Gabrio Piola and Mathematical Physics

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1 Introduction

In the past, the term ‘mathematical physics’ had substantially two meanings. On one hand, it simply indicated modern physics, which considered mathematics its own language; in this sense, Galileo, Newton, Kepler, etc., were distinguished mathematical physicists. On the other hand, it pointed to the branch of science that developed in the XIX century and had enabled the solution of some specific problems governed by partial differential equations, such as, for instance, heat propagation, potential theory, theory of elasticity; in this sense Fourier, Lamé, Gauss, Piola, Beltrami, etc., stood among the most important mathematical physicists. Today the term indicates an academic discipline, practiced by mathematicians, having some principles of physical nature at its basis.

The relation between mathematics and physics – i.e., mathematical physics in the broad meaning – has been the subject of an endless number of papers, from the historical, epistemological and ‘scientific’ points of view. The mathematical physics of the XIX century, potential theory, and the modern mathematical physics are only a little less investigated.

Rather than giving exhaustive accounts of mathematical physics, the objective of this paper is to use some historical instances to define the meaning that the term ‘mathematical physics’ assumed in some selected historical periods. To begin with, the first instances of application of mathematics to physics, then the first appearance of something like modern mathematical physics, and, eventually, a particular kind of mathematical physics theory,

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called rational mechanics, are discussed. For the sake of brevity, the golden age of physics, ranging from Galileo to Newton, has been ignored, without preventing this paper from reaching its objective, that is, the discussion of the meaning of the discipline called mathematical physics.

2 Epistemological aspects

Examining the epistemological aspects of mathematical physics gives us the opportunity to precise the meaning of the term, or at least to stipulate its appropriate conventional meaning. However, it is necessary to explain what a physical theory is in its essence first. It is made up of three parts:

> An abstract calculus, which comprehends undefined or theoretical terms, definitions, principles and inference rules.
>
> A conceptual model, which, more or less, provides a sensible representation of the interested part of the world (not strictly necessary).
>
> Some correspondence rules, which connect the theoretical terms and the theorems of the theory with the experimental data.

For example, a mechanical theory (a particular kind of physical theory) of the solar system has material point, mass, force, displacement, time, and so on, as primitive terms. The principles are the Newtonian laws of motion, and the inference rules are those offered by differential calculus. The conceptual model may be the set of planets, thought as spheres rotating around the sun. The correspondence rules provide numerical values to the mass of planets, to the gravitational constant, to the quantities corresponding to displacements, velocities and accelerations as furnished by the mechanical theory. Fig. 1 shows the general structure of a physical theory.

The essential part is that in grey; the two boxes with dashed sides represent parts which could actually be missed: i.e. the formalized structure, obtained according to the symbols and principles of predicative logic, and the conceptual model which, according to the dominant point of view, has heuristic and didactic value only.

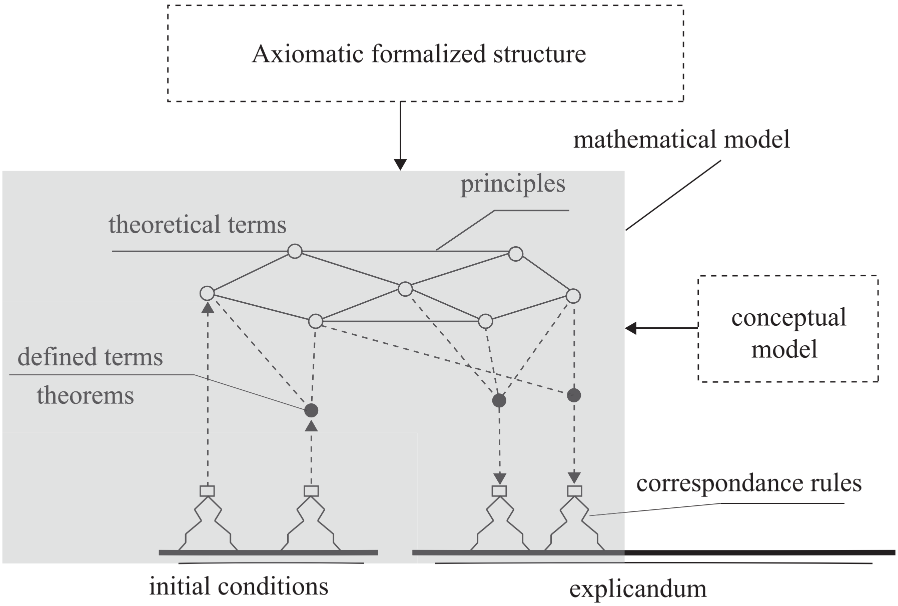

Fig. 1 Scheme of a physical theory

When the correspondence rules are missing the physical theory turns to a pure mathematical theory, or to a physical mathematical theory if its conceptual model refers to the physical word, for example to the way in which heat propagates inside solids.

Of course a gradation between the so defined pure physical theory and pure mathematical physical theory can be stated, mainly according to an ontological point of view. A pure mathematical physical theory is carried out by a mathematician who has found an interest, for unknown reasons, in some principles (axioms or laws), which 'by chance' have a physical meaning. He chooses the principles because of their mathematical interest, and the possibility to show his great skills in mathematics. He has no interest in the fact that the theorems he has found express physical laws; i.e., if the results of the theory are empirically true or false.

Eventually, there is the position of a professional mathematician who has some sensitivity to physical aspects. He chooses with care his 'physical laws', for he wants to be sure they are true. This was for example the case of Fourier and Lamé. Starting from very simple and indubitable empirical physical laws, and complicated analytical passages, they got complex physical laws which necessarily should be ‘true’. In case they were not verified, this would mean there were errors in the experimental apparatus. At the opposite pole there is the position of a pure physicists who has a good understanding of mathematics and considers his theoretical developments as a way to verify the goodness of the principles assumed for the theory, the only thing which is interesting to him.

In previous considerations the difference between pure mathematics and mathematical physics was not specified in detail, and various positions could be assumed. Clifford Truesdell (1919-2000), for instance, does not see the difference and states that mathematical physics is simply a branch of pure mathematics, and in any case it is not applied mathematics. He actually speaks of rational mechanics only, but his considerations apply to any mathematical physical theory:

> Is rational mechanics part of applied mathematics? Most certainly not. While in some cases known mathematical techniques can be used to solve new problems in rational mechanics, in other cases new mathematics must be invented. It would be misleading to claim that each achievement in rational mechanics has brought new light in mathematics as a whole as to claim the opposite, that rational mechanics is a mere reflection from known parts of pure mathematics [42].[1]

One cannot but agree that mathematical physics is not an applied science in the usual meaning of the adjective ‘applied’. Truesdell’s insistence that mathematical physics is a distinct branch of pure mathematics is less convincing. In fact, it is true that developing a physical mathematical theory one can discover new theorems; this is what occurred in the past. But new discoveries can always be framed in the existing mathematics, or open a new branch of pure mathematics no longer connected to physics. So, the fact that mathematical physics be pure or not pure mathematics is in part a matter of words. All depends on the meaning one wants to give to ‘pure mathematics’. If, as most mathematicians think, a pure mathematical theory should concern only objects that are usually classified as objects of mathematics, such as topology, geometry, abstract algebra, theory of numbers do, then rational mechanics and any other mathematical physical theories are not part of pure mathematics, otherwise they are.

[1] p. 337.

3 Different conceptions of mathematical physics

In 1822 Jean Baptiste Joseph Fourier (1768-1830) published the *Théorie analytique de la chaleur* [10], where he formulated the theory of heat conduction in terms of a partial differential equation, and developed methods to solve it. In doing so, Fourier introduced many innovations because the theory of differential equations was at an early stage of development at his time.

The principles of the theory were derived from a small number of 'primordial' empirical facts, the cause of which was not searched for.

> *The principles of this theory are derived, such as those of rational mechanics, by a very small number of essential facts, of which the geometers in no way will consider the cause* [emphasis added], but they accept them as resulting from common observations and confirmed by all the experiments. The differential equations of heat propagation express the most general terms, and bring physical questions to problems of pure analysis, which is the proper object of the theory. They are no less rigorously demonstrated than the general equations of balance and movement [10].[2]

The main principle of the theory of heat transmission was very simple and easily accepted, as it can be derived from elementary and well ascertained experimental facts:

> When two molecules of the same solid are extremely close and have unequal temperatures, the hottest molecule transmits to the coldest an amount of heat exactly given by the product of tree quantities which are: the duration of time, the extremely small difference of temperature, and a certain function of the distance between the molecules[10].[3]

The very nature of heat does not concern the mathematical expressions Fourier derived.

> Even in the absence of certain assumptions on the nature of heat, the knowledge of the mathematical laws heat is subject to

[2] p. XI. My translation.
[3] p. 605. My translation.

is independent of any hypothesis. This knowledge only requires a careful examination of the main facts that can be observed, and which can be confirmed by accurate experiments [10].[4]

What is really important is that from simple and undeniable empirical facts a very sophisticated mathematical theory can be constructed. Fourier results are summarized in the following theorem:

> Theorem IV. It is easy to deduce from the previous theorems the general equations of heat propagation.
> *Assume that the points of a homogeneous solid of any shape have received initial temperatures varying successively by the effect of the mutual action of the molecules, and the equation* $v = f(x, y, z, t)$ *represents the successive states of the solid, we will demonstrate that the function* v *of four variables necessarily satisfies the equation* [10]:[5]

$$\frac{dv}{dt} = \frac{K}{C.D}\left(\frac{d^2v}{dx^2} + \frac{d^2v}{dy^2} + \frac{d^2v}{dz^2}\right) \tag{1}$$

Gabriel Lamé (1795-1870) used largely the term 'mathematical physics' in his works, and specified what was its meaning especially in four books ranging from 1852 to 1861, *Leçons sur la théorie mathématique de l'élasticité des corps solides* (1852), *Leçons sur les fonctions inverses des transcendantes et les surfaces isotherme* (1857), *Leçons sur les coordonnées curvilignes et leurs diverses applications* (1859), *Leçons sur la théorie analytique de la chaleur* (1861) [21, 22, 24, 23].

The titles of these books clearly show the great relevance given by Lamé to analysis and to its high explicative power in physics. In the *Leçons sur la théorie mathematique de l'élasticité des corps solides* of 1852 he defined the 'properly said mathematical physics':

> Mathematical physics, *properly said* [emphasis added], is a modern creation, belonging exclusively to the Geometers of our century. Today, this science actually includes three chapters, variously extended, that are treated rationally, that is to say, they are based on compelling principles or laws only. These chapters are: the theory of static electricity on the surface of conducting

[4] p. 18. My translation.
[5] p. 134-135. My translation.

> bodies, the analytical theory of heat, and the mathematical theory of elasticity of solid bodies. The last is the most difficult, the less complete, and it is also the most useful, as today the importance of a mathematical theory is proportional to the results it can immediately deliver to industrial practice.
> [...]
> No doubt analysis will soon embrace other parts of general physics, such as the theory of light and electrodynamic phenomena. But it cannot be repeated too often, that true mathematical physics is a science as rigorous and accurate as rational mechanics [21].[6]

At the moment, he said, there are only three mathematical physical sciences: the theory of static electricity, the analytical theory of heat, and the mathematical theory of elasticity. They are flanked by rational mechanics, which Lamé seems to consider as the most developed physical mathematical science, to which all the other three, and also other sciences that are coming, should equate.

Lamé's conception of mathematical physics was described very clearly in the foreword of the *Leçons sur la théorie analytique de la chaleur* of 1861. To Lamé, the quantities of interest were represented in all cases by continuous mathematical functions of the three variables (x, y, z) for stationary situations, to which a fourth variable, that is, the time t, had to be added in the dynamic case. Theory can be developed from very simple principles, which only have the status of tentative hypotheses. For example, in the theory of elasticity the first hypothesis was to assume that solid matter is formed by small particles interacting by opposite forces, applied at their centres of mass.

The theory develops via subsequent approximations. The consequences of a hypothesis are compared with well ascertained experimental facts; if there is no agreement between each other, the hypothesis is adapted or replaced, until an agreement is reached. This process can be carried out by a single researcher, but more frequently is a historical process that may last many years. For example, in the case of the theory of elasticity the first theories assumed a homogeneous and isotropic material. However, this assumptions was in disagreement with many experimental results, and therefore it was modified accordingly to this:

[6] p. V-VI.

> After this initial exploration, we return to the starting point, to extend the inaugurated theory to the case of a more general homogeneity of the solid medium, such that the efficient cause of the phenomenon changes with the direction around the same point. But the law of this change is also imperfectly indicated by the facts, that should be completed by the help of a second hypothesis. From this another principle follows, which is still likely, and that leads to a new system of linear partial differential equations, more complicated, but more general than the first ones [23].[7]

Lamé's attitude could be compared with the modern hypothetical-deductive approach, and differs from Fourier's, whose hypothesis was directly inferred from experimental observations and was no longer object of doubts [35].

As far as the theory of heat is concerned, Lamé recalled Fourier's theory, and claimed he was removing Fourier's limitations, for example the hypothesis of isotropy, since that was necessary to study crystalline bodies.

> The course I undertake today has the main purpose to establish the analytical theory of heat, without leaving any hypothetical principle on the internal constitution of the solid, without making assumptions on any law of heat exchange, or the particular radiation, without adopting any restriction for conductivity variations around a point [...]. Indeed, the Theory of Elasticity, completely free of any hypothetical principle, can demonstrate rigorously, basing on the facts, that in diaphanous media, the ponderable individuals vibrate brightly [23].[8]

The hypothesis assumed by Lamé at the basis of his theory was the following:

> Let M and M' be two close points of a solid medium; ζ the distance, of insensible value, separating them; φ the latitude and ψ the longitude of the direction MM'; V the present temperature at M; V', a little lower than V, that in M'; ω and ω', two elements of volume, to which M and M' belong, of very small size compared to ζ. The quantity of heat transferred, during the time dt, by the volume ω' to the volume ω, is: $\omega\omega'(V - V')Fdt$. The coefficient

[7] p. VIII-IX. My translation.
[8] p. V-VI. My translation.

> F, essentially positive, depends on the distance ζ and the angles (φ, ψ) [23].[9]

which corresponds to Fourier's when F is assumed constant.

Lamé considered very important the fact that in all sectors of mathematical physics similar or even identical differential equations were obtained. This fact pointed to the possibility to have a unified theory for the whole of physics. And, indeed, this was Lamé's expectation:

> These historical accounts very naturally lead to three predictions that I will state, as so many propositions to verify. *Firstly*: from the steady state of three of the previous theories, and the incessant progress of the other three, it follows that the partial principles of the capillary motion, electricity, and magnetism cannot be known until when those of the light, elasticity and heat will be known. *Secondly*: since the two theories of elasticity of solid homogeneous bodies and the double refraction of diaphanous crystals have had the same initiator, that is, Fresnel, one may deduce that these two theories should merge into a single one, or into a group under the same partial principle. *Thirdly* eventually, since only two active and distinct theories will remain, one can conclude that from their rapprochement and their future fusion, sooner or later the only truly universal principle of physical nature will derive [25].[10]

Fourier and Lamé, two founders of modern mathematical physics, were still anchored to experimental facts, and for this reason they should be considered both physicists and mathematicians. But when it became clear that the mathematical equations governing physics were well established and all had similar form, their mathematical aspect became appealing. Many mathematicians embarked on the attempt to solve the differential equations of mathematical physics in several situations, substantially ignoring physical implications and leaving to the physicists the burden to verify their results. For instance, Emile Matheiu (1835-1890) and Carl Neumann (1832-1925) moved in this direction.

[9] pp. 2-3.
[10] p. 985. My translation.

Mathieu presented himself as a follower of Lamé. In his studies on the theory of elasticity, where he introduced the fourth order equation:

$$\frac{\partial^4 V}{\partial x^4} + 2\frac{\partial^4 V}{\partial y^2 \partial z^2} + \frac{\partial^4 V}{\partial y^4} + 2\frac{\partial^4 V}{\partial x^2 \partial z^2} + \frac{\partial^4 V}{\partial z^4} + 2\frac{\partial^4 V}{\partial x^2 \partial y^2} = 0 \qquad (2)$$

and called V the 'second potential', to distinguish it from the first potential satisfying the equations of Laplace or Poisson (see next section).[11]

Mathieu's job in mathematical physics was to uniform the different fields of physics, also revising the different results found by his predecessors [1].[12] In fact, he defined mathematical physics as a science whose object is the study of a limited set of partial differential equations:

> The principal differential equations that we meet in mathematical physics are:
>
> $$\triangle u = 0,\ \triangle\triangle u = 0,\ \triangle u = -a^2,\ \frac{du}{dt} = a^2 \triangle u,\ \frac{d^2u}{dt^2} = a^2 \triangle u$$
>
> where t is time. The function u, which represents temperature, potential or 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 derivative must be continuous within this domain [1].[13]

Carl Neumann moved similarly. He recognized that the results of mathematical physics should be confirmed by experiments [39],[14] but also claimed it is not a mathematician's concern to work out a comparison between theory and practice. As a mathematician (or mathematical physicist), he focused his attention on the mathematical description of principles, above all on the improvement of mathematical means [39].[15]

Neumann also discussed the differences of the logical status of mathematical and mathematical physical theories. At that time there was a substantially Aristotelian-Euclidean vision of mathematics, according to which a mathematical theory should be based on indubitable axioms and the resulting theorem should not be disputable. According to Neumann, a physical math-

[11] The previous relation is usually written as $\triangle\triangle V = 0$, with $\triangle$ the Laplace operator.
[12] p. 111.
[13] p. 109-110.
[14] p. 130.
[15] p. 127.

ematical theory was different because some 'axioms' might be hypothetical, and its theorems, indeed physical laws, could not necessarily be true. From this point of view, physical mathematical theories were more interesting for a mathematician because of their greater potential of invention (hypothetical deductive theories).

These considerations by Neumann, partially shared also by Mathieu [1],[16] contributed to the development of the modern concept of a mathematical theory based on premises to which is not required to be true.

4 The theory of potential

Starting from the middle of the XIX century, potential theory and mathematical physics were considered as substantially synonymous. For this reason, this large section is devoted to the origins and development of this peculiar mathematical physical theory.

In his *Theorie de la libration de la Lune* of 1780, Lagrange denoted by V a scalar function with no name attached to. Its use was very convenient because, in the cases of conservative forces, as they are called today, its derivatives allowed to obtain their components [17].[17]

It was, however, Simon Laplace (1749-1827) in his *Traité de mécanique céleste* [27] who introduced a detailed and systematic study of functions having the properties required by Lagrange. In this text several problems, very different from each other, were treated with the methods of rational mechanics, basing on the Newtonian law of attraction: from astronomy, to the theory of capillarity, to the motion of a system of bodies. The role of the potential function (modern term) was central to his research; in the case of a spheroid, the centre of which coincides with the origin of a set of orthogonal Cartesian axes, Laplace considered the attraction that the spheroid exerts on a point of mass m and coordinates x, y, z. Denoting by ρ the mass density of the spheroid and x', y', z' the coordinates of its points, he introduced the function V of x, y, z (his symbols) [27]:[18]

$$\int \frac{\rho dx'dy'dz'}{\sqrt{(x-x')^2+(y-y')^2+(z-z')^2}} \tag{3}$$

[16] p. 110-111.

[17] p. 23-24. Lagrange resumed this concept in the *Méchanique analitique* of 1788 [18], p. 225.

[18] p. 136-137.

the derivatives of which with respect to x, y, z give the components of gravitational forces, and which must satisfy the relation (his symbols):

$$0 = \left(\frac{ddV}{dx^2}\right) + \left(\frac{ddV}{dy^2}\right) + \left(\frac{ddV}{dz^2}\right) \tag{4}$$

now called *Laplace equation.* The equation was introduced for the first time by Laplace for a spherical coordinate system [28].[19] Only later it was brought back again to the case of rectilinear coordinates [27].

Denis Poisson (1781-1840) in a paper published in 1813 [36][20] observed that if the point P suffering the attraction is located within the attractive body itself, Laplace equation is no longer valid, and V satisfies the relation (his symbols):

$$\frac{d^2V}{da^2} + \frac{d^2V}{db^2} + \frac{d^2V}{dc^2} = -4\pi\rho \tag{5}$$

with ρ the density of mass or electric charge at the point P, and a, b, c the coordinates of the point where V is evaluated. Equation (5), called *Poisson-Laplace equation*, is in fact a generalization of equation (4), for if the point P is located outside the body it is $\rho = 0$, and then V satisfies Laplace equation. Poisson tried three different demonstrations of equation (5), but the first rigorous proof was given by Carl Friedrich Gauss (1777-1855) in his famous *Allgemeine Lehrsätze in Beziehung auf die im verkehrten Verhältnisse wirkenden Anziehungs und Abstossungs-Kräfte* of 1840 [11].[21] In the same memory Gauss used the name *potential* for V.

Laplace equation is fundamental to potential theory and it is valid for a large number of phenomena, other than those it was introduced for, such as the dilation of a solid in elastic equilibrium, and the steady state distribution of temperature in a body. Given the importance of Laplace equation in the field of mathematical physics, the need for assigning a symbol to the sum of the second derivatives of a function V was felt; Robert Murphy (1806-1843) denoted it by $\triangle V$, Gabriel Lamé by $\triangle V^2$, George Green (1793-1841) by δV, while the function V such that $\triangle V = 0$ was called harmonic [3].

Different ways were proposed to address equations (4) and (5). In particular, in the memoirs of Gauss [11] and Green [12, 13] and in other scientists'

[19] Relation (4) was already deduced by Leonhard Euler in 1753 during his research on the equations of hydrodynamics [9], p. 300. He was actually referring to a potential of velocities and not of forces.

[20] p. 391.

[21] p. 210.

memoirs it was suggested that the search for a harmonic function could be replaced by the search for the minimum of the following functional:

$$I = \int \left[\left(\frac{\partial V}{\partial x} \right)^2 + \left(\frac{\partial V}{\partial y} \right)^2 + \left(\frac{\partial V}{\partial z} \right)^2 \right] dx\, dy\, dz \tag{6}$$

At that time, this result was not proved rigorously and now goes under the name of *Dirichlet principle*, according to which I must attain a minimum value equal to 0, and the sought harmonic function minimizes I [16].

This principle drew much criticism. First, it was not at all evident that the class of admissible functions, namely, those functions satisfying the given Dirichlet problem, is not empty. Second, it is not said that the integral I always assumes finite values. Finally, I may not have a minimum or, in other words, the lower bound of I could be given by a non admissible function. The latter objection was clearly formulated by Karl Theodor Wilhelm Weierstrass (1815-1897) in 1870[22] [38], building the example of a function that has a lower extreme, but not a minimum. Before 1870, mathematicians had already moved criticism to the legitimacy of Dirichlet's principle, which provided a criterion of existence, the only one known, for the solution of Dirichlet problem. The objections started from Berlin, came to Italy, and Enrico Betti (1823-1892) took part in the controversy, making the difficulties associated with Dirichlet principle known in Italy by means of his charisma. However, it was widely believed that Dirichlet principle held, at least under certain assumptions, and that its rigorous proof was possible. This view was also shared by physicists who, less sensitive to issues of rigour, continued to use Dirichlet principle in their deductions.

The attitude of distrust towards this principle is perfectly understandable if one takes into account the effort of making mathematics rigorous, which was taking place in those years. This process begun in France, with Augustin Cauchy (1789-1857) since the twenties, and later found its continuation in Germany with Weierstrass and his school of Berlin. The goal was to make the entire mathematical foundations based on absolutely rigorous and certain demonstrations. Was thus possible, from this point of view, to build the fundamental results of potential theory and other mathematical theories basing on the challenged Dirichlet principle?

Dirichlet problem and the existence of its solution were deeply connected

[22] But published only in 1895.

to several mathematical theories, such as complex analysis, functional analysis and variational calculus. In the second half of the XIX century, the fact that the validity of Dirichlet principle was doubtful undermined the basis of many developments of these theories which, until then, relied on the existence of the solution to Dirichlet problem. Mathematicians then tried to overcome the obstacle building, over time, the particular solution to the given Dirichlet problem. The methods developed during the XIX century were mainly Green's function (analyzed in the following), the *alternating method* of Karl Hermann Amandus Schwarz (1843-1921), the method of the arithmetic averages of Carl Neumann (1832-1925) and the *balayage* of Henri Poincaré (1854-1912) [3].[23]

Among the many ad hoc methods for solving Dirichlet problem, the method proposed by Green must be discussed. It was reported in his famous paper of 1828 *An essay on the application of mathematical analysis* [12], about the application of the analysis to the study of electricity and magnetism. The section entitled *General preliminary results* is that offering the greatest contributions to potential theory. Relying on physical intuition also, Green felt that a function V harmonic in a domain T, bounded by a surface S, can be expressed in the following way (modern symbols) [12]:[24]

$$V(P) = \frac{1}{4\pi} \int_S V(Q) \frac{\partial}{\partial n} G(Q, P) dS \tag{7}$$

where P is a fixed point, Q a variable point on the surface S, n the inner normal to S, and $G(Q, P) = 1/r + U(Q, P)$ a function vanishing at all points of the surface, with r the distance between P and Q. The function U is nothing but the potential of the charge induced on a conductor layer, having the form of S, from the unit charge placed in P.

The *An essay on the application of mathematical analysis* by Green, which also anticipated some remarkable results obtained by Gauss, and introduced new methods of potential theory, was for many years unknown to most of the scientific world. Only in 1845, William Thomson (1824-1907) had in his hands the *Essay*, and sent a copy to Arthur Cayley (1821-1895), who published it on the prestigious German magazine Journal für die reine und angewandte Mathematik in serial form starting from 1850. The impact of

[23] p. 24.
[24] p. 33-34.

the publication was remarkable and, from that moment on, Green's function became a common method used to address issues of potential theory, and *in primis*, to solve Dirichlet problem.

In the period between 1860 and 1870 some mathematicians, including Enrico Betti, Rudolf Otto Sigismund Lipschitz (1832-1903), Franz Ernst Neumann (1798-1895), and Carl Gottfried Neumann, tried to deduce the functions holding the same role as Green's function in the theories of heat, elasticity, magnetism, and electrodynamics. Their goal was to develop procedures and methods of potential theory to determine the solution of problems similar to Dirichlet's [41]. Among these, the so-called Neumann problem, which aims to determine a function V harmonic in a domain with assigned values for the normal derivative of V on the boundary, must be mentioned. The function similar to Green's function for a Neumann problem was derived in the two-dimensional case by a pupil of Betti, Ulisse Dini (1845-1918), in 1876.

Towards the end of the century, mathematicians came back to Dirichlet principle, trying to provide a rigorous proof in several occasions. Some Italian scientists, stimulated by the problems related to Dirichlet principle, gave an important contribution in this regard, laying the foundations of modern functional analysis [3]. However, it was David Hilbert (1862-1943) who put Dirichlet principle on solid basis. In a two-dimensional case with a sufficiently smooth boundary and the given function supposed piecewise analytical, he proved that the functional (6) admits at least a harmonic function taking assigned values on the boundary [14, 15]. Hilbert observed that Dirichlet principle is a particular problem of variational calculus, and therefore developed a general method for determining the condition necessary to ensure that a function is the maximum or the minimum of a functional. Hilbert 'called back to life' (as he himself wrote) Dirichlet principle by going directly to construct a sequence of minimizing functions, such that their limit exists and is precisely the sought function.

5 The role of Gabrio Piola

Gabrio Piola (1794-1850), substantially a contemporary of Lamé, concentrated his efforts on a particular branch of mathematical physics, that is, rational mechanics, or, rather, rational mechanics of continuous bodies.

Rational mechanics differed from other classical physical mathematical theories because, at least in the formulation of the principles, the empirical element had always been relegated to a corner, and when it was introduced it concerned not systematic experimental observations but observations of the common man.

Archimedes' statics, for instance, evolved on the basis of the simple principles for which a body tends to fall down instead of rise up and *ceteris paribus*, for example for a lever with two arms of same length, the greater weight has the greater effectiveness, and moves the system down. There is no doubt that these principles are extra-logical in nature and in a different world could not be valid. Today there is even the possibility of falsifying Archimedes' principles of statics by empirical experiments: it would be sufficient to set up the bodies in deep space, where they have no weight.

Even in the statics of the XIX century the empirical element is not very evident. There are essentially two approaches, one based on equating to zero the sum of the forces (and moments) that are composed with the parallelogram rule, and one based on the principle of virtual velocities and the calculus of variations, as developed by Lagrange in his *Mécanique analytique.* The French mathematicians who had studied continua, such as Cauchy, Poisson and Lamé, had used the first approach, Piola chose the second one. Although he did not consider it as obvious, he considered it at least indubitable and easily provable from evident principles:

> These thoughts persuade us that he would be a bad philosopher who will persist to wish to know the truth about the fundamental principle of mechanics in the way he clearly understands axioms. [...] *But, if the fundamental principle of mechanics cannot be evident in itself, it should at least be a truth easy to be understood and to be convinced of* [emphasis added] (Piola 1825, p. XVI).

Piola's approach to rational mechanics was similar to that by Lamé to the theory of elasticity, with some important differences. Like Lamé, Piola thought that mathematical physics must proceed from undoubted facts, and make extensive use of modern mathematical analysis to derive theorems as the laws of physics, but he was even more cautious. Lamé's foundation of his rational mechanics was, on one hand, the explicit assumption of particles attracting each other by forces depending on their distance; on the other hand, the implicit assumption of the validity of the usual laws of statics, which then were the parallelogram rule and the vanishing of total forces and moments. Piola believed that Lamé's hypothesis on the constitution of matter and the nature of internal forces were unnecessarily bold. He wanted to assume only the geometrical constraints of bodies, which are in turn considered as mathematical continua, similarly to what was done by Cauchy and Lagrange, as evident.

Regarding the criterion of balance Piola stood out from Lamé, assuming the principle of virtual velocities, as formulated by Lagrange:

> Here is the great benefit 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 [...]. In Analytical Mechanics, however, 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 (Piola 1833, pp. 203–204.).

Piola's work on continuum mechanics concerned fluids and solids. These last were published in various years (Piola 1825, Piola 1832, Piola 1836, Piola 1848, Piola 1856), with *La meccanica de' corpi naturalmente estesi trattata col calcolo delle variazioni* of 1832 probably the most relevant one. The title is ambiguous because *estesi* (extended) at Piola's time meant both rigid and deformable, while Piola in this memoir studied only rigid bodies, which he qualified as solid, a term used by Euler and Lagrange as synonymous of rigid. Piola maintained this ambiguity throughout the paper, since he tended to use notations whichccould be extended to deformable bodies. The reason for this ambiguity stems in his declared intention to study, in a companion memoir, the case of deformable bodies also, even though this intention did actually not concretize.[25]

[25] In the paper Piola speaks about a companion paper that will follow in the journal;

According to Piola, in the study of the equilibrium of a rigid body the only fact of which one has clear evidence is that the distances between the various points of the body cannot vary, regardless of the internal forces that are awakened as a result of the applied active forces. This fact can be expressed with algebraic equations that relate the movements of the various points to a limited number (6) of degrees of freedom, or through differential equations that express the constraint of rigidity locally. Piola chose this second approach, drawing inspiration from what was done by Lagrange in the *Mécanique analytique* for the study of one- and three-dimensional rigid bodies.

Here Lagrange imposed the constraint of rigidity, requiring that the mutual distances of all points of the solid remain unchanged for any virtual displacement. He got a set of differential equations in the Cartesian coordinates of the points (x, y, z), of the form (Lagrange 1811, p. 183):

$$d^n x d^n \delta x + d^n y d^n \delta y + d^n z d^n \delta z = 0, \tag{8}$$

of which only three are independent, for example those corresponding to $n = 1, 2, 3$. Lagrange did not fail to notice that these expressions were already obtained by Euler in his work *Decouvert d'un nouveau principe de mécanique* (Euler 1752, pp. 197-201) in the case of motion of a body fixed to its centre of gravity (Lagrange 1811, p. 184).

At this point Lagrange had to apply the principle of virtual velocities, to which he referred to as the equation of moments:[26]

$$\mathrm{S}(X\delta x + Y\delta y + Z\delta\zeta)\, dm = 0, \tag{9}$$

where X, Y, Z are the components of the active forces for unity of mass m; $\delta x, \delta y, \delta z$ are the virtual displacements, not free but satisfying the constraint relations (8). To account for these relations Lagrange had two possibilities:
Method A. To integrate them to obtain explicit expressions for $\delta x, \delta y, \delta z$, thus depending on integration constants.
Method B. To use his multiplier method and add (8) to (9).

In the case of the mono-dimensional rigid body, Lagrange followed method B, using the first three relations (8) (Lagrange, 1811, p. 175). In the case of

actually this paper never appeared.

[26]The symbol of the equation are Lagrange's, with S indicating the integral.

the three-dimensional rigid body Lagrange chose method A, probably because with only three equations of condition he could not obtain any significant results. By integrating the differential equations of condition he got the following expression of virtual displacements of a rigid body:

$$\begin{aligned}
\delta x &= \delta l - y\delta N + z\delta M, \\
\delta y &= \delta m + x\delta N - z\delta L, \\
\delta x &= \delta n - x\delta M + y\delta L,
\end{aligned} \tag{10}$$

Substituting the expressions (10) into $\delta x, \delta y, \delta z$ in the moment equation (9) Lagrange obtained the classical balance equations of statics in terms of the active forces and their statical moments (Lagrange 1811, p. 185).

Piola followed an inverse path; he took for granted the global equation for rigid bodies (10), the terminal point for Lagrange; by suitably deriving them, he was able to obtain a finite number (6) of differential equation which characterize the rigidity constraint locally.

To write down the equations of motion, the material points of a rigid body were labelled by two sets of Cartesian coordinates. The first referred to axes called a, b, c, as done by Lagrange in the *Mécanique analytique* (Lagrange 1813, Sect. XI, art. 4, p. 277) rigidly attached to the body – reference configuration – and the second to axes called x, y, z, fixed in the ambient space and to which the motion of the body is referred – current configuration –. With Piola's symbols (Piola 1832 , p. 209):

$$\begin{aligned}
x &= f + \alpha_1 a + \beta_1 b + \gamma_1 c \\
y &= g + \alpha_2 a + \beta_2 b + \gamma_2 c \\
z &= h + \alpha_3 a + \beta_3 b + \gamma_3 c
\end{aligned} \tag{11}$$

It was not difficult to Piola to prove the validity and the independence of the two sets of six relations:

$$
\begin{aligned}
&\left(\frac{dx}{da}\right)^2+\left(\frac{dx}{db}\right)^2+\left(\frac{dx}{dc}\right)^2=1\\
&\left(\frac{dy}{da}\right)^2+\left(\frac{dy}{db}\right)^2+\left(\frac{dy}{dc}\right)^2=1\\
&\left(\frac{dz}{da}\right)^2+\left(\frac{dz}{db}\right)^2+\left(\frac{dz}{dc}\right)^2=1\\
&\left(\frac{dx}{da}\right)\left(\frac{dy}{da}\right)+\left(\frac{dx}{db}\right)\left(\frac{dy}{db}\right)+\left(\frac{dx}{dc}\right)\left(\frac{dy}{dc}\right)=0\\
&\left(\frac{dx}{da}\right)\left(\frac{dz}{da}\right)+\left(\frac{dx}{db}\right)\left(\frac{dz}{db}\right)+\left(\frac{dx}{dc}\right)\left(\frac{dz}{dc}\right)=0\\
&\left(\frac{dy}{da}\right)\left(\frac{dz}{da}\right)+\left(\frac{dy}{db}\right)\left(\frac{dz}{db}\right)+\left(\frac{dy}{dc}\right)\left(\frac{dz}{dc}\right)=0.
\end{aligned}
\tag{12}
$$

$$
\begin{aligned}
&\left(\frac{dx}{da}\right)^2+\left(\frac{dy}{da}\right)^2+\left(\frac{dz}{da}\right)^2=1\\
&\left(\frac{dx}{db}\right)^2+\left(\frac{dy}{db}\right)^2+\left(\frac{dz}{db}\right)^2=1\\
&\left(\frac{dx}{dc}\right)^2+\left(\frac{dy}{dc}\right)^2+\left(\frac{dz}{dc}\right)^2=1\\
&\left(\frac{dx}{da}\right)\left(\frac{dx}{db}\right)+\left(\frac{dy}{da}\right)\left(\frac{dy}{db}\right)+\left(\frac{dz}{da}\right)\left(\frac{dz}{db}\right)=0\\
&\left(\frac{dx}{da}\right)\left(\frac{dx}{dc}\right)+\left(\frac{dy}{da}\right)\left(\frac{dy}{dc}\right)+\left(\frac{dz}{da}\right)\left(\frac{dz}{dc}\right)=0\\
&\left(\frac{dx}{db}\right)\left(\frac{dx}{dc}\right)+\left(\frac{dy}{dc}\right)\left(\frac{dy}{db}\right)+\left(\frac{dz}{db}\right)\left(\frac{dz}{dc}\right)=0.
\end{aligned}
\tag{13}
$$

In writing the virtual velocities equation, Piola distinguished between the reference (coordinates a, b, c) and the present configurations (coordinates x, y, z). He wrote the equation with respect to the reference configuration first (not reported here for the sake of simplicity), to concentrate then on the equation in the present configuration, which is given by (Piola 1832, p. 215):

$$
\int da \int db \int dc\, \Gamma H \left[\left(\frac{d^2x}{dt^2}X\right)\delta x+\left(\frac{d^2y}{dt^2}-Y\right)\delta y+\left(\frac{d^2z}{dt^2}-Z\right)\delta z\right]=0,
\tag{14}
$$

where Γ is the mass density in the present configuration, H the Jacobian of the transformation from (a, b, c) to (x, y, z), and $(\delta x, \delta y, \delta z)$ the virtual displacement of a material point of the body.[27]

At this point Piola accounted for the constraint relations (12) and (13). To impose the constraint, Piola followed Lagrange's approach for the mono-dimensional rigid bodies (method B), by adding to the integral on the left side of the variational equation (14) the integral of variational version of the constraint relations. In the following, the developments corresponding to relations (12) only are reported. Introducing the Lagrange multipliers *(A, B, C, D, E, F*), the balance equation (9) according to the original Piola's text are (Piola 1832, p. 215):

$$
\begin{aligned}
&\mathrm{S}da\,\mathrm{S}db\,\mathrm{S}dc\cdot A\left\{\left(\frac{dx}{da}\right)\left(\frac{d\delta x}{da}\right)+\left(\frac{dx}{db}\right)\left(\frac{d\delta x}{db}\right)+\left(\frac{dx}{dc}\right)\left(\frac{d\delta x}{dc}\right)\right\}\\
&\mathrm{S}da\,\mathrm{S}db\,\mathrm{S}dc\cdot B\left\{\left(\frac{dy}{da}\right)\left(\frac{d\delta y}{da}\right)+\left(\frac{dy}{db}\right)\left(\frac{d\delta y}{dba}\right)+\left(\frac{dy}{dc}\right)\left(\frac{d\delta y}{dc}\right)\right\}\\
&\mathrm{S}da\,\mathrm{S}db\,\mathrm{S}dc\cdot C\left\{\left(\frac{dz}{da}\right)\left(\frac{d\delta z}{da}\right)+\left(\frac{dz}{db}\right)\left(\frac{d\delta z}{db}\right)+\left(\frac{dz}{dc}\right)\left(\frac{d\delta z}{dc}\right)\right\}\\
&\mathrm{S}da\,\mathrm{S}db\,\mathrm{S}dc\cdot F\left\{\left(\frac{dx}{da}\right)\left(\frac{d\delta y}{da}\right)+\left(\frac{dx}{db}\right)\left(\frac{d\delta y}{db}\right)+\left(\frac{dx}{dc}\right)\left(\frac{d\delta y}{dc}\right)\right\}\\
&\qquad+\left\{\left(\frac{dy}{da}\right)\left(\frac{d\delta x}{da}\right)+\left(\frac{dy}{db}\right)\left(\frac{d\delta x}{db}\right)+\left(\frac{dy}{db}\right)\left(\frac{d\delta x}{dc}\right)\right\}\\
&\mathrm{S}da\,\mathrm{S}db\,\mathrm{S}dc\cdot E\left\{\left(\frac{dx}{da}\right)\left(\frac{d\delta z}{da}\right)+\left(\frac{dx}{db}\right)\left(\frac{d\delta z}{db}\right)+\left(\frac{dx}{dc}\right)\left(\frac{d\delta z}{dc}\right)\right\}\\
&\qquad+\left\{\left(\frac{dz}{da}\right)\left(\frac{d\delta x}{da}\right)+\left(\frac{dz}{db}\right)\left(\frac{d\delta x}{db}\right)+\left(\frac{dz}{dc}\right)\left(\frac{d\delta x}{dc}\right)\right\}\\
&\mathrm{S}da\,\mathrm{S}db\,\mathrm{S}dc\cdot D\left\{\left(\frac{dy}{da}\right)\left(\frac{d\delta z}{da}\right)+\left(\frac{dy}{db}\right)\left(\frac{d\delta z}{db}\right)+\left(\frac{dy}{dc}\right)\left(\frac{d\delta z}{dc}\right)\right\}\\
&\qquad+\left\{\left(\frac{dz}{da}\right)\left(\frac{d\delta y}{da}\right)+\left(\frac{dz}{db}\right)\left(\frac{d\delta y}{db}\right)+\left(\frac{dz}{dc}\right)\left(\frac{d\delta y}{dc}\right)\right\}.
\end{aligned}
\tag{15}
$$

After lengthy calculations Piola arrived to the following balance equations in

[27]Though Piola is dealing with a rigid body motion he introduced the Jacobian H of the coordinate transformation from (a, b, c) to (x, y, z). Its introduction is useless for a rigid body motion where $H = 1$; but it allows to easily extend the analysis to the case of deformable bodies

the reference configuration (x, y, z) (Piola 1832, p. 220):

$$\begin{aligned}
\Gamma\left[X-\left(\frac{d^2x}{dt^2}\right)\right]+\frac{dA}{dx}+\frac{dF}{dy}+\frac{dE}{dz}=0\\
\Gamma\left[Y-\left(\frac{d^2y}{dt^2}\right)\right]+\frac{dF}{dx}+\frac{dB}{dy}+\frac{dD}{dz}=0\\
\Gamma\left[Z-\left(\frac{d^2z}{dt^2}\right)\right]+\frac{dE}{dx}+\frac{dD}{dy}+\frac{dC}{dz}=0.
\end{aligned}\tag{16}$$

Piola repeated the calculation using the equation of conditions (13) and obtaining an analogous result.

Piola's approach is typical of the XIX century mathematical physics. Starting from the equation of equilibrium (9), with an assumption which could not be subject of any criticism, he got to prove a theorem according to which the relation (9) with the equations of condition (12) leads to the differential equations (16). During this demonstration Piola had to overcome many difficulties of mathematical kind, also coming to introduce an interesting transportation theorem allowing to move from the equilibrium equations written in the reference configuration to those written in the present configuration (Piola, 1832, pp . 234-236).

Given the high level of abstraction that Piola wanted to keep, the equations (16) does not have any mechanical sense. Piola found that his equations could be compared with those found by Cauchy and Poisson (Cauchy 1827, Poisson 1829) for the balance of three-dimensional continua. The Lagrange multipliers *(A, B, C , D, E , F)* are the stress components in an assigned coordinate system in the reference configuration. He kept this position of little interest for the mechanical aspects for all his later works. Only with his posthumous work, *Di un principio controverso della Meccanica Analitica di Lagrange e delle sue molteplici applicazioni* (Piola 1856), Piola gave a convincing sense of the physical relations (9) by recognizing the expressions that are to the left of condition equations (12) and (13) the significance of strain components. More specifically, equations (12) correspond to the left Cauchy - Green deformation tensor and equations (13) to the right Cauchy-Green deformation tensor.

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