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Mini-Workshop: The Reception of the Work of Leonhard Euler (1707-1783)

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Abstract. 2007 marked the tercentenary of the birth of Euler, famous as a major figure in mathematics. 16 scholars came together to discuss the influence of his work in some detail. The topics covered included not only pure but also applied mathematics (with engineering), physics and philosophy.

Mathematics Subject Classification (2000): 01A50, 01A55.

Introduction by the Organisers

Quite a number of scholars work on all aspects of Euler, especially in connection with the continuing preparation of the edition of his Opera omnia. But the historical study of the reception of his work during his lifetime and especially after his death has been rather patchy; for example, in the general volume [1] to commemorate the bicentenary of his death and in the recent Euler handbook [2] to note the tercentenary of his birth few articles dealt with aspects of reception in any depth. (This point holds of reception history in general.)

In our workshop the reception of Euler's work was usually considered up to around 1840. After that, it seems that it was ordinarily either used routinely or replaced, or quite forgotten, or was studied historically; a few exceptions are noted.

Sixteen scholars came together from eight countries to present their ideas on Euler's influence in a number of mathematical areas. The selection of topics is indicated in the table of contents; it includes several from applied mathematics, where scholarship is especially limited. We also discussed some examples of Euler's influence in various countries, but this kind of history is even less well developed than the influence by areas! In addition, we aired other neglected historical questions, such as: why was there such a small reception of the many papers and other writings that were posthumously published by the Saint Petersburg Academy between 1783 and 1862?

The sign 'E n' in the abstracts below indicates the number n of an Euler writing according to the list that was prepared in the early 1910s by the historian Gustav Eneström.

The meeting was a success and we are thinking of building up our efforts into a book on Euler's influence.

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Mini-Workshop: The Reception of the Work of Leonhard Euler (1707-1783)

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Abstracts

Algebra and Analysis

Rüdiger Thiele

I. Let us take the question: What is mathematics? A general belief is that (classical) mathematics deals with space and number, or more precisely with geometric and arithmetic magnitudes. In a widespread mathematical dictionary by Jacques Ozanam (1640-1717) published at the end of the 17^{th} century we find Euclid's view: mathematics is the science of magnitudes which can be decreased or increased.

Almost one century later, in his *Vollständige Anleitung zur Algebra* ('Complete instruction in algebra', 1770) Leonhard Euler repeated this statement but added the idea of functionality. In this mature work algebra is described as a generalization of arithmetic. Algebra, the extended arithmetic, is concerned with magnitudes that submit to certain operations, i.e. when general rules can be stated. Such objects are natural numbers, integers and rational numbers, but further, 'irrational numbers, though they cannot be expressed by fractions, are nevertheless magnitudes of which we may form an accurate idea', and it is 'this property [which] is sufficient to give us an idea of the number'. ([4], ser. I, vol. 1, I, §139). By calculation alone we have a clear and distinct idea of such magnitudes. Here Euler is in accordance with the philosophical view of René Descartes (1596-1650). Furthermore, 'From this definition [of magnitudes], it is evident, that the different kinds of magnitude must be so various, as to render it difficult to enumerate them: and this is the origin of different branches of the Mathematics, each being employed on a particular kind of magnitude' ([4], ser. I, vol. 1, I, §2). Euler then remarked: 'It appears, that all magnitudes may be expressed by numbers; and that the foundation of all the Mathematical Sciences must be laid in a complete treatise on the science of Numbers. This fundamental part of mathematics is called analysis or algebra' ([4], ser. I, vol. 1, I, §5).

Later by the term 'analysis' one understands more the method of determining those general rules whereas algebra has been the instrument to employ the rules. For Euler 'Mathematics is the science of quantity [magnitude], or the science which investigates the means of measuring quantity' ([4], ser. I, vol. 1, I, §2).

II. Once we have measured magnitudes algebra supplies rules and laws for dealing and handling with them. Analysis deals above all with such systems of arithmetical objects as the real numbers used in the measurement of continuously variable magnitudes (the geometric heritage); but Euler dealt also with imaginary numbers, which had no geometric meaning and could not be approximated (like irrational numbers) but nevertheless had rules. In his *Algebra* (1930) Leendert van der Waerden (1903-1996) started the chapter on 'Rings and fields' with these words, which echo Euler: 'The magnitudes with which one operates in algebra and arithmetic are of different nature. Now they are integers, rational, real, complex or algebraic numbers, now polynomials or integer rational functions of n variables, and so on. We will get to know magnitudes of very different nature such as hypercomplex numbers or residue classes, and so on. We can calculate with them like or almost like numbers.' However, in contrast to van der Waerden Euler had no axiomatic system.

It is quite natural in arithmetic to deal with polynomials, and the most interesting question is the so called Fundamental Theorem which looks for the roots of algebraic equations. Euler saw the necessity of proving the related Fundamental Theorem of Algebra without any reference to geometric intuition (report of an algebraic proof in 1746, published in 1751), and so did Jean le Rond d'Alembert (1717-1783) at the same time in an analytic manner. Both men independently demonstrated the Theorem but left gaps. In 1799 Carl Friedrich Gauss (1777-1855) provided his first demonstration but also left gaps. In the end Bernhard Bolzano (1781-1848) gave a satisfying proof.

In the beginning Euler regarded possible roots of an algebraic equation other than imaginary numbers, but from 1743 he restricted the solutions to complex numbers. In 1759 he tried to compose such solutions by roots, but – due to the properties of algebra – he necessarily failed for algebraic equations of a degree $n > 4$. Some results of Euler in number theory touch the origins of group theory (see the abstract by Olaf Neumann).

III. The development of analysis is mainly characterized by this question: What is an arbitrary function? We see firstly the shift from geometry to analysis; secondly the rise of the analytic expression and related power series; and finally the transition to trigonometric series.

As a consequence of the isoperimetric problems posed in 1697 Johann I Bernoulli (1667-1748) gave a first definition of an analytic function composed by the arithmetic rules: 'Here I call a function of one variable a quantity which is composed in some way of this variable quantity and of constants.' [2]

The crucial point is the expression 'in some way'; in other words, any representation is still fixed to the operations Bernoulli was familiar with. Besides a geometric representation (construction) we have for the first time an analytic representation. Euler, a disciple of Johann Bernoulli, immediately accepted this analytic concept of a function, and developed this idea to its limits (power series, even Puiseux and Laurent series). For a long time he did not change the wording of his definition: 'One quantity composed somehow from a greater number of quantities is called its or their function' [3]; but he extended permanently the admissible operations forming the so-called analytical expression: elementary functions, transcendental functions, functions which are defined by integrals or by an implicit equation.

In 1747 Jean le Rond d'Alembert wrote a paper on the vibrating string. He gave a general solution of the problem using two arbitrary functions which however were restricted by him to analytic functions (representable by power series). Dealing with the vibrating string, Euler noticed that the definition with which he begun had ceased to make sense. Consequently, he found it convenient to take also trigonometric series for the new problems of mathematical physics and even geometrical methods. This paper gives insight into Euler's openness in adapting the function concept to mathematical problems because already the simplest cases (plucked string) demand an extension of the function concept with roots in power series (Euler knew that power series cannot represent such initial shapes of a string.) In the Preface of the *Institutiones calculi differentialis* ([4], ser. I, vol. 10), published in 1755 but written in the time of the controversial discussion concerning the vibrating string, Euler gave a general definition he never used explicitly: 'If, therefore x denotes a variable quantity, all quantities which depend in some way on x or are determined by it, are called functions of this variable.'

Indeed, we have a new aspect: Euler speaks of the general dependence of variables instead of the composition of an analytic expression. The crucial words are 'depend in some way' and 'are determined'. In the actual forming of such functions Euler must use the known kinds of determination, which is why this 1755 definition does not differ so much from the 1748 definition.

Euler's 1755 generalization of the function concept was proposed under the needs of mathematical physics and accepted by his followers. Above all, the newly emerging topic of partial differential equations picked up the extension of the concept. But what is an *arbitrary* function? There is no clear definition of such functions, and above all the concept has changed in the course of time. We note functions that cannot be represented by an analytical expression using the elementary functions (but avoiding limit processes). The representation of the totally discontinuous Dirichlet function by a double limit process of continuous functions shows that if one accepts limit operations, than one must accept such compounded functions too. Euler admitted composed functions on an interval, subject to rules or laws there. But in the end such considerations lead to points instead of intervals, and if so extended the function concept is lawless, i. e. we have at each point x different rules for $f(x)$. Among the first mathematicians to see the possibility of such an extension was Jean Baptist Joseph Fourier (1768-1830); in the end such ideas gave rise to the study of point set theory.

Four years after Euler's death, in 1787, the Petersburg Academy posed a prize question on the nature of functions. The winner was Louis François Antoine Arbogast (1759-1803), who advocated Euler's viewpoint and introduced the discontinuity of solutions of partial differential equations in the modern sense (named by him 'discontigüité'). He wrote: 'In opposition Mr. Euler had the daring idea to regard curves not given by any law [arbitrary curves], and he was the first to say that they may be arbitrary, irregular and discontinuous, or composed by different parts of curves or freely drawn by hand which is moving in the space without any [mathematical] rules.' [1]

In 1771 Marie Jean Nicolas Caritat Condorcet (1743-1794), like Euler, smoothed the solutions of equations at non-differentiable points of functions. For Gaspard Monge (1746-1818) functions more general than those expressed by an equation were legitimate mathematical objects. Around 1810 great virtuosity in handling unknown arbitrary functions was shown by Simeon Denis Poisson (1781-1840). However, in the course of his investigations on heat theory Fourier remarked in a paper rejected by Joseph Louis Lagrange (1736-1813) in 1807: 'But herein we have dealt with a single case only of a more general problem, which consists in developing any function whatever in infinite series of sines or cosines of multiple arcs.' He gave a theory of trigonometric series in his *Theorie analytique de la chaleur* ('Analytic theory of heat', 1822). However, not before Dirichlet in 1829 were practical criteria for the representations of certain classes of functions offered, including those of classical physics.

Furthermore, in 1834 Dirichlet gave also a similar definition to Euler's in the Preface of the *Institutiones* (1755). However, Dirichlet still stuck to the classic meaning. His famous counterexample of an everywhere discontinuous function (given in 1829) is aimed more to show that there are bounded functions for which the Riemannian integral does not exist and less to reappraise the function concept. The definition given in 1834 by Nikolai Ivanovich Lobatchevski (1792-1856) is similar. Both Dirichlet and Lobatchevski were guided more likely by 'mechanical' motions than by modern functionality.

In 1854 Riemann presented investigations of the definite (Riemannian) integral in order to establish conditions that a function can be Fourier representable. He extended the results to certain functions which do not fulfill piecewise continuity. Regarding the preservation or destruction of integrability he classified the related sets (the modern description) without making set-theoretical consequences yet which were made by later researchers.

Finally, the arithmetization of analysis culminated especially in Richard Dedekind (1831-1916). By giving a one-to-one correspondence between arbitrary sets, he completely formulated analysis for the first time solely with analytic conceptions without any geometric visualization; *Was sind und was sollen die Zahlen?* ('What are numbers and what they are for?', 1887). Here we have a new shift in the concept, because topological concepts naturally linked with real numbers are lacking. However, that starts a fascinating story beyond our intentions.

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Elaboration of Euler's Ideas on Series in the early 19th Century

Hans Niels Jahnke

Euler's view of divergent series

Usually, mathematicians of the 18th century calculated with power series in the sense of modern formal power series. The question of convergence came up only when numbers were substituted in equations involving such formal series. In most cases mathematicians were sure about convergence for one or another reason and did not explicitly discuss it. Sometimes they gave explicit proofs of convergence. In any case, they tacitly assumed that a formal equality between formal expressions would imply numerical equality. With power series this is in general true, as already N. H. Abel remarked when he criticised this approach in the early 19th century.

In 1760 L. Euler published a paper where he tried to give a definition of the sum of a divergent series [2]. He distinguished between "convergent" and "divergent" series, saying that in a convergent series the terms get smaller and smaller and tend to zero, and that, therefore (?), adding them up would lead to its sum. The problem then is whether it is possible to generalize the notion of sum so that one can attribute a sum to a divergent series. Euler's solution is the 'definition': "the sum of any series is a closed expression out of whose development that series has been formed" ([2], §12). It is clear that he looked upon numerical series always as resulting from power series by substituting numbers. He justified his definition by saying that for convergent series it agrees with the narrower meaning of a sum. Also, when we calculate with indeterminates we can in every case substitute a power series by the finite expression out of which it has been formed. For Euler, this is a principle that he is not ready to give up. Implicit in Euler's argument is a notion of "formal equality" between a finite expression and its infinite expansion, which he did not make explicit. But there are possible interpretations as, for example, polynomial division and generating functions.

Euler gave a famous example which has been extensively treated by Hardy ([3], 26 – 29). It consists in the numerical series $1 - 1 + 2 - 6 + 24 - 120 + \cdots$, for which he found by different methods the value 0,596347362123.

Explications of Eulerian ideas in the early 19th century

In the early 19th century some mathematicians tried to explicate Euler's informal notion of formal equality. Among them were R. Woodhouse (1803: symbolic equality), G. Peacock (1832) and other mathematicians of the English Algebraic School, philosopher J. F. Fries (1822: Mathematische Naturphilosophie), Ch. Gudermann (the teacher of Weierstrass) (1825), M. Ohm (1822: Versuch eines vollkommen konsequenten Systems der Mathematik), and M. A. Stern (1860: Lehrbuch der algebraischen Analysis).

On the other hand, mathematicians of the time became aware of formulae which had been derived in the 18th century and which proved to be incorrect. One of the most famous examples of this sort was the 'representation of the powers of sine and cosine by the sines and cosines of their multiple arcs'. Euler, Lagrange and others had 'proved' for 'arbitrary' exponents m the formulae:

$$\cos mx + \left(\begin{array}{c} m \\ 1 \end{array} \right) \cos (m-2)\, x + \left(\begin{array}{c} m \\ 2 \end{array} \right) \cos (m-4)\, x + \cdots$$
$$= \sum \left(\begin{array}{c} m \\ \nu \end{array} \right) \cos (m-2\nu)\, x \quad = \quad (2\cos x)^m$$

and a similar formula including sines.

In 1811, S. D. Poisson showed these equations to produce incorrect results for, say, $x = \pi$ and $m = 1/3$. For more than ten years numerous mathematicians tried in vain to resolve this paradox and to give the correct summations for the sine and cosine series above. The first to provide the correct solutions were Martin Ohm (1823, Berlin) and L. Poinsot (1825, Paris) (see [4]).

The paradox was one of the motivations for N. H. Abel to write his famous paper about the binomial series in 1826, and Cauchy also treated the problem in that year. However, in contrast to Abel and Cauchy, who read the paradox as an argument against the use of divergent series, both Ohm and Poinsot pointed out that a controlled application of divergent series was a means to successfully treat the problem.

M. Ohm (1792-1872), brother of the famous physicist Georg Simon Ohm, was for most of his life Professor of Mathematics at the University of Berlin (see [1]). He felt a strong motivation to give a rigorous foundation to all of mathematics including elementary algebra, a problem in fact unsolved at the time, and published his respective "Attempt at a perfectly consequential system of mathematics" in 1822 [5]. In a sense this was a competing approach to Cauchy's *Analyse algébrique* of 1821 in that, like Cauchy, it required rigorous proofs of convergence but, in contrast to Cauchy, it was more faithful to 18th-century practices including the use of divergent series. His approach was similar to that of the English algebraic school.

Ohm abandoned the definition of mathematics as a 'theory of magnitudes' ('Größenlehre'). Instead, he considered analysis as a branch of arithmetic ('Zahlenlehre'). The only objects given from the outset were the natural numbers and the seven operations ('7 Verbindungn der Zahlen') $a+b$, $a-b$, $a \cdot b$, $a : b$, a^b, $\sqrt[b]{a}$, and $a?b$, ? designating the logarithm. For these operations elementary universally valid rules were stated as, for example, $a-(b+c) = (a-b)-c$, $a(b+c) = ab+ac$. Then, step by step, 0, negative, rational, irrational and complex numbers were introduced as symbolic expressions ('Zahlformen') $a-a$, $a-b$, a/b, a/b where a, b get infinitely large, and $a+ik$. Today we would speak of adjoining these symbolic objects as ideal elements to the respective domains. Ohm saw clearly that with every step of adjunction he had to define equality of expressions in the new domain, and consequently he introduced the concepts 'additively equal', 'multiplicatively equal', and so on. The only requirement for such a process of adjunction is in Ohm's words 'necessity of results'; i.e. one had to secure that no contradiction could occur in the respective old domain.

A great achievement was the systematic treatment of multi-valued expressions. Ohm called $a^x \cdot a^y = a^{x+y}$ an 'imperfect equation', since, in general, the multiplicities on both sides are different. By multiplying this equation with all roots of unity one got an equation with equal multiplicities on both sides, and this equation he called 'perfect': $a^x \cdot a^y = a^{x+y} \cdot e^{2\pi i(\mu x+\nu y)}$.

In a similar way, power series were introduced as symbolic expressions (equality being defined as equality of the scales of coefficients). Calculating with power series was a process involving the two steps of (1) deriving a formal equation that perhaps entailed a divergent series and (2) of investigating what happens when numerical values are inserted (domain of convergence, problem of multi-valued expressions). This can be exemplified by Ohm's treatment of the binomial series.

$$(1+x)^m = \sum_{k=0}^{\infty} \binom{m}{k} x^k$$

is an imperfect equation since for $m = p/q$ the left hand has q values whereas the right hand is single-valued. It becomes perfect by multiplying with the roots of unity

$$\underline{(1+x)^m} \cdot e^{2\pi i\, m\mu} = \left(\sum_{k=0}^{\infty} \binom{m}{k} x^k \right) \cdot e^{2\pi i\, m\nu}.$$

Ohm still considered this as a formal equation. The passage to a numerical equation required the determination of the domain of convergence and the investigation which value on the left side corresponds to which one on the right, i.e. one had to find a function $\mu = f(\nu)$ expressing this correspondence. Ohm gave a completely rigorous proof of convergence of the binomial series meeting Cauchy's standards whereas the determination of the function f was left to the application of the formula.

In fact, when solving the paradox of the above sine and cosine series the determination of f proved especially complicated. Ohm was able to overcome this difficulty and to arrive at the correct summation. For the result see [4].

Under the term 'algebraic analysis' Eulerian ideas survived in Germany in some institutions until the end of the 19th century. The English Algebraic School can also be seen as partly inspired by Euler's ideas.

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On the reception of Euler's calculus treatises

João Caramalho Domingues

Euler's set of treatises on the calculus [2], [3], and [4] are among his most classical works. At the time of their appearance they were much more advanced than any other calculus textbook (e.g., paying much more attention to differential equations, and in particular to partial differential equations), and were also innovative in several technical and conceptual details (e.g., in centering the calculus on *functions* rather than *curves*, and on the central role given to differential ratios equivalent to derivatives – often called by modern historians "differential coefficients", after Lacroix [5]).

It is quite obvious that these books were read by the major working mathematicians of the second half of the 18th century. But were they read by other people? And were they influential in the teaching of the calculus during that century?

The three treatises enjoyed reprints before 1800 ([2] in 1797; [3] in 1787, and [4] in 1792-1793). But it is striking that while their original editions are spread through 22 years (from 1748 to 1770), these first reprints are concentrated in the 10 years from 1787 to 1797. The first translations also appeared around this period: there was a partial French translation of [2] in 1786, a complete one in 1796-1797, and a German translation in 1788; there was also a German translation of [3] in 1790-1793; the exception is [4], which was only translated (into German) in the 1820's.

What could be the reason for this sudden interest in Euler's treatises? Possibly the same that Lacroix and Paoli gave for writing their own treatises [5] and [6]: the gap between the available textbooks and research works had become too large, and students who only knew the former would never be able to read the latter (these complaints refer to France and Italy, but could presumably be applied to other countries). The publication of Lagrange's *Méchanique Analitique* in 1788 may have been part of the trigger.

An analysis of the most common textbooks of the second half of the 18th century also suggests that the influence of Euler's calculus treatises was felt mainly during the 1790's (with the early exception of [1] in 1777). This can be seen: in the more advanced level of several textbooks published then (namely in their coverage of differential equations); in their greater emphasis on functions rather than curves; in their greater emphasis on the differential coefficient / derivative; in the presentation of the calculus of finite differences as a fundamental preliminary to the differential calculus; and in their *technical* reliance on power series. The latter is often identified as influence from Lagrange (who proposed a *foundational* reliance on power series); but already in [3] Euler had stated that whatever the function

y of x, its finite difference Δy can be expressed as $P\Delta x + Q\Delta x^2 + R\Delta x^3 + \&c$, whence (passing to infinitesimal differences) $dy = P\,dx$.

This Eulerian influence culminated with Lacroix's encyclopedic survey [5]. Lacroix's treatise was clearly modelled on Euler's, with two important distinctions: the addition of geometrical applications (but separated from the analytical theory), and the inclusion of many developments posterior to Euler's treatises (for instance, finite difference equations, or Lagrange's and Monge's work on partial differential equations). Still, many passages in [5] are taken (and acknowledged to be taken) from Euler.

After 1800, it is difficult to tell whether a particular author was directly influenced by Euler's treatises or by Lacroix's. But the latter possibility is more likely. An example is the case of Cauchy's definite integral: it descends from an approximation method appearing in [4], but almost certainly through Lacroix's version (which not only had a few improvements over Euler's, but also was used by Lacroix to discuss the "nature of integrals" as limits of sums).

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One Aspect of the Development of the Calculus of Variations after Euler

Michiyo Nakane

In *Methodus inveniendi* published in 1744, Leonhard Euler formulated the brachistochrone problem to find the curve of $y = y(x)$ that minimizes

$$\int_a^b Z(x, y, y' \ldots, y^{(n)})dx \tag{1}$$

where Z is a function of x, y and the first n derivatives of y with respect to x. Because of this formulation, the classical isoperimetric problems fall into the same category as the brachistochrone problem with a side condition. He demonstrated that the solution of the variational problem must be a solution of equation

$$\frac{\partial Z}{\partial y} - \frac{d}{dx}\frac{\partial Z}{\partial y'} = 0, \tag{2}$$

a form of the so-called Euler-Lagrange equation for extremal curves. Because of his formulation, we regard Euler as the founder of variational theory.

In 1755 Joseph Louis Lagrange, invented a new operator δ. Introduction of the δ-process eliminates geometrical images from variational problems. In 1764, Euler renamed the subject "the calculus of variations."

Lagrange's δ-process stimulated remarkable progress in variational theory. His most important contributions to the calculus of variations are: 1) He successfully expanded the Euler-Lagrange equation to cases where the integrand Z involves plural variables $y_1(x),\ y_2(x), \ldots y_n(x)$ and higher order derivatives of the variables with respect to x. 2) He considered the case where the last part of the curve was varied while the first part was fixed. 3) He made a modest start on considering multiple integral problems of the calculus of variations. 4) He formulated the variational problem with a side condition and developed his original idea of the multiplier rule. 5) In his study of mechanics, he occasionally applied his variational theory.

In addition to elaborating Euler's and Lagrange's works, the theory of the calculus of variations developed in another ways. Adrien-Marie Legendre, Carl Gustav Jacob Jacobi, Charles Delaunay, Simon Spitzer, Otto Hesse and others discussed the problem of finding exact conditions that make (1) minimum. On the other hand William Rowan Hamilton and Jacobi related variational problems to partial differential equations in 1830's.

In his examination of geometrical optics, Hamilton noted that the characteristic function

$$I = \int \nu d\rho \tag{3}$$

where ν is index of refraction, $d\rho$ is an element of the path, described his "system of rays." He noted that $\delta I = 0$ determined the paths of light rays. He thought the increment of I occurs when he passed from one curved ray to another infinitely near it. Then he varied the whole path including two end points to prove that the function I gives the geometrical property of light rays and obtained the partial differential equation for I. Therefore we can find the path determined by the variational principle by solving the partial differential equations. To relate the variational problem to the partial differential equation, it is crucial to vary the whole path with including end points.

Hamilton was aware that the action integral V could become the characteristic function in mechanics. In 1834 and 1835, Hamilton demonstrated this idea in his article "On a General Method in Dynamics." He introduced his original idea of canonical coordinates and found that Lagrange's equation of motion derived from the variational principle

$$\delta V - tH = \delta \int_0^t (T + U) = 0, \tag{4}$$

where T is kinetic energy and the U is the force function, if end positions are fixed. Since the solution of equations of motion reduced to the partial differential equation, variational problems and the partial differential equations can be related.

Jacobi noted this fact and developed a new varitational theory in 1842-43. He generalized Hamilton's theory to purely mathematical variational problems. Finally he arrived at the proto-type of the so-called Hamilton-Jacobi theory.

With contributions from several mathematicians, including Eugenio Beltrami, Rudolff F. A. Clebsch, Adolph Mayer, and David Hilbert, this theory became one of the important branches in the calculus of variations. The variable end-point problem, which developed just after Euler, opened a new stage in the history of the calculus of variations.

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Leonhard Euler's Theory of Space and Time and its Reception by Kant

Helmut Pulte

Euler's relevance for philosophy in general should not be seen in any original contribution to the doctrines of 'school philosophy' of his time. Rather, his thinking marks a *turning point* in the *relation* of philosophy and science and promoted a new attitude on how to reflect upon science. According to Ernst Cassirer, Euler was the first who – at least in the German speaking-tradition – declares science to be of age. With him, classical mathematical philosophy of nature gained a self-awareness that is, so to speak, a *historical* precondition of Kant's approach, because Kant's starting point is 'science as a fact' that demands a new form of scientific philosophy. This *transcendental* philosophy puts the relation of mathematics and experience on a new ground and strongly depends on space and time as 'forms of pure intuition'. Euler's theory of space and time is of some relevance for this new view and seems, more generally, most suitable in order to make 'Cassirer's turning point' visible.

In order to demonstrate this, we treat first Euler's philosophical convictions in general, as far as they are relevant for his understanding of space and time. Here, his criticism of the Leibnizean distinction of 'real' and 'ideal' concepts has to be considered: General concepts of mathematics can always be found in the more special concepts which correspond to physical objects. Mathematical concepts are gained from experience by abstraction and are therefore always applicable to experience. Euler regards a demarcation of ideal and real concepts not only as superfluous, but also as damaging: Such a demarcation makes implausible why mathematics is so successfully applied in natural science and, at the same time, deprives natural science from its most important instrument for uncovering sense deceptions.

In addition, Euler's philosophy of science has to be taken into account. It can be characterised as *essentialism* in the sense of Karl Popper. On the basis of a definition of what he holds to be the 'essence' of bodies, Euler builds up his program of rational mechanics, which includes the derivation of principles of mechanics (both Newtonian and analytical), which are not only regarded as true but even as necessarily true. These principles form the axiomatic foundation of the whole body of rational mechanics, which is deductively developed from it and which is – on the other hand – the foundation of natural science in general. This whole essentialistic and axiomatic-deductive image of science turns out to be crucial for the ontological status of space and time in his philosophical considerations.

Next we present Euler's analysis of space and time. The focus is mainly on his 'Réflexions sur l'espace et le temps' (from 1748), though his earlier and later discussions are taken into account. The 'Réflexions' reveal a new criticism of a relational concept of space and time and they give, for the first time in Euler's career, a *realistic* interpretation of these concepts, though Newton's arguments are not central in this context. In fact, Euler's whole approach is *not* based on premises belonging to his philosophy of nature, but rather on epistemological considerations and his concept of science: We come to the concept of space only when we completely *remove* the body itself in our thinking. Place, which constitutes space, can not be understood as a general property of a body; it is quite different from extension. The Leibnizeans are badly advised when they deny that place has reality, for the simple reason that it is an ideal concept gained by abstraction. Therefore, space must be real in some sense (and the same conclusion holds for time). However, the reality Euler has in mind is not to be understood as based on sense experience, but as a kind of reality in its own right that can only be grasped by *reflection*. It refers to the whole of our empirical knowledge, which is certain and beyond any doubt. Absolutely certain are, first and above all, the principles of mechanics – there truth is 'indisputable'. This holds especially for the principle of inertia: *As* this principle has the status of an indisputable axiom and *as* it is – according to Euler's analysis – in need of the postulate of absolute space, we have to accept absolute space not only as a mathematical hypothesis, but also as a *real entity.* As Euler says: 'It would be quite absurd that such merely imagined things might serve as the foundation of the real principles of mechanics'.

This argument for the reality of absolute space is based on the accepted principles of mechanics, and thereby – according to Euler's concept of science – on the basis of the whole of the empirical knowledge about the world of physical objects. Euler's starting point is this body of knowledge, and then he asks for the conditions, under which this body of knowledge can be regarded as true. This can only be the case if the principles of mechanics are true. Therefore, concepts of space and time have to be established, which serve as foundation of these principles. In Kantian terms, Euler's analysis can be described as an analysis of the *conditions of the possibility of these principles.* We are far from claiming that Euler was the author of a 'transcendental aesthetics' some decades before Kant. But his argument for the reality of space – and that for the reality of time is parallel – is not logically circular, but *transcendental* in a specific sense: Space and time are real in the sense that they are the precondition under which we can grasp the phenomena of the physical world under general concepts and laws. Thus, the kind of reality attributed to space and time corresponds to what Kant later labels as 'empirical reality'.

In his *Theoria motus* (1765), Euler continues his analysis of space and time as concepts in their own rights, which do not fit into the established types of school philosophy: 'However, if the philosophers divide up all realities into certain classes, and conjecture that place can not be related to one of it, then I will prefer to believe that there is something wrong with their classes, because they did not recognize the things related to them [i. e. the classes] sufficiently. The relation of *time* is quite similar [...]'. In the end, it is the function of space and time to make scientific experience possible, in which Euler is interested and which is not sufficiently articulated in the philosophical doctrines of his time.

Finally we handle the reception of Euler's analysis in Immanuel Kant's 'precritical' period and an outlook to Kant's 'critical' transformation of his reception. Kant's reception of the 'Réflexions' is at first positive and without reservation. Later, his position is more complex: He shares Euler's view that space must be regarded as something real, but also remarks that Euler did not 'completely reach' his aim to demonstrate the reality of space. In the end, however, he completely accepts the *main result* of Euler's analysis of space, i. e. that absolute space forms a *new* kind of reality: It is no mere 'object of thinking' ('Gedankending'), and it is no 'object of outward sensation' ('Gegenstand der äußeren Empfindung'). It is a 'basic concept', the reality of which should be 'grasped by ideas of reason', and this basic concept is the precondition of the possibility of outward experience.

In his work *Der philosophische Kriticismus* (1876) Alois Riehl was the first who demonstrated that the concept of space is of utmost importance for Kant's transition from his precritical to his critical period around 1770: Kant's train of thought starts with space as a basic concept that makes experience possible and from *this* starting point comes to the transcendental aesthetics, where space and time are established as subjective forms of our sense, as 'forms of pure intuition'. Euler was certainly no 'school philosopher', but if we fit his theory of space and time into this Riehlean frame, it seems plausible to say that his impact on our modern view of how philosophy and science are related (Cassirer) and of philosophy of science itself should not be underestimated.

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Euler's Influence in Celestial Mechanics

Curtis Wilson

Euler was the first to open up a way of applying the Leibnizian calculus to multiple moving bodies attracting in accordance with Newton's gravitational law. Having discovered in 1739 the importance of trigonometric functions for the solution of certain differential equations, he proceeded to develop, for the first time, a systematic treatment of the *calculus* of trigonometric functions. Introducing the modern notation for these functions, he made explicit their role *qua* functions [8]. Sines and cosines with arguments proportional to the time t could now, by means of the chain rule, be differentiated and integrated. Differential equations expressing the forces acting on the different bodies could be formulated and solved – if only by approximation.

In the early 1740s Euler applied this calculus to the Moon's motions; he published his results (but not their derivation) in 1746. Beginning in the spring of 1746, Clairaut and d'Alembert likewise applied Leibnizian and Eulerian algorithms to deriving the Moon's motions. All three mathematicians achieved theories accurate to about 3 to 5 arcminutes. Their calculations yielded only about half the motion of the Moon's apogee.

Since 1714 the British Board of Admiralty had been offering a handsome prize for a method of finding the longitude at sea to within 60 nautical miles. A lunar theory good to 2 arcminutes would suffice. By 1754 Tobias Mayer had obtained lunar tables accurate to about $1\frac{1}{4}$ arcminutes. A refined version of these tables became the basis of the British *Nautical Almanac*, first published for the year

1767. The superior accuracy of Mayer's tables derived in large part from empirical correction of the coefficients. (A full analysis of Mayer's achievement remains a desideratum.) The *Nautical Almanac* would continue to rely on empirical comparisons till 1862. In contrast, algebraic lunar theories derived from Newton's law, and relying on observation only for the constants of integration, converged with painful slowness. To rival the accuracy of the *Almanac* tables along this route was prohibitively labor-intensive – a difficulty Euler will address.

The study of planetary interactions presented a different difficulty. When a planet moves from conjunction to opposition with a planet farther out, the distance between the two planets varies hugely; their mutual gravitational force varies yet more hugely. For application of the Leibnizian calculus, the algebraic expression of this force needs to be conveniently tractable. Euler invented trigonometric series for the purpose, and showed how the coefficients of the successive terms could be determined [1]. These series were widely adopted in investigations of planetary motion, but in the 1760s Euler came to distrust them. His theory of Jupiter and Saturn had failed to account for a major anomaly in the motion of these planets, and he mistakenly blamed the failure on slow convergence of the series. The cause of the anomaly was discovered by Laplace in 1785.

The slow convergence in lunar theory was real. In 1762 Euler proposed using numerical integration to compute ephemerides of the Moon and planets directly from the differential equations [2]. In 1763, he showed how to obtain, by means of the calculus of finite differences, exact initial conditions of position and velocity from a series of observations of a celestial body on successive days [3]. These methods are used at JPL today in deriving ephemerides of the Moon and planets.

In 1766, Euler's eldest son – presumably expressing his father's thought – proposed a new plan for obtaining an accurate lunar theory [4]. First, the inequalities depending solely on the angular elongation of the Moon from the Sun were to be derived, all other inequalities being neglected. This first step yields a solution of a simplified version of the three-body problem. Then this solution is to be perturbed to take account of inequalities depending on successive powers and products of the other small parameters on which the Moon's motion depends: eccentricity of the Moon's orbit, eccentricity of the Earth's orbit, ratio of the Sun's parallax to the Moon's parallax, and the Moon's orbital inclination to the ecliptic. This was the route followed in Euler's third lunar theory [5].

It was also the route followed in 1877-78 by George William Hill in founding the present-day theory of the Moon's motions [6], [7]. Using Jacobi's integral of the equations, Hill was able to compute the numerical parameters determining the 'variation curve' with a precision of 15 decimal places. When he then introduced eccentricity into this orbit, and computed the resulting motion of the Moon's apse, he obtained a value differing by only $1/60^{\text{th}}$ from that found by observation of the actual Moon. The problem of convergence had thus been effectively vanquished.

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The Reception of Euler's Elasticity: Letters from Legendre to Sophie Germain

Stacy G. Langton

Laplace is supposed to have said, "Read Euler, read Euler. He is the master of us all." But, as Victor Katz remarked, it is difficult to figure out who actually read Euler's works [2, p. 232]. In this talk I described an episode in which, because of an unusual combination of circumstances, we have a written record of mathematicians reading and discussing Euler.

In 1809, the Paris Institute announced a prize competition for the best mathematical explanation of the patterns that had been demonstrated by Ernst Chladni's experiments with vibrating plates. In 1815 the prize was awarded to Sophie Germain. This competition led to the work of Poisson and Navier on elastic plates and solids, and ultimately, starting in the 1820's, to the work of Cauchy.

Around 1811, Sophie Germain began to work on the Paris prize problem. As a preliminary study, she read Euler's 1774 paper "Investigation of the ways in which elastic laminas and rods can vibrate" [E 526]. According to the biography of Germain by L. Bucciarelli and N. Dworsky, she composed a manuscript in which she attempted to generalize some of Euler's results [1, p. 46]. But she apparently encountered a snag in her work, and asked her friend and patron Legendre for help. In January, 1811, Legendre wrote a series of three letters to Germain in which he discussed certain aspects of Euler's paper. The letters were published in 1879 by the journalist and Paris municipal councillor Hippolyte Stupuy, in a book containing reprints of Germain's correspondence and of some of her published works [3, pp. 287–298]. These letters were the subject of the present talk.

Building on his work on the foundations of elasticity theory in the early 1770's, Euler in E 526 derives the partial differential equation for a vibrating elastic rod:

$$\sigma\frac{\partial^2 y}{\partial t^2} = E\frac{\partial^2 y}{\partial s^2} - B\frac{\partial^4 y}{\partial s^4},$$

where t is the time, s is arc-length along the rod, y is the displacement normal to the rod, σ is the density, E is the horizontal tension, and B is an elastic constant, the "stiffness" of the rod. If $B = 0$, we just get the ordinary second-order wave equation. If, on the other hand, $E = 0$, we get the fourth-order equation

$$\sigma \frac{\partial^2 y}{\partial t^2} + B \frac{\partial^4 y}{\partial s^4} = 0$$

for the vibrating rod. Most of E 526 is devoted to finding solutions of this equation.

Separating variables, Euler finds a fourth-order ordinary differential equation

$$X'''' - \frac{\omega^4}{a^4} X = 0,$$

for the spatial contour $X(s)$ of the simple mode, where a is the length of the rod and ω is an eigenvalue which determines the frequency of vibration. Euler now analyzes 6 kinds of vibration, characterized by the boundary conditions imposed at the ends of the rod. For example, if one end of the rod is "pinned", so that its displacement is 0 but it can turn freely about the pin, Euler argues that the corresponding boundary conditions are

$$y = \frac{\partial^2 y}{\partial s^2} = 0.$$

Near the end of the paper he considers a rod which is pinned at both ends and also at its mid-point. The corresponding simple modes are given by certain piecewise-defined functions

$$X = \begin{cases} \alpha e^{\omega \cdot s/a} + \beta e^{-\omega \cdot s/a} + \gamma \sin\left(\omega \cdot \frac{s}{a}\right) + \delta \cos\left(\omega \cdot \frac{s}{a}\right) & \text{for } 0 \le s \le \frac{1}{2}a, \\ \alpha' e^{\omega \cdot s/a} + \beta' e^{-\omega \cdot s/a} + \gamma' \sin\left(\omega \cdot \frac{s}{a}\right) + \delta' \cos\left(\omega \cdot \frac{s}{a}\right) & \text{for } \frac{1}{2}a \le s \le a. \end{cases}$$

The conditions that the rod be pinned at both ends give four boundary conditions. At the mid-point, both piecewise-defined displacements must be 0, and Euler requires that the first and second derivatives be continuous at the mid-point. Thus, there are four conditions at the mid-point, for a total of 8 conditions that must be satisfied by the 8 coefficients $\alpha, \beta, \ldots, \gamma', \delta'$. In order for non-trivial solutions to exist, the determinant of this system must vanish. This vanishing gives a condition on ω, namely,

$$\left(\sin \frac{1}{2}\omega\right)\left(\tan \frac{1}{2}\omega - \tanh \frac{1}{2}\omega\right) = 0.$$

In the course of solving his 8×8 system, Euler has to divide by $\sin \frac{1}{2}\omega$. Later, when he works out the solutions corresponding to $\sin \frac{1}{2}\omega = 0$ and finds that some of them have $\sin \frac{1}{2}\omega$ in the denominator, he multiplies that factor out again. Legendre, in his first letter to Germain, asserts that this is an error on Euler's part. Later, in his second letter, Legendre has understood better what Euler did, but he now discovers an actual error in Euler's work. The solution that Euler writes down for the case $\sin \frac{1}{2}\omega = 0$ is incorrect. In the present talk, I identified the nature of Euler's error.

In his third letter, Legendre observes that the solutions in the case $\sin \frac{1}{2}\omega = 0$ correspond to a rod which is pinned only at the ends, but which vibrates with a node at the mid-point. Consequently the pin at the mid-point is inessential; it provides no reaction. Legendre then speculates that something similar might be the case when $\tan \frac{1}{2}\omega = \tanh \frac{1}{2}\omega$. This speculation is incorrect, however. The solution in this case has a discontinuity in the third derivative at the mid-point, and consequently the pin at the mid-point must provide a reaction.

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Leonhard Euler and the Formation of Hydrodynamics

Gleb K. Mikhailov

I. Leonhard Euler's contribution to the formation of hydrodynamics is twofold, according to the double meaning of the latter term. Today we understand under hydrodynamics the general theory of fluid motion, while Daniel Bernoulli treated it as a theory of quasi-one-dimensional fluid flow that is now the subject of Hydraulics. Until recent times it was unknown that Euler had been in fact closely connected with the formation of hydraulics [2] [3].

Euler's Basel 'Notebook' contains a detailed presentation plan for compiling a large treatise on hydraulics (including topics such as the efflux of fluids from vessels, water flow in rivers, and water waves) and solutions of some problems of the efflux of water through orifices of finite cross-section. At that time there was no mathematically based theory of water flow at all. Therefore, the results obtained here by the 19-year-old Euler should be considered as path-breaking.

After Euler's arrival to Petersburg two similar reports were presented consecutively in the Petersburg Academy of Sciences in the summer of 1727. Delivered by Daniel Bernoulli and Euler, they treated the problem of non-stationary water efflux from vessels. Both men presented the same theory and used the principle of conservation of living forces, whose admirer and propagator was their teacher Johann Bernoulli.

A delicate 'copyright' situation arose in this connection. As a result, Euler set aside his studies in the theory of fluid motion for about 20 years and left the whole field to Daniel Bernoulli. However, I have found in the Petersburg Archives a paper by Euler from 1727 [1], where he expounded his theory of water efflux from vessels.

Daniel Bernoulli's famous *Hydrodynamica* appeared in 1738. The fame of the *Hydrodynamica* deeply wounded Johann Bernoulli, who was always jealous. At once he began to create his own theory of water motion and sent the first part of his *Hydraulica* to Euler in March 1739. Johann rejected here the principle of living forces and used some new, but unclear, conceptions. At the same time Johann promised to send soon to Euler the second, expanded part of his *Hydraulica.* Euler re-conceived the unclear ideas of his old teacher in a much better form than that of their originator. In his reply to Bernoulli of May 1739, Euler outlined a clear exposition of the generalized theory of water motion in vessels and pipes.

Euler's letter put his teacher in a very delicate situation, as Euler had formulated for him practically the main idea of his whole work. As a consequence, the preparation of the second part of *Hydraulica* required more than a year. Actually, it was an entirely new work compiled via short instructions given by Euler. Here comes to the end the latent contribution of Euler to the development of hydraulics.

II. Euler's well-known contribution to the formation of modern hydrodynamics is connected with his works from the 1750s. In 1748 the Berlin Academy announced a competition on the subject of fluid resistance. In 1749 Jean d'Alembert presented for the competition an interesting *Essai*; while it was vaguely written and not quite satisfactory, it contained attempts to obtain general equations of fluid motion in terms of partial derivatives. The academic jury (not without Euler's influence) rejected it, together with all other presented papers, as not confirmed by experiments.

However, the hint contained in d'Alembert's *Essai* encouraged Euler to develop a perfect, irreproachable theory of fluid motion based on his 'new principle' that involved isolating an elementary particle from a continuous medium and applying to it Newton's basic law of dynamics written in components along fixed coordinate axes. He presented it to the Academy in the autumn of 1750.

In the first half of the 1750s Euler read in the Academy a set of papers on fluid flow. The foundations of fluid mechanics were given by him in two fundamental papers published together in 1757 in the Berlin *Mémoires* (E.225, 226). They contain main equations of hydrostatics and hydrodynamics of an ideal fluid, presented in a quite modern form.

In the second paper Euler considers the motion of an elementary fluid parallelepiped, as we do in many manuals till now, and obtains the 'Euler hydrodynamic equations' for an ideal compressible fluid as well as the continuity equation. He notes that to these equations there should be added another one giving the relation between the pressure, density and an additional physical property that affects the pressure and is generally understood to be the temperature. According to Euler, the resulting equations 'include the entire Theory of the Motion of Fluids'.

After deriving the general equations, Euler introduces the potentials of force and velocity and obtains in particular some integrals for the case of an incompressible fluid; they are now usually called Lagrange-Cauchy integrals. In addition, Euler specifically mentions the existence of non-potential flows. The paper concludes with a remark that the equations derived transpose the problems of fluid motion from the realm of mechanics to that of mathematical analysis.

The corresponding mathematical problems of hydrodynamics, especially those for vortex and free surface flows, are really very complicated. Even now we can solve only some of them successfully, mainly using numerical methods.

III. These papers by Euler were followed by a number of his other investigations in the fields of hydrodynamics and acoustics. They were basically accomplished and generalized in a large essay published in 4 parts in the *Novi Commentarii* of the Petersburg Academy of Sciences (1769-1772) (E375, E396, E409, E424). Its second part is devoted to the general equations of hydrodynamics of an ideal fluid and to the study of special cases of fluid flow. Also considered here is the determination of fluid motions according to the given initial conditions and the general equations in the so-called Lagrange (mass) variables; it should be noted that the 'Lagrange variables' were introduced first by Euler in a letter to Lagrange written in 1760. The last part of the essay presents a generalisation of his previous investigation in acoustics and the theory of musical wind instruments. This part (as well as his theory of tides) is more connected with the oscillation theory than with hydrodynamics itself.

Applications of Euler's hydrodynamics were mainly connected – at the beginning of the 19th century – with the theory of water waves. The following essential progress of the hydrodynamics of an incompressible ideal fluid and of gas dynamics was attained by the middle of the 19th century and was connected with studying vortex and free surface flows, as well as shock waves.

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Euler and Engineering

Ed Sandifer

Engineers commonly use four points to define their profession:

design,
analysis, and
construction,
for a practical purpose.

Euler is widely known among engineers, but people seldom call him an engineer. However, we find engineering, to various degrees, in a variety of Euler's work, including:

ballistics	shipbuilding	fountains
windmills	saws	gears
capstans	agriculture	screw of Archimedes
bridge building	columns	optics

He mostly worked on problems of his own choosing in mathematics and rational mechanics. Most of his work was theoretical, and he usually had no particular construction or practical purpose in mind.

There are some conspicuous exceptions. Euler himself did what we would call "engineering" when he advised Frederick II on the construction of the fountains of Sanssouci [1] and on repairs to the Finow Canal. Much of Euler's work on optics also qualifies as engineering, particularly his correspondence with John Dollond [4] as Dollond was inventing and constructing his achromatic lenses, and as Euler developed the general principles of microscopes and telescopes incorporating the new lenses. Rather than recount these episodes here, we defer to the sources cited.

In 1741, as he was leaving St. Petersburg for Berlin, Euler wrote a brief essay, "On the utility of higher mathematics." It was not published in his lifetime, but found print in the 1850s in French (1853) and Spanish (1853), as well as its original Latin (1847).

In this essay, Euler contends that the utility of "elementary mathematics" (algebra and arithmetic) is widely known, but that few people recognize the usefulness of "higher mathematics" (calculus) in practical problems. Euler gives examples in mechanics, hydrostatics, astronomy, artillery, physics and physiology. Euler's sentiments in this essay are reflected in Euler's enthusiastic reception of Benjamin Robins' book on ballistics.

In 1742 Robins published *New Principles of Gunnery*, a 150 page book in which he applied some of the new tools of analysis to ballistics. In 1745, Frederick II asked Euler to identify and translate the best available book on artillery, and Euler recommended Robins' book. What resulted was Euler's 720 page "translation" and commentary.

Euler's 1745 edition was translated back into English by Hugh Brown in 1777, and into French by Lombard in 1783. Napoleon learned the principles of artillery from the Lombard edition.

Frederick Rickey and Shawnee McMurran make the case that the Euler/Robins ballistics texts caused calculus to be introduced into the curriculum at the military academies, first at Woolwich, then in the French schools, and finally at the United States Military Academy at West Point.

At least one of these editions, probably German but maybe French, found its way east to Austria and into the hands of the Slovenian mathematician and artilleryman Jurij Vega. Euler had made some calculations about the combustion of the powder in the firing chamber, using the then-current design of a cylindrical chamber with a firing hole at the top of the chamber. Vega, a professor of artillery at the artillery school in Vienna, realized how those calculations would be different for different shaped chambers and different positions of the firing hole. Adapting Euler's calculations, Vega designed a conical chamber, ignited at the back, and more than tripled the range of Austrian mortars. This was extremely significant for the Austrians during the Napoleonic Wars.

Euler published two important books on naval science, E110-111, *Scientia navalis* in 1749, and E426 *Théorie complette de la construction et de la manœuvre des vaisseaux* in 1773 and he also wrote a number of papers, many for the Paris Prize competition. An account of one of these, E413, "De promotione navium sine vi venti" is given in [3].

E426, the *Théorie complette*, also has an interesting story. Henry Watson translated it into English in 1776. A 1790 posthumous edition includes an account of the life of the translator. It says that, while stationed in India, he used Euler's principles to design and build two sailing ships and that they were among the fastest and most maneuverable ships on the water. When he was unable to secure a license to use them for privateering around the Philippines, he made a fortune using them as trading ships.

He was swindled of his fortune in a scheme by the British East India Company to build wharfs in Bombay, and he died of a fever while on a trip back to England to try to recover his fortune in the courts.

Euler's influence in bridge building is largely based on his work in elasticity theory. He wrote more than a dozen articles on the subject. Euler's buckling formula is found in E8 and in E831, and is regarded as one of the fundamental formulas of engineering. It gives the critical force that a simply supported column can bear without bending in terms of the shape of the column (as measured by its moment of inertia), its length, and a parameter of the material from which the column is constructed, now known as Young's modulus, after Thomas Young (1773-1829, FRS 1794), who was also an Egyptologist, and the one who translated the demotic section of the Rosetta Stone.

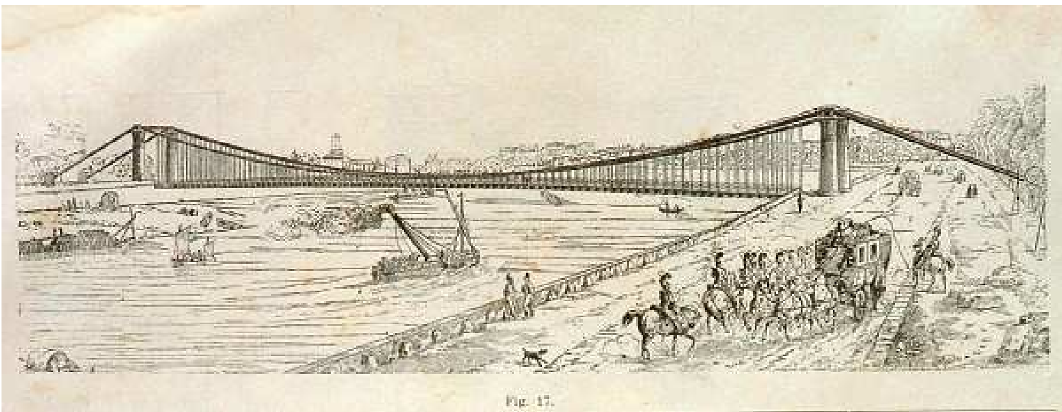

FIGURE 1. The Pont des Invalides

Euler's buckling formula and his related work were quite influential in the 19th century, particularly on the theoretical work in France and on applications in England. In 1826, when he published his influential *Leçons* at the Ecole Polytechnique, Navier [2] included a long historical introduction and gave Euler a particularly prominent role. Despite his many theoretical and practical successes, Navier was known in his own time for an engineering failure. The Pont des Invalides, designed and constructed by Navier, collapsed in 1826, the same year it was built. Just as Euler was unfairly blamed for the failure of the fountains at Sanssouci, [1] so too Navier perhaps bears too much of the blame for the failure of the Pont des Invalides, as it was a failure of the bridge abutments, not the bridge itself, that brought down the bridge. The Dee Bridge, built in 1846 by Robert Stephenson (1803-1859), suffered a similar fate, lasting only a year.

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Some Aspects of Euler's Influence on Approximation Theory and the St Petersburg Mathematical School

Karl-Georg Steffens

As is well-known, Pafnuty Chebyshev (1821-1894) was the first mathematician who deeply investigated the problem of best uniform approximation, which in the classical setting can be stated thus:

> Let f be a continuous function, $a, b \in \mathbb{R}$, $n \in \mathbb{N}$. Find a polynomial of degree at most n, so that
>
> $$\max_{x \in [a,b]} |f(x) - p(x)|$$
>
> will be minimal for all polynomials of degree at most n.

The most important property of a solution of this problem is the fact that it gives an estimation of the error of approximation for every point of the interval $[a, b]$. But due to the difficulty to solve this problem concretely the first results were presented only in the 19th century. Chebyshev himself gave a necessary condition for the solution, stating that there must be at least $n + 2$ points where the error function $f - p$ reaches its maximum, but he did not explicitly mention that these deviation points reach the maximum errors with an alternating sign. Thus, the alternation theorem giving a characterization of the solution was not proved by him. Using Chebyshev's results Kirchberger made a first try, which was completed by Borel and Young (for a detailed discussion see [3, Chapter 4]). An algorithm to compute the best approximation was firstly presented by Remez only in 1934. Here the alternation property was crucial for defining the iterating process generating the solution.

1. Euler as a Forerunner in the Field of Best Uniform Approximation Theory

Nevertheless, some special cases had been discussed before Chebyshev, and the first of all was a problem of cartography Leonhard Euler dealt with during his second stay in St Petersburg. In 1777 Euler analysed the local and global accuracy of the Delislian conic projection in the contribution [2], where he tried to approximate the proportion of longitudes and latitudes of the map to the real proportion of the terrestrial globe.

This paper was the last and concluding of three papers dealing with cartography. It was published in 1777 (Eneström index E490 - E492) and seemed to build upon his former work at the cartographic department of the St Petersburg Academy of Sciences. At first he proved the fact that a part map cannot be projected to the plane preserving the scale in both dimensions. Then he discussed the question of which kind of projection would be the most convenient for a map of all Russia. So discussing several kinds of projections, e. g. stereographic and polar projections, he had argued that De Lisle's projection should be chosen because of the following important properties:

(1) Parallels and meridians intersect perpendicularly
(2) It gives a locally good approximation

So one can use the map for estimating the distance between any two points having a correct orientation.

The mathematical problem that Euler solved there was approximating $\cos x$ by a linear function. He observed that the best approximation could be characterized by the fact that there must be three devíation points where the error function $a - bx - c\cos x$ must reach maximum values alternating in sign - the alternation theorem in its simplest shape!

With this setting he could compute a best map in his sense. This work determined the Euler projection, which was used for a map of whole Russia till the beginning of the 20th century.

2. How Euler's Concepts Influenced Chebyshev's St Petersburg Mathematical School

It is well-known that Euler was the first mathematician of international reputation working in St Petersburg. No doubt that the second one was Chebyshev. So it is quite natural to wonder how the first had an impact on the latter.

In the 1840s in Moscow University mathematical lectures were based on the work of Euler, Lacroix and Cauchy - so as a student there Chebyshev definitely dealt with Euler's work. Later, already as a lecturer of St Petersburg university, he was engaged in the edition of some of Euler's papers on number theory, being encouraged by Bunyakovski. So it is not audacious to assume that Chebyshev very well knew Euler's work.

We know that besides his interest in solving problems and reaching deep results Euler also tried to develop the basic concepts of mathematics to get a more elegant theory without unexplained concepts like the behavior of the solutions of the wave equation outside the characterizing interval that d'Alembert discussed in the 1740s. So problems of this kind led Euler to define functions as 'depending quantities' in E212 abstracting from an analytical expression that he had demanded eight years before following Leibniz in E101.

With this remark it is surprising that Chebyshev had no interest in a further development of mathematical concepts. In 1856, in the year when he became a member of the scientific committee of the ministry of national education, he gave the following programmatic statement

> "The congregation of theory and practice gives the best results, and it is not only practice which gains a benefit from this; science itself is developing under the influence of practice.[...] If the theory gets much from new applications of an old method [...], then it gains even more from the discovery of new methods, and in this case science finds itself a true leader in the practice."

Moreover, he sharply criticized efforts to mix mathematics with philosophy, as he called it:

> "I divide mathematicians into two categories: those who deal with mathematics to solve new problems from nature and whose results are clear, and the others who love mathematics as subject to philosophise about. I think that the second type should not be called mathematicians, and whoever falls on this way won't be a mathematician. According to the question about the statute of the university I therefore clearly said that at the faculty of mathematics lectures about philosophy are not desirable."

We might assume that Chebyshev wanted to lead mathematics in a special direction abusing his enormous influence in science and policy at that time. Surely, it was not so. The above citation was taken from a lecture about probability theory (see [1]), and not from a programmatic paper. To make it clearer, we also should take the following citation:

> "You have already recognized that sometimes I come down to philosophizing, but then it is not about the subject, not about what a quantity, a space are like, but about methods. I do not think about the origins, but about what should be in mind for the solution of new problems. For this all kinds of considerations are useful. But philosophizing about what a infinitely small quantity is, does not lead to anything. Here we have one of two alternatives: Either we go via philosophizing to the point that conclusions using infinitely small quantities were not strict - and then we would have to reject the infinitely small - or we come to the fact to prove the correctness of those conclusions. The experience shows that all people who dealt with similar questions explained this only

> for themselves and did not add anything for the solution of new problems. In these cases I do not recommend you to philosophize."

So there is still a place for the discussion of general concepts of mathematics. But it is justified to say that for himself Chebyshev did not see any use in such considerations. He warned his students, but did not forbid them anything.

How the other members of the St Petersburg Mathematical became engaged in such discussions can be seen in their lectures and programmatic statements. In fact we can say that there were two fractions - those who rejected any discussion about concepts in general (represented by Korkin and Lyapunov), and others who explained new ideas developed by Western schools (e. g. Zolotarev and Posse). But there were no St Petersburg mathematicians who themselves tried to adopt these new concepts into the theories following from problems solved by Chebyshev and his pupils.

So Chebyshev's theory had no chance to get a deep foundation at its birthplace. Consequently, this was laid elsewhere - and in this way Euler did not have a deep impact on his successors in St Petersburg.

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Leonhard Euler and the Wave Theory of Light

Roderick W. Home

In his *Nova theoria lucis et colorum* (1746), Euler presented a systematic critique of Newton's projectile theory of light, according to which rays of light consisted of streams of rapidly moving corpuscles ejected by the luminous source [2]. He argued instead that light is a disturbance transmitted through an all-encompassing subtle and elastic aether in a manner closely analogous to the manner in which sound is propagated through air. Contrary to a widely accepted picture of 18th-century optics, according to which Euler was virtually alone in opposing Newton's views, his ideas won a significant measure of support, especially in Germany. That support persisted for several decades until, in the late 1780s or early 1790s, the discovery that light played a role in various chemical processes for a time persuaded virtually everybody that light was not a disturbance in the aether but a material substance with its own particular set of chemical affinities [6]. However, those who, prior to this, took Euler's discussion seriously, focused almost exclusively on the qualitative arguments presented in the opening chapter of his treatise, and ignored the technical details of the wave theory that he developed in later chapters of the work.

Euler's ideas were a distinct advance over those of earlier upholders of transmission theories. In particular, whereas Christiaan Huygens, for example, in his famous *Traité de la lumière* (1690) held that the propagation of light amounted to the transmission through the aether of an irregular series of impulses imparted by random agitations in the luminous source [7], Euler had a clear conception of both light and sound being propagated as regular successions of pulses – that is, as waves. Frequency was thus a well-formed concept for him, whereas it had not been for Huygens. In the case of light, Euler immediately linked different frequencies of vibration with the different colours that we observe, and he attempted on this basis (albeit with only limited success) to construct a general theory of dispersion and the colours of bodies. However, Euler's theory was also a child of its time and fell short in various ways of the fully-fledged wave theory of light that became established in the 19th century. Above all, Euler lacked the principle of superposition that was exploited so fruitfully by Thomas Young and Augustin Fresnel during the first decades of the 19th century as the basis of a comprehensive theory of interference.

In a paper that Euler presented to the Berlin Academy of Sciences in 1744 [1], he represented the aether, as Malebranche had done [8], as a close-packed array of elastic globules, and light as a series of pressure impulses propagated through this. By 1746, he had abandoned this idea and now pictured wave propagation, whether in aether or air, in the same way as Newton had done, in terms of elements of the medium being physically displaced and set oscillating by vibrating particles in the luminous or sonorous body. This image remained with him for the rest of his life. With it as a starting point, he was able to derive, as Newton had done [9], an expression linking the speed of propagation of impulses with the density and 'elastic force' of the medium. Unfortunately, in the case of sound, where the density and elastic force were both known, the computed value of the speed of propagation differed by approximately 10% from the experimentally determined value, suggesting that there was something wrong with the theory. Newton at this point fudged the figures [10]. Euler, however, attributed the discrepancy to approximations he had had to make in the course of the derivation, that had the effect, he thought, of assuming too small a value for the elastic force. The result left him somewhat dissatisfied with the theory he had developed: 'I am as little satisfied as you', he told the Genevan mathematician Gabriel Cramer, 'with the way in which I explain the propagation of this trembling, and I am persuaded that we still lack some principles of mechanics that are needed for this' [3].

One of Newton's strongest arguments against any transmission theory of light was that any disturbance propagating through a medium would spread into the space behind any barrier placed in the beam, so that shadows could never be formed. In a famous diagram in Book II of his *Principia*, he showed a wave striking a barrier with a small hole in it, with the hole acting as an effective source of waves spreading in all directions in the space beyond the barrier [9, 763]. Anyone advocating a transmission theory of light had to rebut this striking image. Euler's response was to deny the validity of Newton's construction, on the basis of his conception of the way a wave propagated. As already indicated, he envisaged a wave as a disturbance in which elements of the medium were displaced and set oscillating. All such displacements, he insisted, were necessarily in a definite direction, so that, even though light was emitted in all directions from a source, in any direction that one cared to consider, its propagation was rectilinear.

The argument amounts to a rejection of the famous principle enunciated by Huygens, according to which each point in an advancing wave front functions as the source of a new wave. Euler never cited Huygens's work, though he must have known of it either directly or through his friend Johann II Bernoulli. He was not alone, however, in rejecting the principle, which found almost no support until Fresnel made it a central feature after 1816 of his mature theory.

Soon after Euler's *Nova theoria* was published, d'Alembert arrived at and then solved the wave equation for a vibrating string, setting the scene for a vigorous dispute between him and Euler over what kinds of functions were acceptable as solutions to the equation. Then in 1759, Euler arrived at and solved the three-dimensional wave equation [4], giving him the mathematics required to develop a full-blown theory of the propagation of disturbances through a medium. Yet he never did this, despite his unhappiness with the theory of propagation that he had developed earlier. He arrived at the three-dimensional equation while reconsidering the theory of sound, in an attempt to remove the approximations that he blamed for the discrepancy between the calculated and experimentally determined values for the speed of propagation. However, he restricted his discussion to recalculating the speed, without ever developing a general theory of sound. Perhaps he was discouraged from developing the theory further when he found that, even using his new equation, the discrepancy in the speeds remained. (Laplace later showed that it was due to heating of the air as it was compressed as the wave passed through it.) Euler never brought his new mathematics to bear on the theory of light. He published no new papers on this subject, and in his popular-level *Letters to a German Princess*, written in the early 1760s and published in 1768, he presented the same ideas as he had set out in the *Nova theoria*, without so much as a hint at the new possibilities that had since opened up for developing a more adequate mathematical account [5].

Thus so far as the technical development of a wave theory of light was concerned, Euler never advanced beyond the point he reached in 1746. He continued to think of the underlying mechanism of wave propagation in an essentially linear way – one that was still apparent, decades later, as a focus on rays rather than waves in Young's work and even in Fresnel's first papers. It seems that the new mathematical techniques that Euler developed in no way shook his faith in the correctness of his physical picture of what happened during the propagation of a disturbance. Indeed, that picture was critically important to him, for it gave him a plausible response to Newton's otherwise overwhelming argument. However, it also placed significant constraints on the way in which Euler developed (or failed to develop) a comprehensive mathematical theory of light. Here as elsewhere in the history of science, developing the necessary mathematical tools is only part of the story. To arrive at a satisfactory theory, the mathematics must be linked to appropriate physical conceptions.

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Electricity and magnetism

Patricia Radelet-de Grave

The talk fell into three parts corresponding to three parts of the *Lettres à une princesse d'Allemagne*:

Electricity: letters CXXXVIII- CLIV;
Longitudes: letters CLV-CLXXV;
Magnetism: letters CLVI-CLXXXVI

I presented first the prehistory of those subjects, then I explained them the way Euler does it in the *Lettres* and in his publications. Finally I reviewed the influence of Euler's ideas in these domains.

At the time people did not think of an interaction between electricity and magnetism even though scholars such as Gilbert (1600) and Aepinus (1759) compared electrical and magnetic attraction and repulsion. Only Œrstedt, in 1820, will observe an interaction.

1. Electricity

Euler's model for electricity makes use of an elastic fluid, the ether, which is also responsible for optical phenomena and whose motions, due to internal disequilibrium of the fluid, explain the electrical phenomena. In the first letter Euler presents his model, and the later letters on electricity are dedicated to the explanation of the various experiments known at the time. His model was not mathematical.

2. The problem of longitudes

Following Gerhardt Mercator's suggestion made in 1551, Euler proposes to solve the problem of longitudes in a very original but unrealisable manner, using the declination compass. Euler, and also Daniel Bernoulli, want to use not only the measure of the declination but also that of inclination. For that purpose Euler studies the best way to build an inclination compass, and examines theoretically the curves of equal declination, traced on a map of the world by Halley for 1700, using a very great number of observations.

Posterity The theoretical study of the curves of equal declination was pursued by Christoph Hansteen in 1819; but only Gauss succeeded in solving it, having potential theory at his disposal.

3. Magnetism

Euler's model for magnetism elaborates a model given by Descartes in his *Principes de Philosophie* (1644). He also makes use of an elastic fluid, more subtle than the ether that forms vortices around magnets. With the help of those vortices, he explains attraction and repulsion between magnets as well as all other magnetic phenomena known to him. But the aim of Euler, and also of Daniel Bernoulli, was to adjoin the mathematical laws of mechanics given by Newton to the model given by Descartes to obtain a mathematical theory of magnetism.

As in Home's survey of Euler's optics, we see him here struggling with all experimental results he knows about the subject, here electricity and magnetism, in order to let the model he is building fit them. In addition, the various models he builds for electricity, magnetism, optics, and so on, should fit into the unifying structure that he gave in his *Anleitung zur Naturlehre.* This unifying structure supplied the key to the mathematisation of nature by the way of his equations for the motions of fluids.

Euler's influence on Faraday and Riemann. The influence of Euler on Faraday was very important, principally through the *Lettres à une Princesse d'Allemagne* that are quoted many times in Faraday's *Experimental researches.* It helped him elaborating his crucial notion of lines of forces.

Riemann acknowledged for Euler's influence in the following way: *My most important work concerns a new conception of the known laws of nature – their expression by means of other fundamental principles – whereby it became possible to use the experimental data on the interaction between heat, light, magnetism, and electricity for the investigation of the connection between them. Towards this I was led mainly by the study of the works of Newton, Euler, and – on the other side – Herbart.*

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Euler's influence on algebra and number theory until ca. 1830

Olaf Neumann

As to algebra the speaker's main contribution to the Workshop is a new evaluation of Vandermonde's (1735-1796) pioneering work on algebraic equations [11] which had made essential use of Euler's approach to the equations $x^n - 1 = 0$ via the so-called "reciprocal equations" ([7]). It should be emphasized that from Vandermonde's theory there could have been deduced that the regular 17-gon be constructible by ruler and compass (without anticipating Gauss's approach!) but this possibility has gone unnoticed ([7], [8]). As late as 1912 the British author Chepmell published a construction of the regular 17-gon which as a matter of fact rests on Vandermonde's ideas at an obscure place, but, apparently, Chepmell was ignorant of Vandermonde's work ([10]).

Euler's achievements in number theory can be best described as "prototheories" and conjectures based on extensive numerical calculations and sound (sometimes hypothetical) arguments. In contrast to Weil's (1906-1998) excellent overview of these results [12] the present talk keeps Euler's analytical methods out of consideration. Euler's major discovery of the reciprocity law of quadratic residues was couched in the observation, which goes back to 1744, that the prime divisors of $x^2 + Ny^2$ for coprime x, y lie in special arithmetic progressions with difference $4|N|$ (cf. [12], pp. 210-219, 287-291). This discovery was acknowledged without reservations only many decades later by Chebyshev (1821-1894) (1849), H. J. St. Smith (1826-1883) (1859) and Kronecker (1823-1891) (1875). As far as we know Legendre (1752-1833) (1785, see also [6]) and Gauss (1777-1855) (1796, see [3]) came to their re-discovery of the reciprocity law independently of Euler and of each other.

Euler's direct influence can be felt strongly with Lagrange (1736-1813) [4]. He was the one who expanded Euler's fruitful use of quadratic and further irrationalities ([1], 2nd part, §§169-201) into an extensive theory of binary quadratic forms including some special cases of the "composition of forms". But with Lagrange and Legendre [6] this composition did not, in general, induce a uniquely determined "product" of Lagrange's classes of forms, see [12], pp. 333-335. Those intrinsic difficulties were overcome by Gauss [3] who created a complete and perfect theory of binary quadratic forms without the use of quadratic irrationalities. This theory provides also space for Euler's highly original results on his "idoneal numbers" ([3], arts. 303-304; [12], pp. 219-226). Likewise Gauss's book [3] completely absorbed and superseded Euler's results in elementary multiplicative number theory, in particular, the existence of primitive roots modulo a prime (as conjectured by Euler) and the basic facts on power residues. In this regard, on the one hand Euler had been a pioneer of research into a new branch of mathematics, and on the other hand he had been a figure of transition. Thus after 1801 for the first three decades further progress in number theory was made only by a few people, mainly more under Gauss's influence than under Euler's and first of all in France. Here Sophie Germain (1776-1831), pen-friend of Gauss, and Cauchy (1789-1857) should be mentioned. In 1815 Cauchy proved that every positive integer is a sum of $m+2$ so-called $(m+2)$-gonal numbers among them there are at least $(m-2)$ summands equal to 0 or 1 ($m \geq 2$). This result is a bit more precise than an old assertion made by Fermat, and it generalizes Lagrange's theorem for $m = 2$ ("four squares theorem").

A peculiar and spectacular line of number-theoretical research has its origin without any doubt in Euler's (incomplete) proof that the equation $x^3 + y^3 = z^3$ has no solutions in rational integers x, y, z with $xyz \neq 0$ ([1], 2nd part, §§242-243). It is well-known that Fermat (1608-1665) had claimed to have a proof that $x^n + y^n = z^n$ for $n > 2$ would have no solutions in rational integers x, y, z with $xyz \neq 0$ ("Fermat's Last Theorem"). Basically, the proof in the case $n = 4$ is due to Fermat himself. Legendre reproduced Euler's proof for $n = 3$ in his book [6] with minor ameliorations. Besides in 1966 Bergmann (b. 1910) realized that further results of Euler himself make Euler's proof of $x^3 + y^3 \neq z^3$ complete (see [9]). In 1815 the French *Académie des sciences* offered a reward for the proof of "Fermat's Last Theorem" but for many years there was only one (unsuccessful) response to this offer. Some progress on "Fermat's Last Theorem" was due to Sophie Germain (published 1830 in the 3rd edition of [6]) and Abel (1802-1829) (1823). Eventually, in 1823, 1825 and 1828 Legendre and Dirichlet (1805-1859) succeeded to prove Fermat's conjecture in the case $n = 5$. By the way, this result made Dirichlet a widely renowned mathematician who soon afterwards started his career in Prussia.

We are entitled to call Euler the founding father of the Russian school of number theory. After his death in 1783 the Academy of St. Petersburg continued to publish his papers on number theory. Eulerian themes of research were taken up by several mathematicians in Russia. Among them the first outstanding figure was Viktor Ya. Bunyakovskij (1804-1889) who in the 1830's began to publish number-theoretical papers.

Last but not least the speaker indicates a further line of research which had received strong impulses from Euler. This is the search for rational numbers or integers x, y satisfying an (irreducible) equation $f(x, y) = 0$ of third or second degree with integer coefficients. Euler gave complete solutions in many cases by means of clever transformations of f and ingenious methods to obtain further solutions from one or several given ones. His methods were generalized by Lagrange (1777), Cauchy (1826) and Jacobi (1804-1851) (1835) to quite general algebraic formulas which were linked to elliptic and abelian functions by Jacobi. Eventually, in the 19th century those formulas were interpreted in terms of the projective geometry of rational or elliptic curves which gave rise to the "Diophantine geometry" ([5]).

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Euler's Legacy in Probability and Statistics

Robert E. Bradley

Euler is not generally considered a major figure in the history of probability and statistics. Todhunter characterized his work as follows: "although his contributions to the Theory of Probability relate to subjects of comparatively small importance, yet they will be found not unworthy of his own great powers and fame." [6, p. 239] Euler variously addressed problems in probability theory, mathematical statistics and actuarial science. In this talk, we restrict our attention to the first two of these areas, although an assessment of Euler's influence on the development of mortality and annuity calculations would also make for interesting historical inquiry.

As an example of Euler's work in probability theory, we consider E201, "Calcul de la probabilité dans le jeu de Rencontre." In this paper, Euler adressed the problem of derangements: given a permutation of n objects, a "*rencontre*" or "coincidence" occurs when a permuted object remains in its original place. A derangement is a permutation with no coincidences. Euler derived various recurrence relations involving the number of objects permuted and the location of the first coincidence, ultimately showing that the probability that a randomly selected permutation is a derangement converges quickly to e^{-1} as n increases.

Lambert [2] was inspired by E201 to apply this theory to a study of the reliability of weather predictions in German almanacs. Laplace discussed coincidences in some detail [4], but it is not clear to what extent he was influenced by Euler, since the problem and original solution dates back to Montmort, of whose work Euler was apparently unaware. Euler returned to the problem in E738, "Solutio quaestionis curiosae ex doctrina combinationum," written in about 1779, but published only in 1811. In this combinatorial paper, classified as "recreational mathematics" in Euler's *Opera Omnia*, he gave an elegant derivation of the recurrence relation at the heart of this problem.

Euler wrote five papers on lotteries, including two works not intended for publication. We consider E338, "Sur la probabilité des sequences dans la Loterie génoise." This paper concerned a combinatorial problem arising from the type of lottery that was popular all over Europe at the time, which had been introduced in Berlin in 1763. Euler's memoir inspired two related papers by Beguelin, which appeared in the same volume of the *Mémoires* of the Berlin Academy for 1765 and another by Johann (III) Bernoulli in the volume for 1769. More importantly, the paper was an application of Euler's theory of partitions of natural numbers, which inspired a great deal of mathematical activity in the late 18th and 19th centuries; see [1, vol. II, ch. III] for a chronology.

Euler considered the St. Petersburg problem in his posthumously published paper E811. Sandifer has argued convincingly that the paper was written in 1730 or 1731, but withheld from publication in favor of Daniel Bernoulli's paper "Specimen theoriae novae de mensura sortis," published in volume 5 of the *Commentarii* of the St. Petersburg Academy. Like Bernoulli, Euler argued in favor of a utility function based on geometric means, rather than the usual weighted arithmetic means, in order to assess the value of this game with infinite expectation. In the 1770s, Buffon, d'Alembert and Condorcet were all critical of Bernoulli's (and hence Euler's) resolution of the St. Petersburg paradox. However, Laplace discussed the utility function in [4] and it is a vital topic in modern economic theory.

Euler considered the statistical problem of observational errors in E488. This paper was a commentary on a paper by Daniel Bernoulli, which preceded it in the same volume of the *Acta* of the St. Petersburg Academy for 1777. Essentially, Bernoulli proposed the unimodal maximum likelihood estimator for determining the best estimate of a parameter, given a set of observations. Euler countered with an estimator that is equivalent to the modern M-estimator and involves an expression similar to least squares; see [5, p. 302-319]. Commentators, including Todhunter [6, p. 238], Sheynin and Pearson (see [5] for references) have been critical of both E488 and Bernoulli's paper, but we should instead consider the context: a time when modern statistical concepts had not yet been formulated.

Finally, we consider Euler's influence on the subsequent development of statistical methods through his astronomical papers, particularly E120 and E389. E120 won the Paris Prize for 1748, addressing the irregularities in the orbits of Jupiter and Saturn, and consequently the much larger issue of the stability of the solar system. E120 contained important analytical innovations, but the problem of fitting observed data to the model he had developed proved insurmountable to Euler. In attempting to determine the values of seven parameters given more than seventy disparate sets of observational data, he unsuccessfully applied a method now referred to as the Method of Averages. Mayer solved a similar problem related to the lunar orbit with greater success in 1750, using data from his own observations. Euler's difficulties reflect the prevailing opinion in his time that aggregating data increases the effect of observational error, whereas the actual statistical effect is in the opposite direction. In 1785, Laplace finally solved the problem of determining the inequalities in the orbits of Jupiter and Saturn using the Method of Averages [3]. Euler's contributions to data fitting and maximum likelihood estimators were among the elements in the intellectual landscape that led to the development of least squares in the early 19th century.

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Euler's *Lettres à une princesse d'Allemagne*

Wolfgang Breidert

The assessements of the *Lettres à une princesse d'Allemagne* [E 343, 344, 417] differ greatly. For instance, Friedrich Christian Kries [1], considering the philosophical letters unimportant and obsolete, neglected these totally, whereas Andreas Speiser [2] had a high opinion of Euler's philosophy with regard to classical German philosophy, especially to Kant.

Euler did not pretend to be a philosopher, because he disdained the pretentious philosophers. And his contemporaries scarcely esteemed him as a philosopher. There is no peculiarly 'Eulerian' philosophical question, but, so to speak, an external motivation for his concern with 'false' philosophical doctrines. His philosophical position is motivated by his religious belief, which was threatened by materialism, deism, determinism, solipsism, and the Wolffian monadology including questions about infinite divisibility and the concept of body. In the *Lettres* even his physical explanations are mixed with religious remarks: e. g. the astronomical distances force the admiration of God (letter no. 21), and the structure of the eye convinces us of the omnipotence (letter no. 41). However, Euler rejected the argumentative appeal to omnipotence in his criticism of the Newtonian attractionists.

Euler held a realistic dualism concerning body and mind, but he conceded a separate status for space. In this respect he paved the way for Kant's theory of space. Contrary to Kant, Euler avowed that he is unable to refute idealism, but he maintained that corporeal reality is independent from the thinking mind. Against determinism Euler maintained dogmatically the freedom of will, because no man could be mistaken with regard to his own freedom (letter no. 91).

In mathematics Euler admitted infinitely small or large quantities, but he confessed that they cannot be applied to real things. In practice he crossed this gap which he could not bridge in theory.

In spite of his famous clearness in calculation Euler seems to have had no deep demand to treat philosophical problems, even not in mathematics. For instance, his explanations of negative and imaginary numbers in *Vollständige Anleitung zur Algebra* [E 387, § 31, §§ 141-145] are insufficient from the logical point of view. Appealing to his 'remarkably sure intuition' or his mathematical 'sensitivity' seems a poor excuse for his reckless divisions by zero and for his careless use of infinitely small or infinitely large quantities. It is obvious by the example of George Berkeley's *Analyst* (1734) that it was possible at least to endeavour a rigorous foundation of the calculus in the first half of the 18th century.

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On the reception of Euler by the French, 1780s-1830s

Ivor Grattan-Guinness

Jean d'Alembert, Daniel Bernoulli and Leonhard Euler died within 18 months of each other in 1782 and 1783, and the mathematical inheritance came to their principal successors. The French dominated, especially with C. Bossut, G. Monge, P. S. Laplace and A. M. Legendre, and from 1787 J. L. Lagrange on his move to Paris. The next generations produced many more major figures [1]. The influence of Euler was considerable but not preponderant [2], an impression conveyed already by earlier abstracts in this report.

On the calculus, very prominent was the differential and integral theory set out by Leibniz and modified by Euler with his addition of the differential coefficient, the forerunner of our derivative, especially in applications. However, Lagrange's version, reducing the calculus to a branch of algebra by assuming that a mathematical function $f(x+h)$ could always be expanded as a power series in h, with the 'derived functions' defined from the coefficients of h, gained some serious attention. In both theories the integral was specified as some sort of inverse of the differential coefficient or derivative.

Concerning ordinary and partial differential equations, the general theory of solutions was developed, and also many particular methods were found to solve many specific equations. These achievements, especially the second, led to quite a wide range of special functions and infinite series; they also encouraged the theory of polynomial and other equations, in particular properties of their roots. Euler and Lagrange were comparably significant. However, while Euler had made important innovations to the 'calculus of variations' (his name), its generality and algebraic formulation owed most to Lagrange.

From the 1820s this competition between Euler and Lagrange was made more complicated by a new approach to mathematical analysis in general proposed by A. L. Cauchy. Grounded upon a developed theory of limits of infinite sequences of values, he defined the derivative as the limit of the difference quotient and the integral as the limit of a sequence of partition sums, and he allowed in both cases for the possibility that the limit did not in fact exist. In addition, in 1822 he refuted Lagrange's belief in the universality of the Taylor expansion.

In mechanics Euler had been a major figure in the Newtonian version, but he had also advocated the generality of the principle of least action. Moreover, he appealed to God as the guarantee of the source of this generality, and also for the assurance that the planetary system would always be stable. The clash with Lagrange is very direct here, for Lagrange used the principle as an important part of his 'analytical mechanics' in which Newton's laws were theorems rather than assumptions. Further, in a wonderful analysis he attempted to prove the stability of the planetary system; but here we see also positive influence from Euler, for Lagrange based his analysis on Euler's expansion of the principal astronomical variables in infinite trigonometric series, which was long to remain a staple technique in celestial mechanics. Both men developed different theories in branches of continuum mechanics, especially in elasticity theory and fluid mechanics; both received French followers.

In one area of mechanics the roles of these two giants were very different. Lagrange paid little attention to engineering, whereas Euler wrote quite extensively on it. However, his influence was very small among the French, for whom engineering and technology were important sectors of scientific activity. Maybe his limited use of energy/work mechanics detracted followers, who would appeal more to, for example, Daniel Bernoulli or the engineer C. A. Coulomb; but the quietude remains surprising. Striking is the prominent engineer G. Riche de Prony, who openly admired Euler's work in mechanics in general but did not draw much on the writings on engineering.

The contrast between Euler and Lagrange is noticeable in the work of Laplace, who as a mathematician stands on a level comparable with both of them, especially after the publication of his *Traité de mécanique céleste* (first four volumes 1799-1805). His calculus and mathematical analysis was usually Eulerian but celestial mechanics normally Lagrangian, for example in studying the stability problem (but also by using Euler's expansion in trigonometric series). Further, in his book he presented lunar theory following the method used by d'Alembert.

Some of the new generation of French mathematicians active from 1800 onwards were largely responsible for the inauguration of mathematical physics. On heat diffusion (1807 onwards) J. B. J. Fourier used Euler's calculus to find the differential equation but his own trigonometric series method for their solution. (This method is not to be confused with Euler's in celestial mechanics.) On physical optics Laplace advocated from 1805 a Newton-like ballistic theory, and then A. J. Fresnel did not draw upon Euler's wave theory when developing his own much more sophisticated version of it from 1815 onwards. In electricity and magnetism (and for A. M. Ampère after 1820, their connections) Coulomb was much more important a past figure than Euler.

A final respect in which Euler's influence seems to have been very modest, in all topics and with successors of all nationalities, is the huge mass of his posthumous papers: 180 published by the Saint Petersburg Academy until 1830, and 100 in book collections up to 1862. Most of these works seems to have made little impact, though they were not necessarily unread. One significant exception is his paper on the linearity properties of the torque of a mechanical system (E659, 1793), which Laplace used as the basis of a theory of the invariable plane of the planetary system. Another case is a paper (E704, 1798) in which Euler found the Fourier series (without, it seems, recognising its mathematical significance): Fourier learnt of the paper only when it was pointed out to him by the textbook writer S.F. Lacroix, whose knowledge of Euler's work was probably the most detailed among his French successors.

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