THE EARLY HISTORY OF DIMENSIONAL ANALYSIS: II. LEGENDRE AND THE POSTULATE OF PARALLELS

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Abstract: In an attempt to prove Euclid's fifth postulate, Legendre applied the principle of homogeneity. This was apparently the second historical use of dimensional analysis. This paper describes and analyses Legendre's work. It is shown that he assumed as a basis of his argument the inexistence of an absolute length standard – as Lambert did before him. Some reactions against and for Legendre's ideas are presented. It is shown that Legendre's work was premature: his method did not have a solid foundation, and the mathematicians of his time had good reasons to dismiss his arguments as invalid.

Keywords: dimensional analysis; postulate of parallels; foundations of geometry; non-Euclidian geometry; *a priori* proof; Legendre, Adrien-Marie

1. INTRODUCTION

A previous paper[1] has studied what seems to be the oldest instance of use of dimensional analysis in science: the

[1] See the previous chapter in this volume: Roberto de Andrade Martins. The early history of dimensional analysis: I. Foncenex and the composition of forces

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demonstration of the law of force composition, in the Turin paper (Foncenex, 1760-1761) ascribed to François Daviet de Foncenex (1734-1798). The second episode in the history of dimensional analysis that came to our attention was the use of dimensional reasoning by Adrien-Marie Legendre (1752-1833) to derive the fifth postulate of Euclidian geometry, and other geometrical relations. Exactly as in the case of the Turin paper, here again the motivation was the attempt to provide an *a priori* proof of a fundamental law.[2]

From our contemporary point of view, we could perhaps say that there is a very deep difference between the two attempts, since the first one deals with a physical law, and the second one with a mathematical law. However, that difference was not altogether clear in the given historical context (France, in the last decades of the 18th century). Hence, we shall consider the two instances to be on the same footing, although nowadays no one even considers the possibility of applying dimensional analysis to mathematics itself.

We shall begin by studying the historical context of Legendre's work, and then display his ideas and the strong reaction that they have produced. Legendre's attempts to prove the postulate of parallels are part of the history that preceded the development of non-Euclidian geometry. However, since the rise of non-Euclidian geometry is well known and has deserved many historical studies, this part of the subject will be described here as briefly as possible.[3]

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[2] The paper published here was written in 1981 but it has not been published until now. No attempt was made to improve or to update the content of the article. Only slight amendments were made.

[3] There are many books and papers on non-Euclidian geometry and on the history of mathematics that present an account of the subject. A very good report, although somewhat out of date, is the book by Roberto Bonola (1955). A collection of original texts may be found in Engel & Stäckel (1895) or in Sjöstedt (1968) – in this second book, both in the original language and translated into *interlingue*. Two very useful bibliographies describing most works published up to the

2. EUCLID'S FIFTH POSTULATE

The famous postulate of parallels of Euclidian geometry is sometimes described in modern textbooks in this way: "Through a given point can be drawn only one parallel to a given line". Actually, this is Proclus' or Playfair's axiom,[4] which is sometimes substituted for Euclid's original formulation: "That, if a straight line falling on two straight lines makes the interior angles on the same side less than two right angles, the two straight lines, if produced indefinitely, meet on that side on which are the angles less than the two right angles" (Heath, 1952, p. 2). Actually, since Euclid's postulate has always seemed strange and cumbersome to most geometers, several authors have tried to eliminate it or to substitute this postulate by another simpler and more 'intuitive' assumption, Among the many attempts we shall describe those that are directly relevant for the understanding of Legendre's work.

John Wallis (1616-1703) has suggested the following alternative axiom: "To every figure there exists a similar figure of arbitrary magnitude" (Wallis, 1693, vol. 2, pp. 674-677; Sjöstedt, 1968, pp. 83-95; Engel & Stäckel, 1895, pp. 21-30). From one particular instance of this axiom – the existence of *similar triangles*, with equal angles but different sizes – he derived a correct proof of Euclid's postulate of parallels. Since the existence of similar figures of different sizes seemed to him more intuitive than the postulate of parallels, he proposed to substitute the latter by the former assumption.[5]

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beginning of the 20th century have been used: Sommerville (1911) and Loria (1931).

[4] This form of the postulate is equivalente to Euclid's proposition 31 of Book I, which was a theorem, but which was later used as an axiom (Wolfe, 1945, pp. 20-21).

[5] To several mathematicians, since the early 19th century, this seemed the most natural axiom which should be used, instead of Euclid's original formulation. See Carnot (1803, p. 481), Laplace (1878-1912, vol. 6, pp. 471-472; Delboeuf, 1895; Hill, 1927).

A second approach that interests us is the one developed by Giovanni Girolamo Saccheri (1667-1733) and by Johann Heinrich Lambert (1728-1777). Studying a quadrilateral (Fig. 1) where *AD* and *BC* have equal lengths, and where *A* and *B* are right angles, Saccheri discussed the question: can we prove, without assuming the postulate of parallels, that angles *C* and *D* are right angles? (Saccheri, 1733; Halsted, 1920; Sjöstedt, 1968, pp. 96-176; Smith, 1929, pp. 351-382; see also Beltrami, 1889). He showed that this cannot be proved, and that there are three alternatives: (i) *C* and *D* are acute angles; (ii) *C* and *D* are right angles; or iii) *C* and *D* are obtuse angles. There are no other alternatives, because Saccheri proved that both angles must be equal. Depending on the choice between these alternatives, one may derive that the sum of the angles of a triangle is respectively less then, equal to, or greater than two right angles. The hypothesis of the right angle leads to Euclidean geometry, because if the other postulates and axioms of Euclidian geometry are assumed, than the sum of' the angles of a triangle are equal to two right angles if and only if the postulate of parallels holds.

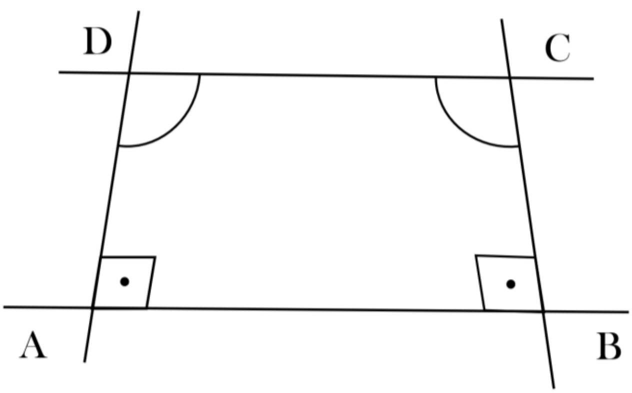

Fig. 1 – Saccheri's quadrilateral

Lambert has developed a series of ideas similar to those of Saccheri, and furthermore he obtained some important new conclusions (Lambert, 1786; Engel & Stäckel, 1895, pp. 135-208; Sjöstedt, 1968, pp. 177-250). If the hypothesis of the acute angles is true, then all triangles will have the sum of their angles less than two right angles, *but this sum will be different for different triangles*. The difference between this sum and two right angles will be *proportional to the area of the triangle*.[6] This implies that it is impossible to build two similar triangles of different sizes, and, generally, it will be impossible to build two similar figures of different sizes. Equilateral triangles with different sizes will have different angles, and there will be a one-to-one correspondence between lengths and angles less than 60°.

It follows from Lambert's work that lengths have an *absolute* significance, if the hypothesis of the acute angle is true, since figures of different sizes will have different properties. We might build absolute length standards, on the hypothesis o€ the acute angle, by choosing as unit, for instance, the side of an equilateral triangle such that the sum of its angles is equal to one right angle (Bonola, 1955, pp. 46-49).

Since, according to the conception of space that was current in the 18th century, it was impossible to have an absolute standard of length, one should deny the hypothesis of the acute angle, as Lambert did, and derive the postulate of parallels.

Now we can discuss Legendre's ideas.

3. LEGENDRE'S PROOFS OF THE POSTULATE OF PARALLELS

In the several editions of his *Éléments de géométrie*, from 1794- onward, Legendre attempted to prove the postulate of parallels in different ways. It seems that he was deeply concerned with this problem all through his life. In an article he published in the year of his death (1833), Legendre discussed

[6] The same consequence holds if the obtuse angle hypothesis is accepted. In both cases, for very small triangles, the difference between the sum of the angles and two right angles becomes negligible.

this subject for the last time, reviewing all his former attempts and presenting a new formulation of some proofs.

It is sometimes said that he added nothing new to the attempts of his predecessors, and that only his style of presenting the proofs was new. In the specific instance to be discussed here, it will be seen that this is a too negative evaluation, and it would not be shared by the mathematicians of Legendre's time.

Most of Legendre's demonstrations have a purely geometric style, but one of them – the one that interests us here – is grounded upon an *analytic* argument. It was published in the first edition of his *Elements*, in 1794, and abandoned for other methods in some editions of his book;[7] but it reappeared in latter editions, and it was presented again in Legendre's last article (Legendre, 1833; Sjöstedt, 1968, pp. 251-325). This proof has been usually neglected by historians of mathematics. Let us reproduce part of this demonstration.

> It is immediately demonstrated by superposition, and without any preliminary proposition, that *two triangles are equal when they have an equal side adjacent to two angles respectively equal*. Let us call this side *p*, the two adjacent angles *A* and *B*, the third angle *C* [Fig. 2]. It is therefore required that the angle *C* be entirely determined, when the angles *A* and *B*, with the side *p*, are known; for, if several angles *C* could correspond to the three given quantities *A*, *B*, *p*, there would be a corresponding number of different triangles that would have an equal side adjacent to two equal angles, which is impossible; hence the angle C must be a determined function of the three quantities *A*, *B*, *p*; and this I express thus, $C = \varphi:(A, B, p)$.

[7] The first edition is Legendre, 1794. The following editions are: 1796 (2nd), 1800 (3rd), 1802 (4th), 1804 (5th), 1806 (6th), 1808 (7th), 1809 (8th), 1812 (10th), 1817 (11th), 1823 (12th). There were some subsequent versions which reproduce the 12th edition, sometimes without the notes at the end of the book, where Legendre's analysis of the postulate or parallels can be found.

Let the right angle be equal to unity; then the angles *A*, *B*, *C*, will be numbers comprised between 0 and 2; and since *C* = φ:(*A*, *B*, *p*), I say that the line *p* cannot enter into the function φ. Indeed, we have seen that *C* must be entirely determined by the given *A*, *B*, *p* alone, without any other angle or line; but the line *p* is heterogeneous with the numbers *A*, *B*, *C*; and if there is any equation among *A*, *B*, *C*, *p*, one might obtain the value of *p* from *A*, *B*, *C*; hence it would follow that *p* is equal to a number, which is absurd; hence *p* cannot enter into the function φ, and we have simply *C* = φ:(*A*, *B*).

This formula already proves that if two angles of one triangle are equal to two angles of another [triangle], the third angle of the former must also be equal to the third angle of the other; and this being conceded, it is easy to work out the theorem we had in view. (Legendre, 1794, pp. 287-288; 11th edition, p. 281; 12th edition, p. 281; Legendre, 1833, pp. 372-373).

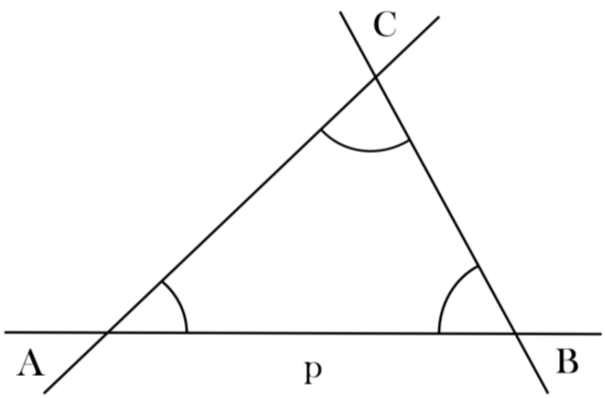

Fig. 2 – The triangle studied by Legendre.

At this point, Legendre was trying to prove the existence of similar triangles, independently of their sizes. If this is proved, then it is straightforward to prove both the right angle hypothesis and the postulate of parallels. In order to prove the existence of similar triangles, he tries to prove that the third angle of any triangle cannot depend on the side of the triangle, it can only be a function of the two other angles. This had been assumed by Wallis, but Legendre attempted to provide a proof of this assumption; and here he used an analytic argument which is just the assumption of the dimensional homogeneity of formulae.

We may derive Legendre's argument from the following assumptions, some of which were not explicitly stated by him:

AML 1 – Angles are geometrical magnitudes of zero dimension – that is, they are pure numbers.

AML 2 – A mathematical function of pure numbers can only yield pure numbers.

AML 3 – A line is not a pure number, and it cannot be represented by pure numbers in the same way as angles.

AML 4 – Heterogeneous quantities cannot be equal.

AML 5 – In the equation $C = \varphi:(A, B, p)$ there is only one quantity with a geometrical dimension, and this is the line p.

If those analytical conditions are accepted, then Legendre's proof is correct. But not all of those assumptions have been accepted by everyone, as will be seen below.

In a footnote to the text transcribed above, added in later editions, Legendre answered a first objection:

> It has been objected against this demonstration that if it were applied, word by word, to spherical triangles, it would result that two known angles are sufficient to determine the third, which does not happen in this kind of triangles. The answer is that in spherical triangles there is one element more than in plane triangles, and this element is the radius of the sphere, which must not be forgotten. Let r be the radius, then, instead of $C = \varphi:(A, B, p)$, we shall have $C = \varphi:(A, B, p, r)$, or just $C = \varphi:(A, B, p/r)$, by the law of homogeneity. But, since the ratio p/r is a number, such as A, B, C, nothing hinders p/r

> from being found in the function, and hence one cannot conclude that $C = \varphi{:}(A, B)$. (Legendre, 1800, p. 312)

Let us ponder upon this objection. In any given spherical surface, a spherical triangle is determined by one side p and two adjacent angles A and B. If we are always referring to this same spherical surface, we may say that the third angle C is a function of the three variable parameters A, B, p. But when we try to put this function $C = \varphi{:}(A, B, p)$ into an algebraic form, we notice that we must introduce a constant parameter r which has the same dimensions as the side p. This does not introduce a new variable, but allows us to transform one of the former variables (p) into a pure number, dividing the side p by the dimensional constant r.

We shall now reproduce the same argument introducing a small change.

Let us think about our universe. In our given universe, a triangle is determined by one side p and two adjacent angles A and B. Since we are always referring to this same universe, we may say that the third angle C is a function of the three variable parameters A, B, p. But when we try to put this function $C = \varphi{:}(A, B, p)$ into an algebraic form, we notice that we must introduce a constant parameter r which has the same dimensions as the side p. This does not introduce a new variable, but allows us to transform one of the former variables (p) into a pure number, dividing the side p by the dimensional constant r.

Legendre assumed that this parameter r cannot exist in our actual space, and this is expressed in the fifth assumption (AML 5). The existence of a constant parameter r would mean that our universe has a determined length scale, or that there is a natural length standard; lengths would have an absolute meaning – which is exactly what Lambert denied in order to prove the postulate of parallels. Hence, the dimensional argument employed by Legendre is, at its bottom, equivalent to Lambert's demonstration – but this equivalence was not noticed at that time.

We therefore see that Legendre's implicit assumption of the inexistence of geometrical dimensional constants is a fundamental step that allowed him to derive the postulate of parallels. In non-Euclidian geometries, this assumption is not true, of course.[8]

This is a very general and important warning regarding any use of dimensional arguments: we must always seek for the existence of dimensional constants related either to the specific system that is being studied, or to our universe. If we do not take into account all the dimensional quantities on which the required result may depend, the method of dimensional analysis will produce wrong results.[9]

Using the same method Legendre also derived other Euclidian theorems from homogeneity conditions. He proved that "in equiangular triangles, the sides opposite to equal angles are proportional"; that "in similar figures the homologous lines are proportional"; that "the surfaces of similar figures are to each other as the squares of the homologous sides"; and several other related theorems about similar solids, circles, spheres – theorems that are valid only in Euclidian geometry. In those demonstrations he assumed that surfaces and volumes are respectively homogeneous with the squares and cubes of lengths, as was usually accepted.

The source of Legendre's method is explicitly stated at the end of the Note that contains these arguments – it is the Turin paper:

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[8] In the context of non-Euclidian geometry, such a parameter does indeed appear; and it seems that the simplest description of our physical universe is one that assumes a non-Euclian space with a curvature radius of about 10^{10} light-years (Einstein, 1951, appendix I; Robertson, 1933; Lemaître, 1949; Rindler, 1967; Sandage, 1970; Barrow, 1978).

[9] At Legendre's time, this was not known. See, however, Lord Rayleigh (1945, vol. 1, pp. 54-55).

> We finally remark that the consideration of functions, which thus affords a very simple demonstration of the fundamental propositions of Geometry, has already been employed with success in the demonstration of the fundamental principles of Mechanics. See the *Mémoires de Turin*, volume II. (Legendre, 1794, p. 294; 11th edition, p. 286; 12th edition, p. 287).

This remark shows that Legendre knew the work ascribed to Foncenex, and he accepted it as valid.[10] Since Legendre did not refer to any other previous use of the same method by mathematicians, it seems that nobody tried to apply the principle of homogeneity to geometrical theorems, before Legendre – although this use had already been suggested in the Turin paper, as has been shown.[11]

The novelty of the use of dimensional arguments in geometry is also implied by a commentary on Legendre's work by Baron Jean Frédéric Théodore Maurice (1775-1851):

> The algorithm of functions had been previously employed with success in establishing the fundamental principles of Mechanics (see the *Miscell. Taurin.* vols. I and II); but this new application does not yield to those which have gone before. (Maurice, 1819)

It might seem that the plural form 'those' ("celles qui l'ont précédée") implies that Maurice knew several applications of the principle of homogeneity earlier than Legendre's work. But this may also be interpreted as referring to the several uses in

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[10] Joseph Pionchon reproduced in his book Legendre's dimensional argument, copied from the 12th edition of the *Éléments* (Pionchon, 1891, pp. 228-234). However, he did not reproduce the two last paragraphs of the Note of that edition, where Legendre mentioned the Turin paper. Pionchon's presentation was written so as to imply that Legendre was the first author to use the principle of homogeneity in *a priori* proofs of formulae.

[11] See my previous paper, in this volume.

the Turin paper itself; or to the uses of dimensional arguments to check equations. Anyway, Maurice seems to be aware of no previous use of dimensional reasoning to prove geometrical principles.

4. REACTIONS TO LEGENDRE'S ARGUMENT

It seems that Legendre's dimensional proof of the postulate of parallels has produced little or no reaction in France. At the beginning of the 19th century, French mathematicians believed that Euclid's Geometry was true and unique, and that the fifth postulate could be proved; hence, Legendre's proof was accepted as correct without much discussion as one among several other equivalent proofs.[12]

The first important criticism of the method came from John Leslie (1766-1832), professor of mathematics at the University of Edinburgh, who had produced his own proof of the fifth postulate. In a Note to the second edition of his *Elements of geometry*, Leslie described Legendre's argument, and he commented:

> To a speculative mathematician this argument is very alluring, though it will not bear a rigid examination. Many quantities in fact appear to result from the combined relations of other quantities that are altogether heterogeneous. Thus, the space which a moving body describes, depends on the joint elements of time and velocity, things entirely distinct in their nature; and thus, the length of an arc of a circle is compounded of the radius, and of the angle it subtends at the center, which are obviously heterogeneous magnitudes. For aught we previously knew to the contrary, the base *c* [or *p*, on

[12] In his last paper on this subject, Legendre stated that no objections had been raised against the correctness of his proofs, but only to their complexity; and he tried in that paper to select his simplest demonstration of the postulate of parallels (Legendre, 1833, pp. 407-408). This was not literally correct, but it seems close to the truth, as regards the French mathematicians.

> the triangle studied by Legendre] might, by its combination with the angles *A* and *B*, modify their relation, and thence affect the value of the vertical angle *C*. In another parallel case, the force of this remark is easily perceived. Thus, when the sides *a*, *b* and their contained angle *C* are given, the triangle is determined, as the simplest observation shows. Wherefore the base *c* is derived solely from these data, or $c = \varphi:(a, b, C)$. But the angle *C*, being heterogeneous to the sides *a* and *b*, cannot coalesce with them into an equation, and consequently the base *c* is simply a function of *a* and *b*, or it is the necessary result merely of the other two sides. Such is the extreme absurdity to which this sort of reasoning would lead! (Leslie, 1811, pp. 403-404)

This quotation shows that Leslie has misunderstood Legendre's method. If Legendre's assumption amounted just to say that we cannot combine heterogeneous quantities into a single function, then Leslie's criticism would be just. If Legendre did not assimilate angles to pure (abstract) numbers, Leslie's argument would also be correct. But Leslie did not perceive this important difference that Legendre established between angles and lines, and this was fundamental to his argument. Hence, Leslie's criticism was unfair.

It seems that the main reason for Leslie's attack against Legendre has been that the latter tried to provide an *a priori* demonstration of the fifth postulate:

> The profound geometer already quoted, pursuing his refined argument, has, from the consideration of homogeneous quantities, likewise attempted to deduce the proportionality of the sides of equiangular triangles. But in this abstruse research, assumptions are still disguised and mixed up with the process of induction. Such indeed must be the case with every kind of reasoning on mathematical or physical objects, which proceeds *a priori*, without appealing, at least in the first instance, to external observations. Of this kind, are some of those ingenious analytical investigations

> respecting the laws of motion and the composition of forces. (Leslie, 1811, pp. 405-406)

I have quoted this passage to show the specific aversion that Leslie (and some other scientists and mathematicians from Great Britain) showed as regards *a priori* proofs – an attitude very different from that of French mathematicians of that time.[13]

However, there was another Edinburgh mathematician who did not have the same opinion: John Playfair (1748-1819). In a review of a French book (Delambre, 1808) where Legendre's analytical proof of the 5th postulate was described, Playfair disclosed a positive evaluation of this method (Playfair, 1810).[14] He first criticized Legendre's *geometrical* proof as too complex, and then he remarked:

> The other demonstration, however, which is in the Notes, possesses the most perfect simplicity, at the same time that it is new; proceeding on a principle that has been long recognized, but from which no consequence, till now, has ever been deduced. (Playfair, 1810, p. 3)

Shortly after the publication, in 1811, of the second edition of Leslie's *Elements of geometry*, Playfair reviewed this book, and criticized Leslie's remarks regarding Legendre's method (Playfair, 1812).[15] After praising again Legendre's argument, Playfair showed that Leslie had misunderstood this proof

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[13] This was a common though not an universal attitude of British mathematicians, and was remarked by one of them, in an anonymous letter to the Editor of the *Philosophical Magazine* (Sigma, 1825). In this article, Sigma referred especially to papers in the *Edinburgh Review* and of the *Encyclopaedia Britannica*, where the French author was attacked. Although Sigma himself seems to support Legendre, his interpretation concerning the grounds of his proofs is completely different from the argument presented by the French mathematician.

[14] The paper was published anonymously, but its author was identified by Leslie.

[15] The review was also published anonymously.

(Playfair, 1812, pp. 89-91), presenting the reasons which have been shown above.

I quote below Playfair's elucidation of Legendre's argument, since his explicitly used the word 'dimension' – avoided by Legendre – and throws a richer light on the argument:

> The quantities *A*, *B*, *C* are angles; they are of the same nature as numbers, or mere expressions of ratio, and, according to the language of algebra, are of no dimension. The quantity *c*, on the other hand, is the base of a triangle, that is to say, a straight line, or a quantity of one dimension. Of the four quantities, therefore, *A*, *B*, *C*, *c*, the first three are no dimensions, and the fourth or last is of one dimension. No equation therefore can exist involving all these four quantities, and them only; for if it did, a value of *c* might be found in terms of *A*, *B*, and *C*, and *c* would therefore be equal to a quantity of no dimension; which is impossible. It would be equal to a quantity of no dimensions, because every function of quantities of no dimension, must itself be of no dimension. (Playfair, 1812, p. 90)

In the above quotation, Playfair explicitly presented some of Legendre's implicit assumptions. The presentation is different, but the two arguments are equivalent.

5. LESLIE'S SECOND ATTACK

Besides Playfair's article, Leslie's criticism was answered by Legendre himself, who wrote a private letter to him. And although Leslie's first charge against Legendre had certainly been refuted, he launched a new attack, in the third edition of his book, in which he reproduced an extract from Legendre's letter (Leslie, 1817, p. 296), and reiterated his earlier criticism, before adding:

> The whole stress of the argument, it may be perceived, lies in the distinction which M. Legendre endeavors to establish between angles and lines – a distinction which I hold at

> bottom to be merely arbitrary. Angles and lines are both equally real quantities, though of different kinds; they are capable of being measured, and consequently represented by numbers, by referring each of them to some definite measure or unit of its own denomination. Angles are measured or expressed numerically by angles, and lines by lines. It is true that the mensuration of angles is facilitated by a reference to a subdivision of the circuit or entire revolution; yet even this mode of denoting angular magnitude is evidently only conventional. As standards for measuring straight lines, nature has furnished the limbs of the human body, and the extent of our globe itself. Such units of mensuration are not indeed very definite or readily attainable; but they are not therefore the less real or prominent. Nor is there any essential difference in principle between the expressing of an angle by degrees, of which 360 or 400 are contained in a complete revolution, and the denoting of a straight line on the French system, for instance by the number of meters it includes, each of which is the forty millionth part of the entire circumference of the earth. Angles and lines hence present to the mind no radical or absolute discrimination, and therefore the argument grounded on such a distinction must lose all its efficacy. (Leslie, 1817, p. 297)

Leslie added that, except for a single exception, he was acquainted with no geometer of any eminence in Britain, who did not admit the fallacy of the argument employed by Legendre. The exception was, obviously, Playfair.

In support of his position, Leslie also quotes part of a letter he received from a mathematician whose name he omits, but who was later identified as James Ivory (1765-1842). Leslie referred to him as the head of the British mathematicians and reproduced his criticism, that amounted to show that Legendre's argument presupposes a geometrical hypothesis equivalent to Euclid's fifth postulate (Leslie, 1817, pp. 294-295).

Leslie's new objection is very strong, indeed. Angles are not abstract numbers. They may be reduced to numbers if we divide them by some unit – but the same holds for any concrete magnitude. What exactly allows us to distinguish the unit of angles as *natural*, and the unit of length as *arbitrary*? That was not a very clear issue, at that time.

Two French answers to Leslie's new argument have been produced: one by baron Jean Frédéric Théodore Maurice (1775-1851) and the other one by Legendre himself.

In his paper, Maurice first tried to elucidate the principle of homogeneity (Maurice, 1819). His argument is not altogether clear, but I think that his ideas are as follows. Arithmetic and algebra only state relations between abstract numbers; Any formula of geometry or physics must be constructed in such a way that it may be reduced to a relation between abstract numbers, by dividing each term of the equation by the same unit. The equations of mechanics seem to relate concrete magnitudes, such as spaces and times, but they are really equations between abstract numbers, since it is always supposed that a line-unit and a time-unit are necessarily included in the equations, so that, every length being divided by its unit, and each time by its unit, only abstract numbers remain.

This view was indeed assumed by most physicists of that time, as we have already shown;[16] this was exactly the principle of homogeneity used by Poisson, and it is not equivalent to or compatible with the ideas used by Legendre,

After some general remarks, Maurice returned to the geometrical problem, stating:

> Consequently, in the case before us, when we have arrived at the general relation expressed by the symbolical equation $C = \varphi:(A, B, c)$, it is *rigorously essential* to its existence that it be capable of being reduced to a relation among abstract numbers. Now, if only the angles A, B, C entered into it, there would be no difficulty: for since each of them expresses a multiple or submultiple of the angular unit, this unit may be made to disappear by means of division. But if the straight-

[16] See my previous paper in this volume, on Foncenex and the composition of forces.

> line *c* enters also, this relation becomes manifestly absurd, since it contains two heterogeneous units, which cannot both be made to disappear from the calculation. […] Thus, it is because the line *c* is *the only* straight line which occurs in the proposed relation, that we are rigorously authorized *a priori* to eliminate this line from it, as a quantity which cannot remain without leading to a manifest absurdity. (Maurice, 1819, p. 90)

Maurice's argument is not equivalent to the one used by Legendre, but it is exactly the one properly criticized by Leslie in his first attack. In Legendre's argument, the essential point is that *angles are abstract numbers*, and Maurice did not use this assumption here. Hence, the elucidation presented by Maurice does not provide an answer to Leslie's criticism.

However, Maurice added some different remarks. He tried to show that a relation between the sides of a triangle and one of its angles is not absurd, although here we have only one angular magnitude. What is the difference between this case and that of the angles of the triangle and one of its sides? Maurice did not state that angles are numbers; but that a transcendental function of the angle (such as *cos C*) might appear in a relation such as $c = \varphi:(a, b, C)$; and this transcendental function is a number.

But why can we find a function that produces an abstract number from a single angular magnitude, and we cannot find a function that produces an abstract number from a single *linear* magnitude? What is, after all, the difference between angles and lines? Maurice tried to ascertain the difference:

> These quantities (angles and lines) regarded as *magnitudes* destined to enter into our calculations, are not homogeneous, when referred to the wholes of which they respectively are parts. The angle is a portion of a finite whole, the straight line is a portion of an infinite whole; so that *every given angle is a finite quantity*, whilst *every given straight line is a quantity infinitely small*, and *only the ratios of given straight lines can enter into our calculations with given angles*. (Maurice, 1819, p. 92)

This was an *ad hoc* argument that did not have any sound justification. It seems that Maurice used here a hypothesis equivalent to: "Two magnitudes are homogeneous if and only if they are both finite or infinitely small relative to the wholes from which they are parts". If we accepted this assumption, we could show that lines and times are homogeneous quantities, since they are infinitely small when referred to the infinite wholes of which they form part. If this criterion of homogeneity was accepted, all magnitudes would be split in just two classes: time, length, surface and volume would be homogeneous; and they would be heterogeneous to the class that includes plane angles, solid angles, etc. But this difference is not altogether clear. We can think about angles corresponding to several revolutions – such as the angle described by the Earth during one year – and there is no upper limit to the value of an angle, if this is conceded. Besides that, we measure time by the angle described by the hands of our analogic clocks; both angles and times have some cyclic properties, but both of them may be regarded as unlimited or infinite.

Let us consider another border-line case. If we take into account curved lines, such as an arc of a circumference, this line may be regarded as part of a finite whole. Are curved lines homogeneous to angles? Are curved surfaces homogeneous to curved lines? It seems that Maurice did not take into account all these consequences of his assumption. He was trying to justify Legendre's method and to criticize Leslie, and any *ad hoc* argument seemed useful, even if it did not agree with Legendre's own ideas and if it had no support.

At another point of his article, Maurice criticized the analogy that Leslie proposed between the unit of angular quantities and the unit of length. There is one *natural* angular standard: the whole revolution, which amounts to four straight angles. But there is no natural unit of length, since the straight line is infinite, and hence any unit of length is arbitrary (Maurice, 1819, p. 94).

In order to support Legendre's argument, it is indeed necessary to show that we can represent angles by abstract numbers in a natural way but that the same cannot be done with lengths. Maurice could have done this, but he did not complete the argument; it seems, indeed, that he did not accept that angles are abstract numbers. Hence, his answer to Leslie missed the fundamental point.

Besides, Maurice's distinction between 'natural' angular units and 'artificial' or 'arbitrary' units of length was not altogether clear. First: we may have several different 'natural' angular units, such as a complete revolution, or the straight angle, or the angle of an equilateral triangle in Euclidian geometry, or the radian. If we do not specify which of these natural units we are using, we cannot know what does it mean that some angular quantity amounts to 0.3 or 2.7, or any other value. So, angles are not indeed pure numbers. Besides, we may have 'natural' length standards if the postulate of parallels is not true, as had been shown by Lambert; and since the whole argument was built by Legendre exactly to prove that postulate, one cannot just assume that there are no natural units of length.

All this shows that Maurice did not understand that the basic point in Legendre's argument was the equivalence between angles and abstract numbers; that he probably did not agree with this assumption; and that nevertheless he tried to defend Legendre's argument by some obscure considerations that do not touch the relevant difficulties. What is still more strange: Legendre gave his support to Maurice's defense, as will be shown below.

Up to the eleventh edition of his book, Legendre did not refer to Leslie's attack. However, in the 12th edition, he added to his Note II the following remark:

> Finally, although the aforementioned theory is established upon the most solid foundations, we should not conceal that it has been attacked by M. Leslie, a famous professor at Edinburgh, in his *Elements of Geometry*, second and third editions; but without entering into any detail of this subject, it

> is enough to say that M. Leslie's objection have been completely refuted, first by M. Playfair, his countryman, in the *Edinburgh Review*, volume XX, and then by M. Maurice, of the Paris Academy of Sciences, in the *Bibliothèque Universelle de Genève*, October 1819. One can also see the discussion of these same objections in the English edition of our *Elements* given by M. David Brewster, Edinburgh, 1822. (Legendre, 1823, p. 287)

In the British edition, Legendre added to his Note II a translation of most of Maurice's article, thus seeming to endorse all his ideas (Legendre, 1822, pp. 230-238). At the point where Maurice stated that angles are finite quantities, and lengths are infinitely small quantities, Legendre added a footnote, remarking that "this is a very subtle and very just metaphysical idea: it is, at the same time, strictly analytical […]" (*ibid.*, p. 235). I fail to see how can a 'strictly analytical' idea be, at the same time, 'metaphysical'.

Legendre's acceptance of Maurice's ideas is quite peculiar. Is it possible to believe that Legendre did not perceive the relevant elements of his argument? We may interpret Legendre's attitude as a strategic move. Legendre was trying to defend himself from Leslie's assault; Maurice appeared and offered his help against Leslie; Legendre accepted Maurice's aid, although the latter's ideas were neither correct nor equivalent to Legendre's original argument. The acceptance of Maurice's support seems unreserved, and Legendre praised even his nonsenses.

In the British edition of his book, Legendre also added his own defense against Leslie's attack (Legendre, 1822, pp. 227-230). The important point made by Legendre in this reply is again the distinction between angles – which he once more regarded as abstract numbers – and lengths; and the difference between the 'natural' angular unit (the right angle) and the arbitrary units of length. But enough has been said about this subject, and we shall not repeat the details of Legendre's vindication. Suffice it to say that this debate had the effect of drawing Legendre to the explicit use of the hypothesis of the inexistence of a natural unit of length.

In his last paper on the theory of parallels, Legendre again advanced the analytical argument (Legendre, 1833). After stating that angles are numbers, or may be represented by numbers, he added: if the third angle of a triangle depended on the base of the triangle, there would exist some way of computing this side from the knowledge of the three angles; and he remarked:

> But the absurdity of a result such as this is manifest; because the relation, whatever it might be, which will determine the side *AD* with the aid of the three numbers […] cannot give for *AD* but a number. […] If this number is 12, for instance, nothing can be derived from this about the absolute value of *AD*, because it would be required to know which unit of length is linked to the number 12 – whether they are millimeters, meters, feet, furlongs, leagues, etc. The nature of the question gives no light about this, it does not show which is the unit of length; and it is precisely the absence of any length unit that makes the above result absurd. We see that by our hypothesis one could retain forever a length measure taken as unity. It would suffice for that to keep the memory of three numbers (the angles of the triangles, or only two if the triangle *ADE* was supposed isosceles, or even one single, if it was supposed equilateral. (Legendre, 1833, p. 391)

Here, Legendre explicitly acknowledged that his proof was grounded on the supposition of the inexistence of a natural unit of length – the basic hypothesis used by Lambert in his proof of Euclid's fifth postulate, as was shown in Section 2 of this paper. Actually, the question of units is not so fundamental for the argument. If there is at least one special constant length that can enter into the formula, then there would be no absurdity in computing the side of the triangle from its angles.

6. REACTIONS PUBLISHED IN THE PHILOSOPHICAL MAGAZINE

A few papers that have appeared in the *Philosophical Magazine*, between 1822 and 1825, commented about Legendre's method. They were possibly a reaction against the publication in 1822 of the English translation of Legendre's book. They will be briefly described in this section.

The first two papers accepted Legendre's analytical argument, although they criticized and tried to improve some of its geometrical assumptions (Ivory, 1822; Meikle, 1822). They will not be discussed here. Shortly afterwards, John B. Walsh (1786-1847) criticized the analytic method (Walsh, 1824). Walsh described the weak point, pointing out that angles are not numbers and that the right angle is an arbitrary unit of angles, not a natural or unique unit. Exactly as angles may be transformed into numbers (by dividing them by the right angle or any other angular unit), exactly in the same way the side of the triangle may be transformed into a number, dividing it by a length unit – the meter or any other standard. Besides these appropriate remarks, Walsh added a lot or rhetoric, and some wrong arguments against Legendre.

The following article was published by an anonymous author, who signed the paper as 'Dis-iota'.[17] This author criticized the geometrical part of Legendre's argument, but accepted and defended the analytical method (Dis-iota, 1824a). In defense of the different roles of angles and lines in geometric equations, he wrote:

> In the problems of plane geometry, where lines and angles are combined in the same equations, the quantities depending

[17] 'Dis-iota" was James Ivory, the British mathematician cited by Leslie in support of his views (Leslie, 1817, 294-295). At the end of his paper, Dis-iota complained about Leslie's use of his letter, and remarked that Leslie had omitted several parts of the correspondence that were favourable to Legendre.

> on the angles invariably contain in their expressions nothing else but ratios, or the quotients of homogeneous magnitudes; which renders the equations independent of the manner in which the angles are themselves compared or measured. It is not the same with regard to lines; for the algebraic symbols of these always involve an arbitrary unit. (Dis-iota, 1824a, p. 162)

If Dis-iota meant that in geometrical equations angles only appear together with and divided by other angles, as the above quotation seems to imply, then Dis-iota was wrong. But perhaps he meant that the angles may be represented by the ratio of the arcs corresponding to the angles in a circle, divided by the quadrant of the same circle, since he used this representation at another point of his paper. But why should we choose the quadrant (or the fourth part of the circumference) and not any other different fraction of the circumference? The arc used for comparison is arbitrary, and therefore Dis-iota's remark does not solve the difficulty. In further remarks on the same subject, he was unable to present a better argument (Dis-iota, 1824b).

Dis-iota incidentally criticized Walsh, who published a paper to back his views (Walsh, 1824). Again, he presented his strong criticisms to those who confound concrete quantities and abstract number.

The next two articles were published by a new anonymous correspondent, who signed his contributions as 'Sigma' (1825a, 1825b). After disclosing the identity of Dis-iota, whom he identified as James Ivory (Sigma, 1825a, p. 101), he presented his own criticism of Legendre's method. Although there are some elementary mistakes in his article, it contains the most lucid exposition of the correct form of the dimensional argument, very similar to the one used nowadays. After denying the validity of Legendre's comparison of angles to abstract numbers, Sigma stated:

> We are […] inclined to search for the cause of the difficulties that have startled us, in some imperfection in the

> mode of expressing or of treating the equations. That imperfection seems to me to be a deficiency in the original equation as given by Legendre. Superposition does not inform him thot the vertical angle (C) is determined by the base (c) and its adjacent angles (A and B) *alone* – but merely that it will be constant it they are constant; and hence that it must be determined by these *variables* and CONSTANTS alone. Noting these constants by γ, the original equation becomes
> $C = \varphi:(c, A, B, \gamma)$. (Sigma, 1825, p. 104)

However, as Sigma added, those constants must be either constant lines or constant angles; but since there are no constant lines, γ can only be a constant angle – the right angle, or any multiple or fraction of the right angle. Now, C depends on four quantities, but only one of them is a length. Applying the principle of homogeneity, we can now exclude this length c from the equation, which becomes:

$$C = \varphi:(c, A, B, \gamma).$$

Hence, Sigma improved Legendre's argument, denying the assumption that angles are abstract numbers (AML 1) and adding the correct assumption (in Euclidian geometry): there are some special or 'natural' angular magnitudes which may enter into the function, but there are no special or 'natural' lengths that may enter into the formula.

Unfortunately, I have been unable to find out the identity of Sigma – someone relevant in the history of dimensional analysis, since he was seemingly the first author who pointed out the importance of considering *constant magnitudes* in dimensional arguments.

7. NON-EUCLIDIAN GEOMETRY

It was to be expected that some criticism of Legendre's method would arise with the development of non-Euclidian geometry. Indeed, Nikolai Ivanovich Lobatschewsky (1792-1856) referred several times to Legendre, always disapprovingly (Bonola, 1955, p. 88; Gonseth, 1955, pp. vi-88, vi-102-114). In the introduction of his *Geometrical researches on the theory of parallels* (Lobatschewsky, 1866), for instance, Legendre's name appears three times concerning the attempts to prove the postulate of parallels, and no other geometer is cited:

> Legendre's efforts have added nothing to this theory [of parallels], since this author has been forced to leave the way of rigorous thought, throwing himself in circular considerations, and using principles that he attempts to exhibit as necessary axioms, without sufficient reasons. […]
>
> The extension of this work [Lobatschewski's own researches] has perhaps hindered my countrymen from following this study, that, after Legendre, seemed to have lost its interest. But nevertheless, I still believe that the theory of parallels still deserves the attention of geometers, and it is for that reason that I propose myself to expose here what is essential in my researches, remarking that, contrary to Legendre's opinion, the other imperfections of principle, such as the definition of the straight line, should not be dealt with here, and have no influence whatsoever upon the theory of parallels. (Lobatschewsky, 1866, pp. 87-88)

Lobatschewsky was deeply concerned with Legendre's influential work. In particular, he felt the need to attack Legendre's analytical argument. Indeed, in Lobatschewsky's theory, there do exist relations between the size of a triangle and the sum of its angles (Lobatschewsky, 1837); therefore, it was essential for him to show that this is not absurd.

Given that Lobatschewsky regarded the actual laws of geometry as empirical truths, which cannot be discovered *a priori*, he compared them to physical laws (Daniels, 1975). Observation must show whether there is any natural unit of length in nature:

> Our theory of the parallels establishes among lines and angles some dependence that nobody has been able to show […] whether it is or is not found in nature. We must at least infer from the astronomical data that all the lines that we are

> able to measure, and even the distances between the celestial bodies, are very small compared to the line that plays the role of unit in our geometry. (Lobatschewsky, 1829, p. 22)

Without paying much attention to the details of Legendre's analytic method, Lobatschewsky directly attacked its central assumption about the homogeneity of equations. He stated that these is no reason to suppose that only abstract numbers (ratios of similar magnitudes) may appear in the relations; and he draws a simile between the law of gravitation and the possible relation between angles and lines:

> It is not doubtful that forces create all the rest: motion, speed, time, mass, and even the distances and angles. Everything is intimately bound to forces – a link that we do not understand in its essence. Hence, we have not the right of assuming that, in a relation between quantities of so different natures, only the ratios of these magnitudes appear. If a dependence between the ratios seems admissible, why should not the same hold for the magnitudes themselves? […]
>
> Ask yourself this, how does distance produce this force? How does it happen that there is a link between two so different things in nature? To be sure, we shall never understand this. However, it is true that forces depend on distances; why should distances not depend on angles? The diversity is similar in the two cases. […] (Lobatschewsky, 1829, pp. 76-77)

We see that Lobatschewsky repeated Leslie's first argument, and he did not discuss the fundamental problems of the analytical method.

Let us proceed to another interesting case: the work of the Canadian mathematician George Paxton Young (1818-1889), who in 1860 rediscovered non-Euclidian geometry (Halsted, 1894).

In a paper published in 1856, Young first discussed Legendre's work (Young, 1856). He had studied Leslie's attack and Playfair's 1812 defense of Legendre, and remarked that "since that time, the validity of Legendre's reasoning seems to have been admitted by the general consent of mathematician" (Young, 1856, p. 520). In his 1860 article, Young again stated that "mathematicians have – by their silence at least – acquiesced in his [Playfair's] verdict" (Young, 1860, p. 341).

Young produced essentially the same argument presented by Sigma. He noticed that Legendre had assumed that when a quantity is *determined* by several others, it may be *expressed* or computed from these and only these. However, Young did not agree with this assumption.

He also remarked that angles are not numbers, and therefore an angle cannot be found without the intervention of some other angle or angular unit. If, for instance, the angle C of the triangle is determined by its three sides a, b, c, it must be a function of the numerical ratios of these sides, *multiplied by an angle*:

$$C = \text{right angle} \times f(b/a, c/a).$$

Since Young did not see any essential difference between angles and lines, he used the same argument and showed that the side c of the triangle could perhaps be computed from its angles A, B, C, by an equation such as (Young, 1856, p. 522):

$$C = \text{unit of linear measure} \times f(A, B, C).$$

After establishing the possibility of non-Euclidian relations, Young proves in his second paper an essential theorem of non-Euclidean geometry: the proportionality between the area of a triangle and the difference between two right angles and the sum of the angles of this triangle.

8. A HISTORICAL EVALUATION OF THE EARLY USES OF DIMENSIONAL ANALYSIS

What was the final influence of Foncenex's and Legendre's uses of dimensional arguments?

The Turin paper tried to provide an *a priori* proof of a physical law – the principle of force composition. Since it became afterwards clear that it is impossible to provide an *a* *priori* proof of the basic laws of mechanics, its method was regarded as flawed. Similarly, Legendre tried to provide an *a priori* proof of Euclid's fifth postulate. Since in the long run it was understood that this postulate is arbitrary and can be either true or false, depending on the accepted kind of geometry, Legendre's proof should necessarily have some mistaken assumptions.

It seems that the natural consequence of those attempts should be the *discredit* of dimensional analysis. Although some time after these attempts, a well-founded theory of dimensions way built by Fourier and used by Lord Rayleigh and other authors, *those particular instances* of use of dimensional arguments (in the Turin paper and Legendre's book) were not instrumental in establishing a valid method.

One may recognize a negative outcome of those works in Hermann Laurent's doubts about the very principle of homogeneity, first published in 1870 (Laurent, 1870, pp. 322-326).[18] Although Laurent cited the name of no author, it seems that he had in mind Legendre's work. He discussed the triangle argument and Leslie's criticism, and concluded that the homogeneity of geometrical formulae is not an *a priori* truth. But he added:

> Nevertheless, the equations of Geometry are homogeneous relative to lines, and this is due to the fundamental equations, from which all the others are derived, being themselves homogeneous […]
>
> In Mechanics, much the same as in Geometry, we cannot establish *a priori* the homogeneity of formulae; nevertheless, these formulae are homogeneous, since the fundamental theorems produce homogeneous relations […] Hence, the homogeneity exists in the mathematical sciences because this homogeneity has been introduced in the fundamental theorems; where it has not been introduced, it does not exist.

[18] The same comments appeared, without any change, in the second and third editions of the book, published in 1878 and 1889.

So, the equations are not homogeneous relative to angles. (Laurent, 1870, pp. 322-324)

Laurent's basic idea was this: there is no *a priori* reason to accept the homogeneity of equations; but this homogeneity is a hereditary property of equations; and in any field where the basic laws are homogeneous relative to same kind of quantity, all its theorems will also be homogeneous relative to that quantity. This allows us to reach some conclusions about the derived laws in a theory where we do already know that the basic laws are homogeneous, but we can say nothing *a priori* about the homogeneity of the basic laws themselves.

This opinion presented by Laurent was certainly not original, since a similar idea was criticized many years before, by Auguste Comte (1798-1857). He argued that the homogeneity of equations *is not* a hereditary property, since from two homogeneous equations of different degrees – for instance, one relative to lengths and another one relative to areas – we may produce another relation that is not homogeneous, by *adding* the two former equations (Comte, 1843, pp. 36-42). Such an addition is a mathematically correct derivation, since from $A=B$ and $C=D$ we may always derive $A+C=B+D$. In order to forbid the addition of equations of different degrees, it would be necessary to postulate the principle of homogeneity. Hence, Laurent's idea about the hereditary property of homogeneity is only true if we already assume the validity of the principle of homogeneity; since Laurent is trying to show that we do not need this principle, his argument fails.

Comte presented the meaning of the principle of homogeneity relating it to the problem of arbitrariness of units, using Fourier's ideas, which he certainly knew (Comte, 1892, vol. 1, pp. 181-184). Comte's approach was correct, and Laurent's was wrong; but since neither the Turin paper nor Legendre have presented a clear view concerning the principle of homogeneity, Laurent's doubts and criticisms are understandable, as being the most natural reaction to these earlier incorrect attempts to use dimensional analysis.

As a final evaluation of the early history of dimensional analysis, we may state that although Foncenex and Legendre had the priority of using this method, their model was not to be followed at that time. Their motivation was a natural undertaking of their time – the *a priori* proof of fundamental laws in mathematics and physics – but the concept of science underlying their works was soon abandoned. The method and concepts used in their contributions were not clearly understood, and some of their characteristics were in open disagreement with the concept of magnitudes and their relations, at that time. The controversies that followed Legendre's publication have shown that nobody had a very clear idea about the subject, and the scientific community did not reach an agreement on it in the following years: some authors seemed to think that Legendre had been refuted, other believed that he had successfully replied to all criticisms.

We may say that the Turin paper and Legendre's work were, in a sense, ahead of their time – but in a bad sense: something was lacking in their method, something that only became available much time after the earlier uses of dimensional analysis. A correct development of dimensional methods had to wait for an elucidation of the concept of dimensions and of the principle of homogeneity. This was done only in 1822, by Fourier. It was probably for this reason that later authors have progressively forgotten the early history of dimensional analysis, and the opinion was gradually built that everything had begun with Fourier.

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