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Mathematics in the Physical Sciences, 1650-2000

Organised by
Niccolò Guicciardini (Siena)
Tinne Hoff Kjeldsen (Roskilde)
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December 11th – December 17th, 2005

Abstract. The workshop "Mathematics in the Physical Sciences, 1650-2000" was organised by Niccolò Guicciardini (Siena), Tinne Hoff Kjeldsen (Roskilde), and David Rowe (Mainz). By focusing on the interplay between mathematics and the physical sciences the aim was to gain an insight into developments that had a crucial impact on modern mathematics. Three particular topics emerged as central themes: 1) The period 1650-1800 raises many issues related to the role of mathematics in natural philosophy during the Scientific Revolution and the Enlightenment. Discussing these issues can enhance our historical understanding of a period in which mechanics, astronomy, navigation, cartography, hydraulics, etc., constituted an important stimulus for advances in mathematics. 2) The period 1800-1920 centred on the problem of probing the geometry of space both mathematically and empirically after the advent of non-Euclidean geometry. 3) The twentieth century was focused on mathematical modelling and the question of a change in the conception of mathematical models in various disciplines after 1900.

Mathematics Subject Classification (2000): 01-06, 01A45, 01A55, 01A61.

Introduction by the Organisers

The workshop was organised by Niccolò Guicciardini (Siena), Tinne Hoff Kjeldsen (Roskilde), and David Rowe (Mainz). During the five days of the conference 25 talks were given and one special evening lecture was organised.

The organizers developed the idea for this meeting in consultation with several other colleagues who attended the conference on early modern mathematics held in Oberwolfach January 5-11, 2003. That meeting brought together historians with considerable expertise on developments outside pure mathematics. Afterwards there was a general consensus among the participants that this format had produced fruitful interactions and some promising new perspectives. The idea behind the present workshop called for a similar, open-ended framework, but covering a broader expanse of time reaching far into the twentieth century. By focusing on the interplay between mathematics and the physical sciences the aim was to gain an insight into developments that had a crucial impact on modern mathematics.

This was achieved by inviting experts on the role of mathematics in the physical sciences who were able to approach this subject from a variety of different perspectives. The speakers addressed major developments relating to the overall theme of the conference which focused on thematic issues structured around three time periods: 1650-1800, 1800-1920, and 1920 up to recent times. Three particular topics emerged as central themes of interest:

1) Several of the talks on the period 1650-1800 concerned historical problems involving the role of mathematics in natural philosophy during the Scientific Revolution and the Enlightenment, in particular issues crossing the disciplinary boundaries between history of mathematics and the so-called mechanical philosophy in the natural sciences. Such an approach is vital for the historical understanding of this period in which mechanics, astronomy, navigation, cartography, hydraulics, etc., constituted an important stimulus for advances in mathematics.

2) For the period 1800-1920 a number of talks centred on the problem of probing the geometry of space both mathematically and empirically after the advent of non-Euclidean geometry. The Riemannian legacy and Poincaré's conventionalism served as two cornerstones for this topic, a topic that gained new impetus through Einstein's theory of general relativity and the emergence of relativistic cosmology in 1917.

3) Throughout the twentieth century, mathematical modelling became an increasingly important tool in the physical sciences, and with these developments the modern concept of mathematical models slowly emerged. Recent research on the history and epistemology of models indicates that the conception of mathematical models changed in various disciplines after 1900. This issue was addressed in a collection of talks, including the case of aerodynamical research in Germany – a topic that is part of the larger complex of issues involving "mathematics and war" now receiving widespread attention. Other problems addressed included mathematical modelling in meteorology during the second half of the twentieth century, one of several fields which exerted a strong influence on the modern conception of mathematical models.

The workshop brought together the core community of historians of mathematics, many of whom have attended past meetings in Oberwolfach, along with a number of historians and philosophers of science with strong interests in mathematical issues. The meeting was characterized by open discussions which, together with the talks, shed more light on the interplay between mathematics and the physical sciences and gave new insights into developments that had a crucial impact on the development of modern mathematics.

The organizers and participants thank the "Mathematisches Forschungsinstitut Oberwolfach" for making the workshop possible in the usual comfortable and inspiring setting.

Workshop: Mathematics in the Physical Sciences, 1650-2000

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The mathematical sciences in the 17$^{\text{th}}$ century are of particular interest in order to understand the historical development of the system of scientific disciplines that dominated much of 19$^{\text{th}}$ - and 20$^{\text{th}}$-century science and society where the mathematical approach prevailed. Physics, having taken the role of leader in the hierarchy of scientific disciplines (a "Leitwissenschaft" in the sense of Norbert Elias), stood as an emblem of this process.

I The mathematical sciences in the 16$^{\text{th}}$ and 17$^{\text{th}}$ centuries

In the early 17$^{\text{th}}$ century the struggle for supremacy in the realm of knowledge was wide open. In the hierarchy of scientific knowledge of the Middle Ages and up to the late 16$^{\text{th}}$ century, the mathematical sciences were subordinate to theology and philosophy, and natural philosophy in particular. Even though the mathematical sciences then began boldly to challenge the traditional primacy of philosophy and theology, the regal insignia in the realm of academic disciplines had not yet been passed over to the mathematical sciences. During the 17$^{\text{th}}$ century the picture changed: The mathematical sciences began to play a leading role in the hierarchy of scientific disciplines, and modes of explanation informed by them increasingly dominated many branches of the sciences and segments of society.

In early modern Europe the term *mathematical sciences* was used to describe those fields of knowledge that depended on measure, number and weight (*Wisdom of Solomon* 11, 20). The *scientiae* or *disciplinae mathematicae* were generally subdivided into *mathematicae purae*, dealing with quantity, continuous and discrete as in geometry and arithmetic, and *mathematicae mixtae* or *mediae*, which dealt not only with quantity but also with quality – for example astronomy, geography, optics, music, cosmography and architecture. The early modern mathematical sciences consisted of various fields of knowledge, often with a strong bent toward practical applications, which only became independent from each other and as scientific disciplines in the process of the formation of scientific disciplines from the late 17$^{\text{th}}$ to the early 19$^{\text{th}}$ century.

One of the important preconditions of this process and the *Scientific Revolution* was the rapidly changing social and epistemological status of the mathematical sciences as a whole from the mid-16 $^{\text{th}}$ through to the 17$^{\text{th}}$ century. The foundations of social and epistemological legitimization of the mathematical sciences began to be laid by the work of mathematicians and other scientists from the mid-sixteenth century onward. The strategy was twofold: since the late 16^{th}-century debate about the certainty of mathematics (*quaestio de certitudine mathematicarum*), the *mathematicae purae* were taken to guarantee the absolute certainty and thereby worth of knowledge produced in all the mathematical sciences, pure and mixed, and the *mathematicae mixtae* were tokens of the utility of this unerring knowledge. From the first half of the 17^{th} century mathematicians declared that the mathematical sciences deserved a new position in the modified hierarchy of scientific disciplines. It is on this basis that we have to understand the growing consensus in the early seventeenth century that the mathematical sciences should take up natural philosophy as an object and their desire to legitimise this transgression of the existing boundaries.

II Visual strategies of legitimization

When it came to strategies of visual legitimization, frontispieces were the main medium used. The nexus between the ideas of 17^{th} century scholars (be they mathematicians, mathematical practitioners, experimental scientists, or natural philosophers) and the visual images they used to represent them – the active encoding of ideas into iconographical signs – is the point at which frontispieces become evidence of scholars' deliberate intentions to make and shape their own images of scientific inquiry. Decoding these visual statements to try to understand these processes, offers insights into the self-perception and self-fashioning of its protagonists, the advancement of their cause and the enhancement of their respective discipline's status.

From the perspective of the mathematical sciences frontispieces played specific and significant roles covering a wide range of functions and audiences. I do not consider all of these, but confine myself to the examples of patronage and advertising, which are both closely related to the issue of legitimization.

III Scientification and mathematisation: Bacon and Galileo

Hand in glove with the various legitimising strategies and the new self-confidence were the promises of scientification and mathematisation that Bacon and Galileo gave almost at the same time. The rhetoric of applicability and utility was essential to most strategies to legitimise the mathematical sciences. This is not to say that the mathematical sciences were actually and efficiently applied (or that they could be applied at all). It is important to realise that the incorporation of elements and methods from the mathematical sciences into other branches of the sciences or arts did not only have the function to optimize their professional and scientific activities, but also to enhance their status.

IV The mathematical sciences and theology/the divine

An attempt to review thoroughly the divine element in the seventeenth century mathematical sciences has not yet been undertaken, but without doubt the divine entered many a mathematician's theoretical reflections on the foundations of his discipline quite naturally. There were two standard arguments for a certain affinity between mathematics and divine creation, namely the saying attributed to Plato that in essence God himself was a geometer (*Deum semper Geometriam exercere*) and the famous passage from the *Wisdom of Solomon* that God had organised the world according to measure, number, and weight.

In general, the relationship between the mathematical sciences and theology, and in particular biblical exegesis, in the late 16$^{\text{th}}$ and 17$^{\text{th}}$ centuries has often been characterized as difficult and strained. This view, however, stems from a perspective narrowed down to the Copernican issue and, in particular, the Galileo affair. In fact, it can be shown that there was a lot of common ground between theology and exegesis on the one hand and the mathematical sciences on the other hand in the late 16$^{\text{th}}$ and early 17$^{\text{th}}$ centuries.

Knowledge about the nature of the relationship between the early modern mathematical sciences (including physics) and theology (and scriptural authority) is still fragmentary even though to understand their association would be an important element in order to historically understand the volatile interaction between science and religion since the *Scientific Revolution*. In particular, the questions have been posed, whether or at what point in the development of physics into a specific scientific discipline the divine element was put out of it. When the mathematical sciences and physics took up the book of nature as an object of inquiry in the 17$^{\text{th}}$ century, was this the beginning of banning God from physics?

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The Debate on the Principles of Mechanics in the Bernoulli-Varignon Correspondence (1714-1722)

Jeanne Peiffer

Der Briefwechsel von Johann Bernoulli mit Pierre Varignon, two volumes published, is to be completed by a third volume, on which I am currently working. In the time period 1714 – 1722, three interesting themes are debated by Johann Bernoulli and his Parisian friend Varignon: ship motion and maneuvering; principles of statics; priority dispute on the invention of the calculus. The first two items allow to put light on some stimulating questions related to the theme of the present workshop.

In 1714, Johann Bernoulli published a book, *Essay d'une nouvelle théorie de la manoeuvre des vaisseaux*, which was a response to an earlier monograph (Paris

1689) by Bernard Renau d'Eliçagaray, a French engineer active in the royal marine. Renau's theory is one of the early attempts to mathematize ship propulsion, ship motion and maneuvering. It led to lively controversies, especially with Christiaan Huygens (1693 – 1694), who disagreed with Renau's determination of the velocity of the ship. In the schematic case of a vessel propelled by two rectangular winds blowing into two rectangular sails, Renau determines the velocity by calculating first the velocities the ship would have if it were pushed by each wind separately in each of the two directions. Then he uses the parallelogram rule to obtain the resultant velocity as the geometric sum of the two fictitious velocities in each of the two rectangular directions. This cannot be, as the vessel is not moving on a plane surface (like a billiard ball), but in a resisting liquid. If the ship moves on with a constant velocity, it is precisely because at any moment the propelling sail force – the pressure of the wind – is equilibrated by the resistance of the water. This is proportional to the square of the velocity, as was then generally admitted, and not to the simple velocity. The velocity of the ship is deduced from this equilibrium situation. In the schematic case above, the resultant velocity is thus the geometric sum of the squares of the velocities the ship would have if two rectangular winds would propel it separately into two rectangular directions.

In his 1714 monograph, Bernoulli took sides with Huygens' approach. He developed a completely new theory based on the above calculation of the velocity. Renau was unable to understand Bernoulli's point of view and, supported by Malebranche and his milieu, didn't give up his. In the absence of a shared concept of force, the debate which ensued between Bernoulli and Renau on one side, Bernoulli and Varignon on the other, was heated and difficult to disentangle. In showing that Renau's solution contradicts the fundamental principles of statics, especially the composition of forces, Bernoulli put the debate on a methodological level, as Huygens had already done. Renau distinguishes forces for which this principle is valid (for weights for instance) and forces for which it is not (the compelling sail forces precisely). Bernoulli is not willing to admit such a distinction. His answer is exemplary for his understanding of how forces act: "La distinction que vous faites entre la force des poids et celle des vents n'est point une raison d'admettre le principe de statique pour ceux-là et de le rejeter pour ceux-ci, car cette distinction ne regarde que les causes productrices des forces. Or il n'est pas question de savoir comment les forces sont produites, il suffit qu'elles soient existantes; de quelque cause qu'elles proviennent, elles feront toujours la même impression, la même action, par conséquent le même effet pourvu que ces forces soient appliquées de la même manière" (letter to Renau, 7.11.1713, publ. in Bernoulli 1714, 193 – 220).

Finally Bernoulli gave a formulation of a new principle, the principle of virtual velocities, which he called "my energy rule": "le grand et le premier principe de Statique est que dans chaque équilibre il y a une égalité d'énergies de forces absolues, c'est à dire entre les produits des forces absolues par les vitesses virtuelles" (unpublished letter to Renau, 12.8.1714). Varignon discussed the fundamentality of this principle, derived it from the principle of the composition of forces, and included it in his *Nouvelle mécanique* (1725), where he showed that for each simple machine Bernoulli's energy rule is valid. Bernoulli himself was aware that his rule applied not only to statics, but more generally to dynamics and hydrodynamics.

The first interpretations of the controversy in the historiography (by Ed. Hagenbach-Bishoff 1884, Pierre Duhem 1905 and Pierre Costable 1957) raise interesting questions for discussion:

First, the nature of the interactions between the practitioner Renau, who has the desire, if not the means, to mathematize the movement of the ship, and the mathematician Bernoulli who finds the motivation for his new principle in the exchange with Renau. This case study clearly contradicts Hagenbach-Bischoff's hierarchic distinction between "Erfinder", "Forscher" and "Theoretiker", where only the last one is credited with contributing, thanks to logic and mathematics, to the progress in the physical sciences .

Second, the question of the controversies. Do they contribute to the assessment of truth, the common ground on which everybody agrees, or do they reveal the internal structure, the rational organization, of a range of phenomena (as Pierre Costabel emphasizes)?

Finally Pierre Duhem's reconstruction of the history of the principle of virtual velocities, which according to him developed continuously from an implicit presence in a 13th century manuscript to Bernoulli's analytical formulation, is to be critically discussed in the light of the evidence given by the Bernoulli correspondences.

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A Latourian Approach to Mathematical Physics circa 1700? Thoughts on Studying Early Modern Mathematics "in Action"

J. B. Shank

How should we understand the initiation of analytical mechanics in Paris around 1700? In 1692, Pierre Varignon began to lay the foundations of this new science by applying the new differential calculus developed by Leibniz and taught to him by Johann Bernoulli and the Marquis de l'Hôpital to the new celestial mechanics of central forces introduced in Newton's *Principia Mathematica* of 1687. (Costabel 1965, 1968; Robinet 1960; Blay 1992) The philosophy of Nicolas Malebranche also played a key role in Varignon's initiative, as did the wider cultural climate that "Malebranchian" thought activated in France. (Shank 2004) Reforms at the Paris Academy in 1699, which oriented the institution more toward the broader public, also shaped Varignon's work, as did the public controversies over his "new science of motion" which this newly public academy both authorized and sought to regulate. (Shank 2000) The result was the initiation of a characteristic French academic science in the early eighteenth century – analytical mechanics – a science that was going to provide the starting point for the more famous French academic achievements of d'Alembert, Clairaut, and Maupertuis a generation later, and Laplace and Lagrange after them.

An older literature treats the rise of analytical mechanics in France as the unproblematic translation of Newtonian mechanics into the analytical language of the Leibnizian calculus (Aiton 1972; Cohen 1980). But more recent work has shown the deeper complexities involved in these developments (Blay 1992; Bos 1974-5; Gabbey 1992; Gingras 2001; Guicciardini 1989, 1999). Varignon's work was in fact an original amalgam which drew from a variety of different influences simultaneously, including Newton. The more important question, therefore, is how and why this particular science succeeded in establishing itself as a characteristic practice in France if it was not over-determined by Newton's legacy or achievement. My presentation argued that Bruno Latour's notion of "science in action" as developed in the book of the same name offers a fruitful way to think about the development and the triumph of analytical mechanics in France (Latour 1988). In offering an account of Latour's rules of method and principles as developed in *Science in Action*, my talk also sought to provoke wider discussion of this particular methodological approach and its applicability to the study of cases in the history of the mathematical sciences. It finally sought to build bridges between "Latourian science studies" and the best recent work in the history of early modern mathematics (Bos 2002; Guicciardini 1999) by suggesting some affinities that exist between the two.

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Newton's Inventive Use of Kinematics in Developing his Method of Fluxions

Kirsti Andersen

Introduction

From the summer 1664 to October 1666, besides being engaged in other research projects, Newton worked intensively on developing a method for solving geometrical problems, among them determining tangents, areas, curve lengths, and centres of curvature. His work resulted in a manuscript, known as The October 1666 Tract (Newton 1666), in which Newton had polished his description so much that one gets the impression that his plan was to present his ideas for a greater circle of scholars. The tract contains what could be called the first version of Newton's method of fluxions. However, Newton did not publish the tract and, presumably he did not show it to many colleagues, either. Instead, he returned to his method regularly and kept revising his presentation of it – most likely because he wanted to improve the theoretical foundation for his technique of determining the ratio between two velocities (by him called fluxions). Actually, almost four decades passed before Newton published a presentation of his method of fluxions (Newton 1704), though he had made allusions to it in his *Principia* published in 1687.

Newton's method of fluxions became known in some of the forms it attained through Newton's subsequent revisions. Hence it is natural that most historians describing Newton's method have focussed on the later adaptations. However, if we want to see how far the creative young mathematician could come with very few tools, it is of relevance to look at Newton's method of fluxions from October 1666. In this paper, I show a few examples of how Newton solved geometrical problems with only the help of an intuitive concept of velocity.

Proposition Six

Newton started his October 1666 tract by claiming: "To resolve Problems by Motions these following Propositions are sufficient". His first five propositions concern velocities, for instance the projection of a velocity in one direction upon another direction, the composition of velocities (the so-called parallelogram rule), and the ratio between the velocities of two points on a rotating line.

In connection with the concept of velocity, it is of relevance to notice that the seventeenth century-scientists were well aware of the importance of introducing line segments with given directions to describe motion, but they did not assign fixed lengths to directed line segments but worked, like the ancient Greek mathematicians, with proportions between the lengths of the line segments. When considering two motions, Newton explained his understanding of their velocities by the expression the "proportion and the position of ... motions" (Newton 1666, 401). I will allow my self to describe this by the term 'a representative of a velocity'.

The sixth of Newton's proposition is very interesting and the core of the present discussion. However, it is formulated in a way that it is unfamiliar to a modern reader and, hence, I start by explaining the problem it was meant to solve. Many of the curves considered in the seventeenth century were inherited from the Greeks who had defined them by motion – Archimedes spiral and the quadratrix being two examples. Newton's basic idea in his new approach to geometry was to develop this kinematic treatment of curves further. Thus, he considered any curve as being generated by a motion – which, itself, more often than not was defined by other motions. To solve, for instance, tangent problems, he was interested in determining representatives of the velocities of the motions generating a curve, and of the velocity of the final motion. In this connection he ran into the following problem.

Let (figure 1 – with my notation rather than Newton's) the curves c_1 and c_2 be moved so that they continuously intersect and let v_1 and v_2 be (representatives of) the velocities of the curves. How can we determine the velocity of the instantaneous point of intersection of the two moving curves?

It was exactly this problem that Newton was confronted with when – on November 8, 1665 – he was working on determining the tangent to the quadratrix.

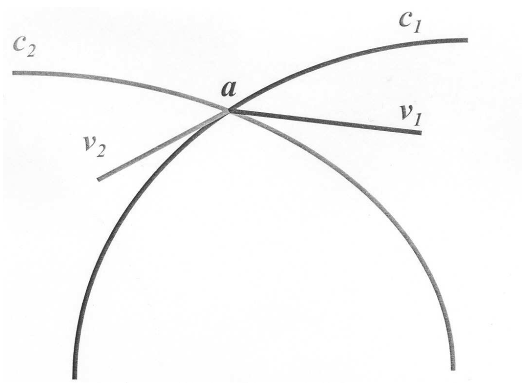

FIGURE 1

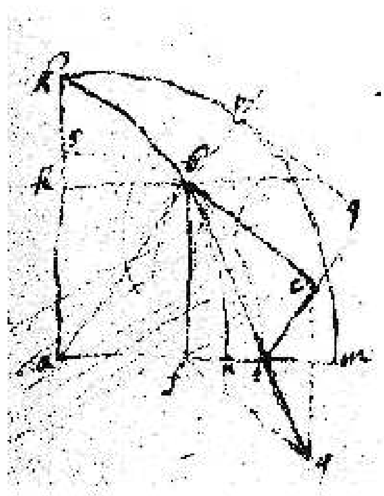

FIGURE 2

First, he composed the two velocities according to the parallelogram rule (*bcgf* in figure 2), but he soon realized his mistake, and it is presumably in this connection that he became aware of the need for proposition six.

The statement of proposition six is as follows (figure 3). Let v_1 and v_2 in figure 1 be represented by *ab* and *ad* respectively, let the line *bc* be parallel to the tangent to the curve c_1 (*ae*) at *a* and the line *ad* parallel to the tangent to the curve c_2 (*ah*), and let finally *c* be the point of intersection of the two mentioned lines. The line segment *ac* is then a representative for the velocity of the instantaneous point of intersection *a*. Newton formulated this as saying that the five line segments *ab*, *bc*, *ad*, *dc*, and *ac* "shall designe the proportion" and positions of the point *a*

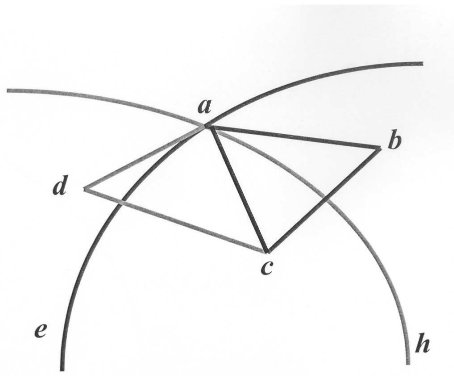

Figure 3

moving: 1. with the line ae, 2. on the line ae, 3. with the line ah, 4. on the line ah, 5. on the curve of intersection (Newton 1666, 401).

Newton applied proposition six to determine the tangent to the quadratrix correctly (*ibid.*, 418). In this particular case the moving curves are straight lines to which bc and cd are parallel. Newton did not prove proposition six, but it can actually be done, based on the parallelogram rule and the ideas of relative velocity and induced velocity (for more on this point see Andersen, 1968, 161–165). It is interesting to notice that in the second half of the 1630s Gilles Personne de Roberval had also been struggling with determining the tangent to the quadratrix and formulated a theorem rather similar to Newton's proposition six (*ibid.*). Roberval's work was only published in 1693, and can therefore have had no influence on Newton's ideas.

1. Proposition Seven

Newton's proposition seven contains a rule for determining the ratio between two (or more) velocities of movements that produce lengths between which there are an algebraic relation. For the sake of simplicity, I only consider two motions. Newton did not only formulate the rule, but he also proved it, and his proof is very illustrative for his way of thinking. While he formulated the rule generally, Newton's proof only covers a special example, but can easily be generalized. It deals with the curve

$$x^3 - abx + a^3 - dy^2 = 0.$$

and runs as follows (Newton 1666, 414). Newton assumed that the curve had been generated by a point moving in the direction of x with the velocity p and in the direction of y with the velocity q. His aim was to find the ratio between q and p. He then looked at the situation after a very small amount of time, which he denoted o and called a moment, had passed. Because of the smallness of o, Newton considered that the motions in that moment as uniform, that is p and q

as constant. Thus, x will increase by po and y by qo. Since the moving point must still be situated on the curve, the above equation still applies, hence

$$(x+po)^3 - ab(x+po) + a^3 - d(y+qo)^2 = 0$$

Using the first equation to reduce the second, Newton obtained

$$3pox^2 + 3p^2o^2x + p^3o^3 - abpo - 2dqoy - dq^2o^2 = 0.$$

Dividing by o and then ignoring the terms still containing o, Newton got the result

$$3px^2 - abp - 2dqy = 0.$$

His rule was thus: multiply each term with x by its dimension in x and by p/x; and similarly for the terms containing y: multiply them by their dimension in y and q/y. Thereby an equation is reached from which the ratio between q and p can be determined.

When I first saw this deduction several decades ago, I was flabbergasted. How is it possible to determine the ratio between two velocities without a definition of a velocity and without a method of determining derivatives? The entire trick is to assume that in a very small time interval, a velocity can be assumed to be constant!

Applications of propositions six and seven

Propositions six and seven are very central for Newton's proofs and for his derivation of new results. One of his more unexpected applications of proposition seven is an astute proof of his first proposition from which he easily deduced the parallelogram rule for velocities (Arthur forth.). Whereas it was more natural that Newton used proposition seven to determine tangents to algebraic curves *ibid.*, 416).

In proposition eight Newton formulated the inverse problem of the one treated in problem seven, that is: given a relation between the ratio q/p and x, determine y. He remarked that if this always could be solved "all problems whatever might bee resolved" (*ibid.*, 403).

As already shown, proposition six was of use for determining tangents to some non-algebraic curves. Newton also showed a very spectacular application of proposition six which was to deduce a rule for determining the centres of curvature (*ibid.*, 419). It requires some patience to follow Newton in his deductions, but it pays to keep following his arguments. At least, I am very impressed by how Newton could derive a result corresponding to the formula for the radius of curvature

$$\frac{\left(1+(y')^2\right)^{\frac{3}{2}}}{y'y''}$$

by considering velocities in various directions and introducing considerations from proposition six (for another opinion, see Whiteside 1961, 377).

Finally, Newton applied motion in introducing the concepts which was later named an evolute and an involute, and with the help of these and his rule for determining the radius of curvature he was able to rectify a number of curves (Newton 1666, 432).

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On the Geometrical Physics of the 17th Century

Henk J.M. Bos

One characteristic of 17th-century mathematics is the predominantly geometrical nature of its concepts and techniques. Although the period witnessed the beginnings of analytic geometry and the calculus, and although equations increasingly made their appearance in the pages of mathematics, the relations between variable quantities were mostly understood in terms of ratios and proportionalities, or in terms of their representations by means of curves with respect to axes in the plane. The function concept acquired its fundamental place in the theories of physics only much later. A special instance of this geometrical style in physics is the representation of the relations between physical quantities such as velocity (v), distance traversed (s), and time (t) in rectilineal motion. Thus for instance Galilei gave these relations for uniform motion, which we usually describe as

$$s = v \times t \ ,$$

as follows:

1): if v is the same, then s is proportional to t
2): if t is the same, then s is proportional to v
3): if s is the same, then v is inversely proportional to t
and if neither v, nor t, nor s is the same, then
4): s is in the compound ratio of v and t
5): v is in the compound ratio of s and the converse ratio of t
6): t is in the compound ratio of s and the converse ratio of v

Evidently the concern about the physical nature of the variables time, space and velocity prohibited the use of algebraic operations for these variables; velocity $\times$ time had no meaning, nor did the ratio of s and v.

In Huygens' derivation of the famous relation between the period T and the length l of a pendulum (in modern terms $T = 2\pi\sqrt{\frac{l}{g}}$, g the gravity constant), we meet the same use of proportionalities. The effect of this use is that in *Horologium Oscillatorium*, the work of 1672 in which Huygens published his results on the motions of free fall and of pendulums, the formula does not appear, nor does the gravity constant g, and the result equivalent to the modern formula is difficult to locate in the text.

This example leads to the question whether and how this style characteristic of mathematics affected the physics of the 17th century. In the case of Huygens' derivation it did, namely in the way the physical constant describing the process of fall was introduced. In the modern theory this constant is g, introduced as the constant which translates the proportionality 'v is proportional to t' ($v :: t$) into an equation '$v = g \times t$' (or equivalently ($s :: t^2$ into $s = \frac{1}{2}gt^2$) in such a way that, with standard units for length and time, the equation fits the process of free fall both numerically and qua dimension. Huygens' physical constant is a different one. He introduced it only at the end of his reasoning, when he had adjusted his results to experimental results expressed in numbers. For that he had to transform the proportionalities $v :: t$ or $s :: t^2$ into relations fit for numerical equalities. He proceeded as follows. A proportionality like $s :: t^2$, for Huygens, meant that, if (s_1, t_1) and (s_2, t_2) were corresponding distance-time pairs then $s_1 : s_2 = t_1^2 : t_2^2$. Therefore he took as fundamental constant describing free fall, the distance — call it s_{sec} — which a body falling from rest traverse in the first scond. Then $s :: t^2$ implies $s : s_{sec} = t^2 : 1^2$ and this relation, once s_{sec} is experimentally determined, can be used for numerical calculations.

As a result, in Huygens' style a physical constant characteristically has a primary dimension (time or length) and is introduced only when numerical results have to be obtained. In contrast we are used to physical constants introduced as proportionality factors, introduced from the beginning in order to write equations, and having more complicated dimensions because they have to balance the dimensions in the equation.

It seems to me worthwhile to investigate a) whether there are other charcateristics of 17th-century physics induced by the more geometrical style of its contemporary mathematics, and b) when and how the transition from the earlier to the later style took place.

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Two Steps Forward, One Step Back: The Space Problem between Riemann and Einstein

José Ferreirós

The question of geometry and physical space from, say, Gauss to Weyl is very attractive to a philosophically minded historian. Many of the relevant actors in that story devoted a great deal of attention to philosophical issues, on top of the mathematical and scientific ones. In cases like Riemann and Einstein, it has been aptly said that we find at work the "*magic triangle*" of Mathematics-Philosophy-Physics. As my title suggests, I can't help having the sentiment that Riemann's deep reflections were extremely adequate for the events that would take place in theoretical physics half a century after his death. By this of course I do not mean to suggest that Riemann somehow "foresaw" the advent of **GRT**, but his conceptual standpoint seems more adequate to this event than the ideas of most other people who worked on the *space problem* in between – including Einstein himself in the celebrated '*Geometrie und Erfahrung*' of 1921 (which in this respect makes a contrast with, e.g., Einstein 1934).

Riemann's intellectual efforts led him to adopt rather innovative viewpoints not only in mathematics, but also in physics and philosophy. In his inaugural address '*Über die Hypothesen, welche der Geometrie zugrunde liegen*' (1854), the *magic triangle* is at work more than anywhere else, combining those diverse innovations; a work of that kind, which was left unfinished, had enormous chances of being quite incomprehensible or unacceptable for contemporary readers.

In epistemology, Riemann was far from Kant, as he accepted Herbart's decided rejection of *a priori* sources of knowledge such as the "pure forms of intuition" and the "categories". There have been attempts to read the 1854 geometry lecture as an argument against Kant, but in my view any such effort is extremely forced and misleading. He also kept some distance from the positivistic ideas that were influencing German intellectuals more and more. His epistemological stance places the origins of all knowledge in experience (*Erfahrung*), without any *a priori* addition, but it emphasizes the way in which our reflecting thoughts (*Nachdenken*) keep adding hypotheses (*Hypothesen*) which are indispensable to the growth of knowledge. One of his manuscripts has interesting remarks to the effect that the word hypothesis has "now" a different meaning than in Newton: "today one understands by hypothesis everything that is added in thought to the phenomena" (*Alles zu den Erscheinungen Hinzugedachte*; Riemann 1876, 493). He goes on to criticize the distinction between "axioms and hypotheses" in Newton, showing how the law of inertia is a hypothesis. All of this is relevant to understanding the views on the geometrical axioms proposed in his (1854).

Riemann's emphasis on the role of hypotheses in physical theories is congenial with Einstein's mature views on the interplay between creative thought and empirical elements in science (Einstein 1933). At this point in my talk we discussed how Einstein evolved from a Machian sceptical empiricism in his early years, to a more sophisticated viewpoint (Holton 1968). The discovery, studying the rotating disk in 1912 (Stachel 1980), that the problem of relativistic gravitation required adoption of a Riemannian space of variable curvature had an inescapable impact. Operationalism with respect to the basic coordinate systems was no longer tenable, as the (semi)Riemannian metric had to be given physical meaning *as a whole* (Einstein 1934; see Einstein 1916, 775 & 779). But it still took Einstein several more years to distance himself from the way of presenting the space problem that was typical around 1900. This classical space problem had emerged from Helmholtz, and it represented a step backward from Riemann's unorthodox and extremely general formulation of the same question.

In comparison with Riemann, Helmholtz's standpoint was certainly closer to positivism due to its operationalist underpinnings. This is reflected in the very titles of their contributions: to Riemann's "Hypothesen", Helmholtz replies "Thatsachen", facts! (Helmholtz 1868). Helmholtz argued that the concept of rigid body, and the free mobility of bodies in space, are essential building stones for *any possible conception* of geometry, as they are necessary for the physical possibility of checking congruence relations, and thus for any geometric measurement (Helmholtz 1868, 197). He presented this approach to geometry as a simpler way of developing Riemann's results (*op. cit.*, 194, 197), although he admitted that his direct approach from free mobility entails a limitation of the great generality of Riemann's analytical study. Helmholtz's problem led to detailed mathematical considerations, culminating in Lie's work. His success in defining the classical space problem had the effect that *most mathematicians in the late 19th century understood Riemann's more general investigations to be only of interest to pure analysis, while for questions of physical geometry it was merely the spaces of constant curvature that had any role to play* (see e.g. Poincaré 1902).

Even Einstein's (1921) was essentially based on the operationalist, Helmholtzian way of posing the space problem. As a result, there was a split between the analytic line of development arising from Riemann's (1854), including research by Christoffel, Lipschitz, Ricci and Levi-Civita on differential invariants, and the *space problem* as discussed by Lie, Poincaré and others, leading up to Minkowski's reaction to special relativity. This whole development was reinforced by the dominance of group-theoretic thinking about geometry around 1900, which left no space for the more general Riemannian geometries (see e.g. Norton 1999). It came as a great surprise when both strands merged in the search for a relativistic theory of gravitation.

For Riemann, the starting point in metric geometry was not the congruence of solids, but rather the invariance of the line element ds, its "independence from position" (Riemann 1854). This was not merely based on mathematical reasons, but on convictions having to do with the foundations of physical theory. Simple physical laws were only to be found at the local level, as expressions valid for points in space and time (Archibald 1991, 269), and at this local level the "empirical concepts" of "solid body and light ray loose their validity" (Riemann 1854, 267). Indeed, these ideas were intimately connected with Riemann's work in theoretical physics, the '*Naturphilosophische Fragmente*' where he strived to produce a unified theory of the basic physical forces – gravitation, heat, light, electromagnetism – on the basis of a geometrically conceived system of dynamic processes in an ether field (see Wise 1981). Working along this line, in 1853, Riemann was led to the assumption that the ethereal element varies in time, so that one can determine ds_i as linear expressions on dx, dy, dz such that:

$$dx'^2 + dy'^2 + dz'^2 = {G_1}^2 {ds_1}^2 + {G_2}^2 {ds_2}^2 + {G_3}^2 {ds_3}^2,$$

with functions G_i depending on time *and* on the spatial coordinates (Riemann 1876, 505), and

$$ds^2 = dx^2 + dy^2 + dz^2 = {ds_1}^2 + {ds_2}^2 + {ds_3}^2.$$

The link to his later differential geometry is quite apparent (see also Bottazzini & Tazzioli 1995), and it explains the intriguing sentence of the geometry lecture where Riemann speculates about a connection between physical forces and the expression for the line element (Riemann 1854, 268). But here he was in all likelihood thinking about electromagnetism, not gravitation (Riemann 1876, 506). In my opinion, what Riemann tried to do in 1854 was to eliminate the duplicity of a Newtonian space and an ethereal field that he had been assuming in his previous work on the unification of physical forces. The Leibnizian, relationalist tendencies that Riemann shared with his preferred philosopher, Herbart (see Ferreirós 2000), thus led to his new and (in Einstein's words) "prophetic" perspective on geometry.

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Curved Spaces: Mathematics and Empirical Evidence, ca. 1830 – 1923

Erhard Scholz

The talk presented a survey of the attempts to find empirical bounds for the curvature of physical space from astronomical data over a period of roughly a century. It covered the following stages: (1) Lobachevsky, (2) Gauss and his circle, (3) astronomers of the late 19th century, (4) outlook on the first relativistic cosmological models. The authors of the first three sections used parallax data with slightly different methodologies and increasing mesurement precision. In the last phase a new methodological approach to physical geometry was opened by general relativity, and two completely new data sets came into the game, mass density and cosmological redshift.

(1) Already in his first publication on his new non-Euclidean geometry (NEG), *N.I. Lobachevsky* gave a rough estimation of space curvature by astronomical data (Lobatschewsky 1829-30/1898). By a simplifying argument he derived the estimation $a < 6.012\ 10^{-6}$ for the diameter a of the earth's yearly orbit, expressed in units $K = 1$ of the "constant" of NEG. We my prefer to read his result the other way round,

$$K > 3 \cdot 10^5\, AU \approx 2.4\, LY \tag{1}$$

($1\, AU = \frac{a}{2}$ the *astronomical unit*, LY *light year*). In many of his other publications Lobachevsky discussed the relationship between physical and astronomical space to his new geometry. Sometimes he included quantitative estimations, although mostly weaker ones than in 1830, sometimes he added qualitative methodological remarks, highly interesting in themselves.

(2) Apparently, *C.F.Gauss* used his high precision geodetical measurements for gaining a first secure empirical estimation for a bound of the constant K already in the early 1820s.[1] Gauss knew well that parallax measurements were completely unreliable at the time. In a letter to H.C. Schumacher (29. June 1831, (Gauss Werke VIII)) he reported on properties of NEG which came close to a method to determine bounds of space curvature by parallax data. Moreover, in his seminars in the 1840s and/or early 1850s he discussed the question of determination of

[1] There has been an extended discussion in the history of mathematics whether or not this report can be trusted. I consider it as reliable; cf. (Miller 1972, Scholz 2004, Scholz 2006).

the nature of physical space by astronomical observations but apparently never claimed to have a solution to it.[2]

B. Riemann dared a more definite claim. At the end of his famous *Habilitationsvortrag* of 1854 he stated in passing that, if the curvature κ of physical space is constant, it is very small in comparison with the reciprocal of the area A of triangles $\triangle$ "accessible to our telescopes". In other words,

$$|\kappa| A(\triangle) << 1 \,, \quad \text{or} \quad |\kappa| << A(\triangle)^{-1} \,. \tag{2}$$

Riemann's formulation expressed the state of art of theoretical evaluation of the results of high precision astronomy at the time of Bessel and Gauss.

(3) Only a few theoretically inclined astronomers of the late 19th century dealt with the question, among them *Robert S. Ball* (1840 – 1913), Astronomer Royal of Ireland at the observatory Dunsink, and Karl Schwarzschild (1873 – 1916). Ball was an experienced astronomer. He refrained from a definite answer of how to determine a hypothetical space curvature from them. He indicated only quite generally that "it would seem" that it can only be "elicited by observations of the same kind as those which are made use of in parallax measurements" (Ball 1879, 519)

A more definite answer was given two decades later by *K. Schwarzschild* in a talk given during the 1900 meeting of the *Astronomische Gesellschaft* at Heidelberg.[3] For the hyperbolic case he arrived at an estimation of the radius of curvature R, $\kappa = \frac{1}{R^2}$,

$$R > 4 \cdot 10^6 \, AU \approx 60 \, LY, \tag{3}$$

an order of magnitude above Lobachevsky's value (due to his sharper parallax values) and with a convincing derivation without *logical* dependence on a precarious simplifying assumption.

(4) The next, by far most radical, turn for modern cosmology came with Albert Einstein's general theory of relativity (GRT) in 1915. GRT changed the role of curvature completely; from now on curvature was insolubly linked to the mass-energy-stress tensor T on the right hand side of the Einstein equation. At the time it was close to impossible, however, to make a reasonable guess of cosmic mass energy density.

For his first relativistic model of cosmological model *A. Einstein* (Einstein 1917) came to a provisional estimation of $\rho \sim 10^{-22} g \, cm^{-3}$ by an evaluation of available counts of stars and nebulae. Because of its great unsecurity, Einstein did not publish the estimation:[4]

$$R \sim 10^7 \, LY \,. \tag{4}$$

After W. de Sitter's invention of the second model of general relativistic cosmology, *H. Weyl* studied a model of diverging time-like flow lines in the de Sitter

[2](Hoppe 1925).

[3]It has been discussed at different places how Schwarzschild determined bounds for the curvature of astronomical space from parallax and other data; cf. (Schemmel 2005).

[4]He wrote about it in a letter to M. Besso (Dec 1916) or mentioned it in conversations.

hyperboloid of "radius" a. The divergence led to the model phenomenon of a systematic redshift expected for light emitted from very distant sources.[5] The infinitesimal linearization of Weyl's formula led to a relation between redshift z and distance d, $z = \frac{1}{a}\, d$ (Weyl 1923, 323). Here the *Hubble constant* H, in later terminology, was $H = a^{-1}$. From the best recent redshift and distance data of nebulae of the local cluster, Weyl arrived at an estimation of the de Sitter radius (identical to the the constant curvature radius R of orthogonal spacelike sections to the flow lines)

$$R = H^{-1} \sim 10^9 \; LY \quad \text{(Weyl 1923, app. III)}\,. \tag{5}$$

This was a splendid estimation, close to what E. Hubble found in his much more precise and detailed measurements at the end of the decade.[6] In 1923 there were now two methods for determining the curvature of the spatial sections of cosmological models. They relied on rather different principles, and led to first estimations which were both $5-7$ orders of magnitude above the highest bounds derived by the methods of the 19th century and differed among each other "only" by two orders.

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Space and its Geometry 1800 – 1900

Klaus Volkert

In my paper I discussed the changing ideas on space and its geometry. Up to around 1750 discussions about space were situated in a metaphysical context – centered around the question of relational or absolute space (cf. Euler's paper "Reflexions sur l'espace et le temps" [1750]). All that had no real impact on solid geometry: Space was more or less absent from solid geometry at that time – it was a matter of course. This thesis was underlined by some citations from mathematical dictionaries (Wolff, Hutton, Klügel) showing that these authors had not much to say on mathematical space.

In Euclid's "Elements" there is no definition of space but only one of a solid (XI, def. 1). From his theorems and their proofs we may infer that Euclid's space was three-dimensional, homogeneous and isotropic. Even the basic property of being three-dimensional is not formulated by Euclid explicitly. We must reconstruct it from his theorem XI, 3: "If two planes cut one another, their common section is a straight line." Even in von Staudt's "Geometrie der Lage" (1847, §20) this theorem is proved without an explicit hypothesis on the number of dimensions of space.

Euclid left another very basic problem to the mathematicians of later times: how to define the congruence of two polyhedra? He stated (IX, def. 20): "Equal and similar solid figures are those contained by similar planes equal in multitude and in magnitude." This definition was critized by R. Simson (1687 – 1768): Take two pyramids with congruent bases but different heights. Put them together in the normal style (by "addition"). But you may "subtract" the smaller one from the bigger one. So you get two polyhedra congruent in Euclid's sense which is absurd.

In particular you can't infer that two polyhedra being congruent in Euclid's sense have the same volume – a fact which is used in the proof that Euclid gave for his proposition XI, 24: "If a parallelipedal solid be cut by a plane through the diagonals of the opposite planes, the solid will be bisected by the plane." The two resulting prismata are symmetric but not congruent in the sense of superposability.

It was Legendre who tried (first ?) to manage these difficulties on a conceptual level. That is, he introduced a distinction between congruent and symmetric solids – the latter being composed by the same polygons but in a different order. Here orientation shows up – a big theme in the 19^{th} century. Legendre gave a systematic treatment of solid geometry – the first after Euclid – which introduced new basic ideas. He incorporated spherical geometry in his textbook ("Eléments de géométrie", 1794) as the missing link between plane and solid geometry – a fact that is highlighted by his ingenious proof of Euler's theorem. In the context of spherical geometry the riddle of orientation was discussed even before Legendre in the 18^{th} century. Segner had remarked that a spherical triangle and its diametrical counterpart are in general not superposable – so one has to demonstrate that they have same area ("Vorlesungen über die Rechenkunst und Geometrie…", 1767 [p. 591]). Legendre gave a first proof for this fact using the idea of equidecomposability. He extented his result to symmetric solids – a problem later discussed by Gauß und Gerling.

A strong impetus to study symmetry came from crystallography. Bravais gave in 1849 a rather complete discussion of the symmetry-types of polyhedra (plane, pointwise and rotational – he missed the rotation-reflection). His method was quite classical: he considered only the vertices of the polyhedra and their images. So there was no explicit idea of transformations and therefore no considerations on the surrounding space. It is not surprising that space became a theme when transformations were considered by Helmholtz and others in the second part of the 19^{th} century (cf. the famous space-problem), a development that was anticipated by Möbius with his theory of geometric "Verwandtschaften". But this is an another story.

It is often stated that around 1800 there was an increasing interest in solid geometry (cf. the book by Paul for example). There were different causes for that: First we may cite Monge and his descriptive geometry – a technique for representing three-dimensional figures in two dimensions. It was designed to help in practical questions and intended to create a common language for scientists, ingenieurs, artists [in the Mongian sense of the word] and craftsmen. The needs of the beginning industrial era demanded obviously a deeper development of solid geometry since all practical production takes place in three dimensions. The experiments performed by Oersted, Ampère and others raised the riddle of orientation of space (cf. Fr. Steinle's paper – I owe much to him for elucidating this problem to me) demanding a precise language to express the facts found in practise. This riddle was also present in crystallography where one had to choose an orientation to fix the axes introduced to describe the structure of the crystals (Weiß, Hessel, Bravais). Later in the 19^{th} century the problem of symmetry showed up in chemistry in the context of isomers (van't Hoff, Le Bel). The symmetrical spatial structure of the molecules causes here different physical behavior. So we may conclude that there was an interaction between solid geometry and its so-called applications – but not in the direct sense that the latter raised a concrete question that was solved by the former. It was more or less an indirect influence, a need for an adequate conceptual frame for example. One could handle every single spatial situation in a more or less ad hoc way – but this doesn't mean to have a good all embracing theory! From a methodological point of view it is clear that it is difficult to grasp this influence but nevertheless it is important.

Möbius commented on the problem of symmetric solids in his "Barycentrischer Calcul" (1827): Two such solids could be superimposed by a rotation around a plane (sic!) if there would be a four-dimensional space: "But since such a space cannot be thought, so coincidence is impossible in this case, too." (Smith 1959, 526) Grassmann and Plücker rejected also the idea of a four-dimensional space. For those mathematicians there was a difference between real metaphysical space and the abstract speculations in mathematics.

A first step to overcome this restriction was taken by Cayley and Cauchy. Around 1850 they started to develop the analytical geometry of n-dimensional spaces – thus generalizing the well known results of the second half of the 18^{th} century on the elementary analytical geometry of space. But they carefully avoided a "recourse to any metaphysical notion" in treating only "analytical points" (not real points) and so on: The new geometry can thus be understood as a "disguised form of algebraic formulation" (Sylvester).

The acceptance of the geometry of higher dimensional spaces took place in parallel to that of non-Euclidean geometry beginning around 1860. In 1869 Sylvester stated: "If Gauss, Cayley, Riemann, Schläfli, Salmon, Clifford, Kronecker, have an inner insurance of the reality of transcendental space, I strive to bring my faculties of mental vision into accordance with them." (Inaugural Presential Adress to the BAAS). Like in the history of non-Euclidean geometry arguments of authority are important here. But there is also an argument of utility in Sylvester: if we decide to refuse transcendental geometry we must refuse also other achievements of mathematics which proved to be useful! This is a nice example of justification in mathematics. To Jordan, who wrote the first systematic treatment of analytical geometry in n dimensions (1875) this was the culminating point of the fusion of algebra and geometry – initiated long ago by Descartes. But even in 1895 Poincaré (in his introduction to his great paper on "Analysis situs") felt obliged to state that the new geometry has "a real object".

A great step towards the acceptance of hypergeometry was taken by Stringham who clarified in 1880 – using absolutely classical tools – the question of the six regular hypersolids (in four dimensions). This was perhaps the first substantial result in hypergeometry which couldn't be discarded as "mere analytical".

The riddle of orientation was discussed intensely in the 1870 in the realm of topology – in particular in respect to "one-sided" surfaces as the Möbius strip. A very classic object of geometry – that is the projective plane – was understood in a new more complete way. The distinction between congruent and symmetric solids disappeared with the introduction of transformations and the reduction of all rigid motions to products of reflections in planes in the 20^{th} century.

To conclude this very incomplete sketch of a fascinating history let me underline four points:

(1) The acceptance of a new basic concept like that of hyperspace is not a result of clear cut definite decision but of a growing evidence of its existence (among others in the sense of usefulness). It presupposed a new understanding of the foundations of mathematics – in particular an emancipation from metaphysical restrictions.
(2) The interactions between the sciences and geometry are numerous. But they were often not of the form of precise problems or questions. They established something like a climate of curiosity or interest. A decisive point was the desire to create a systematic basis for geometry – a point becoming very clear in Legendre's treatise.
(3) Our modern ideas on the application of mathematics to the sciences are not adequate to grasp the developments before 1850. In those days geometry was "mixed" in the sciences (cf. the term "mixed mathematics" which was quite current at that time) and not "applied" to the sciences. So geometry was subject to the restrictions imposed by metaphysics in an inevitable way. Euler was one of the first who denied this point of view.
(4) It must be stated that there were other developments in mathematics which were important to our theme and which couldn't be discussed here. I want to cite two of them: Differential geometry and transformational geometry. The first focussed on the idea that there is a structure imposed on an entity (like a surface), the second on the idea of space as a whole. Both tended to separate the substratum (in modern terms: a set) from its structure (e.g. the metric). So they prepared the modern idea of space being a model for certain "space like" relations; in constructing such a model one feels free to choose the one which seems the most adequate.

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Who's A Conventionalist? Poincaré's Correspondence with Physicists

Scott Walter

Henri Poincaré's engagement with physics was an enduring one, spanning almost the entire length of his scientific career, from his doctoral thesis of 1879 to the end of his life in 1912. This interest in the problems of physics, however, represents a serious challenge for the historian of exact science, for several reasons. First and foremost, there is the hard fact that Poincaré pursued problems of physics in parallel with seemingly-unrelated interests in analysis, topology, geometry, celestial mechanics, electrotechnology, and philosophy of science. Locating the threads tying these disparate disciplines together is only part of the task; attaching them to Poincaré's actual practice of science is another matter altogether. Secondly, the turn of the twentieth century saw the emergence of the sub-discipline of theoretical physics, and a consequential remapping of disciplinary frontiers, a remapping in which Poincaré was an important cartographer, and one whose writings on the interrelations of logic, mathematics, geometry, mechanics, and mathematical and experimental physics exercised a durable influence on scientists throughout the twentieth century.

Historical studies have illustrated Poincaré's innovative approaches to questions of mathematical physics, and his critical, but apparently independent evaluation of leading theories of the day: Maxwellian electrodynamics, kinetic gas theory, Newtonian gravitation, electronic theories of matter, and quantum theory. Likewise, the effectiveness of Poincaré's disciplinary entrepreneurship is better known in part thanks to the opening of the Nobel Archives, which reveal a widespread appreciation of his contributions to physics on the part of the international scientific community.

For its several merits, this historical work has illuminated neither the why nor the how of Poincaré's engagement with physics. These are, of course, topics that Poincaré did not address himself, at least not directly. In his last four years, Poincaré's state of health declined, and he did not find the time to write his memoirs. A good scientific biography has yet to be published, although several lives of Poincaré are in the works. Adding to the difficulty of the biographer's task is the fact that only a small portion of Poincaré's Nachlass has been published. Among the unpublished portion of the Nachlass are two hundred and fifty-seven letters to and from physicists, less than ten percent of which has been exploited to any extent by historians. To obtain an idea of how Poincaré went about doing physics, and why he did so, surely this would be a good place to begin.

What then does Poincaré's correspondence with physicists tell us about his engagement with the problems of physics? One way to approach the question is by examining the relation between the image of Poincaré's physics drawn from his published works, and that arising from his unpublished correspondence. The image we form is multi-faceted, of course, but let us look briefly at just one facet: the thematic image. Are there themes in Poincaré's published work that are echoed in his correspondence? If so, which ones? What themes find no echo in the correspondence? Inversely, we can ask if there are themes addressed in the correspondence that are absent in the published œuvre.

First of all, among the problems of physics addressed by Poincaré in print, and which have an epistolary pendant, we find multiple resonance, the Zeeman effect, questions concerning Lorentz's theory of electrons, and the Rowland effect (i.e., the magnetic action of convected charge). Study of the Rowland effect, in particular, generated a significant volume of correspondence in the period 1901-1903 (38 letters), while only two published articles are linked to the topic, one of which is an edition of his letters to the French physicist Alfred Potier. The themes "missing" from Poincaré's correspondence include the foundation of the second Law of Thermodynamics, kinetic theory in general, probability, and quantum theory.

As for the inverse relation, in his correspondence Poincaré takes up, among other topics, what he called "Le Bon" rays, and N rays. The former were also known as "black light", or "lumière noire", in the coinage of their erstwhile producer, friend and editor of Poincaré, Gustave Le Bon. The latter rays were the work of one of France's leading experimental physicists, René Blondlot. The fact that both phenomena were spurious may seem sufficient to explain Poincaré's reticence to publish, but it is not, as demonstrated by his publications on the equally-spurious *absence* of the Rowland effect. Perhaps after close study of these and other cases present in Poincaré's correspondence, historians will be in a better position to understand how and why Poincaré constructed his singular—and phenomenally successful—physical world-view.

Theoretical Cosmology and Observational Astronomy, circa 1930

Craig Fraser

In popular writing and textbooks on modern cosmology it is stated that the general theory of relativity contributed in a fundamental way to the revolution in cosmology that took place in the 1920s and 1930s. Thus Peebles [1993, 227] writes "General relativity was one of the keys to the discovery of the expansion of the universe..." On the other hand, some astronomers understand the history primarily or even exclusively in terms of improvements in instrumentation and advances in the interpretation of observations, cf. Sandage [1956].

To gain insight into the relationship of theory and observation in cosmology it is useful to examine the period of the 1920s leading up to Edwin Hubble's publication of the redshift-distance law in 1929. That cosmological solutions of the equations of general relativity were derived at precisely the same time that Vesto Slipher and Milton Humason were beginning to detect large systematic nebular redshifts was simply a coincidence. The two developments were largely independent. The advances in telescopic instrumentation that made the nebular research possible followed from improvements in technology and the increased financial support for astronomy in America from government and philanthropic foundations. General relativity by contrast developed within a central European scientific culture with a strong emphasis on advanced mathematics and pure theory. In retrospect, it seems that Hubble's relation would have been detected inevitably with improvements in the size, quality and location of observing facilities; it could well have been discovered earlier or later. It is nonetheless a fact that throughout the decade leading up to the 1929 breakthrough, speculation about the redshifts was often tied in with theorizing in relativistic cosmology. Hubble was aware of Willem de Sitter's writings and explicitly cited the relativistic de Sitter effect in the 1929 paper. It was also the case that general relativists such as Eddington were among the first to explore the implications of Hubble's discovery in terms of dynamical world solutions.

Although the observational discoveries of the period were independent of theoretical work in relativistic cosmology, the converse can not be said to be true. At the time he wrote his 1917 paper de Sitter was aware of Slipher's findings through a report on them published by Arthur Eddington in the Monthly Notices of the Royal Astronomical Society. A more detailed description of these findings was presented by Eddington in his 1920 book *Space, Time and Gravitation*, where he wrote "The motions in the line-of-sight of a number of nebulae have been determined, chiefly by Professor Slipher. The data are not so ample as we should like; but there is no doubt that large receding motions greatly preponderate." [Eddington 1920, 161] It is significant that Friedmann in his 1922 paper cited both de Sitter's paper and the French translation of Eddington's book. The very high redshifts reported in these sources certainly would have raised doubts about Einstein's assumption of a static universe, and suggested the possibility of dynamical cosmological solutions of the field equations. It is also known that Slipher's findings were reported in 1923 in a widely read Russian scientific magazine published in Petrograd, Friedmann's home city. The case of Friedmann is interesting because he more than Lemaître is often seen as someone who was uninfluenced by observation and whose geometric solutions represented a prescient achievement of pure theory. It should be noted that one of the key assumptions of his relativistic solution, the dependence of the scale function only on the time, was later found to hold for the universe as a whole. The relativists were not working in complete isolation from observational work, although it is nonetheless the case that the emergence of dynamic theoretical solutions at precisely this time was a highly unusual event of which there are few parallels in the history of science.

In the work during the 1920s on relativistic cosmology no one with the possible exception of Carl Wirtz and Howard Robertson had predicted a linear redshift-distance relation, or made an attempt to configure the spectroscopic data to what was then known about distances to nebulae. The very status of the nebulae much less their distances was only being clarified during this period. To understand why an expansionist interpretation of the universe was not generally considered before 1930 it is also important to understand the intellectual atmosphere of the 1920s. What most struck scientists of the period about the spectroscopic data was the fact that it might well consist of a verification of Einstein's radical new theory of gravity. It was this theory and its revolutionary implications that excited scientists. The nebular spectral shifts seemed to offer clear and unequivocal evidence for general relativity, much clearer than the fine discriminations involved in interpreting eclipse observations. The focus of scientific attention was on the meaning of the observational data for general relativity and not on the possible fact of universal expansion.

After 1929 when expansion seemed to be the most natural interpretation of Hubble's law, the general relativists were able to turn to the until-then neglected dynamical models of Alexander Friedmann and Georges Lemaître. It is worth noting that Hubble regarded the concept of an expanding universe as a notion rooted in the general theory of relativity. In retrospect, it seems clear that if one accepts the redshifts as due to real velocities – and this is the most obvious explanation – then it follows that the universe is expanding, a conclusion which requires for its warrant no particular theory of gravity much less the formidable machinery of general relativity. In 1933 Eddington wrote that the theorists had for the past fifteen years been expecting something "sensational" [Eddington 1933, 2] along the lines of Hubble's discovery (and there could be no finding more sensational than Hubble's) and seemed almost to be taking some credit on behalf of the theorists for the discoveries coming from the great American observatories.

There seems little doubt that Hubble was concerned to emphasize the purely phenomenological character of his result, its independence from contemporary theorizing in mathematical cosmology. To concede that the redshifts were recessional velocities was in Hubble's view to accept an underlying theoretical approach to cosmology, and possibly to suppose that an achievement of skilled observation owed something to the "invented universe" of the theorist. As he emphasized in a 1936 book, "the conquest of the realm of the nebulae was an achievement of great telescopes." In this book he explicitly referred to the "expanding universe of general relativity," and implied that the acceptance of the redshifts as actual velocity shifts was dependent on acceptance of this theory. Any other explanation of the redshifts would require some new principle of physics, but Hubble felt that this might very well be necessary. His doubts about expansion intensified in the years which followed. Given the very high number assigned at the time to Hubble's constant, it seemed that the universe would have to be very dense, small and young, much more so than was indicated by general observation. In a 1942 article in *Science* he concluded that "the empirical evidence now available does not favor the interpretation of redshifts as velocity shifts." [Hubble 1942, 214]

It is true that world models based on solutions of the relativistic field equations reflected assumptions that were later found to be true of the universe as a whole. Nevertheless, that the investigation of relativistic solutions occurred at the same time as the exciting advances in nebular astronomy was, in the final analysis, an interesting historical coincidence.

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The Reception of Slater's Group-free Method in Early Textbooks on Quantum Mechanics and Group Theory

Martina R. Schneider

Shortly after the new theories of quantum mechanics were introduced in 1925 and 1926, a couple of physicists (W. Heisenberg, E. Wigner, F. London, W. Heitler) and mathematicians (H. Weyl, J. von Neumann) used group theory in quantum mechanics. On the one hand, group theory provided mathematical tools for a qualitative analysis of atomic spectra (quantum numbers, spectroscopic rules), and on the other hand, it provided a mathematical-conceptual framework for parts of quantum mechanics. There was a mixed reaction of the physicists' community to the use of group theory in quantum mechanics. Only a few physicists integrated this new method into their work. A lot of physicists had great difficulties understanding it. Representation theory which was at the core of the group-theoretical method in quantum mechanics had rarely been used before in physics. The term "Gruppenpest" (group pestilence), probably introduced by the physicist Paul Ehrenfest, became a catch word for those physicists who were opposed to group theory. (On the role of group theory in quantum mechanics and its reception by physicists in the late 1920s see, for example, chapter III.4 in [Mehra and Rechenberg 2000].)

The resistance to group theory grew when the young American physicist John Slater published a paper on the classification of the multiplet system and the calculation of energy levels of the different multiplets of an atom with several electrons [1929] – without using group theory. He proved that an old semi-empirically derived method to determine the multiplet system was also compatible with the new wave mechanics. This method was displayed e.g. in Friedrich Hund's monograph [1927a, §25], but based on the out-dated vector scheme. One only needed to set up a table with all combinatorically possible configurations of electrons, reduce it with the help of Pauli's exclusion principle to all physically possible ones and then visualize the table of configurations by a diagram. The diagram was then to be reconstructed by superposing certain simple rectangular diagrams which were related to the quantum numbers L, S (total angular momentum, total spin angular momentum). The simple rectangular diagrams which appeared in the reconstruction then gave rise to multiplets with quantum numbers L, S. This slight modification of Hund's approach in a new theoretic context became known as Slater's method. One essential step in proving the compatibility with wave mechanics, was Slater's introduction of the determinant method – which included the spin function for the first time – to construct antisymmetric wave functions.

In the group-theoretical approach one had to know about the irreducible representations of the group of spatial rotations and of permutations to determine L, S. (Friedrich Hund [1927b] had developed a different group-free approach in the new theoretical framework in 1927 which had not become popular.) Slater's group-free method was warmly welcomed by the physicists' community. Many physicists believed that this was the beginning of the end of group theory in quantum mechanics.

Despite the resistance to group theory Wigner and Weyl continued their direction of research. They were joined by several others, e.g. by the physicists H. Casimir and C. Eckart, and by the mathematician B. van der Waerden. In the early 1930s three textbooks on the group-theoretical method were published by Weyl [1931], by Wigner [1931] and by van der Waerden [1932]. All three of them used a group-theoretical method to determine the multiplet structure, as one would expect. And all three referred to Slater's method in their prefaces. This, however, is almost the only thing they have in common regarding Slater's method.

Hermann Weyl [1931] did not mention Slater's method explicitly. He referred to Slater's article in two footnotes but not in connection with the above method. He referred to it because of Slater's innovative approach to calculate energy levels [Weyl 1931, p.353, fn. 4; p.356, fn. 314]. Weyl thought that

> "Nevertheless, the representations of the group of permutations have to stay a natural tool of the theory, as long as the existence of spin is taken into account, but its dynamic effect is neglected and as long as one wants to have a general overview of the resulting circumstances" [Weyl 1931, p.viif, translation MS].

The fact that Weyl chose to ignore Slater's group-free method might seem reasonable in a book on group theory. The two other authors, however, thought differently: in addition to a group-theoretical method Wigner and van der Waerden decided to include Slater's method.

Eugen Wigner [1931, pp. 308-321] showed how Slater's method tied in with group theory. In fact, he gave a group-theoretical explanation of Slater's method. Wigner showed that the diagram was linked to characters of the irreducible representation of the rotation group. He thought that the importance of Slater's method did not rest upon the avoidance of group theory but upon its relation to group theory:

> "The significant feature of the Slater method is that *it makes it possible to avoid entirely the consideration of symmetry of the Schrödinger equation under permutation of the Cartesian coordinates alone*, by considering instead the invariance under rotations $\mathbf{Q}_R$ of the spin coordinates." [Wigner 1931, p.316, emphasis in original; translation taken from the English edition 1959, p.317].

Contrary to Slater's original aim to use a method not based on group theory, Wigner interpreted Slater's method as examplifying a deep group-theoretical connection. Thus, Wigner changed the context of Slater's method radically. The groupification of Slater's method also had the advantage that the guessing process in the end could be replaced by an easy calculation of L, S from the diagram. Another spin-off was that the limits of applicability of Slater's method became clear. The limitation of Slater's method to electrons (spin $= 1/2$) was the physical reason for Wigner to introduce the irreducible representation of the permutation group.

Unlike Wigner, Bartel van der Waerden [1932, pp. 120-124] kept to Slater's original aim. He optimized Slater's method, shortening the table of configurations and outlining a clear-cut procedure to determine L, S directly from the table without constructing a diagram. Moreover, he set up rules to reduce the problem to situations which were easier to handle and included a list of possible multiplets for electrons with angular quantum number $l \leq 2$. All of this was achieved without the use of group theory.

These different treatments of Slater's method in early textbooks on group theory and quantum mechanics can be seen as three different answers to the physicists' complaints regarding group theory. Wigner and Weyl were not prepared to include Slater's method as such in a book on group theory. They had invented the group-theoretical approach and wanted to advocate it despite resistance from the physicists' community. Wigner and Weyl both constructed the irreducible representations of the permutation group because they thought it was physically necessary. Wigner's way of including Slater's method and Weyl's omission of it both point to their conceptual preference of group theory.

Van der Waerden, however, took the physicists' complaints regarding group theory seriously. This is quite remarkable for an algebraist and shows how familiar van der Waerden was with the needs of the working physicists with whom he had close personal contacts. In Groningen, he had helped the circle around Paul Ehrenfest to understand the group-theoretical approach in 1928/29. In Leipzig, he gave a seminar on it during the winter term 1931/32 which was attended by the physicists Werner Heisenberg, Friedrich Hund and their students. Van der Waerden's textbook developed from this seminar. Thus he had first hand experience of the struggles of physicists with group theory.

The heated debate on the use of group theory in quantum mechanics also indicates a tension within the physicists' community: The two different approaches to determine the multiplet structure represent two different views on methodology in physics. The group-theoretical method was a mathematically and conceptually pleasing approach because it could explain many empirical results from one mathematical perspective. It was thus aesthetically pleasing from a theoretical point of view. However, this point of view was not shared by everybody at that time. Slater's method was a mathematically easy approach which served those physicists well who did not care about the method as long as it was simple and the results were correct. It completely satisfied the needs of the more pragmatically minded physicists. Van der Waerden's inclusion of an optimized version of Slater's method also points to a need for this pragmatic approach from the physicists around van der Waerden and within the physicists' community as a whole.

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Mathematics, Meaning and Methodology. On the Structural Development of Mathematical Philosophy of Nature from Newton to Lagrange

Helmut Pulte

The role of mathematics in 18$^{\text{th}}$ century science and philosophy of science can hardly be overestimated, though it was and is frequently misunderstood. From today's point of view one might be tempted to say that philosophers and scientists in the 17$^{\text{th}}$ and even more in the 18$^{\text{th}}$ century became aware of the importance of mathematics as a means of 'representing' physical phenomena or as an 'instrument' of deductive explanation and prediction: the rise of mathematical physics, so to speak, as a peripherical phenomenon of the new experimental sciences, and the mathematical part of physics as a methodologically directed, constructive enterprise that is somehow 'parasitical' with respect to experimental and observational data. But such modernisms are missing the central point, i.e. the 'mathematical nature of nature' according to mechanical philosophy. Some general features of mathematics under the premise of mechanism will be important to understand the general development of mathematical philosophy in the course of the 18$^{\text{th}}$ century.

'Semantical ladenness' of mathematics

On the premise of mechanism, the primary aim of natural philosophy was the determination of the *motion* of material particles under different physical conditions — the science of motion, so to speak, as the 'hard core' of natural philosophy. Motion itself being regarded as a genuine mathematical concept, natural philosophy had to be not only an experimental, but also (first and above all) a *mathematical* science. Taking this idea of motion seriously, the attribute 'mathematical' should not be understood as 'mathematics applied to science' but rather as 'science, having essentially to do with mathematical entities'. It is important to note, however, that in the course of the 18$^{\text{th}}$ century, rational mechanics — even in its abstract, 'analytical' form that can be found in the works of Euler, d'Alembert and Lagrange — never became a 'purely' mathematical exercise without physical meaning: its concepts and primary laws were located in natural reality, and (therefore) its deductive consequences were expected to be empirically meaningful.

Euclideanism

A second common feature of mathematical philosophy of nature is of equal importance with respect to the role of mathematics: Rational mechanics follows the ideal of Euclidean geometry, or, to be more precise, its concept of science is best described as 'Euclideanism' (in Lakatos's sense). Its most important feature is that its first principles are not only true, but certainly true, i.e. infallible with respect to empirical 'anomalies'. This means, first and above all, that rational mechanics should not be understood as a hypothetical-deductive, but rather as an *axiomatic*-deductive science. In other words: If the hypothetical-deductive method is "at the core of modern science" (as is sometimes claimed), rational mechanics from Newton to Kant is not modern, and if it is defined as 'modern' [neuzeitlich], this characterization cannot be true. The 'historical stability' of classical mechanics from Newton to Einstein is not only due to its empirical success, but also to its Euclideanistic leanings, and the decline of 'mechanical Euclideanism' was a necessary historical premise for its removal at the beginning of this century.

It is, however, by no means evident that primary laws of nature are 'prime candidates' for axioms of a deductively organized theory, nor is it clear whether such a 'metatheoretical coincidence' is possible at all: From natural laws the philosopher-scientist expects truth, empirical generality, explanatory power (mechanical explanation of possibly all phenomena of nature), a certain plausibility and intuitivity with respect to his scientific metaphysics and (perhaps) necessity. From first principles or 'axioms' of a theory he expects, above all, truth, deductive power (entailment of all the other laws of a theory); moreover they are thought to be neither provable by other propositions nor — due to their evidence — to be in need of such a proof. This is a central point of my discussion: Laws have to explain nature, axioms have to organize theories. But a 'congruence' of both demands is increasingly difficult to guarantee when science produces a growing body of knowledge. Traditional mechanical Euclideanism is at stake here.

Orders of science

The plural 'orders' refers to a third point which should be underlined: At the beginning of the 18$^{\text{th}}$ century, there were indeed *fundamentally* different attempts to gain a coherent system of 'mathematical principles of natural philosophy': At least Descartes' 'geometrical' mechanics, Newton's mechanics of (directive) forces, and Leibniz's dynamics, based on the conservation of *vis via*, have to be sharply separated.

Newton's *Principia* was obviously most successful in empirical respect, but it was neither unique in its intention, nor was it faultless or complete in its execution, nor was it understood as 'revolutionary' by the first generation of its readers, as far as the *principles* of mechanics are in question. 'Classical mechanics' and 'Newtonian mechanics' (understood as mechanics laid down by *Newton*) are by no means synonymous names. As far as the foundations of rational mechanics are at stake, the great 'Newtonian revolution' did not take place.

During a period of 'permanent revolution', however, so-called 'formal' elements of science gain a peculiar quality: While a 'conceptual discourse' across the boundaries of actual scientific metaphysics is hardly possible and almost futile (as is best illustrated by the famous Leibniz-Clarke correspondence), the *language of mathematics* becomes even more important for a small (and in a way isolated) scientific community that promotes rational mechanics (as is best illustrated by the continental reception of Newton's *Principia*). This does not mean a sharing of the somehow *naive* view that mathematics in the age of reason worked as a kind of *meta-language*, capable of solving even *philosophical* problems of rational mechanics and, as it were, 'replacing' the Babylonian confusion of the different tongues of metaphysics. It means, however, that mathematics played a key role in making accessible the results of one research program of mechanics to the others, that it was indispensable in integrating those parts which seemed valuable and that it was the only means in order to formulate 'towering' principles (like that of least action or virtual displacements) from which all the accepted laws of mechanics, whether they emerged from their 'own' research program or not, could be derived.

Scientific metaphysics tends towards a separation, mathematics tends towards an integration of different programs. At the end of the eighteenth century, we have one (and *only* one) system which represents *all* the accepted 'mathematical principles of natural philosophy': Lagrange's *Méchanique Analitique*. But did it keep what mathematical philosophy of nature, a century earlier, had promised?

Understanding the Change of Concepts of Science

The three abovementioned features form the basis of my outline. Its aim is a better understanding of the metatheoretical change of rational mechanics which took place in the course of the 18$^{\text{th}}$ century and is most obvious if we compare Newton's *Principia* (1687) and Lagrange's *Méchanique Analitique* (1788).

In general, I argue that there is a growing tension between the *order of nature* and the *orders of science* that leads to a dissolution of Euclideanism at the end of the 18^{th} century, which becomes most obvious in a meaning-crisis of so-called 'axioms' or 'principles' of mechanics.

Rational mechanics increasingly relied on abstract mathematical tools and techniques, thereby 'unloading' its axioms from empirical meaning and intuitivity which at first (with respect to the scientific metaphysics from which they depended) were their characteristics. This process ends in Lagrange's mechanics. It makes use of 'first' principles only as *formal* axioms with great deductive power, but these principles can no longer be understood as laws of *nature* in the original meaning. This is what, in the end, caused a 'crisis of principles' at the turn of the century. Kant, for example, tried to 'synthesize' mechanical knowledge in some principles, which are, under the premises of his system, certain and evident, but he makes by no means clear how the whole body of accepted knowledge could be based on these principles; the unique 'order of science' remains an 'projected' ideal.

The development in question was promoted by the rise of analytical mechanics and opened the way for conventionalism and instrumentalism in mechanics in the course of the following century, starting with Jacobi, Riemann and Carl Neumann and continued by Mach, Hertz, Poincaré, Duhem and others. The dissolution of 'mechanical euclideanism' is a process that continues until the end of the 19^{th} century, but is introduced by the formalisation of rational mechanics in the 18^{th} century, culminating in Lagrange's *Méchanique Analitique.*

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An Everlasting Temptation? Philosophical Perspectives on Action Principles and Variational Calculus

Michael Stöltzner

Over the centuries, hardly another principle of classical physics has to a comparable extent nourished exalted hopes into a universal theory, has constantly been plagued by mathematical counterexamples, and has ignited metaphysical controversies about causality and teleology than did the principle of least action (PLA) and its kin. Primarily responsible for the classical clashes on physical teleology and natural theology was the fact that the PLA designates, among all possible scenarios, the one that minimizes, or at least corresponds to the extremum of a certain quantity. This argumentative figure reverberated the classical topos of a parsimonious nature. [7]

Since antiquity it had been well-known, however, that there are instances where the quest for minimality fails. Already Leibniz, reflecting upon the new variational calculus, contemplated that in such cases the actual world was uniquely distinguished among the possible ones by being the most determined. To posterity, this modal view would repeatedly promise a way to circumvent the notorious metaphysical problem of physical teleology.

Modern philosophy of science has treated the PLA with neglect, more than with suspicion, despite the fact that it provides a simple scheme to formulate the basic laws of theories so different as classical mechanics, electrodynamics, relativity theory, and – in a somewhat different setting – quantum physics. [13] To the Vienna Circle and the tradition building upon it, the PLA even became a shibboleth. [10] For, attributing any genuine physical significance to the PLA – apart from its providing a convenient reformulation of the differential equations of motion derivable from it – introduced unwarranted analogies between physical processes and goal-directed behavior.

The principal aim of my research is to attempt a philosophical classification of the interpretations of the PLA that were advanced during its long history. Despite the obvious fact that such interpretations were embedded into starkly diverging philosophical agendas and were based upon a different knowledge of the mathematical intricacies of the PLA, it seems to me that, having adopted an abstract characterization of the PLA, one can distinguish three types of strategies. In describing the relationship between the PLA (and related integral principles) and the usual differential equations of motions they involve modal commitments of an increasing strength.

Taking M_u as the space of all possible motions between two points, the PLA states that the actual motion u extremizes the value of the integral $W[u] = \int L(t, u(t), \dot{u}(t))$ in comparison to all possible motions, the variations $(u + \delta u) \in M_u$. *Zeroth order*: u is *uniquely determined* within M_u as compared to the other degenerate scenarios. First order: There *exists* a functional $W[u]$ for M_u whose extremal values yield the differential equations of motion. Second order: There exist *structural features* of M_u guaranteeing that $W[u]$ attains its minimum. [11, 8]

Zeroth order formal teleology characterizes the empiricist reading of the PLA put forward by Mach, Petzoldt and Ostwald. [5] Several critics of this view emphasized that, if its antimetaphysical ambition is taken at face value, it can hardly be distinguished from the simple fact that a physical phenomenon can be described by a specific mathematical formula.

The distinction between first and second order formal teleology is modeled after the distinction between necessary and sufficient conditions for the minimality of $W[u]$. (It is true that in this way the classification becomes somewhat anachronistic because this distinction for a long time has described the major line of progress in variational calculus.) Historically, the main advocates of these interpretations were Planck and Hilbert respectively. Planck [6] was convinced that for each domain of reversible physics an appropriate PLA could be found. To him, this represented a structural feature of the real world. Hilbert [2] not only used the PLA as the main tool in the axiomatization of physical theories of both foundational and phenomenological character, it also bears striking similarities to the different steps of the axiomatic method. In his philosophical understanding, Hilbert oscillated between the belief in a non-Leibnizian pre-established harmony of mathematics and physics and a rather pragmatic attitude in case such a harmony was not in sight. The common element of his attitude was, to my mind, his quest for an independent 'architectural' justification – to use a Leibnizian term – that permitted the mathematician to deepen the foundation of physical theory. [3, 9]

If this classification based on modal commitments is sound, the question arises how to assess the ontological status of the possible dynamical scenarios. Especially in the case of second-order formal teleology, one has to make strong assumptions about counterfactual or even counterlegal scenarios in order to reach a mathematically meaningful formulation of the PLA. [1] One option would be to follow the Kantian intuition and consider the PLA, as the conjunction of the actual dynamics and the possible ones, as a formal teleological structure in the sense of Kant's *Critique of Judgement* (that is, as a structure that exhibits purposiveness without any purpose).

I shall, instead, propose a less "teleological" interpretation that comes closer to the original idea of variation. If we understand the PLA as a mathematical thought experiments, in a sense inspired by Mach and Lakatos [4], the alleged systemic properties of the ensemble of actual and possible dynamics simply express the integrity of the thought-experimental set-up. Possible worlds are meaningful only with respect to this set-up. If the thought experiment succeeds, it can well be developed into a real experiment that corroborates the law succinctly expressed by it. This is important because scholarship on scientific thought experiments takes empirical realizability as a criterion of success. If the thought experiment fails, it fails with respect to the applicability of mathematics to a physical situation. This comprises cases where the PLA is minimized by a curve that is unphysical. As with real experiments, one might search for conditions under which the thought experiment is successful or yields a definitively negative result. Such an attitude comes close to Hilbert's optimism about variational calculus that is expressed in his 20th problem. On the other hand, one may hope that explorative thought experimentation is able to unveil a scientifically meaningful mathematical structure.

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Experiment and Mathematisation in Early Electrodynamics

Friedrich Steinle

My talk focuses on the question of how domains that are treated experimentally and qualitatively may become mathematised. To use for a moment Kuhn's famous distinction, my question is how Baconian science becomes mathematical. Electricity, heat and colour provide characteristic cases in which mathematisation and quantification had to start from scratch. Other cases, like magnetism or hydrodynamics, are more complex: there had traditionally been some degree of quantification and mathematics involved. In those cases Kuhn's neat dichotomy does not work, or is at least revealed as one of degrees.

The case of electricity is particularly interesting. Throughout the 18th century, electricity was treated only qualitative (with the exception of Aepinus whose work found no response in his time), whereas a century later it was thoroughly quantified and mathematised. What is more, that process went along several unconnected strands – Poisson's electrostatic theory, Ritter's formalization of galvanism, and Ohm's introduction of quantitative notions, for example, had more or less nothing to do with each other. Even more, the domain that was eventually to become the core of all electrical theory – electrodynamics – had for several decades a curious constellation: there were two prominent, but incompatible conceptual and theoretical frameworks in parallel: action-at-a-distance theories on the one hand, and field theories on the other. It is likely that this dichotomy took its origin in different approaches towards the question how a large and unexplored domain of phenomena should be treated, quantified and eventually mathematised. In my talk, I shall focus on some significant aspects of the early phase of this development – I discuss how Ampère and his fiercest competitor Biot approached the new domain in strikingly different ways.

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Oersted's discovery of electromagnetic action provided a profound challenge for the established Laplacian program of mathematising ever wider physical domains. The fundamental notion of a central force, attractive or repulsive, directed along the straight line between its point-like centres, and depending only on their distance, seemed to be not applicable to the experimental result. The most prominent exponent of that program in physics, Jean-Baptiste Biot, took the challenge and attempted at implementing the Laplacian ideal of mathematisation. Together with his assistant Félix Savart, he set up a measuring apparatus (and was able to surmount the huge experimental difficulties!), determined the dependence of the electromagnetic effect on the distance between wire and magnetic needle, and reduced this macroscopic law to a force law between the supposedly point-like magnetic elements as centres of force. The result is known up to this day as the law of Biot and Savart.

However, his successful approach of mathematisation had strong preconditions and serious 'costs'. From the mathematical toolbox available, it was clear that the only number to be determined was the exponent of the one-term polynomial – there was just no means available to determine any more complex force law. Moreover, Biot had to choose a highly symmetric experimental arrangement and thus to ignore the bewildering complexity of the experimental results that immediately appeared in less symmetric constellations. The central problem that bothered many researchers in Europe – the problem of expressing spatial orientation that obviously played a central role – was blended out by the very apparatus. Biot's program of mathematising electromagnetism, far from being 'innocent' or straightforward, was largely characterized by enforced application of an existing mathematical toolbox and of a well-defined research program (mathematisation via precision measurement), on the cost of deliberately ignoring the puzzling and widely unexplored complexity of the new domain.

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Even before Biot could start his research, an outsider jumped into the new field. André-Marie Ampère, professor of mathematics and mechanics, and not committed to the Laplacian program, started his feverish work with a wide experimental search for regularities or laws, and quickly came to formulate two "general facts" or laws. In the first of them, later known as Ampère's 'swimmer law', he invented new concepts, such as 'left' and 'right' of a current, in order to specify in a general way the direction in which the needle's north pole moved in front of a wire with a definite direction of electric current. He even claimed – somewhat prematurely – that even the complex electromagnetic effects could be covered by his two laws.

Before he could work out this claim in detail, however, he changed his research agenda drastically. On complicated pathways, he had realized an effect in which two spirals of wire with a current running through them attracted or repelled each other, without any ordinary magnetism involved. He saw a completely new domain of research ahead: the domain of interacting electric currents, soon to be labelled "electrodynamics" by himself. And from the outset, he pursued the goal to approach this domain mathematically – a goal to which he would devote several years, and in favour of which he dropped his previous experimental explorations.

In his attempts to mathematise, he drew on the resources around him, i.e. the Laplacian conceptual scheme, though with significant deviations. In particular, he saw more clearly than Biot (and due to his initial exploratory work) that the action was not isotropic, i.e. that angles had to be introduced in the law. He considered very small (perhaps infinitesimal) current elements as centres of a force acting along the straight line between them and depending both on the distance and the angles formed by the two elements. In a bold extrapolation of two empirical results – attraction of parallel currents and repulsion of antiparallel currents – he suggested the force would depend on the cosine of one angle, and the sines of the two others. As to dependence on the distance, he took an inverse square law for granted. The first force law he presented:

$$F \sim \tfrac{1}{r^2} \sin\alpha \sin\beta \cos\gamma$$

was essentially a suggestion with only very weak empirical footholds.

In order to test that formula, Ampère had a complicated apparatus constructed, with the idea to directly measure the dependence of the force on the angles. However, while he described the apparatus in all detail, he never gave any results – it is most likely that he never arrived at stable outcomes. This is not too surprising, since with electro*dynamic* measurements he indeed had to face even more experimental difficulties than Biot had with his electro*magnetic* measurements.

For Ampère, this failure meant that his initial program was stuck – the way to mathematisation via quantification was blocked. But already a few weeks later, Ampère came up with proposing a different pathway, a pathway that involved a bold and hypothetical postulate. If one considered a current element as being made up by two arbitrarily chosen components, the postulate claimed that the electrodynamic force exerted by the current element was equal to the sum of the forces exerted by its components. Ampère was well aware that this postulate needed experimental support, but he would deliver that support only much later. For the time being, however, the postulate allowed him to *mathematically derive* the law from the few empirical results he already had: attraction of currents in parallel position, repulsion in antiparallel position, and no action in rectangular position. It is striking to see that the resulting formula

$$F = \tfrac{gh}{r^2}(\sin\alpha \sin\beta \cos\gamma + \tfrac{n}{m} \cos\alpha \cos\beta)$$

was obtained without a single measurement. Nevertheless, when Ampère presented it proudly to the Academy, he insisted on it being empirically founded.

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The two pathways towards mathematisation taken by Biot and Ampère, respectively, differ drastically. While in Biot's case mathematisation was achieved, at least in principle, via measurement, i.e. via quantification, in Ampère's case we have a process of mathematisation without quantification. And Ampère would cultivate this pathway further: his famous procedure of equilibrium experiments was essentially designed to allow mathematisation while avoiding (the technically difficult or even impossible) measurement – in other words, to reduce the empirical input in the mathematisation process to yes-no responses to well-designed questions. The picture of possible pathways towards mathematisation of electrodynamics will get even much richer and more complex as soon as one includes the cases of Ohm, Faraday, Thomson, Neumann and Maxwell. The relation of quantification and mathematisation is by no means straightforward, but may widely vary, depending on the experimental, conceptual, and mathematical resources available and on the specific scientific cultures. The topic deserves much further research.

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The Rise of Mathematical Modeling

Gerard Alberts

The appeal to mathematical thought has in the 20th century taken on the form of a procedure: mathematical modeling. Bernard de Fontenelle (1699) wrote how knowledge is improved when mathematical thought (esprit géometrique) is transferred to it. While he had to catch a mathematician to write a better book, today we may follow a procedure. Putting mathematics to use for 150 years followed the idea of "applied mathematics" (1800-1950). Then it was superseded by the more general idea of mathematical modeling. Two traditions pointed in that direction. One, from inside applied mathematics where the truth of theory in examples like kinetic theory of gases and electromagnetics had become problematic. Hertz reacted proposing his notion "Bild" (1894), which only gradually was replaced by model, mathematical model. Two, from the outside mathematizing tendencies in economics, social sciences, psychology and the like grew into full-blown qualitative sciences. Burgers' hydrodynamics and Tinbergen's econometrics are given as (Dutch) illustrations of both developments, showing how mathematical model was presented not by the skeptics, but by those who as long as possible stuck to the search for truth. Another Dutchman, David van Dantzig, described mathematical modeling as a practice, a procedure: "General Procedures of Empirical Science." (1946)

Adjusting Mathematics and Experiment: Episodes from Early Aerodynamics, 1900-1918

Moritz Epple

The history of hydro- and aerodynamics has received new interest recently for a number of reasons: these fields, crucial to major civil and military technologies of the 20th century, have offered and continue to offer some of the most difficult challenges to mathematical thought and computation since the mathematization of flows was first envisaged in the 18th century [1], [2].

Aerodynamics, in particular, experienced a rather singular period of formation as a science. After the first motorized flights – carried out with virtually no scientific underpinnings – had shown that a new technology was emerging, the pressure to develop suitable scientific tools for aviation became rather high (not least from the military side). While the subsequent development is still most often described by a smooth success story, the talk discussed some of the difficulties that needed to be overcome before a roughly consistent and roughly adequate theory of the drag and lift of airplanes, a theory of wings and propellers, etc., could be achieved.

One of the decisive issues that required subtle research was the mutual adaptation of mathematical and experimental tools for studying the problem of flight.

From a mathematical perspective, the situation before 1900 was characterized by the availability of differential equations (the Euler equations, the Navier-Stokes equations) which were far too difficult to solve for a realistic situation of a flying body. From an empirical perspective, the situation was not much better: Empirical findings and measurements, e.g. in small wind tunnels, were hardly reliable and impossible to reproduce with some precision in other places.

However, things did not change substantially after the first theory fragments were proposed which enter today's successful approach to flight. This holds both for the Joukowski-Kutta theory of lift, introduced in the early 1900's and idealizing the flow along wings as a two-dimensional ideal flow that can be treated by means of conformal mappings, and for Ludwig Prandtl's boundary layer theory, proposed in 1904. In particular, the phenomenon of the so-called 'separation of the boundary layer' made the basic assumption of Joukowski's and Kutta's explanation of lift (conformal flows) highly problematic. The tension between idealized descriptions and this and other phenomena related to turbulence called for empirical investigation.

The same was true for the next phase of theory development, the work of Ludwig Prandtl's group in Göttingen toward a new, properly three-dimensional theory of wings immediately before and during World War I [3]. Based on a linearization of the Euler equations, this approach was closer to Joukowski-Kutta lift theory than to boundary layer theory and hence equally problematic. Moreover, the three-dimensional situation created additional difficulties (for some time, the assumed distribution of vorticity, and hence lift, along a 'lifting line' produced singularities in local flow speeds contradicting the linearization assumptions).

The reason why such approaches, based on Euler's equations, could nevertheless achieve good results for the flight problem was understood in consequence of another strand of research which had quite different aims. This was the calibration of the experimental systems of aerodynamics that turned out to be necessary after the first two major large wind tunnels had been built in 1908 and 1909 in Göttingen's *Modellversuchsanstalt*, on the one hand, and in Paris by Gustave Eiffel, on the other. While the former institution was built some months earlier, Eiffel was the first to actually make measurements.

A detailed discussion of the fairly large discrepancies of the measured data on the drag of spheres between Paris and Göttingen shows that only the surprising twists and turns of this competition for the most authoritative measurements helped to understand the fundamentals of reliable wind-tunnel testing, including the scaling effects depending on the Reynolds number of flows. Prandtl finally also explained in terms of his boundary layer concept why the transition from the 'subcritical' to the 'supercritical' regime of flows around a body could lead to very different data on drag. It was a quite unintended outcome of this attempt at understanding *experimentation* that the same phenomenon also helped to explain why flight could be described *mathematically* in terms of Eulerian approaches: In the supercritical regime, the regions of turbulence (and the form of the separated boundary layers) were such that they did not preclude flow patterns roughly corresponding to the assumptions and predictions of lift or wing theory.

In conclusion, these episodes show how closely *mathematization* and *experimentation* were intertwined in the formation of aerodynamical science. This may be characteristic of the hybrid research configurations of twentieth century science, but it may also be a feature of using mathematics in a physical context that is more common than usually thought. In the view of the speaker, similar episodes call for a closer cooperation between historians of mathematics and historians of experimentation.

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The Hopfield model: In between the General and the Specific

Andrea Loettgers

Models are an integral part of contemporary scientific practice. In recent studies which focus on how scientists construct, apply, and manipulate models historians and philosophers of science [1, 2, 3] have revealed the large number of different functions models fulfill in science. Here the role of models in interdisciplinary research contexts will be discussed by examining a specific neural network model, the Hopfield model. The model mimics the brain function of auto-associative memory. The model was introduced in 1982 by John Hopfield in an article entitled *Neural Networks and physical system with emergent collective computational abilities.* [4] It was constructed at the interface of theoretical physics, neuroscience, and computer engineering. An examination of the development of the model in the different scientific disciplines shows that the model was received, applied and modified differently by the scientists in the respective scientific communities. The way scientists used the model depended on various interdependent factors: the modeling traditions, scientific cultures, and the understanding of the nature of biological systems. These factors also turned out to be obstacles to the formation of interdisciplinary research. Only by the introduction of new research fields such as computational neuroscience, DNA based computing, and synthetic biology did interdisciplinary research develop. John Hopfield started his scientific career in solid state physics. By the beginning of the 1970's, he went into molecular biology and, later on, he became interested in neuroscience. A closer look at the construction process of his neural network model shows that he approached the problem with the attitude of a theoretical physicist. Computational properties of the brain, he wrote in his 1982 paper, could result from collective properties, meaning from the interaction between the neurons in the brain. Collective properties is a concept which is used in theoretical physics in the investigation of many body. By making the assumption that computational properties result from cooperative properties he drew up on a tradition of modeling many body problems in theoretical physics. In this tradition, models are constructed as simple as possible in order to find the crucial parameters responsible for the observed phenomena. In this case, the model becomes a representation of the phenomena independent from any specific systems [5] and thus can function as a representation for a class of systems which share the properties represented by the model. Neuroscientists reacted very sceptically to the Hopfield model. The model contradicted their daily experiences of the complexity of the brain structure and the processes taking place in the brain. They could not see how to apply the model to their research. Theoretical physicists, on the other hand, found the model very attractive. They used the model in their research on disordered magnetic systems and at the same time they became active in the field of neural networks. The abstract character of the model appealed to computer engineers who started to develop and construct new computer architectures. Depending on the different scientific backgrounds, scientists applied and manipulated the Hopfield model differently, which means it fulfilled different functions in the different scientific disciplines. But different modeling traditions and understanding of the nature of biological systems prevented the model from mediating between the different disciplines and the formation of interdisciplinary cooperation.

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Mathematicians Doing Physics: Mark Kac's Work on the Modeling of Phase Transitions

Martin Niss

Modeling of physical phenomena is an important activity of physics. One of the factors that shape this activity is mathematical tractability because only tractable models are of any use. Since physicists are not always capable of carrying out a mathematical analysis of physically relevant models, there is room for the skills of mathematicians. One of the fields were mathematicians have assisted physicists is the attempt to understand phase transitions, such as the boiling of liquids or the magnetic phase transition (where the heating of a magnet takes it from a magnetic regime to a non-magnetic one). The talk examined the similarities and differences between mathematicians and physicists in shaping the field of phase transitions after World War II.

Shortly after the advent of quantum mechanics in 1926-1927, physicists designed and examined a number of microscopic models of phase transitions. Their experiences gradually led to two lessons. First, an approximation scheme used widely to examine these models does in fact give incorrect results. Second, without this approximation scheme only very few models are soluble. Physicists tried to get scientists who saw themselves as mathematicians interested in these matters and this "campaign" did result in the attention of several mathematicians. These mathematicians assisted in two ways. First, they showed with full mathematical rigor that the formalism of statistical mechanics is capable of accommodating phase transitions. This rather technical contribution was seen by physicists as important because it secured the foundations, but as giving little physical understanding. The second contribution was of much more direct relevance in this respect, as the mathematicians participated in the examination of models. Some mathematicians, including John von Neumann and Norbert Wiener, tried unsuccessfully to derive the physical properties of models proposed by physicists. A few mathematicians invented physical models on their own and were much more successful. Physicists saw these models as significant contributions to the development of the understanding of phase transitions. So, in order to contribute the mathematicans couldn't simply use their mathematical skills to solve problems posed by the physicists. To help, they had to participate more directly in the shaping of the field. In short, they had do physics.

One of the most important mathematicians doing physics was the Polish-American Mark Kac (1914-1984). He started his career in pure mathematics, mainly probability, analysis, and number theory. His collaboration with the physicist George Uhlenbeck during World War II got him interested in physics and in the last part of his career he worked extensively on phase transitions. However, throughout his life, Kac consistently saw himself as a mathematician. Kac's work differed in subtle ways from the work of contemporary physicists and thus shed light on the relation between mathematicians and physicists and their disciplines in a modeling context.

Kac proposed a host of models of phase transitions. Here the focus is on two of those: his first model in this area and his most successful model. Kac introduced the first model, the so-called spherical model, in 1952 as an approximation to another model which represents a magnet [1]. In the five years between his invention of the new model and his publication of it, Kac came to the conclusion that the new model was in fact a better description of magnets than the original model. The second model was designed by Kac to give a constructive proof of the existence of a microscopic model, which leads to the van der Waals equation, a widely used empirical equation for gases [2]. Kac's notebooks reveal that he immediately was able to see a connection between this model and the mathematical theory of stochastic processes; this connection allowed him to derive the properties of the model. At the time, very few physicists were well-enough versed in the theory of stochastic processes to see this connection, so his mathematical background enabled him to provide the solution to this important physical problem.

The two models were received differently by the physics community. Most physicists rejected the first model on the grounds that it was too far removed from physical reality to be of more than purely mathematical interest, but Kac maintained his view despite this criticism. His lack of physical background probably made him more susceptible to models which were unacceptable to contemporary physicists. The second model was much more favorably received as it solved an important physical problem. Since the van der Waals equation was found to be an empirically inaccurate, it did not matter much that the model was not a realistic description of real gases. What mattered was that the model leads to the correct equation. The first model, on the other hand, was supposed to say something about real systems according to Kac. For real systems, the physicists have some demands if they are to accept a model and this model did not meet these demands.

In addition to the proposal of models, Kac tried to put the ideas derived from the study of such models into a more unified description of phase transitions [3, 4]. He tried to find the mathematical mechanism responsible for phase transitions, building on the observation that for both of the above models, the phase transition was reflected in a double degeneracy of an eigenvalue of a certain integral operator. This program did not receive much attention from the physicists. Even among those interested in such a mechanism of phase transitions, Kac's ideas were neglected because they were not found to yield insight into the *physics* of phase transitions. The linear operators could not be given a physical interpretation. Consequently, physicists thought that it was not possible to go from such a mathematical mechanism to the physical mechanism of phase transitions.

Several mathematicians have paricipated directly in the modeling of physical phenomena after World War II and it is the hope of the speaker that further studies will analyse their contribution to this important physical activity.

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Mathematics "for its Connection with the Physical Sciences:" Educational Initiatives at Mid-Nineteenth-Century Harvard

Deborah Kent

Nineteenth-Century American science was both geographically and intellectually removed from scientific and mathematical activity in Europe. Early in the century, mathematics in the United States was strictly an undergraduate subject and very few—if any—of the amateur mathematical practitioners conceived of creating new mathematical knowledge. [1, 2, 3, 4] Those in favour of improving scientific education entered into debates about the role of higher education in the context of American democracy. While echoes of Benjamin Franklin and Thomas Jefferson connected notions of democratic practical education with inexact ideas like invention and vaguely agricultural mechanical studies, new needs for scientifically trained Americans emerged from the nation's growing businesses and expanding infrastructure. [5, 6, 7] Many in the young republic hesitated to import *all* the necessary expertise, but America needed educational reform before it could build and replenish its own supply of scientists.

There was, however, a small group of self-appointed American scientific elites who self-consciously worked to organize the pursuit of science and to establish it as a legitimate profession in the United States.[8] As the mathematician among them, Benjamin Peirce introduced major curriculum reforms at Harvard over the course of his career there, from 1833 to 1880. The archival record indicates Peirce's educational objectives and reflects his efforts to elevate the level of general mathematical education while working to reorient the traditional college curriculum towards one that would encourage scientific education and foster research.

Peirce based his early arguments for renovating the mathematical curriculum on the utility of mathematics and its connection to physical science. In 1839, he successfully introduced an elective system in mathematics that offered students options after the first year of mandatory algebra and geometry. Those who chose to continue for another year could study applied mathematics, such as engineering, surveying, and navigation, or a course designed for future school teachers. The final option was a three year course of demanding study in pure mathematics under Peirce's careful supervision. Although the practical course was very popular, Peirce worried that the engineering program lacked a theoretical foundation. A different concern plagued the Harvard administration, who feared their institution would begin to favour the sciences and, as they saw it, deteriorate into a trade school.

The foundation of the Lawrence Scientific School by Harvard in 1847 provided somewhat of a compromise between elitist conceptions of higher education and constraints of institutional finance and democracy.[9] Peirce and his persistent friends persuaded textile magnate Abbott Lawrence both to fund the school and to specify that it be dedicated to "the acquisition, illustration, and dissemination of the practical sciences forever."[1] With the École Polytechnique and German universities as his model, Peirce designed a demanding course of mathematical study for the Lawrence Scientific School. In 1848, the syllabus of one course at the Lawrence Scientific School under Peirce included Lacroix's *Calcul différentiel et intégral*, Cauchy's *Les Applications du Calcul infinitésimal à la Géométrie*, and Monge's *Application de l'Analyse à la Géométrie*. Other coursework included work of Laplace, Biot, Airy, Gauss, and Bessel.[10] Although it is difficult to determine exactly the number of students Peirce taught at the Lawrence Scientific School, it is easy to conclude that this curriculum was entirely too advanced for them. Peirce nonetheless considered it a great success.

Despite Peirce's optimism, the Lawrence Scientific School deteriorated within a few years and eventually closed completely. It and the elective system were both essentially failed attempts to begin a tradition of mathematical research and engineering excellence at Harvard. Still, within the context of democratic discontent about higher education, Peirce employed the argument of utility to restructure scientific education at Harvard. He managed to encourage more advanced science, to open the door to practical training, and to bring about significant changes to the Harvard curriculum. Although it would be the mid-1870s before this curricular flexibility would be restored, Peirce had introduced Harvard to structured scientific education.

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Modeling in Climate Sciences: Historical, Epistemological, Anthropological and Political Aspects

Amy Dahan

My starting point in this subject was:

- An interest in WWII (near discontinuous change in a lot of fields, particularly meteorology), and Cold War; and the beginning of the computer era
- My paper on the case study of meteorology which revealed important tensions between understanding and predicting,
  - *Archive for Hist.Ex.Sciences,* 2001, **55**, 395-422.
  - *Les Sciences pour la Guerre*, (with D. Pestre) Ed EHESS, 2004.
- An interest in the history and epistemology of models and modeling practices
  - Amy Dahan & Michel Armatte: *Revue d'Histoire des Sciences*, **57**(2), 2004/05, 245-303.

There are two moments of rupture in the history of models and modeling

- First turning point in the 1950's;
- Second turning point in the 1980's:
  - Shift in the objects, phenomena, and systems under consideration: increasing complexity, multiple interactions and feed-back, multiple scales and temporalities;
  - generalized using of computers and numerical simulations;
  - interest for macroscopic topics;
  - new cultural hierarchies and values.

Climatology seems a paradigmatic example of this second moment.

Let us come to Climate Studies

Three main forces shaped climate modeling in the 1970's:

- The development of instrumentation (satellite observations), very well funded by the 1960's and 70's; which provided a new and huge amount of data;
- Second booming technology: computing, and its exponentially growing capacity;
- The emergence of environmental awareness about the anthropic greenhouse effect, in the late 1980's (the International Panel for Climate Change (IPCC) is created in 1988).

We can underline three main interdependent characteristics of Climate Studies for the late 1980's:

- Anthropic sensitivity (forcing by greenhouse effect). There is a lack of a "reference trajectory" and that makes the validation of models problematic.
- It is both a scientific and political field: Emergence of the greenhouse effect on the international political scene; IPCC is created in 1988. Science is closely linked with expertise.
- Integration and feedback loops. Integration (oceans, soils, ice, socio-economy etc) is absolutely necessary, given the systems' complexity and it affects the certainty and reliability of the predictions derived from models.

The main contemporary tool: Global numerical models

In fact, the meteorological tradition became hegemonic in climatology

- General Circulation Models (GCM): 3-dim representations of the atmosphere's movements and changes in its physical state.

In each GCM, there are two parts:

- Dynamical part of the model: difficulties about the initial state (data-laden models), about the boundary conditions (ground surface, vegetation cover, ice fields).
- The dynamics/physics interface: "parametrizations" of physical phenomena under the grid size, e.g., clouds. Diversity and complexity of these practices. No universality in these practices.
- For the scientists, what is the priority? To gain a better understanding to disturbances or to produce the most realistic representations? A source of controversies and debates...

1985-1995: Reconfiguring the field

- Creation of IPCC; links between science and politics became stronger, climate change became a major scientific and geopolitical issue;
- The ocean-atmosphere coupling (1992, 95): importance of J-L. Lions' role;
- Modeling climate change, main scientific trends:

  - acceleration of coupling activity, towards a generalized science of couplings?
  - tendency to integrate a growing number of climate factors (computer here is crucial): carbon cycle, chemical pollutions...
  - multidisciplinarity, collectives projects, extended networks.

Scenarios

- Methodology of the third IPCC report: 3 stages
  - define economic scenarios of the future evolution of greenhouse gas *emissions*,
  - use biochemical models (carbon cycle models...) to establish scenarios of future atmospheric *concentrations* of the greenhouse gases,
  - use GCMs forced by these concentration scenarios to evaluate climatic changes.
- Scenario: halfway between models and narratives.

Three new epistemological aspects

- The particularly heterogeneous and disunified basis on which models of climate change are constructed.
- Computers (and the Web) play a central role in practices and methodology. They are crucial to overcome the disparities and to initiate an integrative process. (See Galison: Computer Simulations and the Trading Zone, in The Disunity of Science, Peter Galison and David J.Stump (eds), Stanford: Stanford University Press, 1996, p 118-157).
- the shift of attention from models and towards modeling, to introduce the category of actors into modeling processes.
  - All constructed models are the outcome of collective work and each model coupling represents an extension of the community of the scientists involved, as well as an increased complexity of the actors' configuration.

Analytic anti-reductionism, transdisciplinarity and networking arguably define the methodology of today's climate change studies.

Political Stakes of the Field

- Global Climate Modeling: is it a **"North Science"?**(!)
  in the sense that the privileged methodology is based on partial differential equations, so it:
  - obliterates the past (the emissions before 1990 in developed countries).
  - "naturalizes" the present (inequity between North and South).
  - globalizes the future (CO2 molecules).
- Globalization and local priorities :
  the physical treatment (the choice of a mean temperature) privileges the global rather than the local.

- Is the representation of countries from the South sufficient?
- Controversies between different priorities: mitigation or adaptation to climate change etc.

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The Contingency of the Laws of Nature in Émile Boutroux and Henri Poincaré

Michael Heidelberger

This article deals with the influence that philosophy had upon mathematics at a time when a new conception of mathematics was coming to the fore. The French philosopher Émile Boutroux (1845-1921) developed a philosophy of nature according to which natural laws lack all necessity. Instead, he argued, they are endowed with contingency as an inherent property. According to this terminology, a law is contingent if it is not completely determined by past events and if the deviation from the determined case is so small that, in most cases, it cannot be detected empirically. This comes close to a form of indeterminism. Boutroux did not regard his assumption of contingency as a metaphysical or anti-empirical, arbitrary postulate, but, rather, as a precondition which is actually more in line with empiricism and much less metaphysical than its deterministic counterpart. The philosophical consequences that follow from this conception led him to a new view of science and mathematics, which became very influential in the Third French Republic. The list of Boutroux's students reads like the intellectual "Who's Who?" of the time. Many philosophers and scientists contributed to this debate – among them Boutroux's brother-in-law Henri Poincaré. It can be shown that the founders of Pragmatism, Charles Sanders Peirce and William James, profited from Boutroux's ideas. Last but not least, Boutroux also influenced his son, the mathematician and philosopher Pierre Boutroux (1880-1922).

The main work in which Boutroux developed his outlook was his 1874 dissertation "De la contingence de la nature." It grew from two seeds: First, Boutroux adopted a thorough critique of Hegel's deterministic philosophy of history from the German philosopher Eduard Zeller, whom he had met in 1868/69 during a year-long stay at the University of Heidelberg on the eve of the Franco-Prussian War. Second, it resulted from a thorough critique of Descartes' philosophy of mathematics, which he laid down in a short Latin thesis accompanying the dissertation.

Boutroux argued there that if the course of nature is rooted in divine will – as Descartes had thought – and if – against Descartes – God's immutability manifests itself in constant creativity, the universe must be conceived of as being intrinsically variable, diverse and full of incomparable individuality. Not all changes can be lawful changes. In 1894, Boutroux expanded his perspective and complemented it with a detailed discussion of the natural sciences and mathematics of his day. (His later books dealt with, among other topics, the relation of science to religion and the philosophy of William James.)

From his theory of contingency, Boutroux was led to defend a fundamental disunity of science. This view broke completely with the doctrine avowed by Plato, Galileo, Descartes and others that the book of nature is written in mathematical letters, or, as Descartes put it, that mathematics (extension) is to be identified with the essence of material things. Boutroux's view is that the special sciences cannot be reduced to fundamental ones and that each science has a certain autonomy from the other sciences. Each science is guided by a special 'regulative idea' which is developed by human understanding itself in order to render nature understandable. With this, Boutroux comes very close to Auguste Comte, who had also taught that the different sciences are ordered in a hierarchy of emergent and irreducible levels. In the same way, mathematical laws have to be regarded as free creations of human understanding. Although they are, it is true, guided by experience, they do not express a synthetic content. On the other hand, the laws of mathematics are not purely analytic either.

If we compare Boutroux's outlook with Henri Poincaré's philosophical views, we find a striking similarity: mathematics, Poincaré maintains, is to some extent an arbitrary enterprise that is only regulated by criteria of consistency and human convenience, but not of truth. Nevertheless, mathematics is not just a tautological enterprise but a creative undertaking. As Poincaré wrote in his "La science et l'hypothèse": "The object of mathematical theories is not to reveal to us the real nature of things; that would be an unreasonable claim. The only object is to co-ordinate the physical laws with which physical experiment make us acquainted, the enunciation of which, without the aid of mathematics, we should be unable to affect." (Poincaré 1902, 51f.) The only reality we can attain pertains to the relations of things but never to the objects themselves.

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Poincaré and the Equations of Mathematical Physics

Jean Mawhin

Poincaré has occupied, from 1886 till 1896, the chair of mathematical physics and probability at the Faculty of Science of the University of Paris. His lectures, which have been published in a dozen of volumes, deal with most aspects of classical mathematical physics.

The most important results of Poincaré on the partial differential equations of mathematical physics are contained in three substantial memoirs published between 1890 and 1896:

[1] Sur les équations aux dérivées partielles de la physique mathématique, *American J. Math.* **12** (1890), 211-294. Oeuvres, tome IX, 28-113

[2] Sur les équations de la physique mathématique, *Rend. Circolo Mat. Palermo* **8** (1894), 57-155. Oeuvres, tome IX, 123-196

[3] La méthode de Neumann et le problème de Dirichlet, *Acta Math.* **20** (1896-97), 59-142. Oeuvres, tome IX, 202-272.

The memoir [1] first introduces the famous sweeping out method (balayage) for proving the existence of a solution to the Dirichlet problem for an arbitrary domain. This is followed by a heuristic approach to the eigenvalue problem for the Laplacian submitted to Fourier boundary conditions. Poincaré first rediscovers H. Weber's recursive determination of eigenvalues and eigenfunctions through minimization of the generalized Dirichlet integral on subspaces orthogonal to the previous eigenfunctions. But he completes Weber's result with an upper bound for the eigenvalues which is equivalent to Fischer's minimax characterization of the eigenvalues, and with a lower bound of the eigenvalues, for the case of Neumann boundary conditions, based on a first version of what is nowadays called Poincaré's inequality for functions with mean value zero.

The memoir [2] gives the first rigorous existence proof of an infinity of eigenvalues going to infinity for the Laplacian under Dirichlet boundary conditions. It is based upon an original expression of the solution of the forced Laplacian as a meromorphic function of the eigenvalue parameter. The proof uses an improved version of Poincaré's inequality mentioned above. In the remaining part of the paper, Poincaré tries to extend the results to the more general Fourier boundary conditions. He is not successful but anticipates at this occasion the concept of weak solution.

The memoir [3] extends the Neumann method, looking for the solution of Dirichlet problem as a double layer potential, from the case of a convex domain treated by Neumann to that of a simply connected domain. It has been the main source of inspiration for Fredholm's theory of integral equations.

Besides those fundamental results, one still owes to Poincaré the first general solution of the telegraph equation and original results on the heat equation.

G.D. Birkhoff's Unpublished Paper on 'Some Unsolved Problems of Theoretical Dynamics'[1]

June Barrow-Green

Birkhoff's first paper on dynamics, which was published in 1912, marked the beginning of a new phase in dynamical theory.[2] Not only did Birkhoff explicitly consider a general dynamical system as opposed to addressing a particular dynamical problem, but he also thought in terms of "sets" of motions — more specifically "minimal" or "recurrent" sets of motions — as opposed to thinking solely in terms of a particular type of motion. Later the same year he created an international sensation by supplying a proof of Poincaré's 'Last Geometric Theorem'.[3] He continued to work on dynamics for the rest of his life, producing many important papers in the area, notably on the restricted three-body problem, stability theory, periodic orbits, and ergodic theory. In particular he was deeply influenced by Poincaré's great treatise on celestial mechanics — the three volume *Les Méthodes Nouvelles de La Mécanique Céleste* — which had appeared in the last decade of the nineteenth century, with the result that much of his most important work related to dynamics and the theory of orbits. Indeed he continued Poincaré's work in so many ways, that the Russian mathematician Nikolai Krylov called him 'the Poincaré of America'.[2]

In 1927 Birkhoff published his seminal text *Dynamical Systems*, which derived from a lecture series he had given at the University of Chicago in 1920. It was the first book on dynamics to deal not just with a single problem, or type of problem, but instead to tackle the most general class of dynamical systems. As Bernard Koopman pointed out in his review, dynamical systems theory had by this date become far removed from its origins in the search for solutions to physical problems and had 'taken the aspect of a deep analysis of methods'.[4] That it had done so was in no small part due to Birkhoff. In his hands the subject had taken on an increasingly abstract character becoming more and more topological. So much so in fact that in the 1950s a new sub-discipline emerged called topological dynamics.[3]

From the end of the 1920s Birkhoff began to draw up lists of unsolved problems in theoretical dynamics. In 1928 he gave a series of lectures at the University of Berlin entitled 'Einige Probleme der Dynamik' ('Some Problems of Dynamics')

[1] See [1].

[2] On the 9$^{\text{th}}$ August 1924 Raymond Archibald, who was in Toronto for the International Congress of Mathematicians wrote to Birkhoff: "The meetings so far have been of extraordinary appeal. Some 600 scientists came from overseas to the association meetings. I spent all day, afternoon and evening, with Russian, Swedish and Norwegian mathematicians—Phragmen, Ore, Malmquist, Kryloff, Bjerkness. Kryloff is a magnificent man and wanted especially to meet you. He said, among other things 'Birkhoff is the Poincaré of America'." Birkhoff Papers, HUG 4213.2 (Box 4, 1924 A-Z), Harvard University Archives.

[3] "By topological dynamics we mean the study of transformation groups with respect to those topological properties whose prototype occurred in classical dynamics. Thus the word "topological" in the phrase "topological dynamics" has reference to mathematical content and the word "dynamics" in the phrase has reference to historical content". [5]

and in 1929 the lectures were published in a condensed form.[6] Having emphasised the importance of qualitative dynamical ideas for the exact sciences, he briefly considered some simple examples before listing eight problems, most of which were concerned with the notion of stability. In 1937 he gave a lecture at the Institut Henri Poincaré in Paris entitled 'Quelques problèmes des la Dynamique théorique'.[4]

But it was in a lecture in September 1941, at the fiftieth anniversary symposium of the University of Chicaco, that he gave his final list of unsolved problems of theoretical dynamics. A summary of his lecture[7] was published soon afterwards and by the time of his death in 1944 he had completed a draft of an extended version which was promised for publication in the *Recueil Mathématique de Moscow.* The draft, which consists of some 40 pages of annotated typescript, survives in the Birkhoff papers at Harvard University Archives.[5]

The nature of the paper is clear from the opening two paragraphs:

"It scarcely seems too much to say that all the basic problems of point-set theory, topology, and the theory of functions of real variables present themselves naturally in purely dynamical contexts. Some of these dynamical problems are best formulated and solved in terms of an underlying abstract space, as important recent Russian and American work has shown. Others are inherently of more special character.

In the present paper I venture to set forth certain unsolved problems of this type which seem to me worthy of further study. The problems are arranged as much as possible in order of decreasing abstractness. They are formulated in terms of positive conjectures in the belief that this procedure is most likely to stimulate further research. In each case indications of the underlying reasons for these conjectures are made. Some new definitions are given . . . and some partial results are deduced etc."

After a lengthy introduction, Birkhoff discusses sixteen problems. The first ten are formulated in terms of abstract spaces; the eleventh is concerned with extensions of results of Sundman on the three-body problem to the motion of a gas; and the last five, which are topological in nature, are concerned with n-dimensional spaces. The final problem, which embodied conjectures concerning a conservative transformation of a two dimensional ring into itself in cases where Poincaré's last geometric theorem does not apply, was the part of the paper that Birkhoff thought his audience would find the most interesting. He ended: "In concluding this unusual form of paper, I venture to hope that the conjectures made will accelerate further advances. However, it must be confessed that most of these problems present difficulties which may be difficult to surmount."

Given the occasion — the Chicago semi-centenary — and given Birkhoff's position at the time as one of the leading figures, if not the leading figure, in American mathematics, it is worth speculating whether Birkhoff in choosing the topic for

[4]No written copy of the lecture appears to have survived. Birkhoff referred to the lecture in 'Some unsolved problems of theoretical dynamics', Note 1. See Footnote 1 above.

[5]See Footnote 1.

his talk had lingering at the back of his mind another occasion and another set of problems, a set of problems that had established the agenda for mathematics in the 20th century. Was Birkhoff perhaps seeking to emulate Hilbert's address to the International Congress of Mathematicians in Paris in 1900? It is of course a question that is impossible to answer and the fact that the paper never got published meant that even if Birkhoff did harbour hopes in that direction, there was little, if any, chance for them to be realised. While mathematicians could get an overall sense of Birkhoff's ideas from the summary in *Science*, they did not have access to the detail they needed in order to make progress along the lines that Birkhoff anticipated. Furthermore, the fact that the summary was published during the War meant that in all likelihood it got subsumed under other rather more pressing concerns.

Consideration of Birkhoff's unpublished paper raises several questions, relating both to the problems themselves and to the context in which the paper was produced. For example:

(1) Which, if any, of his problems have been solved? If so, when, by whom and how?
(2) Was Birkhoff's approach a fruitful one? Have any of the problems (or other problems similarly formulated) led to any new/significant developments?
(3) What can be said about Birkhoff's choice of problem?
(4) To what extent is the manuscript (in)complete?
(5) How does Birkhoff's paper relate to his earlier work on (a) the same topic; (b) related topics; (c) other work he was doing at the time?
(6) How does Birkhoff's paper relate to the work of his contemporaries, particularly those in Russia?
(7) How was Birkhoff's lecture/summary paper received?

Endeavouring to answer these and related questions is the subject of my current research.

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Mathematicians Engage with Relativity: Examples from Unified Theory Work in the 1920s

Catherine Goldstein & Jim Ritter

This talk reports on a small part of a larger joint project on unification theories in the twenties. There already exist historical works on these theories (in particular [4, 5, 1]), and our specific objectives in this project are twofold: first of all, to study more deeply the interactions between mathematics and physics, and then, to use this rich material to test some methodological issues, in particular concerning the dynamics of research, through a systematic network analysis of papers. Our talk focussed only on the first issue (see [2] for the second). To control the multiple parameters affecting the relations between physics and mathematics, and their very definitions, we have selected from our hundred or so authors three of them who shared important characteristics: they were all professional mathematicians, their proposals were taken up by followers, they were all attempting to integrate electromagnetism and gravitation in a unified point of view, in the aftermath of Einstein's and Weyl's theories and all three used a geometric perspective. Our perspective here is mainly comparative, to display how each of these mathematicians explicitly established a frontier between mathematics and physics inside his own work, a frontier which did not coincide with the disciplinary boundaries, as indicated in the reviewing journals for instance, nor with the limits of their effective interactions with physicists.

I – The Geometry of nature according to Whitehead

Whitehead's contributions to physics took place in the period between his famous work on logic with Bertrand Russell, the *Principia Mathematica*, and the philosophical texts of his American period; they consist of several articles, in particular on space-time, and in three books, *An Enquiry Concerning the Principles of Natural Knowledge* (1919), *The Concept of Nature* (1920) and the *Principles of Relativity with Applications to Physical Science* (1922), written when Whitehead was Professor of Applied Mathematics at Imperial College in London.

Whitehead's physics is what he called a pan-physics, a natural philosophy (an excellent introduction to his work in physics is given in [3]). Fundamental physical concepts are not to be identified with variables in mathematical equations (as is the case in Einstein's general relativity), nor are they to be defined by measurement procedures; they should be natural elements, rooted in human experience. Ultimate facts of nature, for Whitehead, are events, connected by space-time relations. In the preface of the *Principle of Relativity*, Whitehead summarizes his point of view in evocative terms:

> "Our experience requires and exhibits a basis of uniformity, and [...] in the case of nature this basis exhibits itself as the uniformity of spatio-temporal relations. This conclusion entirely cuts away the casual heterogeneity of these relations which is the essential of Einstein's later theory.... It is inherent to my theory to maintain the old division between physics and geometry. Physics is the science of the contingent relations of nature and geometry expresses its uniform relatedness."

The uniformity is expressed by adopting Minkowski space as space-time geometry. A crucial tool in order to reconstruct the standard things required in a physico-mathematical theory, like points or particles, is the method of *extensive abstraction*, linked to Whitehead's previous work on the axiomatics of geometry which was supposed to have constituted the fourth volume of the *Principia* and was never published. The method derives geometrical objects (like points) from classes of events as ideal limits, and similarly for particle-events (restrictions of events to 0 dimensions), routes or paths (restrictions to 1 dimension), etc.

The law of motion is given for the so-called impetus I by

$$dI = M\sqrt{dJ^2} + c^{-1}EdF,$$

M being here the mass of the particle M and E its charge; dJ and dF (corresponding to the laws of gravitation and electromagnetism) are supposed to be empirically determined. Whitehead then applies a variation principle to the integral of dI along a route between two particle-events A and B and obtains a system of differential equations of Euler-Lagrange type. For the specific law of gravitation he has posited, based on retarded potentials, a solution can be exhibited and the classical tests of general relativity recovered (see [3] for more details).

Thus two geometries are at work in Whitehead's theory: one is the fixed one attached to the Minkowskian space-time, the other is that of the dynamic space, which hosts the (contingent) laws of nature. Whitehead indeed proposes his own law of gravitation as just one simple possibility among others, and is ready to abandon it for, say, Einstein's law if experiments were ever to require it, but under the condition that it be interpreted as a purely empirical law: neither the mathematical derivation of the law nor the interpretation of Einstein's Riemannian space-time as the real-world space are considered as assets by Whitehead, quite the contrary. In a discussion following an alternative proposal by George Temple [A generalization of Professor Whitehead's Theory of Relativity, *Proc. Phys. Soc. London* 36, 1925, 176-192], based on similar principles to those of Whitehead, this last makes his point clearly: "A further advantage of distinguishing between space-time relations as universally valid and physical relations as contingent is that a wider choice of possible laws of nature (e.g., of gravity) thereby becomes available, and while the one actual law of gravity must ultimately be selected from these by experiment, it is advantageous to choose that outlook of Nature which gives the greater freedom to experimental inquiry.

[...] In investigating the laws of nature what really concerns us is our own experiences and the uniformities which they exhibit, and the extreme generalizations of the Einstein method are only of value in so far as they suggest lines along which these experiences may be investigated. There is a danger in taking such generalizations as our essential realities, and in particular the metaphorical 'warp' in space-time is liable to cramp the imagination of the physicist, by turning physics into geometry."

II – Oswald Veblen and the Princeton school

The successes of general relativity in the early 1920s—and a visit by Einstein himself—inspired two Princeton University geometers to join forces in 1922. Starting that year, the differential geometer Luther Pfahler Eisenhart and Oswald Veblen, topologist and specialist of axiomatic projective geometry, began writing a series of articles, jointly with each other, with other colleagues and their students, as well as individually, which represented a general program for the reconstruction of geometry and physics. The outline of this program was summed up by Veblen in his farewell address as vice-president of the American association for the Advancement of Science in December of 1922.

For Veblen, geometry is simply a branch of physics, indeed its core. The traditional geometries, projective, affine and metric, as organized in Felix Klein's group-theoretical Erlangen program, are created through an increasingly restrictive articulation of blocs of axioms. Adding further, more physical axioms concerning time, mass, etc. produces the kinematics then dynamics of classical physics. But modern physical theories, such as relativity and quantum theory, need a new, more general, geometrical core and a new organizing principle.

In ordinary Riemannian geometry the geodesics are given by the differential equation

$$\frac{d^2x^k}{ds^2} + \Gamma^k_{ij}\frac{dx^i}{ds}\frac{dx^j}{ds} = 0, \qquad (*)$$

where $i, j, k = 0, \ldots, 4$ and the Γ^k_{ij} are the Christoffel symbols of the second kind, formed from the metric and its first derivatives. Now, proposed Veblen, start from the equation $(*)$ and view the Γ^k_{ij} simply as a collection of $4^3 = 64$ coefficients. Then the curves (*paths*) defined by solutions of $(*)$ will serve to characterize a geometry. Of course, not every choice of coefficients describes distinct paths and thus a distinct geometry. Geometries are defined by the equivalence classes of paths; all Γs given by the equation

$$\overline{\Gamma}^k_{ij} = \Gamma^k_{ij} + \psi_i\delta^k_j + \psi_j\delta^k_i$$

with ψ_i a covariant vector, yield equivalent geometries.

The new hierarchy of geometries can now be constructed by imposing more and more axiomatic restrictions on the classes of paths: where there is a connection between the Γ^k_{ij} and the metric of a space one has the familiar Riemannian case; those paths which depend on a particular choice of Γ^k_{ij} independently of a metric will define an *affine* geometry; those which are independent of such a choice form the most general geometry, a *projective* geometry of paths. Most of the attention of the Princeton School in the 1920s was directed to the last case. Under the influence of Veblen's student then colleague, Tracy Y. Thomas, this choice was confirmed and increasingly supplemented by techniques drawn from differential invariant theory.

After some years passed in building up the geometric core, the Princeton group felt itself ready to intervene in physics for the first time on the occasion of Einstein's publication of his 1925 unified field theory (with an asymmetric connection).

The papers published by the Princeton School on this occasion show that whereas some of its members, like Eisenhart, saw their role as the traditional one of clarifying and rigorizing physicists' theories, others, like Thomas, were ready to generalize Einstein's new theory, putting forward a structure which englobed other alternate affine theories like those of Hermann Weyl and Jan Schouten.

Five years later, when Einstein published a series of articles on his Fernparallelismus theory, Thomas and Veblen were ready to go much further. Indeed Thomas, in a long series of articles on the initial value problem in unified field theories, did not hesitate to (twice) modify Einstein's own field equations in the name of "simplicity" from the geometry of paths point of view. Veblen went even further and produced his own rival unified theory, "projective relativity".

III – Élie Cartan

Élie Cartan in Paris became interested in relativity for many of the same reasons as his contemporaries at Princeton: the rising fame of the theory, a visit by Einstein to the French capital and a clear way to use some of the techniques he had pioneered in preceeding years. In fact there were two aspects to Cartan's previous work which he felt could be applied directly: first, the classification and structure of Lie groups and algebras and second, the theory of differential (Pfaffian) forms. He would use these two tools to establish mathematical results which could then be offered to physicists as a basis for their own developments in physical theories.

With the first approach, Cartan would found and extend geometry through an enlarged and modernized version of the old Erlangen program, based now on Lie groups rather than the finite symmetry groups treated by Klein, inapplicable to Riemannian geometry and its generalizations. In this way the physicists would have new spaces, endowed with new geometrical properties in which to found their unified theories. With the second, Cartan promised a means to find and characterize all those differential equations on a space which satisfied given physical or mathematical conditions and might therefore serve as potential field equations to describe the dynamics on these new spaces.

Indeed it was the second focus which provided Cartan's first success in the field. He was able to show in 1922 that the Einstein field equations—both the original 1915 ones and those with the cosmological constant—were the *only* set of field equations which satisfied the various conditions that Einstein himself had set out as being desiderata for a physically acceptable set of field equations. Moreover "my methods allow me to present, in a virtually intuitive form, the gravitational equations of general relativity. [...] The formulas which lead to Einstein's gravitational tensor fit into a few lines; moreover, it is possible to dress them in a geometrical form which allows a precise and rigorous formulation, without any calculation, of Einstein's laws of gravitation." [E. Cartan, *Notice sur travaux scientifiques*, Paris, 1922]

That same year, Cartan used his first, group-theoretic approach to introduce torsion as a new geometric concept. Though he was unsuccessful in a private conversation that year in selling this idea to Einstein as a basis for a possible unified theory, six years later Einstein would make use of just such an extension for his Fernparallelismus theory, one in which the space contained no curvature but a non-zero torsion. In the context of this new theory, Cartan attempted to reproduce his success of 1922 by determining the acceptable class of possible field equations for his new space:

> "For what partial differential equations E must one restrict the general scheme of Riemannian space wih absolute parallelism in order to obtain a faithful image of the physical universe?
>
> [Now] to the logical conditions imposed by the very nature of the question and to the conditions of analytic simplicity, it suffices to add a single condition, drawn from physical determinism for the problem to admit only a very restricted number of solutions, such that the physicist, if M. Einstein's attempt is not a vain one, will have merely to choose among a small number of universes constructed in a purely deductive fashion." [E. Cartan, Le parallélisme absolu et la théorie unitaire du champ, *Revue de métaphysique et de morale*, 1931, 13-28]

A year of calculation allowed Cartan to present a series of possible field equations with 15, 16 or 22 equations; unfortunately by this time Einstein had abandoned the whole Fernparallelismus scheme and was engaged in another, quite different unified field theory effort.

We have followed three cases of mathematicians engaging with the new relativity theory in the early 1920s. Each did so with his own, specific agenda and, though not gone into here, this agenda often had institutional as well as intellectual dimensions. In particular, mathematicians had their own "unity" problem. Mathematicians can instrumentalize physics as well as the opposite. The frontier defined between mathematics and physics, as well as their own role varied. Consider our own triplet of examples:

- Whitehead — geometry and physics are to be seen as rigorously distinct though Whitehead will work in both domains simultaneously.
- Veblen and the Princeton school — geometry is a part of physics; Eisenhart works principally on the geometric core, while Veblen and Thomas work

on both the geometric and physical levels, offering modifications or even entirely new creations.

- Cartan — geometry and physics are totally distinct and Cartan will work only in mathematics, though on objects among which the physicist has only to make his choice.

Thus, the frontier between mathematics and physics and the distinction between mathematicians and physicists do not coincide.

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Einstein's Unified Field Theory within Metric-Affine Geometry

Hubert F. M. Goenner

An example for the interaction of mathematicians and physicists is presented which centers around Albert Einstein's "Unified Field Theory" (UFT) of gravitation and electromagnetism. While the *theoretical* motivation for General Relativity had been the necessity to obtain a *relativistic* theory of gravitation replacing Newton's, UFT lacked a clearer motif than the vague hope for another unification of physical fields. Moreover, there was no need for such an endeavor from the *empirical* side. If so desired, my example may be looked at from the aspect of *mathematical modeling*. On the side of physics we do have the fundamental interactions of gravitation and electromagnetism, described by the corresponding physical *fields*. On the side of mathematics, differential geometry enters with its geometric objects, plus hyperbolic PDE's for the formulation of the field equations. The modeling is called "geometrization" of physics; it amounts to the construction of unambiguous relations between *physical observables* and *geometrical objects*.

First, a brief introduction into the concept of (linear) *affine connection* (with components $L_{ij}^{\ \ k}$ in local coordinates x^j) governing parallel transport of tangent vectors on the four-dimensional space-time manifold, and into *torsion* $S_{ij}^{\ \ k} := L_{[ij]}^{\ \ \ k}$ was given in the talk[1]. If, in addition, an *asymmetric* metric tensor $g_{ik} = h_{(ik)} + k_{[ik]}$ is allowed, then several connections may be defined. Among them we find the usual Levi-Civita connection $\{..\}_h$ of Riemannian geometry formed with the

[1]Symmetrization- (..) and antisymmetrization brackets [..] are used.

symmetric part h of the metric, Hattori's connection (Hattori 1928) constructed from both the symmetric and antisymmetric parts of the metric[2]:

$$\{..\}_{ij\ Hattori}^{k} := \frac{1}{2} h^{kr}(g_{ri,\ j} + g_{jr,\ i} - g_{ji,\ r}) \ ,$$

and Einstein's "plus"- and "minus"- connections $L_{ij}^{\ k}$ and $L_{ji}^{\ k}$, differing by torsion terms.

Einstein's first three papers on UFT were discussed (Einstein 1925, 1945; Einstein & Straus 1946), only. They all employ the metric and the connection as *independent* variables – with altogether 80 components in local coordinates while only 6 + 10 of them would be needed for a description of the gravitational and electromagnetic fields. In Einstein's approach, the symmetric part of the metric h is taken to correspond to inertial and gravitational fields while the antisymmetric part k houses the electromagnetic field. The field equations are derived from such a Lagrangian that General Relativity is contained in UFT as a *limiting* case. There are many possibilities for such a Lagrangian, however. Nevertheless, Einstein uses, without further justification, a Lagrangian corresponding (more or less) to the curvature scalar in Riemannian geometry $\sqrt{-det(g_{ik})}\ g^{lm}K_{lm}(L)$ [3]. These field equations whose alternative forms were named the *strong* and *weak* equations, are also used to express the connection as a complicated functional of the metric and its derivatives. This was achieved not before the 1950s, however.

There are two small differences between Einstein's first paper using metric-affine geometry (Einstein 1925) and his second (Einstein 1945): He now introduced *complex-valued* fields on real space-time in order to apply what he termed "hermitian symmetry"[4] ($\bar{g}_{ki} = g_{ik}; \bar{L}_{ji}^{\ k} = L_{ij}^{\ k}$). After Pauli had observed that such a symmetry could be obtained also with *real* fields, in his next paper (Einstein & Straus 1946) Einstein switched back to *real* fields. Furthermore, he gave two formal criteria as to when a theory could be called a "unified" field theory. The first was that "the field appear as a unified entity", and the second that "neither the field equations nor the Hamitonian function can be expressed as the sum of several invariant parts, but are formally united entities". He readily admitted that for the theory of his present paper (Einstein & Straus 1946), the first criterion was not fulfilled.

Looking at the mutual "directions of influence" between mathematics and physics, we may distinguish three fruitful exchanges. It is known that the mathematician Grossmann provided Einstein with the Ricci-calculus as the means of formulating General Relativity within Riemannian geometry. This very theory then radiated back into mathematics and helped to introduce the most general concept of an affine connection (Hessenberg, T. Levi Civita, E. Cartan, H. Weyl). Next, the transfer of this new geometrical concept into physics led to the unified field theories of Eddington, Einstein, and Schrödinger (Eddington 1921, Schrödinger 1943,

[2] $A_{,\ j} := \frac{\partial A}{\partial x^j}$

[3] For a general affine connection L, its curvature tensor $K^l{}_{ijk}(L)$ allows for two contractions, corresponding to an (asymmetric) Ricci tensor $K_{ik} = K^l{}_{ikl}$ and to what is called "homothetic curvature" $V_{ik} = K^l{}_{lik}$.

[4] The bar signifies complex conjugation.

1944). It seems to me that after this third interaction mathematics no longer did profit from physical theory: the conceptual development of metric-affine geometry took place independently *within* mathematics (L. P. Eisenhart, O. Veblen, J. M. Thomas, T. Y. Thomas). Of course, mathematicians helped theoretical physicists to both solve their equations *and* invented new equations for UFT. Unfortunately, to the *exact* solutions of such equations found, in most cases *no* physical meaning could be given. In physics, the next step in unification would be taken only in the 1960s through the joinder of the weak and electromagnetic interactions in electroweak theory with *gravitation* being left out. In mathematics, important developments leading to differential topology as well as to the theory of fibre bundles originated with E. Cartan – not with anyone connected to the UFTs of Einstein and others.

The various forms of UFT given in Einstein's three papers all suffer from the same problem: metric-affine geometry provides us with too many mathematical objects as to allow for a convincing selection of an unambiguous geometrical framework for a physical theory describing gravitation and electromagnetism. In addition, within metric-affine geometry, the dynamics (i.e. field equations) is highly arbitrary. Moreover, even if UFT had succeeded as a well-put theory, the newly discovered particles (neutron, mesons, neutrino), by the 1940's, would have required another approach taking into account the *quantum* nature of these particles (field quantization). The UFTs of the 1920s to the 1940s did not get to the stage where *empirical* tests could have been made; also, no novel gravito-electromagnetic effects were derived. In a way, UFT was as removed from an empirical basis then as string theory is now. Perhaps, Einstein's successful unification of gravitational and inertial forces within General Relativity and its ensuing fame misled him, and a number of mathematicians and theoretical physicists, to invest their efforts into work on *formal schemes* for a unification of gravitation and electromagnetism.

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