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The History of Differential Equations, 1670–1950

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Introduction by the Organisers

Differential equations have been a major branch of pure and applied mathematics since their inauguration in the mid 17th century. While their history has been well studied, it remains a vital field of on-going investigation, with the emergence of new connections with other parts of mathematics, fertile interplay with applied subjects, interesting reformulation of basic problems and theory in various periods, new vistas in the 20th century, and so on. In this meeting we considered some of the principal parts of this story, from the launch with Newton and Leibniz up to around 1950.

'Differential equations' began with Leibniz, the Bernoulli brothers and others from the 1680s, not long after Newton's 'fluxional equations' in the 1670s. Applications were made largely to geometry and mechanics; isoperimetrical problems were exercises in optimisation.

Most 18th-century developments consolidated the Leibnizian tradition, extending its multi-variate form, thus leading to partial differential equations. Generalisation of isoperimetrical problems led to the calculus of variations. New figures appeared, especially Euler, Daniel Bernoulli, Lagrange and Laplace. Development of the general theory of solutions included singular ones, functional solutions and those by infinite series. Many applications were made to mechanics, especially to astronomy and continuous media.

In the 19th century: general theory was enriched by development of the understanding of general and particular solutions, and of existence theorems. More types of equation and their solutions appeared; for example, Fourier analysis and special functions. Among new figures, Cauchy stands out. Applications were now made not only to classical mechanics but also to heat theory, optics, electricity and magnetism, especially with the impact of Maxwell. Later Poincaré introduced recurrence theorems, initially in connection with the three-body problem.

In the 20th century: general theory was influenced by the arrival of set theory in mathematical analysis; with consequences for theorisation, including further topological aspects. New applications were made to quantum mathematics, dynamical systems and relativity theory.

Workshop: The History of Differential Equations, 1670–1950

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Abstracts

The role of the concept of construction in the transition from inverse tangent problems to differential equations.

Henk J. M. Bos

Tangent problems — given a curve, to find its tangents at given points — are as old as classical Greek mathematics. 'Inverse tangent problems' was the name coined in the seventeenth century for problems of the type: given a property of tangents, find a curve whose tangents have that property. It seems that the first such problem was proposed by Florimod De Beaune in 1639 (cf. [5]). Translated into the formalism of the calculus these problems become differential equations. Much of the activities in the early infinitesimal calculus (second half of the seventeenth century) were motivated by inverse tangent problems, many of them suggested by the new mechanical theory.

The transition to differential equations occurred around 1700. I argue that this transition was much more than a simple translation from figure to formula, from geometry to analytical formalism. It involved, indeed it was a major factor in, the loss of a canon for the solution of problems. In the seventeenth century this canon had been formulated by René Descartes in a redefinition of what it meant to *solve* a problem in geometry cf. [3]. It meant to *construct* the geometrical entity — for inverse tangent problems, the curve — which was required in the problem. Descartes had restricted geometry to algebraic curves, and he had explained how such curves could be constructed (cf. [3, pp. 342–346, 374–375]). But inverse tangent problems often had non-algebraic curves as solution. Consequently mathematicians went outside the Cartesian demarcation of geometry, but they kept to the requirement that if problems required to find some curve, this meant that the curve had to be constructed. There were many methods for constructing curves. For transcendental curves these constructions necessarily involved a transcendental step, mostly in the form of a quadrature which was simply assumed to be possible. There was also a lively discussion on the merits of constructing transcendental curves by instruments (cf. [1] and [2], see also the contribution of Prof. Tournès to the meeting).

I use Charles René Reyneau's (1656–1728) *Analyse démontrée* of 1708 ([4]) to illustrate the persistence of this canon of construction. Reyneau explains all the necessary geometrical procedures:

(1) to construct a curve when its (algebraic) equation is given,
(2) to construct the roots of an equation $F(x) = 0$,
(3) to construct a curve by assuming certain quadratures given ([4, pp. 571, 601, 744–745, respectively]).

The passages on these procedures in Reyneau's book may be seen as examples of a mathematical way of thinking in the process of *fossilization*. Later in the eighteenth century, one finds its traces mostly in terminology: solving differential equations was called, throughout the eighteenth century ‘construction of differential equations.’

By this loss of a canon of construction, however, mathematicians also lost a clear and shared conception of what it meant to solve a differential equation; indeed, the status of differential equations became fuzzy: were they problems? were they objects? When were their solutions satisfactory?

The change from a field with a more or less commonly accepted view on what were the status of the object and the requirements of solutions, to a field in which these issues were fuzzy (and in which at the same time the material for study expanded enormously) was not an easy one. Many puzzling developments in early analysis, and especially delays in developments expected with hindsight, can be explained by the tenacity of the older ideas on problem solving. Indeed adjustment to the new situation meant *habituation* of mathematicians to changes. And habituation takes time.

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Gabriele Manfredi's treatise *De constructione aequationum differentialium primi gradus* (1707)

Clara Silvia Roero

Gabriele Manfredi's book on first-degree differential equations, written between 1701 and 1704 and published in 1707, is the most valuable Italian mathematical treatise of the first twenty years of the 18th century. The structure of the propositions and of the geometrical constructions presented in this work is reminiscent of those used by the Bernoulli brothers and by Leibniz in their own writings. This was certainly because Manfredi's formation had been based on the articles of the *Acta Eruditorum* and on L'Hôpital's *Analyse*, the latter of which was his model in deciding to supply his readers with a systematic collection of the Leibnizian methods for the calculation of integrals and the solution of differential equations, complete with proofs of those points not explicated by their authors.

The aim of this historical study on Manfredi's treatise is to show the sources of inspiration and the genesis of G. Manfredi's work and the favourable impressions that it made abroad, as well as the influence it exerted on Italian mathematical research with respect to the topic of differential equations.

Manfredi's work is divided into six sections, systematically organised with a succession of definitions, propositions, corollaries and examples. He shows, in the first section, how the differential properties of a curve, linked for example to the tangents, normals, radii of curvature, arc lengths, areas enclosed by the curve or volumes of solids of revolution, lead to the first-order differential equation verified by the curve. The second section deals with the problem of the integration of equations such as $A(x)\,dx = B(y)\,dy$ whose integrals are algebraic curves. Differential equations such as $dz = q\,du$, whose solutions are not algebraic curves, are examined in the third section. The fourth section deals with first-order differential equations, non-linear in the differentials and such that the sum of the degrees of the differentials, in each addend, are constant and that only one of the variables appears. The fifth section deals with the construction of differential equations with separable variables $q(t)\,dt = p(u)\,du$, which are not algebraically integrable. The sixth section, certainly the most interesting and original, is devoted to the study of some classes of differential equations which are not algebraically integrable, in which both the variables appear but are not separable.

Manfredi first shows how certain devices lead, in both sides, to exact differentials and then goes on to consider homogeneous differential equations, which he admits he does not know how to integrate with a general procedure, nor how to separate the variables. Specifically, regarding the equation $nx^2\,dx - ny^2\,dx + x^2\,dy = xy\,dx$, Manfredi affirms (Manfredi 1707, p. 167): "...it is not clear how this equation can be constructed, nor do we see how it is possible to integrate it, nor how it is possible to separate the variables from each other." It is relevant here to recall that a few years later, he found the general substitution to be used in these cases, and this method was published in the *Giornale de' letterati d'Italia* in 1714. It is only for the specific example $x^2\,dy = ny\,dx\sqrt{x^2 - y^2}$ that in his treatise Manfredi devised an artifice which allowed him to reach the solution. The most important result in this last section is the determination of the general solution of the first-order linear equation $a^2\,dy = bq\,dx + py\,dx$, where p, q are functions of x and a, b are constants. In order to construct the solution of this equation Manfredi first uses the substitution $\frac{p\,dx}{a} = \frac{a\,dz}{z}$. Manfredi then examines some examples of differential equations which can be reduced to linear equations and at the end of his treatise he deals with a problem of orthogonal trajectories which leads to a homogeneous differential equation, whose solution is found by reducing it to a linear equation.

As this summary shows, Manfredi's book was naturally intended for a select readership of Italian scholars already able to understand differential calculus, and eager to get to grips with integral calculus. Many of the cases examined by Manfredi also appeared in the *Lectiones* on integral calculus which Johann Bernoulli had prepared for L'Hôpital when he was in Paris in 1691-92, but which he did not publish until 1742 [Bernoulli Joh. *Opera* 3, pp. 385-558]. But it must not be supposed that Manfredi had seen these manuscripts, because they were not circulating in Italy at the time Manfredi was writing his treatise. It is also true to say that the sources of inspiration for both texts are the same articles published in the *Acta Eruditorum.* This is also what Manfredi wrote to Leibniz when he sent his book: (Bologna, 3.10.1707, NLB Hanover MS Lbr. 599, f. 1r): "I think you will recognise, as soon as you read [this book], that it is almost all taken from you and your brilliant expositions in the *Acta Eruditorum.* The fact is that without you, no one can make progress in advanced geometry, so useful and numerous are your inventions. So this little book I am sending you must in truth be recognised as yours, and, as it is yours, I should like to recommend it warmly to your benevolence. It was written precisely to allow beginners, especially Italians, to understand integral Calculus; the ignorance and lack of interest towards this subject in Italy is, in fact, abysmal and shameful."

The review of *De constructione* published in the *Acta Eruditorum* in June 1708 was written by Leibniz and Wolff, who had been favourably impressed by the young Italian. There they praise Manfredi for making a good choice of examples from the publications of Leibniz and the Bernoullis to illustrate integral calculus and they stress that his book goes far beyond Carré's little book of 1700, *Méthode pour la mesure des surfaces . . . par l'application du Calcul intégral.* Jacob Hermann, who taught in Padua at that time, wrote to Leibniz that Manfredi's treatise had made a very favourable impression on him, mentioning in particular a problem of orthogonal trajectories, which, in his opinion, had been admirably solved (Padua 13. 10. 1707, GM 4, p. 321): "Some time ago the famous Manfredi sent me, through our honoured Guglielmini who had gone to Bologna from here, his treatise on the construction of first-order differential equations. The aim of this work is to clarify and eliminate doubts regarding the principal inventions concerning the inverse tangent method which appeared in the *Acta Eruditorum* and the Proceedings of the Paris Academy without proof, and in my opinion he has been successful in many cases." In letters to Johann Bernoulli (Padua 19.10.1707, 8.12.1708) Hermann also expressed his preference for Manfredi's treatise, which he had skimmed through rapidly, to the Englishman Cheyne's *Fluxionum methodus inversa* (1703) and to Reyneau's *Analyse démontrée* (1708). In contrast however, Johann Bernoulli did not recognise any degree of originality in Manfredi's book, nor in the methods he had adopted; moreover he wrote to Leibniz (Basle 1.9.1708, GM 3, p. 838): "[Verzaglia] has brought me Manfredi's book *De constructione aequationum differentialium primi gradus*, published a year ago, and so certainly in your hands by now. There are some elegant things in it, but in many cases it is far too prolix and to tell the truth it omits other more necessary and useful things. He has not gone deeply enough into the study of the integration [of differential equations] and the relevant construction, which was, however, his principal aim." Perhaps this work precedeed his wish to publish his lessons on integral calculus for L'Hôpital. Nevertheless, the book was appreciated by Pierre Rémond de Montmort, who wrote to Johann Bernoulli on 15 September 1709: "I have recently received Manfredi's book. It is very good, but it does not prevent us from greatly regretting all that you could have taught us if you had deigned to take the trouble."

Manfredi's book is of great significance in the context of mathematics in the early 18th century, both because of the high level of the investigations in the field of differential equations, and because of the scarcity of books and articles published before that time. The reviews published in the Italian journals *Galleria di Minerva* and *Giornale de' Letterati d'Italia* were very favourable. Yet, despite its validity, or perhaps precisely because it was so much in the vanguard of mathematical research, it did not make an immediate impact on Italian mathematics. The reasons for this are well known: in the first place, the difficulty of the subject for a public which was still unfamiliar with modern mathematics and little accustomed to algebra and Cartesian geometry, much less differential and integral calculus. In the second place, the book was badly printed and full of misprints, as it had been published cheaply at the author's own expense. But those who already knew something of Leibnizian calculus appreciated its usefulness for the direction and development of mathematical research. For distinguished mathematicians such as Jacopo Riccati, Bernardino Zendrini and Giovanni Poleni, it was the basic reference text in their early studies on integral calculus. Especially for Riccati and Zendrini, Manfredi's work was in effect a springboard for the development of new methods and techniques for dealing with differential equations. And Manfredi was to remain a central figure in future research by Italian mathematicians. Among his students in Bologna were Ramiro Rampinelli, Laura Bassi, Flaminio Scarselli, Francesco Maria Zanotti, Giuseppe Antonio Nadi and Sebastiano Canterzani. In particular Rampinelli and his student Maria Gaetana Agnesi greatly benefited from the skill and advice of G. Manfredi and J. Riccati, and were able to produce writings whose importance in the popularisation of the most recent and advanced mathematics was also recognised abroad (Agnesi, *Instituzioni analitiche ad uso della gioventù italiana*, 1748).

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Vincenzo Riccati's treatise on integration of differential equations by tractional motion (1752)

Dominique Tournès

In 1752 in Bologna, Vincenzo Riccati published a short treatise in Latin entitled *De usu motus tractorii in constructione aequationum differentialium*. This paper is interesting because it is the only complete theoretical work that was ever dedicated to the use of tractional motion in geometry. The book contains 72 pages of text and three plates at the end of the volume comprising sixteen figures. Why did Vincenzo write this treatise? This is a rather easy question to answer because Riccati himself tells us the origin of his work and the evolution of his ideas. All comes from the reading of a short passage of a paper written by Alexis-Claude Clairaut in 1742, published in 1745 in the *Mémoires de l'Académie royale des sciences de Paris*. In this passage, Clairaut summarizes in a few lines, without demonstration, a result found by Euler in 1736: the integration by tractional motion of a general form of Riccati's differential equation. Surprised by this result, Vincenzo sought to rediscover a demonstration of it. He then developed various generalizations which led him little by little to an unexpected result: by use of tractional motion, it is possible to integrate in an exact way, not only Riccati's equation, but, more generally, any differential equation.

Before I go over the significance of this result, I must stress that Riccati's work is not only theoretical and abstract. Throughout his paper, Vincenzo wonders about the possibility of making material instruments allowing the actual realization of the constructions which he imagines. In fact, Riccati's work occupies a central place in the history of a certain type of mechanical instruments of integration. A tractional instrument is an instrument which plots an integral curve of a differential equation by using tractional motion. On a horizontal plane, one pulls one end of a tense string, or a rigid rod, along a given curve, and the other end of the string, the free end, describes during the motion a new curve which remains constantly tangent to the string. At this free end, one places a pen surmounted by a weight making pressure, or a sharp edged wheel cutting the paper, so that any lateral motion is neutralized. By suitably choosing the base curve along which the end of the string is dragged, and by suitably varying the length of the string according to a given law, one can integrate various types of differential equations. In this way of solving an inverse tangent problem, one actually materializes the tangent by a tense string and moves the string so that the given property of the tangents is verified at every moment. The length of the tangent is controlled at every moment by a mechanical system (a pulley or a slide channel) and by a second curve which is called the directrix of the motion.

Curiously, instruments of this type were considered and made in two different periods, and it seems that there was no link between the two. The first period spans the sixty years from 1692 till 1752. During this time, many mathematicians were interested in tractional motion: Huygens, Leibniz, Johann and Jakob Bernoulli, L'Hôpital, Varignon, Fontenelle, Bomie, Fontaine, Jean-Baptiste

Clairaut and his son Alexis-Claude Clairaut, Maupertuis, and Euler. In Italy, one can quote mainly Giovanni Poleni, Giambatista Suardi and, of course, Vincenzo Riccati. After about 150 years of interruption, during which one finds no trace of tractional motion, a second group of instruments suddenly appears. This is an amazing case of extinction and rebirth of an area of knowledge. The engineers of the end of the nineteenth century and the beginning of the twentieth century actually rediscovered, in an independent way, the same theoretical principles and the same technical solutions as those of the eighteenth century. Later, we see even more complicated tractional instruments, with two cutting wheels connected between them to be able to integrate differential equations of the second order. In certain large differential analysers of the years 1930-1950, up to twelve cutting wheels would be used to integrate large differential systems.

In 1752, Vincenzo Riccati worked out a theory which explains the operation of all these instruments. The theory rests on various generalizations of the concept of tractoria starting from the tractrix, the first tractoria described by Huygens in 1692. Throughout the chapters of the treatise, we find successive generalizations which allow the integration of more and more extended classes of differential equations. First of all, there are the tractorias with constant tangent, described with a string of constant length dragged along a base curve. These tractorias, the only ones considered by Euler in 1736, allow the integration of one half of Riccati's equations. Vincenzo's first idea consists in a simple but essential remark: to say that the tangent is constant means saying that the free end of the string is permanently on a circle of constant radius having for centre the end which one pulls along the base. By replacing the circle by any curve rigidly related to the tractor point and moving with it, we obtain the notion of tractoria with constant directrix. By using these tractorias, Riccati succeeded in integrating the other half of Riccati's equations (the half which had escaped Euler), as well as some other equations. A second idea consists of making the length of the string vary according to the position of the tractor point. This is the notion of tractoria with variable tangent, which amounts taking a circular variable directrix whose centre is always on the tractor point. Finally, the most general notion consists in controlling the length of the string by a variable directrix whose form varies in any way according to the position of the tractor point.

By means of these four types of tractorias, Riccati shows that one can integrate any differential equation exactly by tractional motion, and that there exist an infinity of different constructions. One can always integrate any given equation by using a tractoria with rectilinear base and variable directrix. One can also integrate the same equation with an arbitrary curvilinear base. The problem consists in choosing the base so that the directrix is the simplest one, and if possible a constant one. Indeed, the tractorias with constant directrix are suitable for the manufacture of material instruments. On the other hand, it is more difficult to conceive instruments for tractorias with variable directrix. Of course it is complicated to manufacture a material curve which can change its shape continuously during the motion. Instruments corresponding to this last type were conceived only very rarely, for rather particular equations. Incidentally, when we say that Riccati's method allows the integration of "any differential equation", we must be precise about the meaning of these words. "Any differential equation" means any differential equation conceivable for the time, that is any equation with two independent variables x and y in which the coefficients of the infinitesimal elements dx and dy are expressions formed using only a finite number of algebraic operations and quadratures. Under these conditions, all the auxiliary curves used by Riccati, the base curves and the directrix curves, are constructible by classical means. Tractional motion is then an additional process of construction that allows us to obtain new curves from previously known ones.

From a theoretical point of view, the *De usu motus tractorii* is the outcome of the ancient current of geometrical resolution of problems by the construction of curves. In a certain way, Vincenzo Riccati has put a final point at this current by showing that one could construct by a simple continuous motion all the transcendental curves from the differential equations which define them. From the practical point of view, the treatise of 1752 proposes a very general theoretical model to explain in a unified way the operation of a great number of tractional instruments, those from the past as well as those to come. However, the work of Vincenzo Riccati was neither celebrated nor influential. It was little read and little distributed. The book probably arrived too late, at the end of the time of construction of curves, at the moment when geometry was giving way to algebra, and at the time when series were becoming the principal tool for representing solutions of differential equations. Thus, in spite of its novelty and brilliance, Riccati's work seemed almost immediately old-fashioned.

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Equations différentielles et systèmes différentiels : de d'Alembert à Cauchy

Christian Gilain

I. D'Alembert : Elaboration d'une théorie générale des systèmes différentiels linéaires, à coefficients constants.
1. Le *Traité de dynamique* de 1743.
Dans le problème V de ce traité, d'Alembert étudie les petites oscillations du pendule multiple, en utilisant le principe mécanique qui porte désormais son nom [6, 5]. Il parvient ainsi aux équations du mouvement, qui forment un système d'équations linéaires du second ordre, à coefficients constants, homogènes (en utilisant la terminologie actuelle). Pour l'intégration d'un tel système, il propose une méthode qui consiste à utiliser des coefficients multiplicateurs constants, choisis de telle manière que, en additionnant les équations ainsi obtenues, on puisse se ramener à une seule équation différentielle ordinaire, à deux variables. Cependant, en 1743, l'intégration de cette dernière équation, linéaire du second ordre, par d'Alembert reste compliquée et ne permet pas d'aboutir à des expressions analytiques explicites des solutions.
2. Les "Recherches sur le calcul intégral" de 1745, 1747 et 1752.
Dans une série de trois mémoires d'analyse pure, écrits en 1745, 1747 et 1752 (dont le premier est resté inédit), d'Alembert construit une véritable théorie des systèmes différentiels linéaires, à coefficients constants, homogènes ou non (voir [2]). Cette théorie repose sur deux éléments essentiels : i) la résolution générale directe des systèmes d'équations différentielles linéaires, à coefficients constants, du 1er ordre, à l'aide de la méthode des multiplicateurs ; ii) la réduction de tout système d'équations différentielles linéaires d'ordre supérieur à un système équivalent d'équations du 1er ordre, grâce à l'introduction de coefficients différentiels successifs comme nouvelles variables. Dans cette période, d'Alembert applique sa théorie, ainsi constituée, à divers domaines des mathématiques mixtes, en particulier la mécanique céleste (voir [1]). Dans ses "Additions" de 1752 à ses recherches de calcul intégral, d'Alembert compare sa méthode à celle d'Euler, pour intégrer l'équation différentielle linéaire d'ordre n, à coefficients constants, homogène. Tout en reconnaissant la plus grande simplicité de la méthode d'Euler, il souligne que la sienne est à la fois plus rigoureuse et plus générale, car s'étendant sans difficulté au cas non homogène.

Ainsi, la démarche de d'Alembert consiste à la fois à intégrer directement les systèmes de n équations différentielles linéaires d'ordre un, sans les réduire à une seule équation différentielle d'ordre n par élimination, et à ramener l'intégration d'une équation différentielle linéaire d'ordre n à celle d'un système d'équations différentielles d'ordre un équivalent.
II. Postérité de la théorie de d'Alembert.
Nous nous intéressons à la réception de cette double démarche de d'Alembert, qui semble avoir été peu partagée par ses contemporains, notamment Euler.

1. Lacroix et le cours d'analyse de l'Ecole polytechnique.
Il est intéressant de regarder la place occupée par la théorie de d'Alembert chez Lacroix, bon connaisseur de l'ensemble des travaux du XVIIIe siècle, et professeur d'analyse de Cauchy à l'Ecole polytechnique en 1805-1807 (voir [7]). Le registre d'instruction, où figurent les "Objets des leçons", montre que, dans son cours d'analyse de 2e année où il expose la théorie des équations différentielles, il suit la deuxième édition de son *Traité élémentaire* [8]. Une présentation de la théorie de d'Alembert y figure, avec ses deux éléments, mais de façon marginale, à la fin de la section consacrée aux équations différentielles d'ordre deux ou supérieur. L'ordre du cours de Lacroix, qui correspond à celui du programme officiel de l'Ecole polytechnique, est ainsi : intégration de l'équation différentielle linéaire d'ordre quelconque, à coefficients constants ; puis, intégration des équations linéaires "simultanées".
2. Cauchy et le cours d'analyse de l'Ecole polytechnique.
Quelques années plus tard, Cauchy, devenu professeur d'analyse à l'Ecole polytechnique, non seulement expose la théorie linéaire de d'Alembert, mais il donne un rôle central à la démarche de l'encyclopédiste, en l'étendant aux équations non linéaires pour fonder la théorie classique des équations différentielles générales. Les "Matières des leçons", figurant dans les registres d'instruction, montrent que, à partir de 1817-1818, et jusqu'en 1823-1824, il change l'ordre du cours d'analyse et présente l'étude des équations simultanées du 1er ordre avant celle des équations différentielles d'ordre quelconque. L'architecture de la théorie des équations différentielles figurant dans son cours d'analyse de l'Ecole polytechnique est radicalement modifiée par rapport à ses prédécesseurs. Elle repose essentiellement sur l'ordre suivant : i) équations différentielles "quelconques" du 1er ordre ; ii) systèmes d' équations différentielles "quelconques" du 1er ordre ; iii) systèmes d'équations linéaires du 1er ordre à coefficients constants ; iv) équations différentielles "quelconques" d'ordre n ; v) équations différentielles linéaires d'ordre n ; vi) équations différentielles linéaires d'ordre n à coefficients constants.

Le socle de la nouvelle théorie générale est constitué par les points i) et ii) où sont démontrés les théorèmes d'existence et d'unicité d'une solution du problème "de Cauchy" (voir [3]). Les équations différentielles d'ordre n se ramènent alors aux systèmes d'équations du 1er ordre. En particulier, l'équation différentielle linéaire à coefficients constants est sans doute intégrée, au point vi), en utilisant, notamment, la résolution du système d'équations différentielles linéaires du 1er ordre équivalent, qui est d'un type étudié avant, au point iii) (cf. [9, leçons 33 à 37]). L'ordre de présentation dans le domaine des équations différentielles linéaires apparaît ainsi solidaire de l'ensemble de l'orientation nouvelle donnée par Cauchy au calcul intégral dans son cours d'analyse de l'Ecole polytechnique. C'est ce qu'il a lui-même confirmé, en 1842, dans une "Note sur la nature des problèmes que présente le calcul intégral" [4, p. 267-269].

Cette démarche de d'Alembert et Cauchy de réduction au premier ordre a été poursuivie, plus tard, par la transformation d'un système de n équations numériques du premier ordre en une seule équation vectorielle du premier ordre dans un espace-produit, confirmant l'importance de cette idée donnant la priorité au 1er ordre pour l'organisation de l'ensemble de la théorie des équations différentielles ordinaires.

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Functions, Series and Integration of Differential Equations around 1800

Giovanni Ferraro[1]

In the 18th century a function was given by one analytical expression constructed from variables in a finite number of steps using some basic functions (namely algebraic, trigonometric, exponential and logarithm functions), algebraic operations and composition of functions. Furthermore series were intended as the expansions of functions and not considered as functions in their own right (see Fraser [1989], Ferraro [2000a and 2000b]).

This conception poses the historical problem of the nature of series solutions to differential equations. Mathematicians knew that a series solution to differential equations was not always the expansion of an elementary function or a composition of elementary functions. However they thought that series solutions to differential equations had a different status with respect to solutions in closed forms: series were viewed as tools that could provide approximate solutions and relationships between quantities expressed in closed forms.

[1] I would like to thank Pasquale Crispino, the headmaster of the school for accountants of Afragola, for having given me permission to attend the Oberwolfach meeting.

To make this clear, first of all, I highlight two crucial aspects of the 18th-century notion of a function:

1) Functions were thought of as satisfying two conditions: a) the existence of a special calculus concerning these functions, b) the values of basic functions had to be known, e.g. by using tables of values. These conditions allowed the object 'function' to be accepted as the solution to a problem.

2) Functions were characterised by the use of a formal methodology, which was based upon two closely connected analogical principles, the generality of algebra and the extension of rules and procedures from the finite to the infinite.

I stress that the term 'function' underwent various terminological shifts from its first appearance to the turn of the nineteenth century. In particular, during the second part of eighteenth century, mathematicians felt the need to investigate certain quantities that could not be expressed using elementary functions and sometimes–though not always–termed these quantities 'functions'. For example, the term 'function' was associated with quantities that were analytically expressed by integrals or differential equations.

This shift did not affect the substance of the matter because non-elementary transcendental functions were not considered well enough known to be accepted as true functions (see, for example, Euler [1768-70, 1:122-128]). This approach to the problem of non-elementary transcendental functions was connected with the notion of integration as anti-differentiation. Eighteenth-century mathematicians were aware that many simple functions could not be integrated by means of elementary functions and that this concept of integration posed the problem of non-elementary integrable functions. However, they limited themselves to compare integration with inverse arithmetical operations: they stated that in the same way as irrational numbers were not true numbers, transcendental quantities were not functions in the strict sense of the term and differed from elementary functions, which were the only genuine object of analysis (see Lagrange [1797, 141] and Euler [1768-1770, 1:13]).

The eighteenth-century notion of a function did not exclude the possibility of introducing new transcendental functions which had the same status as elementary functions, provided that they were considered as known objects. In effect many scholars attempted to introduce new functions (see, for example, Legendre's investigation of elliptic integrals and the gamma function (Legendre [1811-17])). However such investigations remained within the overall structure of eighteenth-century analysis. Indeed, it is true that mathematicians were convinced that additions had to be made to the traditional theory of functions and that the set of basic functions had to be enlarged; nevertheless, they thought that the core of analysis (the formal methodology connected to elementary functions) could and needed to remain unaltered. Furthermore, they often resorted to extra-analytical arguments and, in particular, geometrical interpretations when dealing with transcendental quantities, but this contradicted the declared independence of analysis from geometry, one of the cornerstones of eighteenth-century mathematics.

Around the early 1800s this conception became insufficient for the development of analysis and its applications and in 1812 Gauss changed the traditional approach. To "promote the theory of higher transcendental functions" [1812, 128], he defined the hypergeometric function as the limit of the partial sums of the hypergeometric series. In this way, Gauss viewed the hypergeometric series as a function in its own right, rather than regarding it as the expansion of a generating function. He also changed the role of convergence: from being an *a posteriori* condition for the application of formally derived results, it became the preliminary condition for using a series.

In [WA] Gauss gave another definition of hypergeometric functions: he considered the hypergeometric function as the solution to the hypergeometric differential equation. This implies a concept of integration which differed from those of Euler and Lagrange. In effect, in [WW, 10: 366], Gauss expressed the ancient Leibnizian notion of the integral in an abstract form and assumed that the relation $x \to \int_a^x \phi x\, dx$ led to a new function (provided $\phi x \neq \infty$ along the path of integration). Similarly, the hypergeometric differential equation provided a relationship between certain quantities (except for a few values of the variables where the coefficients were infinite) and therefore led to a new function.

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A puzzling remark by Euler on constant differentials

João Caramalho Domingues

[3, part I, § 246] contains a puzzling remark: given a function $F(x, y)$, where x, y are independent variables, we can take either dx or dy as constant (or neither), but not both. This seems difficult to explain, if we think of the usual equivalence dt constant $\leftrightarrow$ t independent variable [1].

This remark has consequences: putting

$$dF = Pdx + Qdy$$

and

$$\begin{cases} dP = pdx + rdy \\ dQ = rdx + qdy, \end{cases}$$

we can have only

$$ddF = Pddx + pdx^2 + 2rdxdy + qdy^2 + Qddy$$

or

$$ddF = Pddx + pdx^2 + 2rdxdy + qdy^2 \qquad (dy \text{ const})$$

or

$$ddF = pdx^2 + 2rdxdy + qdy^2 + Qddy \qquad (dx \text{ const}).$$

The "natural" formula

$$ddF = pdx^2 + 2rdxdy + qdy^2$$

does not occur.

Why is that so? In the same paragraph, Euler explains that, if dx and dy are both constant, then

$$y = ax + b.$$

This comes probably from $\frac{dy}{dx} = \frac{c_1}{c_2} = a$.

That is, dx and dy both constant implies that y and x can only have linear relations: for example, $y = x^2$ is excluded. (This is easy to verify: let $F(x, y) = xy^2$; then

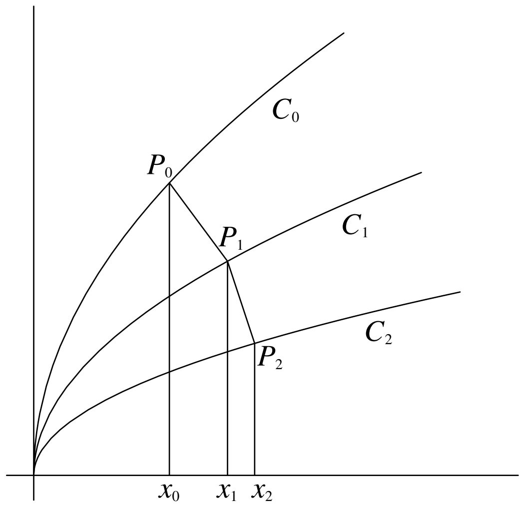
$$pdx^2 + 2rdxdy + qdy^2 = 4ydxdy + 2xdy^2 = [\text{if } y = x^2]\ 16x^3dx^2;$$

but if $f(x) = F(x, x^2) = x^5$, then, holding dx constant, $d^2f = 20x^3dx^2$.) In modern terms, this is not a problem: we want d^2F to be a bilinear map, useful for local approximations. To calculate d^2F along the curve $y = x^2$ we can use the chain rule.

But for Euler the possibility of substituting any function of x for y (or vice-versa) was important, and this had to do with the uses of partial differentiation at the time: partial differentiation did not originate from the study of surfaces, but rather from the study of families of curves, and particularly from trajectory problems [Engelsman 1984]. For example, consider a family of curves

$$y = F(x, a),$$

a being a parameter (we can think of a and x as the independent variables), and an orthogonal trajectory to this family through a point $P_0 = (x_0, y_0)$ on the curve $C_0 = F(x, a_0)$ [picture]. Take da to be constant ($a_1 - a_0 = a_2 - a_1$). The segment of the orthogonal trajectory from P_0 to $P_1 = (x_1, y_1)$ on the curve $C_1 = F(x, a_1)$ is uniquely determined, being orthogonal to the first curve, so that P_1 is uniquely determined. The same for P_2. But then dx is uniquely determined: $dx_0 = x_1 - x_0$



and $dx_1 = x_2 - x_1$. There is no reason for dx to be constant (ie, $dx_1 = dx_0$). While x and a are independent variables at the start, the kind of problems studied implies differentiation along non-linear paths.

It appears that when [3] was written Euler still had in mind only these uses for partial differentiation. ([3] was published in 1755, but according to Eneström a couple of letters from Euler to Goldbach suggest that it was already being written in 1744 and that the manuscript was with the publisher in 1748).

When did Euler start giving different uses to partial differentiation? At this time, in fact: the second part of [3] includes two chapters on maxima and minima, one for (uniform) functions of one variable, and the other for multiform functions and functions of several (in fact, two) variables.

It is interesting that Euler gets this last case wrong. Consider $F(x, y)$, and $dF = Pdx + Qdy$. Euler remarks that if (x_0, y_0) is a maximum or minimum of $F(x, y)$, then x_0 and y_0 are also maxima or minima of $F(x, y_0)$ and $F(x_0, y)$, respectively (and therefore $P = Q = 0$); and that they must agree, ie, both be maxima or both be minima, and therefore

$$\frac{dP}{dx} = \frac{ddF}{dx^2}$$

must have the same sign as

$$\frac{dQ}{dy} = \frac{ddF}{dy^2}.$$

The problem is that he assumes this to be sufficient conditions. Of course his mistake is not a consequence of having

$$ddF = Pddx + pdx^2 + 2rdxdy + qdy^2 + Qddy,$$

since in this case $P = Q = 0$ implies that (even for him)

$$ddF = pdx^2 + 2rdxdy + qdy^2.$$

But it looks like a beginner's mistake: Euler was a "beginner" in uses of partial differentiation other than studies of families of curves.

This mistake was corrected in [4]. There Lagrange seeks conditions for $d^2F > 0$ (minimum) or < 0 (maximum), whatever dx, dy; manipulating

$$d^2F = pdx^2 + 2rdxdy + qdy^2$$

he arrives at the condition that Euler had missed:

$$p\ q > r^2.$$

Interestingly, Lagrange does not argue that since for an extreme

$$P = Q = 0,$$

he can take

$$d^2F = pdx^2 + 2rdxdy + qdy^2.$$

Instead, he just supposes the first differentials dx, dy constant, "ce qui est permis"!

It was allowed, since he was addressing an entirely different kind of problem from families of curves: the search for maxima and minima is a *local* problem where only directions are important, not paths followed. However, it is not at all clear that Lagrange was thinking along such terms.

It is quite likely (but this requires further investigation) that Lagrange was responsible for the adoption of $pdx^2 + 2rdxdy + qdy^2$ as the canonical form of d^2F, not necessarily or exclusively because of [4], but to a great extent because it is that form that occurs in Taylor series.

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Habituation and representation of elliptic functions in Abels mathematics

Henrik Kragh Sørensen

In 1827, N. H. Abel (1802–29) introduced a new class of functions — elliptic functions — to analysis [1]. Abel defined an elliptic function by means of an inversion of an elliptic integral, which in Abel's case took the form

$$\text{(Inv)} \qquad \alpha = \alpha(x) = \int_0^x \frac{dx}{\sqrt{(1-c^2x^2)(1+e^2x^2)}} \quad \rightsquigarrow \quad \phi(\alpha) = x.$$

Here, $\phi(\alpha)$ is the elliptic *function* expressing the upper limit of integration x in (Inv) as a function of the value of the integral. Abel's inversion was first done for a segment of the real axis, then "by inserting xi for x" for a segment of the imaginary axis. There is no use of complex integration, here, only a formal substitution. By means of addition formulae, Abel found that ϕ was a doubly periodic function of a complex variable.

The inversion of the elliptic integral into a complex function covered some 15% of Abel's *Recherches sur les fonctions elliptiques*. The remaining part of that paper was devoted to three apparently distinct problems: the division problem and the division of the lemniscate (35%), obtaining infinite representations for the elliptic function (30%), and the beginnings of a theory of transformations of elliptic functions (20%). The division problem and the analogies between the cyclotomic equation and the division of the lemniscate have rightly been analysed as a key inspiration for Abel's work. Similarly, considerable attention has been given to the transformation theory, in part because this was the corner stone of Abel's fierce and productive competition with C. G. J. Jacobi (1804–51). However, much less attention has been given to the middle part of Abel's *Recherches*, in which he derived various infinite representations for his new functions.

By long sequences of formal manipulations, Abel derived various representations of his elliptic functions in terms of infinite sums and products. Just focusing on the infinite sums, certain differences can be noticed. First, Abel deduced a representation in the form of a doubly infinite series of terms that are rational in the given quantities. However, he did not stop there but went on to derive various representations as infinite series of transcendental terms. Later, in a paper published in 1829, Abel also claimed that the elliptic function could be expressed as the quotient of two convergent power series [2]. This was also the only occassion where Abel commented on the convergence of his representations, albeit in a very short almost laconical way. Thus, a complete hierarchy of infinite representations presents itself to our historical analysis.

When we try to understand why Abel produced more than one infinite representation of elliptic functions, we are faced with a number of suggestions: 1) It could have to do with numerical convergence; indeed there are drastic differences in the speed of convergence of the doubly and the singly infinite series. However, this does not seem to have been a concern for Abel. 2) There could be structural reasons for listing a hierarchy of series; such a hierarchy was imposed around 1700 to bring order into transcendental curves (see e.g. [3]). However, again, this does not seem to have been Abel's purpose. 3) It could be that Abel had to derive the representations because he could use them in other proofs. Although Abel died before the theory of elliptic functions was brought to anything like fruition, he only used these representations once in his rather large mathematical corpus on elliptic functions. Thus, I suggest that to Abel the "raison d'être" of infinite representations must be found in the process of *habituation* — coming to know the newly introduced objects.

Abel's new elliptic functions were part of a wider movement of the late 18th and early 19th century to deliberately enrich analysis by introducing new functions by various means. These functions were pursued with the hope of extending analysis — its domain as well as its methods.

In the second half of the 18th century, analysis had primarily dealt with functions given *explicitly* by formulae, and analysis progressed through formal manipulations — often of infinite expressions. I use the term "formula-centred" to denote this style of mathematics. By the early 19th century, however, the new functions were being introduced by indirect means: differential equations, functional equations, or — as with Abel's elliptic functions — formal inversions. These procedures were not new, but they only gave away little information about the functions, in particular about the evaluation of the function at arbitrary values of its argument. But to bridge this difference, the infinite representations provided the key element. With infinite representations — that were, noticeably, not definitions — mathematicians could habituate themselves with these new functions using the previously prevailing mathematical style. Thus, the process of habituation provided an anchoring of the new style that I term "concept-centred" within the previous formula-centred approach.

Abel's habituation of elliptic functions provides an example of an instance where the framework of a transition between formula-centred and concept-centred styles in mathematics can provide explanations for apparent anomalies. At first, the attention given to infinite representations in Abel's first paper on elliptic functions seemed strange as it apparently led nowhere. However, when one realises that the elliptic functions which were introduced indirectly (essentially a concept-centred step) were not really functions in the formula-centred approach until these representations had been devised, the section seems to have been better explained.

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Lagrange's Series in Early 19th-Century Analysis

Hans Niels Jahnke

Today, Lagrange's series is known only to some specialists. In the 18th and 19th centuries, however, it was an important and frequently treated tool of analysis which was extensively used for astronomical calculations. Lagrange published his result for the first time in 1770 [6]. It runs as follows:

Theorem 1. *Given an equation $\alpha - x + n \cdot \phi(x) = 0$, n a parameter and $\phi(x)$ an 'arbitrary' function. Let p be one of the roots of this equation and $\psi(p)$ an 'arbitrary' function of p. Then the expansion*

$$\psi(p) = \psi(x) + n \cdot \phi(x)\psi'(x) + \frac{n^2}{2}\frac{d\left[\phi(x)^2\psi'(x)\right]}{dx} + \frac{n^3}{2 \cdot 3}\frac{d^2\left[\phi(x)^3\psi'(x)\right]}{dx^2} + L$$

holds, where one has to substitute α instead of x after the differentiations.

In the style of 18th-century analysis, 'arbitrary function' means a function which can be developed in a power series. If ψ is the identity, the series gives simply a root of the equation. The series has some similarity with the Taylor series, the parameter α playing the role of the centre of expansion. The theorem can be seen in the context of implicit functions, as a means to formally invert formal power series or as a universal tool for the solution of arbitrary algebraic or transcendental functions.

Lagrange proved the above relation by purely algebraic (combinatorial) calculations in which the roots play a symmetrical role. Thus the theorem does not give any information regarding the question of which root of the related equation is given by the series.

The series was applied to the solution of transcendental equations of astronomy (Kepler's equation), and it (including generalisations to several variables) played a prominent role in Laplace's *Mécanique céleste*.

In 1798, Lagrange gave a specification of his theorem by stating that his series always represents the numerically smallest root [7].

During the 19th century there appeared quite a few papers on Lagrange's series with new proofs and generalisations to several variables. These papers show that the series has two faces. It can be seen as an interesting combinatorial relation. As such it was treated for example by Cayley and Sylvester and seems to be interesting even today. On the other hand, it is an analytical relation and in this role it belongs to complex function theory.

In fact, Lagrange's series was the starting point and frequent test case for Cauchy's important investigations on the development of complex functions in power series. Cauchy entered the field in 1827 (see [2]). His paper was motivated by an appendix to the second edition of Laplace's *Mécanique céleste* in which the latter had derived the radius of convergence of the series giving a solution of Kepler's equation. Cauchy was struck by the fact that the radius of convergence appeared as the solution of a certain transcendental equation and was got without using the terms of the series. He wanted to understand this phenomenon more generally and studied Lagrange's series. Representing the terms of the series by means of his integral formula he was able to derive an equation whose solution gave the radius of convergence of the Lagrange series without explicitly referring to the terms of the series. Laplace's equation was a special case of this. The paper was to become a paradigm for Cauchy's further papers on the development of complex functions in power series.

In 1830, in the course of the July revolution in France, Cauchy went into exile in Turin. He obtained a chair of "higher physics", and on October, 11, 1831, read an important paper at the Turin Academy under the title *Mémoire sur la mécanique céleste et sur un nouveau calcul appelé calcul des limites* [3]. In this paper he criticised the mathematical methods for determining the trajectories of the heavenly bodies as insufficient, stated that Laplace's *Mécanique céleste* did not contain an adequate proof of Lagrange's theorem though it is basis for Laplace's calculations, and lamented that there is no method of estimating the error for implicit functions. Cauchy promised to solve these problems in his paper. For some people in his audience this sounded offensive since Turin was a leading centre of astronomy and they did not like a criticism of Lagrange, who had been born there.

In our present context the most important part of the paper concerned Cauchy's famous theorem about the radius of convergence of the power series expansion for a complex function.

Theorem 2. *The function $f(x)$ can be expanded into a convergent power series if the modulus of the real or imaginary variable x remains below the value for which the function $f(x)$ is no longer finite, unique and continuous.*

Cauchy said emphatically that he had reduced the law of convergence to the law of continuity. However, for 15 years he was not sure about the conditions of the theorem and changed his mind several times as to whether it is sufficient to require finiteness and continuity only for the function $f(x)$ or also for its first derivative (see [1]). From this general theorem one can easily derive the radius of convergence of Lagrange's series. Cauchy did this only in 1840, whereas in the present *Mémoire* he again used his integral formula. He gave the following

Theorem 3. *Lagrange's series represents the unique root y_1 of the equation $\alpha - y + x \cdot f(y) = 0$ which becomes α for $x = 0$. This holds for all x for which*

$$mod\, x < mod\, \frac{z - \alpha}{f(z)}$$

where z designates the roots of the equation

$$f(z) - (z - \alpha) \cdot f'(z) = 0.$$

Thus, Cauchy had made a statement concerning which root is represented by Lagrange's series for the complex case.

In 1842/43 the young Italian mathematician Felice Chió (1813-1871) read two papers at the academy of Turin on Lagrange's series in which he gave a complete theory for the case of real roots and provided several counter-examples against Lagrange's statement that his series always represents the smallest root of the given equation. The academy refused to publish these papers, so Chió sent them to Cauchy. On Cauchy's recommendation they were read at the Paris academy and finally published ([4]). Chió's main result was

Theorem 4. *If all roots of the equation $u - x + t \cdot f(x) = 0$ are real then in the case of $f(u) > 0$ Lagrange's series represents that root a which is the smallest among* *all the roots greater than u, and, in the case of $f(u) < 0$, the greatest root smaller than u.*

The simplest counter-example, given by Cauchy following the model of Chió, is provided by the equation $a - y + x(y+k)^2 = 0$. Its expansion according to Lagrange is

$$y = a + x(a+k)^2 + \frac{4}{2}x^2(a+k)^3 + 5x^2(a+k)^4 + L$$

whereas a direct solution gives

$$y_- = \frac{1 - 2kx - \sqrt{1-4(a+k)x}}{2x} \quad \text{and} \quad y_+ = \frac{1 - 2kx + \sqrt{1-4(a+k)x}}{2x}.$$

If one expands the square roots in these formulae according to the binomial theorem into power series one sees that Lagrange's series represents y_-. But this is numerically larger than y_+ for $x = 1$ and adequately chosen a and k.

Thus, Lagrange's series was motivation and frequent test case for Cauchy's fundamental papers on the expansion of complex functions in series. The story shows that at the transition from the 18th to the 19th century, a much more difficult problem than convergence was the occurrence of multi-valued functions which led to several mistakes (see [5] for a further example). The related questions were clarified by Puiseux, Riemann and Weierstrass.

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Two Historical Stages of the Hamilton-Jacobi Theory in the Nineteenth Century

Michiyo Nakane

Transformation of variables enables one to solve various types of differential equations. Using ideas of transformation from diverse branches of mathematics, late nineteenth-century researchers established a general theory of transformations.

Thus, the notion of transformation of variables in the theory of differential equations became more general than it had been in the first half of the nineteenth century. There are many examples that give the solution of differential equations using the general notion of transformation. This paper focuses on the following method given in many modern textbooks on analytical mechanics.

Let q_i, p_i satisfy the canonical equations

$$\frac{dq_i}{dt} = \frac{\partial H}{\partial p_i}, \quad \frac{dp_i}{dt} = -\frac{\partial H}{\partial q_i}, \qquad (i = 1, \ldots, n) \tag{1}$$

where $H = H(q_i, p_i, t)$. Consider the transformation

$$P_i = -\frac{\partial S}{\partial Q_i}, \quad p_i = \frac{\partial S}{\partial q_i}. \qquad (i = 1, \ldots, n) \tag{2}$$

If S is a complete solution of a partial differential equation, the so-called Hamilton-Jacobi equation,

$$\frac{\partial S}{\partial t} + H\left(q_i, \frac{\partial S}{\partial q_i}, t\right) = 0, \tag{3}$$

then S is a generating function for this transformation. From this theorem the new set of variables satisfies

$$\frac{dQ_i}{dt} = \frac{\partial K}{\partial P_i}, \quad \frac{dP_i}{dt} = -\frac{\partial K}{\partial Q_i}, \qquad (i = 1, \ldots, n) \tag{4}$$

where $K = \frac{\partial S}{\partial t} + H$, but $\frac{\partial S}{\partial t} + H = 0$ and so we have $\dot{Q}_i = 0$, $\dot{P}_i = 0$. We can easily obtain solutions of equation (1) if we find a complete solution of equation (3).

The name "Hamilton-Jacobi equation" reminds us of another theorem that was presented in Jacobi's lectures in 1842-43: A complete solution of Hamilton-Jacobi equation S gives solutions of the canonical equations (1) through relations

$$\frac{\partial S}{\partial q_i} = p_i, \quad \frac{\partial S}{\partial \alpha_i} = \beta_i, \qquad (i = 1, \ldots, n) \tag{5}$$

where α_i are arbitrary constants involved in the complete solution and β_i are new arbitrary constants. We find this theorem in modern textbooks on differential equations and calculus of variations.

Although both theorems reduce a system of ordinary differential equations to a partial differential equation, the two theorems are quite different because the first one involves the notion of canonical transformation while the second one does not. This paper calls the former theorem Hamilton-Jacobi theorem II and the latter Hamilton-Jacobi theorem I. There is a tendency in the literature to confuse these two theorems. The present paper traces the history of theorem II.

While studying Hamilton's results and obtaining a preliminary version of theorem I in 1837, Jacobi discussed a transformation that preserves the canonical form of the canonical equations. The canonical equations

$$\frac{da_i}{dt} = -\frac{\partial H}{\partial b_i}, \quad \frac{db_i}{dt} = \frac{\partial H}{\partial a_i}, \qquad (i = 1, \ldots, n) \tag{6}$$

where $H = H(a_i, b_i)$, can be changed to new ones

$$\frac{d\alpha_i}{dt} = -\frac{\partial H}{\partial \beta_i}, \quad \frac{d\beta_i}{dt} = \frac{\partial H}{\partial \alpha_i}, \qquad (i = 1, \ldots, n) \tag{7}$$

where $H = H(\alpha_i, \beta_i)$, if the old variables (a_i, b_i) are related to new ones (α_i, β_i) by a function $\psi = \psi(a_i, \alpha_i)$ that satisfies

$$\frac{\partial \psi}{\partial \alpha_i} = \beta_i, \quad \frac{\partial \psi}{\partial a_i} = -b_i. \qquad (i = 1, \ldots, n) \tag{8}$$

Although his investigation was closely related to the Hamilton-Jacobi equation, he neither discovered or proved that a complete solution of this equation was a generating function for a canonical transformation.

In 1843 Jacobi examined a transformation of variables and succeeded in reducing the number of equations of the three-body problem from 18 to 6. This work did not employ a canonical transformation; indeed the canonical equations did not appear in the paper. His contemporaries found that the canonical equations were very useful for analyzing this problem. Using Jacobi's idea, they tried to find an appropriate transformation that made the number of equations smaller or the transformed equations more easily integrable. Whittaker reported on these researches in his survey of 1899.

The French mathematician Radau found in 1868 that an orthogonal transformation, which had the same effect as Jacobi's transformation, preserved the canonical form of equations. Poincaré noted this fact in 1890 and actually transformed canonical equations to new ones by using an orthogonal transformation.

A series of discoveries related to Hamiltonian systems in 1890 seemed to have made Poincaré decide to begin his comprehensive work *Methodes Nouvelles de la Mécanique Céleste* by introducing some well known properties of the canonical equations. In volume 1, published in 1892, he began by demonstrating Hamilton-Jacobi theorem I, which he named Jacobi's first theorem, and next introduced Jacobi's result on the canonical transformation, which he called Jacobi's second theorem. Jacobi had discussed the two theorems separately but Poincaré considered them together because they were both related to properties of the canonical equations. Poincaré applied the theory in his investigation of Keplerian motion and succeeded in obtaining a canonical transformation using a complete solution of the Hamilton-Jacobi equation.

In 1897, Poincaré arrived at the following new theorem about canonical transformations: If there is a relation between the old variables (x_i, y_i) and new ones (x'_i, y'_i) such that $\Sigma(x'_i dy'_i - x_i dy_i)$ is an exact differential, then this transformation will preserve the canonical form of the original equations. In the third volume of *Methodes Nouvelles*, he proved this theorem using a variational principle of mechanics, known today as Hamilton's principle.

In his *Leçons de la Méchanique Céleste* published in 1905, Poincaré presented a full demonstration of Hamilton-Jacobi theorem II using the above-mentioned property of 1897. Poincaré referred to this result as Jacobi's method, although he himself was the first to formulate and derive it. This is one of the reasons that modern textbooks fail to distinguish the two Hamilton-Jacobi theorems. Hamilton-Jacobi theorem II should really be called the Jacobi-Poincaré theorem. Although Radau's orthogonal transformation, Poincaré's starting point, had originated in Jacobi's work, it was Poincaré's contribution to extend and to refine Radau's idea. Jacobi actually demonstrated two theorems that were presented in Poincaré's *Methodes Nouvelles*. But it was Poincaré's achievement to combine Jacobi's two results and show that a complete solution of the Hamilton-Jacobi equation gives rise to a generating function for a canonical transformation.

The Hamilton-Jacobi equation is a central part of analytical mechanics. The history of the shift from Hamilton-Jacobi theorem I to II shows how the notion of canonical transformation came into to analytical mechanics. It is well known that canonical transformations played an important role in the construction of quantum mechanics. Poincaré's formation and proof of the modern Hamilton-Jacobi theorem was thus a crucial factor in the new analytical mechanics of the twentieth century.

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On some of Newton's Methods for finitary quadratures (1664-1666)

Marco Panza

One often argues that for Newton, any function could be easily integrated by series and the problem of integration could thus be solved generally in such a way. A study of Newton's mathematical papers from 1664 to 1666 shows instead that, from the very beginning, finitary integration was a central problem for him. I present the reconstruction of some of Newton's methods.

Although I have used the word "integration", this is not accurate; we should rather say "quadrature" for the period from the summer of 1664 to the fall of 1665, and "inverse problem of speeds" for a second period, namely the fall of 1666.

Newton's first method of quadrature stems from his own reinterpretation of the methods used by Wallis in the first part of the *Arithmetica infinitorum.* Newton understood them as concerned with the search for the measure of a surface, rather than of a ratio between a surface and a polygon, as Wallis had done. This made him able to draw from them two linear algorithms of quadrature for curves of equation $y = x^{\mu}$, where μ is an integer other than -1 (one for $\mu > -1$ and the other for $\mu < -1$).

The second method relies on a theorem proved by Newton by trivially modifying the prove of another theorem of van Heuraet. It states that if two curves referred to the same axis are such that their ordinates y and z satisfy the proportion $stg_x[y] : y = K : z$, provided that K is any constant and $stg_x[y]$ is the sub-tangent of the first curve, and this curve is monotone in the relevant interval, then the surface $\sum_{\kappa}^{\xi}[z]$ delimited by the second curve between the limits $x = \kappa$ and $x = \xi$ is equal to the rectangle constructed on K and on the difference $|y_{\xi} - y_{\kappa}|$. Newton gives this theorem in a note of summer of 1664 and applies it to square a number of curves of equation $z = K\frac{y}{stg_x[y]}$ (that is, $z = K\frac{dy}{dx}$), where y is the ordinate of a curve expressed by a given polynomial equation $F(x, y) = 0$.

Newton's work on the relations between the problems of tangents and quadratures and the corresponding algorithms led him to compose, sometime between the summer and the fall of 1665, two tables that Whiteside presents as tables of primitives. Newton's arguments are however openly geometric and nothing in his previous notes entitles us to suppose that he had defined, even implicitly, a mathematical object that one could identify with a primitive. Thus, I prefer to understand these tables as tables of quadratures.

Another note of the same period marks a turning point in Newton's mathematical researches, since he introduces there the notions of generative motion of a geometric magnitude and of punctual speed of this motion: he associates to every segment x, y, or z a punctual velocity p, q, or r. He also presents an algorithm of punctual speeds, leading from a polynomial equation $F(x, y) = 0$ to the corresponding polynomial equation $G(x, y, p, q) = 0$, of first degree in p and q. This is of course the same as the algorithm of tangents, but in this note Newton does not mention it. He rather applies this algorithm to interpret his previous results about quadratures in a new way so as to prove them without appealing to geometry, as results purely concerned with the inversion of such an algorithm.

The introduction of the punctual speeds of the generative motion of variables allowed him to elaborate a method of quadrature formally equivalent to our method of integration by substitution, which he used to compose two new tables of curves having the same "area". In the second of these tables, Newton reduced the quadrature of a number of geometric curves to the quadrature of curves whose equation has one of the following forms: $y = \frac{\alpha}{x}$, $y = \sqrt{\alpha + \beta x^2}$, $y = \sqrt{\alpha + \beta x^2}$, $y = \sqrt{\alpha + \beta x + \gamma x^2}$. He seems thus to consider these equations as elementary archetypes of algebraic equations expressing curves that it is not possible to square with finitary algebraic tools.

Proposition 8 of the *October 1666 Tract* asks for the determination of an equation of the form $y = f(x)$, starting from the expression of the corresponding ratio $\frac{q}{p}$. Newton was of course unable to solve this general problem in finitary terms. He only gave some rules to solve it in certain cases. The extension of these rules, however, is far larger than before. Moreover, though Newton defines p and q as being speeds of generation of segments, the problem is as such independent from any geometric framework. The expression for y can thus be understood as a primitive.

Newton distinguishes three cases: when the expression of $\frac{q}{p}$ is a sum of monomial with a rational exponent; when this expression is a quotient of polynomials; and when it is irrational. The first case is trivial. To treat the second, Newton follows a method equivalent to the method of integration by reduction to partial fractions. To treat the second he appeals to convenient substitutions, together with the method of indeterminate coefficients. Once again, he seems to consider the previous four forms as elementary archetypes of algebraic expressions deprived of an algebraic primitive and tries to reduce to them a number of other algebraic expressions.

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Differential equations and linearity in the 19th and early 20th centuries: a short review

Ivor Grattan-Guinness

I. Since their inception at the birth of the full calculus due to Isaac Newton and G. W. Leibniz, 'differential equations' (Leibniz's name) have been a major component of the theory. Indeed, they grew steadily in importance, especially from the mid 18th century when partial equations were introduced by Jean d'Alembert and Leonhard Euler and the range of analysable physical phenomena was greatly extended, usually in mechanics. By the end of the century a large range of types of both ordinary and partial equations had been studied, both for their own sake and especially via inspiration from applications; the calculus had been further enriched with the emergence of the calculus of variations in a substantial form by the 1770s thanks to Euler and especially J. L. Lagrange. I consider the place of linearity in the theory of differential equations during the 19th century alongside the extension of linear theories in general. Some influential treatises and textbooks of the mid and late century presented and underlined the importance of linearity.

The work of Joseph Fourier and A.-L. Cauchy inspired much positive reaction from the 1820s onwards, especially but not only with Fourier analysis, and from Cauchy also the inauguration of complex-variable analysis; the place of linear differential equations was thereby still further enhanced. Another aspect of Fourier's influence was his pioneering context, namely a breakthrough into mathematical physics (in his case heat theory): other of its departments, especially electricity and magnetism and their interactions, and optics, were to prove to be equally susceptible to linear modelling. Further factors include much study of the special functions, usually as parts of solutions of linear equations.

Lecture courses on mathematical physics delivered during the century show the same dominance of linear theories; for example, an important one, Bernhard Riemann's, delivered in the 1850 and 1860s and published in posthumous editions into the 20th century, show a steadily increasing presence of linear theories. By the 1890s large-scale volumes on linear differential equations, ordinary and partial, were being produced, and textbooks and treatises on real- and complex-variable analysis often contained substantial sections upon them.

II. By linear theories in general the defining feature is the central and frequent place of the form of linear combination (hereafter, 'LC'):

$$ax + by + cz + \ldots (= d) \tag{1}$$

for some interpretation of the letters and of the means of combination; to the form itself was often associated an equation where the combination "added up" to d above, another member of the algebra in hand. The series was finite (the introduction of Lagrange multipliers in the calculus of variations, say) or maybe infinite (as with Fourier series). Theories in which LC was prominent often also deployed quadratic forms and equations

$$Ax^2 + Bxy + Cy^2 + \ldots (= D) \tag{2}$$

in any number of variables, usually finite; (2) is itself LC relative to its quadratic and bilinear terms.

Early new such algebras flourishing early in the 19th century included differential operators (the 'D' algebra), linear in form in cases such as Fourier's wave and the diffusion equations, where the LC operator

$$xD_x + yD_y + \ldots \tag{3}$$

in the independent variable was a key notion involved in handling partial equations, and also important in the assumption of potentials. Two other important early topics were functional equations; and substitution theory in connection with roots of equations, which was later to help create group theory. In the 1840s onwards new cases included George Boole's algebra of logic, where his expansion theorems exhibited LC; Hermann Grassmann's algebraic study of geometric magnitudes in his *Ausdehnungslehre*; and W. R. Hamilton's quaternions, and later hypercomplex numbers. At that time determinant theory began to be developed, and gained status in mid-century when Arthur Cayley and J. J. Sylvester introduced the conceptual theory of matrices, in which properties involving LC were very prominent; for example, in reforming and solving systems of linear equations, and in defining matrix multiplication. Later cases from the 1870s onwards include Georg Cantor's set theory, which in its topological side featured LC-style decomposition theorems of sets; and Henri Poincaré's development of algebraic topology, where he took a general manifold more or less in Riemann's sense of the term and deployed LC to state its decomposition into an integral number (positive or negative according to a certain definition of orientation) of varieties of lower dimensions, the right hand side of equation (1) being replaced by an unexplained '$\sim \epsilon$'.

The mathematician who extended LC to the greatest measure was E. H. Moore. In the mid-1900s he formulated the first version of what he called 'General analysis', the name imitating Cantor's phrase 'general set theory' for the general aspect of that theory. Linearity was central to his concerns: his special cases included Fourier analysis (where the series themselves exhibit LC) and the extensions to functional analysis and linear integral equations, and associated theories such as infinite matrices, all rapidly expanding at that time. In the end Moore published rather little on his theory, which gained the interest of a few students and other followers; but his vision marked the climax of a long tradition of linear algebraisation of mathematical theories of ever more kinds.

III. Success breeds success, and it is not surprising that the domain of linearity came to be so vast; over a long period mathematicians became accustomed to seek some sort of linear theory, especially in connection with differential equations. The spread of linearity in so many other newer (often algebraic) theories doubtless reinforced the confidence. We have here a case of habituation (see the abstract by Dr. Sørensen); Thomas Kuhn's notion of normal science also fits very nicely. But with regard to applications, the adhesion to linearity seems surprisingly strong when everyone knew that the physical world was rarely if ever a linear place. But the place of non-linear differential equations is surprisingly modest; at times it arises only in special situations such as cases of singularity of solutions of a linear equation. The rise of non-linear mathematics, especially concerning differential equations and their kin, deserves a good study; some of the story lies before 1900, but I suspect much of it occurred long afterwards.

The following list of references is confined to some major primary sources on linear differential equations, especially from late in the 19th century; and to several pertinent historical items.

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Non-Holonomic Constraints from Lagrange via Hertz to Boltzmann

Jesper Lützen

Lagrange showed how one can deal with constraints in mechanics, either by choosing new unconstrained generalized coordinates or by using Lagrange multipliers. Yet in Newtonian-Laplacian physics all interactions were in principle due to forces, so constraints were only approximate mathematical tools. In Heinrich Hertz's *Prinzipien der Mechanik* (1894), on the other hand, the role of the concepts of force and constraint (or rigid connection as he called them) was reversed. Indeed, Hertz abandoned "force" as a basic concept of mechanics. He had two reasons for this move: 1. His own famous experiments on electromagnetic waves had convinced him that electromagnetic interactions took place as local actions in a field (in the ether) as Maxwell had argued, rather than as forces acting at a distance as Weber had argued and he had hopes that one would be able to explain gravitation in a similar way as a field action. 2. He had spotted many inconsistencies in the usual treatises of mechanics, and he believed that the concept of force was mainly responsible for these. Therefore he postulated that interactions take place through rigid connections only, forces being only an epiphenomenon defined *a posteriori* as Lagrange multipliers.

Hertz argued that "experience of the most general kind" teaches us that "*natura non facit saltus*" a knowledge that he specified in three "continuity" axioms. From these axioms he deduced that connections must be described by homogeneous linear first order differential equations of the form

$$\sum_{\rho=1}^{r} q_{\chi\rho} dq_{\rho} = 0, \quad \chi = 1, 2, 3, ..., k, \tag{1}$$

where q_ρ are generalized coordinates of the mechanical system and $q_{\chi\rho}$ are functions of these coordinates. In cases where these differential equations can be integrated in the form

$$F_{\chi}(q_1, q_2, q_3, ..., q_r) = c_{\chi}, \quad \chi = 1, 2, 3, ..., k \tag{2}$$

Hertz called the system Holonomic, in other cases non-holonomic. He argued that it would be unreasonable to forbid non-holonomic constraints because such constraints might be active in the ether. As a footnote one can add that a close inspection of Hertz's mechanics shows, that non-holonomic constraints are in fact not allowed in hidden systems as the ether, but Hertz does not seem to have discovered this problem. As the prime example of a non-holonomic system Hertz mentioned rolling systems, such as the rolling without slipping of a ball on a plane.

Hertz discovered that the usual integral variational principles such as the principle of least action or Hamilton's principle do not hold for non-holonomic systems. In fact this was one of his main arguments against the so called energeticist program of physics which usually took one of these principles as the basic law of motion.

Hertz's rejection of the integral variational principles called for an immediate rescue operation headed by Otto Hölder. In 1896 he published a paper in which he pointed out that if the variations in the variational principles are chosen in the right way, the principles remained valid. Instead of assuming, as Hertz had done, that the varied motion should satisfy the constraints, Hölder assumed that the variations satisfy the constraints. If the system is non-holonomic the varied motion will not satisfy the constraints, i.e. it will not be an admissible motion, so Hölder's variational principle is not about an ordinary variational problem, but it gives the correct trajectories.

Hertz was the first to coin the name (non-)holonomic system, but he was not the first to consider non-holonomic systems or to call attention to the failing of the variational principles for such systems, nor was Hölder the first to point to a way out. Indeed there is a whole history of repeated independent mistakes, rejections and rescues concerning this problem and a connected problem of how to deal with Lagrange's equations in connection with non-holonomic constraints: In fact although one cannot describe a non-holonomic system in terms of freely varying generalized coordinates, one could be tempted to use the equations of constraints to eliminate k of the generalized velocities $\dot{q}_\rho$ in Lagrange's equations. However it turns out that one does not find the correct trajectory that way. This problem was noticed by Ferrers (1873) and Routh (1877), and Ferrers set up an alternative equation that must be used if one eliminates variables in Lagrange's equations. Routh also called attention to the problem with the variational principles, as did Carl Neumann (1888), Hölder (1896) and Vierkandt (1892). Also in France the problems were discovered by Hadamard (1895), but still an error of this kind committed by Lindelöff (1895) was repeated by Appell (1896). Korteweg corrected Appell's mistake in 1900, without noticing that Appell had himself corrected the mistake in several places. Finally, when Boltzmann got the wrong result in connection with a calculation of a rolling system, he published a paper calling attention to the problems (1902). A somewhat more sophisticated discussion was published by Hamel two years later.

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19th-century Lunar Theory

Curtis Wilson

The special difficulty of the lunar theory was already evident in the earliest efforts to develop it analytically. Using disparate approaches, Euler, Clairaut, and d'Alembert all found, as an initial approximation, only about half the motion of the lunar apse. Newton's inverse-square law, they concluded, was wrong or insufficient.

In 1749 Clairaut found that a second-order approximation yielded most of the remaining apsidal motion. Neither he nor Euler nor d'Alembert, however, despite enormous labor, could obtain a theory accurate to more than about 3 or 4 arcminutes. Accuracy to 2 arcminutes was needed to give the longitude at sea to within a degree. Tobias Mayer, using multiple observations to supplement the theoretical calculation, achieved predictions accurate to 1.25 arcminutes. His results became the basis of the *Nautical Almanac*, with later refinements mostly from comparison with observations

Improvements in the theory by inclusion of further gravitational effects were made, especially by Laplace, who looked to the day when the pure theory would be as accurate as the observation-based theory. But he lacked a systematic procedure for perturbations of second or higher order. In 1825 Plana challenged Laplace's calculation of second-order terms in the theory of Saturn. The Berlin Academy then posed the resolution of this controversy as its prize problem for 1830. P.

A. Hansen's prize paper gave the first systematic method for computing higher-order approximations. Later he applied his new method to the Moon, and his lunar theory became the basis of the *Nautical Almanac* from 1862, remaining, with adjustments, in that role till 1922.

Hansen's method, based on Lagrangian formulas, was in principle rigorous; but was its execution strictly correct? From the start Hansen had introduced numerical values for the elements, to avoid getting bogged down in slowly converging series. As a result his steps became untraceable. From the 1870s, Newcomb was finding discrepancies between current observations and the Hansenian tables. How to introduce corrections to the theory in an honest way? It could only be done by starting all over again. Preferable would be a *literal* or *algebraic* theory, spelling out each coefficient symbolically.

Such a theory was Delaunay's, which started from canonical variables giving the elliptical elements. The procedure was systematic. G.W. Hill, when he encountered this theory in the 1870s, was ecstatic. No better set of elements, he said, could be chosen.

But then he discovered that Delaunay, in 20 years of labor, had not managed to carry the calculations of the coefficients of his sinusoidal terms far enough to match current observational precision. The series giving the coefficients converged too slowly. Delaunay had resorted to estimates of terms not calculated. This was exact science? Hill threw up his hands. A new beginning was required.

Hill's new beginning was the *numerical* calculation of a particular solution of the three-body problem; the "Variation Orbit," as Hill called it. All its features derived from the ratio, m, of the Sun's mean motion to the Moon's mean motion. There, in the algebraic role of m, Hill realized, was the gremlin that had made the convergence of Delaunay's series so slow. He obtained the numerical coefficients specifying the Variation Orbit to 15 decimal places. The ratio m was the most exactly known parameter of the Moon's motion; eccentricity, inclination, and parallax being much less exactly known. But once the Variation Orbit was known, the last-named parameters could be introduced in algebraic form, and determined numerically by a least-squares fit with observations.

In addition to computing the Variation Orbit, Hill asked what apsidal motion would emerge if an unspecified eccentricity was introduced, small enough that its square could be neglected. The problem involved an infinite determinant which Hill managed to resolve by a series of adroit moves. An analogous but simpler infinite determinant had been solved a little earlier by John Couch Adams in obtaining the motion of the lunar node. The motions thus calculated proved to be the *principal* parts of the motions of the apse and node, close in value to the observational values.

The completion of Hill's theory was carried out by Ernest W. Brown during the years 1891–1907. Brown found that he could obtain the remaining parts of the motions of the apse and node by the ordinary process of successive approximations. But starting from Adams' form for the differential equations, and using Poincaré's necessary and sufficient conditions for the convergence of an infinite determinant, he also satisfied himself that Hill's determinant remained convergent when eccentricity, inclination, and parallax were introduced - an indication that the theory in its elaborated form was still sound.

Rayleigh's *Theory of Sound* and the rise of modern acoustics

Ja Hyon Ku

1. Acoustical research in the first half of the 19th century. 'Acoustics' was an experimental investigative enterprise in the early 19th century. The group of so-called 'acousticians' included Chladni, Young, Savart, Colladon, Faraday, Wheatstone, Lissajous, Tyndall, Koenig, A. Mayer, etc. Their experimental works were summarized in Tyndall's *On Sound* (1867). In addition to 'acousticians', there were researchers who did research theoretically on the making and transmitting of sound in the mathematical manner. This group included D'Alembert, Euler, Lagrange, Poisson, Sophie Germain, G. Ohm, Kirchhoff, Riemann, Donkin, S. Earnshaw, etc. For them analysis was the central method of dealing with problems associated with sound. Their investigations were not closely connected to the empirical and experimental findings gathered by the 'acousticians'.
2. Helmholtz's mathematical dash in 'acoustics'. In the middle of the century, Hermann von Helmhlotz transformed the character of 'acoustics'. With Helmholtz experiments met mathematics. As a physiologist, Helmholtz had begun to concern himself with acoustical problems since the 1850s and the essences of his research results were collected in his book, *Tonempfindungen*, or *Sensations of Tone* (1862). Helmholtz's reductionist view on the physiological world enabled him to undertake physiological problems in a physical and mathematical fashion. He explained sensation of tones on the base of Fourier's analysis. His resonators helped him to analyze sounds into simple harmonic tones. Nevertheless, *Tonempfindugen* relegated mathematical treatments to appendices, for he knew that physiologists, who were supposed to be its primary readers, were not accustomed to the mathematical approach. Regardless of physiologists, 'acousticians' still concentrated on experimental investigations and presented mainly qualitative explanations of them without concerning themselves with mathematical description.
3. Characteristics of Rayleigh's *Theory of Sound (TS)*.. Rayleigh (1842-1919), who started his acoustical research by examining Helmholtz's resonators, continued experimental explorations as well as mathematical analyses on vibration with the same mathematical competence that had made him senior wrangler in the Mathematical Tripos at Cambridge University. As a result, he published a monumental treatise in the history of physics, *The Theory of Sound*, in 1877-1878.

The primary purpose of *TS* was to gather and arrange mathematics related to sound. *TS* was an unprecedented mathematics-oriented acoustical text, including plenty of experimental researches on sound so that Rayleigh's mathematical analyses were put on firm empirical foundations.

In chapter 4, Rayleigh deduced Lagrange's equation by generalized coordinates and introduced the dissipation function F. In chapter 5, he presented a general reciprocal theorem of his own. Chapter 14 was remarkably original in involving Rayleigh's own experiments on sound transmission and in chapter 15 we find his own theory on secondary waves. Chapter 16 was distinguished by his own theory of resonators and chapter 17 included his original discussion on sound waves propagating in the air.

In *TS*, Rayleigh tried to connect mathematical analyses and experimental findings. Theoretical analyses were supported or justified by the experimental findings. In the other cases, mathematical results were supposed to be guides or touchstones for future experiments. In this manner, Rayleigh extensively employed methods of approximation, especially successive approximation. And *TS* featured investigations of general theories of vibrations and waves, for example, general theories of light, electrical vibrations, tides, water waves, perturbations of celestial bodies, etc. were widely pursued. The differential equations that appeared frequently throughout (e.g. fundamental wave equation, Laplace's equation, Bessel's equation, etc.) connected corresponding phenomena with each other.

4. Conclusions: The rise of modern acoustics. After the publication of *TS*, Rayleigh's acoustical research maintained its productivity. In order to maximize the practicability of his mathematical discussion, Rayleigh helped a committee of the BAAS to make mathematical tables of various functions. In the last 20 years of the nineteenth century, Rayleigh's influence on acoustical research was tremendous, and *TS* created and spread the impression that acoustics was a unitary research area including both experimental and mathematical investigations. How was this possible?

First of all, the information included in *TS* was remarkably varied and abundant. Acousticians found the subjects of their research in the book and could contribute to the development of the area by adding new elements to Rayleigh's ideas. They also found invaluable information and mathematical materials which could not be found in other acoustical writings.

In addition, the mathematical methods introduced in *TS* became guides for theoretical research in the late 19th and early 20th centuries. Many acoustical researchers took *TS* as the starting point of their investigations. Thus both experimentalists and mathematicians came to study the book and *TS* also provided a gathering place for acoustical researchers. The experimental and mathematical traditions were thus connected and became complimentary in *TS*. Afterwards acoustical researchers followed the style of research manifested in *TS*, investigating and writing textbooks in its style. They became interested in both sides, even if some only did research in one. All acoustical researchers came to recognize that they were working in one field, which we can call 'modern acoustics.'

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Potential theoretical investigations by Carl Neumann and the role of mathematical physics

Karl-Heinz Schlote

Carl Neumann (1832–1925) was a prominent mathematical physicist during the last third of the nineteenth and the first decades of the twentieth century. He contributed to this branch by his articles about potential theory, electrodynamics, analytical mechanics, and hydrodynamics. As a disciple of the famous Königsberg physical-mathematical seminar Neumann became familar with its innovative ideas on the interaction of mathematics and physics. Henceforth, problems in mathematical physics became a central thread in his scientific work. He regarded the application of mathematics to physics, astronomy, and related disciplines as an indispensable part of mathematical research and as a fertile source of new knowledge in mathematics and physics.

Just like many of his contemporaries Neumann prefered potential theory as an appropriate method for solving both pure mathematical problems and problems which were connected to physical applications. He worked hard to improve the mathematical methods used in potential theory. As early as 1861, he solved the two-dimensional boundary value problem or the so called Dirichlet problem in the plane by introducing a logarithmic potential (a term he coined). In the following years he treated some three-dimensional problems of potential theory. All these problems could be connected with physical questions, for instance in the theory of heat or in electrostatics.

Neumann probably had no doubts about the correctness of the Dirichlet principle at that time. There are neither any critical remarks in his publication nor did he avoid the use of this principle, like in an article which was connected with his important book about abelian integrals. Five years later, in 1870, Neumann spoke of the Dirichlet principle, which was rightly declared as questionable, and presented his method of arithmetical means replacing it. However, this was only a sketch of the new method and it was not until 1877 that Neumann described his method in detail in his book "Untersuchungen über das Logarithmische und Newtonsche Potential" (Investigations about the logarithmic and Newtonian potential). In the meantime Neumann dealt with problems of electrodynamics. He looked for a solution of the boundary value problem by a double layer potential and constructed a series of a function by an iteration process. Finally he was able to prove that this series converged to a function, which solved the problem. But it has to be remarked that Neumann's method depended heavily on the geometrical properties of the boundary and his proof worked only for convex surfaces.

Neumann also applied his method to some problems of electrostatics and electrodynamics. It is worth mentioning this because he gave an exact description and solution of the second boundary value problem in this context, probably for the first time.

The theory of logarithmic and Newtonian potential was one of Neumann's most important achievements. It contained not only the solution of the first but also of the second boundary value problem and created a solid base for the treatment of many physical questions by means of potential theory. Therefore it was a important contribution to his building of mathematical physics. The existence of a potential (potential function) had been proven up to now often by physical arguments only, but not mathematically. Beside his investigations in physics Neumann worked on his method of arithmetical means and related topics over and over again. He gave a new systematic presentation of the theory and studied especially the properties of the boundary values. One of the problems tackled by him was the question of the properties of the first and second derivatives of a potential function when the function approaches the boundary.

Neumann made important contributions to this topic, but it was not a great success. A turning point in the development was marked only by Poincaré in 1896. He enlarged the applicability of the method of arithmetical means to simple connected domains. He also introduced the so-called Neumann's series, which never occured in Neumann's own publications. Poincaré's results caused a new impetus for dealing with Neumann's method and by 1906 Neumann could give an new condensed and systematic presentation. This was the starting point of a third phase of Neumann's studies on potential theory. Most of it can be seen as a continuation of former investigations. The boundary value problem with mixed boundary values and the problem for a circular arc are maybe the most interesting ones. But Neumann stuck to concrete analytical methods. Although he pointed out the possibilities of generalizing his ideas and methods, there are not any steps towards abstract investigations like those of Fredholm and Hilbert. At the same time there is not any hint in Neumann's work that he took note of Fredholm's or Hilbert's important works at the beginning of the 20th century.

Appreciating Neumann's contribution to potential theory one can state that he developed a systematical representation of the theory by using very different analytical methods and his method of arithmetical means was a very important step forward in the analysis of the 1870s and 1880s. Neumann also derived interesting new results about the development of a periodic function into a Fourier-series and about the convergence of such series. Above all he had a remarkable impact on the application of potential theoretical methods to physical problems and the creation of a mathematically-determined mathematical physics. His strong mathematical-orientated view on theoretical physics and its application to electrodynamics fostered a clear distiction between both disciplines. Together with his colleagues von der Mühll and Mayer, Neumann made mathematical physics a major topic of both research and lectures at Leipzig university in the last decades of the nineteenth century. They established a tradition which is still alive in Leipzig today.

Green's functions and integral equations: some Italian contributions at the beginning of the 20th century

Rossana Tazzioli

Many questions in mathematical physics lead to the so-called Dirichlet problem: to find a harmonic function U in a closed region D, and with given continuous values w on its boundary such that:

$$\Delta_2 U = \frac{\partial^2 U}{\partial x^2} + \frac{\partial^2 U}{\partial y^2} + \frac{\partial^2 U}{\partial z^2} = 0 \text{ in } D; \quad U = w \text{ on } \partial D.$$

The existence of the solution is based on the Dirichlet principle, which was put in doubt during the second half of the 19th century. Only in 1901, Hilbert proved the validity of Dirichlet's principle if D was sufficiently regular and the given function on the boundary of D was piecewise analytical. Therefore, many 19th-century mathematicians used direct methods in order to overcome the difficulties connected with the Dirichlet principle. One of these methods had already been developed by Green in 1828 by employing the so-called Green function. Green deduced the following formula valid for a harmonic function U in a closed and regular region D (with boundary σ):

$$U(P) = \frac{1}{4\pi} \int_\sigma U(Q) \frac{\partial}{\partial \nu} \left(\frac{1}{r} - G(P,Q) \right) d\sigma$$

where P is a point inside D, and Q is a point on σ, ν is the normal to σ drawn outwards, r is the distance between P and Q, and G is a function to be determined, harmonic in the region D and equal to $\frac{1}{r}$ on the boundary. The function G is called the Green function. For two dimensions, $\log \frac{1}{r}$ has to be considered instead of $\frac{1}{r}$.

Helmholtz, Riemann, Lipschitz, Carl and Franz Neumann, and Betti derived functions similar to Green's function in order to solve problems in different fields of mathematical physics. The method of Green's function (or functions similar to it) is useful, since it overcomes the difficulties arising from the Dirichlet problem and finds the solution of the problem directly. However, Green's method is generally hard to follow — Green's function is indeed difficult to find from a mathematical point of view. In fact, it was only possible to find the Green function for particular regions — when D is a semi-plane, a circle, a sphere, or a cube.

Many mathematicians of the 19th century extended the usual Dirichlet problem to more general cases, by generalizing the definition of harmonic function. A function U of the variables $x_1, x_2, \ldots, x_m$, which is C^2 in a regular region D is *polyharmonic* or *harmonic of n degree* if:

$$\Delta_{2n} U = \left(\frac{\partial^2 U}{\partial x_1^2} + \frac{\partial^2 U}{\partial x_2^2} + \cdots + \frac{\partial^2 U}{\partial x_m^2} \right)^n = 0.$$

If $n = 1$, U is a usual harmonic function; if $n = 2$, U is called a harmonic function of the second degree or *biharmonic* function, and so on. In the case where $m = 2$, biharmonic functions had already been defined by Emile Mathieu in 1869, in order to solve questions related to the theory of elasticity — in particular, questions about elastic equilibrium and vibrations of elastic plates.

In Italy, the theory of biharmonic and polyharmonic functions was largely studied in the beginning of the 20th century. For a biharmonic function U in a three-dimensional region D with boundary σ and external normal ν, the following formula is valid:

$$8\pi U(P) = \int_\sigma \left(U \frac{\partial \Delta_2 (r - G_2)}{\partial \nu} - \Delta_2 (r - G_2) \frac{\partial U}{\partial \nu} \right) d\sigma,$$

where the function G_2 (called *Green's function of the second kind*) is biharmonic in the region D and satisfies the following conditions: $G_2 = r$, $\frac{\partial G_2}{\partial \nu} = \frac{\partial r}{\partial \nu}$ on the boundary. Thanks to this formula, U can be found in each point P of D, if the values of U and $\frac{\partial U}{\partial \nu}$ are given on the boundary σ. Again, in a two-dimensional region, $\log \frac{1}{r}$ has to be considered instead of $\frac{1}{r}$.

The generalized Dirichlet problem for polyharmonic functions is stated in a similar way — the function U is polyharmonic in a region D and its values together with the values of its $n-1$ derivatives $\frac{\partial U}{\partial \nu}, \frac{\partial^2 U}{\partial \nu^2}, \ldots, \frac{\partial^{n-1} U}{\partial \nu^{n-1}}$ are given. By generalizing Green's theorems, the n-th Green's function G_n in a three-dimensional region D is easily introduced — it is a $C^{2n}(D)$ function such that $\Delta_{2n} G_n = 0$ in D, and satisfying the following boundary conditions on σ with normal ν:

$$G_n = r^{2n-3}, \quad \frac{\partial^\alpha G_n}{\partial \nu^\alpha} = \frac{\partial^\alpha r^{2n-3}}{\partial \nu^\alpha} \text{ for } \alpha = 1, 2, 3, \ldots, n-1.$$

It is mathematically difficult to deduce the n-th Green function except for particular cases, when the region D has very simple shapes.

I illustrate some different approaches to the study of polyharmonic functions developed by Italian mathematicians at the beginning of the 20th century.

My aim is to show that:

(1) Many Italian mathematicians studied the generalized Dirichlet problem (for biharmonic and polyharmonic functions) in order to solve special questions in the theory of elasticity;
(2) Most of them used the suitable Green function in order to solve the Dirichlet problem for polyharmonic functions;

(3) In the period around 1905 — just after the publication of the papers by Fredholm, Hilbert, and E. Schmidt on the theory of integral equations — many Italian mathematicians changed their approach and started studying the equations of mathematical physics by using Fredholm's new theory;
(4) Levi-Civita believed that Fredholm's approach was fruitful and easy to apply to mathematical physics, and encouraged mathematicians to use the new theory, as his private correspondence shows.

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G. Peano and M. Gramegna on ordinary differential equations

Erika Luciano

In this preliminary research I will assess the historical and mathematical value of Peano and Gramegna's studies on linear differential equations, focusing on the symbolic and vectorial approach, which I believe make these works interesting.

Giuseppe Peano (1858-1932) taught in the University of Turin for over fifty years, creating a famous School of mathematicians, teachers and engineers. He became Professor of *Calcolo Infinitesimale* in 1890 and he was appointed to the course of *Analisi Superiore* in the academic years 1908-1910. His very large production includes over three hundred writings, dealing with analysis, geometry, logic, foundational studies, history of mathematics, actuarial mathematics, glottology and linguistics.

Maria Paola Gramegna (1887-1915) was a student of Peano in his courses (*Calcolo Infinitesimale* and *Analisi Superiore*) and, under his supervision, she wrote the note *Serie di equazioni differenziali lineari ed equazioni integro-differenziali*, submitted by Peano at the Academy of Sciences in Turin, in the session of the 13 March 1910. This article would be discussed by Gramegna, with the same title, as her graduation thesis in mathematics, on the 7 July of the same year. In 1911 Gramegna became a teacher in Avezzano, holding a secondary school appointment at the Royal Normal School. Four years later, on the 13 January 1915, she died, a victim of the earthquake which destroyed that town.

The study of the articles by Peano and by Gramegna on systems of ordinary linear differential equations presents interesting implications. In the winter of 1887, Peano was able to deal with these systems for the first time in a rigorous way, and he submitted an article entitled *Integrazione per serie delle equazioni differenziali lineari* to the Academy of Sciences of Turin. A slightly modified version of this note, in French, would be published the following year in the *Mathematische Annalen.* Here he applied the method of "successive approximations" or "successive integrations" — as Peano preferred to call it — based on the theory of linear substitutions.

The purpose of Peano's article is to prove the following theorem: let there be n homogeneous linear differential equations in n functions $x_1, x_2, \ldots, x_n$ of the variable t, in which the coefficients α_{ij} are functions of t, continuous on a closed and bounded interval $[p, q]$:

$$\begin{aligned} \frac{dx_1}{dt} &= \alpha_{11}x_1 + \alpha_{12}x_2 + \cdots + \alpha_{1n}x_n, \\ \frac{dx_2}{dt} &= \alpha_{21}x_1 + \alpha_{22}x_2 + \cdots + \alpha_{2n}x_n, \\ &\cdots \\ \frac{dx_n}{dt} &= \alpha_{n1}x_1 + \alpha_{n2}x_2 + \cdots + \alpha_{nn}x_n. \end{aligned}$$

Substitute in the second members of the equations, n arbitrary constants $a_1, a_2, \ldots, a_n$, in place of $x_1, x_2, \ldots, x_n$, and integrate from t_0 to t, where $t_0, t \in (p, q)$. We obtain n functions of t, which will be denoted by $a'_1, a'_2, \ldots, a'_n$. Now substitute in the second members of the proposed differential equations, $a'_1, a'_2, \ldots, a'_n$ in place of $x_1, x_2, \ldots, x_n$. With the same treatment we obtain n new functions of t, which will be denoted by $a''_1, a''_2, \ldots, a''_n$. Repeating this process, we obtain

$$\begin{gathered} a_1 + a'_1 + a''_1 + \ldots, \\ a_2 + a'_2 + a''_2 + \ldots, \\ \cdots \\ a_n + a'_n + a''_n + \ldots. \end{gathered}$$

The series are convergent throughout the interval (p, q). Their sums, which we shall denote by $x_1, x_2, \ldots, x_n$ are functions of t and satisfy the given system. Moreover, for $t = t_0$, they assume the arbitrarily chosen values $a_1, a_2, \ldots, a_n$.

In order to prove the preceding theorem, Peano introduces vectorial and matrix notations and some sketches of functional analysis on linear operators.

In 1910, Maria Gramegna again took up the method of successive integrations, in order to generalize the previous theorem to systems of infinite differential equations and to integro-differential equations. The original results exposed by Gramegna in the above-mentioned note *Serie di equazioni differenziali lineari ed equazioni integro-differenziali*, are an important example of the modern use of matrix notation, which will be central in the development of functional analysis. Moreover, the widespread application of Peano's symbolic language gives her work a modern slant. Gramegna proves the following extension of Peano's theorem: we consider an infinite system of differential linear equations with an infinite number of unknowns:

$$\frac{dx_1}{dt} = u_{11}x_1 + u_{12}x_2 + \cdots + u_{1n}x_n + \ldots$$
$$\frac{dx_2}{dt} = u_{21}x_1 + u_{22}x_2 + \cdots + u_{2n}x_n + \ldots$$
$$\ldots$$

where the u_{ij} are constant with respect to time. Let us denote by A the substitution represented by the matrix of the u's. Let x be the sequence $(x_1, x_2, \ldots)$ and x_0 its initial value. We may write the given differential equations as $Dx = Ax$, and the integral is given by $x_t = e^{tA}x_0$, where the substitution e^{tA} has this representation:

$$1 + tA + \frac{t^2A^2}{2!} + \frac{t^3A^3}{3!} + \ldots.$$

In the last section of the article Gramegna applies the new analytic tools she has introduced (the concept of *mole*, the exponential of a substitution, etc.) in order to solve integro-differential equations, already studied by I. Fredholm, V. Volterra and E. H. Moore.

Unfortunately, the note by Gramegna did not have a large circulation. This was probably due to the difficulty, for many mathematicians, of understanding research in advanced analysis presented with Peano's logic symbolism. Besides, this is the last work in *Analisi Superiore* realised under the supervision of Peano, who was dismissed from the course in 1910, some days after the submission of Gramegna's article to the Academy, thereby losing the possibility of training other researchers.

This historical study will continue with the aim of investigating the possible influences of Gramegna's article on research in functional analysis in Italy and abroad.

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A shift in the definition of stability: the Poincaré recurrence theorem

Anne Robadey

In 1888, Poincaré submitted a paper to the competition honoring the 60th birthday of King Oscar II of Sweden and Norway (see [1] and [2]). In that paper, stability was an important issue. Poincaré thought he had "rigorously proven" the stability in the restricted three-body problem. By that he meant that he had proven the existence of many tori, some of them very thin, which contain for all times trajectories with initial point in them. Unfortunately, that result was soon discovered to be incorrect.

In 1889, by the time Poincaré became aware of the error, the paper — which had won the prize — was printed but not yet published [4]. He worked swiftly to write a corrected paper, which was finally published in *Acta mathematica* in 1890 [5]. It was soon followed by two summaries in early 1891: one in the *Bulletin astronomique* [7], presenting for the astronomers the results contained in the 1890 paper, and the second in the *Revue générale des sciences* [6], for an even larger public.

The three stages of that work, 1889, 1890 and 1891, show a very interesting evolution of the notion of stability Poincaré stressed, from the above mentioned one to that given by the so-called recurrence theorem (not Poincaré's word).

The recurrence theorem states that under suitable conditions, verified by the restricted three-body problem, there exists in each region, however small, trajectories coming back to that region an infinite number of times. In the 1889 paper, it occupied a second-rank place. Indeed, it was not mentioned at all in the introduction. Moreover, the recurrence property was presented as a "second sense" of stability, and the recurrence theorem for the restricted three-body problem was shown to result from the stability "in the first sense". Thus it was quite overlayed by the above mentioned result of stability "in the first sense".

By 1890, in contrast, with Poincaré no longer able to prove stability "in the first sense", the recurrence theorem came to the fore. At the same time, it was reinforced with a corollary, asserting that the non-recurrent trajectories are exceptional, that is, their probability is zero. The corresponding notion of stability was now called stability "à la Poisson" instead of stability "in the second sense". And the recurrence theorem, shown to hold for the restricted three-body problem, replaced the foremost "rigourous proof of stability" in the discussion where Poincaré emphasized the improvement of his memoir in comparison to Hill and Bohlin's ones.

That evolution is confirmed by the two summaries of 1891. There, the recurrence theorem, previously included in the part concerning invariant integrals as an application, was now under the title "stability". Moreover, the corollary added in 1890 was strengthened even further: Poincaré stated that non-recurrent trajectories are of probability zero not only when computed with a uniform probability distribution, but also with any continuous probability distribution. In addition, the evocation of the work of Poisson and Lagrange on stability in the solar system, contained in the introduction to the 1889 paper, just before the announcement of the stability result, and which had disappeared in 1890, reappeared in one of the summaries as an introduction to the recurrence theorem.

Thus, the recurrence theorem becomes, quite implicitly in 1890, and very openly in 1891, *the* result of stability in Poincaré's work on the three-body problem. Correlatedly, Poincaré improved his presentation of that result in two ways. Firstly by mathematically strengthening it with the corollary. And secondly by an historical legitimization, mainly by showing the shared properties of his notion of stability with characteristics of the stability results proven by Poisson and Lagrange.

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Differential Equations as a Leitmotiv for Sophus Lie

David E. Rowe

After reviewing recent historiography and the highlights of Lie's unusually dramatic career, this talk concentrated on some key stages in Lie's early work that underscore the centrality of differential equations for his vision of mathematics. A great deal more about this topic can be found in the recent studies of Thomas Hawkins (see bibliography). Lie met Klein in Berlin in the winter semester of 1869 when they attended Kummer's seminar. There Klein presented some of Lie's earliest results on so-called tetrahedral line complexes. These are special 3-parameter families of lines in projective 3-space with the property that they meet the four coordinate planes in a fixed cross-ratio.

Initially, Lie and Klein studied the PDEs associated with these tetrahedral complexes. Geometrically, this meant studying surfaces tangential to the infinitesimal cones determined by a tetrahedral complex, which leads to a first-order PDE of the form:

$$f(x,y,z,p,q)=0, \quad p=\frac{\partial z}{\partial x}, \quad q=\frac{\partial z}{\partial y}.$$

Lie used a special transformation to map this DEQ to a new one

$$f=0 \quad \rightarrow \quad F(X,Y,Z,P,Q)=0$$

which he showed was left invariant by the 3-parameter group of translations in the space (X,Y,Z). This enabled him to reduce the equation to one of the form:

$$F(P,Q)=0$$

which could be integrated directly.

This result soon led Lie to the following insights:

(1) PDEs of the form $f(x,y,z,p,q)=0$ that admit a commutative 3-parameter group can be reduced to the form $F(P,Q)=0$.
(2) PDEs that admit a commutative 2-parameter group can be reduced to $F(Z,P,Q)=0$.
(3) PDEs that admit a 1-parameter group can be reduced to $F(X,Y,P,Q)=0$.

Lie noticed that the transformations needed to carry out the above reductions were in all cases contact transformations. Earlier he had studied these intensively, in particular in connection with his line-to-sphere transformation, which maps the principle tangent curves of one surface onto the lines of curvature of a second surface. Thus, a large part of Lie's inspiration for his early work on PDEs came from realizing that contact transformations can be used to link two spaces, thereby revealing a deep interplay between the differential geometry of various surfaces in the two spaces under consideration. His uncanny ability to visualize such possibilities made a profound impression on Klein, who would later often describe Lie's audacious thinking in his Göttingen seminars to illustrate what he meant by *anschauliches Denken*.

Lie's work on ODEs began somewhat later, around 1873. According to Friedrich Engel, however, Lie had already realized in 1869 that an ordinary first-order DEQ

$$\alpha(x,y)\,dy - B(x,y)\,dx = 0$$

can be reduced to quadratures if one can find a one-parameter group that leaves the DEQ invariant. By 1872 Lie saw that it was enough to have an infinitesimal transformation that generated the 1-parameter group. Thus if the DEQ $X\,dy - Y\,dx = 0$ admits a known infinitesimal transformation T:

$$\zeta\frac{\partial f}{\partial x} = \eta\frac{\partial f}{\partial y}$$

in which, however, the individual integral curves do not remain invariant, then the DEQ has an integrating factor. As was typical for Lie, he gave a geometrical interpretation of these integrating factors in terms of the area of the rectangle spanned by the pairs of vectors tangent to the integral curves and the normal vector linking each integral curve to its image curve under T. This interpretation enabled him to give a general explanation for what it means for an ODE to admit an integrating factor as well as giving a criterion for the existence of the same.

In the case of PDEs, Lie had no trouble extending his geometrical notions of surface elements, contact transformations, etc. to n-dimensional space in order to deal with PDEs of the form:

$$f(z, x_1, x_2, \ldots, x_n, p_1, p_2, \ldots, p_n) = 0, \quad p_i = \frac{\partial z}{\partial x_i}.$$

Thus, in 1872 he defined a general contact transformation

$$T : (z, x, p) \to (Z, X, P)$$

analytically: T is a contact transformation if the condition

$$dz - (p_1\, dx_1 + p_2\, dx_2 + \cdots + p_n\, dx_n) = 0$$

remains invariant under T. He showed further that two first-order PDEs can be transformed to another by means of a contact transformation.

Geometrical ideas continued to motivate Lie's whole approach to PDEs when he took up this topic in earnest in 1873. In his "Nova methodus" Jacobi had introduced the bracket operator

$$(\phi, \psi) = \sum \left[\frac{\partial \phi}{\partial p_i} \frac{\partial \psi}{\partial x_i} - \frac{\partial \phi}{\partial x_i} \frac{\partial \psi}{\partial p_i} \right]$$

within his theory of PDEs. This was a crucial tool for reducing a non-linear PDE to solving a system of linear PDEs. Lie interpreted the bracket operator geometrically, borrowing from Klein's notion of line complexes that lie in involution. He thus defined two functions

$$\phi(x, p), \psi(x, p), \quad x = x_1, x_2, \ldots, x_n, \quad p = p_1, p_2, \ldots, p_n$$

to be in involution if $(\phi, \psi) = 0$. Lie then showed that a system of m PDEs

$$f_i(z, x_1, x_2, \ldots, x_n, p_1, p_2, \ldots, p_n) = 0, \quad i = 1, 2, \ldots, m$$

satisfying $(f_i, f_j) = 0$ remains in involution after the application of a contact transformation. Such considerations led Lie to investigate the invariant theory of the group of all contact transformations.

A clear idea of the centrality of differential equations and differential invariants for Lie's mature vision can be found in the preface to the third volume of his *Theorie der Transformationsgruppen*, which appeared in 1893. Therein he described a forthcoming work on differential invariants and continuous groups with applications to differential equations, a study he planned to publish with the assistance of Engel, but which never appeared. He also noted plans to resurrect his early work on the geometry of contact transformations with the help of Georg Scheffers, a partially realized project that led to *Geometrie der Berührungstransformationsgruppen* (1896). As Lie's preface makes clear, both works were partly motivated by an effort to create a legacy independent of the one that had begun to emerge with the republication of Klein's "Erlangen Program" in the early 1890s (see Hawkins' article from 1984). In the 1893 preface, Lie dismisses Klein's notion of group invariants as essentially irrelevant to his own research program. There he writes: "one finds hardly a trace of the all important concept of differential invariant in Klein's Program. Klein took no part in creating these concepts, which first make it possible to found a *general* theory of invariants, and it was only from me that he learned that every group defined by differential equations determines differential invariants that can be found by integration of complete systems."

At the same time, Lie tried to link his work to that of leading figures within the French community beginning with Galois. Thus, he highlights the importance of his relationship with Camille Jordan; he thanks Darboux for promoting his geometrical work, Picard, for being the first to recognize the importance of Lie's theory for analysis, and Jules Tannery for sending a number of talented students from the École Normale Supérieure to study with him in Leipzig. He also expresses his gratitude to Poincaré for his interest in numerous applications of group theory. Lie emphasizes that he was "especially grateful that he [Poincaré] and later Picard stood with me in my fight over the foundations of geometry, whereas my opponents tried to ignore my works on this topic." Not surprisingly, Lie's break with Klein caused a major scandal within the German mathematical community. Two differing interpretations of this can be found in the biography by Stubhaug and in my earlier article in NTM.

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Beyond the Gravitational Field Equations of General Relativity: Einstein's Search for World Equations of a Unified Theory and the Example of Distant Parallelism Geometry

Tilman Sauer

Einstein's gravitational field equations of general relativity were published in late November 1915 after eight years of intense research. Historically and systematically the Einstein equations represent a generalization of the Poisson equation of Newtonian mechanics, and Einstein published a number of intermediate field equations of gravitation along his path toward general relativity between 1912 and 1915 (see [1], [2] and references cited therein).

Surprisingly, Einstein soon began to consider and investigate modifications of the field equations, e.g. by adding the cosmological constant in 1917. While this modification, of course, embraces the original Einstein equations as a special case, it was not initially conceived of as a generalization but rather as an abandonment of the original gravitational field equations. In 1919, Einstein tentatively added the trace term with a factor of $1/4$ instead of $1/2$ in an attempt to account for the structure of matter. Here the idea was that gravitational forces might account for the stability of the electron. The question therefore arises as to the status of the Einstein equations in Einstein's own research program.

A closer look at his later publications then shows that Einstein investigated and published a considerable number of different field equations combining both the gravitational and electromagnetic fields in attempts to arrive at a unified theory. None of them, however, allowed him to satisfy the demands of his unified field theory program [3]. These demands included: a) the existence of a unified representation of the gravitational and electromagnetic fields that would be not only purely formal, i.e. that would imply some kind of mixing of the two fields but still be compatible with our empirical knowledge, b) the explanation of the existence of two elementary particles, the electron and the proton, and specifically, of the existence of a fundamental electric charge and of the mass asymmetry of the electron and the proton, and c) the possibility of accounting for the features of quantum theory on a foundational level by means of a classical field theory.

Although Einstein did not use the term himself, we believe that one may characterize his program as a search for "world equations" in the sense that the phrase was used by Hilbert in a series of lectures delivered in Hamburg in 1923 [4]. "World equations" would be differential equations for the respective variables representing the fields and they would, in principle, suffice to deduce the whole edifice of physics without the necessity of further independent laws or assumptions.

As an example of the dominance of the problem of finding field equations in Einstein's unified field theory program, we discuss his approach of distant or absolute parallelism [5, sec. 6.4], [6]. This approach was pursued by Einstein in a number of papers that were published between summer 1928 and spring 1931. The crucial new concept, for Einstein, that initiated the approach was the introduction of the tetrad field, i.e. a field of orthonormal bases of the tangent spaces at each point of the four-dimensional manifold. The tetrads were introduced to allow the distant comparison of the direction of tangent vectors at different points of the manifold, hence the name distant parallelism. From the point of view of a unified theory, the specification of the four tetrad vectors at each point involves the specification of sixteen components (in four-dimensional spacetime) instead of only ten for the symmetric metric tensor. The idea then was to exploit the additional degrees of freedom to accommodate the electromagnetic field. Mathematically, the tetrad field easily allows the conceptualization of more general linear affine connections, in particular, non-symmetric connections of vanishing curvature but non-vanishing torsion.

The mathematics of generalized Riemannian spaces with non-vanishing torsion had been developed before in the early 1920's by mathematicians like Elie Cartan, Roland Weitzenböck, Luther Pfahler Eisenhart, and others [5] but Einstein was initially unaware of these works. For him the concept of a tetrad field opened up new and as yet unexplored ways to represent the gravitational and electromagnetic fields in terms of the components of the tetrads.

Einstein quickly focused on the problem of finding field equations for the components of the tetrads, which he conceived of as the fundamental dynamical variables. Since the difference between distant parallelism geometry and simple Euclidean geometry was characterizd by non-vanishing torsion, he first tried to derive field equations from a variational principle with a generally covariant Lagrangian that involved the torsion tensor quadratically. Einstein soon learned from Weitzenböck that there are, in fact, three invariants quadratic in the torsion and found that his initial approach could not determine field equations without ambiguity. When he also found that he could not fully establish compatibility with Maxwell's equations in linear approximation, he tried to derive field equations along a different strategy.

Starting from identities for the torsion tensor that would imply validity of the Maxwell equations from the beginning, he motivated a set of overdetermined field equations that were suggested from the form of those identities. But when it was pointed out to him that he had only assumed and not, in fact, shown the compatibility of the overdetermined set of equations, he returned to the strategy of deriving field equations from a variational principle. But again he came to realize that this approach would not produce acceptable field equations and finally reverted to the strategy of motivating field equations from a set of identities that would also allow him to establish the compatibility of the field equations.

All in all, Einstein published some half dozen field equations in the first year of investigating the distant parallelism geometry. In the mature stage of this approach, when he had settled on a set of field equations that seemed to satisfy his demands, he tried to find particle-like solutions to those equations and to improve on the compatibility proof as well as on their derivation. Significantly, the episode ends with a paper, written jointly with his collaborator Walther Mayer, in which they investigate systematically the possibilities of imposing compatible field equations on a distant parallelism spacetime. Frustrated by the impossibility of finding uniquely determined "world equations" of a unified theory along this approach, he eventually gave it up in favour of another approach characterized by a field of five-dimensional vector spaces defined over four-dimensional spacetime.

This account of the distant parallelism episode as a search for field equations shows some remarkable historical and systematical similarities to Einstein's early search for gravitational field equations in the years 1912–1915 [6] (for an account of Einstein's path toward general relativity along these lines, see [7], [2]). In both cases, it was a mathematical concept that triggered and determined further investigations. In 1912, it was the insight into the crucial role of the metric tensor, and in 1928 it was the concept of a tetrad field that opened up new possibilities of achieving the heuristical goals. In either case, the mathematics associated with those concepts had already been explored to a great extent in the mathematics literature, and it was through the mediation of mathematicians that Einstein learnt about it. In both episodes, Einstein's heuristics quickly focused on the problem of finding acceptable field equations, and in both episodes he was unable for a while to find field equations that would not violate one or more of his heuristic requirements. As a result, he developed two complementary strategies to generate candidate field equations, and in either case he eventually found equations that seemed to satisfy all his demands, at least to the extent that he was initially able to verify this. The demise came in each case through the realization of more and more problems and then, finally, by switching to an altogether different conceptual framework. But while in 1915 the new approach was marked by his final breakthrough to general relativity and resulted in the still valid Einstein equations, the distant parallelism episode was only followed by yet another approach along his unified field theory program. Systematically, the difference between success and failure is to be found in the different goals that he was trying to achieve.

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The Brouwer fixed point theorem and differential equations: a nonlinear story

Jean Mawhin

In 1912, Brouwer ended his paper initiating topological degree theory for continuous mappings between manifolds of the same dimension [4] with the following fixed point theorem.

Theorem 1. *An n-dimensional element is any one-to-one and continuous image of a simplex S of n-dimensional space. Any single-valued and continuous transformation of an n-dimensional element into itself has at least one fixed point.*

In 1910, in an appendix to the second edition of Tannery's book on function theory, extending Kronecker's index to continuous functions [7], Hadamard proves Theorem 1 and calls it Brouwer's theorem.

About thirty years earlier, in 1883, Poincaré [13] had reduced the problem of finding some symmetric periodic solutions of the three body problem to the solution of a nonlinear system of equations in a finite number of unknowns, for which he states and sketches (using Kronecker's index) the proof of the following n-dimensional intermediate value theorem.

Theorem 2. *Let $X_1, X_2, \ldots, X_n$ be n continuous functions of n variables $x_1, x_2, \ldots, x_n$; the variable x_i is supposed to vary between the limits $+a_i$ and $-a_i$. Suppose that X_i is constantly positive for $x_i = a_i$, and constantly negative for $x_i = -a_i$. There exists at least one system of values of the x_i satisfying the inequalities $-a_1 < x_1 < a_1$, $-a_2 < x_2 < a_2, \ldots, -a_n < x_n < a_n$, and the equations $X_1 = X_2 = \ldots = X_n = 0$.*

In 1904, in a paper devoted to the discussion of the nature of trajectories of a mechanical system around an equilibrium [2], Bohl stated and proved the following result, equivalent to the non-retraction theorem for the cube, rediscovered for a ball in 1931 by Borsuk.

Theorem 3. *Let (G) be the domain $-a_i \leq x_i \leq a_i$ $(i = 1, 2, \ldots, n;\ a_i > 0)$. There do not exist functions $F_1, F_2, \ldots, F_n$ defined and continuous in (G) which do not vanish simultaneously and are such that, on the boundary of (G), one has $F_i(x_1, \ldots, x_n) = x_i$, $(i = 1, 2, \ldots, n)$.*

In 1911 in a paper proving the invariance of dimension [3], Brouwer introduced and used the following result.

Theorem 4. *If a continuous mapping in a q-dimensional space transforms a cube of dimension q in such a way that the maximum displacement is smaller than half of the side of the cube, then there exists a homothetic and concentric cube which is entirely contained in the image of the first cube.*

In 1922, motivated by existence questions for multipoint boundary-value problems for ordinary differential equations, Birkhoff and Kellogg gave another proof of the Brouwer fixed point theorem, before extending it to the function spaces $C([a,b])$ and $L^2([a,b])$ [1]. Between 1927 and 1930, in order to prove the existence of solutions to some semilinear Dirichlet problems, Schauder extended the Brouwer fixed point theorem to continuous mappings between compact convex sets of Banach spaces [14]. In 1930, motivated by existence results for multipoint boundary-value problems for ordinary differential equations, Caccioppoli independently rediscovered Birkhoff-Kellogg's fixed point theorem in $C([a,b])$ [5], and recognized the priority of those authors in 1931.

In 1929, in a proof of a global Cauchy problem for a system of ordinary differential equations [8], Hammerstein made use of Brouwer's Theorem 4. In 1931, to study a two-point boundary value problem for a second order equation [15], Scorza-Dragoni introduced the shooting method, reducing the problem to the classical intermediate value problem for real functions of one variable. He also suggested the possibility of dropping the regularity conditions by using "the method of Caccioppoli and Birkhoff". In 1940, applying the shooting method to multi-point boundary value problems for differential equations of order greater than two [6], Cinquini rediscovered (with wrong or incomplete proofs) Poincaré's intermediate value theorem. In 1941, to give a sound basis to Cinquini's results, Miranda proved the equivalence of Brouwer's fixed point theorem and Cinquini's (i.e. Poincaré's) intermediate value theorem [12]. Poincaré's priority would only be discovered in 1974. Despite this equivalence, a ten-year polemic (1941-1950) took place between Cinquini and Scorza-Dragoni, about the elementary character of a proof based upon shooting and the multi-dimensional intermediate-value theorem versus the topological character of a proof using the Schauder fixed point theorem!

The first explicit application of the Brouwer fixed point theorem to problems of differential equations was made in 1943 by Lefschetz [10] and by Levinson [11], to prove the existence of periodic solutions of a periodically forced Liénard equation. As late as 1966, to study variational inequalities, Hartman and Stampacchia [9] stated and proved another statement equivalent to Brouwer's fixed point theorem:

Theorem 5. *Let $\mathcal{C}$ be a compact convex set in E^n and $B(u)$ a continuous mapping of $\mathcal{C}$ into E^n. Then there exists $u_0 \in \mathcal{C}$ such that $(B(u_0), v - u_0) \geq 0$ for all $v \in \mathcal{C}$, where $(\cdot,\cdot)$ denotes the scalar product in E^n.*

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From Partial Differential Equations to Theory of Control : Dynamics of Research and Social Demands. The privileged example of Jacques-Louis Lions (1928–2001)

Amy Dahan

The aim of this lecture was to present synthetically the evolution of the field of partial differential equations and control theory in France in the second half of the 20th Century. We focused on the privileged example of the mathematical work of Jacques-Louis Lions, mentioning also his school and the collective character of his action. We started from the first (theoretical) works in the 1950s, reaching his last papers in the 1990s on climate systems and questions of algorithms and resolution on parallel computers.

We emphasized the close interactions between the dynamics of mathematical research and industrial and social demands, which both contributed to reshape the field of research. In other words, the evolution of the scientific field is the result of a process of constant and narrow hybridation between two lines of development: 1) the development of an academic research program which was coherent by the beginning of the 1950s, used the same privileged methods (variational methods, interpolation spaces, algorithmic methods of finite elements etc.) and revealed the idiosyncrasy of the mathematician; 2) the increasing importance of various industrial and social problems (in mechanics, physics, engineering, economics, climatology, . . .) which stimulated the birth of specific methods, and contributed to new directions of investigation. We must also study this process in the context of the evolution of computing facilities, and a changing institutional picture.

The lecture was divided into four parts and I give here a brief overview of the mentioned elements.

1. A mathematician coming from the "Elite Club" of pure French mathematics.

- He graduated from the Ecole normale supérieure (Ulm). The importance of Bourbaki's abstract mathematical style.
- His mentor was Laurent Schwartz, one of the leader of the second generation of Bourbaki's group.
- He began with linear problems, mixed problems of evolution.
- Then, he oriented his interests towards ill-posed problems, also non-linear questions (meeting with Jean Leray).
- Importance of the meeting with R. Lattès from the Sema: how to optimize the shape of machines, of tools, of constructions, etc). This led them to the method of quasi-reversibility.

2. Some turning points in his early career.

- 1957-58 : numerical resolution and theroretical resolution began to become closely associated; thesis of his first student Jean Céa (method of finite elements, Céa's lemma. . .) tested on the computer of the SEMA (Société d'Economie et de Mathématiques Appliquées).
- 1964-65 : the emergence of a second aim : the optimal control of distributed systems.
- Theory of control was in the Zeitgeist (spatial competition, principle of maximum of Pontrjagin, works of Rudy Kalman, and of Richard Bellman in the US.) but these results concerned ordinary differential equations.
- Lions.
- At the end of the 1960s, a team was ready to tackle industrial numerical problems.

3. 1970's, College de France, IRIA : consecration of applied mathematics and dialog with the industrial world.

- Comment on the title of Lions' chair at the College de France : "Analyse des Systèmes mathématiques et leur Contrôle"
  - the appearance of specific techniques (variational inequalities, . . .) adapted to different concrete problems.
  - the importance of some industrial questions which gave birth to mathematical concepts and innovations.
- Lions tried to organize in a systematic manner a cooperation with other fields (physics, mechanics, fluid mechanics, chemistry, economics, . . .) and with industry. In his team, he gathered a whole spectrum of skills: from very theoretical and abstract people (like H. Brézis) to very applied mathematicians.
- Among numerous collaborators, we can mention:
  - R. Glowinski, specialist of optimization and fluid problems, tried to adapt mathematical methods and problems to computers of medium

power, although they needed much bigger machines. Here the principal partners are the CEA (Commissariat à l'Energie Atomique) and the Institut of Novossibirsk.
   - Chavent : Specialist in petroleum problems; worked on the inverse problems of identification theory (contracts with Elf, etc).
   - Yvon: optimal control of problems of combustion and temperature (important for nuclear centrals).
   - O. Pironneau, who worked (with Glowinski, Périaux, Lions himself) on aeronautics. M. Dassault on optimum design of wings, nose, etc for reducing turbulence. Thanks to these collaborations with Lions's team, Dassault constructed the first aircraft, entirely conceived by numerical methods. This was a big success for French Industry.

4. 1985- 2000 : new spaces, new problems.
   - Lions moved in 1985 as chairman of the CNES.
   - He was preoccupied by the control and stabilization of flexible structures, and modeling of optimal design of combustion chambers (because of two failures in 1985 and 1986).
     - theory of controllability (first exact controllability, then approximated controllability) : famous Hilbert Uniqueness Method, at the "John von Neumann Lecture" at SIAM Congress in 1986.
   - At the end of 1980s, he was acquainted with the Global Change Program:
     - theory of sentinels,
     - new global models for the atmosphere and for the coupled atmosphere-ocean system,
     - approximate controllability of turbulence or chaotic systems.
   - Environmental questions directly connected to energy and economic aspects.
   - Privilegied partners : CEA, EDF.

Concluding Remarks.

On the Habilitation Lecture of Lipót Fejér

Barnabas M. Garay

The Hungarian mathematician Lipót Fejér (1880–1959) is famous for his work on Fourier series, approximation and interpolation theory. It is not widely known that his Habilitation Lecture, "Stability and instability investigations in the mechanics of mass point systems" (University of Kolozsvár; 23 June 1905; Kolozsvár [Cluj/Klausenburg, Transsylvania]) was devoted to certain aspects of the theory of ordinary differential equations.

This choice was obviously determined by Fejér's mathematical environment in Kolozsvár — variational principles of mechanics, formal integrability of first order and second order partial differential equations, shock waves. His colleagues at the department were Gyula Vályi (the first mathematician to receive a Ph.D.

in Hungary), Gyula Farkas (note that The Farkas Lemma [an early version of János Neumann's Minimax Theorem as well as of the Duality Theorem of Linear Programming, an existence result on linear inequalities] is rooted in the theory of one–sided mechanical constraints), and Lajos Schlesinger (the author of the monumental *Handbuch der Theorie der linearen Differentialgleichungen* [Teubner, Leipzig, 1895/1898], the son–in–law of the Berlin mathematician Lazarus Immanuel Fuchs).

Fejér's Habilitation Lecture was originally printed in Hungarian. Together with a German translation, it was reprinted as item 14 of the *Gesammelte Abhandlungen.*

The Habilitation Lecture consists of three parts.

Based on related works by Hill and Poincaré, the first part is a description of various aspects of the three–body problem in celestial mechanics. The second part is a discussion of various stability concepts: *"The concept of stability carries highly different contents even within the framework of mass point systems. It is no use arguing which one of them can be the best since, except for some inherent features of it, stability as a popular concept is so indefinite and so relative that, owing to the variety of existing relations, stability definitions highly differing from one another may be formulated without any contradiction to the popular one."* Among the definitions of stability listed by him we can find the one accepted in general nowadays, too, but it is considered too narrow by Fejér, sharing Felix Klein's opinion (who seems to have identified instability as something exceptional, irregular, and turbulent).

The third part is devoted to the Lagrange–Dirichlet theorem with a particular emphasis on its possible converse. In an accompanying paper (Über Stabilität und Labilität eines materiellen Punktes im widerstrebenden Mittel, *Journal für die reine und angew. Math.* 131(1906), 216–223.), Fejér gives sufficient conditions for instability. This is more an example than a general result. (Moreover, as a simple consequence of La Salle's invariance principle, a great part of his sufficient conditions can be omitted.)

It is worth mentioning here that the discovery of the famous summation theorem is closely related to the Dirichlet problem on the unit disc and goes back to a question of Hermann Amandus Schwarz on Poisson integral representations. During his long life, Fejér had always been aware of the relations between his primary research fields and differential equations, both ordinary and partial. It is also worth mentioning here that the Kolozsvár tradition of differential equations had been continued by Alfréd Haar, Fejér's successor in Kolozsvár/Szeged from 1911 onward, with work on the Fredholm alternative for the biharmonic operator; existence, uniqueness, & regularity for the minimal surface problem under Hilbert's three-point condition; the two–dimensional counterpart of the Du Bois–Reymond lemma in the calculus of variations; the well–posedness of general first-order partial differential equations via a Gronwall–type inequality.

The interested reader can find many more details in the differential equations chapter (written jointly with my late colleague Árpád Elbert) of the forthcoming two–volume presentation of the history of the Hungarian mathematics in the first part of the twentieth century — a truly terrific time.

The End of Differential Equations, or What Can a Mathematician Do that a Computer Cannot?

David Aubin

In 1971, two outsiders, a physicist specializing in statistical mechanics and a mathematician who studied dynamical systems, shocked the fluid dynamics community when they published a controversial article titled "On the Nature of Turbulence". Claiming nothing less than a new "mechanism for the generation of turbulence," the authors, going against current practice, never explicitly wrote down the Navier-Stokes equations (NSE) [10], [1], [2]. For centuries, physicists had aimed at unveiling laws of nature. Following in the footsteps of Newton, they exploited his second law ($F = ma$) with great success. In this paper, I will focus most explicitly on the case of fluid dynamics. Just as Newton had uncovered the dynamical equations governing the motion of planets in the heavens, physicists and mathematicians in the first half of the nineteenth century were able to derive from first principles mathematical relations for fluid flow. Although, except for a few simple cases, it was impossible to exhibit exact solutions to NSE, this derivation had become an inescapable part of the classical physics curriculum.

This talk aims at providing an account of the changes in physical modeling which made it possible that a new model of the onset of turbulence could be proposed without its authors ever feeling the necessity of mentioning the law found a century and a half earlier by Claude Louis Navier and Sir George G. Stokes. Inspired by René Thom's ideas, conceived and written at the Institut des hautes études scientifiques in the spring of 1970 by the French physicist David Ruelle and the Dutch mathematician Floris Takens, the article is remarkable for several reasons reaching beyond its introduction of the famous notion of *strange attractors*, which was to have a very bright future. Above all, Ruelle and Takens's article supplies both a *symptom* and a *direct cause* for crucial changes that have been widely affecting the modeling practice of theoretical physics ever since.

Based on first principles coming from either molecular hypotheses or continuum mechanics, the partial differential equations of physics acquired, in the course of the nineteenth century, an almost ontological status. A telling and much studied instance of this process, which can be seen as originating in Fourier's analysis of heat flow, is provided by the rise of the notion of fields. For Maxwell and Boussinesq, the complex diversity of behaviors exhibited by solutions to partial differential equations reinforced ontological commitments to them [5], [3], [8], [4].

But the question of the relation between microscopic, molecular theories and macroscopic, continuous differential equations always spurred passionate debates. As far as macroscopic physics was concerned, the exploitation of fundamental laws, derived from general principles and expressed by differential equations, only partially justified by statistical and quantum mechanical considerations, remained the physicists' dominant foundation for their modeling practice.

In this context, the turbulence problem for fluid mechanics was a distressing one. Whereas, in traditional histories of physics, the discovery of an equation has often been the culminating point, the history of turbulence started with the equation. Indeed, only when this equation existed did turbulence become a theoretical problem. On the one hand, there was every reason to believe that NSE provided a faithful description of classical fluid flows. On the other hand, it was an experimental fact that extremely complex flows arose when the fluid was submitted to intense external stress; this complexity was called turbulence. The turbulence problem lay in the relation between fundamental equations and their solutions. To bridge the chasm dividing the Navier-Stokes equations from feasible experiments or known solutions, was the "turbulence problem." [7]

For a long time, when unable to solve the equations explicitly, physicists had few mathematical tools which still could have enabled them to account for natural phenomena in a satisfactory manner. Historically, Ruelle and Takens's article signaled the reencounter of physics with qualitative mathematics. It would help to initiate a powerful alternative to the endless quest for the final law of nature. Instead, more and more physicists started to look anew into mundane phenomena, without relying too heavily on fundamental laws. These laws, they began to think, might be unreachable with certainty, but they hoped nonetheless to provide deep theoretical explanations for experimental data.

The article published in 1971 by Ruelle and Takens "investigate[d] the *nature* of the solutions of [NSE], making only assumptions of a very general nature on [the equations]" ([10, p. 168, my emphasis]). It was not so much the detailed structure of the Navier-Stokes equation that mattered, but the very fact that fluids could be described, with an amazing degree of precision, by dissipative differential equations. From this fairly general starting point, and several other technical assumptions which they did not even care to derive from the fundamental equation, Ruelle and Takens were able to redefine the nature of turbulence and "give some insight into its meaning, *without knowing [NSE] in detail*" [9, p. 7]. Quite decisively, they also made *qualitative* predictions that could be tested *in vitro* or *in silico*, that is, by numerical simulations of fluid flows.

In 1971, Ruelle and Takens suggested, but did not show rigorously, that when a fluid was subjected to increasing external stress, it went through a succession of bifurcations, where different modes of vibration—i.e. different frequencies—appeared. So far, this was merely a rephrasing of the model proposed by Lev Landau in 1944 and, independently, by Eberhard Hopf in 1942-1948. But Ruelle and Takens went on to suggest, albeit once again without providing a rigorous demonstration, that this bifurcation sequence had to stop after the manifestation of three different modes, because a "strange attractor" appeared in a "generic" manner, and the fluid motion ceased to be quasiperiodic.[1] Strictly *aperiodic motion* was the new definition they proposed for turbulence. Ruelle's new alternative for physicists' modeling practice displaced the emphasis often put on specific models or fundamental laws of nature, in order to tackle whole classes of models directly. Without resolving the conundrum of the nature of the relationship existing between fundamental laws and observation, this new practice made models cheap and dispensable, and rather focused on some essential topological features of observed behaviors which were assimilated to the structural, yet dynamical, characteristics of classes of models. In short, some physicists *stopped looking at specific representations of nature in order to study the consequences of the mode of representation itself.* This shift is attributed to changes in the status of fundamental laws tied with the advent of the computer and the development of mathematical tools such the theory of dynamical systems.

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