem (PO) – i.e. what is the upper limit to the efficiency of the transformations of heat into work. His reasoning relied in an exemplary way on DNSs (Drago and Pisano 2000). This reasoning, like Avogadro's arguments in chemistry (Drago and Oliva 1999), was centred on a theorem proved without mathematics at all, i.e. by his celebrated *ad absurdum* argument. Moreover, the proof relied on the impossibility of perpetual motion, a sentence stating first of all its impossibility to be translated into mathematics. The new theoretical method made use of a cycle (of transformations), i.e. an unprecedented notion in the characteristic reasoning of previous physical theories (Mach [1896] 1986, chap. XIX).

Owing to their incommensurability with the Newtonian paradigm, both theories generated conflicts through the RVMs in their basic notions. S. Carnot's thermodynamics, as an instance of the Carnot RPM, manifested its incommensurability through the following RVMs: no continuous time, no absolute space but space as a volume, no force, work calculated by $p\Delta V$, etc. In fact, the basic notions of S. Carnot's thermodynamics present more RVMs with Newton's mechanics than with L. Carnot's mechanics; that is, when we compare the formulations of the two mechanics, the differences in the meanings of their basic notions are greater in number than when we compare the thermodynamics of Sadi Carnot with Lazare Carnot's mechanics, although the phenomena theorised by the two theories are very different (Drago 1990). No surprise if even in the necrologies on S. Carnot's death his friends (e.g., see Robelin) qualified his theory as a theory that was "too difficult".

Faraday's electromagnetism also manifested incommensurability with the Newtonian RPM through RVMs of his basic notions: instead of the force–cause, the force–field (a notion imported from chemical notions); instead of a continuous fluid, quantization of an electrical charge; instead of the separation between statics and dynamics in theoretical dynamics, the equality of static electricity and current electricity, etc.. In fact, by listing the basic notions of electromagnetism according to the two rival theories, Newton's and L. Carnot's, one recognises that most of Faraday's notions belong to the Carnot RPM (Drago 2003b, Table 2). No surprise if in this theoretical context, which was dominated by the Newtonian RPM, the full acceptance of both S. Carnot's thermodynamics and Faraday's electromagnetism, each representing a different RPM from the Newtonian one, occurred some decades

[12] In the same years Lobachevsky stressed his opposition to the use of AI (Lobachevsky 1835–1838, *Introduction*); he wrote a book for re-founding calculus by means of discrete mathematics only (Lobachevsky 1833).

later. It is well-known that S. Carnot's theory was reviewed by the *Académie des Sciences de Paris* in 1824; it was re-formulated by Clapeyron in 1834, but Kelvin was unable to recover the original book before 1850, when he, together with Clausius, suggested a new formulation of the theory; S. Carnot's thermodynamics was recognised only 25 years later, in 1850. Faraday's results were not recognized until Maxwell translated them into Newtonian mathematics.

6 The Mid-Century: The Re-births of the Alternative RPMs Through New Theories

In 1850s. Carnot's theory was re-formulated in modern terms. Clausius and Kelvin introduced two principles into the new formulation in order to change the theory into a deductive one (AO); but at the cost of giving precedence to the principle of energy conservation with respect to the results previously discovered by Carnot. Bridgman (Bridgman 1943) compared this paradoxical inversion to a river coming back to the source. However, they confirmed the original mathematics (PI); even the new principle, energy conservation, was represented by means of a mere addition formula. Hence, also this new formulation of thermodynamics was manifestly at odds with the Newtonian RPM. In fact, the two scientists changed the original RPM from the Carnotian to the Descartesian. Moreover, the new formulation preserved all the previously mentioned RVMs in the basic notions of S. Carnot's theory; in addition it attributed a new meaning to what at this time was called – e.g. by Helmholtz, Mayer – "force", for intending energy.

The novelty of the new RPM born from modern thermodynamics constituted a turning point also in the history of some basic notions of previous theoretical physics: it puts an end to (i) the conception of heat as a prolongation of Euler's notion of either a massive fluid (phlogiston) or a fluid without a mass (caloric); rather it was conceived as a form of energy; (ii) Wallis' and Newton's idealisation of a hard body; it was superseded by Leibniz' idealisation of an elastic body; (iii) the old conception of a gas as composed of fixed mass-points interacting through distance-forces; it was superseded by the modern conception (Scott 1970).

Subsequently, electromagnetism was the result of the theoretical effort to solve Faraday's problem (PO), i.e. how to unite those two fields of phenomena, electrical and magnetic, which previously seemed quite different. Its principles exhibited nothing of the metaphysical evidence of the Newtonian ones.

Moreover, classical Chemistry, belonging to the Carnot RPM, was completed as a scientific theory by Mendeleev's invention of the periodic table. Its mathematics amounted to comparing the rational numbers representing the properties of the elements; its basic law, periodicity, was unprecedented in theoretical physics. These new theories again gave rise to pluralism of RPMs in science, this time resulting not from new theories in mechanics, but new physical theories concerning entirely new fields of phenomena.

7 The Debate Between the Two False Consciousness of the Different RPMs

The shock caused by these events among physicists was so great that it broke their long resistance to theoretical novelties; a growing number of physicists and chemists challenged the theoretical and philosophical hegemony of mechanicism, i.e. the Newtonian RPM.

Unfortunately the physicists of the time, unaware that pluralism in the RPMs had already existed for half a century before their time, saw the novelty of a divergence in the foundations of Physics at a low level, i.e. as either a divergence between two basic notions – either force or energy – or a divergence between two theories – either mechanics or thermodynamics (only Rankine (1855) recognised a methodological conflict between Newton's mechanics and thermodynamics); moreover, they considered the divergences between two theories to be win-win conflicts; hence, they ignored incommensurabilities in a time of co-existence of diverging theories.

Whereas W. Thompson launched the idea of the indispensability of a mechanical model in conceiving any physical phenomenon, for a minority of physicists the notion of energy was such an important novelty that they attributed to this new intuitive notion the same dominating role that the notion of force of Newtonian theory had enjoyed in the past.

The growing group of "thermodynamicists" began a polemic with the mechanicists over the foundations of theoretical physics, whether it had to be based on either traditional mechanics or thermodynamics (In the latter group one has to include Mach and, for a first period of his life, Planck). Two programs for developing theoretical physics started.

The mechanicists tried to widen the domain of the applications of mechanics through the invention of new tools (e.g., the notion of tensor, new differential equations and new methods for solving them; it was the birth of modern hydrodynamics and, more in general, Mathematical Physics) but they were unable to produce essentially new theories. In actual fact, the framework of Maxwell's electromagnetic theory was mechanicistic and its main notion was that of vortices (Harman 1998, p. 98). However, some years later, Maxwell had to abandon this notion as inadequate.

Instead, the newly developed thermodynamics showed a great theoretical power; in 1881 its association with chemistry produced physical chemistry, a theory that quickly became – through a great chemist, Ostwald – a prominent theory in both science and society.

The polemic concerned also the role played by Mathematics, seen, however, from an all-or-nothing perspective, i.e. either a theory has to be based on the differential equations given by the infinitesimals, or has to be almost mathematics free. The mechanicist Boltzmann charged thermodynamicists with choosing energy as their basic notion in order to avoid Mathematics; their formulation, being a very simple one, made it too easy for them to generalise their theoretical views to the whole of science (Dugas 1963, 91–92).

As further evidence for an insufficient awareness of the foundations of physics, let us recall that the date of birth of each new theory in the nineteenth century may be contested; each theory was considered as completed, although some foundational parts were added some decades later. E.g., thermodynamics was considered a completed theory in 1850, although a clear illustration of its specific notion, entropy, was given by Planck 45 years later (Planck 1893); moreover, its "zero" principle was suggested much later (Mach 1896) and its third principle was discovered even later (1905). Electromagnetism was considered a completed theory in 1862, when Maxwell wrote his celebrated equations; but its group of transformations was discovered by Lorentz 30 years later (Drago 2003b) and, even more remarkably, its wrong basic notion of the ether was dismissed 50 years later. Statistical mechanics waited in vain for a conclusive proof of its H-theorem. Moreover, the very import of Lagrange's formulation of mechanics, i.e. its essential nature of a theory of invariants, did not become apparent until Noether's theorem (1918).

One may add one more disturbing fact. In glorious theoretical astronomy Poincaré discovered a mistake in the solution of the stability of the solar system – a question which played a critical role in educating the general public to accepting scientific results. Poincaré showed that this question had falsely been considered as solved for one century, because astronomers overestimated AI mathematics as capable of obtaining the convergence of whatsoever series; some series which played crucial roles in past calculations, proved, however, not to be convergent. As a consequence, one of the most impressive results of past science presented to the general public simply collapsed (Barrow-Green).

Unfortunately, the basic choices and hence the true differences among the various RPMs were ignored. No surprise if the debate, by missing the essential points, became an acrimonious dispute among deaf speakers, although in the nineteenth Century for the first time some scientists, beginning from experimental data, constructed entire philosophies (Positivism, Saint-simonism, Materialism). In fact, the objective result of their debates was dubious. Whereas the thermodynamicists' program – to re-found mechanics and the whole of theoretical physics upon the new notion of energy - failed, that of the mechanicists', to include the law of energy conservation within the old theoretical framework of Newton's mechanics was successful. On the other hand, whereas the mechanicist Boltzmann made Herculean efforts to reduce thermodynamics to a statistical interpretation[13] (Boltzmann 1896; Dugas 1963), the thermodynamicist Helmholtz showed, by means of Lagrangian

[13]According to general opinion, this interpretation successfully reduces thermodynamics to Newtonian mechanics, while Boltzmann's basic notions – as shown by a table listing them according to the two rival mechanics, Newton's and L. Carnot's, (Drago 2004a; Drago and Saiello 1995, Table 3) – belong to L. Carnot's alternative mechanics. Moreover, his statistical mechanics, in order to interpret the behaviour of innumerable particles which are subject to mechanical principles (AO), applied a mathematics which was reducible, according to Boltzmann's views, to an essentially discrete mathematics (PI) (Boltzmann 1896). Hence, Boltzmann worked in the Descartesian RPM.

formalism, that the theory of thermodynamics cannot be reduced to Newtonian mechanics, except in the case of a very simple mechanical system, i.e. that described by only one periodic variable (Helmholtz 1884).

Eventually, Ostwald, in his celebrated paper (1895), wanted to impose a sharp division in the scientific community; each scientist had to choose on the basis of a division between mechanicists or thermodynamicists. This publicity of a un-precedented conflict, which for almost half a century split all theoreticians into two different groups, disconcerted the wider public. Even more so its inconclusiveness. In fact, the inadequate appraisal – obtained by means of either a single notion or a single theory – of the new achievements of theoretical Physics had led physicists down a blind alley. "Hence, the 19th Century, which so often celebrated itself as the Century of Science, closed with an unexpected crisis of scientific scepticism" (Brunschvicg 1922, 450)

8 The Crisis in Science of the First Years of the XXth Century

In the first years of the nineteenth Century dramatic events abruptly put an end to the previous debate. Even now this period is remembered by theoretical physicists as a shocking awakening, euphemistically translated in the words *annus mirabilis*.

At this time there emerged further basic novelties that were incompatible with the Newtonian RPM. Rather than some new formulations of an old theory or even new theories, new experimental data, i.e. the energy quantum, $h\nu$, and c, the maximum possible speed, contrasted respectively with the continuum and the infinite space and time of the old RPM.

In fact, the notion of the Newtonian RPM, that of an a priori continuum was definitely excluded by the former theory, whereas the other characteristic notion, absolute space, was excluded by the latter theory. In addition, all the basic notions of the new theories proved to have different meanings from those of the Newtonian RPM. Hence, no one surrogatory notion – either force or energy – nor a surrogatory theory – neither Newton's mechanics nor thermodynamics – of the real foundations of theoretical physics could explain the new theories. This time the theoretical physicists could no longer dismiss the novelties as irrelevant, nor misinterpret them as derived from an extension of the Newtonian RPM.

Actually, the two experimental data represented the re-emergence of precisely the two alternative choices to those of the Newtonian RPM: (i) the choice of discrete mathematics (PI), which was required by the discrete quanta, unavoidably resulting from the interpretation of the spectrum of black body radiation; (ii) the choice of PO, which (although not fully recognised) was required by the big problem of reconciling mechanics with electromagnetism. Hence, the theory of quanta (PI) implicitly suggested either a Carnot RPM, or a Descartesian RPM; whereas special relativity (PO) implicitly suggested either a Carnot RPM or a Lagrangian RPM. In any case, no longer the Newtonian RPM.

9 Conclusions

Surely, in the nineteenth century the established foundations of physics suffered more fundamental revolutions than in other centuries. But, ironically the community of physicists was so unaware of the profound changes occurring at the time, that they ignored the actual evolution of theoretical physics in that century, in particular in the first half of the century they ignored the geniuses, in the second half of the century they ignored the very foundations of the new theories, at the end of the century they ignored the impending radical crisis.

Already a historian (Taton 1964, chap. 9, 1–4) recalled a celebrated appraisal of the nineteenth century, i.e. a "stupid Century". Indeed, the basic choices – and hence the different RPMs – were discovered at the start of the century; L. Carnot did it by comparing the different formulations of the same theory (e.g. mechanics); Lobachevsky did it by comparing the different geometrical approaches; moreover chemistry manifested these choices in its comparison with Newton's mechanics. Surprisingly, subsequent scientists ignored these results.

Even more surprisingly subsequent historians missed them. With regard to the option of the kind of Mathematics, let us recall that Brush and Grattan-Guinness remarked that too many historians of physics leave out Mathematics in their accounts (Grattan-Guinness 1990, 61–63).

This criticism does not apply to Koyré who recognised, although in merely philosophical terms, the option between AI and PI as very relevant in the debate on the birth of modern science. Rather, Kuhn's celebrated account of the history of three centuries of physics (Kuhn 1969) unbelievably completely ignored mathematics (and also a large part of the history of nineteenth century science, e.g. thermodynamics).

However, Brush and Grattan-Guinness and some more recent historians of science (e.g., Truesdell 1960; Garber 1998) offered detailed reports of both the physical and mathematical aspects of the history of Physics in the period we are concerned with. But they did not suggest new interpretations; rather, they suggested either a long list of new details, or a pre-conceived historical account according to advanced mathematics, i.e. AI (in Truesdell, an Eulerian one). In fact, all of these historians share the Newtonian RPM; hence, they could not conceive of any alternative choice determining historical events.

It was for this same reason that the great contributions offered by previous historians – Comte, Mach, Duhem, Jouguet and Brunschvicg – failed to achieve the goal. These historians evaluated Lagrange's mechanics as the greatest achievement in mechanics' history owing to its improved mathematisation, of course, in AI. See for example Mach (Mach 1905, chap. IV, sect. III, 1) and Dugas (Dugas 1950, 309–311). Moreover, they all misinterpreted – as physicists did in the past – the role played by L. Carnot's mechanics as a modest one in theoretical terms and saw it as a mere engineers' physics; hence, they attributed to it a subordinate role to Lagrange's, or at most (e.g. Jouguet, 203) a mere preparation for Lagrange's mechanics. Grattan-Guinness' appraisal of L. Carnot's book (Carnot 1803; see

Grattan-Guinness 1990 sect. 5.2.6 and sect. 16.3.3 under the heading "engineering mechanics") manifests a haste in evaluating it: "a mixture of Euler and Lagrange, with Euler perhaps dominant" (*Ivi*, 295). Moreover, he added no more elements than those of Gillispie's analysis and neither historian related L. Carnot's fundamental equation to the principle of virtual velocities.

In conclusion, they all ignored the new way of organising theory, PO. Surely, in the past the lack of a formalised alternative in Mathematics led both theoretical physicists and historians to consider all kinds of mathematical techniques as subsumed into what was considered the highest level of mathematics, i.e. infinitesimal analysis; hence, they ignored any choice between AI and PI. This pre-conception for AI mathematics as an "indispensable" tool (Hellmann 1993) was the more important pre-conception in the historical accounts of science. Remarkably, one historian stressed this pre-conception with caustic words which apply specifically to the historical development of nineteenth century Physics: "Metaphysics they [the scientists] tended more and more to avoid, so far as they could avoid it; so far as not [i.e. in Mathematics], it became an instrument for their further mathematical conquest of the world" (Burtt 1924, p 303). But such advice was ignored by historians.

Moreover, it is remarkable that one historian of nineteenth century Physics discovered by philosophical ingenuity a foundational pluralism, but a pluralism only in research programmes. Brunschvicg presented four kinds of theoretical physics:

> The physics of central forces [Newtonian mechanics and its prolongation in Laplace's mechanics] [...]. The positivistic physics [Lagrange' mechanics as an ideal theory] [...]. The mechanicist physics [...] [trying to impose in some way mathematics to new experimental phenomena] [...]. Entropy and Energy [the theoretical novelties of thermodynamics].[14]

They almost exactly correspond to the four RPMs; the third one differs from the Descartesian RPM only because it does not qualify applied mathematics as an elementary mathematics; the fourth one differs from the Carnot RPM only because it does not qualify through the foundational choices the divergence of the two notions from the Newtonian ones.

Hence, in past time all historians considered the pluralism of foundational choices as novelties of a merely philosophical interest. Indeed one has to notice a philosophical bias, i.e. the myth of the unity of science, reiterated in each of its branches, e.g. Mathematics and Physics, although some scholars fought against it by using also crude words; e.g.:

> [...] This idea [of the unity of traditional Physics] is a utopian dream, and a rather dangerous one. The Popper view of rationality as a goal-directed, i.e., as problem-solving, method of trial and error is a better view of rationality.[15]

[14] Brunschvicg 1922, 624.

[15] Agassi 1969, 463.

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Index

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