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María de Paz
Robert DiSalle *Editors*

Poincaré, Philosopher of Science

Problems and Perspectives

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Poincaré, Philosopher of Science

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Editors

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Editors
María de Paz
Centre for Philosophy of Science
at the University of Lisbon (CFCUL)
University of Lisbon
Lisbon, Portugal

Robert DiSalle
Department of Philosophy
University of Western Ontario
London, Canada

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Preface

This volume grew out of the Project "Poincaré, Philosopher of Science"[1] of the *Centro de Filosofia das Ciências,* the Center for the Philosophy of Science, at the University of Lisbon. Over several years, in various colloquia and conferences, Poincaré scholars and philosophers of science from around Europe and the Americas joined with the Poincaré Project's members in Lisbon, to consider novel perspectives on all the facets of Poincaré's thought on the philosophy of mathematics and natural science and to try to find a coherent perspective on Poincaré's philosophy of science as a whole. This volume reflects the most important facets of Poincaré's contributions to the philosophy of science, by bringing together some characteristic papers from the Poincaré Project. It is by no means a complete record of the work of the Project, nor can it claim to contain all of the most important papers that eventually emerged from it; many of these have been published in other venues and reached other audiences. The purpose of the volume is, rather, to exhibit the impact of the Poincaré Project on contemporary interpretations of Poincaré's thought, through a broad sample of the innovative scholarship that the Project has fostered—and, even more, to exhibit the extraordinary breadth and depth of Poincaré's work in the foundations of mathematics and science and to encourage the growing interest in the philosophical importance of his work.

The editors would like to thank everyone who participated and assisted in the Poincaré Project, in addition to those whose papers are collected here, but in particular Professor Augusto Franco de Oliveira, leader of the Poincaré Project, and Olga Pombo, head of the *Centro de Filosofia das Ciências,* for their inspiration and support. We would also like to thank Lucy Fleet of Springer for her untiring support of this volume.

Lisbon, Portugal　　　　María de Paz

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Contributors

Augusto J. Franco de Oliveira Department of Mathematics, CFCUL, Emeritus Professor (Évora), University of Évora, Lisboa, Portugal

María de Paz Centre for Philosophy of Science at the University of Lisbon (CFCUL), University of Lisbon, Lisbon, Portugal

Robert DiSalle Department of Philosophy, University of Western Ontario, London, Canada

Janet Folina Philosophy Department, Macalester College, Saint Paul, MN, USA

Gerhard Heinzmann Laboratoire d'Histoire des Sciences et de Philosophie—Archives Henri Poincaré (UMR 7117), Université de Lorraine/CNRS, Nancy, France

Reinhard Kahle CENTRIA and DM, FCT, Department of Mathematics, Universidade Nova de Lisboa, Caparica, Portugal

João Príncipe CEHFCi – Universidade de Évora, Sacavém, Portugal

Laurent Rollet Laboratoire d'histoire des sciences et de philosophie – Archives Henri Poincaré (UMR 7117 CNRS/Nancy université)/Maison des sciences de l'homme Lorraine (USR CNRS 3261), University of Lorraine, Nancy, France

Isabel Serra Centre for Philosophy of Science at the University of Lisbon (CFCUL), University of Lisbon, Lisbon, Portugal

Antonio A. P. Videira Department of Philosophy, Universidade do Estado de Rio de Janeiro, CNPq, Rio de Janeiro, Brazil

Introduction

The work of Henri Poincaré (1854–1912) extends over many fields within mathematics and mathematical physics. In mathematics, he was instrumental in the development of the theory of functions, mathematical logic, topology, and, most famously, non-Euclidean geometry; in physics, he played a role in the development of celestial mechanics, thermodynamics, statistical mechanics, and electrodynamics. It is therefore somewhat astonishing, in retrospect, to reflect on the magnitude and importance of Poincaré's contributions in all these fields. A survey of his original scientific work would indeed be a history of the transition from the nineteenth century to modern mathematics and physics. But his scientific work was inseparable from his groundbreaking philosophical views, and the scientific ferment in which he participated was inseparable from the philosophical controversies in which he played a pre-eminent part. The subsequent history of the mathematical sciences and the philosophy of the mathematical sciences were deeply affected by Poincaré's philosophical analyses of the relations between and among mathematics, logic, and physics, and, more generally, the relations between formal structures and the world of experience.

During the twentieth century, some standard interpretations of Poincaré's philosophical views emerged which, a century after his death, are ripe for reassessment. For example, his philosophy of mathematics is mainly negatively characterized by his rejection of logicism and formalism understood as pure manipulation of symbols. His conventionalist view of the foundations of geometry and physics, too, has yet to be fully clarified; it had a decisive influence on the views of Einstein and the logical positivist movement, but in recent debates over the fate of logical positivism and the foundations of science, Poincaré's original insights into the relation between mathematical structures and experience have not been adequately appreciated. Thus in philosophy of science as in philosophy of mathematics, contemporary debates center on questions whose formation was profoundly affected by Poincaré's work and which can still be further illuminated by a better understanding of what Poincaré contributed.

The essays in this volume are divided by the broad topics of foundations of mathematics, foundations of physics, and general philosophy of science. It might seem, on the one hand, that this division is artificial, because the boundaries cannot be very precisely drawn; Poincaré's foundational work in mathematics is never remote from his interest in physical application, and his work in the foundations of physics always involved reflection on mathematical methods; both were thoroughly colored by his broader philosophical interests. And his philosophical reflections always originated in reflections on specific problems in physics and mathematics, their logical, epistemological, and practical foundations. On the other hand, the inseparability of mathematics, physics, and philosophy within Poincaré's thinking is a central part of the motivation for this entire project and therefore of this volume.

Poincaré's philosophy of science is primarily thought of in connection with his celebrated defense of conventionalism, particularly concerning physical geometry. His argument that our knowledge of the structure of space and time depends on conventional choices—on "definitions in disguise" that assign empirical significance to geometrical concepts—undoubtedly accounts for much of his influence on the evolution of the philosophy of science in the twentieth century. It suggested that because of the crucial role of definitions in applying geometry to the world, the choice between different theories is like the choice between different languages to express the same physical facts. There might be compelling grounds for a particular choice, but these can only come from considerations of simplicity and convenience; there can be no meaningful question of truth. The intrinsic philosophical interest of Poincaré's conventionalism arises from the light that it shed on the relation between physical laws and the definitions of the physical concepts that those laws employ. The idea that the fundamental principles of physics play a peculiar role, distinct both from logical principles and from empirical laws, was a fairly old one going back (at least) as far as Kant's theory of the synthetic a priori: such principles appear to have the force of necessity, yet also to describe the contingent world of empirical facts. The best example was Euclidean geometry, seen as resting on universal and necessary principles that, nonetheless, describe the space of our experience. Kant's explanation was that such principles are determined by the form of our spatial intuition. By Poincaré's time, however, this view appeared to be fatally undermined by the development and the empirical application of non-Euclidean geometry. The first step in this process was the empiricist view of Helmholtz. Where more traditional empiricists, like Mach and Mill, treated the fundamental principles of geometry as inductive generalizations, Helmholtz argued that their inductive basis lay in more primitive physical principles: the free mobility of rigid bodies and the rectilinear propagation of light. Geometry is both formal and empirical, according to Helmholtz, because it is the formal development of postulates whose empirical content ultimately derives from these elementary physical facts.

What distinguished Poincaré's approach was that it offered an explanation of the necessary aspect of geometry, without appealing to the notion of synthetic a priori knowledge. Here there is no need for a detailed account of Poincaré's conventionalism, which is discussed at length in the essays by Folina and De Paz. Nor is it necessary to try to represent conventionalism as a general guiding principle throughout Poincaré's philosophical reflections. Conventionalism, taken in its simplest sense as an account of the role of free choice in the empirical interpretation of mathematics, is only one aspect of Poincaré's thinking. A more important aspect is his emphasis on those fundamental principles that provide criteria for the interpretation and application of fundamental concepts. One role of these principles is to provide implicit definitions of the concepts that occur in them. For example, Newton's second law does not begin from a precisely defined concept of force; it specifies precisely what force is as a measurable theoretical magnitude, by identifying acceleration, as its geometrical correlate. Yet is more than a definition, and so is more than an analytic principle in Kant's sense, that is, a mere exposition of what predicates are "contained" in the concept of force. The empirical content of the law consists in the program that it defines, for determining the forces of interaction among bodies from their observed relative accelerations; it makes their relations intelligible *as* interactions. The form of necessity that it imposes lies in its strict requirement that absolutely every component of every accelerated motion can be traced to a physical source, thus completing an action-reaction pair. In principle, one might be tempted to call this an unfalsifiable claim, compatible with any finite body of empirical evidence. In practice, it defines the perturbation theory for Newtonian mechanics. Within the framework defined by Newton's laws, the investigation of any interacting system can start from the simplest idealized model, and every deviation from the ideal behavior is informative, giving rise to a succession of corrections to the initial simplified estimates of the properties of the system.

We thus see that Poincaré's conventionalism is one facet of a broader philosophical orientation that defines, not only an approach to general questions in the philosophy and methodology of science, but also a perspective on foundational problems in mathematics and mathematical physics, from which the role of formal principles appears in a particularly revealing light. Poincaré's technical researches were never completely detached from his appreciation of the general structural principles that organize particular fields of inquiry and define their fundamental questions. In the foundations of geometry, this orientation directs Poincaré not only to the explicitly philosophical questions surrounding the nature of space and time and the empirical status of non-Euclidean geometry, but also to the exploration of Klein's group-theoretic conception, the connections between geometry and formal logic, and the generalization of geometry through the development of *analysis situs* and the first steps toward modern topology. This expanded conceptual framework for geometry, in turn, enabled Poincaré to develop his distinctive group-theoretic approach to electrodynamics, a clear forerunner to Einstein's theory of relativity. The same philosophical orientation appears in Poincaré's studies on the foundations of probability theory and his uses of probabilistic considerations as a framework for thinking about the foundations of thermodynamics, statistical mechanics, and celestial mechanics.

This complicated mixture of detailed conceptual analysis, in the foundations of science, with reflections on the most general problems in the philosophy of science—methodological, epistemological, and even metaphysical—is an essential feature of Poincaré as philosopher and is therefore the central motivation for the present collection of essays. It starts with a set of essays on general aspects of Poincaré's philosophy of science—beginning with his early philosophical education—and proceeds to essays on some aspects of his work in the foundations of mathematics and physics. It is not meant to offer a complete picture of Poincaré's philosophy, but, rather, a framework for further study of the interactions between philosophical and scientific inquiry that gave his scientific work, remarkable as it was from a purely scientific perspective, its distinctive philosophical character and its enduring relevance to the philosophy of the exact sciences.

Part I
Poincaré's Philosophy of Science

Portrait of Henri Poincaré as a Young Philosopher: The Formative Years (1860–1873)

Laurent Rollet

> *"Il nous arrivait quelquefois de philosopher: Poincaré souriait doucement de la psychologie et de la théodicée naïves qu'on enseignait alors en vue du baccalauréat. Je me souviens également de longues conversations sur les raisons scientifiques et philosophiques de croire à l'existence de la vie dans d'autres planètes".*
>
> (Paul Appell 1925)

Abstract The question of the origins of Henri Poincaré's philosophy gave rise to numerous studies during the last decades. This article proposes to follow a track that has not been explored in detail so far: the aim is to follow Poincaré during his training years in high school in Nancy until he entered the *Ecole Polytechnique* in 1873. Different sources, old or recent, offer the possibility of reconstructing his school career. They also give some clues about his first contacts with the field of philosophy, through his readings, his family and social relationships or the curricula then in effect in public education. To carry out this mainly biographical program, we will first give an account of his family background and explore the characters of his social and cultural world in a small university town. In a second step, we will study in detail the functioning of the high school and of the faculties of Nancy in the 1860s. Finally, we will propose different trails concerning the construction of his philosophical horizon during his youth.

L. Rollet (✉)
Laboratoire d'histoire des sciences et de philosophie – Archives Henri Poincaré (UMR 7117 CNRS/Nancy université)/Maison des sciences de l'homme Lorraine (USR CNRS 3261), University of Lorraine, 91 Avenue de la Libération, F-54000, Nancy, France
e-mail: laurent.rollet@univ-lorraine.fr

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Henri Poincaré as Philosopher: An "Epistemic Bastard"?

Henri Poincaré published, during his life, three philosophical works that achieved great success with the general public and with the philosophical community: *Science and Hypothesis* (Poincaré 1902), *The Value of Science* (Poincaré 1905) and *Science and Method* (Poincaré 1908). These three books – four if we include the posthumous *Last Thoughts* (Poincaré 1913) – constitute the heart of the philosophy of Poincaré, whose influence on the epistemology of the twentieth century cannot be doubted.

However, Poincaré was first an engineer and a scientist, and not a "professional" philosopher. In addition, except for his long collaboration with the *Revue de métaphysique et de morale* (a score of 20 articles from 1893 to his death), many of his philosophical works were published in scientific journals or magazines on science popularization.[1]

What is the philosophy of Poincaré? What type of "philosopher" was he? How did he enter into philosophy? What were his relations with the French philosophical community? Is it possible to have a precise idea of the connections between his scientific practice and his so-called philosophical thinking? Who were the authors who influenced his philosophy?

We could respond to these questions by different strategies: for example, we could invoke the anchorage of Poincaré's thought in his own research practices and in the scientific debates of his time; this would mean giving precedence to the networks of conceptual influences with various scientists (Hermann von Helmholtz, James Clerk Maxwell, etc.).[2] We could also analyze his thought from a more systematic angle, in order to clarify the philosophical structure and establishing its consistency. We could, finally, try to characterize it by relying on the degree of Poincaré's proximity with the professional philosophical community at the end of the nineteenth century.

Whatever strategy or strategies are adopted – which are not necessarily mutually incompatible – it seems difficult to hope to characterize Poincaré's philosophy without defining at least minimally the terms "philosophy" and "philosopher". If it is not certain that we can propose a fully satisfactory answer to such general questions, we can, however, try to give them a historical color by situating them in the context of the first decades of the Third Republic (1870–1914).

At the end of the nineteenth century, French philosophy began a movement of professionalization and became more and more the business of professors. This trend was linked, of course, to the gradual empowerment of the academic field; it created university professors, no longer reciters, but researchers, and it followed the general movement of the development of the educational system. This process is particularly visible when we compare the two halves of the nineteenth

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[1]These articles were then collected in the volumes of the *Bibliothèque de philosophie scientifique* cited above. On the composition of these volumes see Poincaré 2002.

[2]For example, see the important work of Adolf Grünbaum 1963 and Jerzy Giedymin 1982 in the years 1970–1980 or, more recently, the contribution of João Príncipe 2012.

century: before 1850, the most important French philosophers were very often non-professional – thus alien to the academic community – or thinkers without real philosophical training. Conversely, the philosophers of the second half of the nineteenth century were usually perfectly integrated into the academic community and could claim a depth of philosophical formation (many were then educated at the *Ecole normale supérieure*). Professionalization contributed to create a coherent philosophical community and guaranteed the authority of philosophical discourses, an authority secured by the reproduction of the professorial corps: the profession was equipped with specialized journals, specific training, competitions, and it was organized around venues for social interaction – international congresses and learned societies, such as the *Société française de philosophie* (Fabiani 1988, 28–29).

But this era of professionalization was also marked by increasing participation of scientists in philosophical debates, a movement which could only undermine the monopoly of the philosophers over philosophical discourse. The scientists expressed themselves in literary and philosophical journals and challenged disciplinary compartmentalization.

This is the case with Henri Poincaré, Pierre Duhem and other scientists as well. How, then, should we characterize their interventions in the philosophical field? These were not professional philosophers, in the sense that they did not occupy chairs in philosophy, but they were no less perceived as thinkers of first rank on the philosophical scene, and their contributions were widely solicited by the editors of philosophical journals.[3] Moreover, illustrious thinkers like Poincaré or Duhem were only the most visible part of a very broad community that included amateurs, representatives of different scientific disciplines, engineers, military, etc. These hundreds of actors, who published in a large number of philosophical, scientific and popular magazines, who contributed to the emergence of an epistemology among the French, are sometimes designated by the expression *scientist-epistemologists*, a designation proposed by Jean-Claude Pont and Marco Panza (1995). One might prefer, however, without any pejorative implication, the expression "*epistemic bastards*" coined by Christophe Prochasson (1991, 175). This very evocative characterization perfectly describes the disciplinary indecision and the ambiguity of the intervention of these scientists on the philosophical and intellectual scene; they evolved within a vaguely delimited territory comprising science, philosophy and popularization of science, which made their professional identity relatively opaque. These designations certainly do not solve the problems of identity, but at least they highlight the singularity of a certain practice of philosophy at the turn of the nineteenth century, of which Poincaré is without question an exemplar.[4] The study

[3]As well, for the first number of the *Revue de métaphysique et de morale*, 1893, its founders Elie Halevy and Xavier Léon would do everything to obtain an article of Poincaré, helped in this by Emile Boutroux and Henri Bergson (Simon-Nahum 1991).

[4]For a critical discussion of the category of "scientist-epistemologist," see the doctoral dissertation in progress by Jules-Henri Greber. A preview is available in his paper, "Caractériser un contexte à

of his philosophical ideas – from their emergence to their dissemination – must take into account this context of professionalization and reconfiguration of philosophical practices.[5]

But another element of context deserves a special study. We know the important role played by philosophy as a school discipline within French education at the time of Poincaré. Regarded as the crowning achievement of secondary studies, philosophy was an intellectual and social marker; it constituted an essential step in the formation of *l'honnête homme*, in a context where only a minority of students had access to the baccalaureate and higher studies.

Poincaré belonged to this minority, and was therefore subject to this ideal of training of the elite by the study of great authors, and by following a program carefully defined by ministerial committees. The purpose of this article is to follow him during his years of training at the high school of Nancy up to his entry to the *Ecole polytechnique* in 1873. Diverse sources, old or recent (Darboux 1913; Xardel 2012; Bellivier 1956; Appell 1925, as well as his correspondence[6]) offer the possibility of reconstructing his path in a relatively detailed way, and to obtain a few clues to his first contact with the field of philosophy, through his readings, his family and social relations, or the education programs then in force. To carry out this mainly biographical program, we will first recount his family origins, and we will explore the character of his social and cultural universe in a small provincial university. Second, we will study in detail the operation of the high school and the faculties of Nancy in the 1860s. Finally we will propose different paths for how the future scientist was able to expand his philosophical horizon in his younger years.

The Social and Cultural Environment of the Young Poincaré

Henri Poincaré was born in Nancy on April 29, 1854. His mother, Eugénie Launois, originated in Arrancy in the Meuse, and came from a rich family of landowners. His father, Emile Léon, originated in Neufchâteau, in the Vosges. He kept a medical practice, which provided him a comfortable enough income but, at the same time, he pursued a career as a teacher and researcher. He started as assistant professor at the preparatory school of medicine in Nancy at the end of 1850, and he was able to obtain a chair of professor of hygiene in 1872 in the new Faculty of Medicine created at Nancy, following the loss of Strasbourg after the war of 1870. His research focused on diabetes, on the nervous system (E.-L. Poincaré 1873–1874), and also

partir d'une base de données : le cas de la philosophie et de l'histoire des sciences entre 1870 et 1930" (Greber 2012, 541–574).

[5]A study that I have been able to carry out in the course of a doctoral dissertation which resulted in the book, *Henri Poincaré (1854–1912): des mathématiques à la philosophie. Etude du parcours intellectuel social et politique d'un mathématicien au tournant du siècle* (Rollet 2000).

[6]While in press, it is also searchable on-line: http://www.univ-nancy2.fr/poincare/chp/

on industrial hygiene (E.-L. Poincaré 1886), a field in which he seems to have been a pioneer.[7] Distinguished member of the University, member of the local academy,[8] and municipal councilor, Emile-Léon Poincaré was a well-known figure in good society in Nancy, and he enjoyed strong social, intellectual and political support, which his son would also enjoy.

In the immediate background of the Poincaré family, we find first Antonin Poincaré (1825–1911), the brother of Emile-Léon Poincaré. A *polytechnicien*, he had a brilliant career as a hydrographic engineer in the Meuse. His marriage with Marie-Nanine Ficatier-Gillon produced the future President of the Republic, Raymond Poincaré (1860–1934) and the physicist Lucien Poincaré (1862–1920), called to become a vice-rector of the Academy of Paris. In the branch of Launois, we find several uncles trained at the Military School of Saint-Cyr,[9] local elected officials – such as Charles Comon (1825–1897) who would be mayor of the city of Longuyon – or the geologist Auguste Daubrée (1814–1896), a remote cousin who was the director of Paris mining school [Ecole des mines de Paris] at the time when Poincaré studied there.

The political connections of the family were numerous and influential. Through his situation in the municipal council, Poincaré's father was close to Auguste Bernard (1824–1883), mayor of Nancy and senator. The family also counted among its relations several politicians – legislators, senators and even ministers: Jean Eugène Billy (1830–1878), Jules Develle (1845–1919) or even Henri Varroy (1826–1883). These friendships were the mark of an accession to a moderate and conservative republicanism that Henri Poincaré would retain in his adult life.[10] We may add that in an era when the wealthier families developed strategic alliances through marriages, the young mathematician would unite his destiny to Louise Poulain d'Andecy, great-granddaughter of the naturalist Etienne Geoffroy Saint-Hilaire, daughter of an administrator of a well-known French bank (the *Crédit foncier de France*), whose family was linked to that of Jules Ferry.[11]

The Poincaré family's social circle in the years 1850–1870 seems to have consisted predominantly of university colleagues. The Poincaré family was thus very friendly with the Xardel family: just like Emile-Léon Poincaré, Jean Pierre Romain Xardel was a liberal doctor and taught at the preparatory school of medicine; one of his sons, Paul (1854–1933), would be a very close friend of Henri Poincaré at the *Lycée de Nancy* and would pursue a career in the army after studies at Saint-Cyr (ending his career with the rank of two-star general).

[7]Some see him as a precursor of occupational medicine. On the life and work of Emile-Léon Poincaré, cf. Drouelle 1986; Salf 2000 and Joly 2000.

[8]The *Académie de Stanislas*: it brought together a large part of the local elites, including the intellectual and academic elites.

[9]Adrien Launois (1842–1917) and Gaspard-Auguste Launois (1806–1886).

[10]Note that Gaspard-Auguste Launois would be a member of Constituent Assembly of 1848, and that his uncle Antonin would refuse to take an oath to the Emperor in 1852.

[11]Concerning Poincaré's wife, see Rollet 2012b.

The entourage of friends also included two professors of the faculty of sciences of Nancy: Camille Forthomme (1821–1884) and Nicolas Renard (1823–1880). Educated at the *Ecole normale supérieure*, Forthomme was professor of physics at the high school of Nancy during many years (where he had Poincaré as a student) prior to obtaining the chair of the faculty of chemistry in 1869. Well-known in the intellectual circles of Nancy, he was a member of the municipal council and would sponsor the candidature of Léon Poincaré to the *Académie de Stanislas* in 1862. As for Renard, also a *normalien*, he held the chair in pure and applied mathematics and then that of applied mathematics; he would distinguish himself by the work of mathematical physics devoted to the study of electrical and magnetic phenomena under the single-fluid hypothesis. A deep friendship bound the three families, who received each other regularly, and whose children shared the games and recreations (Aline Boutroux was a close friend of the daughter of Renard, Marie (Boutroux 2012)). Also among the relatives of Poincaré were the family of the historian Alfred Rambaud (1842–1905), a Russia specialist, founder of the anticlerical newspaper *Le progrès de l'Est,* and future Minister of Public Education, as well as the family of Jules Rinck, a rich Nancy linen merchant whose son, Elie, would be a very close friend of Henri Poincaré (they studied together at the *Ecole polytechnique*) (Rollet 2012a).

Born into a family of the intellectual bourgeoisie of province, Poincaré grew up in an environment where compliance with social conventions was very important. It was considered good form to attend religious services (as a good Catholic, he would make his communion) and to arrange charity tours. The practice of public lectures was still well established at the university, and attending the meetings of school or the lectures of professors newly installed in their chairs – including those of the Faculty of Letters – was a highly esteemed leisure activity. At the Poincaré home, salons were held, and family friends came to organize theatrical performances (staging, especially, lightweight plays tailored to young ears, such as the vaudevilles of Eugène Labiche); the most common leisure activities were literary games (charades, end rhymes, rebus, poetry), whist, dance, piano, and song. Poincaré's father was, in addition, a passionate traveler, and summer holidays were devoted to wanderings in the Vosges, in Germany, in England or in Paris (which the family visited for a week during the universal exhibition of 1867).

From a young age, Poincaré seems to have done a wide variety of reading. Highly gifted, he possessed an abundant library consisting of books rewarding his educational successes. His childhood friend Paul Xardel recounts that the future scientist helped him to discover the novels of Marcel Aymard, Jules Verne, Emile Erckmann and Alexandre Chatrian, Victor Hugo or the works of Alphonse de Lamartine. He also tells us that he was interested in poetry and history, and that he read some philosophy, unfortunately without indicating his preferred authors (Xardel 2012). Poincaré was also very interested in geography (he was a regular reader of the magazine *Le tour du monde*) and he read popularizations of science such as Louis Figuier's *La terre avant le déluge* (Figuier 1862). With his sister, he corrected proofs of the works of his father. Moreover, at the age of 13, he began to read books in special mathematics (Fig. 1).

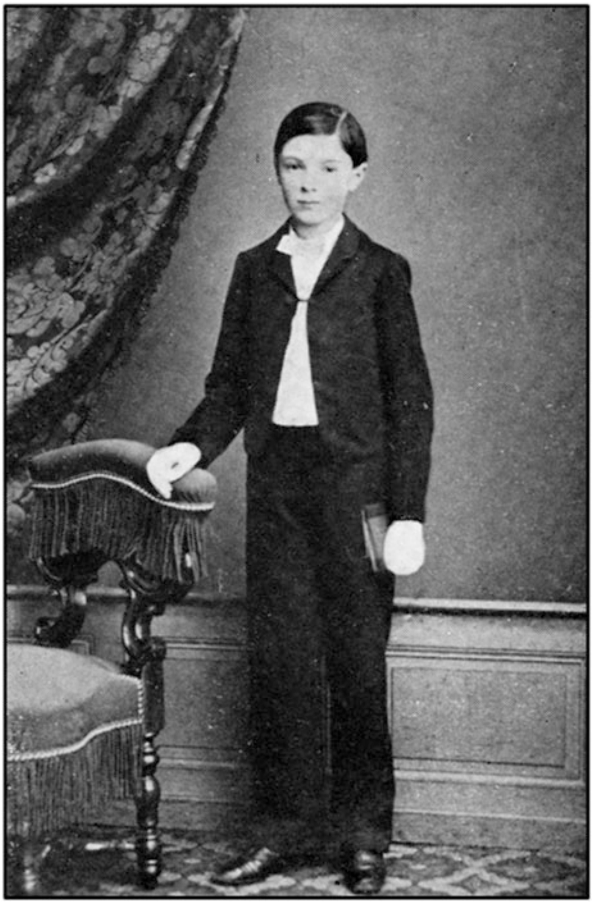

Fig. 1 Henri Poincaré at the time of his communion, 1865 (Source: *Livre du centenaire de la naissance d'Henri Poincaré*, Paris, Gauthier-Villars, 1955)

All of these facts precisely indicate the social and cultural environment in which Poincaré was immersed during his years of training. They are not in themselves surprising in light of the sociology of provincial elite in the second half of the nineteenth century. They do enable us to see however, what might have been his spontaneous philosophy, inherited from his family and its social habitus: the young Poincaré seems to have displayed a great openness toward the sciences and demonstrated a certain faith in scientific and technical progress; he shows evidence of an average religiosity, tending progressively toward agnosticism or skepticism.[12] He was undoubtedly a patriot.[13] He adhered to liberal values and to a moderate

[12]We know from statements made by Poincaré to Doctor Edouard Toulouse in 1897 (Toulouse 1910, 143) that he must have been losing faith toward 18 years of age.

[13]His sister told a detailed story of their children's games, in which the cult of Jeanne d'Arc (Joan of Arc) – very present at that time in Lorraine – played a central role. In the year of his communion

republicanism. The tragic circumstances of the war of 1870 – when he was 16 years old – constituted a deep shock for him: one of his military uncles was taken prisoner at the battle of Sedan, the home of his maternal grandparents in Arrancy was completely looted by the enemy, and his family was forced to billet for months a senior German officer (Boutroux 2012). At a time when patriotic values were very strong – Nancy was a military city – Poincaré could not help adhering to the ideal of a reconstruction of the country by science.[14] This may be the reason why he supported Adolphe Thiers politics in 1873, which on the other side, resulted in a bloody suppression of the Paris Commune.[15]

The Training of Poincaré at the Lycée de Nancy

The Organization of Study in Secondary Education in the 1860s

In the middle of the 1860s, enrollment in secondary education amounted to a little more than 140,000 pupils (Prost 1968, 45), a misleading figure because many of the students did not pursue their schooling up to the baccalaureate and ended their schooling after the small classes (or what was often called the "small high school" [petit lycée]). In 1865, 4,097 baccalaureates of letters and 1,763 baccalaureates of science were given (Meuriot 1919). The baccalaureate degree was therefore the symbol of belonging to a bourgeois and intellectual elite. The Parisian *grandes écoles* (*Ecole polytechnique*, *Saint-Cyr, Ecole normale supérieure*, etc.) were very prestigious among the local notables, who would strive to send their children there when they had the requisite capacities and talents. For years there was a market for the preparation for competitions to enter these *grandes écoles*, in which both the public high schools and the private Parisian institutions took part (Belhoste 2001).

(1865), Poincaré also wrote a play in 5 acts on Jeanne d'Arc (which would be subsequently turned into an opera) (Boutroux 2012).

[14]Recall here the motto of the *Ecole polytechnique*, which takes all its meaning after the defeat of 1870: "Pour la Patrie, les Sciences et la Gloire". Poincaré in 1881 would join the French Association for the Advancement of Science, whose motto was: "Par les sciences, pour la Patrie".

[15]He thus signed, along with his comrades in the *classe de mathématiques spéciales* of the high school, a petition in favor of the president Adolphe Thiers when he was forced to resign by the monarchists in 1873: "The students of special mathematics of Nancy to M. Thiers. Letter of the inhabitants of Nancy to Mr. Thiers. You have appealed to the judgment of history; you could with the same pride and the same confidence have appealed to the judgment of your fellow citizens. You fall under the blows of the coalition parties; you fall against the will of the country. For us, inhabitants of a city still occupied, it is not without a sense of deep pain and anxiety that we learned of the retirement of the great citizen who, since our disasters, has worked tirelessly for the rehabilitation of France and the liberation of the territory. France does not forget any of these great services rendered by you to the homeland and the Republic that you have so rightly proclaimed the necessary form of our government."

However, in 1865, the imperial *lycée de Nancy* was one of the first provincial high schools to establish a section of mathematics dedicated to the preparation of these very competitive examinations.

To study at the high school represented a major cost to families: the tuition fees for an external student ranged from 120 francs per annum for the elementary classes (which would be called college today) to 200 francs per year for the higher division *(classes de rhétorique* and *classe de philosophie).* The expenses for the *classe de mathématiques spéciales* would amount to 250 francs, to which would be added the special charges for conferences, repetitions, and exams (60 to 145 francs, depending on the level of schooling). For boarding students, the annual costs were much more significant, between 800 and 1,000 francs per annum.[16] Needless to say, the French educational system was at this time very rigid: it was closely controlled by the imperial administration, which intended it to perpetuate the cultural model of the dominant conventional humanities.

In Poincaré's time the schooling of pupils was organized into three divisions. The elementary division continued to the end of the seventh-grade class; it was centered on history, religion, Latin and French grammar, geography, mathematics and the learning of a foreign language. After having passed an examination, students could then be oriented toward the division of grammar, which went to the sixth grade at the end of the fourth; this was centered on the teaching of Latin, French, the Greek grammar, history and geography, mathematics and of a living language. Then came the higher division, which was then experiencing major upheavals.

Since the reform of secondary education put in place by the Minister of Public Education Hippolyte Fortoul in 1852, this last step of the secondary studies was organized following the system of bifurcation. The idea of this reform was to put an end to the sterile debates on the pre-eminence of letters or of science, by establishing two Baccalaureate degrees, different but of equal value. All the secondary school students had a common instruction from the sixth grade to the fourth grade. At the end of the fourth, after undergoing a serious examination, they were divided into three categories. The "unfit", or the students whose families did not intend them for long studies, would leave secondary education. The remaining pupils were divided, according to their abilities, within the two sections of the higher division, one to a predominantly literary and the other to the predominantly scientific division.

The scientific section was preparatory to the baccalaureate of science and was particularly directed to students seeking to enter the special schools – such as, for example, Nancy School of Forestry [*Ecole forestière de Nancy*][17] – who were not headed to an industrial or commercial career, or who were considering joining a science faculty or a faculty of medicine. The literary section was preparatory to the

[16]These figures go back to 1876: *Brochure de présentation du Lycée de Nancy*, Archives Départementales de Meurthe-et-Moselle, 1 T 596. For comparison, 200 francs represented nearly 3 months of salary for a worker at this time.

[17]Poincaré would successfully pass these competitions, with the aim of preparing for the more difficult examinations of the Parisian *grandes écoles*.

baccalaureate of letters and was directed to students wishing to do literary studies, or to move on to legal careers. This system of bifurcation had been designed to modernize the teaching, particularly in the opening on the living languages and on the sciences. For the latter, it favored a very utilitarian pedagogy, in order to offer students a training that could help them in the labor market.

The Fortoul reform of 1852 had resulted in the replacement of the *classe de philosophie* by a *classe de logique* and by the abolition of the *agrégation* of philosophy.[18] It was a time in which political authorities sought to influence the teachings in this area, to call in question the weight of the *École normale supérieure* (which the imperial regime was defying) and to reassure the families who might worry about the seditious and potentially immoral nature of philosophy teaching (according to Fortoul, the vocation of the *Ecole normale* supérieure was to train professors, and not rheteurs).

This reform was very much criticized, and imperfectly implemented in the high schools. The parents, who were often very attached to the prestige of the classical humanities at a time when the conflict between the "old" and the "modern" was always alive, considered it incomprehensible. Consequently, the bifurcation would be eliminated in 1864 by the minister Victor Duruy.

Duruy's reform restored the unity of teaching and therefore recreated the *classe de philosophie*, considered again as the crowning achievement of secondary studies. In addition, it restored the *agrégation* of philosophy of 1863.[19] Therefore it restored, by 1865, the unity of a secondary education based on the humanistic education. The baccalaureate of science would come after the complete cycle of literary studies and could be prepared by a mathematics course.

The Tests for the Baccalaureate

It was, therefore, in an educational system marked by profound upheavals that Poincaré undertook his studies at the upper secondary school of Nancy, where he entered as an external on October 1862. He was then 8 years old. Previously he had benefited from the private courses of Alphonse Hinzelin, a friend of the family (who had signed Poincaré's birth certificate as a witness in 1854). A journalist and local scholar, Hinzelin was a regular contributor to the journal *L'impartial*, in which he published many patriotic texts. He would prepare several books devoted to the geography and history of the Meurthe (Hinzelin 1857) as well as vaudevilles. It is he

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[18]For a detailed study of the system of the bifurcation, see the works of Maurice Gontard (Gontard 1972), and Nicole Hulin (Hulin 1982 and Hulin 1986).

[19]In French educational system, the *agrégation* is an elitist national competition for the recruitment of teachers in every discipline (mathematics, physics, philosophy, etc.). During nineteenth century, most of the *professeurs agrégés* came from the *Ecole normale supérieure*.

			Coefficient	Marks
Written examination	Latin Composition		1	4
	Latin Version		1	2
	French Composition		1	3
Oral examination	Analysis of an author	Greek	1	3
		Latin	1	2
		French	1	2
	Philosophy		1	2
	History and geography		1	2
	Elements of science		1	3
			1	3
	Optional test in German		1	3
				29

Fig. 2 Poincaré's marks for the baccalaureate of letters (5 August 1871) (Appell 1925) (In the wake of the Duruy reform, the scheme of notation had been reviewed in depth. Whereas before students were evaluated using balls of different colors, from 1865 onwards a rating scale to 5 notes was adopted: 0, 1 (fair), 2 (fairly well), 3 (Good), 4 (very well))

who probably initiated Poincaré into mathematics, an area in which he had prepared a short manual of calculus in 1860 (Hinzelin 1860).

Poincaré's schooling in small classes was brilliant. He seems to have been just as gifted for humanities as for the sciences. The fourth class [*quatrième*] (1867), however, revealed his mathematical precocity. Georgel, his teacher, said to his mother: "Madam, your son will be a mathematician" (Appell 1925, 16). Poincaré had the choice, after the ninth grade, between a curriculum for literature and a curriculum for science.[20] He oriented himself towards a classical and literary course which was called *classe de rhétorique*. His professor of literature in the latter, Alexandre de la Roche du Teilloy, prepared him for the baccalaureate of letters, and very quickly noticed his originality.[21] In August 1871, Poincaré therefore passed his baccalaureate of letters with honors [*mention bien*] (Fig. 2).

[20]In his book on Henri Poincaré, the mathematician Paul Appell indicates that Poincaré was subject to the system of bifurcation (Appell 1925, 18). This is very likely a confusion or a historical reconstruction (his book was published in 1925), since Poincaré passed his baccalaureate nearly 6 years after the decree abolishing the system.

[21]"One day, I had proposed to him as a subject for composition, preparatory to the baccalaureate of letters, the differences between man and animal; after having read me his work, jotted on small pieces of paper of all sizes, he asked me what mark it was likely to get in the review; I replied that I could not say, very good or mediocre, and that it was too personal, too original, too daring, too strong even for a candidate for the baccalaureate degree. Wishing to retain such a curious study, I made him promise to copy it; his modesty would not allow him to keep his word". Cited in Darboux 1913.

		Coefficient	Notes
Written examination	Scientific Composition	1	0
		1	2
Oral examination	Mathematics	1	3
		1	4
	Physics	1	2
		1	4
			15

Fig. 3 Poincaré's marks for the baccalaureate of science (7 November 1871) (Appell 1925)

Poincaré wanted to proceed directly to his baccalaureate of science in the wake of the baccalaureate of letters, but his jury insisted that he benefit from a special preparation. He therefore followed the course of elementary mathematics up to the autumn session, which took place in November. His mark was only "Assez bien," due to a zero in one of the scientific compositions on geometric progressions. This note was disqualifying, but Poincaré benefited from the clemency of the jury (Fig. 3).

For what reason? The answer is the academic trusteeship, which was exercised over the high schools. The responsibility for the baccalaureate examinations fell to the professors of the faculties of science and of letters, and not to the secondary teachers (the latter would only be associated with juries from 1902). These were therefore the academics who were responsible for organizing the review sessions (two to three per year, depending on the time) and who were questioning the candidates. However, among Poincaré's examiners for the science baccalaureate were none other than Camille Forthomme and Nicolas Renard, great friends of the Poincaré family and who knew very well the value of the candidate. With his two baccalaureate degrees, Poincaré was pursuing his studies in elementary mathematics. His ambition was to prepare for the competitions for the *grandes écoles*. To do this, he joined in 1872 the *classe de mathématiques spéciales* of the high school, where he developed a friendship with Paul Appell. As we know, he was fifth in the competition for the *Ecole normal supérieure*, and first in that of the *Ecole polytechnique*. He would enter in this last school with the rank of major.

The Philosophical Formation of Poincaré

Now, let us return to the philosophical formation of Poincaré. Logically, after the *classe de rhétorique*, Poincaré would have had to get into the *classe de philosophie*. He made the choice, however, not to follow this path. He therefore prepared the philosophical part of baccalaureate degree by himself, by taking a few private lessons (Boutroux 2012). As we have seen previously, several testimonies suggest that he already had a certain philosophical erudition. His choice not to go in the *classe de philosophie* might seem surprising at first glance, but it is not as atypical as one might think. In fact, pupils could be present at a session of the baccalaureate of letters without having done a full year of philosophy, and it was therefore not uncommon for professors of philosophy to have almost no students at the end of the school year (Poucet 1999).

Several questions then arise. What were the programs of education and the pedagogical practices in philosophy in the years 1870–1871? What was the nature of the philosophical examinations to which he was submitted? What philosophical knowledge was he required to demonstrate? From whom did he take particular courses in philosophy? Before proposing elements of an answer to these questions, it is appropriate to turn to the consequences of the reform of bifurcation.

The replacement of the *classe de philosophie* by the *classe de logique* in 1852 had the effect of narrowing the perimeter of philosophical education. Moreover, the abolition of the *agrégation* of philosophy had created a deficit of teachers in this area. As a result, in the 1860s, the philosophy teachers in the high schools did not necessarily have a specific training in philosophy; they came very often from a faculty of arts and held a BA in French and Literature.[22] Moreover, literary education could have played a part in the philosophical formation of students because his program gave a strong emphasis on classical authors.

The Duruy reform restored the class in philosophy under its former name, and reintroduced the *agrégation* in philosophy. While the baccalaureate consisted of oral exams, this created written tests and recast the entire educational program. This reform of philosophy was called for by the intellectual community, by academics and by a large number of secondary teachers, coming both from public and confessional high schools. Some saw in the restoration of the philosophy a challenge for the discipline itself, the deletion of the *agrégation* having contributed to dispersing and disrupting the philosophy teachers. There were others who felt that the issue at stake lay within the framework of the debate over scientific materialism.[23] It goes without saying that the failure of the system of the bifurcation marked an important victory of the literary and the Catholic parties in the intense decades-old struggle between supporters of scientific education and defenders of the literary education (Gontard 1972).

[22]Thus in 1865, in the classical high schools, of 75 Chairs in philosophy, 37 were occupied by professors – that is to say by *agrégés* (17 in philosophy, 20 letters) – and 38 by course instructors (mainly bachelors of Arts and Literature). Within this population of 75 teachers there were 6 ecclesiastics (Poucet 1999, 23–24).

[23]For a corroborating example, consider Father Lacordaire's 1872 *Discours sur les études philosophiques*: "Reason is a gift of God. To kill philosophy is kill reason in its most profound exercise, and in its highest manifestation. Where there is no more philosophy, there really begins the reign of physics. Philosophy not only serves as preparation for Christianity in exercising the reason in and turning toward the interior spectacle of the soul; it is also its shield against a terrible enemy, the sophists .... At any price, we must elevate philosophical studies and introduce to it those who can exert some influence on the intellectual leadership of our homeland" (Lacordaire 1872, 252 and 260).

From 1852, the teaching program in philosophy was organized around four major issues: study of the human mind and language, method in the various orders of knowledge, application of the rules of the method to the study of the principal moral truths, and analysis of philosophical authors. From 1865 onwards, the program of philosophy for the Baccalaureate of Letters was redefined around four major divisions: psychology, logic, morality, and theodicy.

The classical authors in the program were Xenophon (*Memoirs de Socrate*), Plato (*Gorgias*), Cicero (*De re publica*, *Tusculanae quaestiones, De Officiis*), Seneca (*Selected Letters*), Arnault and Nicod (*Logique de Port Royal*), Descartes (*Discours de la méthode*), Pascal (*De l'autorité en matière de philosophie, Réflexions sur la géométrie en général, De l'art de persuader*), Bossuet (*Traité de la connaissance de Dieu et de soi-même*) and Fénelon (*Traité de l'existence de Dieu*) (Fig. 4).

We do not know the details of the tests passed by Poincaré in the examinations, other than French composition. Considering the circumstances – Nancy was then under German occupation – this had for its topic, "How a nation can recover." Poincaré's contribution clearly made a very good impression on its reviewers (Appell 1925). In philosophy, Poincaré passed an oral test of a quarter of an hour that focused on the entire philosophical program. He had to prove to his examiner that he possessed sufficient knowledge on philosophical questions drawn at random from the program. This test was primarily an exercise of memory where the manual played a decisive role.

In 1860–1870, teaching practices in philosophy do not seem to have rested on the drafting of philosophical essays. The codified exercise of the dissertation in French was then being put in place, but it was rather reserved to students who were destined for professorial chairs (the *normaliens*, candidates for the *agrégation*); it did not become the central exercise for the training of secondary school students until the years 1880–1890 (Poucet 2001). Without knowing on what points of the philosophy program Poincaré was questioned, we can at least get a general idea from the subjects treated by the candidates at the *Ecole normale supérieure* or to the *agrégation*: "the laws of nature" (*agrégation* 1850), "the main rules that serve as the basis of induction" (*agrégation* 1860), "human knowledge, the concepts which do not come directly or indirectly from experience" (*agrégation* 1895), "of the true philosophical method, comparison of the methods of Bacon and of Descartes, their similarities and differences" (*Ecole normale supérieure* 1850), "existence and nature of the soul, exposition of the entire philosophy of Descartes" (*Ecole normale supérieure* 1863) (Poucet 2001).

From whom did Poincaré take courses in philosophy? In the absence of evidence, direct or indirect, it seems impossible to answer this question. However, the exploration of the local educational context allows one to formulate a few plausible paths. Who was in the position of teaching philosophy in Nancy?

At the *lycée de Nancy*, there was one professor of philosophy: Jean-Baptiste Dupond (1821–1875). He was educated at the *Ecole normale supérieure* (promotion 1842). After having obtained his *agrégation* in 1848, he had taught at the high

Psychology	Logic	Moral	Theodicy
Of psychological facts and consciousness	Of truth and error. Of the obvious, the certain, the probable	Various reasons for our actions	Existence of God. Evidence of the existence of God
The faculties of the soul: sensitivity, intellectual faculties, activity	Signs and language in their connection with thought	Moral Conscience. Distinction of good and evil. Duty and virtue	Key attributes of God. of Providence. Rebuttals and objections from physical harm and moral evil
Sensitivity: the senses, the sensations and feelings	Of the method: analysis and synthesis	Merit and demerit. Penalties and rewards. Moral sanction	Human destiny. Evidence of the immortality of the soul, moral or religious duties of man toward God
Intellectual Faculties: perception, consciousness, memory, imagination, judgment, reason	Analogy, induction and deduction. reasoning, syllogism	Division of duties. Duties of Man toward himself, toward his fellows, the family and the State	Notions of history of philosophy
Ideas in general, their origin, their characters. Concepts and primary truths	On definition, division and classification		
On activity and its various characters. Voluntary and free activity. Demonstration of freedom	Methods in the different orders of science		
On personality, the spirituality of the soul. Distinction of the soul and of the body and their connections	Authority of the testimony of men		
	Errors and sophistry		

Fig. 4 Philosophy program for the baccalaureate of letters, 1865 (Poucet 1999, 365–366)

schools of Périgueux and Bourges before being appointed to Nancy at the beginning of 1850. Starting in 1864, he offered courses in the literary section as well as in the section dedicated to the preparation for the special schools. Without any possible doubt, Poincaré could have encountered his teaching, about which unfortunately we do not know anything.[24] Dupond seems to have been entirely dedicated to his teaching work; he is not known not to have any publications, and he did not take

[24]He could have been his student in the class of elementary mathematics, after obtaining his two baccalaureate degrees, because instruction in philosophy was often dispensed with.

Fig. 5 Henri Poincaré at 18 (Source: *Livre du centenaire de la naissance d'Henri Poincaré*, Paris, Gauthier-Villars, 1955)

part in the academies and local learned societies. He would keep his position in Nancy until 1872 and then finish his career at the high school in Clermont-Ferrand (Fig. 5).[25]

If we now look at the Faculty of Letters, the chair of philosophy was held by Amédée De Margerie (1825–1905). Educated at the *Ecole normale supérieure* (promotion 1845) and *agrégé* (1847), he had taught philosophy in various high schools in province before being appointed a professor at the Faculty in 1856. A Catholic campaigner, he was very active within labor circles. When the Wallon Act established in July 1875 the freedom of higher education and therefore allowed the creation of private faculties, De Margerie would resign from his position to participate in the foundation of the Catholic Faculty of Lille, of which he would serve as Dean until his retirement. He was one of the organizers of the international scientific congresses of Catholics (1888, 1891, 1893) as well as a staunch advocate of the monarchy and the Catholic tradition. Opposed to liberalism and to gallicanism, he became actively involved in the legitimist camp from 1873, supporting the Count of Chambord and the party of restoration of the monarchy.

Among his abundant publications, one should mention *De la famille, leçons de philosophie morale* (De Margerie 1860), *Théodicée: études sur Dieu, la Création et la Providence* (De Margerie 1865), a handbook of contemporary philosophy (De Margerie 1870) as well as a large number of books of a political inspiration which would enjoy a certain success in monarchist circles.[26] His philosophical point of view was close to the spiritualistic currents represented by Elme Caro or Léon Ollé-Laprune; it was influenced by the Thomist doctrines, and opposed to the skepticism of Kant (De Margerie 1864a) and to Darwinism (De Margerie 1864b). He would serve as director of the Catholic magazine *La quinzaine: revue littéraire, artistique et scientifique* from 1894 to 1907.

[25] His replacement in the high school would be Armand Biechy, author of a thesis on the method of Bacon (Biéchy 1855).

[26] *La restauration de France* (1871), *La solution* (1881), *Avant la bataille* (1881), etc.

De Margerie evolved in the same academic and political circles as Poincaré's father, and the two families were on familiar terms. He was a well-known personality in Nancy: like Emile-Léon Poincaré, De Margerie was a member of the municipal council and of the *Académie de Stanislas*. Elected to this society in 1857, he was the only philosopher to take an active part, through the publication of various articles in the *Memoires de l'Académie de Stanislas.* His son, Antonin De Margerie, was the same age as Aline, Poincaré's sister. Poincaré and Antonin De Margerie were together at the imperial high school and then at the *Ecole polytechnique*.[27] Amédée De Margerie was thus socially close to Poincaré. Such proximity does not by itself suffice as evidence, and does not allow us to say with certainty that it was he who prepared the young candidate for the tests of the baccalaureate. It is, however, a plausible hypothesis. It was, in any case, in front of Amédée De Margerie that Poincaré would undergo his oral exam in philosophy, for which he would get the mark of 2 ("Assez bien"). It can be reasonably assumed that Poincaré would have prepared for the philosophical test using his examiner's manual of contemporary philosophy.

Is it possible to consider Amédée De Margerie as Poincaré's initiator into philosophy? This seems more difficult. In effect, Amédée De Margerie was far from sharing the social and political ideas of Poincaré's father, and it is doubtful that the two families would have gotten along.[28] In addition, if one considers the hostility that the philosopher demonstrated toward Kantianism and Darwinism, or even his positions on traditional values and religion, it seems difficult to consider the future mathematician as his philosophical disciple, since the latter would go in a diametrically opposed direction based on a form of neo-Kantianism, with a distinctly evolutionary view of epistemology. One might even be tempted to see in the reminiscences of Paul Appell, which served as the epigraph to this article, an underhand critical evocation of Amédée De Margerie:

> We used occasionally to philosophize: Poincaré smiled gently at the psychology and the naive theodicy that was then taught in the course of the baccalaureate's degree. I also remember long conversations on the scientific and philosophical reasons to believe in the existence of life in other planets. (Appell 1925, 23)

Conclusions

Recalling the original aim of this paper – to characterize the philosophy of Poincaré – the account of the philosophical formation of the future mathematician has opened certain pathways, but has not reached a definitive conclusion. At least

[27] After his studies at the Ecole polytechnique, Antonin De Margerie (1856–1914) would join the artillery corps and end his career with the rank of colonel.

[28] Aline Boutroux painted a rather critical portrait of the personality of Amédée De Margerie, pointing out in particular that his courses took the form of long and boring sermons (Boutroux 2012). In addition, in his correspondence with his mother while a student at the *Ecole polytechnique*, Poincaré spoke in very distant terms about his relations with the philosopher's son.

we have discovered some unpublished biographical or autobiographical sources, but we have to admit that the documents at our disposal do not allow us to draw more than a quite imperfect portrait of Poincaré as a (very) young philosopher. Was he interested in philosophy in his youth? Certainly, yes. The challenge is then to determine what, in his course of training, in his social and intellectual relations, in his social milieu, in his readings, determined, one way or another, his interest in philosophical questions. As we have seen, the elucidation of this question must take account of different historical contexts in which the trajectory of the young Poincaré was inserted, and it requires us to consider him as a minor player in a system of social constraints. The aim is then to reconstruct this system in the most fine-grained way possible and to incorporate it into the trajectory of his life. This would mean conceiving of the biography of Poincaré, not only as the story of a life in which science played a central role, but also a social and intellectual history.

Our journey stops in 1873, the year Poincaré left Nancy to enter at the *Ecole Polytechnique*. Then began a period of training during which one can find many traces of Poincaré's proximity to philosophy and philosophers. The most essential is without doubt his meeting with his future brother-in-law, Emile Boutroux (who would replace Amédée De Margerie in the chair of philosophy in the Faculty of Letters of Nancy in 1876). On this basis, Poincaré would enter into relationships with Paul and Jules Tannery, Louis Liard and Félix Ravaisson.[29]

Another trace, less known, but perhaps equally important, is the subject of the relationship that Poincaré seems to have maintained with Auguste Calinon (1850–1900?). Educated at the *Ecole polytechnique* (promotion 1870) and then turned towards civil engineering, Calinon pursued the career of industrial engineer in the Lorraine steel industry, not far from Nancy.[30] Author of several studies on mechanics and geometry, he was among those thinkers who attempted in the 1880s to found a philosophical mathematics (Calinon 1885, 1888, 1889, 1893, 1895, 1900), that is to say, a discipline in which the methods and tools of conventional mathematical theory are put at the service of an epistemological problem, specifically concerning the conditions of possibility of a general theory of the determinations of space. He thus conceived of a *general geometry* comprehending all possible geometries, within which Euclidean geometry constitutes only a particular case. Such a geometry would, according to him, invalidate the thesis that Euclidean geometry is the only possible true geometry, without questioning the idea that Euclidean geometry is privileged on empirical grounds.

At first sight, and judging by the correspondence of Poincaré, the two authors encountered one another only in the middle of the 1880s, on the occasion of a short epistolary exchange dated 1886 (Rollet 2007, 122–125). However, we find

[29]An essential source for this period, particularly for the years 1873–1878, is the correspondence of Poincaré with his mother and sister (http://www.univ-nancy2.fr/poincare/chp/). For more details on the meeting Poincaré-Boutroux and its consequences see Nye 1979, as well as Rollet 2000 and Boutroux 2012 (chapter XXVII).

[30]Biographical information about her is very scarce. For an overview, cf. Maubeuge 1975.

that the two men regularly saw one another from 1874, to the point of spending several days together on vacation in the Vosges. In addition, shortly before she married Emile Boutroux, Calinon officially asked for the hand of Poincaré's sister in marriage (Rollet 2012a). Knowing the philosophical conceptions of Poincaré on non-Euclidean geometry, it seems difficult not to consider the existence of an ongoing relationship between the two authors.

For the time being, the life and work of Poincaré continue to resist the efforts of biographers. One hundred years after the death of Poincaré, and in a context of scarcity, these few traces have no other ambition than to establish a few chapters or sections of this biography that we have yet to write.

References

Appell, P. 1925. *Henri Poincaré*. Paris: Plon-Nourrit et Cie (collection Nobles vies, grandes œuvres).

Belhoste, B. 2001. La préparation aux grandes Ecoles scientifiques françaises à la fin du 19° siècle, établissements publics et établissements privés. *Histoire de l'éducation* 90: 101–130.

Bellivier, A. 1956. *Henri Poincaré ou la vocation souveraine*. Paris: Gallimard NRF.

Biéchy, A. 1855. *Essai sur la méthode de Bacon. De l'idée de la science (thèse de doctorat de philosophie)*. Toulon: Imprimerie d'E. Aurel.

Boutroux, A. 2012. *Vingt ans de ma vie, simple vérité … La jeunesse d'Henri Poincaré racontée par sa sœur (1854–1878) – Texte inédit édité par Laurent Rollet*. Paris: Hermann.

Calinon, A. 1885. *Etude critique sur la mécanique*. Nancy: Berger Levrault.

Calinon, A. 1888. *Etude sur la sphère, la ligne droite et le plan*. Paris: Gauthier-Villars.

Calinon, A. 1889. Les espaces géométriques. *Revue philosophique* XXIX: 588–595.

Calinon, A. 1893. Etude sur l'indétermination géométrique de l'univers. *Revue philosophique de la France et de l'étranger* 36: 595–607.

Calinon, A. 1895. *La géométrie à deux dimensions des surfaces à courbure constante*. Paris: Gauthier-Villars.

Calinon, A. 1900. *Etude de géométrie numérique*. Paris: Gauthier-Villars.

Darboux, G. 1913. *Eloge historique de Henri Poincaré*. Paris: Gauthier-Villars.

De Margerie, A. 1860. *De la famille, leçons de philosophie morale*. Paris: Vaton.

De Margerie, A. 1864a. Le scepticisme de Kant, Dialogue. *Mémoires de l'Académie de Stanislas*:129–165.

De Margerie, A. 1864b. L'histoire naturelle et les causes finales, à propos du système de M. Darwin. *Mémoires de l'Académie de Stanislas*:358–393.

De Margerie, A. 1865. *Théodicée: études sur Dieu, la Création et la Providence*, 2nde édition. Paris: Didier.

De Margerie, A. 1870. *Philosophie contemporaine*. Paris: Didier et Cie.

Drouelle, R. 1986. *Léon Poincaré (1828–1892), sa vie, son œuvre en hygiène industrielle et en médecine du travail*. Thèse de doctorat de médecine. Faculté de médecine. Université Rennes 1, Rennes.

Fabiani, J.-L. 1988. *Les philosophes de la république*. Paris: Les Editions de Minuit.

Figuier, L. 1862. *La terre avant le déluge*. Paris: Hachette.

Giedymin, J. 1982. *Science and convention. Essays on Henri Poincaré's philosophy of science and the conventionalist*. Oxford/New York: Pergamon.

Gontard, M. 1972. Une réforme de l'enseignement secondaire au 19ème siècle. *Revue française de pédagogie* 20: 6–14.

Greber, J.-H. 2012. Caractériser un contexte à partir d'une base de données : le cas de la philosophie et de l'histoire des sciences entre 1870 et 1930. In *Les uns et les autres... Biographies et prosopographies en histoire des sciences*, ed. L. Rollet and P. Nabonnand, 541–574. Nancy: Presses Universitaires de Nancy.

Grünbaum, A. 1963. *Philosophical problems of space and time*, 1st ed. Dordrecht/Boston: D. Reidel Publishing Company.

Hinzelin, A. 1857. *Géographie historique, industrielle et statistique de la Meurthe, avec une esquisse historique de la Lorraine et précédée de notions de géographie générale*. Nancy: Grimblot et Vve Raybois.

Hinzelin, A. 1860. *Cours gradué de calcul*. Paris: Hachette.

Hulin, N. 1982. A propos de l'enseignement scientifique : une réforme de l'enseignement sous le Second Empire : la bifurcation (1852–1864). *Revue d'histoire des sciences* 35: 217–245.

Hulin, N. 1986. *L'enseignement scientifique sous le Second Empire : la "bifurcation" et la formation des professeurs de l'enseignement secondaire*. Ecole des hautes études en sciences sociales, Thèse de doctorat.

Joly, J.-S. 2000. *Léon Poincaré (1828–1892), un nom célèbre, une œuvre oubliée*. Thèse de médecine, sous la direction de Georges Grignon. Faculté de Médecine de Nancy, Université Henri Poincaré, Nancy.

Lacordaire, H. 1872. Discours sur les études philosophiques. In *Œuvres*, tome 7. Paris: Poussielgue.

Maubeuge, P.-L. 1975. Hommes de science lorrains : le mathématicien Auguste Calinon et le naturaliste Benoît de Maillet. *Bulletin de l'Académie et des Sociétés des sciences lorraines* 14(4): 133–146.

Meuriot, P. 1919. Le baccalauréat. Son évolution historique et statistique des origines (1808) à nos jours. *Journal de la société statistique de Paris* 60: 8–35.

Nye, M.J. 1979. The Boutroux circle and Poincaré's conventionalism. *Journal of the History of Ideas* XL: 107–120.

Poincaré, E.-L. 1886. *Traité d'hygiène industrielle, à l'usage des médecins et des membres des conseils d'hygiène*. Paris: Masson.

Poincaré, E.-L. 1873–1874. *Leçons sur la physiologie normale et pathologique du système nerveux*, deux volumes. Paris: Berger-Levrault.

Poincaré, J.H. 1902. *La science et l'hypothèse*. Paris: Flammarion.

Poincaré, J.H. 1905. *La valeur de la science*. Paris: Flammarion.

Poincaré, J.H. 1908. *Science et méthode*. Nouvelle édition de l'ouvrage de Poincaré, accompagnée d'une préface et d'un appareil critique; Edité par Laurent Rollet. N° spécial de la revue *Philosophia Scientiæ*, 2011. Paris: Kimé.

Poincaré, J.H. 1913. *Dernières pensées*. Paris: Flammarion.

Poincaré, J.H. 2002. *L'opportunisme scientifique/scientific opportunism*, an anthology compiled by Louis Rougier, ed. Laurent Rollet. Berlin: Birkhäuser.

Pont, J.-C., and M. Panza (eds.). 1995. *Les savants et l'épistémologie vers la fin du 19ème siècle*. Paris: Blanchard.

Poucet, B. 1999. *Enseigner la philosophie – Histoire d'une discipline scolaire 1860–1990*. Paris: Éditions du CNRS.

Poucet, B. 2001. De la rédaction à la dissertation, évolution de l'enseignement de la philosophie dans l'enseignement secondaire en France dans la seconde moitié du 19ème siècle. *Histoire de l'éducation* 89: 95–120.

Príncipe, J. 2012. Sources et nature de la philosophie de la physique d'Henri Poincaré. *Philosophia Scientiae* 16: 197–222.

Prochasson, C. 1991. *Les années électriques 1880–1910*. Paris: Editions La Découverte.

Prost, A. 1968. *Histoire de l'enseignement en France 1800–1967*. Paris: Librairie Armand Colin.

Rollet, L. 2000. *Henri Poincaré: Des mathématiques à la philosophie. Étude du parcours intellectuel social et politique d'un mathématicien au tournant du siècle*. Lille: Éditions du Septentrion.

Rollet, L. 2007. La correspondance entre Henri Poincaré et Auguste Calinon. In *La correspondance de Henri Poincaré, volume 2: correspondance entre Henri Poincaré et des physiciens, chimistes et ingénieurs*, ed. S. Walter, A. Coret, and E. Bolmont, 122–125. Berlin: Birkhäuser.

Rollet, L. 2012a. La gloire de mon frère: Aline Boutroux (1856–1919) et Henri Poincaré (1854–1912). In *Vingt ans de ma vie, simple vérité... La jeunesse d'Henri Poincaré racontée par sa soeur (1854–1878)*, ed. A. Boutroux, Texte inédit édité par Laurent Rollet, 5–51. Paris: Hermann.

Rollet, L. 2012b. Jeanne Louise Poulain d'Andecy, épouse Poincaré (1857–1934). *Bulletin de la Société des amis de la Bibliothèque et de l'histoire de l'Ecole polytechnique*, special issue 'Henri Poincaré : le centenaire', 51: 18–27.

Salf, E. 2000. Léon Poincaré et son œuvre de précurseur. *La lettre du musée: Association des amis du musée de la Faculté de médecine de Nancy* 13: 2–3.

Simon-Nahum, P. 1991. Xavier Léon/Elie Halévy, correspondance (1891–1898). *Revue de métaphysique et de morale*:3–58.

Toulouse, É. 1910. *Enquête médico-psychologique sur la supériorité intellectuelle. Tome II: Henri Poincaré*. Paris: Flammarion.

Xardel, P. 2012. J'avais un ami... Henri Poincaré (texte inédit, 1913). In *Vingt ans de ma vie, simple vérité... La jeunesse d'Henri Poincaré racontée par sa sœur (1854–1878)*, ed. A. Boutroux, Texte inédit édité par L. Rollet, 317–327. Paris: Hermann.

Poincaré and the Invention of Convention

Janet Folina

Abstract Jules Henri Poincaré is famous for his "conventionalist" philosophy of science. But what exactly does this mean? Poincaré invented the category of convention because he thought that there are some central principles in science that are neither based on intuition, empirical data, nor that are arbitrary stipulations. His views here resemble those of Wittgenstein, in particular, as presented in *On Certainty*. The invention of convention is lauded (for example, by Robert DiSalle) as a genuine philosophical discovery. But it is also critiqued (for example by Michael Friedman) as yielding a vision of science that is too rigid – one that is refuted by general relativity. This paper aims to defend Poincaré's views about conventions by focusing on his central idea that conventional choices, though "free", are "guided" by experience. I will argue that conventionalism is not a commitment to *fixed* a priori stipulations, as DiSalle and Friedman propose. Rather, it mandates empirically motivated shifts in (even geometric) conventions – a view surprisingly in accord with Friedman's "relativized a priori", and thus more consistent with general relativity than is generally thought.

Poincaré's views about mathematics and science are fascinating and remain largely plausible. Highlights include the following. Logic is empty, so it is not a source of significant information. Mathematics is not empty; so logicism – the view that at least arithmetic (and possibly more of mathematics) is logic – must be false. In fact, Kant was right that a core of pure mathematical knowledge is based on a priori intuition and thus it has a synthetic a priori status. In this way, mathematics provides an a priori foundation for natural science, which should itself be viewed (at least in part) from a structural realist perspective.

J. Folina (✉)
Philosophy Department, Macalester College, 1600 Grand Avenue, Saint Paul, MN 55105, USA
e-mail: folina@macalester.edu

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How does conventionalism fit into this semi-Kantian picture? Poincaré presents conventions as intermediary principles found in scientific disciplines that lie on the border between pure mathematics (the synthetic a priori) and the natural sciences (the synthetic a posteriori). The disciplines in question, for Poincaré, are geometry and parts of physics. What is the role of conventions? Why does Poincaré introduce this new category into the taxonomy of science? Why do references to conventions virtually disappear in his later philosophical writings? What aspects of his conventionalist philosophy, if any, can be salvaged from its apparent collision with General Relativity?

This paper argues that to better understand Poincaré's invention of convention we must focus on the role of experience in both choosing and evaluating conventions. Poincaré was intrigued by the fact that the principles he came to regard as conventions played an essential role in science, yet failed to fit any traditional semantic or epistemic categories. He argued that they are neither analytic, nor empirical, nor synthetic a priori. (In this way, his vision resembles that of the later Wittgenstein, in particular as presented in *On Certainty*.) This invention constitutes a genuine philosophical discovery, as Robert DiSalle has argued (DiSalle 2006, Chapter 3). I will review his reasons for inventing this scientific category, with the goal of showing that for Poincaré experience plays a crucial role in *determining* conventions, even those in geometry.

Experience also plays an important role in *evaluating* conventions; I appeal to this role, in particular, to reconstruct Poincaré's views as rationally as possible. Of course some of his views may simply be outmoded, such as his unwavering support of Euclidean geometry. He acknowledged that scientific conventions can, and do, shift; and he argued for the coherence and utility of non-Euclidean geometry. Yet he repeatedly asserted that Euclidean geometry need never be given up. This implies a vision of geometric conventions as having a special place in the scientific hierarchy. Indeed Poincaré compared the different geometries to languages, which are neither true nor false, and which cannot in principle be either confirmed or undermined by experience.

In light of this protectionism towards Euclidean geometry, Michael Friedman has argued that Poincaré's geometric conventionalism was refuted by general relativity, which treats physical geometry as empirical (Friedman 1999, especially Chapter 4). The shift to general relativity (GR) showed, in other words, that geometry need *not* be treated as a mere language; instead, it can be regarded as providing part of the (broadened) empirical content of physics. In this way, Friedman also corrects the logical positivists' appeal to Poincaré's conventionalism, showing how – far from supporting GR – it is inconsistent with it.

In the latter part of this paper I attempt to mitigate the inconsistency between GR and Poincaré's conventionalism. Certainly there are some very general conventionalist views that remain true – for example, that there are empirical-sounding sentences that don't play an empirical role in science; that we have to postulate, or presuppose, some (empirical or quasi-empirical) truths in order to test other hypotheses; that science has a framework, or structure, that includes both un-testable and indirectly testable components. Does anything more specific to Poincaré survive?

Now Friedman has shown that GR is inconsistent with Poincaré's central views about geometry; this includes the special, protected status of geometry, owing to its intermediary position in the scientific hierarchy – between the synthetic a priori truths of mathematics and the empirical truths of physics. Also, the way Poincaré appears to construe the function of the scientific hierarchy is no longer acceptable – in particular, the idea that mathematics offers us exactly three geometric alternatives (the three dimensional geometries of constant curvature) from which we must choose one as the basis for physical measurement. So Poincaré's philosophy of geometry cannot be reconstructed as consistent with relativity, no matter how charitable we may try to be. But I will argue that more of conventionalism survives than one might think; that is, conventionalism and relativity are not as inconsistent as they may at first seem.

What can easily be forgotten in hindsight is Poincaré's emphasis on the central role of experience, not only in *making* but also in *evaluating* conventional choices. That is, despite references to liberty in this context, Poincaré did not regard the choice of a geometric system, or any convention, as arbitrary or completely free. In using the term "convention" he is making a claim about convenience, rather than arbitrariness (as in a "mere" convention). Geometric choices are free but *guided by experience*; moreover, geometric choices, like all conventional choices, can be revised in the light of further experience. Why does this matter?

In their critiques of geometric conventionalism, DiSalle and Friedman both cite as a central error Poincaré's rigid classification of geometry as a priori. Rather than conventionalism in general, DiSalle argues that Poincaré's error lies in

> ... his particular view of the privileged status of space. The theory of space will not be overturned by principles of physics, because space is exhaustively defined for us as a pre-physical notion, and because, therefore, the transition from geometry to physics must always introduce extraneous elements into the concept of space. That geometry has always involved such elements, ... was an empiricist conviction that Poincaré never took to heart (DiSalle 2006, 94).

Friedman also argues that Poincaré's vision of physical geometry – though correct for classical physics – is incompatible with relativity theory, because of his presupposition that physical geometry belongs to "the a priori part of our theoretical framework" (Friedman 1999, 85). Similarly, according to DiSalle, Poincaré's understanding of the way the concepts of physical geometry function in science meant that "they had to be considered a priori rather than empirical" (DiSalle 2006, 95).

I want to call into question this way of framing Poincaré's error. It explains the inconsistency between Poincaré's views and GR by supposing that Poincaré was committed to a fairly sharp distinction between the a priori and the empirical elements of a theory; and that the function of geometric and other conventional principles makes them a priori and *not* empirical. Moreover it supposes that, once stipulated, geometric principles are isolated from empirical results owing to their a priori status. In contrast, I will argue that the point of inventing the category of conventions was to provide a *new* classification for certain principles that seemed to be neither a priori nor empirical. Rather, in my view, he regarded them as having an intermediary epistemic status, which he struggled to articulate.

Unlike Quine, Poincaré is not out to dispute the validity of our general distinctions. Poincaré is committed to principles that are clearly a priori – those of logic, mathematics and language (the synthetic and the analytic a priori). He is also committed to the fact that certain scientific assertions are clearly empirical – for example, the more experimental areas of science (often giving the example of optics). What he disputes is the exhaustive nature of these distinctions, introducing the idea that there are principles that cannot be categorized along ordinary lines, or by ordinary criteria. These include, in particular, the conventions of geometry and mechanics, which he argues are neither empirical nor a priori in the ordinary senses; perhaps they seemed to him to be a bit of both.

Conventions *act* a priori, in that they contribute some of the framework principles necessary for the methodology of science. But they are also both *suggested by* and *acted upon* by experience, in that conventions are rooted in experience and prompt choices that must respond to, and sometimes change in the light of, empirical data. In this latter respect, they resemble empirical assertions.

To reiterate, *given*, or *within*, a framework, conventions act as a priori principles. But Poincaré recognized that frameworks change, and these changes come from the influence of experience. (In this sense, he can also be considered a naturalist. See Stump 1989.) Thus Poincaré's view is not that conventions are absolutely a priori, but that they are only relatively a priori.[1] Both Friedman and DiSalle cite the rigidity of Poincaré's conventions as an obstacle to a more flexible view about the presuppositions of science; and that we have to wait for Reichenbach and Carnap for a more modern, empiricist, approach to framework principles (See, for example, Friedman 1999, Chapter 3). In contrast, I see Poincaré's conventionalism as itself providing the basis of this more flexible attitude. For certain periods of time conventions function like a priori truths, but unlike ordinary a priori truths they are susceptible to revision owing to changes in data and/or theory.[2]

To appreciate what might survive of Poincaré's conventionalism, and his geometric conventionalism in particular, we thus need to revisit the crucial roles of experience in articulating and evaluating conventions. Though Poincaré viewed geometry as holding a privileged, *more* protected, position in the scientific hierarchy, he did not regard it as *absolutely* protected by its position and function in the hierarchy. Despite his own comparisons, Poincaré's conventions should not be understood as arbitrary, or analytic like linguistic conventions; nor should they be understood as a priori in any ordinary sense. Scientific conventions have a special character. They must be free in that neither logic, nor mathematics, nor experience

[1]The idea of the relative, or relativized, a priori has recently taken hold, owing largely to the influence of Friedman 1999 and 2001. There is a growing body of literature on the topic, including several new essays by Friedman extending and modifying his views (for example, Friedman 2011, and 2012). See Stump 2011 for an account of Arthur Pap's similar "functional a priori", and Poincaré's influence on Pap. Like Stump, I support an interpretation of Poincaré's conventionalism as rather close to the relativized a priori.

[2]For just one example, he comes to accept the existence of atoms after first denying them, thus giving up a principle of the continuity of nature.

force particular principles on us. That is, there must be more than one viable alternative – a choice must be made. However conventions are also answerable to our empirical situation and data – the facts that guide our initial conventional choices as well as any revision of those choices.

Focusing too much on the freedom of conventions can encourage a misunderstanding of scientific conventions as arbitrary – something Poincaré was, in fact, determined to refute (See Poincaré 1905a/1958, Chapter X). It can also make the refutation of conventionalism seem inevitable, owing to changes in scientific frameworks. In contrast, on this interpretation, conventions are features of a scientific framework that respond to empirical information. Thus, conventionalism is not a commitment to fixed a priori stipulations. Rather, it mandates empirically motivated shifts in (even geometric) conventions – a view surprisingly in accord with the relativized a priori, and thus less in conflict with relativity.[3]

Background and Context

Conventionalism gets the spotlight in Poincaré's first book, *Science and Hypothesis*, where it is presented as a middle position between naïve realism and simple skepticism, one that recognizes both the *complexity* and *structure* of science. As he put it, "To doubt everything or to believe everything are two equally convenient solutions; both dispense with the necessity of reflection" (Poincaré 1902a/1952, xxii).

Complexity

On the one hand, conventionalism should be distinguished from a general skeptical attitude. Overemphasizing the role of choice and construction in science, and underemphasizing the role of experiment, can lead one to skepticism. But doubting everything is a superficial epistemic stance, which is neither justified nor fruitful. Poincaré aims to distinguish his view that conventions, and choices, are *necessary*

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[3]In writing this paper I realize I have entered a thick territory. The literature on, and related to, Poincaré's conventionalism is enormous and I cannot pretend to have mastered it. I have approached the topic by first re-reading Poincaré's central texts and then by addressing just a few secondary works that have particularly influenced me. I hope to make a small contribution to this literature by supporting a slightly more empirical interpretation of conventions. I thank Maria de Paz and Robert DiSalle for inviting me to contribute to this volume; Michael Friedman, Robert Disalle and David Stump for their excellent work on this topic; and David Stump for comments on an earlier, written draft. Several audiences should also be thanked for putting up with early, half-baked talks on some of this material, including those at our Poincaré session at HOPOS, June 2012, and especially those attending the *Foundations of Physics and Mathematics Workshop* at the University of Western Ontario, May 2012.

from a view that renders them as closer to *sufficient*. Scientists do not create facts, as he argues (Poincaré 1905a/1958, Chapter X). The scientist may create a convenient language in which facts can be expressed, but the success of science shows that experience (of facts) is central to scientific methodology. Thus, Poincaré regards conventions as part of an account of science that acknowledges choice but also emphasizes the necessity of experience for objective knowledge. It is part of a picture of science aimed to repudiate both global skepticism and simple relativism.

On the other hand, Poincaré also distinguished conventionalism from the view that science is certain. Overemphasizing the basis of science in the certainty of logic and mathematics can lead one to naïve realism, which also overestimates the certainty of the experimental method (while oversimplifying the structure of scientific judgment). Poincaré describes this view as follows:

> To the superficial observer scientific truth is unassailable, ... Mathematical truths are derived from a few self-evident propositions, by a chain of flawless reasonings . . . . By them the Creator is fettered, as it were, and His choice is limited to a relatively small number of solutions. A few experiments, therefore, will be sufficient to enable us to determine what choice He has made . . . . This, to the minds of most people, ... is the origin of certainty in science (Poincaré 1902a/1952, xxi).

Poincaré here points out two things. First, mathematics is not simply making deductions from a small number of “self-evident” axioms and leading to a few mathematical options. Second, science is not simply conducting a few crucial experiments in order to decide between the narrow set of options provided by mathematics. Just as we shouldn’t overemphasize the importance of convention, so we shouldn’t oversimplify the roles of either mathematics or testing in science. Clarifying the nature and functions of conventions in science is meant to help correct both of these mistakes – that of the skeptic and that of the naïve, or over-zealous, realist. As a philosophical response to both naïve realism and skepticism, conventionalism thus supports the *complexity* of science.

Structure

Conventionalism is also a view about the *structure* of science. As a philosophical position, it opposes thoroughgoing empiricism and homogeneous holism. Geometry cannot be directly tested since tests are done on bodies, not space; because geometry cannot be directly tested, Poincaré argues that geometric empiricism has no rational meaning. And though the different parts of a scientific framework are connected, Poincaré argues, against holism, that a scientific theory is not a simple *set* of homogeneous propositions. The parts of science form a structure, which can be understood. Conventions constitute some of the implicit structure of science, structure that binds mathematics to the empirical world and enables empirical testing. Furthermore, *Science and Hypothesis* argues that the way to understand the parts of the structure of science is in terms of the different degrees to which those parts are testable; these different degrees will correspond to position in the scientific hierarchy.

Conventional sciences comprise the middle two sections of this hierarchy and of the book, between a first section on pure mathematics and a last section on "nature", or experimental science. Conventional principles thus lie between the truths of pure mathematics – the a priori domain provided by logic and intuition – and the truths of the experimental sciences – the empirical domain governed more directly by experience. The category of convention is in this way central to the sciences of geometry and mechanics, which occupy the two middle sections of *Science and Hypothesis*, and which correspond to two main types of conventions for Poincaré.

Conventions are thus situated in Poincaré's hierarchy – between pure mathematics and the more experimental areas of natural science – for they are neither a priori in any ordinary sense, nor straightforwardly empirical, or testable. As Michael Friedman points out, they provide Poincaré with a way to accommodate the quasi-empirical aspects of science that Kant mistakenly took to be a priori (metric and general, physical principles) (Friedman 1999, 81). Though geometric conventions may be closer to pure mathematics, and therefore further from experiment than mechanical conventions, I will argue that like any other conventional science, geometry relies on choice *and* experience.

The Nature of Conventions: Free but Guided

A coherent account of Poincaré's geometric conventionalism begins with the distinction between pure and applied geometry, and it emphasizes the freedom of conventions. That is, we are *free* to develop various pure geometric systems – within some minimal confines such as consistency. And the stipulation, or choice, of which geometric system to apply in physics is also free. It is only after these two steps of pure and applied mathematics that the account emphasizes the role of experience: when the combination of physics plus geometric system is tested. The nature of the scientific hierarchy, on this view, means that geometry is chosen strictly *prior to* testing; and the choice of a geometric system, like the choice of a linguistic convention, is free.

A virtue of this account is its clarity. There is both a logical distinction and a temporal separation between pure mathematics and its application, and between its application and any empirical testing. Furthermore, there is much to recommend it in Poincaré's own rhetoric. Indeed some of his remarks about pure geometry make conventionalism seem like formalism, such as the idea that axioms are *disguised definitions* of the basic geometrical concepts. Here he is arguing against a traditional conception of geometric axioms as meaning-*reflections* that result in *truths* about space. Instead he is endorsing a view closer to formalism about axioms – that they are meaning-*determinations* that *stipulate* how we *use* certain spatial concepts. The traditional view of axioms yields a traditional view of mathematical truth. The alternative view he seems to be endorsing yields a more relativized, or "indexed", concept of truth – that of "true in" one system or another.

In calling geometric axioms "definitions in disguise" (1902a/1952, 50) Poincaré is furthering a picture of axioms as truth-*makers*. Geometry does not lead us to truth; rather, in some sense it creates truth, in that it articulates a framework in which mathematical truths can be discovered. The results are "true in" Euclidean or Lobachevskian geometry, though not true *simpliciter*. Geometric axioms are meaning determinations for Poincaré, because he thinks we don't have a definite pre-theoretic concept of point, line, plane, etc. (See for example, 1905a/1958, 45–46). As in algebra, there is more than one legitimate mathematical structure that instantiates the basic geometric concepts. Thus, the tidy view of Poincaré's geometric conventionalism emphasizes its relation to both algebraic formalism, which preceded it, and Hilbertian axiomatics, the development of which was mostly subsequent.

Despite its clarity, I find this interpretation incomplete. Granted, Poincaré's philosophy of geometry is very close to formalism. Poincaré distinguished pure from applied geometry, and he maintained that results derived within a pure geometrical system remain "rigorously true" even if the empirical world fails to *precisely* satisfy them. But he also believed that if the empirical world failed to approximately satisfy our geometric results, then we would never have developed such a geometry.

For one thing, Poincaré did not approve of formalist approaches to mathematics in general. He famously argued against Hilbert that formal systems do not stand mathematically on their own; rather, they need to presuppose some basic truths of mathematics (such as induction) (See his circularity arguments in Poincaré 1905b–1906b/1996; see also Folina 2006). These basic truths of pure mathematics are synthetic a priori and are forced on us by the nature of our minds.

Secondly, though the basic truths of geometry are *not* synthetic a priori, since geometric axioms are *not* forced on us by our minds, neither are they *merely* formal, arbitrary rules added to the synthetic a priori, pure mathematical basis. This is because geometric principles are not chosen *only* by considerations of consistency. For example, he praises Hilbert for making great progress in geometry, but he also considers his approach to be "incomplete" because it focuses on "the logical point of view alone" (Poincaré 1902c/1994, 167). Lacking in Hilbert's account is a connection between the geometric terms and experience that could help us to understand and choose between different systems of postulates.

Any convention provides a "rule of action"; but unlike games, scientific rules of action are shaped by empirical conditions (Poincaré 1905a/1958, 114). Poincaré sees geometry as intimately connected to experience, and the empirical world, in ways that a *merely* formal set of rules is not. Though he considers geometric axioms to be disguised definitions of the basic terms, by this he means that the terms have no *precise* prior meaning, from informal or experiential contexts – not that they have no prior meaning at all! Experience provides rough, or crude, meanings for the basic geometric terms; and geometric postulates are not empty rules for this reason.[4]

[4]Thanks to David Stump for making me clarify this point.

Geometry requires significant empirical conditions for its very existence, including conditions on the outer world as well as conditions on ourselves, our bodies. In terms of *truth*, work within an area of geometry has a formal-mathematical character: there are well-defined concepts, axioms, deductive proofs, etc. But in terms of *subject matter*, Poincaré saw geometry as less pure and more empirical than other areas of mathematics, owing to geometry's dependence on a number of empirical facts, which he tried to articulate. For him, geometry was, in a sense, too empirical to be like other areas of pure mathematics, yet not empirical enough to be a natural science. Some of the richness of conventionalism involves the ways in which even the pure geometric work is guided and supported by experience (to which we shall turn shortly).

Third, empirical information also plays a crucial role in choosing an applied geometry. The idea that there is a clean separation between the stipulation of an applied geometry and subsequent scientific activity – including physical tests and assessments of the results – just seems too simple for what Poincaré struggled to articulate. It implies that there is a one-way path from pure mathematics, through geometric and mechanical conventions, to physical experiments, with everything in the process, or hierarchy, fixed before the next step is taken. In contrast, in the latter part of this paper I will explore a more complex interpretation, where the "arrows", the influences, between conventions and experimental science go both ways.

Admittedly, Poincaré did seem to think that a geometric system is *ordinarily* fixed, or chosen, prior to physical testing; this would especially have seemed to be the case to him: an important figure at a transitional time.[5] But central to Poincaré's view of conventions is the provision that empirical results can reverberate back through the chain of sciences after the fact, so to speak, of the stipulation. This reverberation can then lead to a revision of those stipulations, those conventions, as well as the more straightforwardly physical parts of a theory. That is, it is *not* the case that once stipulated, conventions, including those of geometry, are absolutely fixed and not revisable. To put it yet another way, there is a little bit of holism and a little bit of empiricism in Poincaré's conventionalism.[6]

For these reasons I will emphasize in what follows the sense in which conventions are *guided* rather than in which they are *free*. Freedom, the idea of a multiplicity of structures, was well known by the early 1800s, from algebra. The inventiveness of the category of convention is in the sense in which they are *guided* by experience.

> Experience leaves us our freedom of choice, but it guides us by helping us to discern the most convenient path to follow .... Some ... have forgotten that there is a difference between liberty and the purely arbitrary (1902a/1952, xxiii).

[5]Thanks to Bill Demopoulos for this point; Friedman 2001 and DiSalle 2006 also emphasize this.

[6]Though note: Stump 1989 cautions that any holism Poincaré endorses is specific; that is, Poincaré does not advance a *general* holism or appeal to *general* under-determination considerations to advance the flexibility of conventions.

First we will review the empirical conditions that surround the *subject matter* of geometry; these are the empirical preconditions for the possibility of pure geometry. Second, we will review the ways in which experience plays a role in *assessing* our choice of applied geometry. Such choices are necessarily responsive to empirical data, owing to the fact that once chosen, a geometric system becomes part of a larger system, of geometry plus physics, which can then be more explicitly tested. I first turn to the role the empirical world plays in fulfilling preconditions for pure geometry.

Empirical Preconditions: The Subject Matter of Geometry

Poincaré argues that geometry begins more in our bodies and less in cognition. For example, it may be motivated by a desire to solve a puzzle: did that object move or did it change? Poincaré's point is that the very distinction between change of place and change of state presupposes something about the world; and we solve the puzzle with our *bodies* and by *observation*, not merely with our *minds* and by *thinking*. Poincaré emphasizes the following empirical preconditions for geometry in Part II of *Science and Hypothesis*. (References will be to page numbers from 1902a/1952, unless otherwise noted, for the rest of this section.)

1. *Solid bodies (or approximate solids) (45)*
If the world were entirely fluid, he argues, there would be no system of distance measures. In such a world we might develop topology, provided there were some jello-type substances or provided some detectable differences between the various fluids. But ideas central to metric geometries, such as identity of line segments, areas, angles, etc., require some roughly solid bodies.

2. *Motion – of solid bodies*
Poincaré points out that the mere existence of solids is not sufficient; if there were solids but they could not move, or did not move, we would not form an idea of displacements (60). So the solids have to be able to move – while remaining approximately in the same state – for the question of a change of position versus a change of state not only to be raised, but also to be intelligible. That is, the motion of (relatively) solid bodies outside of us is what prompts us both to distinguish, and to raise questions, concerning change of place versus change of state.

3. *Motion of our bodies*
In addition, *our* bodies have to move, because this is how we distinguish a change of state from a change of position (57–59). That is, though the motion of other objects may be what *prompts* such questions, determining an *answer* involves the motion of our bodies, according to Poincaré. And since the meaning of any question depends in part on how it is answered, the motion of our own bodies is also central for understanding the distinction between change of state and change of position. Consider an ice cream cone that is melting. I can't "correct" or un-melt the ice

cream by moving my body around the cone, nor by moving the cone relative to my body. This is how I know that the ice cream is undergoing a change of state. In contrast, consider a horse that has run past me. I first see it from the side, and then it turns to face me. By moving my body (provided the horse stands still) I can "correct" the change in how the horse looked and return our relative positions back to an earlier one. Since I can "correct" our relative positions I infer that the horse changed place and not state. In this way, Poincaré thinks that the distinction between changes of position and changes of state requires mobility – that of both other objects and our own bodies (relative to the motion of outer objects).

4. *Consciousness – of our attempts to make corrective motions described above*
Our corrective motions must be voluntary/intentional to prompt us to make the central distinctions, and they must be accompanied by conscious sensations (59). I think the reasoning here is that without consciousness of our attempts to correct relative positions, there would be no consciousness of the group theoretic structure emerging in our encounters with moveable solid bodies.

5. *Homogeneity of space, or free mobility (approximate/empirical)*
Homogeneity and iteration are implicitly assumed in the ordinary geometric construction postulates; and these spatial properties are generated by the perception of (approximate) free rigid body motion. Poincaré acknowledges that there is an empirical catalyst here: the at least approximate homogeneity of space is shown by the at least approximate existence of rigid body motion – which we idealize and assume is indefinitely iterable (45). If physical space were not approximately homogeneous we would not be able to reliably distinguish between change of state and change of position; and the group structure would not emerge in experience.

These central empirical preconditions for geometry must then be joined with some a priori preconditions. According to Poincaré, we're also guided by our minds in certain crucial ways that shape the development of geometry.

1. *Group concept*
The idea of a group guides us to *be* geometrical because we apply it to displacements of rigid bodies. With it we can pursue the above distinction between state and position; and it perhaps motivates us to organize data into certain classes. The group concept, Poincaré asserts, pre-exists in our minds as an a priori concept (rather than an a priori intuition). (For example, 1902a/1952, 70; 1905a/1958, 126.)

2. *Time*
For the idea of groups of rigid motions to emerge we need to be able to comprehend our corrective motions in terms of sequences of sensations (58). And similarly to Kant, Poincaré argues that time is an a priori form of experience. Temporal ordering is imposed on us rather than chosen: "The order in which we arrange conscious phenomena does not admit of any arbitrariness. It is imposed upon us and of it we can change nothing" (1905a/1958, 26). But time is not empirical because if

it were, time would be perceived neither as infinite in extent nor continuous (or even everywhere dense). Suppose that time were the result of labeling and storing actual memories. To this Poincaré objects, "[b]ut these labels can only be finite in number. On that score, psychological time should be discontinuous. Whence comes the feeling that between any two instants there are others .... How could that be, if time were not a form preexistent in our mind?" (1905a/1958, 26, translation slightly modified). Though the measure of time requires conventions about simultaneity and duration, the nature of qualitative time is a priori imposed, and includes the awareness of the successive linear structure of time. Succession of processes is central to mathematical geometry, for geometric constructions are must typically be carried out in a definite order.

3. *Repetition*
Finally, the possibility of *repeating*, or iterating, spatial motions is also presupposed in the ordinary geometric construction postulates (64). This idea, and in particular, general or indefinite repetition, is – Poincaré argues – given a priori as the central a priori intuition underlying all of mathematics. (See, e.g., Poincaré 1902a/1952, chapter I, especially section V.)

Accepting that all of the preconditions are met, we are limited to three options for three-dimensional space: Euclidean (no curvature), Riemannian (constant positive curvature) and Lobachevskian (constant negative curvature). So logic plus intuition (indefinite iteration) plus the other a priori preconditions (psychological time and the group concept) plus the empirical preconditions (solids, motion, consciousness, etc.) yields three possible geometries. We choose Euclidean because it is the simplest model that accords well with experience (1905a/1958, 38–39).

One way experience might yield a different set of options is if no group-theoretic structure emerged from experience. That is, even if the group concept were a priori, we would not think it applied to motion if there were no approximate solid body motions. The point is that unlike arithmetic, Poincaré views geometry as depending crucially on both humans and the world having specific physical properties: we and other objects can move about while (roughly) retaining the rest of our properties (especially shape properties). For Poincaré this is an empirical precondition for the possibility of a mathematically codifiable system of length-measure.

Given the empirical preconditions on geometry, a natural thought is that geometry is empirical. However, Poincaré explicitly refutes this natural thought, arguing as follows. The subject matter of geometry is not actual physical bodies. If it were, it would be "experimental geometry", which would be refuted since physical bodies never precisely satisfy Euclidean definitions. They do not move exactly rigidly, and we cannot physically instantiate perfect circles, straight lines or right angles, for example. Yet mathematical geometry remains "rigorously true" (1902a/1952, 50), so its subject matter must be ideal objects (ideal straight lines, ideal rigid bodies, etc.). Though our experience of approximate rigid bodies provides an empirical precondition for our having the geometrical concepts (70), the subject matter of (pure) geometry is ideal.[7]

Poincaré's preconditions thus contribute to an explanation of geometry as a mathematical subject matter; they make sense of the fact that beings like us in a world like this would do geometry. And they largely define the subject matter of (traditional) geometry. As the simplest option to roughly model our sense experiences, Euclidean geometry is natural but not forced on us. (All of the options are mathematically legitimate.) Thus, he argues that experience also guides us in a second, posterior, sense – in providing criteria for choosing between the mathematical options.

Empirical Information: The Assessment of Geometry

Even though the options are severely limited by Poincaré's preconditions, there are options. So the scientist can raise the question, which geometry is the true model of space? Of course Poincaré famously ridicules this question, comparing it to that of whether the use of meters or yards is the "true" way to measure length. But he does argue that physics *guides* the more explicit choice between geometric systems via criteria such as simplicity and convenience. Whereas the empirical pre-conditions influence us in a pre-scientific sense (explaining the possibility, or maybe even likelihood, of the pure mathematical work), posterior empirical conditions include scientific evidence. Geometry begins with our bodies in the empirical world; and it returns to the empirical world after its mathematical development. At this point, Poincaré argues, convenience, or simplicity, guides the evaluation of a larger system that includes physics *and* geometry.[8]

In this context Poincaré emphasizes the intermediary status of geometric conventions. Though there cannot be a crucial experiment that determines the "true" geometry, testing guides us in our choice.

> In fine, it is our mind that furnishes a category for nature. But this category is not a bed of Procrustes into which we violently force nature, mutilating her as our needs require. We

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[7]It might be objected here that since mathematical geometry is about ideal objects, Poincaré's emphasis on the empirical preconditions for geometry – for its "genesis" – may simply result from confusing the context of discovery with the context of justification. That is, just because certain empirical conditions need to be met in order to account for the existence of geometry does not mean that, once established, the empirical preconditions are relevant to its subject matter. After all, we also need blood in our brains to do arithmetic; but arithmetic is still a bona fide a priori area of mathematics despite this precondition. The difference is that there being blood in the brain is a mere precondition; and has no bearing on what arithmetic is about. In contrast, the empirical preconditions in part *define* the subject matter of geometry: systems of rigid body motions. This context of discovery, in other words, is relevant to the subject matter of geometry in a way unlike the arithmetic case.

[8]Of course, a physical theory that is not empirically adequate would be very inconvenient!

> offer to nature a choice of beds among which we choose the couch best suited to her stature (Poincaré 1898/1996, 1011).

Nature has a stature, which testing and empirical evidence help to reveal. Empirical results guide our choice of a best "couch" for nature's "stature". Remarks like these, where Poincaré emphasizes that conventions are choices *guided by empirical evidence*, are central to my interpretation of conventions as responsive to experience, rather than merely fixed by stipulations in advance of physical testing.

Of course Poincaré also, repeatedly, emphasizes that experience does not make the choice *for* us. The reason is that the various geometric options are all consistent with experience – *provided* one is willing to modify, or add, other physical hypotheses. Chapter V of *Science and Hypothesis* is largely an argument that a geometric system cannot be *directly contradicted* by experience (1902a/1952, 75). The "Euclidean hypothesis" and the "non-Euclidean hypotheses" can both always be used to *interpret* a series of experiments (76); this is why geometric empiricism is meaningless (79).

But any part of a scientific theory can be protected if we are willing to adjust other parts. This is essentially Quine's holism. To be distinctive, conventionalism must connect some special feature of the convention in question, such as its position in the hierarchy, with the *propriety* or *impropriety* of such protection schemes. That is, to distinguish conventionalism from holism, it must be committed to the view that conventions *are*, and *should* be, more protected from refutation than the more empirical parts of science.[9]

Indeed this is Poincaré's view. His remarks about untestability, inter-translatability, and geometry's privileged place in the hierarchy all support a view of geometry as rather like a linguistic or conceptual framework, which Michael Friedman explains rather well:

> [G]eometry cannot depend on the behavior of actual bodies. For, according to the above-described hierarchy of sciences, the determination of particular physical forces presupposes the laws of motion, and the laws of motion in turn presuppose geometry itself: one must first set up a geometry before one can establish a particular theory of physical forces. We have no other choice, therefore, but to select one or another geometry on conventional grounds, which we then can use, so to speak, as a standard measure or scale for the testing and verification of properly empirical or physical theories of force (Friedman 1999, 78).

To determine what forces there are on objects, involves determining which motions are non-inertial motions; and this presupposes some geometric principles (by which

[9]As mentioned above, along these lines, Stump argues that in contrast with Quine, Poincaré's holism is not general, but limited to special cases. Poincaré rejects Newtonian absolute space, and any substantival understanding of space. So adjustments in geometry are legitimate because geometry is not describing a thing with its own properties (space); rather it is just a tool for describing the relationships of bodies. (And Poincaré comes into conflict with GR precisely here since GR is essentially a substantival theory of space.) Whereas I am connecting the degree of protection a principle gets to its position in the hierarchy, Stump is furthering an account of a deeper reason *why* something is a convention, and thus why it occupies a protected position in the hierarchy. See Stump 1989, section V.

we can decide shortest distances and the like). But if such measurements presuppose a system of geometry, then we cannot *decide* geometry *from* them.

This gives geometry a special status (which GR of course rejects). If geometry is prior to testing, and more like a linguistic framework than an empirical hypothesis, then good scientific practice means it *should* be more protected than the empirical aspects of the theory. Along these lines Poincaré argues elsewhere that science would not be fruitful if, in response to negative data, scientists routinely changed the meanings of some terms rather than the empirical content of a theory (1905a/1958, 123). This shows that Poincaré thought that (even if there is no sharp boundary) there is a workable distinction between linguistic and empirical principles. Good scientific practice, then, would seem to recommend the protection of convention along with the explicit linguistic, analytic, principles presupposed.

The protection of geometric conventions, in particular, is furthermore supported by the fact that Poincaré regarded them as more fundamental than those of mechanics – the other "home" of conventions.

> The experiments which have led us to adopt as more convenient the fundamental conventions of geometry refer to bodies which have nothing in common with those that are studied by geometry. They refer to the properties of solid bodies and to the propagation of light in a straight line. These are mechanical, optical experiments. In no way can they be regarded as geometrical experiments (1902a/1952, 136–137).

Geometry is about *ideal* objects, whereas mechanics is about *real* objects.

> On the other hand, the fundamental conventions of mechanics and the experiments which prove to us that they are convenient, certainly refer to the same objects or to analogous objects. Conventional and general principles are the natural and direct generalisations of experimental and particular principles (137).

Principles are empirical laws that have been elevated, but geometric conventions were never empirical.

> Principles are conventions and definitions in disguise. They are, however, deduced from experimental laws, and these laws have, so to speak, been erected into principles to which our mind attributes an absolute value (138).

For Poincaré, then, geometric and mechanical principles are both conventional, in that they both contribute to a testing *framework*. But geometric principles are about ideal objects, and thus they are more abstract than mechanical principles.

To summarize, there are at least three ways in which Poincaré regards geometric systems as similar to linguistic conventions: (i) a geometric system must be fixed prior to testing some physical hypotheses (geometry is relatively necessary); (ii) any of the main geometric "languages" can be substituted (inter-translated) with some systematic changes in the description of the results; and (iii) geometry is about ideal, rather than real, objects. Because geometric principles are more ideal, they closer to mathematics and more like a language than mechanical principles. They will thus naturally be more protected from refutation – on grounds consistent with good scientific practice. So, the vision is the following: all scientific conventions should be somewhat protected from empirical refutation; and geometric conventions should arguably be the most protected since they are the most ideal, or the closest to linguistic conventions. At the two extremes, the truths of pure mathematics and logic are absolutely protected and the experimental areas of science are much more open to revision.

The problem of relativity is that it shows that scientific practice violates this account, which seems to require the relatively strong protection of geometry in the light of new evidence. Can Poincaré explain what leads physics to adopt a new, empiricist approach to space? I will argue below that geometric conventionalism is not as inconsistent with GR as it may seem. Though its position in the hierarchy means geometry is *more* protected than mechanics and the experimental sciences, conventionalism dictates that applied geometry is not to be *absolutely* protected.

The Revisability of Geometry

Poincaré regarded geometric choices as revisable in the light of empirical evidence; this follows simply from the fact that all conventions are subject to empirical checks. Regarding mechanics, he remarks, "if a principle ceases to be fecund, experiment without contradicting it directly will nevertheless have condemned it" (1905a/1958, 110). A convention can thus be fruitful or not without being directly verified or contradicted. It can also "cease to be fecund", so it can be fruitful for a while, after which it is less fruitful. This speaks to a view of scientific theory as in flux, and a view of conventions as responsive to change. That is, changes in scientific evidence and its interpretation can lead to changes in conventions. Is there any reason to think that this attitude applies to mechanical conventions only?

Poincaré is unclear on this point. For example, he writes, "Which . . . . [of the Lie groups] shall we take to characterize a point in space? Experiment has guided us by showing us what choice adapts itself best to the properties of our bodies; but there its role ends" (1902a/1952, 88). This seems to imply that experience plays a pre-conditional role only, after which its "role ends". Yet he writes earlier in the same chapter about the possibility of revising geometry after an initial choice, in the light of experimental data:

> If, therefore, we were to discover negative parallaxes, or to prove that parallaxes are higher than a certain limit, we should have a choice between two conclusions: we could give up Euclidean geometry, or modify the laws of optics, and suppose that light is not rigorously propagated in a straight line (73).

Here a different picture is forwarded, whereby a geometric choice might need to be reconsidered after the fact, owing to later experimental results.[10] Thus Poincaré

[10]Of course Poincaré famously did not think we would ever make such a choice; continuing: "It is needless to add that every one would look upon this solution as the more advantageous. Euclidean geometry, therefore, has nothing to fear from fresh experiments" (1902a/1952, 73). This confidence was probably based on the assumption that the alternative would require too deep of a shift to be worth the scientific and conceptual upheaval. Indeed, relativity was a revolutionary shift, involving

makes apparently inconsistent remarks about the relationship between experience and geometry.

Admitting that there is a lack of consistency, or clarity, I nevertheless think that the more flexible interpretation is more charitable to Poincaré. As he says:

> If our experiences should be considerably different, the geometry of Euclid would no longer suffice to represent them conveniently, and we should choose a different geometry (Poincaré 1898/1996, 1011).

Geometric choices, like other conventions, can cease to be fruitful; and in this way they, like other conventions, are answerable to future scientific findings and choices.

How flexible might Poincaré have been had he lived long enough to know about GR? GR does not simply choose a different geometry from among the three geometries of constant curvature, which is what Poincaré admits as possible. Instead, it introduces a new relationship between space and physics. So we cannot merely invoke his views about choice on the basis of the two quotes above; we must also extend his views.

As we saw above, Poincaré essentially articulates the two basic interpretive options for understanding the crucial results about space regarded as supporting GR.[11] Space (empty) can be fixed as Euclidean, and we can interpret results such as parallax and planetary orbits as physical relationships involving mass, gravity, etc.; we can say, for example, that light bends owing to the gravitational force exerted on it by massive bodies. Alternatively we can adopt a new paradigm, stipulating that light travels in straight lines, or shortest paths, and accept that space is a non-Euclidean, variably curved manifold. On this second alternative what has shifted is which aspect of the theoretical framework is playing the conventional role. For example, it is not just that we have given up the stipulation of Euclidean geometry; in its place is a new stipulation about light (among many other changes of course).[12] The choice at this point depends on the criterion of overall simplicity in the light of the two scientific "packages".

The more modern, holist/empiricist understanding behind GR would I suspect have been difficult for Poincaré, for reasons I will sketch below. Still, in defense of conventionalism more generally, he could point out that GR still requires the stipulation of a convention – that light travels along shortest paths. In addition, in

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changes at many levels, including conceptual levels, and resulting in new relationships between mathematics and physics. Nevertheless Poincaré recognized the *possibility* of revising a geometric choice when faced with new data; it was not the *policy* of conventionalism to prohibit such changes. He asserted that we *would* not adopt a different geometry, not that we could not or should not.

[11]Friedman points out, interestingly, that Poincaré's conventionalism is the only option to GR that Einstein himself recognized (Friedman 2001, 111).

[12]Similarly, Friedman argues that the special theory of relativity proceeds "in perfect conformity with Poincaré's underlying philosophy in *Science and Hypothesis*, by 'elevating' an already established empirical fact into the radically new status of what Poincaré calls a 'definition in disguise'" (Friedman 2001, 111).

its favor is a gain in overall efficiency, which is what conventionalism generally endorses. Nevertheless, it must be conceded that GR does seem to clash pretty squarely with geometric conventionalism.

In addition, several more specific factors favor the view that Poincaré would have (at least initially) preferred the first option – that of stipulating that empty space is Euclidean and treating the relevant results as physical. Firstly, and most obvious, is Poincaré's consistent defense of Euclidean geometry. Secondly, Poincaré does distinguish between mathematics and physics; and he aligns (pure) geometry with the mathematical. For example, he writes that:

> experience brings us into contact only with representative space, which is a physical continuum, never with geometric space, which is a mathematical continuum. At the very most it would appear to tell us that it is convenient to give to geometric space three dimensions, so that it may have as many as representative space (1905a/1958, 69).

There are at least three "spaces" for Poincaré (possibly more if we differentiate between the "spaces" of our different senses): perceptual – which is involved in the pre-conditions for geometry; mathematical – the study of the idealized objects after the mathematical systems are developed; and physical – the "space" of our best physical theories, including the behavior of light and masses in relation to one another. (On this point see also Stump 1989.) The separation of mathematical/geometric space from the "space" of GR is consistent with his differentiation between mathematical space and physical space.

Third, it is not clear that Poincaré regarded Riemannian, variably curved, "geometry" as a *bona fide* geometry. On the one hand, his insistence on generality and the iterability of mathematical operations leads him to dismiss geometries of variable curvature as merely "analytic" (1902b/1982, 63). Distinctive of mathematics, he argues, is generality and the fact that induction applies to its processes (1902a/1952, Chapter I). For geometry to be genuinely mathematical, its constructions must be everywhere iterable, so everywhere possible. If geometry is in some sense about rigid motion, then a manifold of variable curvature, especially where the degree of curvature depends on something contingent like the distribution of matter, would not allow a thoroughly mathematical, idealized treatment. Yet Poincaré also writes favorably about Riemannian geometries, defending them as mathematically coherent (1902c/1994, 163–164). Furthermore, he admits that geometries of constant curvature rest on a hypothesis – that of rigid body motion – that "is not a self evident truth" (1902b/1982, 61). In short, he seems ambivalent.

Whether his conception of geometry includes or rules out variable curvature is unclear. I think we can surmise that he recognized Riemannian geometry as mathematical, and interesting, but as very different and more abstract than geometries of constant curvature, which are based on the further limitations discussed above (those motivated by a world satisfying certain empirical preconditions). These limitations enable key idealizations, which in turn allow constructions and synthetic proofs that we recognize as "geometric".

In any case, it remains plausible that Poincaré would have viewed GR's codification of space as more or less detaching physical "space" from (mathematical) geometry. He might, in other words, still think of mathematical, inert, space as relational, homogeneous, etc.; but physical "space", as GR conceives it, is substantival, for it has an effect on the behavior of objects in it, and it is not homogeneous. Since Poincaré rejects absolute space but accepts (at least the possibility of) the ether, his position on Euclidean geometry may thereby be salvageable along the following lines. Traditional (constant curvature) geometries are mathematical theories of space, while GR, in effect, forwards a physical theory about the ether (one that entails, in fact, that *none* of the ordinary mathematical-geometric options apply).[13] Though (mathematical) geometry may be conceptually prior to physics, and also more ideal than mechanics, we can change our decision about *which* geometric structure should be presupposed, if *any*. "If our experiences should be considerably different, the geometry of Euclid would no longer suffice to represent them conveniently, and we should choose a different geometry" (1898/1996, 1011). If experience could lead us to choose a different geometry, then neither geometry's position in the hierarchy, nor its similarity to a language, ensures it either absolute protection or scientific applicability.

Conclusion

Convention is an invention that plays a distinctive role in Poincaré's philosophy of science. In terms of how they contribute to the framework of science, conventions are not empirical. They are *presupposed* in certain empirical tests, so they are (relatively) isolated from doubt. Yet they are not pure stipulations, or analytic, since conventional choices are guided by, and modified in the light of, experience. Finally they have a different character from genuine mathematical intuitions, which provide a fixed, a priori synthetic foundation for mathematics. Conventions are thus distinct from the synthetic a posteriori (empirical), the synthetic a priori and the analytic a priori.

The importance of Poincaré's invention lies in the recognition of a new category of proposition and its centrality in scientific judgment.[14] This is more important than the special place Poincaré gives Euclidean geometry. Nevertheless, I think it's possible to accommodate some of what he says about the priority of Euclidean geometry with the use of non-Euclidean geometry in science, including the inapplicability of any geometry of constant curvature in physical theories of global space. Poincaré's insistence on Euclidean geometry is based on criteria of simplicity and convenience. But these criteria surely entail that if giving up Euclidean geometry somehow results in an overall gain in simplicity then that would be condoned by conventionalism.

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[13] Interestingly, Einstein also saw GR as a kind of ether theory. Einstein 1920, referenced in Stump 1989, 362, note 104.

[14] Indeed, as Wittgenstein argues, in *any* domain of objective, empirical judgment (See Wittgenstein 1969).

The a priori conditions on geometry – in particular the group concept, and the hypothesis of rigid body motion it encourages – might seem a lingering obstacle to a more flexible attitude towards applied geometry, or an empirical approach to physical space. However, just as the apriority of the intuitive continuum does not restrict physical theories to the continuous; so the apriority of the group concept does not mean that all possible theories of space *must* allow free mobility.[15] This, too, can be "corrected", or *overruled*, by new theories and new data, just as, Poincaré comes to admit, the new quantum theory might overrule our intuitive assumption that nature is continuous. That is, he acknowledges that reality might actually be discontinuous – despite the apriority of the intuitive continuum (1913/1963; compare p. 44 and chapter VI).[16]

As with quantum mechanics, so with relativity: the practice of science depends both on our making certain conventional choices and on our treating these choices as provisional; they are revisable in the light of further experience and better, more efficient theories. For these reasons I have urged an interpretation of Poincaré's conventionalism that de-emphasizes the rigidity of conventions and the hierarchy in which they exist, and focuses instead on their flexibility and responsiveness to new data and new theories. If this is right, then conventionalism is closely continuous with, rather than distinct from, recent work on the more flexible "relativized", or provisional, a priori.

References

DiSalle, R. 2006. *Understanding space-time*. Cambridge: Cambridge University Press.

Ehrlich, P. (ed.). 1994. *Real numbers, generalizations of the reals, and theories of continua*. Dordrecht: Kluwer.

Ewald, W. (ed.). 1996. *From Kant to Hilbert, a source book in the foundations of mathematics*, vols. I and II. Oxford: Clarendon Press.

Folina, J. 2006. Poincaré's circularity arguments for mathematical intuition. In *The Kantian legacy in nineteenth century science*, ed. M. Friedman and A. Nordmann, 275–293. Cambridge, MA: MIT Press.

Friedman, M. 1999. *Reconsidering logical positivism*. Cambridge: Cambridge University Press.

Friedman, M. 2001. *The dynamics of reason*. Stanford: CSLI Publications.

Friedman, M. 2011. Extending the dynamics of reason. *Erkenntnis* 75(3): 431–444.

Friedman, M. 2012. Reconsidering the dynamics of reason: Response to Ferrari, Mormann, Nordmann, and Uebel. *Studies in History and Philosophy of Science, Part A* 43(1): 47–53.

Friedman, M., and A. Nordmann (eds.). 2006. *The Kantian legacy in nineteenth century science*. Cambridge, MA: MIT Press.

[15]Recall, the group concept is part of an explanation of why we are geometrical beings, and why we are naturally limited to a small class of geometries.

[16]Apriority for Poincaré does not thereby guarantee applicability; this is just one way his vision seems quite different from Kant's.

Poincaré, H. 1898/1996. *On the Foundations of Geometry.* Trans. T.J. McCormack (reprint of original translation in the *Monist*). In *From Kant to Hilbert, A Source Book in the Foundations of Mathematics*, ed. W. Ewald, vol. 2. Oxford: Clarendon Press.

Poincaré, H. 1902a/1952. *Science and Hypothesis.* Trans. W.J.G. (reprint of the 1905 Walter Scott publication in English). New York: Dover.

Poincaré, H. 1902b/1982. *Science and Hypothesis.* Trans. G. Halsted (reprinted in *The Foundations of Science* (volume containing Poincaré's *Science and Hypothesis*, *The Value of Science* and *Science and Method*)). Washington, DC: University Press of America.

Poincaré, H. 1902c/1994. Review of Hilbert's *Foundations of Geometry.* Trans. E. Huntington. In *Real Numbers, Generalizations of the Reals, and Theories of Continua*, ed. P. Ehrlich. Dordrecht: Kluwer Academic.

Poincaré, H. 1905a/1958. *The Value of Science.* Trans. G. Halsted. New York: Dover.

Poincaré, H. 1905b/1996, 1906a/1996, 1906b/1996. *Mathematics and Logic I, II, and III.* Trans. G. Halsted and W. Ewald. In *From Kant to Hilbert, A Source Book in the Foundations of Mathematics*, ed. W. Ewald, vol. 2. Oxford: Clarendon Press.

Poincaré, H. 1913/1963. *Mathematics and Science: Last Essays.* Trans. J. Bolduc. New York: Dover.

Stump, D. 1989. Henri Poincaré's philosophy of science. *Studies in History and Philosophy of Science* 20(3): 335–363.

Stump, D. 2011. Arthur Pap's functional theory of the a priori. *HOPOS* 1: 273–289.

Wittgenstein, L. 1969. *On Certainty.* Trans. and ed. G.E.M. Anscombe and G.H. von Wright. Oxford: Blackwell Publishing.

The Third Way Epistemology: A Re-characterization of Poincaré's Conventionalism

María de Paz

Abstract The aim of this paper is to clarify some crucial aspects of Poincaré's philosophy of science, and especially the notion of 'convention'. This will lead to a better understanding of the differing interpretations of his views that have been proposed, and to a reassessment of the conventionalist philosophy of science 100 years after Poincaré's passing. The first section presents a short contextualization of conventionalism with the nineteenth century philosophical landscape. In the second one, we briefly expose two conflicting ways of interpreting Poincaré's conventionalism regarding natural science, that is, physics and mechanics. In the third section, the core of this paper, we analyze the different concepts of convention that are found in Poincaré's works. Finally, we offer some concluding remarks on Poincaré's views in light of present-day philosophical concerns.

A Short Sketch of Poincaré's Conventionalism Contextualized

During the first half of the nineteenth Century, metaphysical idealism was the dominant philosophical stream. It could be characterized as a philosophy that focused on natural human experience, in order to understand the ultimate reality by taking man as a spiritual being. So, it takes the 'I' as the point of departure and focuses on the subject, leaving aside the external aspects of the world. During the course of that century, the positivist philosophy of Auguste Comte also emerged. This stream stressed the primacy of facts, moved away from metaphysics, and sought its ground in the idea of science as the ideal form of knowledge. The dominance of these two streams in academic philosophy continued during the

M. de Paz (✉)
Centre for Philosophy of Science at the University of Lisbon (CFCUL), University of Lisbon, Campo Grande, C4, 4.3.22, Lisbon 1749-016, Portugal
e-mail: maria.depaz@hotmail.com

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second half of nineteenth century. Despite their differences, they dealt with some common problems and, as a result, they influenced each other, giving place to several philosophical streams that took elements from both.[1]

Positivism, with its particular view of science, attracted the attention not only of philosophers, but also of scientists who proposed epistemological interpretations of their professional activity, leading to a rapprochement between science and philosophy. As a result, in the last quarter of nineteenth century, when philosophy of science was not yet an academic subject,[2] several epistemological positions emerged that were held not only by philosophers but also by scientists. Some of these positions were neo-Kantianism, critical positivism, instrumentalism, inductivism, and conventionalism. As is well known, the last was Poincaré's position regarding geometry and physics, and the name 'conventionalism' comes after his own use of the word 'convention'.

In the history of philosophy there have been several conventionalist conceptions, mainly regarding the kinds of norm that govern a society. These means that what is valid or acceptable depends on the agreement of a determined group of people. The idea of governing a society based on agreements among individuals has its origin in the distinction between what is "given by nature" and what is created by man. In this sense, a social conventionalist conception considers society as a human product and, consequently, not natural. This kind of positions is very understandable on the basis of the "social contract" concept. However, the application of these ideas to science, especially to natural science, is not as clear as in the social context. Insofar as science is considered knowledge of nature, how is it possible that such knowledge could be the result of an agreement among individuals? Or more generally, how is it possible that knowledge of nature would be not natural, and would be a human creation?

Conventionalist positions regarding science could be understood in two main ways: on the one hand, we consider truth itself as a matter of convention; or, on the other, we consider that certain propositions, commonly taken as true, are not true, but are conventions.

The first one of these positions is closer to social contractualism, for it treats truth as a matter of stipulation by a group of people. In the case of science, this group will be the scientific community. As a result, the truth of scientific theories,

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[1]There were of course other philosophical schools at that time, such as materialism, spiritualism, and some forms of existentialism. French spiritualism was dominated by Victor Cousin, who was very influenced by Hegel's and Schelling's idealism. In this respect, we can say that every one of these streams emerged as a "reaction against" or as "adding a rider" to, these two main philosophical streams. This idea can also be found in Mandelbaum (1971, 4–5).

[2]Moritz Schlick's chair in "Philosophy of the Inductive Sciences" at Vienna University is usually regarded as the academic instauration of philosophy of science (cf. Friedman 2001, 12). However, Mach's chair on "History and Theory of Inductive Sciences," at the same University (a chair also occupied by Boltzmann), is a precedent. In France, the chairs of "General History of the Sciences" in the Collège de France (created by Comte in 1892), and the chair of "History of Philosophy regarding Sciences" at the Sorbonne (created by Gaston Milhaud in 1909) are precedents likewise.

or the truth of the elements that play a part in science, is decided by reference to the common interests of this group. This assertion answers the question of how the conventions emerge, and the more precise answer is by the decision of a scientist or of a group of scientists. Such a decision can be completely arbitrary, which leads to extreme nominalist positions in which science does not correspond to nature nor has any connection at all with it; rather, it reflects exclusively the particular needs of the scientist or of the scientific community.

This kind of conception is characterized as "nominalist", which is a kind of instrumentalist position, because of the fact that, in general, it is not committed to any ontology, and it treats definitions of scientific terms that are arbitrarily stipulated as linguistic conventions. So, it denies that a definition can state the nature of a thing, or be anything other than the explanation of a symbol. As a result, all scientific truths can be deduced from linguistic conventions regarding the meaning of words.

Nevertheless, the conception that we aim to characterize here, which actually emerges in the historical period that we outlined before, and which corresponds to Poincaré's view, follows the second way of understanding conventionalism. This philosophy aims at showing that certain propositions that are commonly taken as true are not so, but are instead conventions. This does not mean that Poincaré understood truth as conventional; it only means that not every element in science taken as true is in fact true, or even an assertion of truth, because certain central principles are based on conventions. Consequently, our scientific theories are governed by these conventional elements.

Conventionalism in this sense arose in the same context as the "back to Kant" movement. This last movement emerged firstly in philosophy, but it was quite important in science because of the critical foundation for science that it proposed, taking epistemology rather than metaphysics as the fundamental aspect of philosophy. The idea of re-thinking Kant was partly a consequence of a reaction against metaphysical idealism, which in some respects had moved away from Kant's transcendental idealism. As part of the reconsideration of the Kantian perspective, it made sense to consider whether some of the knowledge that Kant considered as synthetic a priori could have a different status, such as conventional.

Moreover, the conventionalist perspective involved a reaction against Comtian positivism, the other dominant stream at the time, by asserting the possibility that not everything in science comes directly from nature, that is, not everything is discovered from empirical observations. On the contrary, conventionalism considered the role of the scientist in the theoretical constitution of science, instead of being a simple data-collector or an event-descriptor. This meant that conventionalism inquired into the creative role of the scientist, and into the dependence of certain kinds of scientific statement on the scientist's free choice.

In sum, then, the conventionalist interpretation of science that emerged by the end of nineteenth century, and that was represented by Poincaré's thinking, pursued two central aims: to challenge the status of certain scientific "truths" in order to show that they are conventions, and to highlight the scientist's role in the constitution of these conventions and therefore in the constitution of science.

Poincaré's conventionalism has its origins in his position regarding geometry. He denied the Kantian *dictum* that geometrical axioms were synthetic a priori judgments and said that they are, instead, conventions. Poincaré held that if the Euclidean axioms were universal and necessary truths, they would be imposed on us in such a way that we could not conceive, or construct a theoretical coherent edifice on, any proposition opposed to them (Poincaré 1902, 74). This mean that there could not be non-Euclidean geometry. As there are, in fact, logically consistent non-Euclidean geometries, Poincaré asks if the axioms of geometry might be experimental truths (Poincaré 1902, 75). The answer to this question is no, just as it was regarding synthetic a priori judgments. The axioms of geometry are not experimental truths, because we can never have any experience at all of the bodies that play a part in geometry: that is, we have no experience of straight lines or of perfect spheres, or of any other ideal geometrical form. Therefore geometrical axioms are neither a priori truths nor experimental facts. Thus, he created a new epistemological category to characterize the kind of knowledge that geometrical axioms represent. Accordingly, his view was in opposition, not only to rationalistic and Kantian aprioristic interpretations of geometry, but also to an empiricist one.

Poincaré extended his position to natural science, namely mechanics and physics, and this is where the main problems of interpretation come to light. Most of these problems concern whether the notion of convention is the same in natural science as in geometry. This problem leads to the two main lines of interpretation that we will now describe.[3]

Two Ways of Understanding Poincaré's Physical Conventionalism

The first of these two lines of interpretation considers physical conventionalism as a mere extension of geometrical conventionalism. On the other side, the second one considers that physical conventionalism is an epistemological position that originated in geometrical conventionalism, but then developed quite independently.

The first kind of interpretation views conventionalism in geometry as a consequence of the development of non-Euclidean geometries. In fact, the existence of alternative geometries to the Euclid's was the starting point for Poincaré's epistemological reflections on geometry, as is shown in his 1887 paper "Sur les hypothèses fondamentales de la géométrie". But, what is the meaning of considering physical conventionalism as a mere sequel or a natural consequence of geometrical conventionalism? It means, first of all, that Poincaré's position regarding natural science does not contribute with anything new to what was already

[3]By this, we do not mean that the interpretation of geometrical conventionalism is fully unproblematic, but as we will show, the notion of convention is univocal for geometry, even if the interpretations are not.

said for geometry. A consequence of this meaning is that as geometry is not true or false, then natural science is not either. Therefore, the conventionalist position would be understood as opposed to the concepts of truth and falsity in science. Accordingly, in our scientific theories we would not have true statements about the world, but only conventional statements that do not tell anything about nature. In natural science, we would be dealing with idealizations rather than empirical objects, just as we do in geometry. So, conventionalism puts into question the kind of knowledge about the world that we have from natural science. Even if we consider that conventions in physics are limited to just a few principles, these principles, situated in the highest level of the theories, could not bear any truth-value, as is the case for geometrical conventions. If scientific theories express any truth, they would not express it at the highest level.

Such an interpretation leads to the idea that science does not advance, at least in the sense that it does not approach a true description of nature. Accordingly, there can be no epistemic progress in science, because we do not obtain any knowledge from our scientific theories. Furthermore, taken to its limit, this argument removes every element in science related to the truth, insofar as there is no question of truth in Poincaré's conception of geometry. This would lead to the understanding of Poincaré's philosophy as 'global conventionalism'. Such an interpretation of Poincaré's philosophy would lead us to consider him a nominalist, and it makes trivial the part of the decision elements that can play a role in science.

This view was held and spread by Grünbaum, although not in such an extreme form, since he did not discuss Poincaré's philosophy of science in general. Grünbaum's interpretation was presented as an argument against Reichenbach's, and it is deeply linked with the reception that Poincaré's work had in the years after his decease, especially within the Vienna Circle and its intellectual heirs.[4] Usually, the interpreters following this line have mainly paid attention to the role played by convention in geometry. Regarding the relation between conventionalism and physical theories, their attention was focused on relativity theory: on the one side, because of the role of geometry in general relativity, mainly because of the so-called 'geometrization of forces'; and, on the other side, because of the role that Poincaré attributes to relativity principle in his dynamics of the electron. As a result, in this interpretive tradition, the relevance of the analysis of other mechanical problems and concepts for the genesis of conventionalism is largely neglected.

This last point is precisely where the second line of interpretation has been focused. This second view came up with Giedymin's criticism of Grünbaum's position (Giedymin 1977), developed during the 1970s and 1980s. It does not deny that geometric conventionalism is the origin of physical conventionalism, which

[4] Grünbaum 1963a, is his most famous contribution, but he also outlines his position in subsequent papers such as Grünbaum 1963b, Grünbaum 1968, and especially Grünbaum 1978, where he discusses what he considers the most important problem in geometrical conventionalism: the so-called 'parallax argument'. See also Rougier 1920 and Reichenbach 1928. We do not want to enter into the details of this interpretation since it is more widespread than the other.

cannot be coherently contradicted, since it is widely accepted that Poincaré used the word 'convention' first for geometry and only later for physics. Instead, this interpretation asserts the epistemological independence and autonomy of physical conventionalism; that is, physical conventionalism may have originated with the use of conventions in geometry, but it was subsequently constituted as a separate doctrine.

This interpretation takes into account the relevance of the "physics of principles" program to Poincaré's thinking. That is, this line considers how the analysis and creation of theories about natural phenomena led to a certain epistemological position regarding natural science. As a result, this view shows how the use of certain fundamental mechanical concepts that have no central part in geometry, such as time, force or energy, prompted the independence of Poincaré's thinking about physics from his thinking about geometry. Prominent among present-day defenses of this conception is the work of Pulte, who showed the independence of physical conventionalism by rooting it in the nineteenth century development of mechanics.

Pulte considers that conventionalism regarding mechanics and physics was not started by Poincaré, although he was its most outstanding representative. He also denies that this position originated in the problems raised by the development of non-Euclidean geometries, even though such problems prompted the use of the word 'convention' in Poincaré's philosophy (Pulte 2000, 48). To Pulte, in fact, conventionalism does not have a single founder. On the contrary, it is a consequence of scientific practice reacting against several philosophical conceptions of science, such as traditional empiricism, rationalism and Kantian critical idealism. In this sense, it fits well with our presentation of Poincaré's conventionalism as a philosophy that, contemporaneously with neo-Kantianism, rejects metaphysical idealism and positivism. In Pulte's view, conventionalism is a philosophical position that came to light by the development of nineteenth century mathematical physics, specifically by the critical foundations of mechanics proposed by Jacobi.

In his 1847–1848 lectures on mechanics, Jacobi criticized Lagrange's analytical mechanics "for its inability to describe the behavior of real physical bodies" (Pulte 2000, 60) and, more importantly, because of the status of the first principles of mechanics. Jacobi believed that in his attempt to give an axiomatic origin to certain mechanical principles, Lagrange proposed a dogmatic view that did not provide a mathematical proof of those principles. As a result he concluded that those axioms could not have the same status as the axioms of pure mathematics. Furthermore, Jacobi reckoned that the fundamental principles of mechanics, although in need of some empirical confirmation, could not be definitively established by empirical evidence. He therefore suggested that they could be considered as conventions, because their adoption implied a certain decision made by the theoretical scientist (Jacobi 1996, 3). Thus, there is always room for choice in the search for mechanical principles (Pulte 2009, 86). Besides, as these principles do not have mathematical or empirical proofs that can provide certainty, we can only assume that they correspond to nature (Jacobi 1996, 3), which implies a decision about the use of these principles, made on the scientist's part. Hence, Pulte establishes the independence of physical conventionalism from Jacobi's conception, pointing out that, exactly as

Poincaré, Jacobi introduced an epistemological category that does not depend only on experience and that cannot be identified with the Kantian a priori principles.

Giedymin also argues that the origins of the conventionalist philosophy of physics can be traced back into the beginnings of nineteenth Century. He finds its roots in the scientific conception named 'the physics of principles,' anticipated by Lagrange and Fourier, among others, but especially by Hamilton (Giedymin 1982, 44). According to Giedymin, the physics of principles aims at subsuming several experimental facts or several empirical laws under principles formulated in an abstract mathematical language which expresses a common structure to different scientific theories, and which reveals the epistemic content that we can obtain from nature. This is a kind of method to extract what is common among rival theories with the aim of avoiding theoretical controversy, or allowing the free choice of the theoretical explanation of those principles. As a result, the theoretical interpretation of the principles is conventional. Furthermore, the principles themselves are conventional because of our decision to take them as condensers of empirical laws. The conventional part can be also appreciated in the language in which they are expressed, that is, mathematical language, which shows that our mind has an active role in the production of knowledge.

The idea of a 'third way epistemology' is taken from Pulte, who addresses it to clarify the status of those physical principles, called 'conventions' by Poincaré, which are "neither inductive generalizations nor are they synthetic a priori propositions imposed by reason" (Pulte 2000, 51). In this sense, Poincaré's conventionalism could be viewed as a middle path between empiricism and rationalism, taking elements from both. But this would not be in the Kantian way, but, rather, an alternative epistemology in which the role of decision and choice, in a pragmatic sense, is essential to the development of knowledge. Accordingly, a convention, as a decisional element for the scientist, would be the third way between an empirical and an a priori judgment.

The outstanding problem of the controversy between these two interpretations of Poincaré's philosophy is precisely the lack of clarity around the notion of 'convention'. But what if this is not a univocal word? What if convention has different meanings in Poincaré's philosophy of science? Then we could state that the polysemy of convention would amount to a polysemy of interpretations that will be rightly founded, depending on the emphasis that one would ascribe to one meaning of convention. A clarification of this notion is our next step. We aim to show that 'convention' is used in several different senses and contexts in Poincaré's work.

Different Senses of the Word 'Convention' in Poincaré

We will divide this part in two: first, the conventions explicitly asserted by Poincaré as such; and second, those elements which can be interpreted as conventions but that are not explicitly asserted by Poincaré as such.

Explicit Conventions

The explicit conventions include: the axioms of geometry; measurement conventions and coordinate systems; the linguistic conventions; disguised definitions; and the principles of mechanics (and of physics generally).

Axioms or Principles of Geometry

This is the most common sense of understanding Poincaré's conventions. None of his commentators misses this sense. There are many texts in which the axioms of geometry are declared as conventions by Poincaré. For example, in *Science and Hypothesis* he states: "*The geometrical axioms are therefore neither synthetic a priori intuitions nor experimental facts*. They are conventions" (Poincaré 1902, 75).

Poincaré dismissed the possibility that the Euclidean axioms are synthetic a priori propositions because, if they were so, they would be imposed on us in such a way that we could not conceive any non-Euclidean geometry. He also dismissed the idea that the axioms are experimental facts, because we cannot have any experience of ideal geometrical objects. Axioms of geometry are not experimental facts also because geometry is not a science submitted to constant revision: it is an exact science (Poincaré 1902, 75), and empirical statements can never be exactly true. He also asserted the freedom to choose among different conventions, which is guided by experience but not determined by it. From this he inferred that the question about the truth in geometry is non-sensical. By stressing the role of decision in the application of geometry, Poincaré, in effect, constituted conventions as a new epistemological category. Thus, the axioms of geometry exemplify his "third way" epistemology.

Measurement Conventions and Coordinate Systems

We have analyzed these two kinds of conventions together, because the determinations that Poincaré pointed out for one kind are always valid for the other. On the one side are those conventions that define systems of measurement, such as the metric system, the Imperial and the US customary units and, of course, the natural units. On the other side are systems constructed to determine positions in space, such as Cartesian coordinates, polar coordinates, cylindrical and spherical coordinates and so on, as well as conventions for the measurement of time. Regarding the latter, Poincaré distinguished time as it is subjectively given to us, or psychological time, from time as defined within an objective system of measurement, or physical time (Poincaré 1905, 42). It is this last one which concerns us.

Poincaré (1905, 43) asks, what is the meaning of saying that the time between midday and one in the afternoon is equal to the time between two and three in the afternoon? This assertion has no meaning by itself: it can only have the meaning that we are able to define for it. That is, it is a matter of convention. In fact, we can understand it as the claim that the minute hand of the clock sweeps out the same *space* (the whole disc of the clock) between midday and one in the afternoon, as between two and three in the afternoon. That is, to measure time, what we do is to spatialize it: we agree that covering the same space at a constant velocity amounts to the same time interval, and this is a convention. As Poincaré says: "There is no way of measuring time more true than another; that which is generally adopted is only more convenient" (Poincaré 1905, 46). So, measurement conventions are not experimental truths nor they are a priori truths, but they are conventions, and thus representative, like geometrical conventions, of the third way epistemology.

In fact, these conventions are explicitly considered as the same kind of convention as geometrical axioms, since, when Poincaré asks about the truth of Euclidean geometry, he also says: "We might as well ask if the metric system is true, and if the old weights and measures are false; if Cartesian coordinates are true and polar coordinates false" (Poincaré 1902, 76). And the conclusion that applies to geometry is the same for the measurement systems: they are not true or false, but only more or less convenient. However, we must emphasize a difference between measurement conventions or coordinate systems and geometrical axioms. Poincaré says that the choice of a geometry is guided by experience, but is the choice of the metric system also guided by experience? Or the choice of a coordinate system? This last is usually chosen by pragmatic considerations, such as coordinate systems useful for graphing particular functions. We also know that the choice of the metric system is guided by simplicity, which is one of the features that Poincaré always stresses for the choice of a particular convention. But we have no indication that such choices are influenced by experimental reasons, whereas for Euclidean geometry, we have the experimental evidence that "it sufficiently agrees with the properties of natural solids" (Poincaré 1902, 76). This is not to state that the choice among measurement systems is arbitrary (it is not, because it is guided by simplicity), or that it is an arbitrary convention, but just to show that the reasons for this choice are not empirical. As a result, it is not exactly the same as the choice among possible geometries. Experience indeed plays a role in the choice of a geometry, for Poincaré; it is the 'occasion' of its creation, by providing us with the knowledge of solid bodies (Heinzmann 2001b, 458). And it also guides us in the choice of the more convenient geometry. This is not the case for measurement conventions, whether regarding space or time measurements. As a result, we can state a difference between these two kinds of conventions, even if it is a subtle difference. Nevertheless, both of them belong to this 'epistemological' category.

Linguistic Conventions

This is the most common kind of convention. They are usually referred to as 'conventions of language' and they are constituted by an arbitrary agreement among the community of speakers. At first sight, it would seem trivial to point out this kind of convention in Poincaré's philosophy, and perhaps that is why they are not pointed out by most of his commentators. However, we think that it is not quite trivial. From our point of view, these are important conventions because science is expressed in language, in a specific language created for it. As a result, this kind of convention is present in every science. By this, I mean that they are present in mathematics and in physics, as is shown by these words from *The Value of Science*:

> If, therefore, during an eclipse, it is asked: Is it growing dark? Everyone will answer yes. Doubtless those speaking a language where bright was called dark, and dark bright, would answer no. But of what importance is that? (1905, 226).
>
> In the same way, in mathematics, when I have laid down the definitions and the postulates, which are conventions, a theorem henceforth can only be true or false. But to answer the question: Is this theorem true? It is no longer to the witness of my senses that I shall have recourse, but to reasoning (Poincaré 1905, 158).

So, linguistic conventions are relevant to science because the constitution of scientific language is essential to express, for example, scientific facts. Then, these conventions must be established prior to the expression of scientific facts. They are at the first levels of science, since language is the way in which science is stated. They are close to measurement conventions and coordinate systems, but they are chosen as an arbitrary agreement by the community of speakers, thus, they do not have simplicity as a criterion of choice. So, the main difference with the others is that they are agreed by a group in an arbitrary way, whereas a coordinate system could be chosen by an individual researcher guided by simplicity. However, they still belong to the new category, but understood in a wide sense. Language is used to classify the facts, not only to express scientific assertions; asserting a fact in scientific language places it within a determined category of facts, that is, in a specific classification, and every classification assumes a certain convention: "Facts are classed in categories, and if I am asked whether the fact that I ascertained belongs or does not belong in such a category, I shall not hesitate.

> Doubtless this classification is sufficiently arbitrary to leave a large part to man's freedom or caprice. In a word, this classification is a convention" (Poincaré 1905, 158).

Disguised Definitions

From his first discussion of conventions, Poincaré associated them with disguised definitions, as is shown in the Introduction to *Science and Hypothesis*, where he discusses the different kinds of hypothesis: "others are hypotheses only in appearance, and reduce to definitions or to conventions in disguise" (Poincaré 1902, 24). These definitions have sometimes been interpreted as 'implicit definitions', but we want to show that this interpretation is wrong. A disguised definition is not necessarily an implicit definition. It is an explicit definition functioning as something different, for example, as an axiom. Poincaré says: "*the axioms of geometry* (I do not speak of those of arithmetic) *are only definitions in disguise*" (Poincaré 1902, 76). This means, as Heinzmann says, that a disguised definition is opposed to an ordinary definition because it is not a simple description of the things, but it is something else (Heinzmann 2001a, 4). In the case of the axioms of geometry it means that disguised definitions play an essential role, because depending on what conventions or definitions we choose, we will have an Euclidean, Riemannian or Lobatchevskian geometry. So, on the one hand, geometrical conventions are linguistic definitions, in the sense that definitions are part of a language; we need the semantic part to constitute a language, and definitions have a semantic function. Geometries could be interpreted as languages, and as such, they are similar to the linguistic definitions that we have described above, because they are conventions regarding the meaning of words, for example, the definition of a straight line is a convention. On the other hand, geometrical conventions are different from the trivial linguistic definitions because they have different functions; linguistic definitions have no other specific function, such as axioms or postulates. Therefore, regarding geometry, definitions in disguise can be identified with the first kind of conventions that we have described, because they play the role of first principles.

Some conventions regarding time, such as succession and synchrony, are also definitions, because physical time requires definitions (Poincaré 1905, 46). Regarding succession, Poincaré states that when we pay attention to very distant events, such as the observation of a very distant star and something happening on earth, it is a question of convention to establish which one of the events happened before or after the other. And it is exactly the same for simultaneity. We cannot immediately determine the simultaneity of distant events because if we use, for example, a method of cross-signals to establish the simultaneity of two events, we have to take into account the travel time of the signal itself. We do not have the intuition of the equality of two durations, and we can only decide the simultaneity of two events by definition. As a result, we need some rules to measure time, and we use these rules to establish a convention that allows us to determine the synchrony of distant events. We launch this convention on the basis of the simplicity and convenience of physical laws:

> The simultaneity of two events or the order of their succession, the equality of two durations, are to be so defined that the enunciation of natural laws may be as simple as possible (Poincaré 1905, 54).

Conventions as definitions in disguise appear not only present in geometry and in the measure of time, but also in physics. For example, when we say that "force is the product of the mass and the acceleration" (Poincaré 1902, 120), Poincaré asserts that this is not an experimental law but only a definition. Because this definition corresponds to the second law of Newtonian mechanics, it is a disguised definition, because it functions not as a definition, but as a law of mechanics.

Understanding this kind of definition is important to understanding which parts of the science are conventional and which are empirical, because some difficulties that we find in mechanics are "due to the fact that treatises on mechanics do not clearly distinguish between what is experiment, what is mathematical reasoning, what is convention, and what is hypothesis" (Poincaré 1902, 111).

The role of definitions in the establishment of geometrical axioms, rules for time-measurement, or mechanical principles makes clear that the latter are neither experimental facts or a priori propositions. These definitions, or conventions, are established by scientists in a way that stresses their creative role. This is another aspect of conventions that makes them a distinctive part of Poincaré's third way epistemology.

Principles of Mechanics (and of Physics)

Poincaré characterizes the principles of mechanics as conventions in the Introduction of *Science and hypothesis*: "In mechanics we shall be led to analogous conclusions, and we shall see that the principles of this science, although more directly based on experience, still share the conventional character of the geometrical postulates" (Poincaré 1902, 26). Nonetheless, Poincaré himself distinguished between geometrical axioms as conventions and principles of mechanics as conventions: the latter are more directly based on experience. If we take this assertion seriously, we cannot agree with the first line of interpretation, which views Poincaré's conventionalism in physics as just an extension of his conventionalism regarding geometry. The link with empirical facts is what introduces an element of verification in science, and as a result an element of truth. Consequently, we cannot agree with an interpretation that removes every element of truth from Poincaré's philosophy of science or, more precisely, from his philosophy of physics.

Now, we have to distinguish between at least two kinds of convention regarding the principles of mechanics. There are those like the law of force, which are definitions in disguise, as we have just shown; and there are others which are extreme idealizations of experimental conditions such as the law of inertia. In this case, we suppose that there could really be a free particle or a free body in motion, in order to verify that "A body under the action of no force can only move uniformly in a straight line" (Poincaré 1902, 112). Once again Poincaré rejects the idea that this could be a synthetic a priori proposition, because if this were the case, the Greeks would not have thought that motion ceases with the cause of motion. But we also know that we cannot have experience of a body not subject to the action of any force. A body on earth is always under the influence of gravitational force, so there is no such thing as a free body, but we act on the assumption that there could be. That is, by convention, we decide to abstract from the action of all forces in order to assert the law of inertia. As a result, it is not an a priori principle or an empirical law, it is a convention. Here again we have a representative of Poincaré's new epistemological category, characteristic of the "third way" epistemology.

Implicit Conventions

Now, we will consider some conventions that we can find in Poincaré's philosophy but that are not explicitly identified as such. We will also show why we think that they can, nonetheless, be interpreted as conventions. These fall into two categories: indifferent hypotheses and natural hypotheses. The purpose of this section is not to compound the problem of the interpretation of the word 'convention' in Poincaré's philosophy by applying it to even more kinds of principle. On the contrary, we want to show its deeper internal coherence, by using this concept to clarify more elements of his philosophy.

Indifferent Hypotheses

Poincaré characterizes this kind of hypothesis twice. First, in the Introduction to *Science and Hypothesis*, he distinguishes different kinds of hypothesis, and second, in Chapter IX, he presents another typology of hypotheses, saying, "There is a second category of hypotheses which I shall qualify as indifferent. In most questions the analyst assumes, at the beginning of his calculations, either that matter is continuous, or the reverse, that it is formed of atoms" (Poincaré 1902, 166). These hypotheses are again characterized in chapter X of *Science and Hypotheses* as follows: "Hypotheses of this kind have therefore only a metaphorical sense. [. . .] They may be useful to give satisfaction to the mind, and they will do no harm as long as they are only indifferent hypotheses" (Poincaré 1902, 176). They are useful in the sense that they have an explanatory role for us, but they cannot be considered either true or false. Accordingly, the utility of these hypotheses is only practical: they help us to save intellectual effort, because they provide convenient images of the theories that they are associated with. Thus, they consolidate scientific concepts by means of their simplicity.

Indifferent hypothesis can be interpreted as "conventions freely invented by the mind," as suggested by Giedymin (1982, VIII) and other commentators (Heinzmann 2009, 166; Uebel 1998–1999, 79). In this sense, "convention" could be understood as an arbitrary agreement, regarding, for example, one mechanical model as more convenient than another, or the atomic hypothesis as more convenient than the hypothesis of continuous matter. Interpreting these indifferent hypotheses as conventions arises from accepting that there is no empirical determination of them; they are completely created by the scientist. Their use depends on the scientist's decision to adopt them because of their simplicity, not because of their relationship with the world. Thus, they are not empirical truths; nor are they a priori truths, because they are neither self-evident nor universally valid. In fact, they are not truths at all.

However, this kind of convention is different from every other kind of convention that we have identified before. From my point of view, that is the reason why Poincaré does not explicitly characterize them as conventions. I think that he wanted to distinguish them from other kinds of convention that are epistemologically relevant. By 'epistemologically relevant' I mean conventions that make a difference in physical theory: if we change those conventions, we get a different theory, and that is not the case with indifferent hypothesis. Indifferent hypotheses are called indifferent, precisely because their adoption does not modify essential points in the structure of a theory or in its epistemological value. As a result, the choice of an indifferent hypothesis depends on its convenience and on its heuristic value for the explanation of the theory, and not on its epistemological merits. So, while such hypotheses are not chosen in a completely arbitrary way, they are indifferent regarding their epistemological basis. In any case, since these hypotheses make statements about the underlying ontology of a theory, they concern metaphysics rather than physics. Their use in physics is heuristic or methodological because their truth or their epistemological status is not relevant to physics, but is a question outside of science.

Evidently such hypotheses are restricted to physics; they do not appear in geometry. This establishes another difference between geometrical and physical conventionalism, since this kind of hypothesis is a feature of the latter but not the former. So, we can say that in Poincaré's theory of natural science, there are some conventions with a purely heuristic function, which would have no place in his epistemological analysis of geometry.

These conventions emerge from the need to simplify our theories. They are sustained by the practical convenience of the theoretical explanations that we can obtain by using them. The criterion of choice among alternative conventions is precisely their heuristic value, the methodological simplicity that they provide to our calculations. As a result, although they are freely invented by the scientist, they are not adopted arbitrarily, because simplicity is a criterion of choice. Interpreting indifferent hypotheses as conventions means that insofar as they are methodological tools, the heuristic tools used to construct our theories depend on the scientist in two senses: first, they are products of the scientist's creativity; second, it is the scientist who decides to use them, based on an individual judgment of their simplicity. So, in the case of indifferent hypotheses, the conventionalist position stresses once again the creative role of the scientist.

Natural Hypotheses

Natural hypotheses are also defined in the same typology of chapter IX of *Science and hypothesis*:

> First of all, there are those [hypotheses] which are quite natural and necessary. It is difficult not to suppose that the influence of very distant bodies is quite negligible, that small movements obey a linear law, and that effect is a continuous function of its cause. I will say as much for the conditions imposed by symmetry. All these hypotheses affirm, so to speak, the common basis of all the theories of mathematical physics. They are the last that should be abandoned (Poincaré 1902, 166).

If we consider the examples provided by Poincaré himself, these hypotheses are taken from experience. But they have a different purpose than empirical generalizations. They are practical rules that help us to constitute our theories of mathematical physics. They are taken from experience, but our aim is not to verify them; rather, we use them to constitute empirical laws.

In this sense, as Walter argues, indifferent hypotheses are practical rules that are not falsifiable, given their role within our theoretical constructions (Walter 2010, 133). That is, they cannot be refuted because of the use that we make of them, by deciding to treat them as valid in order to constitute a theory. This means that there are some elements in the process of the formation of theories that have an empirical origin, but are not falsifable by experience for some reason. From our perspective, this reason is that the scientist decides to not expose them to falsification, but to treat them as exactly true. Thus it is by decision that these theoretical elements are immune to empirical revision.

These hypotheses have a regulative role for scientific practice, because they are rules that guide our procedures for constructing theories. But they also have a constitutive role, because they help us in organizing our empirical knowledge, in putting phenomena in the form of equations. That is why these hypotheses form the common ground of our theories of mathematical physics and that is why they are the last that should be abandoned. Without these theoretical assumptions we will not be able to write differential equations and, consequently, we will not have mathematical physics.

From our point of view, these hypotheses can be interpreted as conventions of a certain kind: firstly, because our use of them is based on the decision to treat them as unfalsifiable. That is exactly what happens with conventions in natural science, especially with conventions such as mechanical principles (Poincaré 1902, 128). They are inspired by experimental laws and transformed into conventions by the use that we make of them, guided by convenience and simplicity. It is we who grant them their conventional status. So, if we stress the role of decision in science, we can identify natural hypothesis as the last kind of conventions. Thus they are neither empirical truths nor self-evident a priori truths.

We can identify these natural hypotheses with the kind of convention described by Giedymin as "elements which though literally false are useful for the attainment of certain cognitive aims" (Giedymin 1991, 5). This means that even if we could prove that these assertions are not strictly true, because they are useful to make generalizations, we decide to use them without submitting them to any empirical tests. As a result, we decide to give them a status which is not true or false, but conventional. So, they fit well with Jacobi's idea that scientist has to assume that there are some principles that correspond to nature, in order to apply them because even if they have an experimental ground, empirical verification does not assure their universality (Jacobi 1996). Thus, conventions of this kind emerge from experience, but they are not falsifiable by it, because of their status as practical rules. They are sustained by the scientist's own determination to obtain a systematic description of certain phenomena.

However, in most of the cases in which we use natural hypotheses, we do not choose among two or more alternative conventions on the basis of their convenience, as we do with indifferent hypotheses. By this we mean that a convention in this sense does not always imply a choice among empirically equivalent theories, as in the case of mechanical models. Sometimes, a convention implies a process of decision not among equivalent alternatives, but between using or not using a certain principle that can be useful for the formation of laws, as in the case for natural hypotheses. The idea that convention does not necessarily imply a choice among two equivalent principles or hypotheses is illustrated by the principle of inertia, which is a convention in the fifth sense stated by us. We do not have an alternative equivalent choice to this principle. We need it as a fundamental principle in physics.

So, a natural hypothesis is motivated by practical reasons, and accepted without consideration of its truth or falsity. Such hypotheses function as pre-conditions that guide the scientist in the selection of facts or scientific laws, and so they are regulative. That is why we can distinguish them from indifferent hypotheses, because the decision to adopt them, and the use that we make of them, is not equivalent. They are epistemologically relevant because their use determines the kind of knowledge that we will obtain. In this sense, they are constitutive.

By asserting that they are constitutive, we agree with Friedman's view that conventions are 'presuppositions' (Friedman 2001, 71) or pre-conditions, as we said above, for the formation of empirical laws. Thus, the kind of necessity for these conventions is not absolute or universal; they are necessary for our science. This means that without these conventions, or with different ones, science would not be the same, either in its methodology or in its results. The very constitution of science would be altered.

This is the last kind of convention that further supports our claim of the polysemy of this word in Poincaré's philosophy. At the same time, we have seen that convention is present in every level of scientific practice, from the most basic hypotheses used to link experimental facts, to the more general and abstract hypotheses that enable us to construct theoretical laws.

Concluding Remarks

In conclusion, we want to note which kinds of convention are especially relevant to geometry, and which ones to physics. For geometry, we have linguistic conventions (3) regarding the meaning of terms, the axioms of geometry (1) in the role of disguised definitions (4), and conventions for measurement and coordinate systems (2). For mechanics and physics we also have linguistic conventions (3), because they are needed to state scientific facts. But for mechanics we have as well the axioms of geometry (1) and the measurement conventions (2), because as Friedman asserts, "we would have no laws of motion if we did not presuppose spatial geometry" (Friedman 1999, 76). So mechanics needs the prior constitution of geometry. In addition, mechanical principles (5) may be conventions in the two forms specified, that is as disguised definitions (4), and as extreme idealizations of experimental conditions. But, for the construction of physical and mechanical theories, we also make assertions regarding their underlying ontology, and here we find the conventions known as indifferent hypotheses (6). Finally, to constitute generalizations and other empirical laws, we need principles such as the principle of physical induction or the idea that the influence of very distant bodies is quite negligible. These are natural hypotheses, and their use is another kind of conventional choice (7).

Now, we can understand that if we pay only attention to conventions as axioms of geometry, which are neither true nor false, we could be lead easily to a nominalist interpretation of Poincaré's philosophy by stressing particular elements, such as the idea of translation or the idea that conventionalism depends only on the choice of a physical geometry.

But we have shown that convention is a key concept in Poincaré's third way epistemology. It plays this role, first, because it is the category that defines the middle path between rationalism and empiricism; taken as a pluralistic concept, convention allows us to subsume several kinds of scientific statement that cannot be located as purely rational or purely empirical. Second, it introduces a novel appreciation of the role of pragmatic decision, guided by simplicity and convenience.

In order to clarify these ideas, we want to consider some questions put forward by Alan Richardson[5]: what is the point of distinguishing the different uses of convention in Poincaré's philosophy? Is it just to clarify the texts? Or is it a kind of defense in order to give support to the pluralism by the assertion of the polysemy of convention? Or is it just to show that Poincaré did not know at all what he was talking about, as Margaret Masterman argued in the case of Thomas Kuhn (Masterman 1970)?

Firstly, we agree that these distinctions do clarify Poincaré's texts and lead to a better understanding of Poincaré's philosophy. More than this, however, clarifying the meaning of convention in natural science also illuminates the role of creation in Poincaré's physical conventionalism. That is, it helps to demarcate what is given from what is created, or the observational content from the scientist's voluntary decisions to construct a convenient systematic description. Explicating the concept of convention also illuminates the distinction between two senses of conventionalism in Poincaré's philosophy, one for geometry and another for mechanics and physics, because this latter, as Pulte suggested contains different elements than the former, such as natural and indifferent hypotheses (cf. Pulte 2000, 54). So, we think that the second line of interpretation is more pertinent than the first one. But we have taken a step beyond Pulte's ideas, by showing the existence and importance of conventions in different levels of scientific practice and not only in mechanical principles.

Secondly, the clarification of these senses of convention is also a defense of Poincaré's philosophy: by clarifying this concept, we find that the polysemy of convention reveals that the solutions proposed by Poincaré, for particular scientific problems, are solutions specific to those problems, not necessarily valid in every moment of the development of a science. This means that the scientist, or better, the scientist-philosopher, has to re-think the principles and concepts on which theories are constructed. And, from our point of view, this fits well with the view of scientific practice as a human activity that is constantly changing. Besides, the plurality of meanings is also coherent with the conventionalist principle of "theoretical

[5]Richardson asked me this question after the symposium on Poincaré presented at the HOPOS conference in 2012.

tolerance", which remarks the scientist's freedom "to work with any interesting theoretical interpretation of experimentally adequate principles" (Giedymin 1982, 190). That is, as long as the principles fit well with the experimental evidence, there could be many theoretical explanations behind them, and this is consistent with the conventionalist viewpoint.

This point was clearly stated by Poincaré: "If therefore a phenomenon allows of a complete mechanical explanation, it allows of an unlimited number of others, which will equally take into account all the particulars revealed by experiment" (Poincaré 1902, 224).

Finally, we certainly do not want to maintain that Poincaré did not know what he was talking about. It is true that there are some inaccuracies or ambiguities in his reflections, regarding, for example, the criteria that guide us in the choice of a geometry as compared with the criteria that guide us in the choice of a measurement system. But in those cases, Poincaré tended to use metaphors and analogies in order to clarify his thoughts and to attain better explanations. And we have also to consider that he was not trying to construct a rigorous philosophy, but only reflecting on the way science is practiced, as a practicing scientist.

As a result, from our point of view, a complete and coherent interpretation of Poincaré's conventionalism must take into account this plurality of meanings of convention, and show that it makes sense in light of the wide range of problems studied by Poincaré from a philosophical perspective. It also makes sense in light of his opinion that science is always changing, and is never definitively constituted. Accordingly, Poincaré's philosophy of science is a philosophy constructed to point out provisional solutions to specific scientific problems, rather than as a system valid once and forever. For that reason, Poincaré's conventionalism is a philosophy of science that is continually rewritten in the course of science.

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References

Friedman, M. 1999. *Reconsidering logical positivism*. Cambridge: Cambridge University Press.

Friedman, M. 2001. *Dynamics of reason: The 1999 Kant lectures at Stanford University*. Chicago: University of Chicago Press.

Giedymin, J. 1977. On the origin and significance of Poincaré's conventionalism. *Studies in History and Philosophy of Science* 8(4): 271–301.

Giedymin, J. 1982. *Science and convention: Essays on Henri Poincaré's philosophy of science and the conventionalist tradition*. Oxford: Pergamon Press.

Giedymin, J. 1991. Geometrical and physical conventionalism of Henri Poincaré in epistemological formulation. *Studies in History and Philosophy of Science* 22(1): 1–22.

Grünbaum, A. 1963a. *Philosophical problems of space and time*. New York: Knopf (Reed. 1973, Reidel, Dordrecht).

Grünbaum, A. 1963b. Carnap's views on the foundations of geometry. In *The philosophy of Rudolf Carnap*, ed. P. Schilpp, 599–684. Chicago: La Salle, Open Court.

Grünbaum, A. 1968. *Geometry and chronometry in philosophical perspective*. Minneapolis: University of Minnesota Press.

Grünbaum, A. 1978. Poincaré's thesis that any and all stellar parallax findings are compatible with the euclideanism of the pertinent astronomical 3-space. *Studies in History and Philosophy of Science* 9(4): 313–318.

Heinzmann, G. 2001a. *L'occasionalisme de Poincaré: l'élément unificateur de sa philosophie des sciences*. Aix-en-Provence: CEPERC.

Heinzmann, G. 2001b. The foundations of geometry and the concept of motion: Helmholtz and Poincaré. *Science in Context* 14: 457–470.

Heinzmann, G. 2009. Hypotheses and conventions: on the philosophical and scientific motivations of Poincaré's pragmatic occasionalism. In *The significance of the hypothetical in the natural sciences*, ed. M. Heidelberger and G. Schiemann, 169–192. Berlin: Walter de Gruyter.

Jacobi, C.G. 1996. *Vorlesungen über analytishe Mechanik. Berlin 1847/48*, ed. H. Pulte. Braunschweig, Wiesbaden: F. Vieweg & Sohn.

Mandelbaum, M. 1971. *History, man, and reason: A study in nineteenth century thought*. Baltimore/London: John Hopkins Press.

Masterman, M. 1970. The nature of a paradigm. In *Criticism and the growth of knowledge*, ed. I. Lakatos and A. Musgrave, 59–89. Cambridge: Cambridge University Press.

Poincaré, H. 1902. *La Science et l'Hypothèse*. Paris: Flammarion (Reed. 1968).

Poincaré, H. 1905. *La valeur de la Science*. Paris: Flammarion (Reed. 1970).

Pulte, H. 2000. Beyond the edge of certainty: Reflections on the rise of physical conventionalism. *Philosophia Scientiae* 4(1): 47–68.

Pulte, H. 2009. From axioms to conventions and hypotheses: The foundations of mechanics and the roots of Carl Neumann's 'principles of the Galilean-Newtonian theory'. In *The significance of the hypothetical in the natural sciences*, ed. M. Heidelberger and G. Schiemann, 77–98. Berlin: de Gruyter.

Reichenbach, H. 1928. *Philosophie der Raum-Zeit-Lehre*. Berlin: Walter de Gruyter.

Rougier, L. 1920. *La philosophie géométrique de Henri Poincaré*. Paris: Alcan.

Uebel, T.E. 1998–1999. Fact, hypothesis and convention in Poincaré and Duhem. *Philosophia Scientiae* 3(2): 75–94.

Walter, S. 2010. L'hypothèse naturelle, ou quatre jours dans la vie de Gerhard Heinzmann. In *Construction. Festschrift for Gerhard Heinzmann*, ed. P.E. Bour, M. Rebuschi, and L. Rollet, 129–135. London: College Publications.

Poincaré, Indifferent Hypotheses and Metaphysics

Antonio A.P. Videira

> *. . . the denial of all metaphysics is still metaphysical, and precisely this is what I call modern metaphysics*
>
> (Poincaré [1904, 217], apud During [2001, 87–88])

Abstract The objective of this paper is twofold. First, Poincaré's ideas regarding the role of indifferent hypotheses in physics are described, and the relationship between this particular type of hypothesis and metaphysics is also analyzed. By formulating a relationship between indifferent hypotheses and metaphysics, the author will seek to determine this concept of metaphysics – albeit in an obscure fashion – in the thinking of the French mathematician. This relationship was not presented by Poincaré himself. It is described here in order to suggest that the failure of the French *savant* in developing a coherent epistemology for science is at least partially due to his reluctance to accept that indifferent hypotheses are a constituent part of scientific practice.

Introduction

In this article, I intend to resume a fascinating and controversial topic in the history of science and in the history of the philosophy of science: the relationship between science and metaphysics from the perspective of the scientist. Of course, my goal is not to try to understand such a relationship throughout the whole of its history. That would be an overly ambitious claim, as well as being quite impractical to carry

A.A.P. Videira (✉)
Department of Philosophy, Universidade do Estado de Rio de Janeiro, CNPq,
Rua Machado de Assis 17/101, Flamengo, 22220-060 Rio de Janeiro, Brazil
e-mail: guto@cbpf.br

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out. Moreover, it is certain that over time this relationship has undergone important changes, which have been positive at some moments whilst having been negative at other times. That is, at some (a few) times, scientists have given metaphysics relative importance, while at other times (most often), they have held it in contempt. Even indifference, which is almost always fueled by scientists and not by philosophers, has been present. In the scope of this text, I am interested in the interaction between science and metaphysics as expressed by the considerations developed by the physicist, mathematician and philosopher Henri Poincaré regarding the controversial role played by (according to him) the so-called indifferent hypotheses in physics.

In general, it is considered that metaphysics gradually moved away from the natural sciences during the nineteenth century (Jungnickel and McCormmach 1990). Over the years, mainly as of the 1830s and the rise of positivism, a doctrine that was developed explicitly for the purpose of combating metaphysics, the latter was perceived as not contributing to the development of science and society. Instead, metaphysics would be considered to constitute an obstacle to scientific progress and, as such, should definitely be eradicated. Thus, at the end of the eighteen hundreds, it was not common to find men of science who were interested in, nor concerned with, issues such as the origin of the world or the destiny of human beings. In retrospect, it is now known that this task – i.e., answering such questions – has not been fulfilled nor could it be. Despite the public contempt for metaphysics, little by little, scientists began to be forced to reflect on the relationship between science and metaphysics. In part, this obligation was triggered by the need to describe events that were not visible to the naked eye. Starting out with thermodynamics, and later because of electromagnetism and matters relating to radiation, scientists were led to construct models that made use of causes which are not reducible to structures comparable to human scale. The use of molecules, atoms and electric current in thermodynamics and electromagnetism are examples of such entities, which lead to discussions about what should actually be considered to be an object with a right to exist in the real world. Do these objects (atoms or electric currents) actually exist or are they mere fiction? (Videira 1997).

Respecting an old philosophical tradition, every question involving the issue of existence was routinely classified as belonging to the domain of metaphysics. In the late nineteenth century and the beginning of the next one, metaphysics still stubbornly remained “close” to science, as some scientists and even Poincaré himself, albeit grudgingly, had to take a stand about the reasons for keeping metaphysics alive. Not only Poincaré reflected on this theme; Boltzmann and Maxwell, to keep up with the other two who had been randomly selected, were also concerned about understanding the intrinsic strength of metaphysics (Videira 1992).

More specifically, in the case of Poincaré, his participation was involuntary in a certain way, and it can be explained not only by the new scientific developments of the era, but it also was a result of the need he felt to respond to the criticisms that were directed at him by the French philosopher Edouard Le Roy, who was one of Henri Bergson’s former students, and by Bergson himself. Accused by Le Roy of being a conventionalist, Poincaré reiterated that scientific laws were creations of the human spirit, even while he maintained his belief that science would be able to describe external reality. Despite the intrinsic interest of this debate, as shown in the final chapters of *The Value of Science*, this article is not meant to comment on it. Here I will limit myself to comment – with no pretense to be exhaustive – on the reasons given by Poincaré himself to try to deny science of any and all interest in metaphysics. Strictly speaking, I think one cannot make a definitive comment on Poincaré's position as to why metaphysics cannot be excluded from reflections about the nature of science. It seems to me that at the end of his life – *The Value of Science* was published seven years before his death – Poincaré entertained some relevant and serious doubts about the coherence of some of his positions that had been previously divulged. The defense of the realist position made it so that Poincaré approached the gateway to metaphysics, through which he consciously refused to pass. Even so, Poincaré recognized that realism could not be supported only by epistemological reasons; in other words, supported by reasons which took into account the specific nature of scientific knowledge. Contrary to what had been believed for a long time, the type of knowledge that is characterized by science cannot be used to support alone the realist position. Being more than a philosophical position, realism is a decision of metaphysical nature.

According to Ubaldo Sanzo, Poincaré developed epistemological thinking without the participation of ontological or metaphysical considerations (Sanzo 1996). Otherwise, the French *savant* would have refused allowing the establishment of the external world to play a significant role in the process of justifying its own epistemology. However, his refusal cost him a certain price. The price paid was that of never being able to answer the following question: how can we support or justify the certainty of our thoughts? Let me explain. As of the 1890s, when it became clear that there undeniably remained a pluralism in physical theories, Poincaré, who himself was a supporter of pluralism, realized that he probably would be unable to find a solution to the problem of the foundation of an epistemology on any rigorously coherent basis. For him, 'a rigorously coherent basis' meant, for example, that the scientist and epistemologist have no obligation to build a philosophy that is systematic and systemizing.

Since the Enlightenment, natural scientists entertained serious suspicions in relation to the attempts at organization of scientific practice that were proposed by philosophers. The adjective 'systematic' could not be used to understand science, since the latter continually changes, modifying the content of its theories. As of the mid-nineteenth century, it seemed increasingly evident that science would always be subject to evolutionary processes, much like what happens in the world of living organisms. As is known, Poincaré was not the only one to believe in the evolution of science, since Boltzmann and Maxwell, along with Ernst Mach and Wilhelm Ostwald, were also supporters of Charles Darwin's theory of evolution (Engels 1995).

In particular, the evolution of physics forced Poincaré to reflect on the effects of these evolutionary processes. Questions such as 'could the emergence of new theories threaten science regarding its ability to understand nature?' or 'how could one argue that the theories and laws of physics remain true if they themselves undergo major changes?' came to constitute the agenda of the scientists mentioned above. According to Elie During, Poincaré's position when he faced this agenda can be expressed in the following words:

> On the one hand, [there was the concern] to recognize and draw the consequences from a constant feature of the history of the sciences: the temporary character of theories, with evidence provided by the succession of scientific revolutions. On the other hand, but *at the same time*, [remained the concern] to recognize the objective value of science, and the fact that an effective position had [to be] effectively taken on what was real, not only in the field of technical and applied sciences (...) but even at the level of theories of physics. How can one reconcile these two concerns? And how can one understand this "effective position on what is real", as supposed by science regarding the world? This is what is at stake in *Science and Hypothesis*. A dual strategic concern is echoed: it is about fighting the spontaneous arrogance and dogmatism of scientists, while *at the same* time defending the value of science against the superficial skepticism that was maintained by "common people". (During 2001, 11–12, emphasis in the original)

Let us return to the expression 'a rigorously coherent basis.' A second possibility to understand it, if we accept the ideas of René Thom (1987), is to avoid epistemological audacity, that is, to refuse to want to answer questions concerning the nature of reality. In other words, one must resist metaphysical temptations. Further on in this article, I will show that the apparently inevitable use of characterized hypotheses, which Poincaré called indifferent hypotheses, weakens the ability to resist formulation of considerations regarding reality, and even reality that is investigated by science. In fact, indifferent hypotheses are a legitimate part of physics, and there is no way to avoid the presence of metaphysics in it. Before proceeding, allow me to comment quickly on some observations about the strategy which I will adopt upon trying to reach the goal that I have set.

Interlude

In order to move toward my goal of showing one of the main philosophical tensions experienced by Poincaré, which, despite his explicit unwillingness, he yielded to the provocations created by metaphysics, I believe it is important to point out the way (i.e., the strategy) that I will accomplish the goal of showing that the French scientist revised his position on 'first philosophy'. I begin by describing what I will not do in this work. For example, I do not intend to go through all the philosophical works of Poincaré. Thus, I recognize that is not my intention to reconstruct his arguments about the nature, purpose and methods of science thoroughly. The perspective I adopt, which is consciously daring or even bold, portrays Poincaré as an ambivalent scientist-philosopher in regards to the relationship between science and metaphysics. In a few instances in his philosophical texts, Poincaré appears to be someone who believes in his own ability to give certainty to scientific propositions. In others, he displays his suspicions about an epistemology which is deliberately constructed without resorting to metaphysical elements.

However, the search for coherence carried out by Poincaré leads me to make but a single comment on the totality of his epistemological production. Upon looking closely at the set of his reflections, it becomes possible for us to see that it was organized so that he could defend his positions without feeling the need to provide detailed arguments. Poincaré's writings, as already mentioned by some commentators (During 2001), were published to express views and opinions – his and those of others – and not to analyze opposing arguments, as currently occurs in the field of philosophy of science. Poincaré seems wary of long and detailed arguments as if their size and complexity could raise doubts in the mind of his reader:

> Behind the elegant prose of articles and prefaces that supply the article in *Science and Hypothesis*, [there is] no concern for the "popularization" of science, and [there is] no desire for systematic exposition. Poincaré did not spend much time with the preparation of his texts and he rarely returned to them. He conceived them as interventions. Rather than to consolidate a philosophical position, his concepts and arguments served him as support, as temporary setups for [the realization] of circumstantial operations. (During 2001, 7)

The above quotation leads us in a direction different from that which is usually followed by the interpreters of his thinking, according to whom Poincaré had developed a clear, well established philosophical position. That does not seem to us to be During's position, and it certainly is not ours. The clarity, candor and accuracy of Poincaré's claims hide the lack of precise, detailed arguments. However, although it is not difficult to verify the presence of these features, it is far from me to conclude that his theses are incomprehensible, because they are simple.

Poincaré Faced with Worldly Issues

Since I do not intend to construct a comprehensive analysis of Poincaré's philosophical thinking, allow me to defend my opinion by resorting to a single text of the French savant. The article, which I have chosen to discuss in favor of my interpretation of Poincaré and metaphysics, does not openly and explicitly discuss the presence of the latter. In the text 'New Concepts of Matter' (Poincaré 1933), the title of his contribution to the volume devoted to materialism, published in 1913, and therefore after his death, Poincaré avoided making a pronouncement about burning issues such as: the meaning of human existence, even if they did figure into the agenda of themes of the book in which his work came out. However, when it comes to metaphysics, only voluntarism is not enough to stop it. An indication of the weakness of voluntarism, which is not to be confused with will power, in which Poincaré is great, is the frequency of the expression "form of thinking" that appears in his text.

If it seems unquestionable that after the First World War people widely discussed the role of visions of the world, it is certain that at the time of the publication of the book about materialism, a year before the war which was scheduled by European powers and which was meant to be the war to end all wars, people also commented about the characteristics by which, for example, human cultures and societies differed from each other. It is not uncommon to find references to the term ‘vision of the world’, linking it to philosophical positions close to relativism and culturalism, since that would serve to embody our understanding of what the world is.

Attempts at explanation – even scientific one – would occur inside the visions of the world. It starts with a distinction, which is unsurpassable for many, between the world and the way we understand it. There would be no possibility of understanding the world without resorting to a vision or image, i.e., without resorting to elements, which are often freely chosen by the scientists themselves or even by lay people, that is, by common people, that prove necessary to make science itself real and effective. One example is the regularity of behavior attributed to natural phenomena. As with other publications by Poincaré, in his text on the conception of matter, we found no reflection on causality, determinism and unity of nature, which are traditional, metaphysical issues that could be analyzed in the light of science.

In the text that was chosen, Poincaré does not seem interested in taking a stand on whether science is materialistic or not, nor whether it would necessarily lead us to accept materialism. His concern is to show how science evolves. Therefore, he does not answer the question of whether science is materialistic or not since he believed that this does not have a precise meaning, which prevents the formulation of a satisfactory response. Poincaré’s problem seems to lie in his inability to understand the meaning of the word ‘materialism’. This is a misleading concept. Nonetheless, and as if not to disappoint his readers, most of which were probably religious practitioners, Poincaré said that not all scientists are materialists, since science does not control their lives, at least not the totality of their lives (Rollet 1996). In other words, science does not reach the level of values that are responsible for decisions regarding how one should live.

As previously stated, the article ‘New Concepts of Matter’ is intended to provide arguments in favor of a certain conception of the evolution of science, particularly physics. Throughout the nineteenth century, and having as motivation the most important transformations which occurred in the scientific disciplines, there was a widespread need to show that science, even as it was going through processes of improvement and replacement of theories and models, could continue to supply the goal of understanding reality. In Poincaré’s viewpoint, this claim can be expressed as if he did not want to deny the effective possibility of attributing truth value to scientific laws and theories. He seems to hold that visions of the world are the basis upon which science builds theories about objects and phenomena, which in turn are known to exist in relationships that are found in theories. Those relationships are more important than the actual objects and phenomena that supposedly exist in nature, and in fact, the ‘true object’ that is recognizable by science is formed by these relationships. Poincaré’s position is sometimes understood as being in favor of realism of a structural type (During 2001).

In the article of 1913, Poincaré states that history moves like a pendulum. On certain occasions, history nears the atomistic position, while after a period of time, and whether it is short or long does not matter, it approaches the opposite position, advocated by supporters of continuity. Without telling us why, at this moment

Poincaré takes a bold step by asserting that this pendulum cannot be avoided, "... science is doomed to oscillate constantly between atomism and continuity, between the mechanism and dynamism and, conversely, [even] these oscillations will never stop" (Poincaré 1933).

The impossibility of imposing an end to these oscillations is explained by the French mathematician as follows:

> This struggle [between the antagonistic positions mentioned above] will last as long as science does, since it is due to the opposition of two irreconcilable needs of the human spirit, of which the latter could not break away without ceasing to exist. [These irreconcilable requirements are:] that of understanding, and we can only comprehend that which is finite, and that of to seeing and we can only see extension, which is infinite. (Poincaré 1933, 50)

The characterization given by Poincaré to the process of development of human knowledge suggests that the need for resorting to visions of the world – which is also inevitable and permanent – stems from a characteristic of the latter, namely, its weakness, that is, in its inherent inability to comprehend totality. In that which concerns him, weakness is not only permanent, but it is also constitutive, originating from the fact that the human spirit observes objects from a viewpoint that is external to him. There is always an insuperable distance between the spirit (or the subject) and the object. In other words, it is never possible to reach a situation whereby it would be feasible to have a metaphysical stillness achieved through the determination of a bridge built between the spirit and the object; the spirit and the object must never be confused or fusioned. Furthermore, for Poincaré, dualism, which is one of the hallmarks of Modern Thought, is inevitable.

Realism According to Poincaré

Realism, a philosophical position that Poincaré does not forego, is directly connected to the objectivity of science. One might even think that realism and objectivity may blend into one. It seems to me that it may still be possible to show the presence of continuity between the different branches of science. Poincaré never forgot a lesson he learned in his own time: that science, with a considerable and seemingly unpredictable frequency, can be revised. In spite of constantly undergoing revisions, science does not need to open up to its capacity to describe reality. Therefore, Poincaré saw himself as being obliged to show how these revisions do not prevent the recognition, expressed through theories and laws, that reality is intelligible and understandable. How then can one reconcile these two apparently irreconcilable demands between the 'fact' that science has a (very busy) history and its objective value as perceived in its ability to say things about reality? The key to answering this question lies in the belief, which was never abandoned by the French scientist, that reality is displayed in the invariable relationships that science formulates from observations of natural phenomena. Whether in physics, or in geometry, the 'real' objects of science are constituted by relationships, which remain invariable in a certain group of transformations. For Poincaré, this invariability is the most faithful characteristic of scientific objectivity (Zahar 2001).

Evolution and transformation should not prevent the formulation of a unified conception of science; hence Poincaré's concern about finding a solution to the above question can ensure continuity between theories. This continuity between two theories is not the result of the analysis of objects of a certain type, nor of the particular nature of a process that exists in nature, but rather of the logical forms of the structures that underlie their physical content. The analysis of natural objects and processes does not absolutely supply us with what theories talk about, which nonetheless does not prevent them from being considered true.

It can be stated that Poincaré was a skeptic with regard to the physical content of theories; but regarding structures, he was a realist. When he was led to try to determine his position, Poincaré was content to say that a scientific theory is, at best, likely to correctly represent certain structures of reality, without ever being able to say what these structures are. Thus, at the epistemological level, he took a pessimistic position.

Poincaré maintained that there were no hypotheses about nature – here he refers to essences – of things, but only about the relationships between these things. With regard to the ontologies retained by mechanical models, and the nature of the entities and processes postulated by both of them (ontology and nature), they are topics to be decided on conventional levels. They are called indifferent hypotheses that are related to entities and processes, of which one cannot have direct experience. The concept of Poincaré's theory of physics can be understood as being structural. His physics was structural physics. According to him, structures are linked to the notion of a group, and the latter is characterized by a set of operations governed by general properties of combination (associativity, reversibility, etc.). In short, Poincaré's realism is supported by the existence of mathematical relations, which remain unchanged even if the phenomena described may differ amongst themselves.

Dangerous, but Necessary Hypotheses

Poincaré distinguished at least three types of hypotheses in physics (Poincaré 1933). However, as noted above, his attention was particularly focused on indifferent hypotheses, since they generally consisted of assertions concerning the structure of matter. The adjective 'indifferent' which is used by Poincaré to underline this kind of hypotheses, although it may be comfortable regarding the construction and understanding of a specific theory, exerts no influence on the scientific value of this same theory, whose laws are based on differential equations, which are responsible for its structure (Poincaré 1933).

If at several times Poincaré is direct and very economical in the use of the words used to express his thoughts, situations arise in which he is obliged to pursue the discursive formulations necessary to avoid the presence of metaphysics. The latter means, in the case which interests us here, the formulation of speculative thoughts and theories about reality, its structure and its modes of operation and organization. Even while believing it is almost impossible to end metaphysical discussions, Poincaré believed it would be useful to discuss the nature and function of the constituent elements of any theory of physics, and even more so when those elements could be used as a gateway for metaphysics to be mixed with science . In particular, Poincaré believed that the most favorable elements for the existence of such a mixture would be hypotheses. His distrust should not be seen as meaning the acceptance of the positivist attitude in science. Indeed, Poincaré tried to avoid "ontological boldness": "... Poincaré's epistemology is, in fact, a persistent and persevering reduction of scientific hypotheses to pure and simple conventions" (Daston and Galison 2007).

Like virtually all scientists after Kant, Poincaré thought that science could never know things as they are, that is, it is within the domain of essences they would be found, once and for all, forbidden to scientific practice. In the case of science, the support given to the Kantian thesis can be explained by a deliberate attempt to avoid metaphysical speculation. Metaphysics would not be helpful in seeking out solutions to scientific problems. On the contrary, any attempt to resort to metaphysics would imply the emergence of new problems for which no one could provide solutions (Giedymin 1982).

Since the prohibition of the use of hypotheses had been formulated by Newton, they were seen – mainly by positivists, empiricists and inductivists – as suspicious and dangerous, which however was not enough to keep them from being used. Hypotheses seemed to be indispensable, and their presence a necessity. Yet Poincaré believed it to be possible to control the use of hypotheses if the reason for their need was understood. An intermediate step, to be carried out so that the understanding of the necessity of using hypotheses could be achieved, was the recognition of different types of hypotheses that were frequent in physics. Some hypotheses could be subjected to empirical testing. In this case, they were called conceptive truths. A second type performed the function of fixing one's thinking, clearing up the logical schema of a theory, just like its internal structure. Here, the role played by hypotheses is didactic. Finally, a third type of hypotheses is called apparent hypotheses. They are apparent because they can be reduced to definitions or to conventions in disguise. The strength of such hypotheses is due to their ability to accurately check mathematics and science by employing the latter. In Poincaré's thinking, these hypotheses play a key role, since they are freely created by the human spirit (Sanzo 1996). It must be noted that such conventions are imposed on science but not on nature. Any attempt to impose them on nature is to be seen as a remark in favor of metaphysics, and this is done by the imposition that natural phenomena may follow the rules of human thinking.

However, stating that these hypotheses are conventions does not mean that they are arbitrary, at least not totally and absolutely. Experiments can serve to formulate such hypotheses, since it is useful to show us the easy way in its formulation. Although it may sound ambiguous, and perhaps even contradictory, the rigor of the sciences is due to the presence of apparent hypotheses that are created by scientists, which makes the conscious effort of scientists responsible for accuracy.

Being aware, accuracy is the result of a decision, making the spirit of the scientist more acute during the formulation of scientific theories.

As it is the result of a decision, the control of hypotheses comes to be associated with voluntarism. How can one ensure that voluntarism is enough to prevent the proliferation of hypotheses, thus jeopardizing the accuracy of scientific theories? That is, how can one trust the common sense of scientists? It became important to formulate a regulator criterion to prevent the proliferation of hypotheses, and it should ideally be economical and easy to use it. Poincaré was concerned with the formulation of a criterion that would meet this need. His criterion was mainly quantitative, and his preference tended to be for those theories that used the fewest possible assumptions. For example, Poincaré preferred energetism over atomic theory, because the former no longer required the use of atoms, even as fiction.

At the end of the nineteenth century, the issue regarding the reality of atoms was not an issue that really worried Poincaré. His attention was focused on knowing whether the atomic hypothesis had an unavoidable role in the construction of theories of physics such as thermodynamics. The answer to this problem would be found through experiments, since they are the only source of truth for science.

Stating that hypotheses are conventions does not mean they all are admissible. Once again, use and excessive belief in the "powers" of the hypotheses are sources of serious problems for scientists. To avoid excessive liberality, Poincaré sought to assess the degree of admissibility of a hypothesis by means of the resources of fecundity and simplicity.

As shown in the discussion with E. Le Roy, Poincaré did not accept easily the conventionalist label, although that qualification is not so unbefitting for him (Sanzo 1996). His discomfort was not so much due to the label, but rather to the confusion that was often established between conventionalism, nominalism, and skepticism. Being comfortable is not the same as being arbitrary. In order to establish this distinction between convenience and arbitrariness, Poincaré analyzed the nature of truth in physics and the truth found in mathematics. They are two types of criterion of truth. Mathematics can tell us nothing about reality. Its role in physics arises during the work of organizing the theories and laws of the latter.

In order to clarify his viewpoints about the differences between physics and mathematics, Poincaré compared the first to a library. The collections of books that are classified and organized in the halls of libraries are substitutes for the laws and theories of physics, i.e., collections of books and magazines are substitutes for the latter. Experimental physics is the only field of science that can enrich and enhance a library's catalog, as it discovers new natural facts. On the other hand, the goal of mathematical physics is to organize the library's catalog, it is up to mathematical physics to organize the facts "collected" by experimental physics. This arrangement, or organization, provides no new information to librarians. The library does not become richer or more complete if its catalog is better designed. No collection is found in it as though it resulted from a donation provided by mathematical physics. The latter can help the reader to find more easily the book he seeks. By pointing things out for librarians and indicating gaps in their collections, mathematical physics suggests the performing of new experiments, which may eventually increase the knowledge of phenomena that occur in nature. In other words, we can say that mathematical physics contributes to the spread of scientific laws in order to increase and improve the efficiency of science.

Every generalization is a hypothesis. This is enough to show the relevance that hypotheses have for science. Every hypothesis goes further than that; it states more than what is found in the experiments that were performed. Strictly speaking, a hypothesis cannot avoid going beyond them, because from its constitution a statement is to be made, for which today – or even forever – empirical evidence is not yet available. In order to avoid the "excesses" present in hypotheses, it is necessary and compulsory to submit them to the demands of empirical verification. The abandonment of hypotheses, which are not sustained by facts, prevents speculation and dogmatism, as it helps create a barrier against metaphysics.

Mathematical physics plays an important role in the exact formulation of hypotheses, which in a considerable number of situations, are tacit and unconscious. The requirement of conceptual precision, which is to be obeyed in the formulation of hypotheses, obligates scientists to formulate the exact content of their hypothetical statements. But why does generalization usually take a mathematical form? This is due to the fact that an observable phenomenon consists of the superposition of a large number of elementary phenomena, which are similar to each other. Mathematics allows scientists to combine something similar with something else that is similar. Their goal is to find the result of a combination, taken as a whole, without having to rebuild it part by part.

The mathematical physicist recognizes the homogeneity of a physical object because it has an admirable degree of symmetry. Indeed, mathematical symmetry enables physicists to conceptualize perceived analogies between different phenomena; this was the case of Maxwell, who, according to Poincaré, had a deep intuition to find symmetries. The Scottish physicist, according to his continental colleague, always used his intuition to find the mathematical analogies between optics, magnetism and electricity to formulate his own version of electromagnetic theory. The requirement of symmetry between the fields of physics allowed Maxwell to create and to find physical analogies. The hypotheses imposed by symmetry are the common basis of all theories of mathematical physics. This should make it possible to have knowledge of the hidden harmony of things, or to find the symmetries that lie behind phenomena.

Indifferent hypotheses receive this designation because they do not change anything in a theory. One of Poincaré's examples was mathematical analysis, which can be stated hypothetically, at the very beginning, that matter is continuous or discrete. Regardless of the position adopted, it does not change the method of application of infinitesimal calculus. These indifferent hypotheses would not be dangerous, as long as it were possible to know explicitly that they are present. They may even be useful, whether as an artifice of calculation or to support understanding through the use of concrete images. There is therefore no reason to suspect indifferent hypothesis before proceeding to analyze their content and wording. In short, the recognition of their hypothetical character is not enough to ban them from the scientific scene.

Conclusion

According to Poincaré, one cannot obtain information about an interesting experiment if it does not go through a process of generalization. Bits of information that are not connected to each other have no interest whatsoever. The element that puts the information together is a hypothesis which, like the bricks that form a house, constitutes the whole of the experiment. Hypotheses are indispensable to science. The verification provided by experiments is insufficient to allow for safe and definitive control over the hypotheses; yet control is necessary to prevent or decrease speculation in science. Actually, it is impossible to reduce to zero the level of speculation in science, even if it is due to the unavoidable presence of tacit and unconscious hypotheses in science. These often are not even recognized as hypotheses, which make them particularly dangerous. Critical analysis, mathematical physics and experiments can help ward off metaphysics, but not enough to make it disappear once and for all.

Against expectations, and fueled mainly by empiricists and positivists, generalization and unity in science are not obtained by means of empirical facts or ideas that are empirically verified, whether by generalization or by unity, both are based on hypotheses that are freely formulated by scientists. These hypotheses can be modified in a process that is infinite and endless.

It is an odd conclusion, since the pace of science therefore seemed to be dictated by metaphysics and not by experiments or theory. However, one thing seems certain, and even though this conclusion has been obtained from an analysis of Poincaré's own thinking, he himself would never accept it. Accepting it, according to him, would mean denying the possibility of practicing science.

References

Daston, L., and P. Galison. 2007. *Objectivity*. New York: Zone Books.

During, E. 2001. *La science et l'hypothèse; Poincaré*. Paris: Ellipses Marketing.

Engels, E.M. 1995. *Die Rezeption von Evolutionstheorien im 19. Jahrhundert*. Frankfurt: Suhrkamp.

Giedymin, J. 1982. *Science and convention: Essays on Henri Poincaré's philosophy of science and the conventionalist tradition*. Oxford: Pergamon.

Jungnickel, C., and R. McCormmach. 1990. *Intellectual mastery of nature. Theoretical physics from Ohm to Einstein*, The Now Mighty Theoretical Physics, 1870 to 1925, vol. 2. Chicago: The University of Chicago Press.

Poincaré, H. 1933. Les conceptions nouvelles de la matière. In *Le Matérialisme Actuel*, ed. H. Bergson, 49–67. Paris: Ernest Flammarion.

Poincaré, H. 1998. *O Valor da Ciência*. Rio de Janeiro: Contraponto Editora.

Rollet, L. 1996. Henri Poincaré – Vulgarisation scientifique et philosophie des sciences. *Philosophiae Scientiae* 1: 125–153.

Sanzo, U. 1996. Contre l'ontologie: Poincaré et les hypothèses scientifiques. *Philosophiae Scientiae* 1: 27–43.

Thom, R. 1987. La philosophie des sciences de Henri Poincaré. *Cahiers d'histoire et des philosophies des sciences* 23: 71–83.

Videira, A.A.P. 1992. *Atomisme Epistémologique et Pluralisme Théorique dans la Pensée de Boltzmann*, Ph.D. Thesis, University of Paris VII.

Videira, A.A.P. 1997. O Atomismo Epistemológico de Ludwig Boltzmann. *Cadernos de História e Filosofia da Ciência* 7: 53–79.

Zahar, E. 2001. *Poincaré's philosophy: From conventionalism to phenomenology*. Illinois: Open Court.

Part II
Poincaré on the Foundations of Mathematics

Poincaré in Göttingen

Reinhard Kahle

Abstract In this paper we discuss the relation between Henri Poincaré and the Göttingen mathematician David Hilbert, in particular, in connection with Poincaré's visit to Göttingen in 1909.

Introduction

Henri Poincaré (1854–1912) was one of the foremost mathematicians in the world, with an extremely broad range of scientific activities, including pure and applied mathematics, mathematical physics, and astronomy.[1] From his extensive correspondence,[2] one can see that Poincaré was in contact with essentially the entire mathematical world of his times, and this included, of course, the two mathematical centers of Germany, Berlin and Göttingen. After Gauß, Göttingen remained a leader in mathematics with mathematicians like Dirichlet and Riemann; however, due to the organisational talents of Felix Klein, Göttingen was able to open, step by step, more professorships for Mathematics. The appointment of David Hilbert in 1895 turned out to be the luckiest one, as Hilbert soon reached the international mathematical forefront, alongside Poincaré.

In this paper, we retrace some influence Poincaré had on Hilbert's work on the foundations in mathematics, in particular by a talk Poincaré gave in 1909 in Göttingen. Regarding Poincaré's relations to the Göttingen mathematicians, it

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[1]For a general view on Poincaré, his life and his work see the two recent books Verhulst 2012 and Gray 2013.

[2]See the more than 2,000 letters documented and partly digitalized by the Archives Henri Poincaré in Nancy at http://www.univ-nancy2.fr/poincare/chp/hpcoalpha.xml.

R. Kahle (✉)
CENTRIA and DM, FCT, Department of Mathematics, Universidade Nova de Lisboa,
2829-516 Caparica, Portugal
e-mail: kahle@mat.uc.pt

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is worth mentioning that Poincaré's work was, initially, closer to Klein's.[3] They started a correspondence in June 1881,[4] and although Klein left the field in 1882,[5] he kept a friendly relationship with Poincaré as one can see from his correspondence. Poincaré travelled to Göttingen to visit Klein when he was on a trip to Halle to visit Cantor in May 1895.[6] But next to two letters to Klein,[7] we have no further information about this visit to Göttingen. Klein also tried, in 1902, to invite Poincaré to the meeting of the International Astronomical Society which took place in Göttingen; however, the invitation was unsuccessful.[8] When Poincaré was in Göttingen in 1909, the Hilbert family "gave a large reception for the Frenchman and for Klein whose sixtieth birthday fell during the visit" (Reid 1970, 120); but we have no further information about his contact with Klein by that time, and all correspondence concerning the visit is already in the hands of Hilbert.

Poincaré and Hilbert

First Meeting

Hilbert met Poincaré for the first time when visiting Paris in 1886. His first impressions are documented in letters to Klein:

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[3]See Klein's exposition in the closing chapter on automorphic functions in Klein 1927, Vol. 1 (English translation in Klein 1979). He opens the report thus: "It is now time for me to tell of the appearance of H. Poincaré and of the personal relations which developed between us and which laid the foundations for the further development of the whole subject" (Klein 1979, 355).

[4]By this time, Klein was still in Leipzig; he moved to Göttingen in 1886. Poincaré was already since 1884, i.e., before the arrival of Klein, corresponding member of the (Hanover) Royal Society of Sciences in Göttingen; he became foreign member in 1892.

[5]It is a known story that Klein's mathematical work stopped after he collapsed during some kind of competition with Poincaré; here his own words (Klein 1979, 360):

> In fact, I was again able to precede Poincaré by a little, for my offprints were sent off at the end of November 1882; while the first issue of the *Acta*, which contains Poincaré's first paper, appeared at the beginning of December 1882 [. . .]
>
> The price I had to pay for my work was extraordinarily high –my health completely collapsed. In the next years I had to take long leaves and to renounce all productive activity. [. . .] My real productive activity in theoretical mathematics parished in 1882.
>
> Thus Poincaré had a free field and, until 1884, went on to publish his five great papers on the new functions. [. . .]

[6]The visit to Halle is documented in a letter from Cantor to Poincaré from 15.12.1895 (Poincaré 1986; Cantor 1991). See also Décaillot 2011, 30.

[7]Undated letters from Poincaré to Klein (Poincaré 1989, XXVII and XXVIII).

[8]See the letter of Klein to Poincaré from 14.1.1902 (Poincaré 1989, XXXII) and the one from Karl Schwarzschild to Poincaré from 22.4.1902 (Poincaré 1989, footnote 120, 138; a scan of the letter is available at the webpage mentioned in footnote 2) and the report of the meeting (Kreutz 1902).

> He lectures very clearly and to my way of thinking very understandably although as a French student here remarks, a little bit too fast. He gives the impression of being very youthful and nervous. Even after our introduction, he does not seem to be very friendly; but I am inclined to attribute this to his apparent shyness, which we have not yet been in a position to overcome because of our lack of linguistic ability. (Reid 1970, 23)

And:

> But about Poincaré I can only say the same –that he seems reserved because of shyness, but that with skillful treatment he would open up. (Reid 1970, 23)

The International Congresses of Mathematicians

As far as we know, Poincaré and Hilbert met next only at the International Congress of Mathematicians (ICM) in 1900 in Paris. At this congress Hilbert gave his famous "Mathematische Probleme" speech, and although there is no report on direct response or interaction with Poincaré, there is a letter from Poincaré to Hilbert where he granted him 15 min more for the talk.[9] Reid describes the impact of the talk as follows:

> His rapidly growing fame –exceeded now only by that of Poincaré– promised that a mathematician could make a reputation for himself by solving one of the Paris problems. (Reid 1970, 84)

In fact, the International Congresses of Mathematicians quickly became the most, prominent platform from which to address the entire mathematical world. For the first congress in 1897 in Zürich Poincaré prepared an opening address, "an informal talk [. . .] on the way in which pure analysis and mathematical physics serve each other" (Reid 1970, 55). In fact, Poincaré did not attend the meeting because of the dead of his mother, but the paper was read by Jérôme Franel (Verhulst 12, 45f). Also Hilbert did not attend the congress in Zürich, but he was impressed by Poincaré's paper (Reid 1970, 55). When he prepared his Paris talk, he "wanted to reply to Poincaré with a defense of mathematics for its own sake, but he also had another

[9]Cf. http://www.univ-nancy2.fr/poincare/chp/text/hilbert05.xml, *Poincaré a Hilbert, Ca. 1899-début 1900:*

> Mon cher Collègue,
>
> Nous serons très heureux d'entendre votre communication. Nous vous accordons volontiers trois quarts d'heure; seulement ne le racontez pas, tout le monde ferait la même demande. Pour ce que vient de vous-plus on aura, plus on sera content.
>
> Votre bien dévoué,
>
> Poincaré.

(Cod. Ms. D. Hilbert 312, Handschriftenabteilung, Niedersächsische Staats- und Universitätsbibliothek. A transcription and commentary appeared in Poincaré 1986, 208).

idea" (Reid 1970, 69). Asking Minkowski for his opinion, he was in favor of this other idea:

> I have re-read Poincaré's lecture ... and I find that all his statements are expressed in such a mild form that one cannot take exceptions to them [...] Most alluring would be the attempt at a look into the future and a listing of the problems on which mathematicians should try themselves during the coming century. With such a subject you could have people talking about your lecture decades later. (Reid 1970, 69)

In 1904 the ICM took place in Heidelberg. Hilbert delivered a talk (Hilbert 1905b) which contained the first outline of what later –in the 1920s– became *Hilbert's programme*. Poincaré did not attend the congress in Heidelberg, but he discussed Hilbert's talk already in 1905 (Poincaré 1905).[10] And in 1908 in Rome, he presented a plenary lecture which can be considered as a response to Hilbert.[11] One gets the impression that two of the world's leading mathematicians used the ICMs as a communication platform.

The Bolyai Prizes

In 1905 the Hungarian Academy of Science launched a new prize for mathematicians, in honor of Janos and Farkas Bolyai (the famous founding son-father duo who initially investigated non-Euclidean geometry) worth the impressive amount of 10,000 gold crowns. The committee of the first edition of this prize consisted of Julius Kőnig, Gustav Rados, Gaston Darboux, and Felix Klein. As Reid writes:

> [...] but, even before the committee met, it was clear to everyone in the mathematical world that the choice would be between two men. The final vote was unanimous. The Bolyai Prize would go to Henri Poincaré, [...] but the committee also voted unanimously that, as a mark of their high respect for David Hilbert, the report which they made to the Academy on their choice would treat his mathematical work to the same extent that it treated Poincaré's. 'No cash, but honor', Klein wrote Hilbert regretfully from Budapest. (Reid 1970, 106)

In the published report of Rados (1906) one finds an appreciation of Hilbert's work alongside Poincaré's, even though the latter took the prize. It should not come to a surprise, then, that in the second edition of the Bolyai Prize, in 1910, the recipient was David Hilbert. This time, Poincaré was in the committee (together with Julius Kőnig, Gustav Rados and Gösta Mittag-Leffler) and he also wrote the report on Hilbert's work (Poincaré 1912). However, the prize was awarded in absentia and therefore Poincaré and Hilbert did not meet at this opportunity.[12]

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[10] In this context, it is worth mentioning that Poincaré reviewed in 1902 (Poincaré 1902), Hilbert's famous book *Grundlagen der Geometrie* which acquainted Poincaré with Hilbert's *axiomatic method*.

[11] Poincaré was present in Rome, but due to his poor health during the conference, the talk was, in fact, read by Gaston Darboux, cf. Gray 1991 and Verhulst 2012, 50.

[12] More information about the Bolyai prize —which vanished with World War I, and which was revived only in 2000— one may find at the site of the Hungarian Academy of Science,

The 1909 Visit of Poincaré to Göttingen

Paul Wolfskehl (1856–1906), a Mathematician and son of a rich banker in Darmstadt, bequeathed an important amount to the first person to prove Fermat's Last Theorem. This is the famous Wolfskehl Prize.[13] The money was deposited with the Royal Academy of Science in Göttingen who was entitled to use the interest to invite mathematicians to Göttingen. As chairman of the respective committee, it was Hilbert who sent the first invitation, in 1908, out to Henri Poincaré.[14]

It is worth noting that most of the survived correspondence between Poincaré and Hilbert (7 of 10 letters) is concerned with the organization of this visit. We reprint in Fig. 1 the letter of Hilbert to Poincaré from February 25, 1909, which gives a good illustration of the style of the correspondence. It also indicates that the topics of the last two talks of Poincaré were chosen on request of Hilbert.[15]

Poincaré's Talks in Göttingen

During his visit, Poincaré gave six talks which were published in 1910 (Poincaré 1910).[16] The first five talks were given in German, the last in French, and their titles read (in English):

1. On Fredholm's equations
2. Application of the theory of integral equations for the flood movement of the sea
3. Applications of integral equations for Hertz waves

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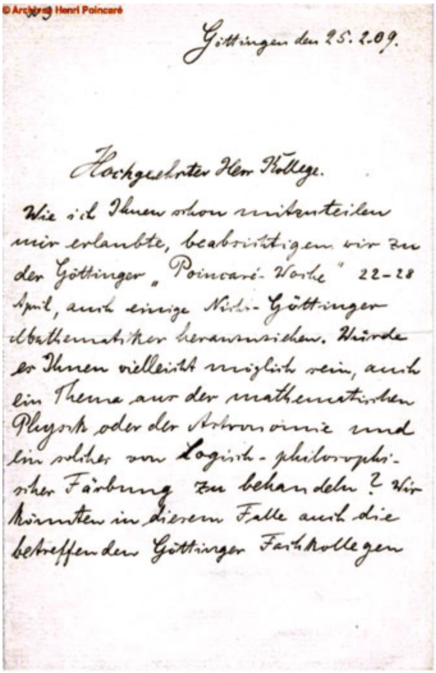
http://www.mathe.bme.hu/akademia/jbimp.html. It contains a link to an interesting historical note (cf. Szénássy) where one reads: "I also mention that Poincaré and Hilbert did not receive the award in Budapest: instead, it was delivered to them by official channels. As far as I know, Hilbert had never been to Budapest; [. . .]".

[13]It was awarded, in 1997 just 10 years before a 100 year limit expired, to Andrew Wiles. The amount was no longer comparable with the original value but still around 75,000 DM (German Marks). For more about the history of Paul Wolfskehl and the Wolfskehl Prize, see Barner 1997. In addition to the story that the money of the Wolfskehl donation melted during the German hyperinflation and two monetary reforms, we learned once (without being able to recall the source) that the Wolfskehl commission was forced to invest their money in German war bonds, which, of course, were completely worthless after the war. However, infracting the order some money was kept aside.

[14]About the following years we know from Reid (1970, 1351), that in 1910 H. A. Lorentz was invited; in 1911 no lecturer was invited but Zermelo received a prize of 5,000 Marks; in 1912 Sommerfeld was invited; and in 1913 a conference on the Kinetic Theory of Matter was organized; finally, in 1914 Haar and Debye were invited as guest professors. Hilbert also planned to invite Bertrand Russell, whose visit due to World War I never materialized (Sieg 1999, Appendix B).

[15]We thank the Archives Henri Poincaré, UMR 7117 CNRS – Nancy-Université, Université Nancy 2, France (Prof. Gerhard Heinzmann) for the permission of the reprint.

[16]A scanned version of the chapters of the book is available under http://projecteuclid.org/euclid.chmm/1263313049, a text version under http://www.univ-nancy2.fr/poincare/bhp/hp1910sv.xml.



Göttingen den 25.2.09.

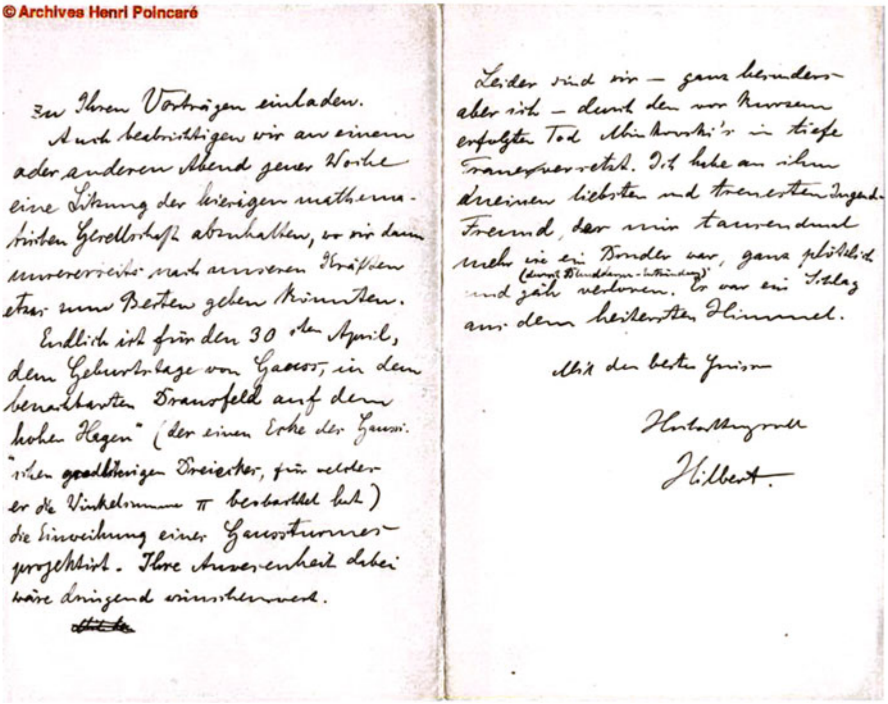
Hochgeehrter Herr Kollege.

Wie ich Ihnen schon mitzuteilen mir erlaubte, beabsichtigen wir zu der Göttinger „Poincaré-Woche" 22–28 April, auch einige Nicht-Göttinger Mathematiker heranzuziehen. Würde es Ihnen vielleicht möglich sein, auch ein Thema aus der mathematischen Physik oder der Astronomie und ein solches von logisch-philosophischer Färbung zu behandeln? Wir könnten in diesem Falle auch die betreffenden Göttinger Fachkollegen

Göttingen, February 25, 1909

Highly honored Mr. Colleague,

As I already took the liberty to write you, we plan to consult for the Göttingen "Poincaré week", April 22–28, some non-Göttingen mathematicians. Would it maybe possible for you, to discuss a topic from mathematical physics or astronomy and one of logico-philosophical character? In this case, we could also invite the respective Göttingen colleagues to your lectures.
[A paragraph on the mathematical society in Göttingen]
[A paragraph on an event for Gauß's birthday on April 30th]
[A paragraph on the death of Minkowski]
Best regards
Yours respectfully
Hilbert



zu Ihren Vorträgen einladen.

Auch beabsichtigen wir an einem oder anderen Abend jener Woche eine Sitzung der hiesigen mathematischen Gesellschaft abzuhalten, wo wir dann unsererseits nach unseren Kräften etwas zum Besten geben könnten.

Endlich ist für den 30ten April, dem Geburtstage von Gauss, in dem benachbarten Dransfeld auf dem „hohen Hagen" (der einen Ecke der Gauss'schen geodätischen Dreiecks, für welches er die Winkelsumme π beobachtet hat) die Einweihung eines Gaussturmes projektirt. Ihre Anwesenheit dabei wäre dringend wünschenswert.

Leider sind wir — ganz besonders aber ich — durch den vor kurzem erfolgten Tod Minkowski's in tiefe Trauer versetzt. Ich habe an ihm meinen liebsten und treuesten Jugendfreund, der mir tausendmal mehr wie ein Bruder war, ganz plötzlich (durch Blinddarm-Entzündung) und jäh verloren. Es war ein Schlag aus dem heitersten Himmel.

Mit den besten Grüssen

Hochachtungsvoll

Hilbert.

Fig. 1 Hilbert's letter to Poincaré from February 25, 1909

4. On the reduction of Abel integral and the theory of Fuchs functions
5. On transfinite numbers
6. The new mechanics

The first four concern integral equations, the last is about relativity theory. In the following we will focus on the fifth one, which is of "logico-philosophical character".[17] Before, let us recall a report of Reid of a rather cool reception of Poincaré in Göttingen:

> Socially and mathematically, the situation was delicate. The breakdown which had changed the entire course of Klein's career had been brought about by his competition with the young Poincaré. Now the leading mathematicians in the world were Hilbert and Poincaré, but the Bolyai Prize had gone to Poincaré. To many people in Göttingen, the Frenchman's presence was an unwelcome reminder that the mathematical world was not a sphere, with its center at Göttingen, but an ellipsoid.
>
> Poincaré's choice of subjects for his lectures did not help the situations. He decided to speak on integral equations and relativity theory, both areas in which he had made substantial contributions, and he probably chose these topics because he knew that the Göttingen mathematicians were interested in them. But a foreign mathematician who was present was very surprised at the coolness with which the famous guest was received. '*We* were surprised', one of the Göttingen docents explained, 'that Poincaré would come and talk to *us* about integral equations!' (Reid 1970, 120)

The cool reception is also discussed in a recent paper by Barrow-Green (2011, 41ff) providing the following evidence from a letter of Oswald Veblen to Georg Birkhoff, Berlin 25.12.1913:

> [Hilbert] also struck me as being both urbane and magnanimous, although the stories one hears do not bear this out –for example, the stories told from the German point of view about Poincaré's visit to Göttingen put Hilbert and the others in rather a bad light.

Reid notes, however, that Hilbert maintained a friendly attitude towards Poincaré (for instance, by addressing him as "My Dear Friend"). One may ask whether the mentioned "coolness" was only a reaction of some followers who saw in Poincaré a rival of their admired master Hilbert, while the masters themselves had no problems at all. The only "first hand report" we found in Reid's Courant biography (Reid 1976) where she writes:

> Hilbert offered his own assistant to [Poincaré], and as a result Courant had the opportunity to observe together the two men who were unanimously acknowledged as the greatest mathematicians in the world at that time. They treated each other with a great deal of respect, he told me, but there was no spark between them like that between Hilbert and Minkowski.

Integral equations were one of the central research topics of Hilbert and stood at the beginning of his activity in Physics (Reid 1970, Chap. XVI and p. 126f). In 1912, he published the important monograph *Grundzüge einer allgemeinen Theorie der linearen Integralgleichungen* (Hilbert 1912), which consisted of a compilation of papers published between 1904 and 1910 in the *Nachrichten der K. Gesellschaft*

[17] For more information about all talks, see Gray 2013, 416 ff.

der Wissenschaften zu Göttingen. In the final version we do not find any reference to Poincaré's talks in 1909 in Göttingen (but Poincaré is of course, mentioned in connection with other references).[18] In lack of other evidence we cannot judge the impact of Poincaré's talks on integral equations; we have, however, a lot of evidence concerning the impact of his fifth talk on transfinite numbers.

The Talk on Transfinite Numbers

Poincaré's talk on transfinite numbers in Göttingen, delivered on April 27, 1909, was not particularly original; one may consider it as a synopsis of earlier papers, in particular the three on *Mathematics and Logic* (Poincaré 1905, 1906a, b), which also found their way into the highly influential book *Science and Method* published in 1908 (Poincaré 1908).[19]

The Göttingen talk start off with a discussion of *Richard's paradox*, moves on with the proposal of *predicative definitions*, and finishes with a rather harsh criticism of set theory, addressing explicitly Zermelo's well-ordering theorem.

In the following, we reprint the English translation by William Ewald (1996, 22.G).[20]

On transfinite numbers

[1] Gentlemen! I wish to speak to you today about the concept of transfinite cardinal number; in particular, I want to speak first of an *apparent* contradiction that this concept contains. About that I say the following in advance: in my view an object is only thinkable when it can be defined with a finite number of words. An object that is in this sense finitely definable, I shall for brevity call simply 'definable'. Accordingly, an undefinable object is also unthinkable. Similarly, I shall call a law 'expressible' if it can be expressed in a finite number of words.

[2] Now, Richard has shown that the totality of definable objects is denumerable, that is, that the cardinal number of this totality is $\aleph_0$. The proof is quite simple: if α is the number of words in the dictionary, then with n words one can define at most α^n objects. If one now lets n grow beyond all limits, one sees that one never gets beyond a denumerable totality. The power of the set of thinkable objects would then be $\aleph_0$. Schoenflies has objected against this proof that one can define several objects, indeed even infinitely many, with a single definition. As an example he cites the definition of constant functions, of which there are obviously infinitely many. But this objection is inadmissible, because by such definitions it is not at all the individual ojects that are defined, but their totality, which is a single object: in our example the *set* of constant functions. The objection of Shoenflies is therefore not conclusive.

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[18] Here, and at many other places, one may ask how good Hilbert was in mentioning work of others. In his lecture notes he was usually sparingly in the bibliography and one gets the impression that he was not too much concerned with references.

[19] These papers are reprinted in English translations, together with indications of the changes made in the book edition in Ewald 1996, 22.D-F.

[20] We are indebted to William B. Ewald for the permission to reprint his translation. The paragraph numbers are additions of the translation.

[3] Now, as is well known, Cantor proved that the continuum is not denumerable; this contradicts the proof of Richard. The question therefore arises which of the two proofs is correct. I maintain that they are both correct and the contradiction only apparent. To support this contention I shall give a new proof of the Cantorian theorem: we therefore assume that an interval *AB* is given, and a law by which every point of the interval is correlated with a whole number. For the sake of simplicity we shall designate points by the numbers correlated to them. We now divide our interval by two arbitrarily chosen points A_1 and A_2 into three parts, which we designate as sub-intervals of level 1; these again we divide into three parts and obtain sub-intervals of level 2, we imagine this process continued into the infinite, whereby the length of the sub-intervals decreases beneath every bound. Now point 1 belongs to one or, if it coincides with A_1 or A_2, at most two of the sub-intervals of level1; there is therefore certainly one to which it does not belong. Here we look for the point with the lowest number, which now must be at least 2. Among the three sub-intervals of the second level which belong to the interval of the first level in which we find ourselves, there is again at least one to which the last-considered point does not belong. We continue the process with this interval, and so obtain a sequence of intervals which has the following properties: each of them is contained in all the preceding intervals, and an interval of the n^{th} level contains none of the points 1 to $n - 1$. From the first property it follows that there must be at least one point which is common to them all; but from the second property it follows that the number of this point must be greater than any finite number –that is, no number can be correlated with it.

[4] Now what have we presupposed for this proof? We have presupposed a law that correlates a whole number to every point of the interval. Then we were able to define a point to which no whole number is correlated. In this regard the different proofs of this theorem do not differ. But for that it was necessary that the law first be determinate. According to Richard, such a law should seemingly have to exist, but Cantor has proved the opposite. How do we get out of this dilemma? To start, we ask about the meaning of the word 'definable'. We take the table of all finite sentences and strike out all those which define no point. We correlate the remaining sentences with the whole numbers. If we now undertake the scrutiny of the table anew, it will in general turn out that we must now let several sentences stand that we had earlier struck out. For earlier the sentences in which one spoke of the law of correlation itself had no meaning, since the points were not yet correlated to the whole numbers. These sentences now have a meaning and must remain in our table. Should we now set up a new law of correlation the same difficulty would repeat itself, and so no *ad inifinitum*. But herein lies the solution of the apparent contradiction between Cantor and Richard. Let M_0 be the set of whole numbers, M_1, the set of all points of our interval definable after the first scrutiny of the table of all infinite sentences, G_1 the law of correlation between the two sets. A new set M_2 of definable points arises through this law. But a new law G_2 belongs to $M_1 + M_2$, through which there arises a new set, M_3, etc. Now, Richard's proof show that, wherever I break the process off, a law always exists, while Cantor proves that the process can be continued arbitrarily far. Therefore there exists no contradiction between the two.

[5] The appearance of contradiction comes from the fact that Richard's law of correlation lacks a property which I designate, with an expression borrowed from the English philosophers, as 'predicative'. (In Russell, from whom I borrow the word, a definition of two concepts A and A' is not predicative if A occurs in the definition of A' and conversely.) I understand by this the following: every law of correlation presupposes a determinate classification. I now call a correlation predicative, if the corresponding classification is predicative. And I call a classification predicative if it is not changed by the introduction of new elements. But this is not the case for Richard's classification; rather the introduction of the law of correlation alters the division of the sentences into those which have a meaning and those which have none. What is here meant by the word 'predicative' is best illustrated by an example. If I am to deposit a set of objects into a number of boxes two things can occur: either the objects already deposited are conclusively in their places, or, when I deposit a new object, I must always take the others out again (or at any rate some of them). In the first

case I call the classification predicative, in the second not. Russell has given a good example of a non-predicative definition: let A be the smallest whole number whose definition requires more than a hundred German words. A must exist, since one can define only a finite number of numbers with a hundred words. But the definition which we have just given of this number contains less than a hundred words. And the number A is thus *defined as undefinable*.

[6] Now, Zermelo has objected against the rejection of non-predicative definitions that a great part of mathematics would become invalid as well, for instance, the proof of the existence of a root of an algebraic equation.

[7] This proof, as is well known, runs as follows:

[8] An Equation $F(x) = 0$ is given. One now profess that $|F(x)|$ must have a minimum; let x_0 be one of the arguments for which the minimum occurs, so that

$$|F(x)| \geq |F(x_0)| \,.$$

From this it then follows that $F(x_0) = 0$. Now here the definition of $F(x_0)$ is not predicative, for this value depends upon the totality of the values of $F(x)$, to which it itself belongs.

[9] I cannot admit the legitimacy of this objection. One can reshape the proof so that the non-predicative definition disappears. To this end, I consider the totality of arguments of the form $(m + ni)/p$ where m, n, and p are whole numbers. Then I can draw the same conclusions as before, but the value of the argument for which the minimum of $|F(x)|$ occurs does not in general belong to the arguments considered. In this way we avoid the circle in the proof. One can demand of every mathematical proof that the definitions, etc., occurring therein be predicative; otherwise the proof would not be rigorous.

[10] How do things now stand with the classical proof of the Bernstein theorem? Is it unobjectionable? The theorem states, as is well known, that if three sets A, B, and C are given, where A is contained in B and B in C, and if A is equivalent to C, then A must also be equivalent to B. So here too it is a question of a law of correlation. If the first law of correlation (between A and C) is predicative, then the proof shows that there must also be a predicative law of correlation between A and B.

[11] Now, as far as the second transfinite $\aleph_1$ is concerned, I am not entirely convinced that it exists. One reaches it by considering the totality of ordinal numbers of the power $\aleph_0$; it is clear that this totality must be of a higher power. But the question arises whether it is self-contained, and therefore of whether we may speak of its power without contradiction. There is not in any case an actual infinite.

[12] What, then, are we to think of the famous *problem of the continuum*? Can one well-order the points of space? What do we mean thereby? There are two cases possible here: either one asserts that the law of well-ordering is finitely statable, and then this assertion is unproven; even Zermelo does not claim to have proved such an assertion. Or we grant the possibility that the law is not finitely statable. Then I can no longer attach any sense to this statement; it is for me merely empty words. Here lies the difficulty. And that is indeed the cause of the conflict over the theorem of Zermelo, a theorem that is nearly a stroke of genius. This conflict is very peculiar: one side rejects the postulate of choice but holds the proof to be correct; the other admits the postulate of choice, but does not acknowledge the proof.

[13] However I could speak about this for many more hours without solving the question.

One may note, that Poincaré did not repeat here his criticism of the alleged circularity concerning the use of induction in Hilbert's foundational programme, which was addressed for instance, quite explicitly, in Poincaré 1906b.[21]

The Aftermath of the Talk

Poincaré's talk on transfinite numbers in Göttingen is known for two reasons: its role in the ambient polemic with Zermelo and its motivation for Chwistek's turn to predicativity.

A statement like "There is not in any case an actual infinite"[22] is clearly an offense in the presence of Zermelo, who had just published his axiomatization of set theory. Of course, it was not the first time that he faced this allegation, and the discussion with Poincaré was already manifest in several publications. However, the personal confrontation between the two in Göttingen is a zenith point. The full discussion is well-documented in the recent Zermelo biography (Ebbinghaus 2007, §2, in particular pp. 64–67), with explicit reference to the 1909 talk (Ebbinghaus 2007, 110), but there is one particular testimony concerning Zermelo's reaction. More than 50 years later, in 1964, Richard Courant reported the following (Courant 1981, 162)[23]:

> I remember once when Henri Poincaré came to Göttingen shortly before his death to give a number of very interesting talks on different topics; [. . .] another was on the foundations of mathematics. It was a violent attack against Cantorism and against the principles of choice and theorems such as the one about well-ordering. Zermelo had just proved the fact that every set can be well-ordered and was sitting near him at his feet. Poincaré wanted to be polite (he could be devastatingly impolite if he tried to be friendly) and he thundered against the Cantor attitude and against the trend in mathematics to do something in this direction. He said, "Even the almost ingenious proof of Mr. Zermelo has to be completely scotched and thrown out of the window". Zermelo, who was a very passionate and very strange fellow, was in despair and fury and at the dinner the same day he would have shot Poincaré if he had been a little bit more skillful, but he was a clumsy person.

The talk of Poincaré was also attended by the Polish logician Leon Chwistek, who was in Göttingen at that time. Chwistek took Poincaré's objections seriously and started to work on a predicative version of the theory given in Whitehead and Russell's *Principia Mathematica* (Whitehead and Russell 1913). For it, he was explicitly acknowledged in the second edition of the *Principia* (Whitehead and Russell 1927).[24] His work, however, was not well received. We should not omit that

[21] See Sieg 1999, 7.

[22] The original German reads: "Ein actual Unendliches gibt es jedenfalls nicht".

[23] Also Barrow-Green 2011, 41f refers to this quotation, but remarks "Courant's memory –he was recalling events that had taken more than fifty years earlier– might not have been entirely reliable".

[24] For a detailed discussion of his work in relation to the Principia see Linsky 2011.

Hilbert had a PhD student, Heinrich Behmann, who wrote between 1914 and 1918 his thesis on the *Principia Mahtematica* with special attention to the reducibility axiom (which is essential for impredicativity) (Mancosu 1999). However, we are not aware that –in contrast to Chwistek– Behmann's work can be related to Poincaré.

Here we would like to illustrate a third, less known, result of Poincaré's Göttingen talk: its influence on Hilbert's foundational work.[25] It is known that from the turn of the century on, Hilbert had a particular interest in the foundations of mathematics. This is evident from the second problem of the famous problem list presented at the International Congress of Mathematicians in Paris in 1900 – the consistency of the axioms of arithmetic. Hilbert's address in 1904 at the International Congress of Mathematicians in Heidelberg is usually regarded as the first public sketch of what later became *Hilbert's programme*. Following the "official" historiography, as given for instance by Blumenthal in 1935 in the *Collected Works* of Hilbert (Blumenthal 1935; Hilbert 1935), his foundational research paused, stopping at this point in 1904, until it resumed in 1917.[26] However, a closer inspection shows that Hilbert did not put these matters aside completely. Indeed, in 1905 he gave a lecture course in Göttingen on *Logische Principien des mathematischen Denkens* (*Logical principles of the mathematical thinking*) (Hilbert 1905a), and he gave further lectures on similar topics in 1910 (Hilbert 1910) and in 1914/15 (Hilbert 1915). In this context, the talk of Poincaré in 1909 left a definite trace in Hilbert's work: the official notes of the lecture course in 1905 contain marginal with explicit references to Poincaré.

In Fig. 2 we reprint page 202 of these lecture notes; they were written by Ernst Hellinger, but the marginals are clearly in Hilbert's hand. It is evident that Hilbert reused these lecture notes.[27] They are occasionally commented, but the number of marginals on the given page is exceptionally high. All three, and one more on the following page can be related to issues raised in Poincaré's talk.

The marginals on the side and on bottom of the page are given in Figs. 3 and 4.[28]

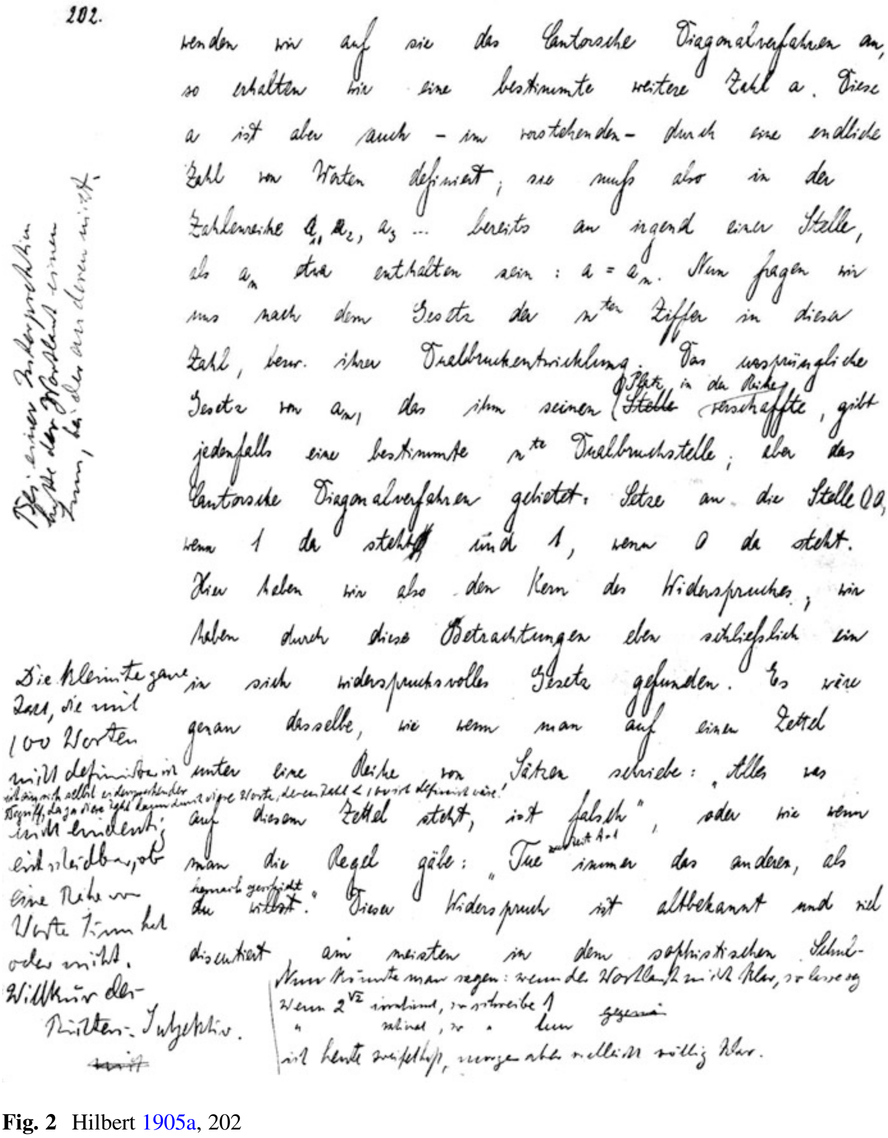
Both remarks are related to the Poincaré's question "about the meaning of the word 'definable'" (§4, above).

[25]This aspect was, with more emphasize on the paradoxes, already highlighted in Kahle 2011; the current section reuses, in particular, the marginals of Hilbert in his lecture notes from 1905.

[26]Addressing Howard Stein, Sieg sees one of the reasons for the postponement of Hilbert's work in *Proof Theory* in Poincaré's criticism of the potential vicious circle in the approach concerning induction (Sieg 1999, footnote 15, p. 7). This, however, should not be extended to foundational work in general.

[27]For instance, the draft notes for a lecture in 1914/15 contain an explicit reference for them.

[28]We are indebted to the Mathematical Institute of the University of Göttingen (Prof. Jörg Brüdern) for the permission to use here copies of the original lecture notes kept in the library of the Mathematical Institute.


202.

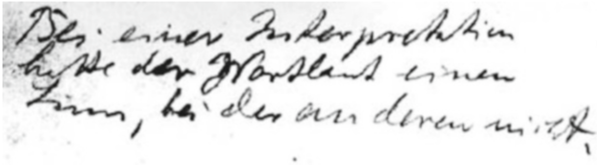
wenden wir auf sie das Cantorsche Diagonalverfahren an, so erhalten wir eine bestimmte weitere Zahl a. Diese a ist aber auch – im vorstehenden – durch eine endliche Zahl von Worten definiert; sie muß also in der Zahlenreihe $a_1, a_2, a_3 \ldots$ bereits an irgend einer Stelle, als a_n etwa enthalten sein: $a = a_n$. Nun fragen wir uns nach dem Gesetz der n^{ten} Ziffer in dieser Zahl, bezw. ihrer Dualbruchentwicklung. Das ursprüngliche Gesetz von a_n, das ihm seinen Stelle verschaffte, gibt jedenfalls eine bestimmte n^{te} Dualbruchstelle; aber das Cantorsche Diagonalverfahren gebietet: Setze an die Stelle 0, wenn 1 da steht und 1, wenn 0 da steht. Hier haben wir also den Kern des Widerspruches; wir haben durch diese Betrachtungen eben schließlich ein in sich widerspruchsvolles Gesetz gefunden. Es wäre genau dasselbe, wie wenn man auf einen Zettel unter eine Reihe von Sätzen schriebe: „Alles was auf diesem Zettel steht, ist falsch", oder wie wenn man die Regel gäbe: „Tue immer das anderen, als du willst." Dieser Widerspruch ist altbekannt und viel discutiert am meisten in dem sophistischen Schul-

Fig. 2 Hilbert 1905a, 202

The third marginal takes up, quite literally, Berry's paradox, as it was given at the end of §5. In English it reads:

> The smallest integer which is not definable by 100 words is a self contradicting concept, since this number would be defined by these words which are less than 100! not clearly decidable whether a sequence of words makes sense or not. Arbitrariness of [?]: subjective.

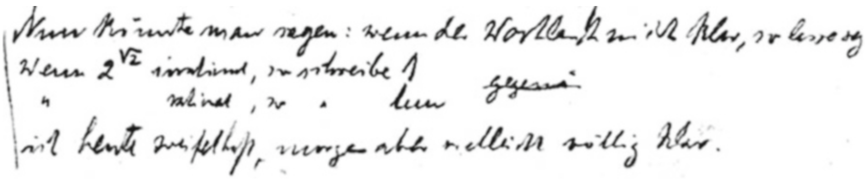
The use of "100 words" as a measure suggests that indeed Hilbert learned Berry's paradox from Poincaré's talk.



Bei einer Interpretation hätte der Wortlaut einen Sinn, bei der andern nicht.

"In one interpretation the text made sense, in the other not."

Fig. 3 Hilbert 1905a, 202, detail



Nun könnte man sagen: wenn der Wortlaut nicht klar, so [illegible]
Wenn $2^{\sqrt{2}}$ irrational, so schreibe 1
" rational, so " [illegible] [illegible]
ist heute zweifelhaft, morgen aber vielleicht völlig klar.

"Now one could say: if the text is not clear, so leave it out[?]: if $2^{\sqrt{2}}$ is irrational, write 1; if it is rational, write [?] is today equivocal, but tomorrow maybe completely clear."

Fig. 4 Hilbert 1905a, 202, detail

The last marginal given on page 203, mentions Poincaré by name and gives an interesting although not cogent counterargument to his repeated table scanning procedure in §4, see Fig. 5.[29]

Berry's paradox found its way in several further lectures by Hilbert. This paradox is of interest because it shows that the mathematical paradoxes are not just a consequence of transfinite set theory (as it was probably the general feeling among foundational thinkers at the beginning). Hilbert's interest in the foundations of mathematics are, at least in part, motivated by the *mathematical paradoxes*, and his lecture notes show a constant struggle with them (Kahle 2006). Even if there is no further reference to Poincaré's talk in later publications, we find a striking reminiscence to it in the second volume of Hilbert and Bernays's seminal monograph *Grundlagen der Mathematik:* as preparation for Gödel's theorems Bernays formalizes *Richard's paradox*, (Hilbert and Bernays 1939, S. 273ff). From a modern perspective, the treatment of Richard's paradox looks quite unmotivated at this place; but it might have been chosen as it was a challenge given to Hilbert by Poincaré.

[29]We follow Ewald in translating "Tafel" by "table" although "blackboard" would be a more literally translation.

"The procedure of Poincaré, where he passes the tables again and again shows clearly the problems, but contains a contradiction in itself. Because the rule according to which one has to pass the tables once and—with respect to the executed substitutions—again, is also written on a table and has therefore a sense already in the first pass."

This counterargument looks quite ingenious and should apply to a table with *all* finite sentences (as in Poincaré's presentation). However, it should be easy to construct a "Berry-style" analog where one considers only definitions of a certain length which still allow self-reference to switch the meaningfulness of some sentences from round to round, while the rule to pass the board again and again might be excluded as being too long.

We are not aware of any trace of Hilbert's counterargument outside this lecture notes, and it was apparently never put forward in a broader discussion.

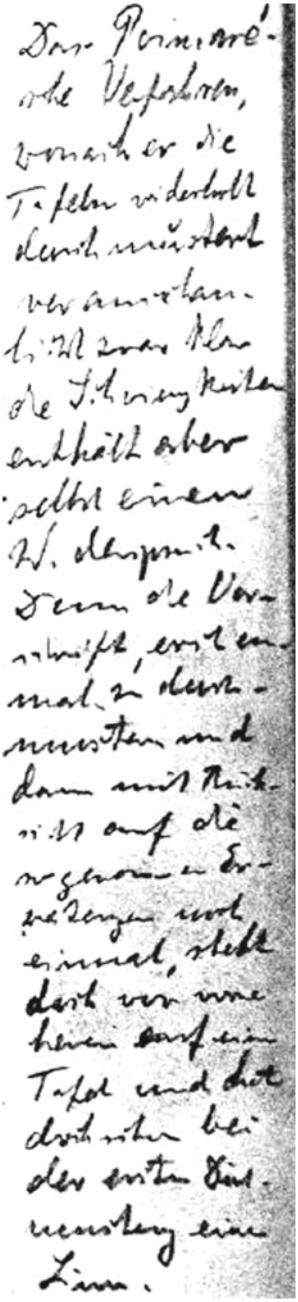

Das Poincaré'sche Verfahren, wonach er die Tafeln wiederholt durchmustert, veranschaulicht zwar klar die Schwierigkeiten, enthält aber selbst einen Widerspruch. Denn die Vorschrift, erst einmal zu durchmustern und dann mit Rücksicht auf die vorgenommenen Ersetzungen noch einmal, steht doch wieder auf einer Tafel und hat daher schon bei der ersten Durchmusterung einen Sinn.

Fig. 5 Hilbert 1905a, 203, detail

Acknowledgments I would like to thank again the Archives Henri Poincaré, the Mathematical Institute of the University of Göttingen, and William B. Ewald for the permission to reprint material of which they hold the copyright. I also thank Hassan Tahiri, the editor of Kahle 2011, for the suggestion to include here the scans of Hilbert's original texts. Finally, I'm indebted to Jesse Alama, Gerhard Heinzmann, Ferdinand Verhulst, and Wilfried Sieg for comments on earlier versions of this paper.

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References

Barner, K. 1997. Paul Wolfskehl and the Wolfskehl Prize. *Notices of the American Mathematical Society* 44: 1294–1303.

Barrow-Green, J. 2011. An American goes to Europe: Three letters from Oswald Veblen to George Birkhoff in 1913/1914. *The Mathematical Intelligencer* 33(4): 37–47.

Blumenthal, O. 1935. Lebensgeschichte. In *Gesammelte Abhandlungen*, vol. III, ed. David Hilbert, 388–429. Berlin: Springer.

Cantor, G. 1991. *Briefe*, ed. H. Meschkowski and W. Nilson. Berlin: Springer.

Courant, R. 1981. Reminiscences from Hilbert's Göttingen. *The Mathematical Intelligencer* 3(4): 154–164. Edited transcript of a talk given at Yale University on January 13, 1964.

Décaillot, A.M. 2011. *Cantor und die Franzosen*, Mathematik im Kontext. Berlin: Springer.

Ebbinghaus, H.D. 2007. *Ernst Zermelo*. Berlin: Springer.

Ewald, W.B. (ed.). 1996. *From Kant to Hilbert*, vol. 2. Oxford: Oxford University Press.

Gray, J. 1991. Did Poincaré say 'set theory is a disease'? *Mathematical Intelligencer* 13: 19–22.

Gray, J. 2013. *Henri Poincaré*. Princeton: Princeton University Press.

Hilbert, D. 1905a. Logische Principien des mathematischen Denkens. Vorlesung Sommersemester 1905, Ausarbeitung von Ernst Hellinger (Bibliothek des Mathematischen Instituts der Universität Göttingen).

Hilbert, D. 1905b. Über die Grundlagen der Logik und der Arithmetik. In *Verhandlungen des Dritten Internationalen Mathematiker-Kongresses in Heidelberg vom 8. bis 13. August 1904*, ed. A. Krazer, 174–185. Leipzig: Teubner.

Hilbert, D. 1910. Elemente und Prinzipienfragen der Mathematik. Vorlesung Sommersemester 1910, Ausarbeitung von Richard Courant (Bibliothek des Mathematischen Instituts der Universität Göttingen).

Hilbert, D. 1912. *Grundzüge einer allgemeinen Theorie der linearen Integralgleichungen*. Leipzig: Teubner.

Hilbert, D. 1915. Probleme und Prinzipien der Mathematik. Handschriftliche Notizen zur Vorlesung im Wintersemester 1914/15, (Niedersächsische Staats- und Universitätsbibliothek Göttingen, Cod. Ms. D. Hilbert 559).

Hilbert, D. 1935. *Gesammelte Abhandlungen*, Analysis, Grundlagen der Mathematik, Physik, Verschiedenes, Lebensgeschichte, vol. III. Berlin: Springer. 2nd ed. 1970.

Hilbert, D., and P. Bernays. 1939. *Grundlagen der Mathematik II*, Die Grundlehren der mathematischen Wissenschaften in Einzeldarstellungen, vol. 50. Berlin: Springer. 2nd ed. 1970.

Kahle, R. 2006. David Hilbert über Paradoxien. Volume 06-17 of *Pré-publicações do Departamento de Matemática, Universidade de Coimbra.* http://www.mat.uc.pt/preprints/ps/p0617.pdf.

Kahle, R. 2011. Hilbert and Poincaré and the paradoxes. In *Poincaré's philosophy of mathematics: Intuition, experience, creativity*, Cadernos de Filosofia das Ciências, vol. 11, ed. H. Tahiri, 79–93. Lisboa: Centro de Filosofia das Ciências da Universidade de Lisboa.

Klein, F. 1927. *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*. Springer, 1926 and 1927. 2 vols.; reprinted in one volume, Springer, 1979.
Klein, F. 1979. *Development of Mathematics in the Nineteenth Century*, vol. IX of *Lie Groups*. Brookline: Math Sci Press. Transl. of Klein (1927) by M. Ackerman.
Kreutz, H. 1902. Versammlung der Astronomischen Gesellschaft in Göttingen. *Astronomische Nachrichten* 159: 323–328.
Linsky, B. 2011. *The evolution of Principia Mathematica*. Cambridge: Cambridge University Press.
Mancosu, P. 1999. Between Russell and Hilbert: Behmann on the foundations of mathematics. *The Bulletin of Symbolic Logic* 5(3): 303–330.
Poincaré, H. 1902. Les fondements de la géométrie. *Bulletin des sciences mathématiques* 26: 249–272.
Poincaré, H. 1905. Les mathématiques et la logique. *Revue de métaphysique et de morale* 13: 815–835.
Poincaré, H. 1906a. Les mathématiques et la logique. *Revue de métaphysique et de morale* 14: 17–34.
Poincaré, H. 1906b. Les mathématiques et la logique. *Revue de métaphysique et de morale* 14: 294–317.
Poincaré, H. 1908. *Science et méthode*. Paris: Flammarion.
Poincaré, H. 1910. *Sechs Vorträge über ausgewählte Gegenstände aus der reinen Mathematik und mathematischen Physik*. Leipzig: Teubner.
Poincaré, H. 1912. Rapport sur le Prix Bolyai. *Acta Mathematica* 35: 1–28.
Poincaré, H. 1986. La correspondance d'Henri Poincaré avec des mathématiciens de A à H. *Cahiers du séminaire d'histoire des mathématiques* 7: 59–219.
Poincaré, H. 1989. La correspondance d'Henri Poincaré avec des mathématiciens de J à Z. *Cahiers du séminaire d'histoire des mathématiques* 10: 83–229.
Rados, G. 1906. Rapport sur le prix Bolyai. *Bulletin des sciences mathématiques (II)* 30: 103–128.
Reid, C. 1970. *Hilbert*. Berlin: Springer. Reprinted together with Reid 1976 in Reid 1986.
Reid, C. 1976. *Courant in Göttingen and New York*. New York: Springer. Reprinted together with Reid 1970 in Reid 1986; the page numbers refer to the reprint.
Reid, C. 1986. *Hilbert–Courant*. New York: Springer. Joint reprint of Reid 1970 and Reid 1976.
Sieg, W. 1999. Hilbert's programs: 1917–1922. *The Bulletin of Symbolic Logic* 5(1): 1–44.
Szénássy, B. Contributions to the history of the Bolyai-prize. http://www.math.bme.hu/akademia/contrib.html. Accessed 20 Feb 2014.
Verhulst, F. 2012. *Henri Poincaré*. New York: Springer.
Whitehead, A.N. and B. Russell. 1913. *Principia Mathematica*. Cambridge: Cambridge University Press, 1910–13. 3 vols.; 1st ed.
Whitehead, A.N. and B. Russell. 1927. *Principia Mathematica*. Cambridge: Cambridge University Press, 1925–27. 3 vols.; 2nd edition of Whitehead and Russell 1913.

Poincaré and the Principles of the Calculus

Augusto J. Franco de Oliveira

Abstract Poincaré wrote several papers and sections of books on geometry and space, less on the continuum, but very little on the basic notions of the infinitesimal calculus. The 1905 paper "Cournot et les Principes du Calcul Infinitesimal" is one of the very few places where Poincaré expounds his own views on the subject, although by way of comment of Cournot's *Traité élémentaire des fonctions* ... (1841) and *Traité de l'enchaînement des idées* ... (1861), which he cites profusely, and whose ideas in this subject were otherwise neglected by both contemporaries and modern commentators. In this paper we analyze Poincaré's paper with two aims: to characterize briefly his ideas regarding the calculus and, in particular, the use of actual infinitesimals, and the relation between these findings and his concept of the continuum. It would seem that Poincaré endorses most if not all of what Cournot has to say about infinitesimals.

Intuition and the Continuum in Poincaré: Some Background

Infinitesimal calculus relies on the real continuum, so we begin by looking briefly at Poincaré's ideas concerning the real continuum.[1]

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[1]This is discussed at length, for example, in Folina 1992, especially Chaps. 6 and 9. Also useful for historical background is Bell 2010.

A.J.F. de Oliveira (✉)
Department of Mathematics, CFCUL, Emeritus Professor (Évora), University of Évora, Rua dos Arneiros 28, no. 3-C, 1500-059 Lisboa, Portugal
e-mail: ajfrancoli@gmail.com; francoli@kqnet.pt

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As we know, intuition plays a relevant role in Poincaré's conception of *mathematical proof* and in all of his philosophy of mathematics, and also in his conception of the two main mathematical domains, that of arithmetic (arithmetical intuition, which gives rise to mathematical induction) and that of geometry (geometric intuition of continuity). It is with this second sort that we are concerned here.

Geometric intuition is of a *synthetic a priori* nature, much in the sense of Kant. It provides an epistemological foundation for the concept of the real continuum (in any dimension), which is neither the constructivist continuum (too trivial and restrictive in order to develop classical analysis) nor the set-theoretic continuum of Dedekind or Cantor, obtained from below (rationals) by set-theoretic means which appeal to both objectionable axioms (infinity, choice, power set) and impredicative definitions.

Poincaré does not accept such notions as that of an *arbitrary collection of subsets* of an infinite set like the set of natural numbers. For that matter, even this set as a whole entity is questionable, since he only admits the potential infinite, and because of this he is nearer to intuitionism/constructivism. On the other hand, however, he does not admit the constructivist (or any other, e.g. set-theoretic) construction of the continuum from below. For him, the classical continuum is a genuine mathematical domain, a primitive realm we are aware of in virtue of *a priori* geometric intuition. It is something which is known intuitively, via the form of experience which enables us to link our sense experiences and to possess a concept of an enduring object (so called "outer" experience). The mathematical continuum is the result of mathematical refinement of this fundamental primitive intuition. In this sense it is more classical than intuitionistic, but it is not a totality in the usual sense of set theory.

With regard to the basic notions of the infinitesimal calculus proper, namely the use of infinitesimals or infinitely small numbers (or variable quantities, or whatever, see below), let us recall that the process of rigorization (or arithmetization) of the calculus took place mainly during the latter half of the nineteenth century, but can be thought as having started earlier in that century with Cauchy and Bolzano. This process can be regarded as a three-fold process in the foundations of the calculus which comprises of:

1. the (slow) eradication of actual (numerical) infinitesimals in favor of limits in the definitions of the basic notions (continuity, derivative, integral);
2. the eradication of intuitive geometrical properties of curves in proofs of theorems of the calculus; and finally,
3. the set-theoretic construction (e.g. by Dedekind and Cantor in the early 1970s) of the fundamental set of the real numbers—the *arithmetical continuum*.

(2) and (3) are intimately related, because it turns out that one of fundamental properties of the real numbers—the *Archimedean Property*—is incompatible with non-zero actual infinitesimals, and for this very reason Georg Cantor saw no future for actual numerical infinitesimals. Neither did Bertrand Russell.

An *infinitesimal quantity* was traditionally regarded as one which is in some sense smaller than any finite quantity but not necessarily equal to zero. For applications, but also in the minds of many mathematicians (for example, Cournot, see below) an *infinitesimal (*an *infiniment petit*) is a quantity so small that its square and all higher powers can be *neglected (rigourously)*. In the theory of limits the term "infinitesimal" does not refer to a special sort of number but rather to any *sequence of numbers* or *real function* (an object of a type higher than that of numbers) whose limit is zero. An *infinitesimal magnitude*, on the other hand, was conceived as a continuum "viewed in the small," an "ultimate part" of a continuum (in any dimension), which remains continuously divisible, so it cannot be confused with a point. It is in this sense that in the seventeenth century mathematicians like Kepler held that continuous curves are "composed" of infinitesimal straight lines.

But the eradication of actual numerical infinitesimals mentioned above was not definitive, as we now know from the creation in the 1960s of *nonstandard analysis* [first, by A. Robinson, later versions by H. J. Keisler, E. Nelson and others—there are now several distinct (and even incompatible) versions of nonstandard analysis]. Of course, Poincaré could not have known that this was going to happen but, as we shall see, it is reasonable to suspect that, if it had happened in Poincaré's time, it would not have surprised him at all, in spite of the fact that he would, for the most part, simply reject the modern foundations of nonstandard analysis, probably for the same reasons that he rejected the set-theoretic foundations at the end of the nineteenth century and beginning of the twentieth.

Poincaré frequently uses notions like "infinitely small", right from his first work on the three body problem, where at a place he supposes that one of bodies is infinitely small. A clarifying passage as to his acceptance of infinitesimals can be found in Chap. II of Poincaré (1902), *La Science et l'Hypothèse*, pp. 27–28:

> No; the works of Du Bois-Reymond demonstrate it in a striking way.
>
> We know that mathematicians distinguish between infinitesimals of different orders are infinitesimal, not only in an absolute way, but also in relation to those of first order. It is not difficult to imagine infinitesimals of fractional and even irrational order, and thus we find again that scale of the mathematical continuum which has been dealt with in the preceding pages.
>
> Further, there are infinitesimals which are infinitely small in relation to those of the first order, and, on the contrary, infinitely great in relation to those of order $1+e$, and that however small e may be. Here, then, are new terms intercalated in our series... I shall say that thus has been created a sort of continuum of the third order.
>
> It would be easy to go further, but that would be idle; one would only be imagining symbols without possible application, and no one would think of doing that. The continuum of the third order, to which the consideration of the different orders of infinitesimals leads, is itself not useful enough to have won citizenship, and geometers regard it as a mere curiosity. The mind uses its creative faculty only when experience requires it.

Poincaré's attitude towards the continuum resembles in certain respects that of the intuitionists (see below): while the continuum exists, and is knowable intuitively, it is not a "completed" set-theoretical object. It is geometric intuition, not set theory, upon which the totality of real numbers is ultimately grounded.

Du Bois-Reymond believed that a full understanding of the continuum was beyond the capabilities of mathematicians. However he had already developed a theory of infinitesimals in *Über die Paradoxen des Infinitärcalcüls* ("On the paradoxes of the infinitary calculus") in 1877. He writes:

> The infinitely small is a mathematical quantity and has all its properties in common with the finite ... A belief in the infinitely small does not triumph easily. Yet when one thinks boldly and freely, the initial distrust will soon mellow into a pleasant certainty ... A majority of educated people will admit an infinite in space and time, and not just an "unboundedly large". But they will only with difficulty believe in the infinitely small, despite the fact that the infinitely small has the same right to existence as the infinitely large ...
>
> Were the sight of the starry sky lacking to mankind; had the race arisen and developed as cave dwellers in enclosed spaces; had its scholars instead of wandering through the distant places of the universe telescopically, only looked for the smallest constituents of form and so were used in their thoughts to advancing into the boundless in the direction of the immeasurably small: who would doubt then that the infinitely small would take the same place in our system of concepts that the infinitely large does now? Moreover, hasn't the attempt in mechanics to go back down to the smallest active elements long ago introduced into science the atom, the embodiment of the infinitely small? And don't as always skillful attempts to make it superfluous for physics face with certainty the same fate as Lagrange's battle against the differential? (See O'Connor and Robertson 2005)

Cournot and the Infinitesimals

Poincaré wrote many papers and sections of books on geometry and space, less on the continuum, but very little on the basic notions of analysis.[2] As far as I know, his paper "Cournot et les Principes du Calcul Infinitesimal" (1905),[3] contains a substantial amount of what is known about his views on this subject, albeit in an indirect or implicit manner. This paper is not very well known, practically it is never mentioned in philosophical analysis of Poincaré's writings, but it is very rich as a source of philosophical ideas, namely of relations between mathematics and the natural world. We must also be aware that on some occasions Poincaré changed his opinions on some subjects during his philosophical years, and that his writings are often not easy to understand without some kind of interpretation.

In this paper, Poincaré mainly comments on Cournot's points of view, as exposed in two major books: *Traité élémentaire des fonctions* ... (1841), see p. 23, and *Traité de l'enchaînement des idées* ... (1861), p. 24, which he cites profusely, but he also expresses opinions of his own on the subjects under review. My conjecture is that,

[2] A number of papers by Poincaré on philosophy of mathematics have been translated in Portuguese: Henri Poincaré, *Filosofia da Matemática. Breve Antologia de textos de Filosofia da Matemática de Henri Poincaré.* Org. and ed. by A. J. Franco de Oliveira. Cadernos de Filosofia das Ciências, 10, 2010, Centro de Filosofia das Ciências da Universidade de Lisboa.

[3] The similarity between Poincaré's title and mine is no accident.

in talking about Cournot's views, Poincaré is revealing some of his own ideas about the subject discussed, some of which are in agreement with Cournot's.

Let's begin by summarizing some of Cournot's ideas on the calculus.[4]

The so-called two methods of the calculus, that of limits and that of infinitesimals (*infiniment petits*), the Newtonian and the Leibnizian, respectively, seemed to the critics of Cournot's time to differ profoundly and not be simply reduced to a difference of notation. To Cournot himself the two methods appeared to be not merely distinct, but opposite. Each was the exact inverse of the other. How and why? The answer is found in his scheme of thought, in his philosophical tenets, and is near at hand. Cournot was a thoroughgoing realist. There was an external universe quite independent of any thinker. Kant was wrong in regarding space and time as merely conditions upon the understanding, as forms inherent in the constitution of the human mind and not in the exterior things which it perceives. Space and time, however, and other magnitudes are continuous. Natural changes, as growths, expansions, contractions, velocities, accelerations, are continuous. These proceed infinitesimally. The *natural* order is *from* the *infinitesimal* (*infiniment petit*) *to* the *finite*. This *natural* order is also (for Cournot) the *rational as opposed* to the *logical*, which latter, unlike the former, depends upon the thinker. The thinker is man, who, because of his infirmity, cannot proceed *rationally* but only *logically*, *i.e., from* the *finite to* the *infiniment petit*. Accordingly the method of limits is logical, but not rational, while that of infinitesimals is rational, but not logical. Both, however, are available for dealing with continuities. Both are rigorous, the former directly, the latter indirectly, *through* the former.

For Cournot, the distinction or opposition between the *rational* and the *logical* is fundamental and very significant, despite their etymological equivalence. Rational order holds of things considered in themselves independently of thoughts or thinkers. Logical order is merely a property of language regarded as instrument of thought. The former consists in, resides in, first principles, simplicities, which are quite independent of their discovery and out of which the complex directly arises. The latter proceeds in the inverse order, by a kind of *reductio ad absurdum,* indirectly from the complex and secondary to the simple and primary. *Reason* is, then, something absolute and would be the same for thinkers of different psychological constitution from that of man. *Cause,* on the other hand, is relative, has a double origin, physical and psychological. Consequently all truths, all verities, be they mathematical theorems or physical phenomena, have their *reason,* but only phenomena have their *cause*.

Cournot mentions the notion of "infinitely small" in the Preface (pp. viii, x), which he attributes to Leibnitz, but never defines "infinitesimal" or "infinitely small" before using this notion for the first time on p. 81 of the *Traité élémentaire*.

Let us now turn to Poincaré's 1905 paper on Cournot.

[4]For a more extensive account see the review by Keyser 1905.

Poincaré on Cournot and Infinitesimals

In his paper, Poincaré begins by saying that he has difficulty in understanding past controversies over the principles of the calculus, and that "we are willing to see no more than a difference of notation between the two founders of the integral calculus [Newton and Leibnitz]". Cournot had written (Preface, p. ix of *Traité*) that both theories complement each other.

Poincaré refers to "Cournot's theory of compensation of errors" as a simple answer to objections by philosophers [Bishop Berkeley, Bernard Nieuwentijdt], but I did not find this theory developed in Cournot's *Traité*. Perhaps he was thinking of the author of such a theory, which was not Cournot but the earlier (by 50 years) Lazare M. Carnot (1753–1823), in *Réflexions sur la Métaphysique du Calcul Infinitesimal*, see p. 22, Carnot (1970). For Poincaré, the remaining doubts were lifted by "what has been called the arithmetization of mathematical analysis". But this process, essentially of a logical nature, had the consequence of separating mathematics away from nature, so there still remains room for philosophical indagation as to whether "the procedures of the differential and integral calculus, nowadays completely justified from the logical point of view, can be legitimately applied to nature. The continuum that is offered to us by nature and that which is in a certain manner a unity is similar to the mathematical continuum".

If we admit that natural phenomena can be represented by numbers and mathematical functions, the rules of the infinitesimal calculus can be applied to these functions. However (my translation), "What the observation gives us directly, is not a number, it is a feeling which is not itself expressible by a number since we cannot distinguish it from other neighboring feelings (. . .) physical continuity consists precisely in this kind of fusion of the elements in close vicinity. (. . .) We see that as our observation aids improve, the boundaries between which the number representative of an unspecified natural phenomenon must remain become increasingly narrow, but the smaller and smaller gap in-between will never become rigorously null. We believe however that this progress has no bounds, that we will never be able to say, for example: a weight could never be evaluated with a margin of error less than one thousandths or of one millionth of a milligram. This is precisely the postulate which we admit implicitly when we apply the laws of mathematical analysis and in particular those of the infinitesimal calculus to nature."

Next, Poincaré makes these points more precise by means of some examples from science, and concludes: "In this way the physicist can always apply the rules of the calculus without fear a rebuttal from experience". Before beginning his citations and comments on Cournot, he argues to the effect of dismissing other conceptions of the real world, namely those that regard the world as discontinuous (such as those of atomistic philosophers like Évellin (1881) and J. Bertrand).

Then there come two long citations of Cournot, with the aim of explaining Cournot's position with respect to these problems. A summary of these was given above, but it is interesting and instructive to read the whole texts as cited by

Poincaré. In this section, all citations from either Poincaré or Cournot are translated by me; those by Cournot are rendered in italics so as to be distinguished from those by Poincaré.

> Indeed, he says (Théorie des fonctions,[5] t. I, p. 85), if we could compare, from the beginning, the method of limits and the infinitesimal method, we would see that both tend to the same purpose, which is to express the law of continuity in the variation of quantities, but they do so by means of inverse processes. In the first method, given a question on the quantities that vary continuously, it is first assumed that they change suddenly from one state to another; and then we search what happens when we tighten more and more the interval which separates two consecutive states. It is clear that only in retrospect can we obtain in this way the simplifications which result from the continuity ...
>
> In any case the infinitesimal method is not just an ingenuous trick; it is the natural expression of the mode of generation of the physical quantities which grow by smaller elements that any finite quantity. Also, (...) when a body cools down, the relationship between the elementary variations of heat and time is the reason for the relationship which exists between the finite variations of these very quantities, the term reason being taken here in its philosophical meaning.
>
> From this point of view, one was able to say justifiably that the infinitely small exist in nature, and it would certainly be advisable to call $f'(x)$ the generating or primitive function, and $f(x)$ the derived function, contrary to what Lagrange did.
>
> In short, the infinitesimal method is better appropriated to the nature of things.
>
> It is the direct method, from the objective point of view. On the other hand, the concept of the infinitely small can only be defined logically in an indirect manner by means of the limits[6]; so that from the logical and subjective point of view, the conclusive rigor belongs directly to the method of limits and indirectly to the infinitesimal method, while the latter becomes, using certain definitions of words, a pure translation of the first.
>
> "Geometers have another way of expressing the same thing", he says again (L'enchaînement des idées fondamentales,[7] t. I, p. 87). (...) It would be wrong to see in this expression of infinitely small nothing more than an abbreviation agreed upon, a form of language, apparently more convenient because it is most commonly used. (...)

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[5] *Traité élémentaire de la théorie des fonctions et du calcul infinitésimal,* 2 volumes, Paris, Hachette, 1841.

[6] It is not clear exactly how this is supposed to have been done. Perhaps Cournot had in mind A. Cauchy's (*Cours d'Analyse,* 1821) definition of infinitesimals in terms of variable quantities (such as sequences) tending to zero, or L. M. Carnot's remark "We will call every quantity, which is considered as continually decreasing, (so that it may be made as small as we please, without being at the same time obliged to make those quantities vary the ratio of which it is our object to determine,) an Infinitely small Quantity." (*Réflexions ...,* 1797, 1821, English translation 1932, §14), or later "the difference between any quantity and its limit is exactly that which we should call an infinitely small quantity" (*Réflexions ...,* §100). Defining infinitesimals in terms of limits turns out to be an idea which is difficult to reconcile with the idea of an *actual* infinitesimal number or quantity.

[7] *Traité de l'enchaînement des idées fondamentales dans les sciences et dans l'histoire,* 2 volumes, Paris, Hachette, 1861.

And again (*ibid*., p. 37):

> A body that moves from the rest position begins by having an infinitely small velocity; at the same time it remains contrary to reason that there is in the world today a body animated by an infinitely large velocity.
>
> All that is infinitely small escapes our observation, but not the conditions of natural phenomena; everything that is infinitely large escapes both our observations and the actual conditions of the production of phenomena.

Note the asymmetry between the infinitely small and infinitely large, at least as regards our observations of nature or of moving bodies.

Poincaré also comments on this: "It seems that the sharpness of these quotes leaves nothing to be desired. The infinitely large can have no actual existence, but it is not the same with the infinitely small. On the contrary, from the objective point of view, the infinitely small pre-exists to the finite. It is our human logic which proceeds from the finite to the infinitely small, nature always proceeds from the infinitely small to the finite. Newton remained faithful to human logic, Leibnitz was closer to nature. They therefore complement each other; the former could not give us but an imperfect image of the world, the latter could not do without what he borrowed from the former, or, in his own way, without which he would have been lacking in demonstrative rigor".

Does this citation represent some kind of "approval" or endorsement on the part of Poincaré? In any case, Poincaré admits that infinitesimals can have actual or objective existence, but can this existence be accepted mathematically? From a mathematical point of view, if we wish to have an extended (Leibnizian) mathematical continuum which comprises non zero infinitely small numbers, and we wish to preserve as many rules of calculus as possible, then the non-zero infinitesimals are invertible and their inverses are the infinitely large numbers. Certainly in the history of the calculus up to the middle of the nineteenth century the question was hardly raised, if at all.

After discussing and refuting again some arguments that come from atomistic positions, Poincaré goes on to say:

> So these infinitely small that are the true reason of things are not atoms and, on the other hand, they are also not becoming, as they are rationally prior, so to speak, to observable finite quantities. Leibnizian infinitesimals, it is true, are only just becoming, or at least do not play any role in mathematical reasoning, this is where the infinitesimal method becomes 'a pure translation of the method of limits'.
>
> (. . .) The opposition between the logical and the rational order is an idea that is frequent in Cournot. The human mind is forced to rise from the given data, which is complex, to the principles, which are simple; this is the logical order that is imposed on us by the weakness of our intelligence; it is the order of discovery, but we do not possess perfect knowledge until we retreat from simple principles to complex consequences, following the rational order, that is to say the order that is adopted by Nature itself. (. . .)

And further down (p. 64):

> The rational order should not be confused with the logical order, although one of these words has the same root in Greek that the other has in Latin. The rational order belongs to things, considered in themselves, the logical order is the order of the language, which for

us is the instrument of thought . . . We distinguish very well among the proofs of the same theorem, all blameless in terms of the rules of logic, those which give us the true reason of the theorem proved, that is those which follow in logical sequence of the propositions the order in which the corresponding truths are generated, as one is the reason for the other. Accordingly, we say that a proof is indirect when it reverses the rational order, when the truth obtained as a consequence in the logical deduction is conceived by the mind as contradicting the truths which serve as logical premises.

The typical indirect proof is obviously the proof by *reductio ad absurdum*. Cournot is well aware that the method of limits leaves nothing to be desired from the point of view of mathematical rigor and that in reality it comes down to a *reductio ad absurdum*, identical to the exhaustion method of the ancients.. (. . .)

Let us therefore rely on the indirect method to return to the principles; but let us not hasten back as soon as we can, to the direct method, the method conforming to the rational order that makes us know the real reasons of things.

One may wonder what meaning Cournot attached to this word reason, but he takes pain to explain it to us and distinguishing the reason from the cause. (. . .) So the cause is something relative, which depends on the psychological constitution of the thinking subject; reason, however, is independent of the subject, it is something absolute. For Cournot, who does not hesitate to believe in an external world whose existence is quite independent of the subject, this means that the cause is only an appearance, and the reason is the reality. (. . .)

And the infinitely small are the reason of things, but it seems at first that the main difficulty is not even suspected. These infinitely small, the reason of things, are they perpetually becoming, like the Leibnizian infinitesimals? For those of us who do not believe in the possibility of conceiving an external world independent of the thinking subject, this would be the easiest and most natural solution. The primary reason always flees before the mind who seeks it but can never reach it, and it would be the Leibnizian infinitely small which would best symbolize this eternal flight.

But there is nothing that allows us to assign such a thought to Cournot; (. . .) So when Cournot says that the infinitely small are the primary reason of things, it is indeed a primary reason placed outside of us, it is not an indefinitely small, and as it is not an Évellinien atom, it must be an actual infinitely small. The contradiction that most minds believe they see in the actual infinite did not concern Cournot; he did not take this objection to be of any great value.

It has often been repeated, he says (loc. cit., p. 37)*, that the idea of the infinite has a mathematical purely negative value . . . Arithmetic gives me the idea of the infinite in the sense that nothing limits the series of numbers; this is nothing but a negative idea, if you will, it is just the timely idea of the indefinite rather than that of the infinite. But when I conceive the infinity of time and space, it is really an infinite in* actuality*, necessarily imposed on my mind and of which I have a clear idea, although I can make an image or representation of it. If it is the continuous movement which involves the* effective *existence of an infinite number of intermediate positions, I have not only a clear idea, but a representation of the phenomenon.* Here we could argue that what we can represent is the physical continuum, very different, as we have explained, from the mathematical continuum.

Anyway, Cournot's thinking seems clear; the contradiction that we believe to have found in the notion of actual infinite is merely apparent; and it is only due to the weakness of our minds, it exists only in the logical order logic is alien to the rational order.

Such a solution certainly does not satisfy everyone, and I do not think, however, that any realistic solution could satisfy everyone, but I find sufficient to have just highlighted the philosopher's true thought; it only remains for me to find out how he justified it in his own eyes. Up until now we have seen only claims, it is time to see the reasons that support them. Where does this tranquility with which he believes to have discovered the real reasons for things come from?

It is the case that he believes that above formal logic there is another logic (*loc. cit.*, p. 3) by means of which we realize the reasons for distinguishing the essential from the accidental, the absolute from the relative, the reality from the appearance. What device do we have for distinguishing the absolute from the relative motion; for example, why do we prefer the Copernican system to that of Ptolemy, it is because it is simpler, and we conclude that not only that it is more convenient, but also that it is more real (*loc. cit.*, p. 18). (. . .)

However, Newton's law lets us know the infinitely small variation in velocity suffered by celestial bodies under the influence of their mutual attraction in an infinitely small time lapse. Kepler's laws, on the contrary, let us predict the finite variations of that very velocity in a finite time. And as the same difference in simplicity is found in all physical problems, we must conclude that it is the infinitely small, that is to say, the simple, that is the reason for the finite, that is, the complex.

If you wanted to replace the gentle ramp of Leibnizian continuum by the Évellinien stairs, however numerous and close the steps you would never find the same simplicity, because the magnitude, ceasing to be continuous, in the mathematical sense of the word, would cease to be homogeneous, since everything could remain similar to the part. And then we would have to admit that it is the simple, that is to say, the continuous, which is the appearance, and the complex, that is to say, the discreet, that is the reality. We should then believe that the glass through which we see objects gives them a simplicity that is not theirs.

This seems impossible to Cournot. In summary, it is the belief in the simplicity of nature, a belief itself based on the principle of sufficient reason, from which he draws his conviction.

If we complicate a formula, he says (loc. cit., p. 104), as new facts are revealed to the observation, it becomes less and less probable as a law of Nature . . . If, on the contrary, the facts acquired in observations subsequent to the construction of the hypothesis are well connected by it, especially if the facts predicted as consequences of the hypothesis are subsequently confirmed, the probability of the hypothesis can increase up to leave no room for doubt in an enlightened mind.

This simplicity, this symmetry that becomes the *criterion* of certainty, cannot be met except in the mathematical ideas of order and form. This is where Cournot finds '*the pre-eminence and the role of the mathematical sciences. Mathematics is the science par excellence, the most perfect example of scientific form and construction.'* The world, in short, should be simple and it cannot be so if it is built on the model of mathematical quantity.

Conclusion

If infinitesimals are the reasons of things and exist in nature (something that Poincaré does not deny nor criticize in any way), should not mathematics try to diminish the "gap" between the rational or natural order and the logical or mathematical?

I have tried to find affinities between Cournot and Poincaré on the principles of the calculus. Although I cannot affirm this with certainty at this point, I have the feeling (or intuition) that it is so, as a working hypothesis.

One of the assets of nonstandard analysis is precisely that of a better modelization of natural phenomena. Poincaré saw this to be true in the practice and philosophy of mathematicians that lived before his time, in the form of infinitesimals, at the hands of Cournot and others, and, it seems to me, that at least he was sympathetic towards these notions and methods. How much of this he practiced himself remains to be investigated.

References

Bell, J. 2010. Continuity and infinitesimals. *The Stanford encyclopedia of philosophy*, Fall 2010 Edition, ed. E.N. Zalta. http://plato.stanford.edu/archives/fall2010/entries/continuity/. Accessed 20 July 2010.

Carnot, L.M. 1970. *Réflexions sur la Métaphysique du Calcul Infinitesimal I, II*, Seconde Edition. Paris: A Blanchard; Paris: Gauthiers-Villars, 1921.

Évellin, F.J.M.A. 1881. *Infini et quantité: Étude sur le concept de l'infini en philosophie et dans les sciences*. Paris: Germer Bailliére et cie.

Folina, J. 1992. *Poincaré and the philosophy of mathematics*. London: Palgrave.

Keyser, J.C. 1905. Review. *The Journal of Philosophy, Psychology and Scientific, Methods* 2(16): 443–445.

O'Connor, J.J., and E.F. Robertson. 2005. *Paul David Gustav du Bois-Reymond*. http://www-history.mcs.st-andrews.ac.uk/Biographies/Du_Bois-Reymond.html. Accessed 15 Nov 2010.

Poincaré, H. 1902. *La Science et l'Hypothèse*. Paris: Flammarion (Reed. 1968).

Poincaré, H. 1905. Cournot et les Principes du Calcul Infinitesimal. *Revue de métaphysique et de morale* 13: 293–306. Repr. in *Dernières Pensées* since 1926; Also repr. as Chap. IX, (107–115) of H. Poincaré, *Scientific Opportunism/L'Opportunisme scientifique, An Anthology*. Compiled by L. Rougier, edited by L. Rollet. Birkauser, 2002.

Does the French Connection (Poincaré, Lautman) Provide Some Insights Facing the Thesis That Meta-mathematics Is an Exception to the Slogan That Mathematics Concerns Structures?

Gerhard Heinzmann

Abstract There are at least two versions of modern structuralism and each has its proper difficulties: if one adopts the *in re* version, the crucial feature is that the background ontology is not understood in structural terms; if one adopts the *ante rem* version, the crucial feature is that the talk about structures is exposed to a kind of third man objection.

The main thesis of this paper is that Poincaré's conventionalism and Lautman's structuralism must be ranked among these sources of structuralism that try to escape the mentioned difficulties.

Poincaré uses a psycho-physiological approach in order to justify his conventionalism in geometry, which is an improvement of an attenuated version of *ante rem* structuralism, and Lautman proposes a metaphysical dialectic in order to justify his anti-foundationalist position, which brings *ante rem* and *in re* structuralism together. Poincaré's approach fails for technical reasons whereas Lautman's approach fails for its aporetic conceptual vagueness.

My present concern is to incorporate the French historical inheritance in the systematic discussion of mathematical structuralism.

Introduction

I first give a short outline of standard results on structures from a philosophical point of view as they can be find in the works of Shapiro 1997, Resnik 1997 or Chihara 2004.

I shall call a domain of objects together with certain functions and relations on the domain satisfying certain given conditions a "system". A special group, e.g.

G. Heinzmann (✉)
Laboratoire d'Histoire des Sciences et de Philosophie—Archives Henri Poincaré (UMR 7117), Université de Lorraine/CNRS, 91, av. de la Libération, F-54001 Nancy, Cedex
e-mail: Gerhard.Heinzmann@univ-lorraine.fr

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Poincaré's group of displacements is a system. The abstract form of a system is called a structure. Under structuralism I understand the philosophical thesis that mathematics is not concerned with any particular ontology but with structures or with systems that share a common structure.

According to the structuralists' account, mathematical objects are places in structures. What is the difference between "place" and "object"? There are different possibilities to answer this question and, consequently to interpret the relation between structure and systems that exemplify them.

The first position says that places are offices and not officeholders. The "places are-offices perspective" presupposes a background ontology that supplies objects that fill the places of the structure. A structure is anything that can be exemplified by a type of systems, but there is no such thing as "the structure". The number structure is a pattern common to all number systems, which are not necessarily isomorphic. This position is a so-called structuralism without structures, and it is in this sense anti-realistic and eliminative. The eliminative structure program paraphrases places-are-objects statements in terms of a places-are-offices perspective with respect to systems different from the structure. For an anti-realistic structuralist place-are-objects statements are not to be taken literally: the apparent singular terms mask implicit bound variables. They are disguised definitions. Talk of numbers is convenient shorthand for talk about all systems that exemplify the structure. Talk of structures generally is convenient shorthand for talk about systems. The essence of a number being just its structural relation to other numbers, anything at all can be "4" when it occupies the place corresponding to the office "4" in a system exemplifying the natural number structure. But we cannot and need not answer the question whether $1 \in 4$ (von Neumann's notation) or not (Zermelo's notation). We have no objects with an internal composition so that the last question is a meta-mathematical one concerning the background-theory, which is normally the set hierarchy V. In other words, the back-ground theory cannot be a mathematical theory if mathematics is considered in an anti-realistic structural way. Otherwise, one should find a background-theory for the set theoretical structure etc.

The second interpretation of the token-type relation between systems and structure is a realistic one: tokens can, so to say, be destroyed. Contrary to the *in re* structuralist, the *ante rem* structuralist takes the pattern to exist independent of any systems that exemplify it. The structure is prior to the mathematical objects it contains. For the *ante rem* realist the distinction between position and object is a relative one. The idea is that the places of the natural number structure, considered from the "places-are-objects perspective", can be organized into a system, and this system exemplifies the natural number structure whose places are now viewed from the "places are offices-perspective". The "places-are–office perspective" refers here not to a system different from the structure. So, in a sense, each structure exemplifies itself. When we invoke the "places-are object-perspective", in "2 is $\{\{\varnothing\}\}$" the "is" is an identification, in contrast, when we invoke the "places are-offices perspective", the "is" constitutes a copula relative to a system exemplifying the structure.

The problem raised by this interpretation is the status of a structure as type. Are there some identity-conditions? Is a structure an entity? The answer is surely: no! But what does it mean, then, to speak of a structure?

Poincaré

Now, concerning Poincaré, it is obvious that he was strongly influenced by and attuned to a philosophical movement consisting of a mixture of positivism and Neo-Kantianism, namely the so-called "Boutroux Circle" (Nye 1979).[1] The members of the circle criticized at the same time Comte's determinism and Kant's static view of the mind's structure. The existence of consistent non-Euclidean geometries, used by Poincaré in order to overcome difficulties of the theory of "real" geometry as a tool in function theory, leads him to study "the structural relations between Euclidean and non-Euclidean geometry" (Nye 1979, 111). Whereas the existence of different geometries prevents one from considering geometric propositions as necessary truths determined by a priori intuitions, mathematical exactness and the impossibility of defining distance empirically prevent one from considering geometric propositions as empirical descriptions. This is why Poincaré introduces "propositions" in the core of his theory of mathematical knowledge not as genuine bearers of general semantic information, but as hypotheses. The axioms of metric geometry are *apparent hypotheses,* i.e. conventions, neither true nor false. The introduction of this formal and decisional aspect in mathematics was held always as the modernist aspect of Poincaré's philosophy of science. On the contrary, it is little known that Poincaré should be ranked with Duhem among the forerunners of Quine who survive his criticism of logical empiricism:

(a) for his conception of geometrical conventions as a kind of bicephalous selection of analytical but non-logical propositions, "guided" at the same time by experience.[2]
(b) for his "relationalism" which in certain aspects comes close to Quine's doctrine: both attempt to account philosophically for the incompleteness of (scientific) objects. Poincaré argues that what science can attain

> is not things themselves, as the dogmatists in their simplicity imagine, but the relations between things; outside those relations there is no reality knowable. (Poincaré 1902, XXIV)

What are the links between Poincaré's relationalism and the geometrical conventions? They are nothing but two different aspects of Poincaré's structural approach. Concerning the relational aspect, David Stump remarks:

[1]Cf. for the following (Heinzmann/Stump 2013).

[2]Because Poincaré distinguished very well between analytic and synthetic sentences, the analogy to Quine must be restricted to conventions.

> Poincaré and Hilbert argue for a new conception of geometrical systems [. . .] Poincaré holds that outside of the context of an axiomatic system, geometrical primitives mean nothing [. . .] He argues that geometry concerns only the relations expressed in the axioms, and not some inherent features of the primitives [. . .] The set of relations that holds between the primitives constitute the form, not the matter, of geometric objects, and these are what is studied. (Stump 1996, 482–84)

Geometry is the study of the form of the group together with its properties. This form (=structure) of the group preexists in our minds, and certain relations of our experience are represented by *apparent hypotheses* or *conventions.* The empirical objects satisfying the relations as matter of the form are described by *indifferent hypotheses*. They concern the *ontological* but not the structural determination of elementary phenomena. Such indifferent hypotheses are mere metaphorical crutches, useful for thought but "unverifiable" and "useless" as such (Poincaré 1902, 156). They are conventional in the usual sense of the word; that is to say, they are "arbitrary," but compelled by rational agreement. In general, the ontological determination of singular objects is, from a scientific perspective, an over-determination: scientific objectivity is purely relational, while the *relata* remain inaccessible to human knowledge.

Thus, the form of the group constitutes the relational aspect of Poincaré's structuralism. But what does it mean that the relational form is described by apparent hypotheses or conventions?

Poincaré's approach is not identical with Hilbert's axiomatic approach. His often quoted structural *Credo,* saying that in mathematics the word "existent" means "exempt from contradiction" (Poincaré 1902, 44; 1905a, 819), must be seen under a non-Hilbertian light. For reasons concerning above all the involvements of impredicative procedures, Poincaré excludes proving mathematical reliability by a consistency proof in the Hilbertian way. He takes a structural position without completely disengaging meaning and knowledge from ostension. Nevertheless, Poincaré begins his alternative reliability construction only *apparently* with sensations as ostensive contacts with the given. In reality, he introduces, similarly to Helmholtz's conception of intuition as imagined sensible impressions, a representation of a two-places sensation relation, based on the *imagination* of single sensations.[3] They are the office-holders of the categories (forms) of sensible space and of groups. Poincaré's conventions in geometry are the tools to close the gap between the *exactness* of a structure and the *objectivity* of sensation-relations based on an imagined ostensive contact (*reflecting on sensations*). If this interpretation is right, then Poincaré's concept of structure is not the new Hilbertian one deriving from his axiomatization of Euclidean geometry, but constitutes a development of the

[3]Representation of an object in the sensible space means nothing else than the deliberate and conscious reproduction of muscular sensations *thought* necessary to reach the object: "When it is said, [. . .] that we "localise" an object in a point of space, what does it mean? It simply means that we *represent to ourselves* these movements that must take place to reach that object [. . .] When I say that we represent to ourselves there movements, I only mean that we *represent* to ourselves the muscular sensations which accompany them" (Poincaré 1902, 57; *our emphasis*).

traditional algebraic one and concerns continuous groups. His epistemological project has a strong affinity with Schlick's *General Theory of Knowledge*.

By recognizing Poincaré's legacy for the structural point of view, Schlick insisted on the conventional aspect of his structuralism. He saw in Poincaré's conventions a third type of definitions between the axiomatic or implicit and concrete or ostensive ones. These conventions are, as (Friedman 2007, 100) observes, "crucial for an understanding of how we achieve a coordination between concepts and empirical reality in the mathematical exact sciences". I quote Schlick:

> To define a concept implicitly is to determine it by means of its relations to other concepts. But to apply such a concept to reality is to choose, out of the infinite wealth of relations in the world, a certain group or complex and to embrace as a unit by designating it with a name. By suitable choice it is always possible under certain circumstances to obtain an unambiguous designation of the real by means of the concept. Conceptual definitions and coordinations that come into being in this fashion we call conventions (using this term in the narrower sense, because in the broader sense, of course, all definitions are agreements). It was Henri Poincaré who introduced the term convention in this narrower sense into natural philosophy; and one of the most important tasks of that discipline is to investigate the nature and meaning of the various conventions found in natural science. (Schlick 1979, 91f)

So, according to Schlick, Poincaré's conventions combine conceptual definitions and coordinations.

How should one understand this affirmation?

An answer can be found by examining four steps in Poincaré's psycho-physiological reconstruction of the genesis of geometrical space. It gives an instantialist view of geometrical relations. In his early articles, Poincaré argues that geometry concerns only the relations expressed in the axioms, and not some inherent features of the primitives: "What we call geometry is nothing but the study of formal properties of a certain continuous group; so we may say, space is a group" (Poincaré 1898, 41).

The first step of Poincaré's construction of geometrical groups proceeds from the *observable* fact that a set of impressions can be modified in two distinct ways: on the one hand without our feeling muscular sensations, and on the other, by a voluntary motor action accompanied by muscular sensations. So, similarly to Carnap's Aufbau, the starting point is here the definition (guided by experience) of two two-place relations: an *external chance* α (with '$x\ \alpha\ y$' for '*x changes in y without muscular sensation*') and an *internal change S* (with '$x\ S\ y$' for '*x changes in y accompanied by muscular sensations*').

In the second step, he introduces a classification of external changes, some of which can be compensated by an internal change, while others cannot. The first are called **changes of position**, the second **changes of state**. One presupposes that the compensation is by convention exact and not approximate. In the third step, Poincaré defines, *modulo* an identity condition with respect to the compensation by internal changes, the equivalence class of changes of position, and calls it a *displacement* (see Poincaré 1905b, Chap. IV):

1. Two internal changes have to be considered identical if they have induced the same muscular sensations.

2.
$$\left.\frac{\alpha, \beta \text{ external}}{S \text{ internal}}\right\} \text{changes}$$

$$\alpha \sim \beta \quad \textit{iff} \quad \exists S \quad \left(S\alpha \doteq S\beta \doteq I\right)$$

that means: two external changes are equivalent if they possess a common character (i.e. they can be canceled by S).

3.
$$S \approx S' \quad \textit{iff} \quad \exists \alpha \quad \left(\alpha S \doteq \beta S' \doteq I\right)$$

4. If $\sim$, $\approx$ is an equivalent relation, then the equivalence class of the changes of position is a *displacement*. So we can recognize that two displacements are identical.

The fourth step and Poincaré's main result is that each set of displacement classes (external and internal) forms a group in the mathematical sense.

The group of displacement is in fact an adjustment to the general group preexisting in our mind as a form of our understanding, which the specific displacement-structure (=transformation group) exemplifies. In other words, the form in the mind leads to a special kind of platonic universals or *ante rem* structure. Thus, the genesis of geometry is based on an epistemological process founded on previous classifications, carried out as a relationship between a structure as norm of invariance and conventional adjusted systems as instantiations or exemplifications of these norms (the sensation compensation are only approximate). The exemplification of the group-structure by a variety of systems (=element of harmony) is not a logical but an esthetical operation without an explicit identity (harmony) criterion (=mathematician as artist). This is why the exemplification of structures and the esthetic perspective are "solidary" [VS, Chap. V].

Now, the special variant of convention, where there exists a choice between different possibilities, only becomes involved at a further step of the sensory-mathematical construction where the properties of the transformation group are studied and decisions are taken concerning the distance. It follows that the axiom of Euclidean distance is a conventional definition influenced by simplicity and commodity and guided as a whole by experience: it is a *disguised definition* or an *apparent hypothesis*. Poincaré uses the term *disguised definition* up to 1899, the year of Hilbert's famous *Foundations of Geometry*, to express the fact that language apparently used descriptively is not so in actuality. Certain axioms appear descriptive, but instead constitute the only way to *define* certain entities (see Poincaré 1899, 274). Such entities are found to be defined only up to structural equivalence: they reflect well the truth of certain relations between relata whose qualities remain—as according with Helmholtz and others—unknowable.

Contrary to *in re* structuralists, Poincaré's structure of a general group is not ontologically, but rather epistemically dependent on its instances. Contrary to *ante rem* structuralists, Poincaré doesn't speak of the structure as such but uses it as a metamathematical tool for his psycho-physiological genesis of *real* actions with *imagined* sensations: the structure is not itself a position in a meta-structure but the psycho-physiological procedure is the *ratio cognoscendi of* its existence in our mind. In this sense geometry is as such a whole system, understood by a *pragmatic* procedure, which is irreducible to a combination of clearly distinguished parts of conceptual analysis and aesthetic exemplifications.

As Philippe Nabonnand remarked, Poincaré's presentation of geometrical space is as a whole circular:

> in his 1898 paper, [he] put forward a (mathematical) explanation of the three dimensions of space. He observed that the Euclidean group, selected after many conventions, can be seen as acting on a space of three, four or five dimensions. The choice of a three-dimensional space is justified by considerations of commodity. Unfortunately, Poincaré's argument is vicious because the choice of the Euclidean group was grounded on Lie's classification of transformation-groups operating on R^3. (Heinzmann and Nabonnand 2008, 171)

Nevertheless, Poincaré noted his mistake and introduced in 1905 (VS) a three-dimensional physical continuum in order to justify his utilization of Lie's classification. The consequence is that Geometry is no longer independent of any mathematical space (Nabonnand). The structure of space must be presupposed as a primitive notion, contrary to the pragmatically suggested group notion existing in our mind!

Lautman

Between 1930 and 1940, three PhD students and friends at the *Ecole Normale Supérieur* at Paris, having a common interest in logic and philosophy of mathematics, were a driving force leading to the work of Bourbaki, and they had the common fate to disappear prematurely. Jacques Herbrand was a mathematician, Albert Lautman a mathematically well-trained philosopher, and Jean Cavaillès a philosopher and historian of set theory. Lautman is less well known than Cavaillès as a scientist (Jean Petitot wrote in 1987 one of the first articles on Lautman) and as a resistance fighter: nevertheless, like Cavaillès, he was killed in 1944 by the German occupying power.

Albert Lautman defended his PhD in 1937 with a principal and a complementary thesis, entitled respectively *Essai sur les notions de structure et d'existence en mathématiques* and *Essai sur l'unité des sciences mathématiques dans leur développement actuel.*[4] He shared with Poincaré the opinion that formalism and intuitionism fail together as reliable positions on the foundations of science, that is, as philosophical views of the nature of scientific objects and of scientific understanding (Lautman 2006, 181). His purpose was to solve the Hilbertian problem of the conflicts echoed in mathematical practice through the structural method used in Algebra and the constructive method, conceiving the real numbers

[4] Reprinted in (Lautman 2006).

and the operations of Analysis as generalizations from number theory. The tool he imagined is an *adequate* interpretation of the structural method so that the conflict in fact disappears in favor of the algebraic method (Lautman 2006, 87).

Lautman's intuitionistic opponents were Pierre Boutroux and Maximilien Winter, who formulated their theses in the books *l'Idéal scientifique des mathématiciens* (1920) and *La méthode dans la philosophie des mathématiques* (1911). Boutroux considered "independent mathematical entities with respect to the theories where they are defined." Speaking of "algebraic or logical clothes by which we seek to represent such a being," he presupposed, according to Lautman, a kind of neutrality of the formalism with respect to that which is formalized.

There was also a formalist opponent to Lautman: naturally, this was not Hilbert, but Carnap and the Vienna Circle around 1937. As did Cavaillès and Herbrand, Lautman went in the late twenties to Germany (Berlin, 1929). The French neo-Kantian tradition, enriched with the German experience of the fertility of structural relations, led him to oppose the reductionist and "static" character of Logical Empiricism. Theories, rather than isolated concepts or primitive notions linked by primitive logical propositions, have to be objects of the scientific philosophy. Mathematical reality should not be conceived as "being static" but as the result of the possibility of determining certain beings from one other, i.e. the result of a set of links (Lautman 2006, 226).

Lautman distinguished two points of view of the concept of structure: the *syntactic* or genuine structural perspective, and the *semantic* or extensive perspective. This distinction is identical to or at least very close to our modern *ante rem—in re* distinction (Lautman 2006, 66). Both perspectives belong, according to Lautman, to metamathematics: the first concerns the construction "of certain perfect structures, [. . .] and this regardless of whether there are concrete ("effective") theories having the properties in question" (Lautman 2006, 131). Lautman associated with this *syntactic* structural (*ante rem*) perspective such proof theoretic properties as "provable", "refutable", "irrefutable" or "non-contradictory".

The semantic perspective, concerning the existence of interpretations, uses the extensive processes of set theory by considering the fields of individuals that can serve as values to arguments in a formula of the theory. The semantic properties associated with this perspective are validity, satisfiability etc. (Lautman 2006, 182). According to this *in re* interpretation, the properties of mathematical beings are of a structural kind, exemplified by different systems. Take, for example, the property of divisibility of the number 21. If the domain is the field K of rational numbers, the result is: 3, 7; if it is the field K $\sqrt{-5}$, the result is: 3, 7, $(1+2\sqrt{-5})$, $(1-2\sqrt{-5})$.

The question then arises, how did Lautman conceive the relation between the two perspectives on structure? The answer can be found by analyzing the split between the old *genetic* method and the new structural method in mathematics with respect to the relation between essence and existence (Lautman 2006, 65). According to the classical point of view, the question concerning this relation is still asking about the same being. According to the structural point of view, by contrast, when the transition from essence to existence is possible, it always concerns the passage of one kind of being to another kind of being. For example, according to the classical point of view in Analysis, the relationship between "discontinuous" and "continuous" or between "finite" and "infinite" is conceived as an expansion of the finite (discontinuous) or by the narrowing of the infinite (continuous), where the finite (discontinuous) is still considered in extension as a part of infinity (continuity). On the contrary, the structural point of view sees in the finite and the infinite not two extremes of a move to make, but two distinct kinds of being, each with its own endowed structure, supporting relations of similarity between them. But how should one compare them? At first glance, Lautman's answer sounds very vague: "By focusing on the frame of beings (armatures des êtres), which are compared, one indeed discovers between the finite and the infinite an analogy of structures" (Lautman 2006, 122/123). How can we speak about structures?

Lautman's answer is based on a more liberal concept of "structural content": to conceive "a structure whose elements are neither *entirely arbitrary* nor *built up really* but conceived as a *mixed form* that derives its fruitfulness of its dual nature" (Lautman 2006, 46).

In perspicacious way, Lautman identified the completeness theorem of the predicate calculus as a trivial technical realization of the intended dialectic between essence and existence, which is inadequate to be extended to more complex theories (Lautman 2006, 183/184). Naturally, when the system is not complete, there is no equivalence between the non-contradiction of the system and the existence of an interpretation of this system. The existence of a model "is a stronger requirement than non-contradiction, so that there will be a dissociation between the [genuine] structural view and the extensive point of view" (Lautman 2006, 184).

Now, in order to understand the internal unity of the *ante rem* and *in re* perspective on a structure, Lautman uses a biological metaphor: He notes: "It is obvious that the mathematical entity as we understand it is not unlike a dynamic living thing" (Lautman 2006, 140). However, the structural conception and the dynamic conception of mathematics seem at first opposed: "one tends, he says, to consider a mathematical theory as a whole, [. . .] independent of time, the other on the contrary does not separate the temporal stages of its development" (Lautman 2006, 130).

Hence Lautman's vision considered structures from a distinctive perspective, nearer to mathematical practice: the mathematical solutions of the problems they pose should contain an infinite number of degrees. Partial results and comparisons, stopped halfway when organized under the unit of the same theme, could perhaps, in their movement, manifest emerging links between abstract ideas. These links Lautman proposed to call "dialectical" (Lautman 2006, 131). He tried to develop a conception of mathematical reality that would combine the two kinds of structuralism with the life-metaphor attributed to theories. The understanding of a mathematical entity must involve two reciprocal aspects: "the essence of a form being realized in a matter created by the form, and the essence of a matter giving rise to the forms drawn by the structure of the matter" (Lautman 2006, 186).

Like Poincaré, Lautman viewed the ontological commitment as concerning relations and not objects, and the mathematical activity or experience as the *ratio cognoscendi* of a structure, determining within this process new elements. Contrary to Poincaré, however, he viewed the relations in question as only regulative, and saw no fixed structure preexisting in itself or in our minds. Like Neurath and Quine, Lautman was not seeking for an *ab ovo* sub-basement of mathematics, but staying afloat in the boat, he presupposed a preliminary background structure. Nevertheless,

> the reality of mathematics is not made of the act of the intellect that creates or understands, but it appears to us in this act and cannot be fully characterized independently of those mathematics that are their indispensable support. [. . .] The reality inherent in mathematical theories is that they are participating in an ideal reality that is dominant with respect to mathematics, and which is knowable only through it. [. . .] We see in mathematics a way of structuring a basic domain [structure] interpretable in terms of existence for some new things [. . .] that the structure of the domain seems to preform. (Lautman 2006, 66–68)

Now, I think that what interests us today in Lautman is not his platonistic solution itself, i.e. the proposal that the intrinsic reality of mathematical entities, facts or theories lies in their dialectical participation in ideas which dominate them (Lautman 2006, 237) and which are themselves realities. What is subtle is his insight in the essential difference between the nature of mathematics and the nature of the Dialectic. This insight leads to an alternative interpretation of Poincaré's and Quine's thesis of the incompleteness of mathematical objects and the ideas to which they belong: this incompleteness is neither an epistemic deficiency possessed finally by all objects according to Poincaré, nor purely a verbal accommodation with respect to a set theoretic progression possessing itself ontological commitment (Quine 1986, 401), but an ontological peculiarity: "Ideas are not models whose mathematical entities are merely copies. The Genesis is no longer seen as the creation of the concrete material from the idea, but the advent of concepts related to the concrete in an *analysis* of the Idea." (Lautman 2006, 238; *my emphasis*). In fact he distinguishes "notions" and "ideas" in order to underline the different status of philosophy and mathematics. While mathematical notions "describe existing relations between mathematical entities", the ideas describing dialectical relations do not assert any existing relation between *notions. Ideas* concern possible relationships between such *notions,* as, for example, between "formal systems" and their models or the relationship between the infinite and the finite. The analogies between structures cannot be expressed on the level of structures. The identity of a structure is not a mathematical subject, and the concept of structure as such not a mathematical object. The analogies between structures are as ideas "incarnated" or, as Poincaré would say, suggested "in the very movement of the mathematical theories" (Lautman 2006, 12). The systematical point of this parallel to Poincaré was seen by Lautman himself when he remarked that "the process of linking theory and experience symbolizes the relation between ideas and mathematical theories" (Petitot 1987, 105) The ideas have, according to Lautman, no ontological commitments and no anteriority with respect to their instantiations (Petitot 1987, 87), but rather raise questions and "are only the problematic issue relating to any of the existing situations." (Lautman 2006, 242/243). In short, as Petitot expressed it, the dialectic between ideas and notions is historic and ideas are by no means irreducible essences of an intellectual world (Petitot 1987, 95).

If we try treating structures as individuals and describing their relations, we are treating structures themselves as positions in a structure of structures. Lautman avoids such a circularity in the following way: "Metamathematics embodied in the generation of ideas [. . .] cannot give rise in turn to a meta-metamathematics; the regression stops when the mind has reached the patterns by which the dialectic is constituted. We see our reference to Platonism is well justified" (Lautman 2006, 232). In other terms, the classical view of structures cannot be substituted, without precautions, by a structural view of structures. Structures are neither mathematical objects nor properties of such objects, because they depend also, as we have seen, on a system of representation. Structures, as Lautmanian ideas (minus Platonism), are *patterns.* What I mean by "pattern" is a schema whose general and singular aspects are in a perpetual interplay or in a dialectical link. The concept of "pattern" makes it possible to avoid *ante rem* and *in re* structuralism (cf. Oliveri 2007, 163).

In this sense, Resnik is right, and Lautman would agree, that the structural approach to mathematics "would be no worse off than set theory, which cannot recognize its own universe of discourse as a set". Indeed, this limitation has only a negative bearing on structuralism, "if structuralism [is] purported to be a mathematical theory rather than a philosophical account of mathematics". But, we have seen that Lautman was not pursuing a foundational program, but rather hoping to achieve a deeper understanding of mathematical practice. His solution: philosophically, the concept of structure is dominated by a dialectical idea of a "pattern" that brings two perspectives together: (a) the structure as an essence of a form, realized in a matter, created by the form, and (b) the essence of a matter giving rise to the forms. This dialectical *ante rem—in re* solution seems no worse off than the *ante-rem* dialectic that considers structures in an alternating perspective, either from a "place-to-be-filled" or from a "places-are-objects" point of view, i.e. the doctrine that each structure can exemplify itself (Shapiro 1997, 89). This is formulated in a Lautmanian way by Chihara: "A structure is the abstract form of a system, and insofar as it exemplifies itself, it must be a system which has as its form the very form that it itself is" (Chihara 2004, 67).

References

Boutroux, P. 1920. *L'idéal scientifique des mathématiciens*. Paris: Félix Alcan.

Chihara, C. 2004. *A structural account of mathematics*. Clarendon/Oxford/New York: Oxford University Press.

Friedman, M. 2007. Coordination, constitution, and convention: The evolution of the a priori in logical empiricism. In *The Cambridge companion to logical empiricism*, ed. A. Richardson and T. Uebel, 91–116. Cambridge, UK: Cambridge University Press.

Heinzmann, G., and P. Nabonnand. 2008. Poincaré, intuitionism, intuition and convention. In *One hundred years of intuitionism (1907–2007)*, ed. M. Atten, P. Boldini, M. Bordeau, and G. Heinzmann, 163–177. Basel/Boston/Berlin: Birkhäuser.

Heinzmann, G., and D. Stump. 2013. *Henri Poincaré, Stanford encyclopedia of philosophy*, ed. Edward N. Zalta. http://plato.stanford.edu/entries/poincare/

Lautman, A. 2006. *Les Mathématiques, les idées et le réel physique*. Paris: Vrin.

Nye, M.J. 1979. The Boutroux circle and Poincaré's conventionalism. *Journal of the History of Ideas* XL: 107–120.

Oliveri, G. 2007. *A realist philosophy of mathematics*. Milton Keynes: College Publications.

Petitot, J. 1987. Refaire le "Timée". Introduction à la philosophie mathématique d'Albert Lautman. *Revue d'Histoire des Sciences* XL(1): 79–115.

Poincaré, H. 1898. On the foundations of geometry. *The Monist* IX: 1–43.

Poincaré, H. 1899. Des fondements de la géométrie. A propos d'un livre de M. Russell. *Revue de métaphysique et de morale* 7: 251–279.

Poincaré, H. 1902. *La Science et l'hypothèse*. Paris: Flammarion, 1968; English translation: *Science and Hypothesis*. London: The Walter Scott Publications, 1905.

Poincaré, H. 1905a. Les mathématiques et la logique. *Revue de métaphysique et de morale* 13: 815–835. In *Poincaré, Russell, Zermelo et Peano. Textes de la discussion (1906–1912) sur les fondements des mathématiques: des antinomies à la prédica- tivité*, ed. G. Heinzmann. Paris: Libr. A. Blanchard, 1986, 11–34.

Poincaré, H. 1905b. *La Valeur de la science*. Paris: Flammarion, 1970; English translation: *The Value of Science*. New York: Dover, 1958.

Quine, W.V.O. 1986. Reply to Charles Parsons. In *The philosophy of W.V. Quine*, ed. L.E. Hahn and P.A. Schilpp, 396–403. LaSalle: Open Court.

Resnik, M.D. 1997. *Mathematics as a science of patterns*. Oxford: Clarendon.

Schlick, M. 1979. *Allgemeine Erkenntnistlehre (21925 Berlin)*. Frankfurt: Suhrkamp.

Shapiro, S. 1997. *Philosophy of mathematics*. Oxford: University Press.

Stump, D.J. 1996. Poincaré's curious role in the formalization of mathematics. In *Henri Poincaré: Science and philosophy, International Congress Nancy, France, 1994*, ed. J.L. Greffe, G. Heinzmann, and K. Lorenz, 481–492. Paris/Berlin: A. Blanchard/Akademie Verlag.

Winter, M. 1911. *La méthode dans la philosophie des mathématiques*. Paris: Félix Alcan.

Part III
Poincaré on the Foundations of Physics

Henri Poincaré: The Status of Mechanical Explanations and the Foundations of Statistical Mechanics

João Príncipe

Abstract The first goal of this paper is to show the evolution of Poincaré's opinion on the mechanistic reduction of the principles of thermodynamics, placing it in the context of the science of his time. The second is to present some of his work in 1890 on the foundations of statistical mechanics. He became interested first in thermodynamics and its relation with mechanics, drawing on the work of Helmholtz on monocyclic systems. After a period of skepticism concerning the kinetic theory, he read some of Maxwell's memories and contributed to the foundations of statistical mechanics. I also show that Poincaré's contributions to the foundations of statistical mechanics are closely linked to his work in celestial mechanics and its interest in probability theory and its role in physics.

Introduction

The scientific oeuvre of Poincaré is immense, even if we consider only the fields of mechanics, astronomy, and mathematical physics. His interest in the theories of elasticity, waves, electromagnetism, and thermodynamics, as well, is marked by significant contributions. One of his contemporaries noted that he was more a conqueror than colonizer: he contributed significantly to many areas without staying there too long. Many of his memoirs and articles have an unfinished and open character. These general characteristics apply to his contributions to statistical mechanics.[1]

[1] "A contemporary said of him, he was a conqueror, not a colonialist." Boyer et Merzbach 1968, 676, §27. 3.

J. Príncipe (✉)
CEHFCi – Universidade de Évora, Rua Auta Palma Carlos, N° 18, 2 Dt°,
2685-125 Sacavém, Portugal
e-mail: jpps@uevora.pt

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The primary aim of this paper is to show the evolution of Poincaré's views on the mechanistic reduction of the principles of thermodynamics, placing it in the context of the science of his time. The second is to present some of his work, around 1890, on the foundations of statistical mechanics. He looked first to thermodynamics and its relationship with mechanics, inspired by Helmholtz's on monocyclic systems. After a period of skepticism about the kinetic theory, he carefully read some of the memoires of Maxwell and contributed to the foundations of statistical mechanics. I also show that Poincaré's contributions to the foundations of statistical mechanics depend closely on his work in celestial mechanics and his interest in probabilities and their role in physics.

Classical statistical mechanics treats systems of material bodies subject to the laws of mechanics and with a huge number of degrees of freedom. It allows to one infer observable properties of these systems using statistical methods. Its initial domain was quite limited, to the case of gases. The kinetic theory of gases, for simplicity, had three formulations: the elementary kinetic theory of Clausius (1857–58), based on the concept of mean free path; Maxwell's second theory, which leads to the Boltzmann equation (1866); and the ensembles approach of Maxwell-Boltzmann-Gibbs (1879). Maxwell and Boltzmann, from specific models (elastic spheres, material points interacting through a Newtonian potential), then took the path of greatest generality to justify the equilibrium distribution, equipartition and the tendency towards equilibrium. This path is based on the formulation of Hamilton's mechanics, and it involves Liouville's theorem and the ergodic hypothesis. Josiah Willard Gibbs' 1902 book, *Elementary principles of statistical mechanics*, presented these methods in a systematic and independent way, compared to the initial context where ideas have emerged – that of the kinetic theory of gases. Twentieth- century statistical mechanics would be applied to more general systems, and its development would be closely linked to the history of quantum theory.

The kinetic theory of gases was struggling to establish itself at least until the end of the nineteenth century, except in the United Kingdom. The specific heat anomaly, and the small number of specific predictions, could be invoked against it. Its main achievement was a theory of transport phenomena, an area where it provided new relationships with, and access to, molecular parameters. Given the structuring role generally given to mechanics, the mechanical reduction of thermal phenomena could not fail to win favor; but further reductions existed which did not presuppose any specific model for substance and that did not make use of probabilities. The thermodynamics of the principles, a macroscopic theory, had a much more extensive domain than the field of kinetic theory. The analytical theory of heat, concerning heat diffusion, was able to develop without any connection to the kinetic theory.

Strictly Mechanistic Reduction of Thermodynamics

Up to 1870, French scientists, despite their interest in the work of Clausius, showed very little interest in kinetic theory. The reception of the first kinetic theory of gases became a conceptual framework dominated by the tradition of laplacian molecular physics, and the optical tradition, originated by Fresnel and Cauchy. These two traditions share a molecular ontology, where everything is explained by postulating the existence of atoms or molecules centres of force. Concerning the nature of heat, vibration theory, proposed by Ampère (1835), allowed for a qualitative unity of light and heat, in the context of the Laplacian ontology. Ampère wrote: "it is to molecular vibrations and their propagation in their environment that I attribute all phenomena of sound; it is to atomic vibrations and their propagation in the ether that I attribute all those of heat and light." These traditions bore many fruits in the fields of elasticity, hydrodynamics, elastic ether theory, etc. They enabled a unifying vision, ensuring consistency between the various theories, with celestial mechanics playing the role of an archetype; they benefited from the intellectual authority of masters such as Newton, Laplace, Fresnel, Ampère, etc.; and they were institutionally strengthened by the centralized and hierarchical character of the scientific community. These traditions coexisted with a more recent attitude of theoretical agnosticism, in experimental and theoretic work of Victor Regnault, who, however, still did not deny the molecular ontology. The identity of French physics also depended on a somewhat vague ideal of rigour and clarity in research and in the presentation of the results. Around 1885, Ampère's version of the molecular physics program was still alive. It still promised a unifying vision. (Ampère 1835, 436, 434–435; Príncipe 2008, "Conclusions.")

After 1850, molecular physics in the style of Laplace or Ampère found itself in competition with other approaches, especially outside France: (phenomenological) thermodynamics and kinetic theories. The latter involved only a minority of scientists around the world, because they had very few applications, many anomalies, and they involved ways new and difficult reasoning, especially in the second theory of Maxwell. Also, it should be noted that in the second half of the nineteenth century, there had been several kinetic conceptions of heat, and that someone like Clausius could accept or at least recognize this pluralism. This situation can be compared to that of the multiplicity of contemporary mechanical theories of the optical ether. The French, strong on Regnault's work on static properties of gases and vapours, were particularly sensitive to the anomalies of the kinetic theories. They were working especially in the tradition of Ampère's vibrational conception of heat. It was only after 1890 that the French took the kinetic theory as an object of scientific research, a change due to the intervention of scientists of a younger generation, more open to foreign physics. Henri Poincaré and Marcel Brillouin, both born in 1854, took an interest in Maxwell's second theory and the foundations of statistical mechanics, in a way shaped by their own research programs.[2]

[2]On the survival of several kinetic conceptions of heat, see Príncipe 2008, 8, and Chaps. 4 and 6.

In 1886 Poincaré obtained the chair of mathematical physics and probability calculus at the Sorbonne, which favored even more his interests in theoretical physics. He taught the mathematical theory of light, and in the spring of 1888, he taught a course on Maxwell's *Treatise on Electricity and Magnetism*. In the following years, he taught the electrical theories of Helmholtz, Hertz, Larmor and Lorentz. In 1888–89, he taught thermodynamics. He considered the question of compatibility between mechanism and thermodynamics, by analyzing the mechanical analogies proposed by Hermann von Helmholtz between the second principle and monocyclic systems described in the Hamiltonian formalism.

One should not confuse the mechanical analogies between the second principle and periodic or monocyclic mechanical systems, developed by Boltzmann, Clausius and Helmholtz, with concrete models of heat motion, in particular that of the kinetic theory. These analogies are formal analogies, and do not imply anything about the precise nature of the movement that is heat. These analogies were already of interest to the French scientists. Although around 1870, kinetic theory was taught in schools according to the views of Clausius, from the research point of view the French took a special interest in the analogy that Clausius proposed between the second principle and behavior of periodic systems. These analogies are compatible with vibration theory, the microscopic model for the material is not specified, and probabilistic considerations played no role.[3]

General Characteristics of Helmholtz's Approach

In 1884, in "On the statics of monocyclic systems," Helmholtz introduced the notions of polycyclic and monocyclic systems, presenting an analogy to the second principle for the case of reversible processes. In a memoire of 1886, "On the principle of least action," he distinguished between complete and incomplete systems and considers irreversible processes. In this analogy the system obeys the conservation of energy, and is described by the Langrangian equations that can be derived from the principle of least action. The use of this principle allows him to avoid assuming particular atomic models. This strategy originated in Maxwell's use of the Langrangian method in his electromagnetic theory, to obtain the field equations without a detailed model of the ether; Poincaré considered this strategy to be Maxwell's great innovation (Poincaré 1890b, préface; see J. J. Thomson 1888, 4; Klein 1972, §5, 70–71; Bierhalter 1993, 442).

The last chapter of Poincaré's *Thermodynamique* is devoted to "The reduction of the principles of thermodynamics to the general principles of mechanics." Here

[3]Boltzmann was the first to develop these ideas; see Boltzmann 1866, Clausius 1871; Boltzmann 1871. A review of these articles may be found in Truesdell 1975, 59–60. On Clausius and the French, see Príncipe 2008, Chap. 7.

Poincaré expounds and criticizes the ideas of the German scientist.[4] Consider, following Helmholtz and Poincaré, a general mechanical system obeying Lagrange's equations (or, equivalently, Hamilton's equations). The system is described by a set of n generalized coordinates q; the corresponding velocities are $\dot{q} = dq/dt$; the state of the system is described by a single function, its Lagrangian:

$$L = L(q, \dot{q}) = T - V,$$

where $T(q, \dot{q})$ is the kinetic energy of the system, $V(q)$ the potential of the internal forces. Let P be the generalized external force corresponding to the generalized coordinate of the same index, and $p = \partial L/\partial \dot{q}$ the generalized quantity of motion; then for each generalized coordinate we write the respective Lagrangian (Poincaré 1892a, §311):

$$\frac{d}{dt}\frac{\partial L}{\partial \dot{q}} - \frac{\partial L}{\partial q} = P.$$

The dynamical evolution is governed by the system of these n equations.

Helmholtz distinguishes two groups of generalized coordinates: those which vary very slowly, the q_a, and those that vary rapidly, the q_b. The parameters that vary slowly are controlled by a macroscopic observer (for example volume, or the center of gravity of a body). A suggestive terminology was proposed by J. J. Thomson: he distinguished between macroscopically controllable variables q_a and the non-controllables q_b, corresponding to molecular motions, defining the thermal state of a body. When these rapid periodic motions are described by several non-controllable generalized coordinates q_b, Helmholtz speaks of polycyclic systems. In a monocyclic system, we admit the existence of certain relations between the velocities of the different parts of the system in such a way that these periodic motions are described by a single coordinate; those rapid motions that take place without altering the configuration of the system are analogous to the rotation of a flywheel or of a fluid circulating in a vortex (J. J. Thomson 1888, Chap. VI, "Temperature," §46; see Poincaré 1892a, §314; Langevin 1913, 706).

The Analogue of the Second Principle for Reversible Processes

Helmholtz mechanically defined a function sharing the same properties as entropy and the role of the temperature is played by the *vis viva* of these rapid movements. For the case of reversible processes that are infinitely slow, Helmholtz formulated

[4]Darrigol described the method usually employed by Poincaré : "He read scientific texts quickly as a whole, and reconstructed the reasonings in his own manner. The result was often clearer than the original, revealed some essential features in full light, but overlooked other important ones", Darrigol 2000, 353.

three "natural" hypotheses. First, the velocities of the non-controllable coordinates are much greater than those of the controllable coordinates: $\dot{q}_b >> \dot{q}_a \approx 0$ (*hypothesis* I). The non-controllable coordinates are cyclic (or gyrostatic) – (hypothesis II). Therefore, they do not figure in the Lagrangian, and the corresponding equations are[5]:

$$P_b = \frac{d}{dt}\left(\frac{\partial L}{\partial \dot{q}_b}\right) = \frac{dp_b}{dt}.$$

If dQ is the energy transmitted during the change of coordinates q_b, we have:

$$dQ = \sum P_b dq_b = \sum P_b \dot{q}_b dt = \sum \dot{q}_b \frac{dp_b}{dt} dt = \sum \dot{q}_b dp_b.$$

The kinetic energy is a homogeneous and quadratic function of the generalized velocities (if the connections don't depend explicitly on the time). Since the terms containing the $\dot{q}_a$ are infinitely smaller, we have:

$$\frac{d}{dt}\left(\frac{\partial L}{\partial \dot{q}_a}\right) \approx \sum_b \alpha_{ab} \ddot{q}_b.$$

Admitting that the non-controllable coordinates have very small accelerations (since we are considering an equilibrium situation, and the constant temperature will be represented by the kinetic energy corresponding to an observably constant molecular velocity[6]) –*hypothèse* III, the anterior derivative is zero and the Lagrangian becomes:

$$\frac{\partial L}{\partial q_a} = -P_a \text{ (Poincaré 1892a, §316).}$$

By the previous considerations and by the theorem of homogeneous functions of degree n:

$$2T = \sum \dot{q}\frac{\partial T}{\partial \dot{q}} = \sum \dot{q}\, p \approx 2T_b = \sum \dot{q}_b\, p_b.$$

For the case of a monocyclic system, containing a single gyrostatic coordinate, we have (Poincaré 1892a, §317; Helmholtz 1884a, §3: "Monocyklische Systeme"):

$$dQ = \dot{q}_b dp_b, \quad 2T_b = \dot{q}_b\, p_b.$$

[5]The hypothesis that the non-controllable variables do not figure in the potential energy is, from a modern point of view, reasonable for ideal gases but not for real gases, liquids and solids, where the interactions between molecules can't be ignored.

[6]Bierhalter maintains that Helmholtz was inspired by the first kinetic theories, for which the velocities of gas molecules were equal and constant. (Bierhalter 1993, 434 and 443.)

We can thus find an integrating factor of dQ for this case:

$$\frac{dQ}{\dot{q}_b} = dp_b \text{ and so } \frac{dQ}{T_b} = 2d\,(\log p_b)\,.$$

We thus have an analogue to the second law of thermodynamics (for reversible processes) if we allow that the temperature corresponds to the kinetic energy. This is suggested by the kinetic theory of gases, as Helmholtz had remarked in his first article.[7]

Poincaré then analyzed the case of thermal equilibrium between two bodies. The coupling (called "isomore", after the Greek expression for "same denominator") between two monocyclic systems with the same integrating factor (temperature) corresponds to the condition of thermal equilibrium. Since in a monocyclic system, it is impossible to operate directly on the gyrostatic coordinates q_b by means of external forces, heat cannot be transmitted across these coordinates except by its coupling to another monocyclic system, and the coupling has to be isomore. Poincaré did not see how this theory would explain the fact that two bodies in contact, with the same temperature would not exchange calorific energy:

> It is necessary to explain why, when two bodies with the same temperature are placed in contact, no heat passes from one to the other. The explanation has been attempted. The two bodies have been compared to two pullies with equal rotational velocities; when the pullies are turned, there is no shock and no transmission of living force from one to the other; when the two bodies are placed in contact, there will be no shocks between the molecules, the latter having the same velocity since the temperatures of the two bodies are the same. This explanation is far from satisfying.

By this, perhaps Poincaré means that the explanation is not compatible with the equipartition of energy: if two gases at the same temperature have molecules with different masses, their velocities should be different.[8]

Vibratory Motion and Monocyclic Systems

Poincaré asserts: "Molecular motions appear to be vibratory motions this way and that around a fixed point." He does not say that this is restricted to solid bodies. He is probably referring to the vibratory theory of heat. Poincaré wants to show that in this case the kinetic energy is still an integral divisor of dQ, which represents

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[7] "Hier tritt die Analogie mit der kinetischen Gastheorie schon sehr deutlich heraus. Die Temperatur θ ist der lebendigen Kraft proportional" (Helmholtz 1884a, fin du §3.) Martin Klein notes that Helmholtz had recognized that thermal motion is not strictly monocyclic: "I have affirmed from the beginning that thermal movement is not strictly monocyclic." translated from Helmholtz 1884a, 757; see Klein 1972, 67.

[8] Poincaré 1892a, §331. See Bryan 1891, §26 et §27, Helmholtz 1884b, end of §6 "Koppelung je zweier Systeme"; Bierhalter 1993, 446. The name for the coupling is first explained at the beginning of §5 of Helmholtz 1884a.

an original contribution (Poincaré 1892a, §322–326; quote from the beginning of §322; see also §315).

Allow that there is only one parameter that varies rapidly. It is not cyclic, since it figures in the potential energy of a vibratory motion. Here hypotheses I and III remain valid, but not hypothesis II. The potential and kinetic energies are:

$$V = \frac{A\left(q_a\right)\ q_b^2}{2} + C\left(q_a\right), \quad T = \frac{B(q_a)\ \dot{q}_b^2}{2}.$$

The Lagrangian corresponding to this coordinate q_b is:

$$\frac{d\ (B\dot{q}_b)}{dt} + Aq_b = -P.$$

For a stationary vibratory motion P is zero and A and B are constant; in that case:

$$A = \omega^2 B, \quad q_b = h \sin\left(\omega t + \varphi\right), \quad \dot{q}_b = h\omega \cos\left(\omega t + \varphi\right).$$

Given the extreme rapidity of the oscillations, if one considers a sufficiently long time, it is the mean value of the kinetic energy that intervenes. As $\overline{\cos^2 x} = 1/2$, we have:

$$T = \frac{Bh^2\omega^2}{4} = \frac{Ah^2}{4}.$$

We can calculate the work of the force P during "a time δt, very small in an absolute sense but nonetheless very large in relation to the period of vibration":

$$\delta Q = -\int P dq_b = \int \frac{dB}{dt}\dot{q}_b dq_b + \int B\frac{d\dot{q}_b}{dt}dq_b + \int Aq_b dq_b.$$

The first factor in the first integral of the second member may be considered as constant, the derivative *dB/dt* being small; the integral of $\int \dot{q}_b dq_b$ taken over a time δt is replaced by the product of δt with the average value $h^2\omega^2/2$ of $\dot{q}_b^2$; thus the first integral of the second member becomes:

$$\frac{dB}{dt}\delta t\frac{h^2\omega^2}{2} = h^2\omega^2\delta B$$

To calculate the two other integrals, Poincaré develops A and B by reference to increasing powers of t. The fact that δt is small permits one to consider only the linear part of these linear developments; the first derivatives of A and B are considered as constants. Moreover, one can choose δt in such a way that at the beginning and at the end of this interval q is null. After some clever calculations, Poincaré arrives at the expression (Poincaré 1892a, §325):

$$\frac{\delta Q}{T} = 3\frac{\delta B}{B} + 2\frac{\delta\left(\omega^2 h^2\right)}{\omega^2 h^2} - \frac{\delta A}{A},$$

which is an exact differential. Then, Helmholtz's theory permits the generalization of useful results for perfect gases to other states (of matter); in conclusion:

> Clausius's theorem [for reversible processes, *dQ/T* is an exact differential] is, in consequence, well enough proven for the case of a vibratory state of molecules in the case of a swirling state (Poincaré 1892a, end of §325)

Irreversibility and Mechanism

For a holonomic mechanical system, the kinetic energy is a quadratic function of the generalized velocities $\dot{q}$. To make the system return to its initial state by the same path, we can change the sign of the time parameter (change *dt* to *–dt*); then the $\dot{q}$ become – $\dot{q}$ but the quadratic terms do not change, nor does $V = V(q)$; thus the Lagrangian function remains the same. The same considerations apply to the Lagrangian equations $\frac{d}{dt}\frac{\partial L}{\partial \dot{q}} - \frac{\partial L}{\partial q} = P$, since dt and $\dot{q}$ simultaneously change sign while q and P remain indifferent. Therefore, Poincaré writes: "the sytem, when it reverts to its initial state, passes again exactly through those states that it had assumed in departing form the initial state; the transformations are therefore reversible" (Poincaré 1892a, §326).

However, Helmholtz found systems, called incomplete systems, for which the kinetic energy contains powers of odd exponents. He also showed that all the general equations that are valid for complete systems retain their form for the case of incomplete systems. In particular, the kinetic energy is an integral divisor of the quantity of heat for incomplete monocyclic systems. But if for complete systems, $T = T(q_a, \dot{q})$ is a quadratic function of the generalized velocities, in the case of incomplete systems T' can have terms of odd degree with respect to the generalized velocities, because one part of the q_a, $= q_c$, depends on the $\dot{q}_b$. The consequence is that a change of sign of the time implies a change in the Lagrangian – "irreversible phenomena could thus take pace with incomplete systems; this is what Helmholtz admits." The analogy for irreversibility consists in comparing the thermal motion of molecules with hidden stationary movements. In the case of the spinning top, the top that spins is distinguished from the dead top by its capacity to resist the action of external forces that tend to change the direction of the action of rotation. Helmholtz conceives of this top is enclosed in a shell, thus remaining invisible and inviolable by humans.[9]

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[9]Poincaré 1892a, 442. An illustration of a case where the living force ceases to be proportional to the square of the velocity is that of a wheel turning on an axis equipped with a centrifugal force regulator; if the angular momentum increases, the bearings of the regulator recede from the axis while increasing the moment of inertia, so that the kinetic energy is not simply proportional to the square of the angular velocity. Poincaré 1892a, 431.

In spite of the interest of Helmholtz's ideas, Poincaré, by a sufficiently general argument, shows that they cannot account for irreversible phenomena. In his note, "On the attempts at a mechanical explanation of the principles of thermodynamics," he poses the following question: "Can we, by representing the world as composed of atoms, explain why heat never passes from a cold body to a hot one?".[10]

Suppose a general mechanical system obeying the equations of Hamilton. The Hamiltonian is:

$$H\,(p,q) = \sum p_a\,\dot{q}_a - L,$$

summing over the variables p and q.

For the case where the system is shielded from all external action, the Hamiltonian equations are, $P_a = 0$:

$$\dot{q}_a = \frac{\partial H}{\partial p_a}, \quad \dot{p}_a = -\frac{\partial H}{\partial q_a}.$$

If natural processes simultaneously obey the equations of mechanics and Carnot's principle, there must exist a function $S(q,p)$, "that is constantly increasing and that we will call the entropy". Then we can prove:

$$\frac{dS}{dt} = \sum \left(\frac{\partial S}{\partial q}\frac{dq}{dt} + \frac{\partial S}{\partial p}\frac{dp}{dt} \right) = \sum \left(\frac{\partial S}{\partial q}\frac{\partial H}{\partial p} - \frac{\partial S}{\partial p}\frac{\partial H}{\partial q} \right) > 0.$$

Or, again, using the Poisson brackets,

$$\frac{dS}{dt} = \{S, H\} > 0.$$

Poincaré thought that he could demonstrate the impossibility of such an inequality while admitting that "the system, while remaining soustrait of all external action, is subject to such connections that the entropy is susceptible of a maximum". This state should correspond to a state of equilibrium. We can develop H and S in a power series $(q_\alpha - q_\alpha^o)$, $(p_\alpha - p_\alpha^o)$, where the index o refers to the situation of equilibrium. The first term of the expansion can be cancelled owing to the fact that the two functions, H and S, are defined up to a constant. Since we assume the expansion is done close to the values corresponding to a maximum of entropy, the first derivatives cancel for q_α^o, p_α^o. If we consider small variations around the equilibrium configuration, we can restrict ourselves to the quadratic terms. The entropy will then be represented by a quadratic form (where the x represent either the q or the p and the derivatives are calculated from their equilibrium values):

[10]Poincaré 1889, 550. Helmholtz's papers are explicitly cited at the beginning of this note. The proof appears also in Poincaré 1892a, §328 ff.

$$S = \sum_{i,j} \frac{\partial^2 S}{\partial x_i \partial x_j} \left(x_i - x_i^o\right)\left(x_j - x_j^0\right).$$

Since we admit that S has a maximum for q_α^o , p_α^o, this form is negative definite.

In order that the Hamiltonian can also be represented by a quadratic form, the first-order terms of its development should cancel. Poincaré justifies this as follows: "The derivatives of H cancel each other equally, because this maximum is an equilibrium position and so $\dot{p}_\alpha$ and $\dot{q}_\alpha$ must cancel." The form H can be definite or indefinite. Poincaré tells us nothing about the relation between these conditions and those that can represent thermodynamic equilibrium.

Admitting that S and H are representable by quadratic forms near the maximum entropy, Poincaré shows that their Poisson bracket is also a quadratic form that is not positive definite. This result is intuitive, in the sense that the Poisson bracket transforms the squared terms of the quadratic forms into rectangular forms (of indefinite sign). Note that if the development of the Hamiltonian in series carries linear terms, the plausibility increases of the impossibility of the inequality $(S,H) > 0$ increases (Poincaré 1892a, §330).

Poincaré ends this note with the following conclusion:

> We should conclude that the two principles of the increase of entropy and of least action (understood in the Hamiltonian sense) are irreconcilable. Thus if Mr. Helmholtz has shown, with admirable clarity, that the laws of reversible phenomena derive from dynamics, it seems probable that we will have to look elsewhere for an explanation of irreversible phenomena, and give up on the familiar hypotheses of rational mechanics from which one derives the equations of Lagrange and Hamilton. (Poincaré 1889, 553)

In 1891, this note provoked a severe critique from George Bryan, who insisted that the equilibrium conditions imposed by Poincaré implied that all parts of the system are at rest. Since the entropy of a monocyclic system is the logarithm of a moment, if the latter is zero then the entropy will be infinite, contrary to Poincaré's supposition. This criticism seems correct. Bryan is doubtless also correct that the kinetic molecular interpretation of temperature is incompatible with Poincaré's equilibrium conditions. Zermelo briefly mentioned Poincaré's note as another attempt to show that irreversible processes cannot always be explained by Helmholtz's theory. Finally, the note got the attention of Louis de Broglie, for whom "these attempts at an interpretation of the second law of thermodynamics that is mechanical, but not statistical, have only led to very fragmentary results taht only apply to very special models." (Bryan 1891, 106–107; de Broglie 1948, Chap. V: 119; Zermelo 1896; see Bierhalter 1993, 455 and Brush 1976, §14.7, note 4).

Poincaré and Maxwell's Kinetic Theory of Gases

Poincaré published two editions of his course on thermodynamics. The second, in 1908, differed little from the first, except insofar as Poincaré's opinion on the kinetic theory was concerned. At the end of the preface to the first edition, Poincaré repeated the conclusion of his 1889 note: "I end with the theory of monocyclic systems. I will only cite my conclusion: Mechanism is incompatible with Clausius's theorem." In an issue of Nature in 1892, there was a debate between Poincaré and P. G. Tait. Tait accused Poincaré of having forgotten the kinetic theory in his course of *Thermodynamique*. Poincaré responded that he "wanted to remain completely apart from molecular hypotheses," and that he found the kinetic theory "not very satisfying". In the following year Poincaré's position regarding the kinetic theory would become rather more favorable.[11]

Poincaré began to take an interest in the kinetic theory of gases in the course of his lecture on the papers of Maxwell, which was probably connected with his interest in ionic theories of electromagnetism (notably that of Lorentz), as the development of theoretical microphysics favored atomistic theories of heat. In 1893, Poincaré carefully read Maxwell's paper of 1866 and raised a correct objection to Maxwell's reasoning to justify the law of adiabatic expansion of a gas. This interesting criticism went straight to the foundations of statistical mechanics. Poincaré would take an interest above all in the most abstract justifications for equilibrium distribution, equipartition, and the tendency to equilibrium. That is to say, he favored the ensemble approach of Hamiltonian mechanics and he quickly saw the connection with a theorem in the three-body problem.[12]

The Article "Le mécanisme et l'expérience"

Poincaré spoke for the first time about the importance of his recurrence theorem for the attempts at a mechanistic reduction of Carnot's principle in the article "Le mécanisme et l'expérience" (1893a), published in the inaugural issue of the *Revue de Métaphysique et de Morale*. Experience shows that in nature there are "a crowd of irreversible phenomena," which appear to be difficult to reconcile with mechanistic reduction. Poincaré divided mechanists into two groups. One was the side of Helmholtz, who did not use statistical reasoning, and the other was the English. Speaking of Maxwell (whom he considered to be English), he wrote:

> The apparent irreversibility of natural phenomena has to do with the fact that molecules are too small and too numerous for the coarseness of our senses .... Maxwell introduces the fiction of a "demon" whose eyes are subtle enough to distinguish molecules, and whose hands are small enough and quick enough to grasp them. For such a demon ... there would be no difficulty in making heat pass from a cold body to a hot one ....The kinetic theory of gases is up to now the most serious attempt to reconcile mechanism with experience. (Poincaré 1893a, 536)

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[11] Poincaré 1892b, 485. Boltzmann stated, at the end of the preface to the first part of his *Leçons* (1896a), that "no one wanted to give much space to my work. It was cited with respect by Kirchoff and by Poincaré just at the end of his *Thermodynamique*, but not used when the occasion presented itself."

[12] Poincaré 1893b; see the reference to this criticism in Boltzmann 1896a, note à la formule (187), see also Príncipe 2008, §10.4.1. On Poincaré's contributions to electromagnetism and the theory of electrons, see Darrigol 2000, Chap. 9, especially §9.3.3.

Poincaré here speaks of the thought experiment now known as "Maxwell's demon." In a letter to P. G. Tait in December 1867, reprised in his *Theory of Heat* (1871), Maxwell considers a finite being capable of seeing individual molecules. Controlling a barrier that separates the two parts of a chamber full of gas, this being could provoke a flow of heat (without compensation, that is without consuming work) letting only the fastest-moving molecules pass in one direction and only the slowest in the other. Maxwell therefore admits that the validity of the second law is only statistical (Maxwell to Tait, 11 déc. 1867, see also Maxwell to Strutt, 6 December 1870, in Maxwell 1990, vol. 2, 328–334, 582–583). Poincaré adds that the kinetic theory is not incompatible with his recurrence theorem:

> An easily established theorem teaches us that a finite world, subject only to the laws of mechanics, will always pass again through a state very close to its initial state. On the contrary, according to accepted experimental laws, (if we grant them an absolute validity, and if we wish to push their consequences to the fullest), the universe tends to a certain final state from which it will not be able to depart. In this final state ... all bodies will be ... at the same temperature .... Has anyone remarked that the English kinetic theories can escape from this contradiction? The world, according to them, first tends toward a state where it would remain for a long time without any apparent change ... but it would not maintain that state forever .... it would remain there only for an enormously long time, even longer than the number of molecules is large. This state would therefore not be the definitive death of the universe, but a kind of sleep, from which it would awaken after millions of millions of centuries.

This theorem, and the status of mechanism, were discussed by Zermelo and Boltzmann in 1896. The latter asserted, like Poincaré that the recurrences, for the usual macroscopic systems, escape our experience (Poincaré 1893a, 536. See Brush 1976, §14.7, 632–640).

The Recurrence Theorem

The recurrence theorem appears in Poincaré's paper, "Sur le problème des trois corps et les équations de la dynamique," which received the Oscar II of Sweden Prize, January 21, 1899.

The Three-Body Problem

The three-body problem is one of the most celebrated problems of mechanics: given three material points interacting according to the law of universal gravitation, freely moveable in space; to find their motions from given initial conditions. From 1750 to the end of the nineteenth century, several hundred articles were published on this subject. Poincaré's paper went through two formulations (1889 et 1890), of which only the second was published. The notion of the stability of a system, initially defined by the confinement of the variables that define the system, was replaced in 1890 by that of Poisson: the movable point P (describing, for example, a planet), should return after a sufficiently long time, if not to its initial position, then to an arbitrarily nearby point to the initial position (recurrence).[13]

Some periodic solutions were already known. Poincaré studied the non-periodic solutions (the asymptotic and the doubly asymptotic solutions) and developed qualitative methods. These non-periodic solutions are infinitely improbable, but "taken together with the periodic solutions ... make up, so to speak, the tangled fabric formed by the totality of general orbits."[14]

The Concept of Integral Invariant

The concept of the integral invariant was created by Poincaré in the framework of his research on the differential equations of Hamiltonian systems. Recall his definition:

$$\frac{dx_1}{X_1} = \frac{dx_2}{X_2} = \cdots = \frac{dx_n}{X_n} = dt,$$

a system of differential equations. Let $x_1^0, \ldots, x_n^0$ be any point in a domain $D(0)$ of k dimensions. This set of points will occupy, at another instant t, another domain of k dimensions, $D(t)$. A k-dimensional integral over the domain $D(t)$ is an integral invariant of order k of the system of equations if the value of this integral is independent of t. The typical example is the constant volume of a determinate part of an incompressible fluid. For a Hamiltonian system with n degrees of freedom, Poincaré shows that:

$$I_1 = \int \sum_i dq_i dp_i, \; I_2 = \int \sum_{i,k} dq_i dp_i dq_k dp_k, \ldots\ldots, \; I_n = \int dq_1 dp_1 dq_2 dp_2 \ldots dq_n dp_n,$$

are integral invariants. In particular, the integral I_n is an integral invariant corresponding to the condition of incompressibility of a fluid in the phase space (Liouville's theorem).

Poincaré took great advantage of the invariants I_1, I_n in his researches on some special solutions (periodic solutions of the second type and doubly asymptotic solu-

[13]On the history of the problem, see Whittaker 1899 and Barrow-Green 1997. The first version, that of 1889, was printed but not published, because a crucial error was detected in the demonstration of stability. It was in the second version that the recurrence theorem played a decisive role in the structure of the paper. See Robadey 2006.

[14]Von Zeipel 1921, in *Œuvres* de Poincaré, vol. 11, 308. Here is an example of an asymptotic orbit, in a system consisting of a Sun, an Earth, and two moons of infinitely small mass: "Suppose an observer placed on the Earth and slowly turning on himself so as to be in constant view of the Sun. The Sun will appear to him to be at rest, and the moon L1, with a periodic orbit, will appear to describe a closed curve C. Moon L2 will then describe for him a sort of spiral of which the arms, more and more tightly wound, will indefinitely approach the curve C." Poincaré 1891, *Œuvres* vol. 8, 532–533.

tions) and on the question of the stability of motion. He immediately remarked on the existence of unstable orbits: "The existence of asymptotic solutions . . . suffices to show that if the initial position of point P is suitably chosen, the point P will not re-pass an infinite number of times as nearly as one might like to the initial position". Poincaré went on to establish the exceptional character of these unstable solutions: "There will be an infinity of solutions of the problem that will not have stability . . . in the sense of Poisson; but there will be an infinity that do have it. I would add that the first can be regarded as exceptional" (Poincaré 1890a Sect. 8, "Usage des invariants intégraux", *Œuvres* vol. 7, 313–314).

Poincaré began by demonstrating the following theorem. Consider a space of N dimensions and assume that the hypervolume $\int dx_1 dx_2 \ldots dx_N$ is an integral invariant; if the point P remains at a finite distance and if we consider any region g_0 of this space, no matter how small the region s, there will be trajectories that cross it an infinite number of times. The demonstration shows that the total volume of the series of regions of space that succeed the region g_0 becomes infinite if there is no recurrence (Poincaré 1890a, *Œuvres* vol. 7, 316). The calculation of the time of return is a very delicate problem on which Poincaré, as far as I know, said nothing in his papers.

The Exceptional Character of Trajectories Without Recurrence

After his study of asymptotic solutions, Poincaré studied possible trajectories without the property of recurrence. The previous demonstration did not seem to allow for this type of trajectory, and it seemed necessary to harmonize the two results. The quasi-periodic character is almost always there in the evolution of a conservative system; Poincaré expressed it using the concept of probability. This concept appears explicitly in the enunciation of the corollary of the recurrence theorem in the final version of the paper (1890a):

> Corollary. It follows from the preceding that there exists an infinity of trajectories that cross the region $\delta(P_0)$ infinitely many times; ... but there may exist others that only cross the region a finite number of times .... It will suit our purposes to say that the probability that the initial position of a mobile point P belongs to a certain region $\delta(P_0)$ is to the probability that the initial position belongs to another region $\delta'(P_0)$ as the volume of $\delta(P_0)$ is to the volume of $\delta'(P_0)$.
>
> The probabilities being thus defined, I propose to establish that the probability that a trajectory $\delta(P_0)$ starting from a point does not cross this region more than k time is zero, no matter how large k is or how small the region $\delta(P_0)$. That is what I mean when I say that trajectories that only cross $\delta(P_0)$ a finite number of times are exceptional. (Poincaré 1890a, *Œuvres* VII, p. 316)

The historian Anne Robadey remarks that the recurrence theorem (and its corollary), of which the proof is non-constructive, represents, in the history of mathematical theorems, one of the first examples in which a property is shown to be valid for "almost all" of the objects in a given class. Poincaré directly connected the concept of probability and the relative measure of a region. Today we characterize the exceptional character of trajectories without recurrence by saying that they constitute a set of measure zero. The measure theory developed by Borel, Lebesgue, and others came after this paper of Poincaré's. The development of ergodic theory is intimately connected to these developments. The influence of Borel on Lebesgue, and the influence of Poincaré on the latter, has already been remarked on. George Birkhoff, one of the mathematicians who contributed the most to the theory of ergodicity, at a conference on "Probability and physical systems" (1931), considering the problem of exceptional trajectories (and its lack of physical significance in light of the impossibility of rigorously determining the initial conditions), eulogized Poincaré as the first to use, in an intuitive manner, considerations "of probability 1"; that is, the first to consider, in problems of theoretical mechanics, sets of measure zero (Von Plato 1994, 110; Poincaré 1896).

"On the Kinetic Theory of Gases" (1894)

In 1894, Poincaré wrote an article presenting his lecture on the foundations of statistical mechanics and analyzed Kelvin's criticism of the validity of the ergodic hypothesis (1892). This criticism immediately aroused the interest of several British scientists (Watson, Burbury, Bryan and Rayleigh) as well as that of Boltzmann. Poincaré showed that Kelvin's examples were not genuine counter-examples to equipartition.[15]

Poincaré recognized that great efforts had been expended to develop the kinetic theory, and that the results of those efforts had not been proportional to the effort expended; he stated:

> I doubt that, up to the present time, it can account for all the known facts. But it's not a question of knowing whether it is *true*; that word, where such a theory is concerned, *has no meaning*; it is a question of knowing whether its fertility is spent, or whether it can still help with further discoveries. (Poincaré 1894, 246)

By that, Poincaré wanted to indicate that the kinetic theory has the status of an analogy, a scientific illustration in the sense of Maxwell (see Príncipe 2010, 2012).

After recalling the basic conception of the kinetic theory, already presented by the Bernoullis, Poincaré emphasized that "the theory only took on its definitive form when Clausius proved his virial theorem." The internal virial allows us to understand how remote the behavior of real gases can be from that of an ideal gas. Then he mentions Clausius's hypothesis of the proportionality of the energies associated with the components of molecules to the kinetic energy of translation. This postulate of Clausius is justified by the theorem of equipartition, of which

[15]See Thomson 1892 and Brush 1976, §10.9, who concludes: "The outcome seemed to be a general agreement that most of Kelvin's test-cases did not prove any violation of the equipartition theorem, but, on the other hand, that one could not be sure that the theorem was always valid in systems of a finite number of particles". See also Príncipe 2008, §10.4.2.

one possible foundation is the ergodic hypothesis. Recall first the genealogy of that hypothesis, which came to Poincaré from reading Maxwell's 1879 paper, "On Boltzmann's Theorem on the average distribution of energy in a system of material points," in which Maxwell took up the global approach introduced by Boltzmann. In 1868, Ludwig Boltzmann criticized Maxwell's proof of the stability of the distribution with respect to binary collisions, and introduced the distribution that Gibbs would call micro-canonical. In the case of a gas subject to the action of an external force field, he introduced the global distribution, a function of the positions and the velocities of the N molecules of a gas:

$$\rho\left(\vec{r}_1, \vec{r}_2, \ldots \vec{r}_N, \vec{V}_1, \vec{V}_2, \ldots \vec{V}_N\right),$$

$\rho\, d\sigma$ giving the fraction of the time (considering a very long time) that the system spends in the element $d\sigma = d^3r_1 d^3r_2 \ldots d^3r_N d^3V_1 d^3V_2 \ldots d^3V_N$. He first shows that if a system is contained at an instant t within a volume element $d\sigma$ of the phase space, then at a later instant $t + \delta t$ it will be contained in a volume element $d\sigma'$ with the same volume ($d\sigma = d\sigma'$, Liouville's theorem). He deduces from this that the density ρ is constant along the entire trajectory. Finally, he admits that the trajectory of the system in this $6N$ dimensional space fills the energy level $E =$ cte. It then results that the density ρ is uniform on this level. Starting from this distribution, characterizing a large isolated system, Boltzmann arrived at the characteristic distribution of a small subsystem (one molecule, for example) that is weakly coupled (thermally coupled) with its complement (the remainder of the large system, which plays the role of thermostat). If E^* is the energy of this subsystem, then the distribution associated with it is αe^{-2hE^*}. The equipartition of the energy for the quadratic degrees of freedom is a consequence of this distribution. This law, now known as the Maxwell-Boltzmann distribution or the canonical Gibbs law, still remains an essential element of statistical mechanics.[16]

In 1879, Maxwell attributed to Boltzmann "the general solution of the problem of the equilibrium of kinetic energy among a finite number of material points," and noted that "The only assumption which is necessary for the direct proof [of the equipartition theorem] is that the system, if left to itself in its actual state of motion, will, sooner or later, pass through every phase which is consistent with the equation of energy".[17]

In 1894, Poincaré noted that the mean value of a dynamical magnitude should, if it is accessible to observation, be comprised of "the mean taken at once with respect to time and with respect to the various molecules; it is, so to speak, a mean value of mean values." This assertion suggests that for him, the equivalence of the two

[16]Boltzmann 1868. A partial translation appears in Barberousse 2002, 150–165. See Darrigol and Renn 2000.

[17]Maxwell 1879, *Scientific Papers*, 714. Maxwell recognized that one could imagine systems where this condition (the ergodic hypothesis) is a false, but he admits that, for a gas enclosed in a container, the interaction of the molecules with the barrier permits an explanation of its validity. ibid., 714–715.

means was not evident (Poincaré 1894, 249). Poincaré gives the following form to the equipartition theorem:

> If there is no other uniform integral than that of the living forces, and if the living force of the system is decomposable into two independent parts, the mean values of these two parts, over a very long time, will be among themselves as the number of their degrees of freedom.

Poincaré noted that the existence of other uniform integrals, for the case of a material system that is free in space (for which there is conservation of linear momentum and of angular momentum), changes this form (a case considered in the second part of Maxwell's 1879 paper). The modified form insists that the energy must be the only uniform integral (see below; Poincaré 1894, 253).

Poincaré recognized the anomaly of specific heats, but he believed that this difficulty, though unresolved, would perhaps not be insurmountable (Poincaré 1894, 255). The isotropic distribution of velocities for a gas at equilibrium, without action by an external force, is another consequence of "Maxwell's theorem." All "the preceeding suffices to show the importance of Maxwell's theorem [the equiprobability of domains of equal volume in the available phase space of a system]; this is the veritable cornerstone of the theory of gases, which would be lost without it." Poincaré gave a form of "Maxwell's postulate," allowing him to justify "Maxwell's theorem," which corresponds not to the ergodic hypothesis, but to the quasi-ergodic hypothesis:

> Maxwell admits that, whatever the initial situation of the system, it will always pass an infinite number of times, I don't say through all the situations compatible with the existence of integrals, but as close as one would like to any one of these situations.[18]

This expression was surely inspired by his recurrence theorem, in which return is not exact.[19]

A Theorem on Non-uniform Integrals

Liouville's theorem implies that the motion of a representative point defines a continuous point transformation that conserves the extension in phase. In the ensemble approach this implies that the distribution function corresponding to the

[18]Poincaré 1894, 252, 255–256. Equiprobability is considered in Poincaré 1896, §89 (course on probability of 1893–94). There he considers a conservative mechanical system obeying Hamilton's equations, for which the initial conditions are unknown; he admits that the probability of finding it within a volume is proportional to the magnitude of the volume, and deduces Liouville's theorem.

[19]Brush 1976, 372, believes that Poincaré confused the two hypotheses. It would be more natural not to assume that Poincaré was unaware of the distinction between the two hypotheses, but that he found the second one more natural. Maxwell did not distinguish them. Maxwell 1879, *Scientific Papers* 2, 720. In rebuttal, von Plato 1994, 102, praises the 1894 article: "[It] contains the essential concepts that much later became the tools of the trade of ergodic theory: the requirement that the trajectories be dense, and that this holds, except for a set of initial conditions of probability 0".

permanent state should be constant along each trajectory. Therefore the equilibrium distribution should, in all generality, have the form

$$\rho_0 (q, p) = F(E, \psi_2, \ldots, \psi_{2n-1}) ,$$

F being an arbitrary function of the integrals ψ_i (functions of p and of the q that remain constant along the length of each trajectory) of the system of $2n$ Hamiltonian equations for a conservative system. Maxwell, in 1879, believed that it is the ergodic hypothesis that justifies that the function F depends only on the energy. Boltzmann reflected a great deal on the justification of the ergodic hypothesis and therefore on the "effacement" of the $2n-2$ first integrals, and it is probable that these reflections made him doubt the validity of that hypothesis for the general case of gases composed of polyatomic molecules.[20]

Toward 1890, Poincaré formulated a theorem asserting the non-uniformity of the integrals, apart from energy, of the canonical equations of celestial mechanics. This result concerned perturbative methods of solving Hamilton's equations. The theorem illuminated one of the major problems in the foundations of classical celestial mechanics – the justification of the role of energy in the distribution function. A difficult and often ignored question arises. Chapter V of the first volume of the *Nouvelles méthodes de la Mécanique céleste* (1892c) is dedicated to the non-existence of uniform integrals of the canonical equations. Consider a conservative mechanical system, described by $2n$ parameters: n coordinates q and n conjugate momenta p. Poincaré admits that the mechanical system is stable in the sense that no particle leaves a limited region of space. The kinetic energy, the potential energy, and the total energy are easily defined. The $2n$ canonical equations admit $2n-1$ integrals that are independent of time. These integrals are in general non-uniform functions:

> The canonical equations of celestial mechanics do not admit (excepting those exceptional cases that are discussed separately) uniform analytic integrals apart from the energy. (Poincaré 1892c, 8, 253. See also Born 1925, Brillouin 1964, 109)

A uniform integral of Hamilton's equations is a function of the p and the q that remains constant in the course of the evolution of the system. According to the theorem, the energy is the only "well behaved" integral; the others are non-analytic functions, with discontinuities and "bizarre" behaviors. A non-uniform integral of the canonical equations can take a value infinitely close to a given value in the neighborhood of any point of the phase space.

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[20]Boltzmann early on doubted the validity of the ergodic hypothesis, which is why he preferred in 1871 to return to a generalization of Maxwell's *Ansatz*. When he adopted ensembles, he preferred not to justify them by ergodicity, but rather by the empirical fact that the thermodynamic behavior of a system does not depend on initial conditions for given external thermodynamic conditions; see Gallavotti 1994, §3, Barberousse 2000, Chap. V, 158.

This result had already figured in the paper on the three-body problem (1889–90). There Poincaré considered the attempts to integrate the equations of celestial mechanics by trigonometric series whose convergence was unproven. He showed that the series introduced by Hugo Gyldén and by Anders Lindstedt were divergent. This divergence followed from the above general result: the absence of a uniform analytic integral apart from the integral of the living forces that will be valid for all the equations of dynamics (see Robadey (2006, 22, 25–26, 31) and Barrow-Green (1997 § 5.9)).

Poincaré's proof supposes the existence of multiperiodic perturbative solutions by the method of Delauney (variables action-angle). He shows by reductio that if there exists another uniform integral besides the energy, the nullity of its Poisson bracket leads to impossible relations for its Fourier coefficients at various orders of perturbation. Note that the validity of Poincaré's theorem is doubted by some modern authors.[21]

Léon Brillouin notes that non-analyticity (non-uniformity) is closely connected with non-separability:

> This condition [established by Poincaré's theorem] resulted in discontinuities in the solutions obtained by the Hamilton-Jacobi method. It may be explained by the following statement: For a given mechanical problem with energy conservation and no dissipation, one may find a few variables that can be separated away from the system. When this has been done, one is left with the hard core of non-separable variables. This is where the Poincaré theorem applies, and specifies that the total energy is the only expression represented by a well-behaved mathematical function. Many other quantities may appear as "constants" of a certain motion, but they cannot be expressed as analytical and uniform integrals. This means that any kind of modifications in the problem may provoke an abrupt and sudden change of the "constants". This discontinuity may be the result of a very small change in any parameter in the mechanical equations, or, also, in any small change in the initial conditions. (Brillouin 1964, 128)

For him, "The Poincaré theorem contains the justification of Boltzmann's statistical mechanics, which should apply when (and only when) the total energy remains the only well-behaved first integral". In effect, it is reasonable to admit that the forces between molecules and the interactions between partitions are perturbations removing all degeneracy in an action-angle development.[22]

Poincaré himself did nothing to make his theorem known to physicists. His discussion of the role of the principle of conservation of energy, in the preface to his *Thermodynamique* (1892a), does not mention this result.He mentions it

[21]Kolmogorov in 1954 published a theorem contrary to Poincaré's. Arnold and Moser generalized Kolmogorov's result and formulated a theorem known by the acronym KAM. See: Arnold 1978; Cercignani 1998, 158.

[22]Brillouin 1964, 125–126. Borel was one of the rare authors who stated this theory in a treatise on statistical mechanics. Borel 1925, 20.

only in his 1894 article on the kinetic theory, saying only that energy is the only uniform integral for the kind of system for which Maxwell's postulate is reasonable (Poincaré 1894, 253).

Conclusion

The scientific personality of Poincaré is characterized by the breadth of his interests, his familiarity with both French research traditions and foreign works, his predilection for the big questions, his critical spirit, and his subtlety. He took a profound interest in celestial mechanics, electrodynamics, thermodynamics, the calculus of probabilities, among many other questions. His creativity allowed him to build bridges between different domains of his research.

Poincaré was aware of the problem of the mechanistic reduction of Carnot's principle. First, he was interested above all in Helmholtz's work on monocyclic systems. The issue had already had an echo in France (Alfred Ledieu and Jules Moutier were interested in a similar analogy proposed by Clausius). Poincaré admired Helmholtz's work in other domains, which doubtless encouraged this more specific interest. Poincaré taught and developed these ideas, shortly after their publication; he extended Helmholtz's argument in the case of vibratory motions that represent heat in Ampère's conception. And he showed that, in spite of their interest, these considerations would not allow for an explanation of irreversibility. At that time, he knew only the outlines of the work of Maxwell and of Boltzmann on the kinetic theory. Electromagnetism was one of the subjects of his first courses on mathematical physics (1889/90); Poincaré gave particular emphasis to the epistemological significance of Maxwell's Lagrangian formulation of electromagnetism, which is one of the great examples of a new phase in the evolution of that physics that Poincaré called "the physics of the principles". In this framework, Maxwell formulated the theorem of the existence of an infinite number of mechanical models compatible with a Lagrangian system, which suggests an argument for the underdetermination of theories by empirical evidence. In addition, Maxwell's reflections anticipated Poincaré's idea of a plurality of inter-translatable languages. This was an idea that encouraged Poincaré's interest in all of Maxwell's work.[23]

Poincaré was able to establish connections between his research in celestial mechanics and the foundational problems of classical statistical mechanics (the ergodic hypothesis and irreversibility). In these two domains, he gave a central role to the concept of probability for continuous variables. He noted that if his recurrence theorem were incompatible with the absolute validity of the second principle, it would be compatible with the probabilistic interpretation of entropy. Another result obtained by Poincaré, the non-uniformity of the first integrals of Hamilton's

[23] Príncipe 2012. Helmholtz was then a foreign member of the Académie des Sciences.

equations, also concerned the foundations of statistical mechanics. The importance of this result was not emphasized by Poincaré, and it remained in the shadows until the 1920's. It stays ignored by most treatments of statistical mechanics. He also touched on the problem of the limits of prediction in classical mechanics. In his so-called popular works, Poincaré affirmed his epistemological pluralism, and often spoke of the kinetic theory and the importance of probabilities.

In 1906, Poincaré would publish a paper on the kinetic theory of gases, in which he showed a profound understanding of Gibbs's treatise and gave a very subtle analysis of irreversibility. He introduced two concepts, *coarse-grained entropy* and *fine-grained entropy*, which represent a "substantialization" of the ideas discussed in Chap. XII of Gibbs's treatise: fine entropy always remains constant, while coarse entropy, that of the physicists, "that which depends on our usual means of investigation," is constantly increasing (Poincaré 1906, *Œuvres*, vol. 10, 591). The tendency to irreversibility is therefore a consequence of the limitations on our means of observation. Poincaré would treat two problems that were simpler than that of gases (the small planets, and a gas in one dimension) to show that the tendency to equilibrium can be treated analytically. He showed that, for a system with a finite number of particles, recurrences are inevitable and Carnot's principle is not absolutely valid. Poincaré also showed that, in a system that comes to equilibrium, its apparent disorder may hide a latent order because of previous state of equilibrium. This last notion is motivated by his reflections on the initial notions of Boltzmann's treatise, notions of disposition without molar organization – *molar ungeorgnete* – and of disposition without molecular organization – *molekular ungeordnete.* The article ends with the difficult problem of rarified gases. Poincaré suggests that the behavior of gases can be composed as a mixture of the behavior of a gas in one dimension and the three-dimensional gas of the kinetic theory; for short times of evolution, the first kind of behavior is fundamental. (See Príncipe 2008, §10.8).

Poincaré's epistemological conceptions, his appreciation of the limits of classical mechanics, and his taste for the theory of probability explain his openness to probabilistic explanations in physics, an openness that was rather rare at this period in France. His writings on probability and on the kinetic theory inspired the next generation of researchers, especially Émile Borel.

References

Ampère, A.M. 1835. Note de M Ampère sur la Chaleur et sur la Lumière considérées comme résultant de mouvements vibratoires. *Annales de chimie et de physique* 58: 432–444.

Arnold, V.I. 1978. *Mathematical methods of classical mechanics*, 2nd ed.: 1989. Berlin: Springer-Verlag.

Barberousse, A. 2000. *La Physique face à la probabilité*. Paris: Vrin.

Barberousse, A. 2002. *La Mécanique statistique de Clausius à Gibbs*. Paris: Berlin.

Barrow-Green, J. 1997. *Poincaré and the three-body problem*. New York/London: The American Mathematical Society/The London Mathematical Society.

Bierhalter, G. 1993. Helmholtz's mechanical foundation of thermodynamics. In Cahan 1993, 432–458.

Boltzmann, L. 1866. Über die mechanische Bedeutung des zweiten Haupsatzes der Wärmetheorie, Kaiserliche Akademie der Wissenschaften zu Wien, matematisch-naturwissenschafliche Klasse. *Sitzungsberichte* 53(2): 195–220; also in Boltzmann 1909, 1: 9–33.

Boltzmann, L. 1868. Studien über das Gleichgewicht der lebendigen Kraft zwischen bewegten materiellen Punkten, Kaiserliche Akademie der Wissenschaften zu Wien, matematisch-naturwissenschafliche Klasse. *Sitzungsberichte* 58: 517–560.

Boltzmann, L. 1896a. *Vorlesungen über Gastheorie I. Theil*. Leipzig: Barth.

Boltzmann, L. 1896b. Zu Hrn Zermelo's Abhandlung über die mechanische Erklärung irreversibler Vorgänge. *Annalen der physik* 60: 392–398. English translation: Brush 1966, §8.

Boltzmann, L. 1896c. Entgegnung auf die wärmetheoretischen Betrachtungen des Hrn. E. Zermelo. *Annalen der physik* 57: 773–784. English translation: Brush 1966, §8.

Boltzmann, L. 1871. Zur Priorität der Auffindung der Beziehung zwischen dem zweiten Haupsatze der mechanischen Wärmtheorie und dem Prinzip der kleinsten Wirkung. *Annalen der physik* 143(2), 23(5): 211–230.

Borel, E. 1906. Sur les principes de la théorie cinétique des gaz. *Annales de l'Ecole Normale Supérieure* 23: 9–32; *Œuvres* 3: 1669–1692.

Borel, E. 1925. *Mécanique statistique classique*. Paris: Gauthier-Villars.

Born, M. 1925. *Vorlesungen über Atommechanik*, vol. 2. Berlin: Springer. English translation: 1927. *The Mechanics of the Atom*. London: Bell.

Boyer, C.B., and U.C. Merzbach. 1968. *A history of mathematics*, 2nd ed. New-York: Wiley.

Brillouin, L. 1964. *Scientific uncertainty and information*. New York: Academic Press.

Brush, S.G. 1976. *The kind of motion we call heat*, 2 vols. (1986 paperback edition). Amsterdam: Elsevier Science Publishers – North Holland.

Bryan, G.H. 1891. Report on the present state of our knowledge of thermodynamics, especially with regard to the second law. *Report of the ... meeting of British Association for the Advancement of Science*, 85–122.

Cahan, D. (ed.). 1993. *Hermann von Helmholtz and the foundations of nineteenth-century science*. Berkeley: University of California Press.

Cercignani, C. 1998. *Ludwig Boltzmann the man who trusted atoms*. Oxford: Oxford University Press.

Clausius, R. 1871. Ueber die Zurückfürhrung des zweiten Hauptsatzes der mechanischen Wärmetheorie auf allgemeine mecanische Principien. *Annalen der physik* 142: 433.

Darrigol, O. 2000. *Electrodynamics from Ampère to Einstein*. Oxford: Oxford University Press.

Darrigol, O. and J. Renn. 2000. *The emergence of statistical mechanics*, Contribution to the Enciclopedia Italiana.

De Broglie, L. 1948. *Diverses questions de mécanique et de thermodynamique classiques et relativistes*, Manuscript course text, with preface by G. Lochak. Berlin: Springer.

Gallavotti, G. 1994. Ergodicity, ensembles, irreversibility in Boltzmann's and beyond. *Journal of Statistical Physics* 78: 1571–1589.

Grattan-Guiness, I. 1980. *From the Calculus to Set Theory 1630–1910: An Introductory History*. Trans. M.M. Perez, 1984. Madrid: Alianza Editorial.

Helmholtz, H. 1884a. Studien zur Statik monocyklisher Systeme. *Sitzungsberichte der Akademie der Wissenchaften zu Berlin* 6 März 1884, 159–177; text no. 115 of *Abhandlungen*.

Helmholtz, H. 1884b. Principien der Statik monocyklisher Systeme. *Journal für die reine und angwandte Mathematik* 97: 111–140 et 317–336; text no. 116 of *Abhandlungen*.

Helmholtz, H. 1886. Ueber die physikalische Bedeutung des Princips der kleinsten Wirkung. *Journal de Crelle* 100: 137–166 et 213–222; text no. 120 of *Abhandlungen*.

Klein, M.J. 1972. Mechanical explanation at the end of the nineteenth century. *Centaurus* 17: 58–82.

Klein, M.J. 1973. The development of Boltzmann's statistical ideas. In Cohen 1973, 53–106.
Langevin, P. 1913. Henri Poincaré: Le physicien. *Revue de Métaphysique et de Morale*, 704 et suiv.
Maxwell, J.C. 1866. On the dynamical theory of gases. *Philosophical Magazine* 32; also in *Philosophical Transactions* 1867, 157: 49–88. Also in Maxwell 1890, XXVIII: 26–78.
Maxwell, J.C. 1871. *Theory of heat*. London: Longman.
Maxwell, J.C. 1879. On Boltzmann's theorem on the average distribution of energy in a system of material points. *Cambridge Philosophical Society's Transactions* 12: 547–570; also 1890, XCIV: 713–742.
Maxwell, J.C. 1890. *The scientific papers of James Clerk Maxwell*. Cambridge: Cambridge University Press.
Maxwell, J.C. 1990. *The scientific letters and papers of James Clerk Maxwell*, 3 vols. (vol. 1, 1990; vol. 2, 1995), ed. P. Harman. Cambridge: Cambridge University Press.
Poincaré, H. 1889. Sur les tentatives d'explication mécanique des principes de la thermodynamique. Académie des Sciences. *Compte rendus hebdomadaires des séances* 108: 550–553; *Œuvres* 10: 231–233.
Poincaré, H. 1890a. Sur le problème des trois corps et les équations de la dynamique. *Acta Mathematica* 13: 1; *Œuvres* 7: 262.
Poincaré, H. 1890b. *Electricité et optique I. Les théories de Maxwell et la théorie électromagnétique de la lumière* (lectures at the Sorbonne 1888). Paris: J. Blondin ed.
Poincaré, H. 1891. Le problème des trois corps. *Revue Générale des Sciences* 2: 1–5; *Œuvres* 8: 529.
Poincaré, H. 1892a. *Thermodynamique*, lectures presented during the fist semester of 1888–89, (1st ed. de 1892; 2nd ed. 1908). Paris: George Carré ed.
Poincaré, H. 1892b. Réponses de Poincaré à Tait à propos de l'article de Tait Poincaré's Thermodynamics. *Nature* 45: 414–415, 485, et 46: 76; *Œuvres* 10: 236–239.
Poincaré, H. 1892c. *Les nouvelles méthodes de la mécanique céleste*, vol. 1 (entire edition published between 1892 et 1899); English translation: Dover (New York), revised edition, Albert Blanchard (Paris).
Poincaré, H. 1893a. Le mécanisme et l'expérience. *Revue de Métaphysique et de Morale* 1: 534–537.
Poincaré, H. 1893b. Sur une objection à la théorie cinétique des gaz. Académie des Sciences. *Compte rendus hebdomadaires des séances* 116: 1017–1021; *Œuvres* 10: 240–243.
Poincaré, H. 1894. Sur la théorie cinétique des gaz. *Revue Générale des Sciences* 5: 513–521; *Œuvres* 10: 246–263.
Poincaré, H. 1896; 2ème éd.: 1912. *Calcul de Probabilités*. Paris: Gauthier-Villars.
Poincaré, H. 1906. Réflexions sur la théorie cinétique des gaz. *Journal de Physique* 5: 369–403; *Œuvres* 10: 587–619.
Poincaré, H. 1954. *Oeuvres de Henri Poincaré*, 11 vols. Paris: Gauthier-Villars.
Príncipe, J. 2008. *La réception française de la mécanique statistique, thèse doctorale dirigée par Olivier Darrigol*. Paris: Université Paris Diderot (Paris 7).
Príncipe, J. 2010. L'analogie et le pluralisme méthodologique chez James Clerk Maxwell. *Kairos Journal of Philosophy and Science* 1: 55–74.
Príncipe, J. 2012. Sources et nature de la philosophie de la physique de Henri Poincaré. *Philosophia Scientiae* 16(2): 197–222.
Robadey, A. 2006. *Élaboration d'un énoncé qui porte sur le degré de généralité d'une propriété: le travail de Poincaré autour du théorème de récurrence*. Paris: REHSEIS.
Thomson, W. 1892. On a decisive test-case disproving the Maxwell-Boltzmann doctrine regarding distribution of kinetic energy. *Philosophical Magazine* 33(5): 466.
Thomson, W. 1893. *Conférences scientifiques et allocutions*, traduites et annotées sur la deuxième édition (1891) par P. Lugol, agrégé de Sciences physiques, Professeur au Lycée de Pau, avec des extraits de mémoires récents de Sir W. Thomson et quelques notes par M. Brillouin, maître de conférences à l'ENS; Paris: Gauthiers Villars.

Truesdell, C. 1975. Early kinetic theory of gases. *Archive History of Exact Sciences* 15: 1–66.
Von Plato, J. 1994. *Creating modern probability*. Cambridge: Cambridge University Press.
Whittaker, E. 1899. Report on the progress of the solution of the problem of three bodies. *Reports of the British Association*, 121–159.
Zermelo, E. 1896. Ueber einen satz der Dynamik und die mecanische Warmetheorie. *Annalen der Physik* 57: 485–494. English translation: Brush 1966, 2: 208–217.

Henri Poincaré: A Scientist Inspired by His Philosophy

Isabel Serra

Abstract This paper attempts to analyze the philosophical connections that Poincaré established between the domains of physics and mathematics, both explicitly in his philosophical work, and implicitly in his original solutions in mathematics. Particular emphasis will be placed on the signs of coherence or incoherence between what is explicit and what is implicit, that is, between his thought in general and his scientific practice. In Poincaré's early work on the group-theoretical approach to differential equations, we see the beginnings of an original way of connecting geometry with physics. Similarly, in his attack on the three-body problem in celestial mechanics, and his study of the stability of the solar system, we see a geometrical approach replacing the analytical one. His group-theoretic approach to geometry later became the basis for his approach to the "dynamics of the electron" between 1904 and 1906, an important part of the history of relativity theory. These are examples of the ways in which, according to Poincaré, "l'esprit mathématique" leads to the "true, profound analogies," that is, the deep structural forms, at the foundations of our physical theories. The understanding of these connections in practice will illuminate Poincaré's philosophical view of the connection between mathematics and physics. But we can say that, on the other hand, Poincaré's philosophical views also influenced his scientific work.

Introduction

Henri Poincaré is often regarded as one of the "last universalists". His universalism, which expressed itself at the end of his life by the plurality of his fields of knowledge and the scope of his work, was also expressed, since his youth, by his ability to create

I. Serra (✉)
Centre for Philosophy of Science at the University of Lisbon (CFCUL), University of Lisbon, Campo Grande, C4, 4.3.22, Lisbon 1749-016, Portugal
e-mail: isabelserra@netcabo.pt

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intersections between different fields of knowledge. Crossing the barriers between mathematics, physics and philosophy, as well as between different mathematical fields, Poincaré produced innovative and unexpected results. It is not difficult to find, in the whole of his scientific and philosophical work, examples that illustrate this scientific ability.

In order to highlight the links established by Poincaré between various fields of mathematics, but also between geometry, physics, and philosophy, it is essential to emphasize certain points of his scientific and philosophical career, especially those contributions that can be considered as the major ones in light of the interconnections between different areas: Fuchsian functions, non-Euclidean geometry and differential equations, non-Euclidean geometry and philosophy and, ultimately, geometry and physics.

The Fuchsian Functions: A Starting Point of the Crossing of Knowledge

When Poincaré wrote: "the mathematician should not be for the physicist a simple provider of formulas; there must be a more intimate collaboration between them" (Poincaré 1905a, 104), he was highlighting that the intersection of knowledge was for him an explicit choice, at least at the later stage of scientific maturity.

The link he saw between mathematics and physics, revealed by the phrase just quoted, is clearly present throughout his work. Sometimes, the relationship between the two domains was established involuntarily during his research, and they only become obvious with the development of results, as was the case with the invention of Fuchsian functions. Poincaré himself verified that such things happen. The case of Fuchsian functions illustrates his question: "Who has taught us to know the real, deep analogies that the eyes do not understand but reason is able to see?" and especially the answer: "It is the mathematical spirit, which disdains the material to focus only on the pure form" (Poincaré 1905a, 106), such as to conform itself to fit to the case of Fuchsian functions.

In 1997, Jeremy Gray and Scott Walter published some previously unpublished work of Poincaré (Gray and Walter 1997, 1–25), including the discovery of the Fuchsian functions, as well as Poincaré's establishment of the relationship between these functions and the transformations of non-Euclidean geometry. This discovery of Poincaré resulted from one of his first research topics, the theory of differential equations in the complex domain. Between 1878 and 1881 he worked on several aspects of this issue and produced results that would prove to be fundamental in the evolution of mathematics. His geometrical perspective and his use of the theory of groups in the study of differential equations have opened new avenues, in particular, as regards to its applications. On this research Jeremy Gray writes:

> The author was very interested in the overall theory of differential equations, whether first-order real gold linear and complex. Within two years his work was to transform both

> subjects completely, opening up whole new aspects of research in the one, and in the other leaving little, it has been said, for his successors to do. (Gray 1981, 282)

Poincaré himself recalled this research almost 30 years later. To illustrate the process of mathematical invention, he described the moment when, during a trip on an omnibus, it connected two mathematical fields:

> At the time where I was putting the foot on the step, the idea came to me, seemingly without anything in my earlier thoughts to prepare me, that the transformations I used to define the Fuchsian functions were identical to those of non-Euclidian geometry. (Poincaré 1908, 361)

This idea of Poincaré, which emerged at the beginning of his scientific career, was the starting point of several avenues of research that he himself, but also other mathematicians, travelled in the following years. First of all, the theory of functions that Poincaré called "Fuchsian" (later called "automorphic") brought "a power of discovery to non-Euclidean geometry, whose interest up to then had appeared to be limited to philosophy" (Walter 1996, 95). Indeed, this work of Poincaré is at the origin of the change in status of non-Euclidean geometry. As a result of his publications on the subject, in the following years, many mathematicians took an interest in the topic and taught it in the universities (Walter 1996, 95–96). Poincaré contributed decisively to the birth of this new branch of geometry, whose importance was still unsuspected.

The discovery of Fuchsian functions and their connection with non-Euclidean geometry was an event that had a strong impact in science, and "has helped open a fertile ground for the new methods of the theory of groups" (Gray 1984, 10).

The discovery was also rich in consequences for the scientific journey of Poincaré. Non-Euclidean geometry became for him an object of philosophical thought, which led him to question the empirical origin of geometry and the nature of physical space (Poincaré 1902, 63–108). His geometrical conventionalism was developed within this framework (Giedymin 1977, 271–301). In addition, we can see non-Euclidean geometry as one of his paths to physics. At the time when Poincaré was working on Fuchsian functions, it was impossible to know what relationships existed between Euclidean geometry and physical geometry. Indeed, it is only much later that non-Euclidean geometry would find an application in relativity. On the other hand, the qualitative study of differential equations in Poincaré's first research work also has connections with the three-body problem, as well as with topology. For Nabonnand, "we have to see the origins of Poincaré's topological investigations in his early work on the qualitative theory of differential equations" (Nabonnand 2000, 35). Stillwell also emphasizes "the importance of the theory of Fuchsian functions for the genesis of some of Poincaré's topological ideas" (Nabonnand 2000, 35). Research on non-linear differential equations and the study of the global properties for their solutions thus links the work of Poincaré to non-Euclidean geometry, topology, physics and the philosophy of science. Another link with physics was established through non-Euclidian geometry which, in turn, greatly influenced his philosophy.

The ability to integrate different areas of knowledge is characteristic of Poincaré's thought, and it can be seen as one of the sources of his philosophy, often considered as "the philosophy of a scientist". However, it is important to ask like Laurent Rollet asked, "did Poincaré's philosophy benefit from as much extensive study as his scientific work?" (Rollet 2007, 7) This question, as well as Rollet's answers, has led to new perspectives on the interaction between science and philosophy in the works of Poincaré. Indeed,

> A body of evidence supports an anchorage of Poincaré's thought in traditional philosophy, i.e. within an intellectual sphere that does not necessarily have mathematical or physical theories as its object. (Rollet 2007, 7)

To take Rollet's questions a bit further, one might consider whether, in addition, the scientific thought of Poincaré was influenced by philosophy. Did the philosophical knowledge of which Rollet speaks contribute to Poincaré's scientific choices? Was his research guided by his "philosophical spirit"? These hypotheses might help to explain his ability to cross boundaries between different areas of knowledge given that Poincaré's philosophical thought would work as an inspiration for his scientific work, which, in turn, would be a source of his philosophical thought.

Differential Equations: A Path to Geometry

Differential equations are historically related to the development of mathematical analysis and problems in geometry or mechanics. Until the eighteenth century focus was on calculating solutions of these equations for already known functions. However, in the nineteenth century, methods evolved. Non-integrability of certain differential equations led in particular to the use of geometry to study the qualitative behaviour of their solutions. The geometric study of differential equations starts with L. Cauchy (1789–1857), C. Briot (1817–1882), J. C. Bouquet (1819–1885), and L. I. Fuchs (1833–1902). Leaving aside the analytical point of view, these mathematicians studied the properties of integral curves in the neighborhood of a point. Poincaré studied the global behavior of the curved solutions of differential equations, based on their results though breaking with local terms used by his predecessors. He analyzed these solutions "over the extent of the plane" (Poincaré 1881, 376), using the group-theoretic approach in the treatment of Riemann surfaces (Gray 1981, 273).

This innovative approach would prove to be very fruitful. In fact, in the publication of his work, Poincaré noted the "vast field of discoveries that opens before the surveyor" (Poincaré 1881, 377) as well as one of the applications of his method, the three-body problem (Poincaré 1881, 376), of which his study would bring him King Oscar II of Sweden Prize in 1887.

The study of the geometric behavior of curved trajectory solutions of differential equations was presented in a series of memoirs published in 1881 in the *Journal des Mathématiques Pures et Appliquées* (Poincaré 1881, 375–422), 1882 (Poincaré 1882, 251–286), 1885 (Poincaré 1885, 167–244) and 1886 (Poincaré

1886, 151–217). One of the results of this study is the first definition of what is now called "chaos":

> Occasionally, small differences in the initial conditions generate very large ones in the final phenomena. A small error in the first would produce a huge mistake in the latter. Prediction becomes impossible. (Poincaré 1908, 62)

The work of Poincaré on differential equations contains still other aspects and other major consequences, in particular the development of the study of the functions that he has named "Fuchsian".

> At that time he developed the qualitative theory of differential equations in the 1880s, the young Poincaré develops the theory of Fuchsian functions with the explicit objective of integrating linear functions with algebraic coefficients. (Nabonnand 2000, 36)

Poincaré used and developed some ideas of Lazarus Fuchs on solutions of these differential equations, which led him to establish contact with the German mathematician.[1] In this scientific and friendly correspondence, Poincaré asked and received Fuchs' permission to name the solutions of differential equations "Fuchsian functions". Poincaré's first papers on this work were submitted in 1880 to the Academy of Sciences for the prize competition in the mathematical sciences (Poincaré 1916–1956, vol. i., 336–372). Some of these results have been published after the discovery by Jeremy Gray of three supplements to this memoir by Poincaré in the competition (Gray 1981, 297). Such is the case with the connection between Fuchsian functions and non-Euclidean geometry that Poincaré highlighted in this memoir. It was this discovery that made Poincaré famous among the mathematicians of the time (Gray 2012, 179). Not only it has had important consequences for mathematics, but it is also rich in historical details (Gray 2012, 178–179).

What should be emphasized here, is that in addition to the prestige that it brings to Poincaré's role in mathematics, the idea of linking the Fuchsian functions to non-Euclidean geometry shows Poincaré's rare ability to associate mathematical work and philosophical thinking, which is a style that can also be seen as a sign of his philosophical thinking. In spite of that, one of the characteristics of his early work was ignorance of publications related to his research, especially in German literature. According to Jeremy Gray,

> The published work makes abundantly clear the astounding clarity of Poincaré's mind, coupled to an almost equally dramatic ignorance of contemporary mathematics. There is no mention of the work of Schwarz on the hypergeometric equation (. . .) nor is there any mention of the work of Dedekind or Klein, and even Hermite's work on modular functions, which he must have known, seems to have been forgotten. We shall see that these omissions are not mere oversights; Poincaré genuinely did not then know the German work. The contrast with the deliberately well-read Felix Klein could not be more marked. (Gray 1981, 298)

[1] The correspondence between Poincaré and Fuchs was published in Poincaré 1916–1956, Vol. XI, 13–25.

In any case, Felix Klein initiated and maintained a correspondence with Poincaré after reading the three notes "On Fuchsian functions" published in *Comptes Rendus* (Poincaré 1916–1956, vol. II, 1–10). Following this correspondence, in which a number of considerable ideas was traded (Gray 1981, 303–332), in particular on groups and non-Euclidean geometry, the two mathematicians published several works. In one of his articles, Poincaré establishes the relationship between number theory and non-Euclidean geometry (Poincaré 1916–1956, vol. II, 38–40), that is a new relationship between two mathematical fields, which he has had the pleasure of discovering (Gray 1981, 324). The following year Poincaré published two articles in the *Acta Mathematica*, one on the Fuchsian groups (Poincaré 1916–1956, vol. II, 108–168) and the other on Fuchsian functions (Poincaré 1916–1956, vol. II, 169–257), which had results that went far beyond those of Klein (Gray 1981, 333).

It may be said that Poincaré and Klein developed this work in a collaborative way, but the vision of Poincaré was much deeper and more modern (Gray 1981, 323), even if he was unaware of some results of Riemann. Poincaré was able to connect several areas of knowledge throughout his work, which was consistent with the idea of mathematical unity defended by Klein. However,

> This insight of Poincaré, so painfully gained by Klein, testifies to the strong hold Klein's idea of mathematical unity had upon him. The paradox is that Klein, who had done so much to further non-Euclidean geometry in the 1870's, did not appreciate it here. (Gray 1981, 323)

Once more the results of Poincaré show the interweaving between philosophy and mathematics inherent in his thinking.

Poincaré's work on differential equations occupied the first years of his scientific life and covered multiple aspects of the issue. He especially made connections that other scientists mathematicians did not, even those with more extensive knowledge. Indeed, he brought together differential equations Fuchsian functions, non-Euclidean geometry, and the theory of groups. As Jeremy Gray highlights, throughout tens of pages in his work *Differential equations and group theory from Riemann to Poincaré,* some relations between these areas were already being considered, but Poincaré, despite his "dramatic ignorance of mathematics of his time" (Gray 1981, 298), fleshed them out with a determination that was not annihilated by trade with Fuchs or Klein (Gray 1981, 273–379).

It is also possible to identify Poincaré's philosophical thought in his research on differential equations in the way that he considered the relationship between physics and mathematics.

The interaction between physics and mathematics, which had been much transformed since the first mathematical studies of mechanics in the seventeenth and eighteenth centuries, had a great evolution in the course of nineteenth century, and even a complicated one. It looks as if during this century there was a divorce between physics and mathematics, to use the term used by Jeremy Gray (1981, 273–339). Indeed, most mathematicians, even those who seemed to give much importance to ties with physics, had not established these links in their publications. In fact in Germany, pure mathematics only emerged as a specialty in the University (Gray 1981, VIII).

The treatment of differential equations by Poincaré, as well as by other mathematicians, exhibited the characteristics of purely mathematical work and developed independently of applications, even when these equations were originally related to problems in physics. Recognition of the relationship between these two areas, then growing separately, appears, however, at several times in the work of Poincaré as part of his philosophical thought. The way he envisaged the relationship between physical phenomena and mathematical instruments, allows, moreover, a certain reading of its physical "conventionalism". Indeed, it seems that the possibility of finding results in a purely mathematical way, but still consistent with the phenomena, played a role in Poincaré's conventionalism. One can even place the question if it was the case that for Poincaré, the extensive use of mathematics in the analysis and prediction of the physical facts made physics "conventional."

Poincaré's first research work, namely the study of differential equations, which was developed somewhat in ignorance (for example, of the publications of Riemann) led him to several mathematical innovations, in particular the idea of linking these equations to the automorphic functions, to geometry, and to groups. Poincaré was not looking for working links between physics and philosophy, which he actually found later. However, in this first phase of the scientific life of Poincaré, we can say that we can already see a link with physics being established through geometry, one of the instruments that he later uses in the study of differential equations. This incursion of the geometry in the treatment of a subject, which was especially associated with mathematical analysis, can be seen as an example of what Poincaré himself called "mathematical intuition":

> Thus, logic and intuition have each their necessary role. Both are indispensable. Logic, which can only give certainty, is the instrument of demonstration: intuition is the instrument of invention. (Poincaré 1905a, 37)

In the classification that Poincaré made of mathematicians (Poincaré 1905a, 27), he would certainly belong among the intuitive ones. The power of discovery made by his geometric approach to differential equations is an illustration of this intuitive mind. On the other hand, the discovery of the link between Fuchsian functions and non-Euclidean geometry, which can be also considered as the fruit of his intuition, had a great impact on Poincaré's scientific and philosophical future. In particular, these geometries brought him closer to the question of the nature of the physical space.

The ideas developed so far may be summarized by saying that Poincaré's mathematical results, although obtained using mathematical instruments and methods, eventually carried him along roads shared by other areas of knowledge. To complete this journey he needed qualities that it would be difficult to describe without resorting to psychology, or to cognitive science or perhaps, even better, to studies concerning mathematical creation (Van-Quynh 2013).

Non-Euclidean Geometry: A Tool and a Philosophy

The appearance of non-Euclidean geometry, as was developed by Bolyai (1802–1860) and Lobachevsky (1792–1856) in the middle of the 19th century, resulted in a set of discussions and work around the foundations of geometry. The first publications that take the new geometries into account are those of Riemann (1826–1866) and Helmholtz (1821–1894) (Torretti 1984, 154).

Helmholtz in his 1870 conference "On the origin and significance of the axioms of geometry" (1870), presented the work of Bolyai, Lobachevsky, Gauss (1777–1855), and of Riemann and himself as the scientific foundation of an empiricist philosophy of geometry in opposition to the Kantian a priori (Torretti 1984, 163). His theses, which exerted a profound influence on Poincaré, also contain the first assertions of conventionalist character in geometry (Torretti 1984, 163). Indeed, he offered some examples of visualization of non-Euclidean situations to emphasize that the axioms of geometry are not given a priori.

One of the issues raised by the emergence of non-Euclidean geometry is the compatibility of multiple geometries with physical measurements: In the nineteenth century and the physics of the time, geometry was naturally interpreted as the science of space, and space was understood as a real entity. But paradoxically, the geometric proposals could neither be corroborated nor refuted by events that took place in this "real" space. Indeed, from Plato to Kant, including for the empiricists, geometry was thought to describe reality, although it is the result of independent experience and a priori knowledge (Torretti 1984, 244). The invention of non-Euclidean geometry naturally raised the question of the nature of the physical space. Even if it was the case that from the point of view of mathematics the existence of multiple geometries posed no problem, it remained puzzling from the point of view of physical measurements which was the true geometry of space.

For Helmholtz, geometry is not simply a working basis for mechanics, but must be built jointly with it (Torretti 1984, 169). According to him, if to the axioms of geometry we add proposals relating to the physical or mechanical in the body, then this same outcome would put forward a system of proposals which could be refuted or confirmed by experience. Helmholtz thought it was possible to determine the foundations of geometry from the principles of mechanics. Thus geometry would be characterized by the properties of the movements of rigid solids. Furthermore, Felix Klein showed that these movements form a group, bringing another dimension to the results of Helmholtz:

> These are the work of Helmholtz, inspired by the research of Riemann, which form the logical link between them and the theory of groups of transformations. (Rougier 1920, 56)

Sophus Lie (1842–1899), in *The theory of groups of Transformation* (Lie 1888–93), included a few aspects of the work of Helmholtz. In particular, he applied the transformation group theory to the problem of Helmholtz in order to determine a system of postulates, which is at the base of general geometry, and that would be tantamount to the determination of all possible types of kinematic displacement.

Lie's approach to the problem of the movement of rigid solids was influenced by the conception of his friend Felix Klein. According to Lie, it was Klein, who first suggested the use the theory of transformation groups in this area (Torretti 1984, 171).

In 1872, Felix Klein, in his "Erlangen program" (Klein 1892, 87) defined, unified, and classified the different geometries using precisely the concept of transformation group. His research helped in particular to highlight the fundamental property that motions have when they form a group. Lie's work showed that geometry can be reduced to the study of a group; in particular, ordinary geometry is the study of the Euclidean group of displacements. We can therefore define geometry as the study of geometric properties that remain invariant under a group of transformations, a point of view which was systematized by Félix Klein (Klein 1891, 173–180). After Klein's work, each geometry, Euclidean or non-Euclidean, is characterized by the group to which it corresponds and the transformations and their associated invariants. In this sense a given geometry may be equivalent to another, and what distinguishes them are the transformation groups and invariants associated of each geometry.

At the time when he wrote his work on Fuchsian functions, Poincaré shows that he already knew non-Euclidean geometry, although it is difficult to determine the origin of this knowledge (Gray and Walter 1997, 15–16). And it is also the case that for Poincaré, geometry is nothing but the study of a specific group (Gray and Walter 1997, 15–16).

The equivalence of geometries that results from the work of Riemann, Helmholtz, Klein and Lie, among others, as well as the use that Poincaré made of non-Euclidean geometry, became a subject of philosophical reflection for Poincaré, the results of which were published in 1887 (Poincaré 1887, 203–216), in 1891 (Poincare 1891, 769–774), and later republished in *Science and Hypothesis* (1902). One celebrated result is the geometrical conventionalism that remains up to now the subject of so many questions and interpretations (Giedymin 1992, 423–443).

The use of non-Euclidean geometries was a success from the mathematical point of view. The three supplements discovered by Jeremy Gray, mentioned before, reveal how the idea of linking the Fuchsian functions to transformations of non-Euclidean geometry allowed him to advance so quickly in his research (Gray and Walter 1997, 15).

Although, according to Scott Walter, "a likely source of the philosophy of geometry of Poincaré lies in the debates around the logical consistency and the physical meaning of non-Euclidean geometry from 1870 to 1880" (Walter 1996, 89), it is legitimate to ask whether there is not also, as Rougier suggested, a link between Poincaré's use of non-Euclidean geometries in his early work on differential equations and his conventionalism:

> It seems that Poincaré was able to use this theory very soon, following the famous use he made of non-Euclidean geometries during his research on Fuchsian functions, to solve a problem of ordinary geometry. (Rougier 1920, 147)

However, while the discovery of the connection between Fuchsian functions and non-Euclidean geometries awoke the interest of mathematicians in non-Euclidean geometry (Walter 1996, 95), the same did not happen with Poincaré's conventionalism.

Indeed, Poincaré's conventionalism seems to make discussion of the nature of the physical space unnecessary, which did not please geometers, who were not about to abandon the ability of empirically establishing the geometrical structure of the space (Walter 1996, 90) nor, of course, did it please physicists. Nevertheless, if Poincaré's philosophical position on geometry seems to have distanced him from the problem of the nature of the space, it maintained a fundamental role in Poincaré's relationship with physics. In fact, one of the pathways that led Poincaré to physics a few years later were the non-Euclidean geometries. His interests in the problems of physics were also awakened by his teaching of mathematical physics.

Geometry, Physics, and Philosophy

As Professor at the Sorbonne from 1886, Poincaré taught various subjects, including mathematical physics. Even though until then he was primarily a mathematician (Darrigol 2000, 352), his links with physics had already been established, whether through his research subjects, or through geometry. Furthermore, "mathematical work on the theory of differential equations led him naturally to become interested in mathematical physics" (Houzel and Paty 1999, 7).

Houzel and Paty describe Poincaré's teaching at the Sorbonne in the following manner,

> Far from merely reproducing the well-established knowledge of the time, Poincaré presented and discussed the most recent and newsworthy research. So he introduced Maxwell's theory in France and also the work of Hertz, Helmholtz and especially Lorentz's on electrodynamics. (Houzel and Paty 1999, 2)

The analysis of the theories of electrodynamics led him to discuss, in particular, the use of mechanical models in the description of electromagnetic phenomena (Serra and Paz 2010, 267–272). Even if "most theorists have a constant predilection for explanations by mechanics or dynamics means" (Poincaré 1902, 178), Poincaré did not think it fundamental to choose this or that mechanical model to infer physical laws, but only insisted above all that the model had to be simple (Poincaré 1902, 186).

Poincaré's conception of the use of mechanical models can be summed up in the phrase, "if it does not satisfy the principle of least action, there is no mechanical explanation possible; if it does, then there's not only one, but an infinite number of them, and so it follows as soon as there is one, there is an infinite number of them" (Poincaré 1902, 223).

Poincaré developed the arguments that characterize his "physics of principles' and his conventionalism as applied to physics not only throughout his philosophical work, but also in the prefaces of his books (Darrigol 2007, 221–240), In the "General

Conclusions of the third part" of *Science and Hypothesis*, Poincaré synthesizes his ideas on experience, convention and the principles of mechanics that he had already presented in several articles writing that,

> The principles of mechanics therefore come to us in two different aspects. On the one hand, there are the truths based on experience and verified in a very approximate way, as far as nearly-isolated systems are concerned. On the other hand are the assumptions applicable to the whole of the universe and regarded as strictly true. If these assumptions have a generality and certainty that was lacking in the experimental truths from which they are drawn, it is because they are finally reducible to simple convention that we have the right to adopt, because we are certain in advance that no experience will come to contradict them. However, these conventions are not absolutely arbitrary; they do not arrive from our caprice. We adopt them because some experiences have shown us that it would be convenient to do so. This explains how experience could constitute the principles of mechanics, and why, however, it can never reverse them. Compare this with geometry. The basic propositions of geometry, such as for example the postulates of Euclid, are no more than conventions, and it is also as unreasonable to ask whether they are true or false as to ask whether the metric system is true or false. (Poincaré 1902, 152)

For Giedymin, Poincaré's research on the foundations of the geometry has its origin in his physical conventionalism (Giedymin 1977, 271). More recently Gerhard Heinzmann also wrote,

> Setting aside of the difference in size (. . .) Poincaré used in mechanics the same procedure as in geometry in going from empirical laws, understood as general assumptions, to principles, including explicitly conventional elements. (Heinzmann 2012, 9)

Even if "in physics itself, i.e. in optics and electrodynamics, the conventional elements seem weakened compared to the mechanical" (Heinzmann 2012, 12), the conventionalism of Poincaré can be seen as a way to cope with the crisis experienced by the physics of his time. Indeed, the conventionalist conception allows him not only to justify the inconsistencies between the different theories, but also to consider in an original way the replacement of one theory by another (Poincaré 1905a, 123–128).

More than justifying the philosophical relevance of Poincaré's physical conventionalism – as has already been argued (Serra and Paz 2010, 278–281) – it should be emphasized that the conventionalist philosophy brings into his thought yet another link between geometry and physics. If, following Giedymin, we consider that his physical conventionalism was inspired by his geometrical conventionalism (Giedymin 1977, 271) it is through philosophy that the link is established.

Research in mathematical physics, particularly in the field of electrodynamics, would make Poincaré establish further links between geometry, physics, and philosophy. This is what we will discuss in the next section.

Geometry Meets Electrodynamics

During the year 1895 Poincaré published a series of articles discussing several theories of electrodynamics, in particular that of Lorentz, whose theory was "the least defective" (Poincaré 1901, 611). Indeed, at the time, despite its flaws, Poincaré considered that Lorentz's theory "gives a very simple explanation of some phenomena" (Poincaré 1902, 242), which had until then eluded Maxwell's theory. In 1900 in a conference at Leyden (Poincaré 1900, 464–488), Poincaré gave a new interpretation of the Lorentz transformations, in particular of the concept of local time introduced by the Dutch physicist (Reignier 2004, 2). This concept is seen as "one of the most important steps of the discovery of relativity" (Reignier 2004, 5). And "it is possible that the interpretation of Poincaré of local time from Lorentz worked as a trigger for Einstein's thinking" (Darrigol 2004, 4).

The concept of transformation group, which was present from Poincaré's first research on differential equations, would prove equally essential in the context of his work on the equations of electrodynamics. Lorentz had found transformations of coordinates to ensure that Maxwell's equations have the same form in all inertial frames. Poincaré showed that these coordinate transformations, which he called "Lorentz transformations,"[2] define a group, the "Lorentz group." According to Jean Reignier this "constitutes the essence of the principle of relativity" (Reignier 2004, 9). Indeed, considering that the principle of relativity can be deduced from symmetry properties of physical laws, i.e. from the invariance of equations of physics under the action of a group. Therefore, it is possible to suggest that Poincaré arrived at a version of the theory of relativity based on the Lorentz group. His results were published in the paper, "Sur la dynamique de l'électron" (Poincaré 1905b, 494–550). Research on invariants associated with this group led to other results obtained by Poincaré, but also by Minkowski, following his reading of Poincaré's article.

The recognition of the scope of Poincaré's ideas and of their importance to the physics of the twentieth century led Feynman to declare:

> It was Poincaré who had the idea of analyzing how you can transform the equations of physics without changing them. He was the first to take account of the symmetry of physical laws. (Feynman 1967, 94)

The Poincaré symmetry group, so called by Wigner (1967, 15–19), of which the Lorentz group is a subgroup, defines the set of transformations preserving the structure of space-time in special relativity.

Poincaré's role in the emergence of relativity has been the subject of controversy and countless publications. Here, we can only highlight certain aspects of his contribution. We want to emphasize especially that the application of the concept of group in the Lorentz transformations is a paradigmatic example of Poincaré's way of creating by integrating knowledge from different fields. Once more, in the context of relativity, Poincaré used a mathematical object – group – in an innovative way, as he had done from the very beginning of his scientific life in his research on differential equations. And, once more, his idea proved to be successful. Indeed, in the twentieth century, the concepts of symmetry groups and invariance principles have become essential in modern physics, particularly in quantum mechanics (Wigner 1967, 47).

[2]The term "Lorentz transformations" designates a group of transformations that forms a subgroup of the Poincaré transformations.

Conclusions

The first scientific work of Poincaré (1878–1881) connected several branches of mathematics in unexpected ways. In the following decades his research would lead to other connections in various fields, not only within mathematics but also between mathematics, physics and philosophy.

This network of interactions and influences was probably possible because the research subjects were suitable. However, the successful construction of this network by Poincaré happens primarily from his very specific way of approaching problems. Thus, we can ask ourselves: Wasn't Poincaré working out his scientific ideas just like a philosopher?

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References

Darrigol, O. 2000. *Electrodynamics, from Ampère to Einstein*. Oxford: Oxford University Press.

Darrigol, O. 2004. Faut-il réviser l'histoire de la relativité? *La Lettre de l'Acad.Scienc.* n^{o} 14. http://www.academiesciences.fr/activite/archive/dossiers/Einstein/Einstein_pdf/Darrigol%20_amp.pdf, consulted on July 2012.

Darrigol, O. 2007. Diversité et harmonie de la physique mathématique dans les préfaces de Henri Poincaré. In *Pour comprendre le XIX^{e} siècle: Histoire et philosophie des sciences à la fin du siècle*, ed. J.C. Pont et al., 221–240. Florence: Olschki.

Feynman, R. 1967. *The character of physical law*. Cambridge: MIT Press.

Giedymin, J. 1977. On the origin and significance of Poincaré's conventionalism. *Studies in History and Philosophy of Science* 8: 271–301.

Giedymin, J. 1992. Conventionalism, the pluralist conception of theories and the nature of interpretation. *Studies in History and Philosophy of Science* 23: 423–443.

Gray, J. 1981. *Differential equations and group theory from Riemann to Poincaré*, Ph.D. Thesis, Warwick.

Gray, J. 1984. Fuchs and the theory of differential equations. *Bulletin (New Series) of the American Mathematical Society* 10(1): 1–26.

Gray, J. 2012. Poincaré and the idea of a group. *NAW* 5/13(3): 178–186.

Gray, J.J., and S.A. Walter (eds.). 1997. *Three supplements on Fuchsian functions by Henri Poincaré*. Berlin: Akademie-Verlag.

Heinzmann, G. 2012. L'occasionalisme de Poincaré: l'élément unificateur de sa philosophie des sciences. http://ebookbrowse.com/poincare-mathemartical-reasoning-pdf-d299441269, 1–15, consulted on July 2012.

Helmholtz, H. 1870. Über der Ursprung und die Bedeutung der Geometrische Axiome [On the origin and significance of the axioms of geometry]. In *Epistemological writings, H. Helmholtz.* Trans. M. Lowe, ed. R. S. Cohen and Y. Elkana, 1–38. Dordrecht/Boston: Reidel.

Houzel, C., and M. Paty. 1999. Henri Poincaré (1854–1912), Encyclopædia Universalis. In *Dictionnaire de l'Astronomie*, 696–706. Paris: Albin Michel.

Klein, F. 1891. *Ann. Ec. Norm. Sup.*, 87–102; 173–199. Ed. originale: *Vergleichen.de Betrachlnngen über neuere geomelrische Forsckungen, Programm zum Eintritt in die philosophische Fakultàt;* Erlangen (1872).

Lie, S. 1888–93. *Theorie der Transformationsgruppen*, 3 vols. Lepizig: Teubner.
Nabonnand, P. 2000. Les recherches sur l'œuvre de Poincaré. *SMF – Gazette* 85: 33–48.
Poincaré, H. 1881. Sur les courbes définies par une équation différentielle I. *Journal de mathématiques pures et appliquées* S. 3(7): 375–422.
Poincaré, H. 1882. Sur les courbes définies par une équation différentielle II. *Journal de mathématiques pures et appliquées* S. 3(8): 251–286.
Poincaré, H. 1885. Sur les courbes définies par une équation différentielle III. *Journal de mathématiques pures et appliquées* S. 4(1): 167–244.
Poincaré, H. 1886. Sur les courbes définies par une équation différentielle IV. *Journal de mathématiques pures et appliquées* S. 4(2): 151–217.
Poincaré, H. 1887. Sur les hypothèses fondamentales de la géométrie. *Bulletin de la Société Mathématique de France* 15: 203–216.
Poincaré, H. 1891. Les Géométries non-euclidiennes. *Revue générale des Sciences Pures et Appliquées* 2: 769–774.
Poincaré, H. 1900. La théorie de Lorentz et le principe de réaction. *Œuvres* IX: 464–488.
Poincaré, H. 1901. *Electricité et Optique*. Paris: Gauthier-Villars.
Poincaré, H. 1902. *La Science et l'Hypothèse*. Paris: Flammarion (1968).
Poincaré, H. 1905a. *La Valeur de la Science*. Paris: Flammarion (1970).
Poincaré, H. 1905b. Sur la dynamique de l'électron, Académie des Sciences. *Comptes-rendus* 140: 1504–1508.
Poincaré, H. 1908. *Science et Méthode*. Paris: Editions Kimé (1999).
Poincaré, H. 1916–1956. *Oeuvres*, 11 vols. Paris: Gauthier-Villars.
Reignier, J. 2004. Poincaré synchronization: From the local time to the Lorentz group. *Proceedings of the Symposium Henri Poincaré* (Brussels, 8–9 October 2004). www.ulb.ac.be/sciences/ptm/pmif/ProceedingsHP/Reignier.pdf, consulted on July 2012.
Rollet, L. 2007. *Henri Poincaré: Des Mathématiques à la Philosophie, Étude du parcours intellectuel, social et politique d'un mathématicien au début du siècle*, Thèse, Archives – Centre d'Études et de Recherche Henri-Poincaré.
Rougier, L. 1920. *La philosophie géométrique de Henri Poincaré*. Paris: Librairie Félix Alcan.
Serra, I., and M. de Paz. 2010. Poincaré and the language of physics in the nineteenth century. In *Estudios de Lógica, Lenguage y Epistemologia*, ed. D. Fernández Duque, E.F. Gómez-Caminero Parejo, and I. Hernández Anton, 267–284. Sevilla: Fenix Ed.
Torretti, R. 1984. *Philosophy of geometry from Riemann to Poincaré*, 2nd ed. Dordrecht: Reidel (1st ed. 1978).
Van-Quynh, A. 2013. Dossier: Introspection and Intuition in Mathematics *Kairos*, *Journal of Philosophy & Science* 6: 159–160.
Walter, S. 1996. *Hermann Minkowski et la Mathématisation de la Theorie de la Relativité Restreinte (1905–1915)*, Thèse, Université de Paris.
Wigner, E. 1967. *Symmetries and reflections*. Indiana: Indiana University Press.

Poincaré on the Construction of Space-Time

Robert DiSalle

Abstract One of the enduring challenges for the interpreter of Poincaré is to understand the connections between his analysis of the geometry of space and his view of the development of the theory of space-time. On the one hand, he saw that the invariance group of electrodynamics determines a four-dimensional space with a peculiar metrical structure. On the other hand, he resisted Einstein's special theory of relativity, and continued to regard the Newtonian space-time structure as a sufficient foundation for the laws of physics. I propose to approach this question by considering the privileged position that space plays, according to Poincaré, in our conception of the physical world, and particularly in the construction of the fundamental concepts by which physical processes submit to objective measurement. Poincaré's position results from granting the concept of space an epistemological priority that, in the face of modern physics, it was unable to sustain.

Introduction

It is a striking fact that Einstein's special theory of relativity was discovered by Einstein, and not by Henri Poincaré. The elements of the special theory were certainly in Poincaré's hands before Einstein introduced it: apart from a complete mastery of the electrodynamics of moving bodies as then understood, Poincaré had the conviction that electrodynamics phenomena would never violate what he called "the relativity principle"; he also saw how to represent the Lorentz transformations as the invariance group of a four-dimensional structure, equivalent to what became

R. DiSalle (✉)
Department of Philosophy, University of Western Ontario, London, Ontario N6A 5B8, Canada
e-mail: rdisalle@uwo.ca

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known as "Minkowski space-time." He had even undertaken an epistemological analysis of the concept of simultaneity, and its connection with the speed of light, in an essay that was known to Einstein before 1905 (Poincaré 1898). It was not entirely without grounds, therefore, that Einstein's theory was referred to as "the relativity theory of Poincaré and Lorentz" (Whittaker 1951). Yet Poincaré did not accept the chief implication of the relativity theory, as articulated by Einstein and Minkowski: a new understanding of space and time in which time is relative, the speed of light is invariant, and the concept of motion relative to the ether has no place. Instead of treating the symmetry group of electrodynamics in the way that Minkowski did, that is, as the fundamental symmetry group of a novel space-time structure, Poincaré treated it as characterizing electrodynamical systems evolving within Newtonian space-time, which he continued to regard as a sufficient foundation for the laws of physics.

One of the enduring challenges for the interpreter of Poincaré is to understand his response to special relativity, and his view of the development of space-time theory generally, in connection with his philosophical views regarding geometry and space, and above all his conventionalism. One approach to this problem is to ask, to what extent was his comparatively conservative treatment of electrodynamics influenced by his conventionalist approach to geometry in general? (cf. Torretti 1983). To one who views geometry as a matter of conventional choice, it might seem that resisting relativity is as defensible as resisting non-Euclidean spatial geometry: if the facts are compatible with more than one theory, physicists are justified in choosing the simplest alternative. This view is undoubtedly an important part of the context of Poincaré's response to relativity. But Poincaré's conventionalism was more than a doctrine of choice among empirically equivalent alternatives; it was also a profound analysis of the origins of our knowledge of space and its relation to our hypotheses about physics—an analysis that, arguably, made possible the transformed conception of space-time introduced by Einstein and Minkowski. I propose, therefore, to begin with a related but quite different question: why did not Poincaré extend to space-time the kind of epistemological analysis that he had applied, with such success, to the notion of space? I suggest that a fuller understanding requires an understanding of the privileged position that space plays, according to Poincaré, in our conception of the physical world, and particularly in the construction of the fundamental concepts by which physical processes submit to objective measurement. Poincaré's epistemological analysis of the construction of space could be extended to the construction of space-time, and it was Minkowski who argued that, given the new developments in electrodynamics, such an extension was epistemologically necessary. From this perspective, Poincaré's position results from granting the concept of space an epistemological priority that, in the face of modern physics, it was unable to sustain.

Poincaré's Conventionalism and Twentieth-Century Philosophy of Science

Because Poincaré's conventionalism, on one interpretation or another, became such an influential part of twentieth-century philosophy of science, it is worthwhile to distinguish some of Poincaré's central motivations from their later uses.[1] In the logical empiricist tradition, and its twenty-first-century aftermath, conventionalism concerned the nature of physical theory as a species of abstract mathematical structure. It raised the question of how an abstract formalism can possibly yield claims about the concrete physical world; this became a problem of "coordination" between a mathematical structure and a set of empirical claims. Logical empiricism's solution, inspired by Poincaré, was that a theory becomes interpreted when the formal structure is supplemented by a convention, an arbitrary stipulation that links ("coordinates") fundamental elements of the formalism with elementary empirical facts. In the nineteenth century, once it became clear that Euclidean spatial geometry was only a special case within a family of logically equivalent geometries—the three-dimensional geometries of constant curvature—it became equally clear that their formal structures were free of any connection to claims about physical space. In itself, then, a geometry developed from a set of consistent axioms could not be meaningfully said to be true or false. Empirical claims about space could be derived only by way of interpretive stipulations, for example, that the displacements of approximately rigid bodies define a measure of congruence, or that the paths of light-rays define straight lines. The truth of any such claim, evidently, is always relative to an interpretation.

This way of thinking about the relation between geometry and experience places the question of choice between theories, and therefore of theoretical progress, in a peculiar light. Poincaré's view has not only, as it were, the positive implication that applying formal structure to the world requires an arbitrary stipulation; it has the corresponding negative implication that empirical evidence, by itself, cannot decide between theoretical alternatives. In the case of spatial geometry, Poincaré's celebrated depictions of strange physical worlds, with equivalent Euclidean and non-Euclidean interpretations, made the second point particularly vivid. Since the geometry of space can be empirically determined only by measurement, and since measurements necessarily depend on stipulations regarding the physical processes that these measurements exploit—the displacements of rigid bodies or the propagation of light—it follows that measurements cannot be uniquely interpreted as tests of geometrical claims; they are tests, at once, of the geometrical claims and the physical hypotheses that are implicit in the measurements. The optical measurement of the angle sum of a large triangle, on the stipulation that light

[1]For an insightful recent analysis of Poincaré's conventionalism, and its significance for the development of the philosophy of science, see Ben-Menahem (2006). For analysis of Poincaré's role in the history of analytic philosophy, see Coffa (1983, 1991).

travels in a straight line, may be a test of Euclidean geometry; on an alternative stipulation, it may be a test of whether light travels in a straight line. In effect, if the measured angle sum differs from 180°, the claim space is non-Euclidean, and the claim that light does not characteristically travel in straight lines, are two (among many) equivalent ways of expressing the same facts. Only considerations of simplicity, or perhaps other methodological considerations, can justify a decision between them.

This analysis can be extended in a straightforward way to the decision between special relativity and Lorentz's electrodynamics. Einstein stipulates that the velocity of light is invariant and isotropic, and determines that time and length (and whatever depends on their measures) varies with the choice of inertial frame in accord with the Lorentz transformations; Lorentz takes time and length to be invariant, but hypothesizes that the lengths of objects and the time-intervals of clocks are altered by interactions between moving bodies and the ether. But these can be represented as equivalent interpretations, based on opposing initial stipulations, of the same empirical fact—namely, that the apparent speed of light is the same in every inertial frame. Against this background, it is not impossible to sympathize with Poincaré's indifference to special relativity, or even with his resistance to it, given sufficiently compelling methodological reasons for adhering to Lorentz's theory. Moreover, it is easy to find remarks by Einstein about the role of arbitrary stipulation in the construction of his own theory. "A common time" for different observers can be defined only if we "establish *by definition* that the 'time' required by light to travel from A to B equals the 'time' it requires to travel from B to A" (1905, 894). But he gives no explicit justification for the use of light-signals in particular. And in later remarks, he speaks as if the isotropy of light-propagation, and its use in time measurement, is fixed by an arbitrary stipulation. In his popular exposition of his work (1917), he considers a possible objection to his principle: how can we test the hypothesis that the speed of light is isotropic, unless we already have a way of measuring time? The answer is that the principle is only a definition. "Only *one* requirement is to be set for the definition of simultaneity: that in every real case it provides an empirical decision about whether the concept to be defined applies or not"; that light takes the same amount of time to travel in both directions "is neither a supposition nor a hypothesis, but a stipulation that I can make according to my own free discretion, in order to achieve a definition of simultaneity" (1917, 15). In his Princeton lectures (1922), he raises the question of why light-propagation should play such a central role in his theory, and gives no better answer than that "It is immaterial what kind of processes one chooses for such a definition of time," except that it is "advantageous ... to choose only those processes concerning which we know something certain" (1922, 28–29). In short, it is easier to make sense of Poincaré's rejection of Einstein's theory, if it makes sense to regard the latter as differing from Lorentz's only in its choice of a language in which to represent the same empirical content.

Poincaré's conventionalism, then, is a crucial part of the context within which he could assert, so to speak, the epistemic right to maintain a Lorentzian interpretation of all the evidence for Einstein's theory. The notion that interpretations are arbitrary, however, is not the whole explanation for Poincaré's reaction to special relativity, nor is it the whole content of Poincaré's conventionalism itself. Another aspect, closely related yet distinct, is crucial to understanding the significance of Poincaré's view for subsequent philosophers of science, and points to a clearer understanding of the relation between Poincaré and Einstein. This is Poincaré's notion of "definition in disguise": a principle that has the form of a factual assertion, but says nothing directly about the world; instead, it fixes the meanings of the concepts that occur in it. The most familiar example is (again) the principle that light travels in a straight line, which appears to have the form of a law of nature. It could not be an empirical claim, however, unless the concept "straight line" were empirically well defined independently of this principle. Instead, the principle stipulates, in effect, that the paths of light-rays provide the criterion by which straight lines are identified in nature, and by which other objects or trajectories are judged to be straight. Similar examples are Einstein's aforementioned account of simultaneity, in which equal time-intervals are defined by the propagation of light, and Newton's second law, which specifies that force is measured by acceleration (cf. Poincaré 1902). It is the empirical significance of a concept, that is, the empirical criteria for its application that is assigned by such a definition, even where the concept may be thought of as well understood in some other sense. Empirical definition in this manner may be thought of as a species of implicit definition, in which concepts are understood by means of the axioms in which they occur, which axioms therefore do not appeal to or presuppose—though they may be motivated by—some pre-theoretical intuitions associated with the concept. When Russell (1899) objected that the undefined primitive concepts of geometry, such as shape, are known by intuition, Poincaré argued that shape is implicitly defined by the principle of free mobility: the proposition that "bodies can be moved in space without change of shape" does not add to a previously-defined concept of shape, but is partly constitutive of any understanding of "shape" that we have. The proposition thus states that "in order for measurement to be possible, it is necessary that figures be susceptible of certain movements, and that there be a certain thing that will not be altered by those movements and that we will call 'shape'" (1899, 259). Hilbert's (1899) axiomatization of geometry gave the clearest statement, and vindication, of this general way of thinking, to the consternation of those mathematicians to whom geometrical concepts were inseparable from intuitions about space. Euclidean geometry, on this view, does not particularly have constructions in space as its subject matter; its subject matter is the abstract structure that the axioms define, and its objects are any objects at all that may be thought of as satisfying the axioms. Thus the meaning of "straight" is logically defined by the axioms. A principle such as "light travels in straight lines" is a step toward an empirical interpretation of the abstract formalism, by arbitrarily assigning a physical meaning to a mathematical term, thus "coordinating" the abstract formal structure with a physical structure.

Experience and Geometry

It is just this way of thinking about the interpretation of a physical theory that poses the "problem of representation" in its starkest form: how is it possible for an abstract formal structure to be a representation of the concrete physical world, or even of the phenomenal world of our experience? Van Fraassen, following Reichenbach (1965), identified this as "Reichenbach's problem of coordination."

> [T]he basic perplexity emerges in its purest form when we ask [what does it mean] to embed the phenomena in an abstract structure. Or to represent them by doing so? . . . Hence the most fundamental question is this: How can an abstract entity, such as a mathematical structure, represent something that is not abstract, something in nature? (Van Fraassen 2008, 240)

Van Fraassen's perplexity starts from Reichenbach's idea that the mathematical representation of the world, understood as a formal relation or isomorphism between the mathematical structure and the phenomena, begs the main question: a mathematical structure can represent the phenomena only on the assumption that the latter, too, already have a mathematical representation. This appears to be an insuperable problem in principle, and Van Fraassen argues that it only has a pragmatic solution (2008, Chap. 11). Yet a large part of the difficulty is the way in which the problem is posed. Demopoulos (2013) suggested a more illuminating way of thinking about the problem of representation:

> . . . .[It] should be far from obvious that Reichenbach and Van Fraassen have succeeded in raising a genuine problem for the representational use of an isomorphism. To begin with, such a use of the notion requires only that we bring the things correlated under a concept; doing so does not by itself constitute a mathematical representation. Nor does such a conceptual representation reduce to or presuppose a mathematical representation. Any reasonable formulation of the problem of how mathematics represents reality must be predicated on the assumption that we can provisionally take for granted what is meant by conceptually representing reality.
>
> Were we to follow Reichenbach and Van Fraassen, we would be forced to reject Frege's celebrated solution to the problem of how arithmetic applies to reality . . . . [For] Frege, this is explained by the fact that our judgments of cardinality rest on relations between concepts, and concepts sometimes apply to reality. (Demopoulos 2013, 89)

Demopoulos's remark refers to Frege's use of "Hume's principle" in his account of the natural numbers, as a definition of numerical identity:

> For any concepts *F* and *G*, the number of *F*s is identical with the number of *G*s if and only if the *F*s and the *G*s are in one-one correspondence. (Demopoulos 2000, 210)

This example suggests that the problem of representation is not the insuperable one of finding a correspondence between an abstract formalism and a large set of concrete particulars, but the more tractable task of connecting a mathematical representation with a conceptual representation. Poincaré's examples of the empirical interpretation of mathematics reveal the essential role played by representations of this kind. The most striking case is the connection between the formal structure of geometry and the conceptual representation of space. The elementary experiences that are responsible for our awareness of space are not only retinal images or tactile sensations, but also kinaesthetic sensations occasioned by our voluntary movements. But spatial geometry is not a representation of the set of those particulars. Rather, it is a representation of the conceptual scheme through which we interpret those sensations, in developing a spatial awareness. More precisely, this conceptual scheme constitutes our awareness of space. Helmholtz had first identified this conceptual scheme, considering how we come to a notion of space in the first place—how, in other words, we come to distinguish certain characteristic features of the world of experience as its peculiarly spatial characteristics. Arguably, Newton and Kant had taken steps to analyze our knowledge of the geometrical features of space, and their relation to our spatial intuition. But they did not further analyze the basis for our spatial intuitions themselves. Helmholtz, by contrast, sought to explain what had generally been taken for granted by philosophers, namely, our awareness that a certain kind of intuition is in fact distinctively spatial, i.e. that certain differences in our environment are sensed specifically as changes of spatial relations. These are the changes that are controlled by our own voluntary actions: we can bring about such a change by our own voluntary movement, or cancel it by a contrary movement, restoring our environment to its previous state (1878, 225–227). This immediate sensation of arbitrary and reversible changes of perspective is, in fact, one of the most striking differences between space and time. For Helmholtz, this shows that the intuition of space that Kant had taken as a starting point has, after all, a deeper origin—an origin, moreover, that depends on a contingent feature of the physical world, namely, the free mobility of rigid bodies, of which our own arbitrary shifts of perspective provide a psychologically immediate exemplar.

Poincaré extended Helmholtz's analysis in ways that illuminated its philosophical significance in several ways. On the one hand, Poincaré brought out the conventional aspect of the link Helmholtz had revealed between geometry and physics. Helmholtz regarded it as a fact that certain bodies are approximately rigid, and that certain motions leave their dimensions approximately unchanged; Riemann had regarded this as only a hypothesis, to be corrected by more exact empirical knowledge. Poincaré, however, regarded this principle as a definition like the principle that light travels in a straight line: while it is an empirical fact that a large set of bodies approximately maintain their relative dimensions, it is impossible to determine empirically that these really do move without any change of dimensions, for they provide the empirical criteria for such changes. This was the basis, as we noted above, for Poincaré's response to Russell. On the other hand, Poincaré saw, more clearly than Helmholtz, how the elementary conceptual scheme of changes in spatial perspective, which constitutes the basis for our intuitive notion of space, also constitutes the basis for our mathematical representation of space. Implicit in our understanding of such changes is the principle that they may be not only executed at will, but also combined and reversed to return to a previous perspective. But these pre-systematic notions of combination, negation, and identity provide the basis for the mathematical concept of a group. The group of rigid motions, in other words, is not merely an abstract formalism that, with the help of appropriate stipulations, can be given an empirical interpretation. It is, instead, a direct mathematical representation of a conceptual scheme that characterizes one of the most elementary systems of empirical judgments: judgments about the relative sizes and positions of the objects in one's environment.[2]

From Poincaré's analysis, we can see that the twentieth-century problem of scientific representation, as described above, was misleadingly posed, because it began with a false step. The task of connecting scientific theories with experience is not a task of representing an infinite set of concrete particulars with an abstract formalism. It is, rather, a more tractable task, one that moves in quite the opposite direction, and that has actually been accomplished by the earliest development of geometry. It is only since the nineteenth century that one can consider the infinite possibilities for abstract geometrical structures, and ask how they can be interpreted as theories of actual space. But the initial task of formal geometry was, instead, to give a formal geometrical interpretation to an elementary conceptual structure—a structure sufficiently elementary to capture the most primitive conception of spatial experience, but formal enough to serve as the foundation for the group of rigid motions. The further elaboration of geometry, through nineteenth-century innovations and even the twentieth-century conception of space-time, continues in the same direction. Even these revolutionary developments maintain their connection with experience—and their empirical content—as extensions of that elementary conceptual scheme.

Poincaré furnished a fairly clear account of how such an extension works, even if subsequent philosophical glosses of his views have made it harder to recognize. Again, from the twentieth-century perspective, the "definition in disguise" that light travels in a straight line is one of the arbitrary coordinative definitions that assigns empirical content to the otherwise uninterpreted formalism of spatial geometry. For Poincaré, however, spatial geometry is itself an interpretation of our primitive awareness of voluntary motion and its proto-mathematical (group-theoretic) structure. The association between these two conceptual schemes is not the result of any arbitrary decision, though of course we are free to consider alternative schemes; it is the primitive association through which we have any conception of space at all. In this primitive scheme, the treatment of lines of sight as straight lines is usually an obvious and spontaneous move. This association between light propagation and straight lines becomes a matter of conventional decision only when the question arises of extending our primitive local conception of space to arbitrarily large areas. The local conception is uniquely—though, again, not necessarily—captured by the group of rigid motions. But there are at least two obvious ways of extending it, theoretically, to arbitrarily large areas. One is by assuming that arbitrarily long lines of sight are straight—or, more precisely, are definitive of straightness on larger scales. The other is by assuming that the Euclidean relations that hold locally, on scales that are within our immediate perceptual grasp, continue to hold at arbitrarily large scales. Through most of the history of geometry up to the nineteenth century,

[2]On the connection between Helmholtz's empiricism and Poincaré's conventionalism, see DiSalle 2006 (Chap. 3).

it was never doubted that these two ways would always (even necessarily) lead to the same result. Once it was understood that they might diverge—chiefly through the work of Gauss, Bolyai, Lobatchevsky, Riemann, and Helmholtz—empiricists such as Riemann and Helmholtz argued that the laws of physics, present or future, would determine how ordinary geometry would extend to larger or smaller scales (cf. Riemann 1867; Helmholtz 1870). Poincaré recognized that empirical evidence alone could not compel us to take one way or the other.[3]

The Relativity of Space and the Relativity of Motion

This view of space, then, provides the background against which we can understand Poincaré's reaction to special relativity, and his view of the electrodynamics of moving bodies and Einstein's new conception of space and time. Einstein's theory developed from a critical analysis of the separation between space and time. The contradiction that Einstein identified as his starting point, between the invariance of the velocity of light and the relativity principle, is resolved when we see, on the one hand, that it presupposes the invariance of simultaneity; and, on the other hand, that the available means of defining simultaneity—that is, of giving objective criteria for applying the concept to any pair of events—fail to define an invariant relation. In short, the relation that constitutes the separation between space and time, by distinguishing relations in space at a given moment from spatio-temporal relations among events at different times, has, on Einstein's analysis, a purely relative significance. The crucial point, for our purpose, is that the independent status of spatial relations turns out to depend on a spatio-temporal principle, because the relation of simultaneity is mediated by a dynamical principle, that is the constancy of the velocity of light. But the constancy of the velocity of light turns out to satisfy, unexpectedly, a novel kind of relativity principle, on which simultaneity is relative. For Poincaré, however, space is constituted completely by a principle that is prior to the laws of dynamics, the principle of free mobility. The extension of this principle to the space, or space-time, of physics requires the adoption of conventions that connect geometry with dynamics.

This view of the autonomy of space is, in turn, central to Poincaré's understanding of relativity in general. Relativity is, for Poincaré, implicit in the notion of space; it is an immediate consequence of the homogeneity of space. In this sense it is a pre-dynamical principle, one that follows from the primitive conception of space based on our own local displacements.

> [A]s I am conscious that, in passing from the position A to the position B, my body has remained capable of the same movements, I know there is a point of space related to the point a′ just as any point b is related to the point a, so that the two points a and a′ are

[3] See in particular Torretti (1977) for the philosophical origins and development of empiricism and conventionalism.

> equivalent. This is what is called the homogeneity of space. And, at the same time, this is why space is relative, since its properties remain the same whether it is referred to the axes A or to the axes B. Thus the relativity of space and its homogeneity are one and the same thing. (Poincaré 1908, 113)

From the perspective of space-time theory, it is easy to point out the error in Poincaré's analysis, an error that has been, indeed, endemic in philosophical debates over space and motion from the time of Leibniz. The relativity of space in Poincaré's sense, meaning the homogeneity of space, is a completely separate issue from the relativity of motion in the sense of modern physics. From the assumption that the parts of space are indistinguishable—that there is no preferred origin, and that spatial translations, reflections, and rotations effect no genuine changes in the states of physical systems—it does not follow that change of spatial location over time makes no physical difference, or is not an objective change. Confusing these two issues was the essential error in some of the classic arguments of Leibniz (1716) against Newton's theory of absolute space; Newton also held that space is homogeneous, and that its parts are indistinguishable, but this was perfectly compatible with holding that it does make a difference whether a body occupies the same spatial location at different times. This is analogous to the relativistic principle that there is no preferred velocity, but any change of velocity makes a genuine physical difference. The question of absolute motion, in short, has to do, not with space by itself, but with space-time; it concerns the question how, or even whether, space is connected through time. Or, in other words, it concerns the question whether, in addition to spatial relations among things considered at a given moment, there are also spatio-temporal relations such as "same place at different times," or "same velocity at different times". Such questions pertain to the laws of dynamics, not to the geometry of space alone.

Maintaining this mistaken connection between the homogeneity of space is one of the two central errors that characterize Poincaré's view of relativity in general. When he claims that dynamical distinctions between states of motion are inherently philosophically suspect, it is the homogeneity of space that justifies him; this is why the concept of absolute rotation, which he admits is inseparable from classical mechanics, strikes him as nonetheless a philosophical embarrassment. But the dynamical distinction between rotation and non-rotation, as measured by centrifugal effects, even if it does challenge the most general thesis of the relativity of motion, certainly does not contradict the homogeneity of space. Geometrically it depends, not on the rotating body's changing relation to the individual parts of space, but on the relative velocities of the individual parts of the rotating body.

This is why the relativity of motion is a spatio-temporal issue, completely separate from the issue of the relativity of space in Poincaré's sense. It is a straightforward matter to spell out the issue, and even to resolve it, using the framework of four-dimensional geometry. Here classical space-time is a four-dimensional space, decomposable into three-dimensional subspaces by the relation of absolute simultaneity, which separates space at each moment, and the momentary configuration of the universe, from space at any other moment; each spatial "slice" is homogeneous, but the relativity of motion concerns the trajectories or world-lines that connect positions of objects over time. According to Newtonian mechanics, there is no privileged set of trajectories that represent the same spatial locations at different times—what Newton called "absolute space"—but even if there were, this would not affect the homogeneity of space itself. The latter concerns only the structure of space, but the question of privileged trajectories concerns the dynamical laws that distinguish them.

This criticism of Poincaré might appear, however, to be anachronistic and unfair, based on a twentieth-century perspective that was only beginning to develop near the end of Poincaré's life. It would therefore be illuminating to consider his historical context. By the time that *La science et l'hypothèse* was published (1902), the concept of absolute space had already been subjected to a severe critical examination. One part of this was Mach's critical analysis of Newton's principle of inertia and the concept of absolute rotation, which suggested the possibility of new laws of motion and eventually motivated Einstein to seek a "general theory of relativity" (Mach 1883). But the more relevant part, for the present discussion, developed entirely within the framework of Newton's laws, and led to the insight that the concept of absolute space is completely superfluous: the essential Newtonian conceptions of force, mass, and acceleration define an equivalence class of frames of reference, without requiring a distinguished "absolute space" as a privileged rest-frame. It is therefore a misconception that special relativity eliminated absolute space from physics; by the time Einstein began his work on electrodynamics, the concept of inertial frame was already widely known, and absolute space was already widely understood to be unnecessary. Thomson (1884) introduced the notions of "reference-frame" and "reference-dial-traveller," i.e. a spatial frame and a temporal standard relative to which motion may be measured, and a new expression of Newton's laws: For any system of particles moving anyhow, there exists a reference-frame and a time-scale with respect to which every acceleration is proportional to and in the direction of an applied force, and every such force belongs to an action-reaction pair. Moreover, any frame in uniform rectilinear motion relative to such a frame is also an inertial frame. Independently, Lange (1885) offered an essentially equivalent conception, with the more suggestive terminology of "inertial system" and "inertial time-scale." Both versions were emphatically cited by Mach, in the second (1889) and later editions of *Die Mechanik*,[4] as eliminating the need to appeal to absolute space; indeed, Mach credited Newton himself with the essential idea, as expressed in his fifth Corollary to the laws of motion.

According to Mach, in other words, even Newton had taken pains to show that the solutions to problems of motion, as undertaken in the *Principia*, are independent of the assumption of absolute space.

By 1905, Einstein, who obviously had read Mach with some care, apparently felt no need to defend the idea that Newtonian mechanics satisfies the relativity principle, and, instead of absolute space, requires only "a coordinate system in

[4]On the development of the concept of inertial frame, see DiSalle (2009); on Mach's role in this development, see DiSalle (2002).

which the equations of mechanics are valid" (Einstein 1905, 892). By the relativity principle, any system that is in uniform motion relative to such a system is physically equivalent to it; the open question was whether electrodynamics could show a similar invariance, instead of defining the velocity of electromagnetic waves as relative to a stationary medium, the ether, which thereby defines a kind of privileged rest-frame. This did not imply a violation of the relativity principle, however: the velocity of light relative to the ether is, after all, still a relative velocity, as was noted by Maxwell (Maxwell 1878, 35), and by Poincaré himself. Therefore it was coherent to maintain the equivalence of inertial frames, while acknowledging that one subset of them happens to represent the rest-frame of a physical object, the ether.

Poincaré, however, does not seem to have been aware of this development in the foundations of Newtonian mechanics. His second error, then, is his conviction that the Newtonian distinctions among states of motion, which he admits are well founded within the theory, require the supposition of absolute space. Given this conviction, it was therefore natural for him to suppose that absolute rotation posed a philosophical difficulty. Obviously, Poincaré understood the classical principle of relativity, as the principle that

> . . . the accelerations of the various bodies that make up an isolated system depend only on their relative velocities and positions, and not on their absolute velocities and positions, provided that the mobile axes to which their relative motions are referred are engaged in a uniform rectilinear motion. Or, if you prefer, their accelerations depend only on the differences of their velocities and the differences of their coordinates, not on the absolute values of those velocities or those coordinates. (Poincaré 1902, 136)

This way of expressing the principle evidently does not exclude the possibility of taking absolute space to be the background against which these relative motions are understood. But Poincaré explicitly rejected the existence of absolute space. The difficulty that he faced, at least at the time of writing *La science et l'hypothèse*, was that absolute space appeared to him to be implicit in Newtonian dynamics, because (he thought) it is implicit in the dynamical distinction between inertial and non-inertial, especially rotational, motion. That such a distinction can be made—for example, that the earth's rotation can be experimentally established by its equatorial bulge or by Foucault's pendulum experiment—"is a fact that shocks the philosopher but that the physicist is forced to accept" (Poincaré 1902, 99).

> And yet, to say in such a case that the earth turns, would that have any meaning? If there is no absolute space, can something turn without turning in relation to something? And, on the other hand, how can we admit Newton's conclusion and believe in absolute space? (Poincaré 1902, 138)

Apparently unaware of the resolution provided by the concept of inertial frame, Poincaré resolves the difficulty by treating the rotation of the earth as a matter of convention. The reasonable inference to draw from the experimental evidence is,

> It is more convenient to suppose that the earth turns, because in this way one can express the laws of mechanics in a much simpler language.
>
> That does not change the fact that absolute space, that is to say the reference-point to which one must refer the earth in order to know if it really rotates, has no objective existence. For

> the affirmation "the earth turns" has no meaning, since no experiment would permit us to verify it . . . or, rather, the two expressions "the earth turns" and "it is more convenient to suppose that the earth turns" have one and the same meaning; there is nothing more in the one than in the other. (Poincaré 1912, 141)

Poincaré's remarks highlight an important aspect of the philosophical perspective from which, around 1905, he approached the problems in electrodynamics that led Einstein to special relativity. In short, the relativistic aspect of Newtonian mechanics, which had been a focal point for Einstein, was not entirely clear to Poincaré.

Conclusion: Poincaré and Special Relativity

After the emergence of special relativity, and of Minkowski's four-dimensional account of it (1908, 1909), Poincaré appears to have seen the classical relativity principle, and the problem of absolute space, in a much clearer light. Now, in an essay on "L'espace et le temps" (1912), he makes a much clearer distinction between the relativity of space and the relativity of motion, designating the former as "psychological relativity," and the latter as "physical relativity" (1912, 42). The former refers not only to the homogeneity of space, or the indifference to changes of position and orientation, but also to the indifference, as far as our perceptions are concerned, to any deformation of objects that leaves their relative sizes unchanged. "We can only perceive modifications in the forms of objects that differ from the simultaneous modifications in the forms of our measuring instruments" (38). "Geometry is possible only because of our choice to regard certain instruments as rigid, and to study the group of their displacements" (40). Physical relativity, however, is much more restricted than psychological relativity: changes of coordinates must preserve the differential equations of physics. These will necessarily be altered if we shift to an accelerating or rotating frame of reference, for then centrifugal or inertial forces will have to be introduced. On the basis of this principle of relativity, experiments such as Foucault's pendulum can be regarded as demonstrating the rotation of the earth. Poincaré adds:

> There is something in this that shocks our ideas about the relativity of space a bit, ideas that are founded on psychological relativity, and this discord has seemed embarrassing to many philosophers. (Poincaré 1912, 42)

As we have seen, Poincaré himself had formerly felt this same embarrassment, and so this remark can be read as a diagnosis of his own past error. Now, that he is aware of the relativistic interpretation of Lorentzian electrodynamics—what he calls "the principle of relativity of Lorentz" (52)—he appears to appreciate that classical mechanics and "the new mechanics" are equivalent from the point of view of "physical relativity," insofar as each is characterized by a group of transformations of their spatial and temporal coordinates. The difference arises from the peculiar nature of the Lorentz transformations, which do not preserve spatial and temporal measurements individually. Shapes are deformed in the passage from one Lorentz coordinate system to another, while simultaneity relations and time intervals are altered. The important point, Poincaré notes, is that "on the new conception, space and time are no longer entirely distinct entities that may be viewed separately, but two parts of a single whole, and two parts that are too closely intertwined to be easily separated" (Poincaré 1912, 53).

In effect, Poincaré articulates the same view of special relativity that Minkowski had articulated (1908, 1909). This ought to be unsurprising, given the already-mentioned steps that Poincaré had earlier taken (1905) toward the appropriate four-dimensional formulation of special relativity; perhaps one could wonder at his calling it *Lorentz's* principle of relativity, and omitting the name of Einstein. The crucial difference is that Poincaré represents the new theory and the old as equivalent alternatives, and the choice between them as a matter of convention. According to Minkowski, on the contrary, Einstein had shown that the new principle of relativity "is not an artificial hypothesis, but rather a novel understanding of the time-concept that is forced upon us by the appearances" (Minkowski 1908, 56). The core of this new understanding is the relativity of time, following from the relativity of simultaneity. As Minkowski notes, the difference between Einstein's space-time structure and the Newtonian structure, with absolute simultaneity, may be represented as the difference between a group of transformations preserving the finite velocity c and a group in which c increases to infinity. But the choice between them is not conventional; it is determined by Einstein's destructive critical analysis of the concept of absolute simultaneity.

> Lorentz called the t' combination of x and t the local time of the uniformly moving electron, and applied a physical construction of this concept, for the better understanding of the hypothesis of contraction. But to have recognized clearly that the time of the one electron is just as good as that of the other, that is, that t and t' are to be treated equally, was first the merit of A. Einstein. (Minkowski 1909, 107)

The choice, then, is not a mere question of simplicity; Einstein has shown that the symmetry relations of the electrodynamics of moving bodies determine a corresponding symmetry in the measure of simultaneity, in which it is relative to the choice of inertial frame. That is, the invariance of the velocity of light makes it impossible to determine an absolute relation of simultaneity.

Poincaré pointed out, rightly, that the alternative "convention" also determines a theory of time, and imposes general constraints on any definition of the simultaneity relation. For these reasons one can say that it defines concepts of time and space, including length and simultaneity (Poincaré 1912, 51). What it does not provide, as Einstein showed in 1905, are empirical criteria for the application of these concepts, of a kind that would enable us to establish invariant measures; Einstein could supply a natural criterion of simultaneity, but it leads directly to the consequence that simultaneity is relative. Therefore there is no constructive procedure, in principle or in practice, for constructing the geometry of Newtonian space-time. Even though Poincaré explains how the determination of length and time follows from the dynamical symmetries of classical mechanics, his underlying assumption is that lengths and times (and therefore simultaneity) are measureable independently of the dynamical theory, which indeed takes those measures for granted. Einstein, however, has located the need for a convention at the more fundamental level, one that supplies empirical criteria for the application of his own theory, and therefore poses a challenge to supply something equivalent for the Newtonian theory. The Newtonian definition of simultaneity, however, is no longer connected to any empirical means of applying it. From the point of view of the epistemology of time, the Newtonian and the Einsteinian conventions are therefore not equivalent.

We can see from this comparison that, in one sense, Poincaré's view of space-time theories, and in particular of the novel space-time theory that Einstein and Minkowski constructed—on empirical and theoretical foundations that were so well known to Poincaré himself—were profoundly influenced by his view of space, and his epistemological analysis of spatial geometry. From his deep conviction of the autonomy of space, as the schematic structure of our experience of local motion, he first developed a view of relativity as a principle determined by the homogeneity of space alone. On this view he found it difficult to bring the relativity of space and the classical relativity of motion, as a symmetry principle of dynamics, into a philosophically coherent whole. In another sense, however, Poincaré may be seen as failing to bring his analysis of space to bear on the question of space-time. His account of space reveals that its group structure has a direct empirical significance, through our experience of free mobility—a significance that is not uniquely and necessarily determined, insofar as we are free to define its significance by different conventions, but that is unquestionably sufficient, at least at small spatial scales. Einstein revealed, however, that, except at the very smallest scales, determining spatial intervals requires an understanding of simultaneity, sufficient to establish its empirical significance. Poincaré had enlarged on this very theme, and on the crucial role of assumptions about light in determining simultaneity, in his essay "La mesure du temps" (1898). It was Einstein, however, who showed that the invariance of the speed of light, at one stroke, undermines the Newtonian conception of simultaneity and defines a new one, with its own direct empirical significance.

On this new definition, again, simultaneity becomes relative, and the objective measure of space is no longer possible. Then Minkowski showed that the invariance of the speed of light provides the constructive principle for a new space-time structure, just as rigid displacements provide a constructive principle for the structure of space. Because this constructive principle itself provides a destructive critique of the previous conception of simultaneity, however, it does not permit us to regard the previous conception of space-time as an equivalent alternative. For the old assumption of absolute simultaneity was crucial to extending the space of local displacements to a theory of space-time; it alone made possible that autonomy of spatial structure on which Poincaré, and the classical picture of space-time, relied. In other words, the isolation of spatial structure from dynamical structure—from the structure of space-time—could no longer be maintained, given the facts of electrodynamics and the want of an empirical criterion of simultaneity. In these new experimental and theoretical circumstances, Poincaré's insight into the relation between formal structure and experience could be carried forward, not by Poincaré himself, but by those who saw how to extend its application to the construction of space-time.

References

Ben-Menahem, Y. 2006. *Conventionalism*. Cambridge: Cambridge University Press.

Coffa, J.A. 1983. From geometry to tolerance: Sources of conventionalism in the 19th century. In *From Quarks to Quasars*, Pittsburgh Studies in the Philosophy of Science, vol. X, ed. R.G. Colodny. Pittsburgh: University of Pittsburgh Press.

Coffa, J.A. 1991. *The semantic tradition from Kant to Carnap*. Cambridge: Cambridge University Press.

Demopoulos, W. 2000. On the origin and status of our conception of number. *Notre Dame Journal of Formal Logic* 41: 210–226.

Demopoulos, W. 2013. Carnap's analysis of realism. In *Logicism and its philosophical legacy*, ed. W. Demopoulos, 68–89. Cambridge: Cambridge University Press.

DiSalle, R. 2002. Reconsidering Ernst Mach on space, time, and motion. In *Reading natural philosophy: Essays in the history and philosophy of science and mathematics to honor Howard Stein on his 70th birthday*, ed. D. Malament. Chicago: Open Court Press.

DiSalle, R. 2006. *Understanding space-time: The philosophical development of physics from Newton to Einstein*. Cambridge: Cambridge University Press.

DiSalle, R. 2009. Space and time: Inertial frames. In *The Stanford encyclopedia of philosophy,* (Winter 2009 edition), ed. Edward N. Zalta. http://plato.stanford.edu/archives/win2009/entries/spacetime-iframes/.

Einstein, A. 1905. Zur elektrodynamik bewegter Körper. *Annalen der Physik* 17: 891–921.

Einstein, A. 1917. *Über die spezielle und die allgemeine Relativitätstheorie (Gemeinverständlich)*, 2nd ed. Braunschweig: Vieweg und Sohn.

Einstein, A. 1922. *The meaning of relativity*. Princeton: Princeton University Press.

Helmholtz, H. 1870. Ueber den Ursprung und die Bedeutung der geometrischen Axiome. *Vorträge und Reden* 2: 1–31.

Helmholtz, H. 1878. Die Thatsachen in der Wahrnehmung. *Vorträge und Reden* 2: 215–247.

Hilbert, D. 1899. *Grundlagen der Geometrie*. Leipzig: Teubner.

Lange, L. 1885. Ueber das Beharrungsgesetz. *Berichte der Königlichen Sachsischen Gesellschaft der Wissenschaften zu Leipzig, Mathematisch-physische Classe* 37: 333–351.

Leibniz, G.W. 1716. Correspondence with S. Clarke. In Gerhardt (1960), vol. VII, 345–440.

Mach, E. 1883. *Die Mechanik in ihrer Entwickelung, historisch-kritisch dargestellt*. Leipzig: Brockhaus.

Mach, E. 1889. *Die Mechanik in ihrer Entwickelung, historisch-kritisch dargestellt*, 2nd ed. Leipzig: Brockhaus.

Maxwell, J.C. 1878. *Matter and motion*. New York: Van Nostrand.

Minkowski, H. 1908. Die Grundgleichungen für die elektromagnetischen Vorgänge in bewegten Körper. *Nachrichten der königlichen Gesellschaft der Wissenschaften zu Göttingen, mathematisch-physische Klasse*, 53–111.

Minkowski, H. 1909. Raum und Zeit. *Physikalische Zeitschrift* 10: 104–111.

Poincaré, H. 1898. La mesure du temps. *Revue de Métaphysique et de Morale* VI: 371–384.

Poincaré, H. 1899. Des fondements de la géométrie; a propos d'un livre de M. Russell. *Revue de Métaphysique et de Morale* VII: 251–279.

Poincaré, H. 1902. *La Science et L'Hypothèse*. Paris: Flammarion.

Poincaré, H. 1905. Sur la dynamique de l'électron. *Comptes rendues de l'Académie des Sciences* 140: 1504–1508.

Poincaré, H. 1908. *La Science et La Méthode*. Paris: Flammarion.

Poincaré, H. 1912. L'espace et le temps. In Poincaré (1913), 97–109.

Poincaré, H. 1913. *Dernières Pensées*. Paris: Flammarion.

Reichenbach, H. 1965. *The Theory of Relativity and A Priori Knowledge*. Trans. Maria Reichenbach. Berkeley/Los Angeles: University of California Press. Originally published as *Relativitätstheorie und Erkenntnis Apriori*, Berlin, 1920.

Riemann, B. 1867. Ueber die Hypothesen, die der Geometrie zu Grunde liegen. In *The collected works of Bernhard Riemann*, ed. H. Weber, 272–287. Leipzig: B.G. Teubner, 1902; reprint, New York: Dover Publications, 1956.

Russell, B. 1899. Sur les axiomes de la géométrie. *Revue de Métaphysique et de Morale* VII: 684–707.

Thomson, J. 1884. On the law of inertia; the principle of chronometry; and the principle of absolute clinural rest, and of absolute rotation. *Proceedings of the Royal Society of Edinburgh* 12: 568–578.

Torretti, R. 1977. *Philosophy of geometry from Riemann to Poincaré*. Dordrecht: Riedel.

Torretti, R. 1983. *Relativity and geometry*. Oxford: Pergamon Press.

Van Fraassen, B. 2008. *Scientific representation: Paradoxes of perspective*. Oxford: Oxford University Press.

Whittaker, E.T. 1951. *A history of the theories of aether and electricity*, Revised edition. London: Thomas Nelson.

About the Authors

Augusto J. Franco de Oliveira: Augusto J. Franco de Oliveira is Emeritus Professor at the University of Évora. His research interests cover logic, nonstandard mathematics and the philosophy of mathematics.

María de Paz: María de Paz is PhD student at the Centre for Philosophy of Science at the University of Lisbon and at Department of Theoretical Philosophy of Complutense University of Madrid. Her research concerns Poincaré's conventionalism and its relation with mathematical physics.

Robert DiSalle: Robert DiSalle is Professor of Philosophy at the University of Western Ontario. His research concerns the philosophy and history of modern physics, especially the philosophical foundations of space-time theories.

Janet Folina: Janet Folina is Professor of Philosophy at Macalester College, St Paul, MN. She is interested in the philosophy of mathematics and its history. In particular, she enjoys trying to better understand how the philosophical views of mathematicians affect the discipline of mathematics.

Gerhard Heinzmann: Gerhard Heinzmann is Professor of Philosophy at the University of Lorraine (Nancy) and Director of the Center of Humanities (MSH Lorraine). His research concerns philosophy of language and philosophy of mathematics, especially around Henri Poincaré.

Reinhard Kahle: Reinhard Kahle is Professor at the Department of Mathematics at Universidade Nova in Lisbon, Portugal. His research interests cover mathematical logic, philosophical logic, and logic in Computer Science as well as the history and philosophy of logic.

João Príncipe: João Príncipe is Assistant Professor in the Physics department at the University of Évora. His research concerns the history of French physics in nineteenth and twentieth centuries, Historical Epistemology and the History of Ideas in Portugal.

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Laurent Rollet: Laurent Rollet is Lecturer in philosophy and history of science at the University of Lorraine (Nancy). He is a researcher at the Laboratoire d'histoire des sciences et de philosophie—Archives Henri Poincaré, where he is in charge of the edition of the administrative and private correspondence of the mathematician.

Isabel Serra: Isabel Serra is retired Professor of History and Philosophy of Science at the University of Lisbon. Her research interests cover the interaction between philosophy and history of science, especially on late nineteenth and twentieth century.

Antonio A. P. Videira: Antonio A. P. Videira is Professor of Philosophy at the State University of Rio de Janeiro. His research concerns the philosophy of science, the philosophy of nature and the history of science.

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