THE EARLY HISTORY OF DIMENSIONAL ANALYSIS: I. FONCENEX AND THE COMPOSITION OF FORCES

Roberto de Andrade Martins

Abstract: This paper describes a forgotten episode of the history of dimensional analysis. An article published in 1761 by François Daviet de Foncenex contains the first known attempt to derive a physical law – the parallelogram rule of forces – using the principle of homogeneity. The motivation of that work was the wish to provide an *a priori* proof of the basic laws of mechanics. The context and consequences of the paper are described. It is shown that this attempt was not grounded upon clear and solid assumptions and that its basic ideas were implicitly in conflict with the conceptions of that time.
Keywords: dimensional analysis; parallelogram of forces; laws of mechanics; a priori proof; Foncenex, François Daviet de

1. INTRODUCTION

Dimensional analysis is a method designed to produce scientific quantitative laws from a formal condition: the requirement of dimensional homogeneity (see Langhaar, 1951; Sédov, 1977). This technique received much attention in the last quarter of the 19th and in the first half of the 20th century. Today, it is not a very popular subject, although it has shown a moderate success in the realm of highly complex phenomena,

MARTINS, Roberto de Andrade. *Studies in History and Philosophy of Science I*. Extrema: Quamcumque Editum, 2021.

where a detailed theory of the processes is missing – such as in fluid dynamics and stellar structure.

Some books on dimensional analysis contain information about the history of this subject, and a few authors have sketched several components of its evolution (Ravetz, 1961; Macagno, 1971; Higgins, 1957). Although too little has hitherto been written on the development of dimensional analysis, the following outlook of its history emerges. The study of the dimensions of physical magnitudes has undergone a series of metamorphoses, and was linked to several subjects, such as: (i) a concern about unit conversion, and the use of dimensions to test the homogeneity of formulas (Fourier); (ii) the theory of models and similitude relations, studied by Galileo and Newton, and later linked to the method of dimensions, in the 19th century (Bertrand, Ledieu); (iii) the application of the method of dimensions, in the second half of the 19th century, to the derivation of formulas for complex phenomena (Rayleigh, Reynolds), and later to the foundations of physics (Rayleigh, Jeans, Einstein) and applied physics (Buckingham, Riabouchinsky) at the beginning of the 20th century; (iv) parallel to the development of the technique of dimensional analysis, the study of "absolute" measurement (Ampère, Weber, Gauss) led to the study of the dimensions of electromagnetic magnitudes (Maxwell, Jenkins, Vaschy, Hertz); the discussions about this subject soon became intermingled with controversies about ether models and the "essence" of electricity and magnetism. From those controversies came the popularity and stimulus for the study of dimensional concepts in the late 19th century; most textbooks on electromagnetism of that period included a section on the theory of physical dimensions, and this did not occur in other fields of physics; (v) in the two last decades of the 19th century, some authors tried to establish the foundations and to systematize the study of physical dimensions (Herwig, Vaschy, Piochon); (vi) in the first decade of the 20th century, interest on the ether and discussions about electromagnetism declined, and ultimately dropped to the background; after 1910, most authors ignored the previous history of dimensional analysis. With the publication of Bridgman's book (Bridgman, 1922), the modern period of dimensional analysis begins, and previous works were gradually forgotten.

This seems to be a correct, although incomplete, sketch of the history of the study of physical dimensions. Some interesting episodes have been left outside of the account, and the names of many contributors to this field have been omitted.

But even the above sequence of episodes never received a detailed historical study. Why has the history of this subject been hitherto so neglected? Two main reasons seem to have led to this state of affairs: (i) Most physicists today scarcely know that dimensional analysis exists, and most scientists probably think that this is a dead subject; its importance in current scientific research is negligible, and accordingly not many people would be driven to search its history; (ii) To most modern scientists – and even historians – it also seems that this subject has never been an important one, and that no deep problems have ever arisen concerning it; so, it does not deserve a detailed study. This is a wrong opinion, however.

Physicists will certainly be surprised when told that several famous scientists have devoted some of their time to this subject, studying its foundations, using dimensional analysis in fundamental research, and engaging into bitter discussions about physical dimensions. Besides, there are very deep aspects of dimensional analysis that have been overlooked and that do still allow fruitful foundational research – it is a forgotten but far from dead subject. For all those reasons, it seems that dimensional analysis still deserves a detailed study – and the best beginning seems to dig up its early history.

The specific aim of this paper is to study how dimensional analysis began. There is little doubt that the concept of physical dimensions now in use has been explicitly formulated for the first time by Fourier (1822, pp. 135-140). However, the use of dimensional analysis – the use of dimensional arguments in the derivation of equations – is not found in Fourier's works. This circumstance has led authors to think that dimensional analysis was created *after* Fourier's work. It has even occurred that someone who has made extensive use of dimensional analysis was credited with its origin. Thus, Riabouchinsky (1911) pointed to Rayleigh's 1899 work on capilarity as the first instance of this technique. This is far from true.

Other authors have tried to find the roots of dimensional analysis much time *before* Fourier, in the works of Galileo and Newton (Higgins, 1957, p. 331; Larmor, 1926, pp. 736-738; Ravetz, 1961, p. 9). It is true that Galileo has contributed to the theory of models – a field later linked to dimensional analysis – and that Newton has demonstrated a principle of mechanical similitude that was afterwards appplied to the theory of models and associated to dimensional reasoning. But both the concept of dimensions of physical magnitudes, and their use together with the principle of homogeneity to derive scientific laws, have arisen much later than that.

The use of dimensional analysis presupposes the use of functions and mathematical analysis within science. This did not occur in physics before the 18th century. Hence, it would not be wise to search for any instance of dimensional analysis before that century.

It seems natural to think that dimensional analysis must have been created after Fourier's elaboration of the concept of physical dimensions, in the 19th century; but actually the *use* of this technique has preceded the formulation of a *theory* of dimensions. The two earlier instances of the use of dimensional analysis that I have been able to find have appeared in an article on the foundations of mechanics, signed by François Daviet de Foncenex, published in the scientific proceedings of the Turin Academy (Foncenex, 1761); and in the *Elements of geometry* of Adrien Marie Legendre (1794). It seems to me that those two contributions to dimensional analysis have not hitherto been noticed by historians,[1] although both of them have been known to historians of other fields,[2] and they were sometimes cited by 19th century authors.[3]

Those two episodes, together with their historical context and consequences, will be studied in this paper and in its follow-up.[4] It will be shown that the origin of dimensional analysis was the search for *a priori* proofs of the fundamental laws of mechanics and geometry – specifically, the law of composition of forces, and the postulate of parallels.

In order to understand the significance of the article ascribed to Foncenex (hereafter called 'the Turin paper', for reasons that will become clear later), it is necessary to state the situation of mechanics – and in particular of the law of addition of forces – in the 18th century.

---

[1] This paper was written in 1981. Its main results were published in summary form in the same year (Martins, 1981). Another paper by the author, in Portuguese, added further information on the subject (Martins, 2004). However, the detailed descriptions presented in the current paper and in the next one of this volume have not been published up to now, that is, forty years later. In the interim, following the hint presented in the 1981 published paper, other authors have mentioned Foncenex's work. I will not present here a review of the studies on this subject published after 1981. Except for this note, the present chapter only reproduces the content of the original 1981 manuscript.

[2] The relevance of those works for the history of non-Euclidian geometry and mechanics has been remarked by Roberto Bonola (1955, pp. 53, 55-60, 197-199). However, this author has not noticed that Foncenex and Legendre have provided the first instances of dimensional analysis.

[3] Reference to the relation of Legendre's work and dimensional analysis may be found in Ledieu (1883) and Pionchon (1891, pp. 228-234).

[4] Roberto de Andrade Martins. The early history of dimensional analysis: II. Legendre and the postulate of parallels, in this volume.

2. THE PARALLELOGRAM OF FORCES

Newton's mechanics was ostensibly grounded upon three "axioms of motion" (Newton's three laws). The law of force composition (the parallelogram rule) is presented in Newton's *Principia* as a corollary to his laws of motion (Newton, 1952, p. 15), not as an independent law. However, it *is* an independent law, and cannot be derived from the three laws of motion.

The composition of uniform motions had already been studied by Galileo, and the idea can be traced back to Greek authors – indeed, it appears in the pseudo-Aristotelian *Mechanics*. Parallelograms of force have appeared several times in the 16th and 17th centuries, but it was only in 1687 that this law has been justified and applied to mechanical problems. It was simmultaneously presented by Newton, Varignon, and Lamy (Crowe, 1967, pp. 2, 13-14; Costabel, 1966; Montucla, 1802, pp. 609-610).

Once stated and applied with success, the rule was accepted, but its geometrical dress bothered many authors. Could this law be proved by a geometrical argument? Was it possible to provide an *a priori* proof of this law ? The usual answer was: Yes. Many attempts to devise a simple and correct proof of the law have arisen, from Newton's time to the 20th century (Dugas, 1950). One of the most famous was the one presented by Daniel Bernoulli (1726). As will be seen below, it has indirectly influenced the composition of the Turin paper.

In the 18th century, the word 'physics' was related to the idea of empirical studies; mechanics was not regarded as part of physics, by French researchers – it was considered to be a part of mathematics. At that time, all the branches of mathematics were believed to contain correct *a priori* knowledge. It was not thought to be a formal and conventional science, as we now believe. This change of outlook came only after the rise of non-euclidian geometry. People thought that mathematics should be grounded upon a few intuitive or apoditic axioms, and everything should be derived from those principles and definitions, by rigorous proof. It was expected that some non-evident principles of mathematics – such as the fifth postulate of Euclid's geometry – would be either eliminated as unnecessary, or derived from other clearer *a priori* truths. Mechanics, as a part of mathematics, should follow the same model.

This attitude can be clearly noticed in d'Alembert's work – and, as will be seen below, those ideas have strongly influenced the Turin paper. D'Alembert's attitude to mechanics follows his general ideas about science (d'Alembert, 1805, p. 30). He believed that science should be grounded on true principles – simple and recognized facts that can neither be denied nor explained – such as the impenetrability of bodies, in mechanics, which he thought to be the source of their mutual actions. Everything else should be derived from those simple principles.

In the preface of his *Traité de dynamique*, d'Alembert stated his opinion:

> The safety of the [parts of] Mathematics is an advantage that those sciences borrow from the simplicity of their subjects. It is necessary to admit that, since not every part of Mathematics has an equally simple subject, likewise the appropriate certitude, that which is founded upon Principles necessarily true and evidents by themselves, does not belong equally and in the same way to all those parts. Many of them, grounded on Physical Principles, that is, upon Experimental truths, or upon mere hypotheses, are just what could be called, let us say, of an Experimental certitude, or are even mere suppositions. To say more exactly, we can only regard as marked by the stamp of evidence those that deal with the calculus of magnitudes and the general properties ofextension, that is, Algebra, Geometry, and Mechanics. (d'Alembert, 1743, p. i)

D'Alembert stated that the foundations of Mechanics have been neglected: its principles are either obscure in themselves, or obscure demonstrations are provided for those principles (d'Alembert, 1743, p. iv). He proposed to reduce the number of the principles, to deduce them from clearer notions, and to apply them. He uses in his mechanics three fundamental principles: the law of inertia, the principle of composition of motions, and the equilibrium law (he used this name to refer to the collisions of bodies, not to the lever).

In the second edition of his *Traité*, d'Alembert discussed the problem raised by the Academy of Berlin: 'Are the laws of motion and equilibrium of bodies necessary or contingent laws?' (d'Alembert, 1758, pp. xxiv-xxix). He divided the problem in two parts: (1) Which are the laws of motion and equilibrium that would follow necessarily from the most basic properties of matter and motion? (2) Are those the observed laws? According to d'Alembert, it could happen that God would choose to apply to the world not the simplest laws, but other different laws, and this is the motivation of the second question. After developing his arguments, he reaches the final answer:

> From all those reflections, it follows that the known laws of statics and mechanics are those that result from the existence of matter and motion. But experience proves that those laws are indeed observed in the bodies around us. Therefore the laws of equilibrium and motion, such as those that observation inform us, are necessary truths. (d'Alembert, 1758, p. 397)

It seems to me that this is clearly a Cartesian attitude,[5] although sometimes d'Alembert criticizes Descartes. See for instance this ironical sentence: "... but nobody ignores that the Cartesians (a sect that today has almost disappeared)..." (d'Alembert,1805, p. 369; d'Alembert, 1743, p. v).

Since d'Alembert believed that the basic laws of mechanics could be derived from other simpler ideas, the problem was reduced to finding those simple ideas and the most plain

[5] I agree with Hankins' opinion on this point (Hankins, 1970). See, however, Cane's criticism and Hankins' reply (Cane, 1976; Hankins, 1976).

derivations of the laws. D'Alembert chose the law of composition of motions as one of the basic principles of his mechanics; therefore, part of his job was to prove it from simple ideas. He provided one demonstration in his *Traité*; a simpler derivation was presented later (d'Alembert, 1743, p. 22; d'Alembert, 1761-1780, vol. 6, pp. 360-369).

3. THE TURIN PAPER: MOTIVATION AND CONTENT

D'Alembert's ideas have greatly influenced the Turin paper. The motivation of that article is exactly the same as d'Alembert's. At the beginning of the paper, its aim is stated: to prove the laws of inertia, of composition of forces, and of equilibrium; and to answer the question: are the laws of mechanics necessary or contingent truths? (Foncenex, 1761, p. 299).

The main source of the ideas in the Turin paper seem to be d'Alembert's works. In this paper, d'Alembert is cited several times, and called 'un très-grand Géomètre' and 'l'homme Célèbre' (Foncenex, 1761, p. 299); the *Traité de dynamique* is cited, and at another point the paper refers to d'Alembert's article on *force* in the *Encyclopédie*,[6] and calls him 'illustre Ecrivain' (Foncenex, 1761, p. 304). He again cites d'Alembert and refers to his *Opuscules mathematiques*, at the same paragraph where a reference to Bernoulli may be found (Foncenex, 1761, p. 313). Since Bernoulli is cited by d'Alembert in that work, it seems likely that the author of the Turin paper did not read Bernoulli's original demonstration of the parallelogram law. Besides Bernoulli and d'Alembert, the only author cited in the Turin paper is a certain Mr. Formey (probably Jean-Henri-Samuel Formey), whose ideas he criticized (Foncenex, 1761, p. 318).

While studying the question proposed by the Berlin Academy, the paper refers to d'Alembert's belief that God could

[6] I believe that he refers to the article on "Composition du movement" (d'Alembert, 1778-1779).

violate the rational laws of mechanics; but this opinion is not accepted. The following argument is presented: being part of Mathematics, Mechanics has laws as evident as those of Geometry, and those laws cannot be violated. God can choose to act upon the bodies, and to direct their motion. He could even choose to make all bodies to move in circles, without any apparent reason. However, in this case, God's action would be a new force, and this force would necessarily obey the laws of mechanics (Foncenex, 1761, pp. 299-301, 318-319).

The article is divided in five parts: the introduction, the first numbered section, 'On the Force of Law of inertia', the second 'On the composition of forces', the third 'On the principle of equilibrium', and the fourth 'On the Lever'. Although the lever law may be derived from other principles, in the last section the author chose to provide an independent demonstration of this law, because "it seems very difficult to decide whether we should make the equilibrium of the lever to depend on the composition of forces, or conversely to deduce the latter principle from the equilibrium of the lever" (Foncenex, 1761, p. 301). There are two relevant passages where the principle of dimensional homogeneity is used: the derivation of the law of force composition; and the lever law.

The proposed demonstration of the parallelogram rule begins with a *Lemma*: before proving the general law of composition of forces, the paper proposes a proof that two forces of equal intensity and applied to the same body have a resultant that is proportional to their intensity and to a function of the angle between them. We reproduce below this part of the article:

> Lemma. If two equal forces with their quantities and directions being represented by the lines *CA*, *CB*, act upon any body *C* [Fig. 1], it is evident that this body will not be able to obey at the same time to those two forces: because it cannot move at the same time along *CA*, and along *CB*; it will therefore take a direction *CM* different from *CA* and from *CB*, and the line *CM* must necessarily divide the angle *ACB* in two equal parts, because, the forces *CA*, *CB* being equal by

supposition, everything that disposes *CM* to approach *CA* will equally dispose it to approach *CB*. This having been set, it is also evident that we may imagine a third force *CM* that produces alone the same effect upon the body *C*, as *CA*, *CB* conjointly. Besides, the quantity [intensity] of the force *CM* cannot depend but on the quantity of *CA* or *CB* and of the value of the angle *ACB*, and consequently if we make $CA = CB = a$, $CM = z$, $ACB = \phi$, we shall have $z = \textit{funct.}\ (a, \phi)$.

But the force *CM* being of the same nature as the [*force*] *CA*, it is necessary that they contain one same number of dimensions; that gives $z = CM = \textit{funct.}\ (a, \phi) = a.\textit{funct.}\,\phi$, since the dimension of ϕ is null. (Foncenex, 1761, pp. 305-306)

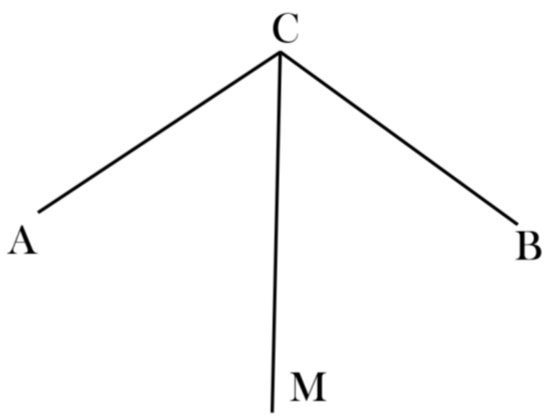

Fig. 1 – Composition of forces (Foncenex, 1761, planche 4, f. 1)

In a footnote the author remarked:

It follows from this that, the angle ϕ remaining constant, z is always proportional to a; we could in the same way demonstrate by this method, in a direct and very natural way, many theorems about the proportionality of the sides of figures, and a great number of other propositions of Geometry, and of Mechanics. (Foncenex, 1761, p. 306)

From the style of this note and other sentences of the paper, one may infer that the author was not aware of any previous use of this method. He says that "we *could* in the same way demonstrate... many theorems... of Geometry and of Mechanics" ("on pourroit de même démontrer par cette méthode..."), and this implies that this has not yet been done. At another place of the paper, we read: "The completely analytical demonstration that I have found has seemed to me otherwise worth finding its place here for its singularity" (Foncenex, 1761, pp. 301, 313). It seems therefore that the author did believe that his method was new.

A similar dimensional argument is used again twice in the paper. At one place, a variation of the demonstration of the parallelogram law is presented, where the author shows that the same results are reached if instead of forces proper, we consider the composition of what we now call moments; he first derives the basic lemma taking *mass* as the variable parameter, and afterwards does the same using *velocity* as the relevant parameter (Foncenex, 1761, p. 304). At another place, the author uses the same method to study the equilibrium of the lever (see Fig. 2), and starts by a new lemma:

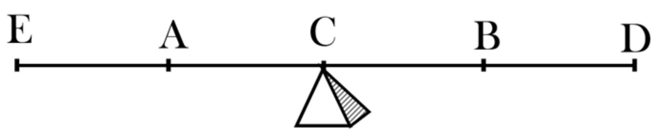

Fig. 2 – Lever equilibrium (Foncenex, 1761, planche 4, f. 7)

> Lemma. If two equal forces = p (as, for instance, two equal weights) act in parallel directions upon the lever AB at the points A and B at equal distances from the fixed point C, it is at once evident that the lever will be in equilibrium relative to the point C, since everything is equal on one side, and the other: I also say that the point C will support the same effort, as if the forces $p + p$ were directly applied to C; because this effort, or the force that would equilibrate them if it would act

> at C in the opposite direction, cannot depend but on the quantity p, and, if we want, of the distance CA, which I call x; this force will therefore be expressed by *fonct.* (p, x), and this we may demonstrate to be equal to p.*fonct.* x, as in the lemma of the Article I. (Foncenex, 1761, pp. 319-320)

It is very important to remark that in the case of the composition of forces (or momenta), one of the variables was the angle, with null dimension; but here, we have *two variable parameters which do not have null dimension*.

The dimensional arguments used in the Turin paper make use of some implicit assumptions. We may analyse the argument as if derived from the following premises:

TP (Turin Paper) 1 – Whenever two magnitudes are of the same nature, they have the same number of dimensions.

TP2 – Angles have null dimensions.

TP3 – Forces, masses and velocities have a number of dimensions different from zero.

TP4 – The dimensions of force are different from the dimensions of length.

TP5 – If z is a function of a and b; if z and a have the same number of dimensions (different from zero); and if b is a magnitude of null dimensions or has dimensions different from a and z; then we must have z = a.f(b); that is, z must be directly proportional to a and to a function of b.

Those assumptions are sufficient – and, it seems to me, they are the most natural ones – to justify the steps where the Turin paper uses dimensional arguments.

But what exactly were the concepts of 'dimension' and 'number of dimensions' used here? Where did these ideas come from? There are two alternatives: either the author has created and used a concept of his own; or he is applying ideas previously developed and known. But when an author creates and uses for the first time a new concept, he usually elucidates its meaning. This elucidation would be particularly necessary in the present case, since there was a previous use of the word ‘dimension’ in geometry, and he should establish a distinction between his concept and the geometrical concept, if there was any difference between them. But in no place of the Turin paper can we find an elucidation of the concept of dimension; and in no other work signed by the same author can we find that elucidation. We may infer that the author is probably using a previously known concept, not a new one. Let us therefore recall what was the meaning of ‘dimension’ at that time.

4. THE CONCEPT OF DIMENSION IN THE 18TH CENTURY

In Diderot’s *Encyclopédie* we find an article on ‘Dimension’ which was probably written by d’Alembert, who was the author of most scientific articles (d’Alembert, 1778-1779, vol. 10, pp. 1058-1059). It provides an obscure definition of dimension as “the extension of a body considered as measurable or susceptible of measure”, and provides as instances: length, breadth, depth. In the same article we find the use of the word ‘dimension’ to denote algebraic powers or exponents (see also Rosenfeld & Cernova, 1967). The two uses of the word are related through a geometric instance: if a and b are two lines, then their product ab may represent the area of a rectangle with sides a and b; but a rectangle is a geometric figure with two dimensions, and lines have one dimension; hence, any product of two lines, or the second power of a line (or one dimension) may be regarded as corresponding to a figure of two dimensions. Also, the product abc may be interpreted as the volume of a solid that has three dimensions; as a general rule, the exponent or number of linear factors in a geometrical formula will correspond to the number of dimensions of the geometrical entity related to that formula, and this establishes a relation between algebrais powers and geometric dimensions. From this use have arisen in Antiquity our expressions ‘square’ for the second power, and ‘cube’ for the third power of any quantity.

Within geometry (but only in this field) the first assumption of the Turin paper (TP1) was well known and used. Each kind of geometric entity was supposed to have a specific number of dimensions: solids have three dimensions, surfaces have two, lines have one, points have none. There are no other geometric dimensions. Since only homogeneous quantities can be compared to one another, added together or divided by one another,[7] then, in geometry, the necessary and sufficient condition for the possibility of comparing, adding and dividing two quantities was their equality of number of dimensions. Hence, the principle of homogeneity became a condition about the number of dimensions of the concerned quantities. This requirement of dimensional homogeneity was widely used in analytic geometry, since the time of Fermat and Descartes (Fermat, 1891-1896, vol. 1, pp. 91-103; Descartes, 1664, pp. 67, 77, 80).

The second assumption was the statement that angles have null dimensions. The geometrical status of angles was not altogether clear in Antiquity, and it remains obscure (Heath, 1956, vol. 1, 176-180). Angles are geometrical entities, no doubt. If they have null dimensions, then they share the same nature of0 points, and this does not seem acceptable. But if an angle is thought as the space included between two lines, it is of the same nature as a surface, and would have two dimensions; but since the area corresponding to an angle would always be infinite, angles do not have a finite ratio to any limited surface, and therefore angles and finite surfaces are not homogeneous

[7] The ancient ideas about magnitudes can be found in Euclid’s *Elements* (Heath, 1956, vol. 2, 112-120).

quantities.[8] This is not an elementary problem, and we cannot try to solve it here. We just need to remark that in the 18th century it was generally accepted that angles had null dimensions, being similar to abstract numbers. Hence, this supposition, although problematic, was not new.

The last assumption of the paper (TP5) may be considered a consequence of the requirement of dimensional homogeneity of formulas; but although this principle was accepted in geometry, I have been unable to find any previous use of this condition to *derive* the form of an equation. Previous authors have only used the principle to *verify* formulas. Hence, this assumption of the Turin paper seems original.

The two remaining assumptions (TP3 and TP4) are remarkable, and show a departure from the purely *geometrical* concept of dimension. The concept of dimension is now applied to *physical* parameters. This is a bold step, and the author of the paper probably had no clear idea about this use. The number of dimensions was known and understood for geometrical entities and abstract numbers; but what could be the number of dimensions of *force*, or *mass* ? If they did have a definite number of dimensions, they would be dimensionally equal – and therefore homogeneous to – some kind of geometrical entity, and could therefore be equated or added to it in mathematical formulas. But this would be an unacceptable idea, at that time.

Notice that, at the time of publication of the Turin paper, the concept of dimension was quantitative (numerical) and not qualitative. Different *kinds* of dimensions were not discussed. ‘Dimension’ was exactly equivalent to ‘geometrical dimension’. It is true that d’Alembert refers to the possibility of considering *time* as a fourth dimension,[9] but he states this as a mere curiosity,

[8] Homogeneous quantities must have a finite ratio. A line is not homogeneous to a surface because there is not a finite ratio between them (Heath, 1956, vol. 2, 112-120).

[9] In the *Encyclopédie* article referred above, d’Alembert stated: “I have said above that it is impossible to conceive more than three dimensions. A friend of mine believes that we can regard duration as

and this was certainly not a common idea. Our modern conception of different kinds of dimensions did not exist at that time.[10]

We may find some previous use of the concept of dimension within physics. One of them, indicated by Macagno (1971), appears in Descartes' speculations. But Descartes' use is obscure and had no influence on the later development of the concept of physical dimensions (Martins, 1981). Even if the author of the Turin paper knew Descartes' ideas, they could not have aided him.

Another instance, also remarked by Macagno, is that made by Euler, who explicitly talks about dimensions, and homogeneity conditions in mechanics (Euler, 1948). However, before studying his contribution, let us go back to earlier ideas.

In Antiquity, it was accepted that to multiply or to divide two heterogeneous magnitudes was absurd, except in the case of geometrical magnitudes (Bochner, 1963). So, while we ordinarily represent the lever law as the equality between two *products* of length versus force

$$F_1.L_1 = F_2.L_2 \,,$$

Archimedes could only understand this law as an equality of *ratios* of homogeneous quantities:

$$F_1/F_2 = L_2/L_1 \,.$$

Although in the 13th and 14th centuries some authors did already define speed as the ratio of two concrete quantities – as space divided by time (Crombie, 1961) – Galileo in the 17th century still represented the relation between space, time and speed in a way that, in modern notation, can be rendered as:

---

a fourth dimension, and that the product of time versus a solid would somehow be a product of four dimensions; this idea may be contested, but it has some merit, it seems to me – at least that of novelty" (d'Alembert, 1778-1779, vol. 10, pp. 1058-1059).

[10] The idea only appeared with Fourier (1822, vol. 1, pp. 135-140).

$$d_1/d_2 = (v_1/v_2).(t_1/t_2),$$

for uniform motion (Galilei, 1842-1856, vol. 13, p. 152), and

$$e_1/e_2 = (t_1/t_2)^2$$

for uniformly accelerated motion (Galilei, 1842-1856, vol. 13, p. 168). He also presented the relation between mass, density and volume (Galilei,1842-1856, vol. 12, p. 21) in a way that we represent (in modern notation) as:

$$M_A/M_B = (d_A/d_B).(V_A/V_B).$$

This was the usual way of understanding physical relations: as equations between pure or abstract numbers, and not as relations between physical magnitudes of different kinds. That is also the notion which we may find in the works of d'Alembert, Carnot and Lagrange, shortly before or after the time of the Turin paper (d'Alembert, 1805, p. 404; Carnot, 1803, p. 11).

In the beginning of his book *Theoria motus corporum solidorum seu rigidorum*, Euler assumed the same idea. After defining velocity as the ratio of space divided by time (Euler, 1948, pp. 28-29), he asks: How could we divide space by time, since they are heterogenous quantities? We cannot say how many times is a time such as *ten minutes* contained in a space such as *ten feet* – and if we could divide a space by a time, then this time would be contained in that space. But Euler shows that all relations between speed, space and time may be reduced to ratios between homogeneous quantities and equations between pure numbers, in the same way that was envisaged by Galileo and other former authors; in this way, all difficulties disappear.

But if all physical laws are to be reduced to relations between numbers, no dimensional requirement can be applied to them, since there is no homogeneity restriction to the form of relations between adimensional quantities. For instance: according to our modern ideas, a physical equation such as

$$F = m.a$$

is dimensionally correct, but

$$F = m/a$$

would be wrong. But this second equation can be written as:

$$F_1/F_2 = (m_1/m_2).(a_2/a_1),$$

and this equations is correct, from the dimensional point of view. Hence, the Turin paper could not make use of an interpretation similar to Euler's.

However, at another place of his book, Euler used a different approach. He again discussed the problem of comparison of heterogeneous quantities, and also the problem of arbitrariness of units, proposing a method of *absolute measurement* of mechanical quantities (Euler, 1948, p. 82). As will be seen below, he attempted to reduce all physical magnitudes to lengths and abstract numbers.

Euler assumed, as we do, that weight and forces are homogeneous quantities; but he also stated that he would *use* weight as a measure of mass, because at each place they are proportional; and *he accordingly accepts them as homogeneous quantities* (Euler, 1948, p. 87). Since in mechanical equations there appear ratios of force to mass, those ratios become abstract numbers. Euler also assumes that times are always to be referred to (or divided by) the second, and hence, whenever a symbol *t* for time appears in an equation, an absolute number is to be understood by this letter.

Euler states that whenever forces appear in an equation, they are to be divided by the weight of the body to which they are applied, and hence only an absolute number will appear in the place of forces. Velocities are to be measured by the space traversed *in one second*. Euler then expresses velocities by spaces, and this amounts to regard velocities and lines as homogeneous quantities (Euler, 1948, p. 89). Hence, in all physical equations, only two kinds of quantities appear: either absolute numbers, or geometrical lines. Accelerations are also regarded as homogeneous to lines, since time is regarded as an abstract number (Euler, 1948, p. 90).

Euler remarked that it is easy to notice the homogeneity of the equations of motion, since the space traversed by a body, its velocity and acceleration are *linear quantities of one dimension* ("sint quantitates lineares et quasi unius dimensionis"), and times and the ratios of force per mass are considered as absolute numbers, which must be reckoned as of null dimension ("qui nullam dimensionem constituere sunt censendi") (Euler, 1948, p. 91).

Here we find, possibly for the first time, the application of the ideas of geometrical dimension and dimensional homogeneity to mechanical quantities and mechanical laws, although in a way completely different from Fourier's, for instance. Although there is no reference to Euler in the Turin paper, it is possible that its author was familiar at least with some of Euler's work, as will be seen below. But even if Euler's use of dimensions in mechanics were known to the author of the Turin paper, he could not be using those ideas in his derivation, since they are not compatible with assumptions TP3 and TP4. For Euler, as has been shown, forces are always to be divided by the weight of the body, and hence to be considered as quantities of null dimension; and if we accept this, no dimensional requirement can be applied to the equation of composition of forces, and nothing can be concluded from the relation $z = \mathit{funct.}\ (a,\phi)$.

Notice also that in his derivation of the lever law, the author must assume that forces and lengths have different dimensions, and that it is not possible to produce a non-dimensional quantity from forces and distances; he is therefore assuming that forces are not geometrical entities, and that they do not have purely geometrical dimensions. Since, in that derivation, he again takes the force out of the function, he cannot assume that forces have null dimension. This is only compatible with the idea of different *kinds* of dimensionality – an idea that we do not find in Euler.

The ideas about the dimensions of physical magnitudes used in the Turin paper were therefore new and at variance with the conceptions of that time. But the author probably did not realize this, since he neither states that his ideas are new, nor elucidates his concept of the dimensions of physical magnitudes.

5. THE AUTHORSHIP OF THE TURIN PAPER

Let us now explain why up to this point we are mentioning “the Turin paper”, instead of referring to Foncenex, its putative author. The reason is this: it seems that at least the *ideas* of that article are due to Lagrange. Let us see the relevant evidence.

Lagrange was born in Turin, in 1736, and at the age of 19 or 16 he began his teaching career in that same city, at an artillery academy (Delambre, 1867, vol. 1, p. ix). Foncenex was one of his students, there. At this time, Lagrange had already been influenced by Euler (Genocchi, 1883).

In 1757, at the age of 21, Lagrange joined Count Saluzzo di Menusiglio and Giuseppe Cigna to create the Academy of Sciences of Turin (Gorresio, 1883). In 1759 this Academy published its first volume of memoirs, called *Miscellanea philosophico-mathematica societatis privatae Taurinensis*. This volume contained an article on imaginary and complex numbers signed by François Daviet de Foncenex, with a note by Lagrange (Foncenex, 1759). This shows the close association betweem them at that time. That was Foncenex’ first paper.

Lagrange sent this first volume to outstanding scientists and mathematicians, including d’Alembert. The later, in reply, sent to Lagrange the four first parts of his *Opuscules mathématiques* (d’Alembert, 1761-1780). In the first one, d’Alembert presented his demonstration of the parallelogram rule which was cited above. This may have been the stimulus for the composition of the Turin paper.

In the very first letter from d’Alembert to Lagrange, with the date of September 27, 1759, we find a reference to Foncenex (Lagrange, 1867-1892, vol. 13, pp. 3-4).

In 1759 and 1760, six new members joined the Turin Academy; among them we find Daviet de Foncenex (Gorresio, 1883). In 1761 the second volume of memoirs was published, now under the noble name of *Mélanges de philosophie et mathématique de la Societé Royale de Turin*. Here appeared the paper on the fundamental principles of mechanics "*par Monsieur le Chevalier Daviet de Foncenex*".

It is possible that Lagrange has shown to Foncenex d'Alembert's work on the law of force composition, and that he discussed those ideas with him. It is also possible that he has suggested to Foncenex the main ideas of the paper, and stimulated his pupil to write the article, while he was himself busy with his researches on the theory of sound. This very bulky study was published at the same time as Foncenex's paper (Lagrange, 1760-1761).

Delambre states that, according to Lagrange himself, "he provided Foncenex with the analytical part of his Memoirs, leaving to him the care of developing the arguments about the formulas" (Delambre, 1867, p. xi). Genocchi states that Foncenex's paper on the principles of mechanics "is said to have been made by Lagrange or with his help" (Genocchi, 1883, p. 86; Genocchi, 1869). There must be some truth behind those rumours. It is remarkable that in his *Mécanique analytique* Lagrange refers to the Turin paper, but he does not cite Foncenex by name: "See the second volume of the *Mélanges de la Société de Turin*" (Lagrange, 1867-1892, vol. 11, p. 19). Let us also notice that other later authors do also refer to the paper without telling the author's name (Legendre, 1794; Fourier, 1888-1890, vol. 2, pp. 475-521; Laplace, 1878-1912, vol. 8, pp. 69-197). Was this because everyone knew that Foncenex was not the author? Maybe Lagrange did not mention Foncenex because the real author of the paper was himself. That is not conclusive evidence, however, since at this same place Lagrange *criticized* the demonstration of the principle of force composition presented in the Turin paper, and does not use it in his own book.

Let us add some information about Foncenex (Anonymous, 1857-1866; Anonymous, 1843-1847). François Daviet de Foncenex was born at Thonon, in 1734, being therefore two years older than Lagrange. His only relevant scientific papers were those cited above, published bu the Turin Academy. Shortly after the publication of the paper on the foundations of mechanics, and through Lagrange's influence, he was placed by the king of Sardaigne at the head of his Navy; in 1766, Lagrange told d'Alembert that Foncenex was at the sea. Afterwards he became governor of Sassai and Villefranche. In 1789, he published his third and last known scientific contribution: a description of an upwards thunderbolt rising from the Villefranche beacon. In 1792, he was accused of weakness or treason because he did not duly defend Nice, and he was arrested for one year. In 1799, the year of his death at Casals, an edition in book form of his work on the principles of mechanics was published in Turin. One of the biographical notes states that he left several manuscripts on algebra and geometry, but nothing is known about their content (Anonymous, 1857-1866).

6. THE INFLUENCE OF THE TURIN PAPER

The Turin paper did not produce any considerable immediate impact. Three papers, by d'Alembert, Laplace, and Fourier, have referred to it and corrected an analytical mistake of the article. Once the error is corrected, it is seen that the derivation of the paper does not conduce to the usual law of the lever (Bonola, 1955, pp. 181-199; Fourier, 1888-1890, vol. 2, pp. 475-521; Laplace, 1878-1912, vol. 8, pp. 69-197). It is quite interesting to notice that the law of force composition is now known to hold in any kind of geometry – since it is a differential law. But the lever law, which refers to an extended body, assumes different forms in different geometries. The corrected derivation of the lever law of the Turin paper is compatible with non-Euclidian geometries – a result later studied by Angelo Genocchi (1869a; 1869b; 1878). But it was only one century after the publication of the Turin paper that non-Euclidian mechanics was developed and studied by de Tilly, Mansion, Andrade, and others (see Bonola, 1955, pp. 181-199; Grigorian, 1960). Those developments, not directly linked to our subject, will not be described here.

Fourier's article is a proof that this author did know the Turin paper before he wrote his *Théorie analytique de la chaleur*, where he presented his considerations on dimentions of physical magnitudes. Therefore, it is possible that the Turin paper has influenced Fourier's later work on the dimensions of physical magnitudes and the homogeneity of physical laws.

The Turin paper was read by Legendre, and has motivated his work on dimensional analysis. This important development will be dealt in our next article.[11]

Within classical mechanics, the dimensional method used in the Turin paper did not become influential. The only direct effect that I have been able to find is the reproduction of the derivation of the composition of forces in the second edition of Poisson's *Traité de mécanique* (Poisson, 1833).[12] We shall present a detailed study of this case, since it illustrates how difficult it was to combine the assumptions of the Turin paper with the concepts of that period.

Contrasting with the Turin paper, Poisson takes the care of explicitly describing the principle of dimensional homogeneity, before using it. It is interesting to remark that Foncenex did not use the word 'homogeneity', and that Poisson uses it, while avoiding to refer to the 'dimension' of mechanical magnitudes. Let us quote Poisson:

> The equations that we shall consider will contain abstract numbers, such as the number π, logarithms, trigonometric lines, etc; they will also contain other quantities of several natures, that will also be represented by numbers expressing their ratios to arbitrarily chosen units, granted that each unit

[11] See the next paper of this volume.

[12] The first editon of Poisson's treatise (Poisson, 1811) does not contain a corresponding passage.

> will be the same for every quantity of the same kind. Changing the magnitude of one or several units, the numbers that express the corresponding quantities will vary inversely as that magnitude, and, notwithstanding this completely arbitrary change, the equations that contain them must still hold. It is necessary, for this to happen, that their forms obey certain conditions, easy to verify in each particular case, and that are called, in the most general acception, the conditions of *homogeneity of the quantities*. Any equation that does not satisfy them, will be wrong for this reason, and must be rejected. (Poisson, 1833, vol. 1, p. 39)

As will become clear in the following quotation, Poisson regarded each unit as independent of the others, except in the case of the units of length, area, and volume. He did not try to reduce all magnitudes to a set of a few fundamental units, as Fourier did; this takes from his method all its practical value, and produces consequences that clash with modern dimensional analysis.

> Thus, representing by *F* a given function, let us suppose that we have
> F(f,f',...L,L',...m,m',...t,t',...) = 0; (a)
> *f*, *f'*,... being forces, *L*, *L'*,... lines, *m*, *m'*,... masses, *t*, *t'*,... times. If we represent by *n*, *n'*, *n''*, *n'''* several abstract numbers, and if we reduce at the same time the unit of force in the ratio of one to *n*, the linear unit in the ratio or one to *n'*, the unit of mass in the ratio of one to *n''*, the unit of time in the ratio of one to *n'''*, the numbers *f*, *f'*,... *L*, *L'*,... *m*, *m'*,... *t*, *t'*,... will become *nf*, *nf'*,... *n'L*, *n'L'*,... *n''m*, *n''m'*,... *n'''t*, *n''' t'*,..., and the equation (a) must still be valid, that is, one must still have
> F(nf,nf',...n'L,n'L',...n''m,n''m',... n'''t, n'''t',...) = 0,
> whatever may be *n*, *n'*, *n''*, *n'''*. If the equation included surfaces *s*, *s'*,... and volumes *v*, *v'*,... , their dimensions should be reported to the same unit as the lines *L*, *L'*,... and those quantities *s*, *s'*,... *v*, *v'*,... would consequently become n'^2s, n'^2s',... n'^3v, n'^3v', ... by the modification of this unit. (Poisson, 1833, vol. 1, pp. 39-40)

Here, Poisson explicitly referred to the relation between the units of length, area, and volume, and fails to mention other relations, such as that between velocity and length. Since he says that the equation must hold "whatever may be *n*, *n'*, *n"*, *n'''*," this implies that the units of force, length, mass, and time, could be arbitrariyly and independently changed without affecting the equation. This does not correspond to our current notion.

Poisson presented an instance of his principle, showing that a particular equation formerly presented in his book did satisfy the homogeneity principle. However, the formula he tested only contained geometrical quantities. He next proposes a new rule:

> It is impossible that the equation (a) may contain a single quantity of some kind alone; when it contains two – for instance, two forces *f* and *f'* – and we solve [the equation] relative to one of them, obtaining
> f' = F(f,L,L',...m,m',...t,t',...),
> it is necessary, by the homogeneity of the quantities, that *f* be a factor of all the terms of the new function *F*, or, said otherwise, it is required that we have:
> f' = Nf;
> *N* being a factor that will contain no quantity of the nature of *f* and *f'*, and will not vary with the unit of force. (Poisson, 1833, vol. 1, p. 41)

Notice that, if Poisson's principle of homogeneity was correct, then any formula such as

$$F = m.a = m.d^2x/dt^2$$

would be deemed wrong, since it contains only one quantity of each kind. Poisson uses equations such as this, in his book, but he did not discuss this problem. Actually, in the main text of his book, we may find one single use of his principle of homogeneity: in the derivation of the parallelogram rule. The demonstration follows the general lines of the Turin paper, and is probably derived from it, although Poisson did not refer to it.

Let us reproduce the relevant part of the argument, in order to compare it to the proof in the Turin paper.

> The resultant of two equal forces always cuts into two equal parts the angle comprised between their directions; because there would be no reason for it approaching more one of these two forces, or for its direction to leave their plane more to one side than the other; its direction is therefore known, and we need only to determine its magnitude.
>
> To find it, let *MA* and *MB* be the directions of the components, their common value being represented by *P*. Let also *2x* be the angle *AMB*, and *MD* the direction of the resultant, in such a way that *AMD* = *BMD* = *x*. Its intensity cannot depend but on the quantities *P* and *x*; representing it by *R*, we shall have
>
> $R = f(P,x)$.
>
> In this equation, *R* and *P* are the only quantities whose numerical expression varies with the unit of force; according to the principle of homogeneity of quantities, it is therefore required that the function $f(P,x)$ takes the form $P\phi x$. Thus we have
>
> $R = P\phi x$;
>
> and the question is reduced to the determination of the form of the function ϕx. (Poisson, 1833, vol. 1, pp. 45-46)

Notice that Poisson did not use the assumption that angles have null dimension. Let us explicitly state his assumptions, in order to show how different they are from those of the Turin paper:

SDP1 – The units of each kind of mechanical quantity are arbitrary and independent of other units.

SDP2 – The equations of mechanics must remain valid if we multiply each kind of quantity appearing in them by arbitrarily chosen numbers (remarking, however, that the geometrical quantities are not independent of one another).

SDP3 – An equation cannot contain one single mechanical quantity of some kind; and when it contains only two, they will necessarily be proportional to one another.

In the specific instance of the derivation of the parallelogram law, Poisson arrives to the same result as the Turin paper, but their premises are completely different. Notice that the assumptions of the Turin paper are compatible with modern dimensional analysis, and those of Poisson are not. But at that time, Poisson's ideas were much more natural and acceptable than those of the Turin paper.

It seems that Poisson did not pay much attention to the consequences of his principles. It is likely that his only motivation was to provide a justification for the proof of the law of composition of forces.

7. CONCLUDING REMARKS

Since our main theme, dimensional analysis, was historically linked to the search for a proof of the law of composition of forces, let us briefly refer to the later phases of this subject.

In 1875 – a hundred and four years after the publication of the Turin paper – Darboux, while proposing a new demonstration of the law of force composition, presented a brief review of former works on the subject, and remarked: "Today, we seldom find a scientific journal where we do not find at least one demonstration of the parallelogram law" (Darboux, 1875; see also Aimé, 1836). Why did so many people attempt to find the proof of this law? Perhaps because it looked like a geometrical theorem (not like a physical law) and so one was tempted to derive it from *a priori* notions. Even in the 20th century 1941 we may find Birkhoff presenting a new derivation of this law, and emphasizing that its main ingredient is the *a priori* principle of sufficient reason (Birkhoff, 1941).

There was some difference between the attitudes of British and French authors regarding this subject.[13] While in France there was a wide acceptance of the possibility of providing *a priori* demonstrations of the laws of mechanics, British scientists usually regarded the laws of mechanics as empirical truths, and did not pay so much attention to those attempts of demonstration. In the early 19th century, Whewell presents a very modern and lucid discussion of the epistemological status of the laws of motion, showing that their general and abstract forms are indeed *a priori* truths; but that the particular formulations that render them applicable to the reality are empirical and *a posteriori* (Whewell, 1834). The specific relation between mechanics and geometry, and the conditions that allow us to produce an apparently geometrical derivation of the law of force composition are clearly and correctly discussed by Goodwin and de Morgan, some time later (Goodwin, 1847; Morgan, 1864). Let us remark that de Morgan uses a dimensional argument in his article.

The empirical approach to mechanics of British scientist, perhaps a legacy of Newton's misunderstood *hypotheses non fingo* (Bell, 1942), was not a fertile ground for the creation and development of methods that proposed to provide an *a priori* proof of the basic principles of mechanics. The French science of the 18th and early 19th centuries, however, deeply influenced by Descartes' rationalism, was probably the best field for the search of such methods – and this allowed the creation of dimensional analysis.

As we have seen, however, although the motivation for the creation of dimensional analysis was strong and clear, it did not have a good conceptual support, at the time of publication of the Turin paper. The attempt was premature and was not grounded

[13] One may consult some interesting British accounts of the differences between French and British science – and other cultural differences: Anonymous, 1820; Anonymous, 1821; Anonymous, 1821-1822.

on firm foundations. It made use of ideas in disaccord with those common at the time, and it was probably for that reason that the method was later wrongly stated by Poisson.

The most important and direct influence of the Turin paper was to stimulate Legendre's work on dimensional analysis; this subject will be dealt with in our following paper.

ACKNOWLEDGMENT

This work has been supported by the Brazilian National Council for Scientific and Technological Development (Conselho Nacional de Desenvolvimento Científico e Tecnológico – CNPq).

BIBLIOGRAPHIC REFERENCES

[ANONYMOUS]. State of science in England and France. *Edinburgh Review*, **34**: 383-422, 1820.

[ANONYMOUS]. English and French literature. *Edinburgh Review*, **35**: 158-190, 1821.

[ANONYMOUS]. Stewart's 'Introduction to the Encyclopaedia'. *Edinburgh Review*, **36**: 220-267, 1821-1822.

[ANONYMOUS]. Foncenet [*sic*], François Daviet de. Vol. 7, p. 321, in: MICHAUD, Joseph François (ed.). *Biographie universelle ancienne et moderne.* 21 vols. Bruxelles: Ode, 1843-1847.

[ANONYMOUS]. Foncenex, François Daviet de. Vol. 13, p. 240, in: HOEFER, Jean Chrétien Ferdinand (ed.), *Nouvelle biographie depuis les temps les plus reculés jusqu'a nos jours.* 46 vols. Paris: Firmin Didot, 1857-1866.

AIMÉ, M. Démonstration du parallélogramme des forces. *Journal de Mathématiques Pures et Appliquées*, **1**: 335-338, 1836.

BELL, Arthur E. Hypotheses non fingo. *Nature*, **149**: 238-240, 1942.

BERNOULLI, Daniel. Examen principiorum mechanicae, et demonstrationes geometricae de compisitione et resolutione virium. *Commentarii Academiae Scientiarum Petropolitanae*, **1**: 126-142, 1726.

BIRKHOFF, George David. The principle of sufficient reason. *Rice Institute Pamphlet*, **28**: 24-50, 1941; reprinted in vol. 3, pp. 778-804, in BIRKHOFF, George David. *Collected mathematical papers*. 3 vols. New York: American Mathematical Society, 1950.

BOCHNER, Salomon. The significance of some basic mathematical conceptions for physics. *Isis*, **54**: 179-205, 1963.

BONOLA, Roberto. *Non-Euclidean geometry: a critical and historical study of its development.* New York: Dover, 1955.

BRIDGMAN, Percy Williams. *Dimensional analysis*. New Haven: Yale University Press, 1922

CANE, Edric. Jean d'Alembert between Descartes and Newton. *Isis*, **67**: 274-276, 1976.

CARNOT, Lazare Nicolas Marguerite. *Principes fondamentaux de l'équilibre et du mouvement.* Paris: Bachelier, An XI [1803].

COSTABEL, Pierre. Varignon, Lamy et le parallélogramme des forces. *Archives Internationales d'Histoire des Sciences*, **19**: 103-124, 1966.

CROMBIE, Alistair Cameron. Quantification in medieval physics. *Isis*, **52**: 143-160, 1961.

CROWE, Michael J. *A history of vector analysis*. Notre Dame: University of Notre Dame Press, 1967.

D'ALEMBERT, Jean Baptiste le Rond. *Traité de dynamique*. Paris: David l'Aîné, 1743.

D'ALEMBERT, Jean Baptiste le Rond. *Traité de dynamique*. 2nd. edition. Paris: David, 1758.

D'ALEMBERT, Jean Baptiste le Rond. *Opuscules Mathématiques, ou mémoires sur différens sujets de*

Géométrie, de Méchanique, d'Optique, d'Astronomie &c. 8 vols. Paris: David, 1761-1780.

D'ALEMBERT, Jean Baptiste le Rond. Composition du movement. Vol. 8, pp. 763-765, in: DIDEROT, Denis; D'ALEMBERT, Jean Baptiste le Rond (eds.) *Encyclopédie ou dictionnaire raisonné des sciences, des arts et des métiers.* 3rd. edition. 39 vols. Genève: chez Jean-Léonard Pellet, 1778-1779.

D'ALEMBERT, Jean Baptiste le Rond. Dimension. Vol. 10, pp. 1058-1059, in: DIDEROT, Denis; D'ALEMBERT, Jean Baptiste le Rond (eds.) *Encyclopédie ou dictionnaire raisonné des sciences, des arts et des métiers.* 3rd. edition. 39 vols. Genève: chez Jean-Léonard Pellet, 1778-1779.

D'ALEMBERT, Jean Baptiste le Rond. *Oeuvres philosophiques, historiques et littéraires. 2. Essai sur les élémens de philosophie ou sur les principes des connoissances humaines.* Paris: J.-F. Bastien, 1805.

DARBOUX, Gaston. Sur la composition des forces en statique. *Bulletin des Sciences Mathématiques et Astronomiques*, **9**: 281-288, 1875.

DELAMBRE, Jean-Baptiste Joseph. Notice sur la vie et les oeuvres de M. le Comte Joseph-Louis Lagrange. Vol. 1, pp. viii-li, in: SERRET, Joseph-Alfred (ed.). *Oeuvres de Lagrange.* 14 vols. Paris: Gauthier-Villars, 1867-1892.

DESCARTES, René. *La géometrie*. Paris: Charles Angot, 1664.

DUGAS, René. *Histoire de la mécanique*. Neuchatel: Griffon, 1950.

EULER, Leonhard. *Theoria motus corporum solidorum seu rigidorum*. Bernae: Societatis Scientiarum Naturalium Helveticae, 1948.

FERMAT, Pierre de. Ad locos planos et solidos isagoge. Vol. 1, pp. 91-103, in: TANNERY, Paul; HENRY, Charles (eds.). *Oeuvres de Fermat*. 3 vols. Paris: Gauthier-Villars, 1891-1896.

FONCENEX, François Daviet de. Sur les quantités imaginaires. *Miscellanea Philosophico-Mathematica Societatis Privatae Taurinensis*, **1**: 113-146, 1759.

FONCENEX, François Daviet de. Sur les principes fondamentaux de la mechanique. *Mélanges de Philosophie et Matématique de la Société Royale de Turin* **2**: 299-322, 1760-1761.

FOURIER, Jean Baptiste Joseph. *Théorie analytique de la chaleur*. Paris: Firmin Didot, père et fils, 1822. Reprinted in: FOURIER, Jean Baptiste Joseph. *Oeuvres de Fourier*. Edited by Gaston Darboux. 2 vols. Paris: Gauthier-Villars et fils, 1888-1890. See specially 135-140.

FOURIER, Jean-Baptiste. Mémoire sur la statique contenant la démonstration du principe des vitesses virtuelles et la théorie des moments. Vol. 2, pp. 475-521, in: DARBOUX, Gaston (ed.). *Oeuvres de Fourier.* 2 vols. Paris: Gauthier-Villars et fils, 1888-1890.

GALILEI, Galileo. *Discorsi e dimostracioni matematiche intorno a due nuove scienze*. Vol. 13, in: ALBÈRI, Eugenio; BIANCHI, Celestino (eds.). *Le opere di Galileo Galilei*. 15 vols. Firenze: Società Editrice Fiorentina, 1842-1856.

GALILEI, Galileo. *Discorso intorno alle cose che stanno in su l'acqua o che in quella si muovono*. Vol. 12, in: ALBÈRI, Eugenio; BIANCHI, Celestino (eds.). *Le opere di Galileo Galilei*. 15 vols. Firenze: Società Editrice Fiorentina, 1842-1856.

GENOCCHI, Angelo. Dei primi principii della meccanica e della geometria in relazione al postulado d'Euclide. *Annuario* della *Società Italiana delle Scienze*, [3] **2**: 153-189, 1869 (a).

GENOCCHI, Angelo. Intorno ad una dimostrazione di Daviet de Foncenex. *Atti della Reale Accademia delle scienze di Torino* **4**: 323-327, 1869 (b).

GENOCCHI, Angelo. Sur un mémoire de Daviet de Foncenex et sur les géométries non-euclidiennes. *Memorie della*

Reale Accademia delle Scienze in Torino, [2] **29**: 365-389, 1878.

GENOCCHI, Angelo. Note biographiche intorno ai tre fondatori della R. Accademia delle Scienze. Pp. 86-95, in: CALAMIDA, Umberto (ed.). *Il primo secolo della R. Accademia delle Scienze di Torino.* Torino: Stamperia Reale di G. B. Paravia, 1883.

GOODWIN, Harvey. On the connexion between the sciences of mechanics and geometry. *Transactions of the Cambridge Philosophical Society*, **8**: 269-277, 1847.

GORRESIO, Gaspare. Notizia storica della R. Accademia delle Scienze di Torino. Pp. 3-6, in: CALAMIDA, Umberto (ed.). *Il primo secolo della R. Accademia delle Scienze di Torino.* Torino: Stamperia Reale di G. B. Paravia, 1883.

GRIGORIAN, Aszot Tigranowicz. Les travaux sur la mécanique non-euclidienne en Russie. *Scientia*, **95**: 347-350, 1960.

HANKINS, Thomas L. *Jean d'Alembert: science and the Enlightenment*. Oxford: Clarendon Press, 1970.

HANKINS, Thomas L. Response. *Isis*, **67**: 276-278, 1976.

HEATH, Thomas Little. *The thirteen books of Euclid's Elements.* 2nd. edition. 2 vols. New York: Dover, 1956.

HIGGINS, Thomas J. Electroanalogic methods. *Applied Mechanics Review*, **10**: 331-335, 443-448, 1957.

LAGRANGE, Joseph-Louis. *Mécanique analytique*. Vols. 11-12, in: SERRET, Joseph-Alfred (ed.). *Oeuvres de Lagrange.* 14 vols. Paris: Gauthier-Villars, 1867-1892.

LAGRANGE, Joseph-Louis. Recherches sur la nature et la propagation du son. *Mélanges de Philosophie et Matématique de la Société Royale de Turin* **2**: 39-148, 151-316, 319-332, 1760-1761.

LANGHAAR, Henry L. *Dimensional analysis and theory of models.* New York: John Wiley & Sons, 1951.

LAPLACE, Pierre Simon de. Recherches sur l'integration des équations différentielles aux différences finies et sur leur usage dans la théorie des hasards. Vol. 8, pp. 69-197, in P.

S. Laplace, *Oeuvres complètes de Laplace.* 14 vols. Paris: Gauthier-Villars, 1878-1912.

LARMOR, Joseph. Units, dimensions of. Vol. 27, pp. 736-738, in *Encyclopaedia Britannica.* 13th edition. 32 vols. Chicago: Encyclopaedia Britannica, 1926.

LEDIEU, Alfred. De l'homogeneité des formules. *Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences de Paris*, **96**: 1692-1696, 1883.

LEGENDRE, Adrien Marie. *Éléments de géométrie*. Paris: F. Didot, an II (1794).

MACAGNO, Enzo O. Historico-critical review of dimensional analysis. *Journal of the Franklin Institute*, **292**: 391-402, 1971.

MARTINS, Roberto de Andrade. The origin of dimensional analysis. *Journal of the Franklin Institute*, **311**: 331-337, 1981.

MARTINS, Roberto de Andrade. A busca da ciência a priori no final do século XVIII e a origem da análise dimensional. Pp. 391-402, in: MARTINS, Roberto de Andrade; MARTINS, Lilian Al-Chueyr Pereira; SILVA, Cibelle Celestino; FERREIRA, Juliana Mesquita Hidalgo (eds.). *Filosofia e História da Ciência no Cone Sul: 3º Encontro*. Campinas: AFHIC, 2004.

MONTUCLA, Jean Étienne. *Histoire des mathématiques.* 2nd. edition. 2 vols. Paris: [s.n.] 1802.

MORGAN, Augustus de. On the general principles of which the composition or aggregation of forces is a consequence. *Transactions of the Cambridge Philosophical Society*, **10**: 290-304, 1864.

NEWTON, Isaac. *Mathematical principles of natural philosophy*. Chicago: Encyclopedia Britannica, 1952.

PALACIOS, Julio. *Análisis dimensional.* Madrid: Espasa-Calpe, 1956.

PIONCHON, Joseph. Théorie des mesures: introduction a l'étude des systèmes de mesures usités en physique. Paris: Gauthier-Villars et fils, 1991.

POISSON, Siméon Denis. *Traité de mécanique.* 2 vols. Paris: Courcier, 1811.

POISSON, Siméon Denis. *Traité de mécanique.* 2nd. edition. 2 vols. Paris: Bachelier, 1833.

RAVETZ, Jerome. The representation of physical quantities in 18th century mathematical physics', *Isis*, **52**: 7-20, 1961.

RIABOUCHINSKY, Dimitri Pavlovitch. Méthode des variables de dimension zéro et son application en aérodynamique. *Aérophyle*, **1**: 407-408, Sept. 1911.

ROSENFELD, Boris Abramovich; ČERNOVA, M. L. Algebraic exponents and their geometric interpretation. *Organon*, **4**: 109-112, 1967.

SÉDOV, Leonid Ivanovich. *Similitude et dimensions en mécanique*. Moscou: MIR, 1977.

WHEWELL, William. On the nature of the truth of the laws of motion. *Transactions of the Cambridge Philosophical Society*, **5**: 149-172, 1834.

Scientiarum Historia et Theoria Studia, volume 1

Roberto de Andrade Martins

Studies in History and Philosophy of Science I

Extrema: Quamcumque Editum, 2021

Summary

Paperback edition: ISBN 978-65-996890-1-7
Kindle edition: ISBN 978-65-996890-0-0

Available at:
https://www.amazon.com/dp/6599689019